The Philosophy of Scientific Debate Traditions

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Abstract of the Dissertation

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In this dissertation, I develop an account of the structures and dynamics of debate in science. This account consists of a framework of concepts that identify and organize the features of scientific debates. These concepts are applied to the histories of two concrete case studies of ongoing scientific debates. Further concepts regarding the specific changes that may occur within scientific debates are abstracted from these case studies. This central concept I develop is 'the scientific debate tradition.' This refers the various rival positions of a scientific debate, and the relationships between them, as they change over time. I investigate the implications of these changes for our conception of scientific practice. Through the analysis of the concrete case studies, I identify a number of specific modes of debate that a tradition may take up. Changes to the relationships between rival debate positions occur as a scientific debate shifts from one mode of debate to the next. In the first chapter, I explore relevant work in the philosophy of science on 'traditions' of scientific practice, especially Imre Lakatos's notion of the 'scientific research programme.' The second chapter develops the notion of the scientific debate tradition and its associated framework of concepts. The third chapter offers a case study of a debate in the field of neurobiology over the nature of 'synaptic vesicles,' tiny organelles responsible for neurotransmission. The fourth chapter offers a case study of a debate from the field of developmental psychology over children's ability to understand that others have separate minds and perspectives. In the final chapter, I apply the notion of the scientific debate tradition to the two case studies. From the histories of these case studies I abstract a series of concepts regarding the modes of debate which a tradition can possess.

Dedicated to my parents

Cynthia Rosenberger
and
Robert Rosenberger

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Introduction

Before they come to a close, scientific debates evolve in significant ways. The philosophy of science and the field of science studies have made progress in addressing how science discovers what is true or real, and how controversies in science come to a close. However, an underexamined issue is how scientific debates develop and change before they end. Changes in structures, modes, and relationships of scientific debates over time have significant effects on the positions that make up those debates.

In what follows, I develop an account of the structures and dynamics of debate in science. This account consists of a framework of concepts that identify and organize the features of scientific debates. These concepts are applied to the histories of two concrete case studies of ongoing scientific debates. Further concepts regarding the specific changes that may occur within scientific debates are abstracted from these case studies.

The central concept developed in this dissertation is 'the scientific debate tradition.' This refers to all of the positions of a debate in science, and the relationships between them, as they change over time. The notion of the scientific debate tradition includes a framework of concepts regarding the specific structures of these relationships. These concepts regard features of scientific debate such as the 'primary' and 'branch' debates, the efforts to 'maintain' the debate's coherency, and the changing 'modes' of debate that characterize the discussion.

The notion of the scientific debate tradition sheds new light on the changing relationships between the rival positions of scientific debate. I refer to these changes as 'debate evolution,' and investigate the implications of these changes for our conception of scientific practice. Through the analysis of the concrete case studies, I identify a number of specific modes of debate that a tradition may take up. Debate evolution occurs as a scientific debate shifts from one mode of debate to the next.

The notion of the scientific debate tradition builds on a line of thought from the philosophy of science that conceptualizes science in terms of advancing structures of scientific theories and research heuristics. This line of research includes notions such as Kuhn's 'paradigms,' Lakatos's 'research programmes,' Laudan's 'research traditions,' and Galison's 'experimental traditions.' Also, I appropriate a number of concepts from the field of science studies regarding the ways research results are transmitted to others. I adjust these concepts for use in the articulation of the ways scientific debates are established and maintained. Despite building on the insights from the history of the philosophy of science and science studies, my account goes beyond this history by focusing on the evolving relationships between the positions of scientific debates.

In the first chapter, I review work from the history of the philosophy of science relevant to the account I develop in the following chapters. The central philosophy I consider is Imre Lakatos's 'methodology of scientific research programmes.' The works of figures such as Thomas Kuhn, Larry Laudan, Ian Hacking, Mara Beller, Don Ihde, and Peter Galison are also reviewed. In addition, I summarize several concepts from the field of science studies that will be useful in the next chapter.

In chapter two, I develop the notion of the 'scientific debate tradition.' This includes articulating a number of concepts such as the 'problem hub,' 'primary' and 'branch' debates, 'debate nodes,' 'maintenance,' and 'evolution.' Applying this structure to the case studies of the following chapters enables an investigation into the ways scientific debates evolve.

Chapter three offers an original case study of ‘the synaptic vesicle debate,’ an ongoing dispute in the field of neurobiology. Neurobiologists disagree about the nature of ‘synaptic vesicles,’ which are tiny, spherical organelles located within neuron terminals, and are responsible for neurotransmission. Two major positions have been developed. They have changed in significant ways through this history.

Chapter four offers an original case study of ‘the theory-of-mind debate,’ an ongoing dispute in the field of developmental psychology. Psychological researchers disagree about how children of a certain age come to understand that others possess separate minds and perspectives. A large number of positions have been developed in the history of this debate, though only three have emerged as major points of the discussion.

In the fifth and final chapter, I apply the notion of the scientific debate tradition to the two case studies of the previous chapters. This facilitates an analysis of the ways these scientific debates have evolved over time. From the histories of these case studies I abstract a series of concepts regarding the modes of debate (or ‘modalities’) which a tradition can possess. Lastly, I consider the implications of the notion of the scientific debate tradition for relevant work in the philosophy of science.

Chapter 1: Philosophical Views of Scientific Practice

In this chapter, I review concepts from the philosophy of science relevant to the subject of this dissertation—the structures and dynamics of scientific debate. During the course of this chapter I review some background of the philosophy of science so these concepts can be understood in their historical context. In the chapters that follow, I appropriate these concepts for use in my own account.

During the earlier part of the twentieth century, the philosophy of science was dominated by the logical empiricist program. Logical empiricists developed ways to apply the mathematical logic of Russell, Frege, Wittgenstein, and others to the subject of scientific investigation. They attempted to analyze scientific procedure in terms of logical relations between the statements of scientific theory and observational findings. According to the logical empiricists, science builds and justifies its theories on the basis of purely observational claims, and an observational claim can be made independently from the context of any theory (e.g. Mach, 1886; Carnap, 1928; Carnap, Hahn, and Neurath, 1929; Ayer, 1936; Reichenbach, 1938; Carnap, 1939; Hempel and Oppenheim, 1948; Hempel, 1962; Hempel, 1965).

An example of a key influential accomplishment of logical empiricism is Carl G. Hempel's Deductive-Nomological (D-N) model (e.g. Hempel and Oppenheim, 1948; Hempel, 1962; Hempel, 1965). In this view, one investigates a scientific theory by first deducing a testable implication. If this logical implication is observed to occur, the theory receives confirmation. In Hempel's view, this same structure applies to scientific explanation. According to the D-N model, a scientific explanation of an event consists of the set of scientific laws and also the particular conditions from which a description of the event can be logically deduced.

Logical empiricism began to decline considerably around mid-century (van Fraassen, for example, uses the phrase “spectacular crash” to describe the fall of this program (1980, 2)). This resulted from a number of crippling internal difficulties that arose within its account (e.g. Quine, 1951; Goodman, 1955; Putnam, 1962; Achinstein, 1965). Also, rival accounts began to develop in the philosophy of science literature which, though not necessarily any more successful, were advanced without subscribing to many of logical empiricism’s central tenets (e.g. Toulmin, 1953; Hanson, 1958; Kuhn, 1962; Lakatos, 1970). Despite these difficulties, logical empiricism continues to be a constitutive context-setter and foil for current work in the philosophy of science.

One continuing approach in the philosophy of science is the 'probabilistic' perspective, including Bayesian approaches. With roots in logical empiricism, probabilists explain the association between theories and their supporting evidence in terms of relations of mathematical probability (e.g. Carnap, 1950; Hempel, 1965; Hesse, 1974; Franklin, 1990; Earman, 1992). Probabilism addresses a number of the problems encountered by logical empiricism. It also raises issues regarding scientists’ beliefs, degrees of confirmation and disconfirmation, and the nature of probability itself.

A major contemporary discussion in the philosophy of science is the debate over ‘scientific realism.’ Philosophers of science take a number of positions regarding whether theories in science should be taken to provide true accounts of the world (e.g. van Fraassen, 1980; Hacking, 1981a; Laudan, 1981a; Hacking, 1983; Leplin, 1984;

Churchland and Hooker, 1985; van Fraassen, 1989). A number of anti-realist accounts have been developed, each challenging the position that entities posited by scientific theories should be understood to actually exist. These debates raise questions concerning the goals of scientific research, the distinction between observable and unobservable entities, and the lessons offered by successes and failures in the history of science.

Feminist perspectives critique biases within the practice of science, and develop correctives (e.g. Harding, 1986; Tuana, 1989; Keller, 1993; Schiebinger, 1993; Longino, 1990; Keller and Longino, 1996; Haraway, 1997). Feminist projects include attempts to uncover discriminatory hiring practices in science and biases regarding how and what science investigates. Work from this perspective also considers how the content of scientific findings can be contaminated by gender biases and other social factors. The effects of the history of the scientific enterprise on conceptions of gender and on the lives of disadvantaged people are examined. Fundamental questions are raised regarding the nature of objectivity and scientific values.

Another contemporary area of research in the philosophy of science investigates the nature of scientific progress. An important jumping off point in these discussions is Thomas Kuhn's model of scientific revolutions, which controversially regards science as moving through a series of large-scale paradigm shifts (Kuhn, 1962). An influential line of continuing research involves critique and supplementation of this model (e.g. Lakatos, 1970; Laudan, 1977; Kitcher, 1993; Longino, 2002). Philosophers in this tradition investigate the relationships between scientific theories and the larger programs of research they belong to. They also discuss the conceptualization of science, or 'cognitive values,' that must be present for research to progress.

A further contemporary area of philosophy of science highlights the roles of experimentation and instrumentation in scientific work. Like those who study scientific progress, philosophers of this type look closely at the history and the practices of science. On this approach, the experimental practices and the material instruments involved in scientific research deserve philosophical reflection in their own right, related to—but not relegated beneath—traditional epistemological questions (e.g. Hacking, 1983: Franklin, 1986; Ihde, 1991; Galison, 1997).

In this chapter, I explore these last two trajectories: philosophical work on scientific progress and on experimentation and instrumentation.

In the last three decades, some of the most influential and provocative theorizing about the nature of science has emerged from fields outside of the philosophy of science, such as sociology, anthropology, women's studies, and cultural studies. Researchers in what is sometimes called 'science studies' explore science's role in society, the social aspects of scientific research, and the implications of science's inherently social character. With philosophical underpinnings in figures such as Kuhn, Foucault, Wittgenstein, and Marx, some work in this field focuses on the mechanisms of power and persuasion at work in the relationships between science and society. At times researchers in this field have advanced the (famous and notorious) claim that knowledge is wholly or partially 'socially constructed.'

Some work in science studies proceeds with detailed investigations of the practices of scientists within the laboratory (e.g. Knorr-Cetina, 1981; Pickering, 1984; Collins, 1985; Lynch, 1985; Latour, 1987). Other work focuses on the relationship between science, technology, and the institutions of the community (e.g. Shapin and

Shaffer, 1985; Haraway, 1997; Fuller, 1988). One major paradigm of the field is the 'strong programme,' or the Sociology of Scientific Knowledge (SSK). Members of this movement investigate the roles that the interests of scientists and others play in scientific practice and results (e.g. Bloor, 1976; Barnes, 1977; Collins and Pinch, 1993; Pickering, 1984; Shapin and Shaffer, 1985). Another major paradigm of science studies is Actor-Network-Theory (ANT). ANT theorists seek to identify all parts of the 'network' of 'actors,' be they human, object, instrument, institution, funding procedure, strategy of persuasion, or anything else that contribute to the processes through which a scientific claim becomes what the community considers a fact (e.g. Callon, 1980; Latour, 1987; Law and Hassard, 1999; Latour, 2005).

In section three of this chapter, I briefly review several concepts developed by science studies practitioners rearding the social processes by which a scientific research result becomes known to the larger scientific community. These concepts are useful for the study of scientific debate in following chapters.

The reaction of philosophers of science to the rise in the influence of science studies (though varying in degree from philosopher to philosopher) has been generally similar. While regarding highly the detail of the case studies conducted by these researchers, philosophers have severely criticized science studies' broad claims concerning the 'relative' status of truth, and argue that science studies misappropriates philosophical concepts such as 'underdetermination' and the 'theory-laden' nature of data. A virtual cottage industry of philosophers of science criticizing (to different degrees) the claims of science studies practitioners has emerged (e.g. Chalmers, 1990; Laudan, 1990; Kitcher, 1993; Bunge, 1999; Franklin, 1999; Hacking, 1999; Longino, 2002). I review the work of four contemporary philosophers in this chapter: Peter Galison, Ian Hacking, Don Ihde, and Larry Laudan. While at times taking issue with the claims of science studies, Galison, Hacking, and Ihde are often amenable to particular aspects of such work. Each are influential 'bridge figures' between the philosophy of science and science studies. Laudan, however, (as, in my view, the larger majority of philosophers of science) is harshly and consistently critical of science studies. This dissertation shares much with the general form of science studies research; I conduct extensive case studies of contemporary scientific work. I do not, however, intend the claims I make here to repeat the same sorts of moves that philosophers of science have found problematic.

In this chapter, I explore three specific areas of the research: philosophical views of scientific progress, philosophical views of experimentation and instrumentation, and work from the field of science studies regarding the transmission of research.

In the first section of this chapter, I review philosophical views of scientific progress, centrally focusing on Imre Lakatos's understanding of 'scientific research programmes.' Lakatos's philosophy is the main work with which I contrast my own claims in following chapters. In order to make Lakatos's claims clear, it is necessary to also review parts of Thomas Kuhn and Karl Popper's work, since Lakatos's claims function as a response to the work of these two philosophers. In this first section I also address three works related to Lakatos's view: Laudan's notion of 'research traditions,' Ian Hacking's reading of Lakatos, and Mara Beller's 'dialogical' history of research in quantum mechanics.

In the second section of this chapter, I review philosophical views of 'traditions' of scientific research regarding experimental techniques. I consider Peter Galison's work on the history of experimental conventions, and the interactions between those who work through different conventions. Don Ihde's work on the 'trajectories' of instrumentation is also reviewed.

The third section of this chapter reviews a number of concepts from the field of science studies. Science studies scholars develop concepts to describe the social processes through which a scientist's claims travel through the scientific community. Several of these concepts are useful in following chapters for thinking about how ongoing scientific debates, with their complex interactions between rival positions, change over time.

1. Philosophical Views of Scientific Progress: Lakatos and Kuhn

Philosophers struggle to conceptualize scientific progress.[1] The issue requires consideration of the strategies of scientific reasoning, and also the actual history of scientific research. The issue raises sociological questions: how does science advance in practice? It also raises normative ones: how should science advance? What should scientific progress mean? In this section, I review two influential philosophical accounts of scientific progress: those of Kuhn and Lakatos.

Imre Lakatos's notion of the 'scientific research programme' is specifically useful to this dissertation. Lakatos's position challenges the traditional and intuitive view that science progresses as it gets us closer and closer to some external truth. He instead

[1] Endnotes for this chapter are found on pages 63-67.

claims that scientific progress necessarily includes competition between rival collectives of researchers. Lakatos's claims regarding these issues will serve a jumping off points for the account of scientific debate I develop in later chapters.

To understand Lakatos's claims, it is necessary to first review the two projects to which his own centrally responds: Thomas Kuhn's notion of 'paradigm shifts,' and Karl Popper's notion of 'falsification.' In the *first subsection* below, I review the aspects of Kuhn's work necessary to properly contextualize Lakatos's project. The *second subsection* reviews the details of Lakatos's position. I also review critiques of his philosophy relevant to the account developed in this dissertation. This subsection includes five parts: Lakatos's view of Popper's notion of falsification, a review of Lakatos's notion of 'research programmes,' Lakatos's account of Bohr's research programme, Larry Laudan's notion of 'the research tradition,' and Ian Hacking's reading of Lakatos. In the *third subsection*, I articulate a general critique of Lakatos's philosophy relevant to the claims made in the next chapter: that his account fails to sufficiently address the level of interaction that occurs between rival positions in a scientific debate. In the *fourth and final subsection* of section one, I suggest that the project of this dissertation—the study of scientific debate—constitutes a next step in this line of philosophical thought.

1a. Kuhn's Paradigms

In *The Structure of Scientific Revolutions*, Kuhn claims that the advance from one large-scale scientific theory to the next is a critically important aspect of scientific practice (1962; I refer to the second edition, published in 1970). According to Kuhn, historians and philosophers of science have not understood the significance of the shifts between these large-scale theories. In this view, for example, philosophers have failed to understand the effects that the shift from Aristotle's to Galileo's physics, or Newtonian to Relativistic physics, has had on scientific practice.

The central unit in Kuhn's philosophy is his notion of the 'paradigm.' The paradigm of a particular discipline is its system of shared issues, methods, and meanings. Kuhn writes that a paradigm is "the source of the methods, problem-field, and standards of solution accepted by any mature scientific community at any given time" (Kuhn, 1970, 103). Scientists working within a paradigm practice what Kuhn calls 'normal science.' Those performing normal science engage in 'puzzle-solving' activities. Puzzles are the problems defined by the paradigm. The paradigm even dictates what the solutions to puzzles should be; the analogy is that one working on a jigsaw puzzle knows what picture should be revealed when it is complete.

Addressing the puzzles set forth by the paradigm does *not* call the paradigm itself into the question; puzzle-solving works within the rules and procedures of the paradigm to achieve the goals and aims the paradigm has provided. Kuhn writes, "In so far as he is engaged in normal science, the research worker is a solver of puzzles, not a tester of the paradigm" (1970, 144).

This view of normal-science-as-puzzle-solving is controversial because it casts science, in its most typical mode, as *not* engaged in a search for novelty. He says, "Perhaps the most striking feature of the normal research problems we have just encountered is how little they aim to produce major novelties, conceptual or phenomenal" (Kuhn, 1970, 35). According to Kuhn, things are going well for normal science when the puzzles of the paradigm can be addressed without uncovering *any* novelty (Kuhn, 1970, 52). Success occurs in normal science when these puzzles can be answered without encountering any surprises.

Kuhn claims that there is a great benefit to the extreme restriction and focus demanded by paradigm: these restrictions make success in science possible. He says, "By focusing attention upon a small range of relatively esoteric problems, the paradigm forces scientists to investigate some part of nature in a detail and depth that would otherwise be unimaginable" (1970, 24). In this view, there is no other way for scientists to achieve such a high level of fit between data and theory except through the normal science which occurs within a paradigm (Kuhn, 1970, 64-65).

Despite the scientific progress that normal science makes possible, Kuhn claims that the governing paradigm at times experiences a significant shift. He calls this shifting of paradigms a 'scientific revolution.'

A scientific revolution begins when the reigning paradigm moves into what Kuhn calls a state of 'crisis.' He claims that the typical work of science at times encounters anomalies. The very fact that a finding can be anomalous or surprising is because the paradigm leads scientists to expect specific things. The eventual encounter with anomalies is an ordinary aspect of scientific research (Kuhn, 1970, 79). However, sometimes these anomalies can grow in importance. He says that when "an anomaly comes to seem more than just another puzzle of normal science, the transition to crisis and to extraordinary science has begun" (Kuhn, 1970, 82). A state of crisis is a general atmosphere of unease with the paradigm due to the disheartening challenges posed by an anomaly.

The very problems being addressed, the very procedures for approaching those problems, and the very concepts used to order the world, may all need to be overhauled in order to address crisis-provoking anomalies. Kuhn explains,

> So long as the tools a paradigm supplies continue to prove capable of solving the problems it defines, science moves fastest and penetrates most deeply through confident employment of those tools. The reason is clear. As in manufacture so in science—retooling is an extravagance to be reserved for the occasion that demands it. The significance of crises is the indication they provide that an occasion for retooling has arrived (1970, 76).

As a crisis continues, the rules of the paradigm begin to blur and loosen. A crisis period becomes resolved in one of three ways: (1) the paradigm could turn out to be able to handle the anomaly, (2) the anomaly might persist despite major changes, and researchers might decide that the anomaly will simply remain as such for the time being, or (3) a rival paradigm may emerges with an ensuing battle over its acceptance, possibly resulting in revolution (1970, 84). This final possibility is the focus of Kuhn's investigation. However, crisis alone is not a sufficient condition for bringing about a scientific revolution; *an alternative paradigm* must emerge as a rival if the reigning paradigm is to undergo serious testing (1970, 145).

If an alternative paradigm does emerge as a contender, it is possible that the field of science may shift to this new paradigm. Scientific revolution occurs as this new paradigm replaces the old. Revolution brings change to the problems a science addresses, the concepts it uses, the procedures it employs, its standards of proof, and its very understanding of what exists and what does not.

Controversially, Kuhn claims that separate paradigms should be considered 'incommensurable,' acting in different worlds with different rules (Kuhn, 1970, 150). While some terms may stay the same through a revolution, the meanings of those terms before and after the revolution may be radically different. As an example, he reviews the paradigm shift from Classical to Relativistic physics. He says, "What had previously been meant by space was necessarily flat, homogeneous, isotropic, and unaffected by the presence of matter. If it had not been, Newtonian physics would not have worked. To make the transition to Einstein's universe, the whole conceptual web whose strands are space, time, matter, force, and so on, had to be shifted and laid down again on nature whole" (Kuhn, 1970, 149).

Because of these dramatic changes, the debate over whether a paradigm shift should occur is not a conventional scientific dispute; it does not regard issues of proof. It cannot. Competing paradigms cannot be contrasted in any normal way since they do not share any of the same measures (Kuhn, 1970, 148). Those defending rival paradigms effectively talk past one other (Kuhn, 1970, 170). The most persuasive argument that can be in favor of moving to a new paradigm, Kuhn explains, is that it provides the tools necessary to resolve the crisis of the waning paradigm (1970, 153). But the new paradigm's ability to address the crisis-provoking anomaly may not be very much better than that of the old paradigm. Also, it is unlikely that the new paradigm will be able to claim even a similar degree of success in solving many of the problems already addressed by the long-standing paradigm. Kuhn explains,

> But paradigm debates are not really about relative problem-solving ability, though for good reasons they are usually couched in those terms. Instead, the issue is which paradigm should in the future guide research on problems many of which neither competitor can claim to resolve

> completely. A decision between alternate ways of practicing science is called for, and in the circumstances that decision must be based less on past achievement than on future promise (1970, 157-158).

Scientists adopt the new paradigm because, after losing faith in the current paradigm due to the state of crisis, they put their faith in the potential for success offered by the new one.

Kuhn sees a sociological component to the way these conflicts are resolved. Researchers of an older generation, experienced with the methods and conceptual frameworks of the crisis paradigm, may choose to resist the revolution. Resistance, Kuhn claims, is as rational as the choice to shift (1970, 151). Kuhn offers examples, explaining that "Newton's work was not generally accepted, particularly on the Continent, for more than half a century after the *Principia* appeared. Priestly never accepted the oxygen theory, nor Lord Kelvin the electromagnetic theory, and so on" (Kuhn, 1970, 150-151). In contrast, the younger generation of scientists, raised within the crisis moment of the paradigm and less anchored to it by a lifetime of commitment, are more likely to be those who support the scientific revolution (1970, 144).

1b. Lakatos's Research Programmes

Ian Hacking writes of Imre Lakatos, "we have the not unfamiliar spectacle of a writer whose placings of himself are not always helpful. If we read his papers as they come we get a body of doctrine that is entertaining but collectively not very coherent" (Hacking, 1981, 128). Despite his efforts to create a coherent account of scientific progress, and to relate this account to the history of the philosophy of science, Lakatos's project is generally taken to be incomplete. What does exist is not generally taken to hang together as a fully consistent body of thought. In this section, I review just those parts of his work that will be useful to my work in later chapters. The segment of his work I address here, I believe, is itself coherent, provocative, and valuable. It is also flawed.

In his groundbreaking, monograph-length article entitled "Falsification and the Methodology of Scientific Research Programmes," Imre Lakatos develops his account of progress in science (1970). He presents his view of rational scientific progress, introduces his notion of the 'scientific research programme,' substantiates his account with a number of extended examples from the history of science, and situates his view within the history of the philosophy of science. Lakatos's notion of the scientific research programme will be the central concept from the history of philosophy of science my own account of scientific debate will be set against.

This subsection is divided into five (unnumbered) sub-subsections. The *first*, entitled 'Lakatos on Falsification,' outlines Lakatos's understanding of his own philosophy as an extension of the work of Karl Popper. I also review Kuhn's influence upon Lakatos, and Lakatos's defenses of Popper's claims against Kuhn's criticisms. The *second* sub-subsection, entitled 'The Features of Research Programmes,' reviews the details of Lakatos's notion of the scientific research programme. The *third*, entitled 'Lakatos's account of Bohr's research programme,' summarizes Lakatos's extended reinterpretation of the history of Niels Bohr's work on quantum physics. The *fourth*, entitled 'Laudan's Research Traditions,' reviews some of the features of Larry Laudan's notion of the 'research tradition,' which he develops as a replacement for both Kuhn's and Lakatos's concepts. In the *fifth* sub-subsection, entitled 'Hacking's reading of Lakatos,' I consider Ian Hacking's controversial interpretation of Lakatos's project.

Lakatos on Falsification

Lakatos consistently portrays himself as a Popperian; as student of Popper, Lakatos develops his own views as improvement on Popper's work, making central use of Popper's influential notion of 'falsification.' Popper sets his arguments against the traditional accounts in the philosophy of science which hold that scientific theories can ever be verified (or "justified" as Lakatos says), and also probablilist accounts. (In a broader context, Popper's view is a response to the verificationist theories of meaning characteristic of the Vienna Circle.) According to Popper, a scientific theory must include an account of what it would take to show the theory to be incorrect, i.e. falsified. In this view, science is engaged in the project of continuously testing theories in order to determine how well they stand up to technical criticism.

Lakatos differentiates several versions of falsification so he can clarify the notion for his own use, address misconceptions, and defend against criticisms. Lakatos discusses three versions: (1) what he calls *dogmatic falsification*, a common misconception, (2) *naïve methodological falsification*, an important advance, and the version Lakatos claims Kuhn mistakenly argues against, and (3) *sophisticated falsification*, the version Lakatos defends.

Lakatos begins his discussion of falsification by first attempting to dispel myths and misrepresentations. He claims that those who argue against the coherency of the notion of falsification often set up a straw man. To demonstrate the vulnerability of too simple a version of this notion, Lakatos reviews a concept he calls 'dogmatic falsification.' For the dogmatic falsificationist, science is a series of theoretical conjectures which, while they cannot ever be proven, can be shown to be false. This brand of falsificationist, Lakatos explains, holds that, "*counter*evidence is the *one and only* arbiter which may judge a theory" (1970, 96). In this view, science progresses as a parade of merely conjectural theories that are each overthrown by the hard facts which are discovered in the process of theory testing.

Lakatos claims that the 'dogmatic' version of falsification is ultimately unsound. It suffers from two fatal errors. First, the dogmatic falsificationist mistakenly takes there to be a clear demarcation between theory and data. Lakatos disagrees that independent facts of the world can be used to invalidate scientific theories in any simple way. Second, the dogmatic falsificationist mistakenly holds that observations about the world, separate from the theories in which they arise, can be validated independently from the practices of theorizing (Lakatos, 1970, 97-98). To illustrate these two problems for the domantic falsificationist, Lakatos provides the example of Galileo's claim that he had proven there to be mountains on the moon and spots on the sun, thus refuting the traditional theory that objects in the heavens (such as the moon and sun) are perfect spheres. Galileo's observations did not persuade those who held the perfect sphere theory of heavenly objects, since they did not recognize the validity of Galileo's procedure for producing those observations, i.e. his use of the telescope. They did not believe Galileo had falsified their theory since they did not agree with his own theories of optics and telescope use (Lakatos, 1970, 98).

In Lakatos's view, a superior version of falsification has been offered: 'naïve methodological falsification.' He claims that this is the version put forward by Duhem and Popper. For the naïve methodological falsificationist, it is not simply the case that all elements of all theories are set up to be tested by empirical observations. The scientist "makes unfalsifiable by *fiat* some (spatio-temporally) singular statements which are distinguishable by the fact that there exists at the time a 'relevant technique' such that 'anyone who has learned it' will be able to *decide* that the statement is 'acceptable'" (Lakatos, 1970, 106). Some elements of the theory are—for a time—not subject to falsification in order to make further investigation of a position possible. (This resonates with Kuhn's claim that normal science often continues despite recognized anomalies.) As we will see in Lakatos's account of Bohr's research programme (reviewed below), Bohr explicity ignored certain inconsistencies in his model of the atom to allow for further research. In the example above, Galileo temporarily ignores the criticisms of the telescope leveled by the perfect-sphere-theorists for the sake of continuing his research.

For Lakatos, there are two problems with the naïve methodological falsificationist view (which also apply to the dogmatic view). First, the naïve methodological falsificationist mistakenly holds that tests occur only between an experiment and a single theory. Lakatos claims that a test in science always occurs between an experiment and *rival* theories. He says, "tests are—at least—three-cornered fights between rival theories and experiment" (Lakatos, 1970, 115). Secondly, the naïve methodological falsificationist only sees the possibility for the falsification of a theory, rather than any sort of confirmation. In Lakatos's view, Kuhn's criticisms of falsification apply to the naïve methodological version. Recall that Kuhn claims that a scientific revolution can occur only when both the current paradigm is in a state of crisis, and an alternative paradigm has emerged. Kuhn critiques Popper on these grounds. He claims "falsification, though it surely occurs, does not happen with, or simply because of, the emergence of an anomaly or falsifying instance. Instead, it is a subsequent and separate process that might equally well be called verification since it consists in the triumph of a new paradigm over the old one" (Kuhn, 1970, 147). Lakatos develops his own version of falsification, one which he claims survives Kuhn's critique.

Lakatos's own version of falsification, one that he claims Popper also in some ways held, he calls 'sophisticated falsification.' For the sophisticated falsificationist, it is never a singular theory that is evaluated though scientific practice, but 'sets of theories' (Lakatos, 1970, 118). In this view, "to apply the term 'scientific' to a *single* theory is a category mistake" (Lakatos, 1970, 119). These 'sets of theories' are evaluated in terms of the new facts predicted by the advancing set, and also in terms of the new facts corroborated through the experiments that correspond to the set. Lakatos develops two concepts to articulate how a set of theories advances, and also how a set may fail to advance; sets of theories are either *progressive* or *degenerative*. A set of theories is 'theoretically progressive' when the new theories added to the expanding set predict new facts. The set is 'empirically progressive' if these predictions become corroborated. The set is instead 'theoretically degenerative' or 'empirically degenerative' if there is a failure to predict and corroborate new facts. Thus Lakatos challenges the intuition that a theory which does not predict the correct outcome ahead of time should be considered falsified and forever abandoned. That standard could result in the abandonment of perfectly good theories which require only small adjustments to become progressive.

The sophisticated falsificationist understands falsification to be a process in which one set of theories eventually replaces another. A set of theories may degenerate, offering only ad hoc explanations for new facts being discovered by others. However, the central claims of this set do not become falsified until it is replaced by a rival set which is itself progressing. He says, "*The idea of growth and the concept of empirical character are soldiered into one*" (Lakatos, 1970, 119). The naïve methodological falsificationist sees 'corroborated counterevidence' as a sufficient condition for falsifying a theory. The sophisticated falsificationist does not agree. According to Lakatos, a new set of theories should *not* be seen as more reliable simply because it is new and its rival has encountered counterevidence; falsification occurs to a degenerating set of theories only after it is replaced by a rival *progressin*g set. Lakatos believes this view addresses Kuhn's comments and surpasses them. In Lakatos's view, the notions of progress and degeneration are superior to Kuhn's account of the paradigm shift.

The Features of Research Programmes

Lakatos refines his account of the 'sets of theories' discussed above. He develops the concept of the *scientific research programme* to refer to a set of theories which can be together assessed as either progressing or degenerating. Research programmes, he explains, contain a set of rules (or heuristics). The also contain two sorts of theories. The *hard core* theories of the programme represent the unquestioned and unchanging aspect of the programme. The *auxiliary hypotheses*, or *protective belt*, are those which support the hard core, but may be altered or sacrificed as the programme advances.

The two categories of rules that govern the actions of the research programme are called the *negative heuristic* and the *positive heuristic* (Lakatos, 1970, 133). The 'negative heuristic' is the rule that any theory that contributes to the programme should not challenge the programme's hard core theses; a theory that challenges the hard core of a programme is by definition not contributing to that programme. Thus Lakatos has provided a way for a research programme to be eventually falsified; since the hard core theses of a programme cannot simply be adjusted, those working within that programme must abandon the programme if they are interested doing research which runs against its hard core.

The positive heuristic is a more flexible set of rules. It defines the ways research within the programme should be conducted. A research programme advances with the production of 'auxiliary hypotheses.' Unlike the hard core, the auxiliary hypotheses are expendable; they may be altered or sacrificed in the process of defending the hard core theses. Lakatos claims that the auxiliary hypotheses form a 'protective belt' around the hard core of the programme. He says, "It is this protective belt of auxiliary hypotheses which has to bear the brunt of tests and get adjusted and readjusted, or even completely replaced, to defend the thus-hardened core" (Lakatos, 1970, 133). A research programme becomes progressive if adjustments to auxiliary hypotheses predict new facts and/or corroborate predictions. Also, through ad hoc adjustments to the protective belt, anomalies can be dealt with. These adjustments can be small or large. A significant adjustment to the positive heuristic of a programme Lakatos calls a 'creative shift' (Lakatos, 1970, 137).

Lakatos gives the example of Newton's scientific research programme. The hard core of this programme consisted of Newton's three laws of motion and also his law of gravity (Lakatos, 1970, 133). According to Lakatos, Newton's programme was consistently theoretically progressive and intermittently empirically progressive. The progressive status of Newton's programme existed despite a number of anomalies, rather than in the absence of them. Lakatos notes, "Most, if not all, Newtonian 'puzzles', leading to a series of new variants superseding each other, were foreseeable at the time of Newton's first naïve model and no doubt Newton and his colleagues *did* foresee them: Newton must have been fully aware of the blatant falsity of his variants" (1970, 136). This emphasizes an important point; the progressing or degenerating status of a research programme is determined by how well it predicts and corroborates new facts, not how much trouble it has digesting problems. Lakatos says, "Our considerations show that the positive heuristic forges ahead with almost complete disregard of 'refutations': it may seem that it is the 'verifications' rather than the refutations which provide the contact points with reality" (1970, 137).

With the notions of progression and degeneration, Lakatos claims to alter the common understanding of scientific rationality. He claims that the logic of science works very slowly. He says, "rationality works much slower than most people tend to think, and, even then, fallibly" (Lakatos, 1970, 174). This is the case for a number of reasons. For example, Lakatos claims that assessments of a programme's status can only be reliably made in retrospect. One can only assess *after the fact* whether a programme had been truly degenerating or instead had simply been in a slump. One cannot be sure at the time whether or not a creative shift is waiting just around the corner to reinvigorate the programme.[2]

Also, Lakatos claims that evidence which appears to be conclusive and inconclusive today, may not appear the same way to others in the future. Some of the major experiments in the history of science which today are commonly considered to have settled controversies were not taken to be so conclusive in their own day. Lakatos notes that for example, "The anomalous behaviour of Mercury's perihelion was known for decades as one of the many yet unsolved difficulties in Newton's programme; but only the fact that Einstein's theory explained it better transformed a dull anomaly into a brilliant 'refutation' of Newton's programme" (Lakatos, 1970, 158-159). Lakatos claims that scientific research cannot be described in terms of "instant rationality"; it is only in retrospect that a programme can be seen to be clearly degenerating, and that major experiments can be interpreted to have led to the falsification of a scientific research programme.

An important and deeply embedded aspect of Lakatos's philosophy of science is his notion of 'rational reconstruction' (a term he uses differently than the logical positivists). Controversially, Lakatos recommends that research in the history of science be conducted in a way that demonstrates how the efforts of scientists have *rationally* led to our present understanding of the truth. He suggests that those conducting historical studies of science should not be beholden the actual details of history, but instead may feel free to 'reconstruct' how the events of history led 'rationally' to the facts. While much has been written on the Lakatos's notion of rational reconstruction, I do not focus on it here. I simply utilize the structures of scientific practice and progress that he has developed. Lakatos's account of the work of Neils Bohr reviewed below, for example, exemplifies his notion of rational reconstruction (see footnote 4).

Lakatos's account of Bohr's research programme

Many attempts have been made to use Lakatos's system to inform the historical study of science. Zahar's study of Lorentz, Lakatos and Zahar's study of Copernicus, Worrall's study of the optical revolution, Glass and Johnson's account of the rivalry between the 'orthodox' and 'Marxist' research programmes in economics, Gholson and Barker's analysis of the rivalry between the 'cognitive' and the 'conditioning' programmes in the psychology of learning, and Stuewer's account of Compton's research programme, all represent examples of historical studies explicitly based upon Lakatos's concepts (Zahar, 1973; Lakatos and Zahar, 1975; Stuewer, 1976; Gholson and Barker, 1985; Worrall, 1988; Glass and Johnson, 1988; Worrall, 1988).[3] Here I review Lakatos's own account of Neils Bohr's research programme from the history of research in quantum physics.

Lakatos begins his case study of Bohr's research programme with a note that his observations should be taken "not with a grain, but with tons, of salt" (1970, 140). If interpreted from a particular perspective, the movement of Bohr's research, from his model of the structure of the atom to the Copenhagen Interpretation of quantum theory, contains all of the central elements of Lakatos's account of scientific research programmes: hard core and auxiliary theories, a negative and positive heuristic, creative shifts, a period of degeneration complete with ad hoc adjustments, and eventually the emergence of a replacement research programme.

According to Lakatos, Bohr's research programme emerged within the context of a 'background problem,' a deep inconsistency that existed between two well-corroborated theories. On the one hand, Rutherford's theory claimed that atoms consisted of negative particles revolving around a positive center. On the other hand, the Maxwell-Lorentz theory of electromagnetism claimed that such a structure should collapse. Bohr initiates his programme by ignoring this inconsistency and pressing forward with a model of the Rutherford-type (Lakatos, 1970, 143). This unsolved inconsistency remained at the base of Bohr's programme. Lakatos claims that Bohr's notions of correspondence and complimentarity were ad hoc strategies designed to combat this inconsistency (1970, 142). However, Lakatos notes that the inconsistency did not pose a major problem so long as the programme was productive, which it famously and importantly was. He explains, "everyone was aware of it, only they ignored it—more or less—during the progressive phase of the programme" (Lakatos, 1970, 142). Lakatos takes this example to support his claim that programmes may proceed quite forcefully despite anomalies, so long as they are predicting and corroborating new facts.

Lakatos identifies the claims which make up the hard core of Bohr's research programme. One example is Bohr's claim that energy is not emitted continuously, but instead during moments of state change. Another example of a hard core claim of this programme is that ordinary laws of mechanics govern the atom while it remains in a stationary state, but other laws govern during moments of state change (Lakatos, 1970, 141). According to Lakatos, the positive heuristic of this programme stems from its guiding analogy; he says, “All this was planned right at the start: the idea that atoms are analogous to planetary systems adumbrated a long, difficult but optimistic programme and clearly indicated the policy of research” (1970, 146). For Bohr’s programme, first the hydrogen atom was to be worked out. Then elliptical orbits on a fixed plane would be worked out. Then artifacts of the model would need to be removed and more complicated atoms would need to be considered. Then the effect of electromagnetic fields would be studied.

The first version of Bohr’s model explained the Balmer/Paschen spectral lines of the hydrogen atom. As new experimental evidence emerged, Bohr advanced his model to account for that phenomenon and made successful predictions for further evidence. He even suggested correctives to the experimental designs (Lakatos, 1970, 148-149). Thus, according to Lakatos, Bohr’s programme was both empirically and theoretically progressive. With the help of other adherents to the programme, it experienced several progressive creative shifts in its history. Examples of progressive creative shifts to the positive heuristic include Summerfield’s introduction of relativistic mathematics into the system, and also the addition of Pauli’s exclusion principle (Lakatos, 1970, 153).

Though Lakatos praises Bohr’s programme highly in its progressive period, he pulls no punches in his critique of the Copenhagen Interpretation which followed. In Lakatos’s view, the Copenhagen Interpretation represents the degeneration of Bohr’s programme. He claims that the Copenhagen Interpetation no longer offered theoretical and empirical progression, but instead produced slippery, philosophically incoherent, and ad hoc positions. He says, “After 1925 Bohr and his associates introduced a new and unprecedented lowering of critical standards for scientific theories. This led to a defeat of reason within modern physics and to an anarchist cult of incomprehensible chaos" (Lakatos, 1970, 145) ('anarchist' is a technical term for Lakatos, referring to the claim that inconsistency is a somehow a basic property of nature). Lakatos claims that 'wave mechanics' represents the rival progressing scientific research programme which replaced Bohr's programme in the end.[4]

Laudan's Research Traditions

In reaction to Kuhn and Lakatos's accounts of scientific progress, Larry Laudan develops his own analysis of the macro-level structures of science. His notion of the 'research tradition' provides a useful contrast to Lakatos's position.

The primary articulation of Lauden's notion of the 'research tradition' appears in his 1977 work *Progress and Its Problems*.[5] He begins by distinguishing between two ways the term 'theory' is used in the history of philosophy of science. The term 'theory' refers to a specific hypothesis, axiom, or set of proposals used for making predictions during experimentation; this kind of theory may or may not be supported by the experimental data. According to Laudan, the term 'theory' is also used to refer to large-scale, general sets of doctrines, such 'evolutionary theory' or 'atomic theory.' Laudan says, "In each of these cases, we are referring not to a single theory, but to a wide spectrum of individual theories" (1977, 71). In Laudan's view, Kuhn and Lakatos both appreciate the difference between these two classes of theory. The advantage of their accounts is that they can address scientific progress in terms of the movement of these more ‘global’ theories.

Laudan sees Lakatos’s account of science as an improvement on Kuhn’s in many respects: (1) Lakatos’s account allows for coexisting programmes, rather than a linear progression of paradigms, (2) Lakatos observes competition between programmes, rather taking them to be incommensurable, and (3) Lakatos attempts (with, for example, his notion of the protective belt) to decipher the relationship between global theories and their component theories.

Despite this praise, Laudan offers a list of important criticisms of Lakatos’s account. I review here only those criticisms which are relevant for my discussion of scientific debate in the next chapter. One criticism Laudan makes of Lakatos’s account (and Kuhn’s as well) is that it considers scientific progress to occur only in terms of an increase in empirical content (Laudan, 1977, 77). According to Laudan, progress can also occur ‘conceptually.’ He claims that some instances in the history of science in which a theory suddenly became widely accepted are better explained as a conceptal advance, rather than an empirical advance; Lakatos’s slow-going, retrospective view cannot account for these instances.[6] Laudan also claims that Lakatos’s definition of the theories that make up a research programme is too restrictive. Laudan disagrees with Lakatos’s claim that a new theory belongs to a programme *only* if it entails the previous theories of that programme. Laudan instead claims that, “As we shall see shortly, in the vast majority of cases, the succession of specific theories within a maxi-theory involves the *elimination* as well as the addition of assumptions, and there are rarely successive theories which entail their predecessors” (1977, 77). Also, Laudan claims that Lakatos is too restrictive in his claim that the hard core of a research programme is not open to any fundamental changes over time.

To adjust for these and other difficulties within Lakatos's account, Laudan offers a replacement concept to describe the global structures through which scientific progress happens: *the research tradition*. A research tradition provides scientists a general outline for research concerning both the sorts of entities that exist and the general methods which researchers should use. He says, "Put simply, *a research tradition is thus a set of ontological and methodological 'do's' and 'don'ts'*" (Laudan, 1977, 80). A research tradition consists of many different and possibly incompatible theories. But these theories investigate similar entities with similar research methods. Laudan's research traditions are unlike Lakatosian research programmes in two respects: (1) a research tradition's 'core' is composed of a set of general ontological and methodological rules, rather than a research programme's set of hard core claims, and (2) the specific theories which compose a research tradition are various and perhaps contradictory, rather than forming a linear progression. Laudan summarizes, "By contrast, *research traditions are neither explanatory, nor predictive, nor directly testable*. Their very generality, as well as their normative elements, precludes them from leading to detailed accounts of specific natural processes" (1977, 81-81).

Another important aspect of research traditions is Laudan's account of how they change over time. One way a tradition changes is when its particular theories advance and adjust. The second way in which traditions change is by its very core elements undergoing alterations. The fact that Laudan sees any room for change in the core of a tradition indicates another important point of contrast between his philosophy and

Lakatos's. Laudan explains, "Since Lakatos argues, we define a research tradition or research programme in terms of its central doctrines (doctrines which Lakatos argues we make true by fiat or convention), any change in those central tenets is *de facto* the abandonment of the research tradition which was defined as the set of those tenets" (1977, 97). But Laudan claims that while Lakatos's view has the advantage of offering a potentially clear and simple way for identifying research programmes, this aspect of his account does not correspond well to the actual history of science. Laudan's project continues with the consideration of ways that the core aspects of a research tradition may evolve.

Hacking's reading of Lakatos

In his article, "Lakatos's Philosophy of Science," Ian Hacking offers a controversial and influential interpretation of Lakatos's central project (1981b). Hacking begins by claiming that though Lakatos himself often attempts to explain the nature of his central project, his attempts do not leave the reader with a clear or consistent vision. Because of Lakatos's tragic, early death, contemporary philosophers of science are left to speculate about what his rich and insightful, though enigmatically inconsistent, work could have had as its primary aim. Hacking disagrees with the standard reading that Lakatos was simply attempting to render consistent the philosophies of Popper and Kuhn, with the ultimate goal of providing a historically sensitive understanding of scientific rationality. According to Hacking, Lakatos's project was much more radical.

Hacking portrays Lakatos as a person from two very different philosophical worlds. In his view, Lakatos attempts to bring these two worlds together. They are: (1) the general world of Anglo-American philosophy of science, and in particular the debate between Popper and Kuhn, and (2) the Hegelian world of philosophy which concerned the thinkers of Lakatos's Hungarian origin. Hacking explains,

> The common English-speaking attitude is that knowledge is growing just if we are getting more at the truth. It is not just that some of us define knowledge as justified true belief, but that the truth is conceived of as fixed, while knowledge is to be defined as that which gets at this pre-existing truth. Hence in English philosophy knowledge is to be characterized externally, in terms of how well it represents reality. That is exactly what Lakatos is not primarily concerned with (1981b, 130).

Hacking takes Lakatos's central project to be to turn the traditional English-speaking attitude toward scientific discovery on its ear. He claims that Lakatos's aim is to produce a conception of rational scientific pursuit which is not based upon a representational theory of truth (Hacking, 1981b, 129).

According to Hacking, Lakatos accomplishes this by basing his account of scientific rationality on a sophisticated notion of *growth*. Hacking says, "Lakatos tried to make the growth of knowledge a surrogate for a representational theory of truth" (1981b, 141). In this view, the notions of 'research programmes,' 'progressive,' 'degenerative,' 'hard cores,' etc., are all part of Lakatos's attempt to make the notion of a representational theory of truth unnecessary. Rather than evaluate a scientific claim on the basis of its correspondence to reality, Lakatos claims that a set of claims should be evaluated in terms of their contribution to the growth of knowledge.

Hacking claims that Lakatos shares similarities to some philosophers that are not so central to the English philosophy of science tradition. While above I characterized

Lakatos's work as falling within a tradition that includes Popper, Kuhn, and Laudan, Hacking claims that Lakatos's challenge to the representational theory of truth places him in another tradition which includes Friedrich Nietzsche and C. S. Peirce (1981b, 131). These philosophers also attempt to replace a representational view of truth with a notion of methodology.

Hacking completes his interpretation of Lakatos's project by relaying one worry which arises for both Lakatos and Peirce. Hacking explains that the possibility of large-scale changes to a discipline of science (such as paradigm shifts, or 'cataclysms,' as Peirce would say) puts in jeopardy the consistency of Lakatos's account of scientific reasoning; if knowledge does not consistently grow, but instead grows for a period and then undergoes a radical and incommensurable shift, then Lakatos's understanding of reason applies only to specific moments of science, not to scientific rationality generally. Both Lakatos and Peirce have the same response to this worry: they deny the possibility of paradigm shifts. Hacking himself does not see Kuhn's work as posing this problem for Lakatos, but he does worry that large scale 'style' shifts may occur in the history of science in which new sorts of data and conceptions of objectivity emerge (1981b, 142).[7]

1c. The Debate Critique of Lakatos

Like Popper, Lakatos takes criticism to be central to the practice of science. Lakatos retains the centrality of criticism in his account by emphasizing the importance of rivalry between research programmes. Like Kuhn, he takes a theory to be falsified only if an alternative theory exists. Contra Kuhn, he stresses that rival research programmes co-exist. But what does it mean for rival programmes to co-exist? What is entailed in the rivalry between programmes?

I claim that Lakatos's account underestimates the level of interaction that exists between rival programmes. This underestimation has an important consequence; Lakatos fails to place sufficient attention on the potential for rival research programmes to have effects on one another. I refer to this general criticism as *the debate critique of Lakatos*. It opens up space for the work of this dissertation; in the following chapters I show that rival scientific positions relate to one another not only through relative progression and degeneration, but through complex conventions of debate.

In this subsection, I review three examples that substantiate the debate critique of Lakatos. *Example #1* regards A. F. Chalmers's observations regarding a debate between programmes in the history of scientific research on electricity. *Example #2* regards the work of Mara Beller. She analyzes the debates between quantum physicists and provides an interesting contrast to Lakatos's account of Bohr's research programme reviewed above. With *Example #3,* I briefly consider the tradition of research regarding non-locality in physics emerging from the debate between Einstein, Bohr, and others over the reality of concepts in quantum physics.

Example #1: Chalmers's remarks on electricity research

In an introductory text to the history of the philosophy of science, *What is This Thing Called Science?*, A. F. Chalmers provides an original example of rival Lakatosian research programmes (1978). In the process, he anticipates what I refer to above as 'the debate critique of Lakatos.'

In his analysis of the history of theories of electricity, Chalmers identifies two rival programmes: the first he dubs, 'the action at a distance theory,' and the second Faraday's 'field theory' (1978, 82). The action-at-a-distance theory took electricity to be a substance in the form of a liquid or particles which exists within electrically charged objects. In this view, electrically charged objects are able to act a distance upon one another across empty spaces. Chalmers explains, "Before Faraday's successes it was the action at a distance theory that was progressive. It led to the discovery of the ability of a Leyden jar to store electricity and to Cavendish's discovery of the inverse square law of attraction or repulsion between charged bodies" (1978, 82). But the field theory, which conceptualized electricity as occurring within the area that surrounds an electrified object, emerged and became the dominant progressing paradigm. Faraday's long list of accomplishments, including the discovery of electromagnetic induction, the electric motor, the dynamo, and the transformer, and then Hertz's production of radio waves (which was predicted by the field theory), all represent accomplishments contributing to the progressive status of this research programme. However, the concept of the electron, which emerged from the action-at-a-distance programme, continued to keep this rivalry active, retaining this programme's status as progressive despite the strong progression of the field theory. According to Chalmers, the elements of *both* of these programmes constitute the contemporary conception of electricity.

Chalmers remarks that, "Incidentally, the interaction between the two programmes, and the fact that the classical electromagnetic theory emerged as a reconciliation of the two programmes, inheriting the fields from one and the electron from the other, suggests that research programmes are not as autonomous as the Lakatos account suggests" (1978, 83). I suggest that this stray observation represents an example of the debate critique of Lakatos. In Chalmers's example, scientific progress results not simply from the progression of one programme with respect to another, but through the interaction between rivals.

Example #2: Beller's dialogical account of quantum physics

In her work *Quantum Dialogue: The Making of a Revolution*, Mara Beller develops an account of the debates which resulted in the formulation of the various versions of quantum mechanics (1999).[8] Beller refers to her style of analysis as 'the dialogical approach.' Shunning the use of overarching conceptual structures such as paradigms or programmes, she focuses on the ongoing technical debates between researchers. She explains, "It gradually becomes clear to me that the need for ongoing revision has a fundamental historiographical cause. This cause is intimately connected with the complex dialogical nature of thought and with the strategies used to flatten it into linear monological narratives" (Beller, 1999, 3). Beller proceeds by investigating the ways that entrenched positions and ongoing debates constituted research in quantum theory. She continues by exploring the rhetorical strategies used to establish the Copenhagen Interpretation as the dominant theory.

Beller conducts her 'dialogical' investigations by reanalyzing some of the most influential and notoriously confusing texts from the history of quantum physics research.

She re-sets these works in terms of the debates in which they were written. Rather than assuming that a text offers (or attempts to offer) a coherent theory, Beller shows that texts contain specific responses to specific interlocutors with which the author of the text has been in debate. One central example of Beller's analysis is Niels Bohr's famously influential (and famously difficult) 'Como lecture,' in which he first introduces his notion of 'complementarity' (the idea that at the quantum level, pairs of variables can only be measured each with a loss of definition of the other) (Bohr, 1927). The standard interpretation of this paper includes many assumptions, such as a similarity between Bohr's and Heisenberg's positions at the time, and that the central aim of the paper was to address the issue of wave-particle duality with the notion of complementarity (Beller, 1999, 117-118).[9] Beller instead claims that the Como lecture contains a number of responses to interlocutors and critiques of Bohr's rivals. She says, "Without identifying the interlocutors of each sentence of the Como lecture, it is impossible to understand the meaning of these sentences and the connections among them. Yet when we realize that the text is filled with implicit arguments with the leading physicists of the time—Einstein, Heisenberg, Schrödinger, Compton, Born, Dirac, Pauli, and the lesser known Campbell—the fog lifts and Bohr's presentation becomes clear" (Beller, 1999, 120). In Beller's view, the Como lecture demonstrates that Bohr's own claims are specifically directed to doubters of the central tenets of his theories (e.g. Einstein), with those offering rival theories (e.g. Schrödinger), and with those working within his own theories (e.g. Heisenberg).

Beller's dialogical account of Bohr's Como lecture diverges from the standard interpretation in several respects. For example, rather than understanding Heisenberg and

Bohr as ever coming to agreement about central features of the Copenhagen Interpretation (as they had publicly claimed to have done many times), Beller claims that interviews and subtle readings of their texts reveal otherwise. She explains, "According to the usual accounts, soon after their heated arguments over the uncertainty paper, Bohr and Heisenberg reached complete agreement. Yet genuine unanimity of opinion between the two men never really occurred" (Beller, 1999, 143). She claims that several aspects of the Como lecture can be better understood within the context of their ongoing disagreements. Another example of a divergence from the standard interpretation of the Como lecture is Beller's reading of Bohr's notion of complementarity. She claims that as the notion was first presented in the Como lecture, Bohr did not cast the duality between waves and particles symmetrically. According to Beller, in this lecture Bohr instead continues to rely heavily on a notion of wave packets. She claims that this aspect of the Como lecture is a reaction to specific debates with Einstein and others.

Beller develops dialogical interpretations of several major papers in the history of quantum physics research, including Heisenberg's uncertainty paper and Bohr's response to the EPR paradox, claiming each to be better elucidated when cast in terms of the specific debates in which each writer was engaged. She continues her analysis by investigating the rhetorical strategies which led to a common conception of the dominance and inevitability of the Copenhagen Interpretation (Beller, 1999).

Beller's interpretation of Bohr's work differs in important ways from Lakatos's account of Bohr's programme reviewed above. Beller's stress on the debates which direct Bohr's work casts a different picture from Lakatos's view of Bohr's research programme. These two accounts differ importantly in terms of their attention to the interaction between rivals. Lakatos does identify some level of interaction between rivals; he claims that research programmes, such as Bohr's, derive their 'progressive' or 'degenerative' status in relation to the progression of rivals. Beller sees rival scientific work as much more interrelated. For Lakatos, Bohr's programme is a series of claims that develop independently from rivals, yet progress in comparison to them. In Beller's view, for even a single paper to make sense, it must be read in terms of the technical debates between many positions. My own analysis of scientific debate in the next chapter is more like Beller's in this respect. My analysis draws out the conventions and consequences of the deep and technical interconnections between rival programmes.

Example #3: the non-locality tradition in quantum physics

As a third example, I briefly point to a tradition of research in quantum physics which has emerged from the debate between Einstein and Bohr over the nature of quantum events. Einstein was displeased with the randomness and indeterminacy at the heart of quantum physics. Together with his colleagues Boris Podolsky and Nathan Rosen, he designed a thought experiment to reveal that work in quantum theory was incomplete (1935). Often called the 'EPR experiment' (or EPR paradox), referring to the initials of its creators, Einstein and his colleagues ask readers to imagine two particles (electrons or photons in different versions) whose fates are tied together. As physicist Leon Lederman explains, "There are ways of creating a pair of particles flying apart from each other so that if one spins up the other must spin down, or if one spins right the other must go left" (Lederman, 1993, 183). In this scenario, if the spin of one were to be known, the spin of the other, no matter how far it had traveled, would be able to be immediately deduced. Einstein and his colleagues claim that the way these particles communicate so quickly (i.e. with superluminal speed) cannot be understood by quantum physics (Herbert, 1985, chap. 11; Lederman, 1993, chap. 5). This moment in the debate between Einstein and the proponents of the Copenhagen Interpretation has produced a variety of effects upon the history of research in physics.

One effect is the change in Bohr's position that resulted from the EPR debate. As Beller explains, "The EPR challenge forced Bohr to make basic changes to his philosophy of complimentarity, undermining the notion of disturbance on which his pre-1935 philosophy was built" (1999. 155). In Beller's view, Bohr and his colleagues (e.g. Born, Heisenberg, Kramer) continued to work hard to retain the complementarity principle as a fixture of the Copenhagen Interpretation. Psychologizing Bohr, Beller speculates, "His heuristics, which he internalized because of a long and successful use by himself and others, eventually became the most essential feature of all possible research programs for Bohr" (1999, 165). Insofar as the Copenhagen Interpretation goes on to become the reigning interpretation of quantum physics, the effects the EPR debate have had lasting effect.

Another example of an effect of the EPR debate is a tradition of research that has emerged from Einstein's ideas. The EPR thought experiment inspired David Bohm's interpretation of quantum theory, and John Stewart Bell's groundbreaking experiments on non-locality.

While a professor at Princeton, Bohm penned his *Quantum Theory* as an introductory text for the field (1951). He attempted to provide an account consistent with the Copenhagen Interpretation (to which, as a student of Oppenheimer, he subscribed to at the time). In order to keep his account as intuitive as possible, he also attempted to keep it as close as he could to classical concepts. While Bohm includes a refutation of the EPR paradox in his textbook, he also sets the seeds of his own classically-based interpretation of quantum mechanics (Bohm, 1951, chap 22, sec 15). He goes on in following work to develop an account of quantum theory which understands both particles and waves to exist independent of observation. Basing his theory on de Broglie's concept of the 'pilot wave,' he claims that only particles are directly observable. His description of waves offers a consistent explanation of the EPR paradox; in Bohm's account, waves undergo large-scale changes at superluminal speeds. His account remains deterministic by relying on the existence of hidden, unobservable variables, unlike the Copenhagen Interpretation (Bohm, 1952; Bohm and Hiley, 1993).

Building on Bohm's theoretical framework, a continuing tradition of research has developed in which experiments are used to test the implications of the EPR paradox. Lederman explains, "Thanks to a theorem developed in 1964 by a particle theorist named John Bell, it became clear that a modified form of the EPR thought experiment could actually be done in the lab" (1993, 187). These experiments show that the strange effect of superluminal communication between separated particles does in fact occur (e.g. Bell, 1964; Bell, 1966; Aspect et al., 1982). Science writer Nick Herbert remarks, "Bell's theorem shows that the faster-than-light character of Bohm's pilot wave is no accident. Without faster-than-light connections, an ordinary object model of reality simply cannot explain the facts" (1985, 51). Physicists continue to refine and expand the experiments which stem from this trajectory of theory and experimentation.

The three examples of this subsection reveal that there is a high level of interaction between the rival positions of a scientific debate. These interactions have effects on the positions of the debate and on the progress of the total body of research. Beller's analysis emphasizes ways in which a scientist's work is deeply embedded within the debates in which he or she is involved. Chalmers's and my examples show that bodies of research have developed from the rivalries between scientific research programmes.

1d. The Importance of a Whole-Debate Perspective

The review of Lakatos, Kuhn, and the others follows a line of thought in the field of the philosophy of science. The work of this dissertation represents the next step in this line of thought. In what follows, I focus on the changing interactions that take place between scientists in debate. This focus builds on the developing account of the importance of criticism and historical change seen in the works of Popper, Kuhn, and Lakatos.

The line of thought begins with Popper's concept of falsification. The falsificationist view provides a central place for criticism in science. In this view, science progresses as theories are tested and refuted. Appealing to the history of science, Kuhn reinterprets this notion. According to Kuhn, a theory is falsified only when it is replaced by another theory (in Kuhn's view, through scientific revolution). Lakatos develops this line of thought further, demonstrating that progress occurs in science through the rivalry between theories. Where Kuhn understands progress to take place as a new theory supersedes a former, Lakatos understands progress to take place as coexisting theories move forward with respect to one another. Lakatos emphasizes the central importance of rivalry for the advancement of science.

In what follows, I take this line of thought further by investigating the structures and dynamics of rivalries in science. I attempt to articulate the forms that these rivalries take, and consider the ways these forms change. Where Lakatos conceives of the relationships between theories only in terms of their relative progression, I consider the specific shapes and characters these relationships take. In so doing, my analysis builds from and surpasses Lakatos's account in a particular respect; I focus on the criticism, change, and progress which occurs as the relationships between rival theories transform.

I claim that taking the next step along this line of thought requires adopting what I call a *whole-debate perspective*. A whole-debate perspective is a view of scientific debate which includes its various rival theories and their changing relationships with one another. From this perspective, scientific progress is investigated not only in terms of the relative progress of particular theories, but in terms of the changes that occur to the discussion between these theories.

2. Philosophical Views of Experimentation and Instrumentation: Ihde and Galison

While the bulk of work done in the philosophy of science concentrates predominantly on theory, several philosophers work to articulate the roles of experimentation and instrumentation in science. Robert Ackermann, Davis Baird, Allan Franklin, Peter Galison, Patrick Heelan, Ian Hacking, Don Ihde, Nicholas Rasmussen and others shift the focus of traditional philosophy of science (e.g. Ackermann, 1985; Baird, 2000; Baird, 2001; Franklin, 1986; Franklin, 1990; Galison, 1987; Galison, 1997; Ihde, 1991; Ihde, 1998; Heelan, 1983b; Hacking, 1983; Rasmussen, 1997). These authors lament the way that historians and philosophers of science primarily portray science as a progression of theories. In their view, the significance of the material instantiation of science is undervalued. The work of these authors reveals technical philosophical issues regarding experimental techniques and laboratory technologies. For example, Ackermann, Franklin, and Galison delineate a philosophy of experimentation, analyzing the constraints under which experimentation occurs (e.g. Ackerman, 1985; Franklin, 1990; Galison, 1987). Heelan and Ihde articulate the practices of data interpretation that occur in the laboratory (e.g. Heelan, 1983a; Heelan, 1983b; Ihde, 1998).[10] Hacking's analysis of the nature of instrumentation has become a central reference point in the realism/anti-realism debate (e.g. Hacking, 1981a; Hacking, 1982; Hacking, 1983).

The particular topic from this area of research relevant to this dissertation regards the conventions of scientific practice which concern instrumentation. The two concepts I review are Ihde's notion of 'instrumental trajectories,' and Galison's notion of 'experimental traditions.' These notions offer tools for articulating how science advances through particular collectives of people who possess specific relationships to their instruments.

2a. Ihde's Instrumental Trajectories

In his work *Instrumental Realism*, Don Ihde develops an account of the history of scientific instrumentation at odds with the typical view (1991). He claims, "The dominant view of science held by most philosophers would hold that the primary trajectories of a science-technology relation are those which can be circumscribed by the overall idea of a *science-driven technology*. This occurs at the simplest level in the notion of 'pure' science eventually producing some 'applied' effect. I began this primer by questioning that set of priorities. By now it should be clear that there is another direction of effect; there is also a *technology-driven science*" (Ihde, 1991, 136). According to Ihde, the technologies of field of science, such as laboratory instruments, influence the directions its research takes. These technologies play a role in determining what questions are asked and what kind of answers can be found.[11]

Ihde follows this line of thought further, stating, "It is what I call the inclination of a trajectory. Such inclinations are related to the capacities of a technological possibility leading to the productive capacities of an experimental science" (Ihde, 1991, 137). The instrumentation of a field of science does more than simply constrain what work *can* be done; it participates in guiding what work *will* be done. Ihde calls this an 'instrumental trajectory.' He says, "Magnification 'suggests' more magnification; resolution more resolution, until eventually, we reach not only the historic refinements of microscopes and telescopes, but their contemporary variants which also present whole-image results isomorphic with ordinary vision—NMR, sonograms, etc. This is following a technological trajectory with its fascination" (Ihde, 1991, 138). The particular paths taken by a field of science are influenced by the way that its particular instrumentation can be further advanced.

Ihde investigates many ways that science has been influenced by the materials involved in scientific practice, outlining various instrumental trajectories. In what may be his most ambitious project, he traces out the history of imaging technologies through the history of Western science (e.g. Ihde, 1998; Ihde, 2000; Ihde, 2003; Ihde, 2007). His most sustained investigation into this topic is his current project, *Imaging Technologies: Plato upside down* (Ihde, in preparation).

Imaging technologies alter our bodily relationship with the world. They make it possible to visualize objects that are otherwise imperceptible with the naked-eye alone. These technologies have been indelibly important to scientific research for much of its history. In Ihde's rendition of the history of imaging technology in science, several styles of imaging are identified. He recognizes three different imaging styles: what he calls 'isomorphic imaging,' 'non-isomorphic imaging,' and 'constructive imaging.' Ihde charts instrumental trajectories regarding these three styles of imaging.

The first category of imaging technologies Ihde identifies regards pictures that resemble real-world phenomena. He refers to these techniques as *isomorphic imaging*. Images produced by techniques of this category gather much of their persuasive power from their ability to produce an 'aha' response in an observer who recognizes the content to appear similar to the real world object. Roughly speaking, the greater the isomorphism, the greater the capacity an image has to produce the 'aha' response. It is important to understand that through the process of making visible what is otherwise invisible, imaging technologies necessarily *transform* the object of study. Though the images produced by isomorphic imaging techniques contain content which looks similar to the object of study, Ihde claims that training is often required for one to see that content. A scientist must learn the skills necessary to properly interpret what is happening in a picture generated by a laboratory technology. For example, an astronomer may require training to be able to see the resemblance between a shadowy two-dimensional image gathered by a satellite, and the mountain range on another planet which is revealed.

The central example of an isomorphic imaging device Ihde explores is the camera obscura, a container within which light enters through a small hole. This projects a picture of the outside world on the opposite inside wall. Despite producing an image which bears an isomorphic relation with the outside world, many transformations occur to the content of a camera obscura image. For example, the image is inverted. Also, the real-world objects outside are flattened into a two-dimensional projection. Despite these transformations, users can easily be taught to understand the projected image as immediately isomorphic with the real-world object being imaged.

Ihde claims that the device has played a central role in early modern Western philosophical thought, providing the principal metaphor in discussions of perception and the human mind. It was utilized by figures such as Leonardo Da Vinci, René Descartes, and John Locke. The device continues to play a pivotal role in the history of science. Ihde claims that the camera obscura provides the foundation of a general ‘isomorphic’ instrumental trajectory that continues today. The next step of this instrumental trajectory includes the lens-based technologies of microscopes and telescopes, and photography.[12] This instrumental trajectory also has two branches, a ‘non-isomorphic’ branch and ‘constructive’ imaging branch.

The second style of imaging techniques Ihde calls *non-isomorphic imaging*. Rather than producing an image which bears a resemblance to the object of study, this style of imaging produces images that have a text-like character. He says, "These non-isomorphic images are not picture-like, but they are material indicators of meanings" (Ihde, in preparation, chapter 5). The experience of interpreting non-isomorphic images is analogous to the experience of reading. Like understanding written language, these images require one to possess specific hermeneutic skills in order to be interpreted. One example is spectroscopy. This technology produces spectral lines that reveal the elemental composition of the object under study. These lines of light, which Ihde refers to as 'Nature's bar codes,' are physical objects that convey information about the material being investigated. In Ihde's view, spectroscopy is an iteration of the instrumental trajectory which began with the camera obscura. Other examples of the non-isomorphic imaging style include the logic machines of microphysics, and DNA sequencing endeavors in genetics.

Ihde calls the third style of imaging techniques *constructive imaging*. This category of imaging technique refers to the creation of interpretable images from otherwise non-visible data. An example is devices that make it possible to visualize the wavelengths of light that occur beyond the visible spectrum, such as NASA images of nebulae in outer space. In these nebulae images, false colors are used to represent the otherwise invisible wavelengths. Though these objects are invisible, visible images are 'constructed' from false colors.

According to Ihde, rather than taking us beyond bodily vision as some have claimed, all of the imaging technologies reviewed necessarily share an embodied character. Ihde claims, "Embodiment is an anthropological constant in the sense that if images are to be perceived, 'read', interpreted, they must have the shape and range of visibility (or be hearable, touchable, etc.) which matches our capacities" (in preparation, chapter 10). Through each of these three styles of imaging produces very different sorts of readouts, all produce a material output which we experience through naked-eye vision, and which require at least some degree of interpretation.

In this view, all the examples above are moments along an instrumental trajectory that has evolved from an origin in the camera obscura.

2b. Galison's Experimental Traditions

In an extensive (800+ page) study of the history of experimentation in the field of microphysics in the last century entitled *Image and Logic*, Peter Galison develops several influential concepts to help understand the movement of the institutions of science (1997). His analysis of the 'traditions' of experimental techniques, and the limited ways in which these traditions have interacted, yields not only an influential history of recent physics, but also a useful account of scientific practice in general.

The core of Galison's account of the history of microphysics is his distinction between two large-scale traditions of experimentation. Galison develops the notion of 'experimental traditions' to conceptualize the conventions of these two communities of researchers. 'Experimental traditions' differ from one another along three general axes, or 'continuities' as he calls them: pedagogical, technical, and demonstrative (Galison, 1997, 21-22). Galison uses the term *pedagogical continuity* to refer to the fact that in each tradition, a history of teacher/student relations occurs with regard to the experimental techniques. He uses the term *technical continuity* to refer to the scientists' conventions regarding the use of particular instrumentation. The two traditions in the history of microphysics that Galison identifies are distinguished by their daily interactions with different complex technologies. According to Galison, crossing the lines between traditions is difficult because the expert knowledge of each tradition is deeply rooted in experience with different technologies. Galison calls the third aspect of an experimental tradition the *demonstrative (or epistemic) continuity*. This refers to the fact that a scientific community is deeply defined by the particular way it argues its points. The two major traditions in microphysics that Galison identifies have separate demonstrative, technical, and pedagogical continuities.

Image and Logic consists of Galison's extensive account of the history of microphysics. He claims that the field is constituted by two major experimental traditions: the *image tradition* and the *logic tradition*. The image tradition includes a history of experimental techniques such as the cloud chamber, emulsion physics, and the bubble chamber. Galison claims that the central demonstrative continuity of this tradition is the construction of images of 'golden events.' A golden event is "an individual instance so complete, so well defined, so 'manifestly' free of distortion and background that no further data had to be invoked" (Galison, 1997, 23). According to Galison, researchers of this tradition argue their scientific points by creating and interpreting images of golden events.

The other major tradition in the history of this field is what Galison calls the *logic tradition*. The logic tradition consists of a history of research involving Geiger counters and electronic selection mechanisms which register large amounts of data. These data are gathered by experimenters and analyzed with statistical methods. The demonstrative continuity of this tradition is quite different from that of the image tradition; in the logic tradition, "a single event, whether it was the click of a Geiger counter or the pulse from a complex array of counters, was meaningless in itself. Data in the logic tradition became persuasive only in their statistical aggregation" (Galison, 1997, 453). According to Galison, scientific arguments in this tradition are made on the basis of large amounts of statistically-analyzed data.

The dissimilarity of the technologies of each tradition leave a gulf between these two communities of experimenters not simply in terms of their expertise, but in terms of what form each understands persuasive data to take. Galison explains, "Neither the cloud chamber nor the bubble chamber employs complicated electronics, neither depends on high-voltage technology, and neither produces a logically selected, high-volume output of statistical clicks. Where the image detectors produce fine-grained photographs that often had to prove their worth through a single event, the logic detectors deliver course-grained but plentiful tracks that convince through overwhelming enumeration" (1997, 425).[13] According to Galison, the difference in these demonstrative continuities divides these communities of researchers into separate traditions largely isolated from one another.

On the basis of his analysis of the history of microphysics, Galison offers a controversial view of the general structure of scientific research. He claims that, "science is disunified, and—against our first intuitions—it is precisely the disunification of science that brings strength and stability" (Galison, 1997, 781). Despite separate "quasi-autonomous" traditions existing within a field of science, moments of collaboration occur between traditions without full translation of the separate theories, languages, and goals of those involved. Borrowing tools from the field of anthropology, Galison claims that the two traditions can be viewed as separate 'cultures.' These cultures, while vastly different, are not entirely isolated. He says, "it was a continuing goal of both the image and logic experimenters to reproduce the epistemic virtues of their opponents within their own traditions" (Galison, 1997, 401). The moments of the history of this field in which attempts were made to amalgamate the two experimental traditions present philosophically interesting and scientifically important junctures.

Still working with analogy to anthropology, Galison develops the concept of the *trading zone* to describe the point of interaction between traditions. An important aspect of this notion is that "within this trading zone both sides are perfectly capable of working within established behavioral patterns. But the understanding each side has of the exchange of money is different" (Galison, 1997, 804). A trading zone between experimental traditions does not create a place in which total translation or complete calibration occurs; instead, a space is created in which both sides work together successfully while at the same time never fully understanding the other's ways. Each side also retains its own separate goals. Continuing with the analogy to anthropology, he develops the concepts of 'pidgins' and 'creoles.' *Pidgins* are the amalgamated languages established within a trading zone so that members of the different traditions can communicate and learn about the other's methods. A creole is a pidgin language that has been developed over a longer period. He says that a creole is "a pidgin extended and complexified to the point that it can serve as a reasonably stable native language" (Galison, 1997, 832). In practice, a creole emerges as a new generation of scientists is trained within a highly developed pidgin language. Galison investigates examples of trading zones developed within the history of microphysics, for example the electronic statistical studies of bubble chamber imaging (1997, chapters 6 & 7).

2c. The Importance of Traditions to Scientific Debate

One thing revealed by these reflections on instrumentation is that scientific practice is a necessarily social enterprise, moving forward through concrete histories of relations between groups of researchers, and between researchers and their instruments. The work of Galison and Ihde shows how scientific practice is deeply situated within concrete histories of interacting social collectives and technological innovations. The work of these figures also demonstrates that philosophical reflection on episodes of the history of science has the potential to yield new concepts that can help to elucidate both specific moments of scientific research, and the nature of science more generally.

It is within this spirit that I develop an account of traditions of scientific debate in chapter two. There I provide a series of concepts that articulate the roles that collectives of debate participants play in the movement of scientific research. By limiting the focus of investigation to traditions of debate in science, as Ihde and Galison have focused their studies to experimental and instrumental conventions, an incomplete picture of scientific practice will be delivered. It is from this very incompleteness that this account derives its use. By spotlighting a specific aspect of science, a particular target of study can receive a detailed and sustained philosophical analysis.

However, before this analysis can begin, I must briefly review one more area of thought on the nature of scientific research. Since scientific debate is necessarily a social process, it is prudent to review some concepts regarding the social aspects of science developed in the field of science studies. In particular, it is important to review concepts which regard the transmission of one's research results to others of the scientific community.

3. Sociological Views of the Transmission of Research

Scientific debates necessarily involve interaction between debate participants, and also interaction between those participants and the larger audience of scientists. It is useful to plumb the field of science studies for concepts that describe the social structures through which participants of a scientific debate engage one another's work.

The field of science studies can be characterized in many ways. Though members of this field develop many different types of accounts of science, a common trait of this work is a commitment to detailed study of scientific practice. A large number of sociological case studies have been conducted in the last thirty years, and practitioners have offered many influential generalizations regarding the social conventions of scientific practice. Above, I briefly pointed out that the most controversial generalizations offered by science studies may be those of a philosophical nature, including relativist notions of truth and 'social constructionist' claims.[14] It is not my intention to replicate these sorts of claims here. I focus on some basic empirical generalizations regarding the practices of communicating scientific research results to others in the field.

A number of attempts have been made to articulate the ways that scientific research results are disseminated, support of others is achieved, and credit is attributed. I review four concepts central to the field. They are: 'trials of strength,' 'black-boxing,' 'obligatory passage points,' and 'translation.' While these concepts have been advanced by many researchers, including Michel Callon, Karin Knorr-Cetina, Trevor Pinch, and Steve Woolgar, the primary texts to which I refer are Harry M. Collins' *Changing Order*, and Bruno Latour's *Science in Action* (Collins, 1985; Latour, 1987). I review these concepts below.

Trials of Strength: Working from the Actor-Network-Theory (ANT) perspective of science studies, Bruno Latour claims that when scientists compete with one another to become the one that is commonly regarded to possess the dominant theory, this competition should be understood as a 'trial of strength.' In this view, each competitor attempts to gather as many actors as possible to convince the community of scientists that his or her scientific claim is the one that should be considered correct. Without the endorsement of the larger community, a scientist's claim remains simply that: the claim of a single person. According to Latour, "You may have written a fierce paper that settles a controversy once and for all, but if readers ignore it, it cannot be turned into a fact; it simply *cannot*" (Latour, 1987, 40).

Latour's portrayal of scientific practice highlights the work it takes to enlist the many actors necessary for successful persuasion. These actors can include, for example, the backing of other scientists, the use of instrumentation, the creation of new objects, and of course the data collected. One example of a factor of persuasion he explores in detail is the use of references in academic papers (1987, 33). According to Latour, a large list of references at the end of a paper represents not only the citations of related work, but also a roster of allies whose work has been built on. He claims that, "attacking a paper heavy with footnotes means that the dissenter has to weaken each of the other papers, or will at least be threatened with having to do so" (Latour, 1987, 33). While, of course, a long reference list does not itself force the larger community of scientists to accept the claims of a paper, it does constitute one element in the network of factors that make a paper persuasive.

Since the publication of *Science in Action*, several important critiques of this view have emerged. For example, at the start of this chapter, I referred to the critiques of relativism leveled by philosophers of science against social constructionists such as Latour. Latour's model has also come under fire from proponents of the SSK tradition of the sociology of science. Contentious debates have taken place between the ANT and SSK camps over issues such as the agency of non-human objects, and the nature of 'causes' in social constructionist accounts of science (e.g. Collins and Yearley, 1992a; Woolgar, 1992; Callon and Latour, 1992; Collins and Yearley, 1992b, as well as Bloor and Latour's "Anti-Latour" exchange: Bloor, 1999a; Bloor, 1999b; Latour, 1999).[15] Also, several writers claim that the language of Latour's 'trials of strength' too strongly casts scientific debate as a kind of combat (e.g. Starr and Griesemer, 1989; Amsterdamska, 1990; Haraway, 1997; Downey, 1998; Crease et al., 2003). For example, Donna Haraway critiques Latour's rhetoric, commenting on the masculine values reflected in the use of war metaphors to describe scientific practice. She explains, "War is the great creator and destroyer of worlds, the womb for the masculine birth of time. The action in science-in-the-making is all trials and feats of strength, amassing of allies, forging of worlds in the strength and numbers of forced allies. All action is antagonistic; the creative abstraction is both breathtaking and numbingly conventional" (Haraway, 1997, 34).

Olga Amsterdamska advances an important critique of the 'trial of strength' account. She claims that Latour's account is empty because *it explains too much*; the account may cover science, but it also covers non-science forms of persuasion. This dangerously includes unethical forms of persuasion. She writes, "We may well be unable to provide an unequivocal demarcation between science and non-science, to formulate an ahistorical definition of rationality, or to ensure a firm foundation of knowledge; but it does not follow from this that all methods of gaining assent or reaching consensus are equivalent. Do we really want to believe that there is no difference between the power of arms, of dictators and policemen, and of what we in our culturally specific way call rational argument?" (Amsterdamska, 1990, 502).

In a recent interview, Latour is pressed on these issues (Crease et al., 2003, 23). He claims to accept the criticism. He claims that the heavy use of combat metaphors in works such as *Science in Action* is meant to serve a specific purpose. He says, "The priority twenty-five years ago was to open up the immunized world of science to social theory. My priority was to bring out as much force and dispute in the sciences as possible" (Crease et al., 2003, 23). Latour's defense is that the portrayal of science as combat-like highlights those aspects of science in which persuasion is important to its workings. Practitioners of science studies claim that these aspects of science are exactly the objects that the methods of sociology and anthropology can help to elucidate.

In chapter two, I build on several of Latour's insights. I also consistently cast science in terms of rivalry and competition. However, I do not take my account to fall prey to the criticism that it portrays science as warlike; instead, the account I develop emphasizes relationships between interlocutors, processes of joint investigation through scientific debate, and the potential for scientific debate to contribute to scientific progress.

I also do not take my account to fall prey to the criticism that it is over-general. The goal of this project is to understand how scientific debates evolve. Some aspects of this account relate to practices idiosyncratic to science, such as the changing relationships scientific debates share with forms of experimental data. Some aspects of my account could apply not only to scientific debates, but to other forms of debate as well. These latter aspects are not any less important to the goal of this project.

Black-Boxing: This concept, advanced in subtly different ways by science studies researchers such as Harry M. Collins, Bruno Latour, Trevor J. Pinch, and Steve Woolgar, is used widely in studies of the closure of scientific controversies (e.g. Collins 1985, 64; Latour and Woolgar 1986, 242; Latour 1987, 33; Pinch, 1992; Woolgar 1988, 39). In this view, a scientific result becomes 'black-boxed' when a controversy closes and a consensus is reached. A black-boxed scientific research result is one that is no longer questioned by scientists. It is instead used and built on in further scientific research. In this view, much work of the kind reviewed above (in the summary of 'trials of strength') is required for one's scientific research result to win out over all competitors and become black-boxed. In this lexicon, a researcher is in the process of 'black-boxing' a claim when he or she works to establish their claim as one that should no longer be analyzed, but instead should be assumed to be established.

This term is also useful in the analysis of the process of challenging a scientific claim that has become an institution within a field of practice. In a case in which one would like to challenge a long-standing notion, one must challenge the notion's black-boxed status. This requires the challenger to attempt to 're-open' the black box. The challenger must reanimate whatever debate once existed with respect to the long-standing notion in question. It takes much work to breathe new life into a controversy that is commonly considered to be closed. It requires convincing others to question something they take for granted.

Obligatory Passage Point: The term 'obligatory passage point,' introduced by Bruno Latour, refers to a specific type of black-boxed result (1987, 139). An obligatory passage point is a black-boxed result that one must engage when attempting to do particular work. Referring to the details of a specific example he has reviewed, Latour says, "take note that the black box is in between these two systems of alliances, that it is the obligatory passage point that holds the two together and that, when it is successful, it concentrates in itself the largest number of hardest associations" (1987, 139). In this view, a high goal of a scientist is to have his or her work become a necessary point of reference and acceptance.

For example, an author of an academic paper may be compelled to cite particular major figures or texts. A collective of researchers may implicitly and loosely institutionalize rules that stipulate which other works should be explicitly recognized (for example, by citation). An author cites these works to show that he or she has the requisite familiarity with the field to be able to join the discussion at the appropriate level. Citation is just one example. Passing through an obligatory passage point can occur in many ways, and in many cases may be integral to the work being done. One may pass though an obligatory passage point through the utilization of a specific procedure or instrument, or by building on important already-established claims.

Translation: The notion of 'translation' has been developed by many researchers in science studies, including Michel Callon, Karin Knorr-Cetina, and Bruno Latour (e.g. Callon, 1980; Knorr-Cetina, 1981; Latour, 1987). These researchers claim that for one's scientific research result to travel through the larger scientific community, the work of many people is required. He or she who claims to be responsible for the research result cannot force his or her results to travel through the community by virtue of his or her own effort alone; many allies are required. Latour says, "To picture the task of someone who wishes to establish a fact, you have to imagine a chain of the thousands of people necessary to turn the first statement into a black box and in which each of them may or may not unpredictably transmit the statement, modify it, alter it or turn it into an artifact" (Latour, 1987, 104).

The act of translating involves more than simply advertising the importance of one's research results; it involves convincing others that it is in their own interest to take up and futher transmit those results. The term 'translation' refers to the act of aligning the various interests of the many people required for one's research result to be transmitted through the community. All of these people must have their interests translated in such a way that the research result remains intact, and remains attributed to the one doing the translating.

Chapter One Wrap Up

The goal of this chapter has been to review the concepts from the history of the philosophy of science and science studies which I borrow and build on in what follows. This has required an outline of the works of several figures, including Kuhn and Lakatos, in their historical contexts. In particular, Lakatos's notion of the scientific research programme will provide a useful point of contrast for the work that follows. The concepts from the field of science studies reviewed above will be appropriated and adjusted for use in the following analysis. Also, the concepts developed by Ihde and Galison reviewed above provide analogies for the sort of work I hope to accomplish, and will also be revisited at the end of the last chapter.

Notes for Chapter 1

1. There are many works in the history of the philosophy of science that explicitly address the issue of scientific progress, or can be interpreted to address this issue. For example, figures such as William Whewell and Joseph Priestly conceptualize scientific progress in terms of a process of steady unification of advancing theories (e.g. Priestly, 1767; Whewell, 1847). Logical empiricists tend to conceptualize scientific progress as the replacement of weaker scientific theories with stronger ones, a position exemplified by Ernest Nagel's account of 'intertheoretic reduction' (e.g. 1961). Influential contemporary accounts tend to have a more pragmatic character, conceptualizing scientific progress in terms of a theory's ability to address issues. Examples include Larry Laudan's account of science's 'problem solving effectiveness,' and Philip Kitcher's account of 'explanatory effectiveness' (e.g. Ludan, 1977; Kitcher, 1993). See Losee, 2004 for a helpful introduction to these issues.

In this chapter I focus on Kuhn and Lakatos. Both of these figures are extremely influential in discussions of these issues. However, I do not address Lakatos's important and controversial view of scientific progress as the discovery of 'novel facts.' My goal in this chapter is instead to highlight Lakatos's view of progress as occurring through rivalry between competing scientific theories. His account of progress-through-rivalry provides a helpful point of contrast for my own work on scientific debate in later chapters.

2. This aspect of Lakatos's philosophy, i.e. one can only determine a programme to be degenerating after the fact (since it could possibly experience a change in fortune), has been seen as a problem by critics (e.g. Feyerabend, 1970; Laudan 1997).

3. Of course there have been many criticisms of the Lakatos-informed studies of the history of science. Laudan, for example, claims that neither Lakatos's study of Bohr (reviewed here), nor Zahar, 1973, nor Lakatos and Zahar, 1975, actually stay true to Lakatos's own interpretation of scientific progress. Chen suggests that the difficulties in Worrall's Lakatosian account of the optical revolution could be solved by adjusting for several of Laudan's observations (Chen, 1988). Thomason critiques Lakatos and Zahar's account of the Copernican revolution (Thomason, 1992).

4. I take Lakatos's view that the Copenhagen Interpretation (CI) represents the degenerating phase of Bohr's programme to be an example of his brand of 'rational reconstruction,' and perhaps an example of why he began this account with the warning that it be taken with "tons of salt." It is clear from the quote in the text above that Lakatos disapproved of the CI of quantum mechanics. However, historically it is not the case that the CI had simply been replaced by wave mechanics; the CI becomes the dominant interpretation. By claiming the CI represents the degeneration of Bohr's programme, Lakatos appears to 'reconstruct' this history in terms of his view of how the rational progression of quantum physics should have occurred.

5. While 1977's *Progress and Its Problems* represents Laudan's most sustained investigation of 'research traditions,' other sites examples include (e.g. Laudan, 1981b; Laudan, 1996).

6. For Laudan's discussion of the importance of conceptual problems for any account of science, see (1977, chapter 2).

7. Hacking later fleshes out the notion of 'style' in his important article "'Style' for the Historian and Philosopher of Science" (Hacking, 1992).

8. *Quantum Dialogue* is considered controversial in that it claims that those who forwarded the Copenhagen Interpretation employed manipulative rhetorical maneuvers to persuade others to accept theirs as the exclusive dominant interpretation. While on the one hand Beller's work was award winning, on the other, some reviewers took exception to her iconoclasm (e.g. Gomatam, 2000; Greenberger, 2000). These reviewers would presumably also take issue with Lakatos's view, reviewed above (footnote 4), that the formulation of the Copenhagen Interpretation represents the degenerative phase of Bohr's programme. Beller's most notorious version of her views on Bohr's rhetorical strategies may be (Beller, 1998).

9. In the standard interpretation of the Como lecture, Bohr attempts to provide a clear and consistent account of complimentarity but instead provides a confusing text. According to Beller, examples of this standard view include (Jammer, 1974; Folse, 1985; Howard,

1994). In particular, Beller disagrees with the view that Bohr had taken up the notion of light quanta by the time of the Como lecture (a view held by Jammer, 1966; MacKinnon, 1982).

10. See also my own contributions to this topic in the philosophy of experimentation, including the analysis of the case study offered here in chapter three (Rosenberger, 2005; Rosenberger, forthcoming a; Rosenberger, forthcoming b).

11. Robert Ackermann's notion of 'data domains' and Ian Hacking's notion of 'style' are also helpful for conceptualizing the roles played by experimental methods in scientific progress (Ackerman, 1985, 125; Hacking, 1992).

12. It should be noted that Ihde does not hold that lenses are imaging devices since they enhance vision, not create images. A person sees through lenses; images are looked at and interpreted.

13. A critique of the history which Galison offers, questioning the actual separation of the image and logic traditions, occurs in (Staley, 1999).

14. The 'science wars' is the title some give to the often acerbic debates that have taken place over the notions of the social construction of science. Some of the most notorious science wars texts include (Gross and Levitt, 1994; Ross, 1996; Koertge, 1998; Labinger and Collins, 2001). I have already noted in the introduction to this chapter some of the

many contributions that philosophers have made to the criticism of the field of science studies.

15. For critiques of the politics of these debates, see (Fuller, 1996; Fuller, 2000). A helpful summary of the 'Anti-Latour' debate appears in (Kaposy, 2000).

Chapter 2: The Form of Scientific Debate Traditions

In this chapter I introduce the basic framework of concepts that are used to investigate case studies in later chapters. In the first section, I define the central concept of this dissertation: *the scientific debate tradition*. In the second section, I apply this notion to a concrete example of scientific debate. From this example I abstract a number of concepts regarding the relationships that exist between rival positions. In the third section, I apply these concepts to a fictional example of a scientific debate tradition and consider how the different ways debate traditions change over time can be conceptualized.

1. Scientific Debate Traditions

Philosophers of science have considered the importance of 'traditions' to scientific research. Examples include Kuhn's 'paradigms,' Lakatos's 'research programmes,' and Galison's 'experimental traditions.' These conventions of practice represent essential aspects of the way science operates; science moves forward by virtue of the complex and structured involvement of many different people, instruments, and experimental procedures. In this dissertation, I focus on a particular set of scientific conventions: those regarding scientific debate. The central concept for this investigation is the *scientific debate tradition*.

A ‘scientific debate tradition’ refers to all of the rival positions regarding a specific topic over time, and to all of the complex relationships between them. When scientist A and scientist B engage in a debate over an issue, the scientific debate tradition regarding their debate refers to several things. It refers to the positions of both scientists A and B, to all of the interactions between them over time, and to all of those related positions regarding this topic that now become obligated to contend with the results of the debate between these two scientists.

Investigating scientific debate traditions requires adopting a *whole-debate perspective*. In taking up this perspective, one focuses on the rival positions of a debate, and the effects they have on one another. This perspective can be helpfully contrasted with the philosophical accounts of ‘traditions’ of scientific research reviewed above. Lakatos and Laudan (contra Kuhn) make explicit the importance of rivalry between traditions. For example, in Lakatos’s view, the central claims of a research programme cannot be falsified without the existence of a progressing rival programme. But as suggested above, this account does not sufficiently analyze the relationships between rival scientific positions. A wider perspective that attends to the totality of a scientific debate is required. An investigation from a whole-debate perspective may reveal a richly detailed set of changing interactions between the opposing positions of a scientific debate. This perspective enables analysis of structural changes in a debate over time at the level of the whole debate, rather than only at the level of the individual line of research.

1a. The Debate Evolution Thesis

I claim that scientific debates do not always, or even frequently, become resolved by a process of one position in its original form defeating all others. I call this *the debate evolution thesis*. Rather than finding resolution in the terms of their initial formulations, scientific debates often dramatically shift in structure over time, their various positions changing and adjusting in many ways. As rival positions interact while attempting to address the central questions of the debate, these interactions have effects on the character of the rivalry itself, and on the contents of those positions.

The debate evolution thesis is meant to offset an assumption that operates in much of philosophy of science and science studies. Philosophers of science often attempt to understand how scientific methods reveal the truth about the world, and whether the objects of scientific research should be considered 'real.' Science studies practitioners often investigate how 'facts are constructed' and how controversies close. I claim that both philosophy of science and science studies tend to misrepresent an important aspect of scientific debate—the fact that debates do not ordinarily become resolved by one side remaining intact and defeating all contenders. By focusing primarily on undisputed facts that have been established by science, and then working backward to learn about the mechanisms through which science has discovered them, these thinkers obscure the way that scientific debates may take many important twists and turns.

With the debate evolution thesis, I claim that the usual course for a debate in science involves changes in the conventions of interaction between rival positions, and changes in the positions themselves. These changes come as those holding rival positions challenge one another through cross-examination and through the defense of their answers to the central questions under debate. As one position moves forward, the rival positions within the discussion adjust to these changes. A typical scientific debate does not simply involve one position beating out all others; it involves a developing discussion in which what it means to engage in the debate, and what it means to continue to defend the same position, changes over time.

This thesis is falsifiable. How well it holds up will be determined by how well the set of concepts associated with the notion of the scientific debate tradition can be usefully employed as tools in the analysis of concrete examples of debate in science.

1b. Debate Nodes and the Problem Hub

A scientific debate tradition is composed of a number of positions that disagree about the answers to a particular scientific question or questions. Let us refer to the positions of a debate tradition as *debate nodes*. A 'debate node' is a side of an ongoing debate, a position with respect to rivals. As the theories that make up a particular node advance and change, the node changes only with its relationship to rival nodes. A node of a debate tradition could split, could face an increase in its number of rivals, or could combine with a rival. It could also remain static over time as its constituent scientific theories grow and advance.

The nodes of a debate are configured in terms of a *problem hub*. The 'problem hub' is the central question or questions which the different nodes address. The rivalry between nodes occurs with regard to this hub. A line of research may yield results which are relevant to a number of separate issues. Depending on which of these issues is focused on, a very different branching tree of allies and opponents may be revealed.

To illustrate this point, imagine a field of scientific study constituted by six lines of research: 1, 2, 3, 4, 5, and 6. A main issue of the field is O, on which positions a and b can be taken. These six lines of research have turned up different results with regard to issue O. Lines 1, 2, and 3 have yielded data which support position a. Lines 4, 5, and 6 support position b. The positions of these six lines of research with regard to issue O can be denoted as follows: 1a, 2a, 3a, 4b, 5b, and 6b. Those working in these various research lines respond to one another's work, critique those supporting the rival position regarding issue O, and defend against criticisms. This simple example can be understood as an instance of a scientific debate tradition. Issue O represents the problem hub of the tradition. This tradition consists of two debate nodes, one which encompasses the three lines of research that support position a, and one encompassing the three lines of research supporting position b. A tree-shaped diagram can be used to chart the relationships between these lines of research with regard to issue O. This graphic illustration conveys the way these six research lines organize into the two debate nodes of this tradition. See Figure 2.1.

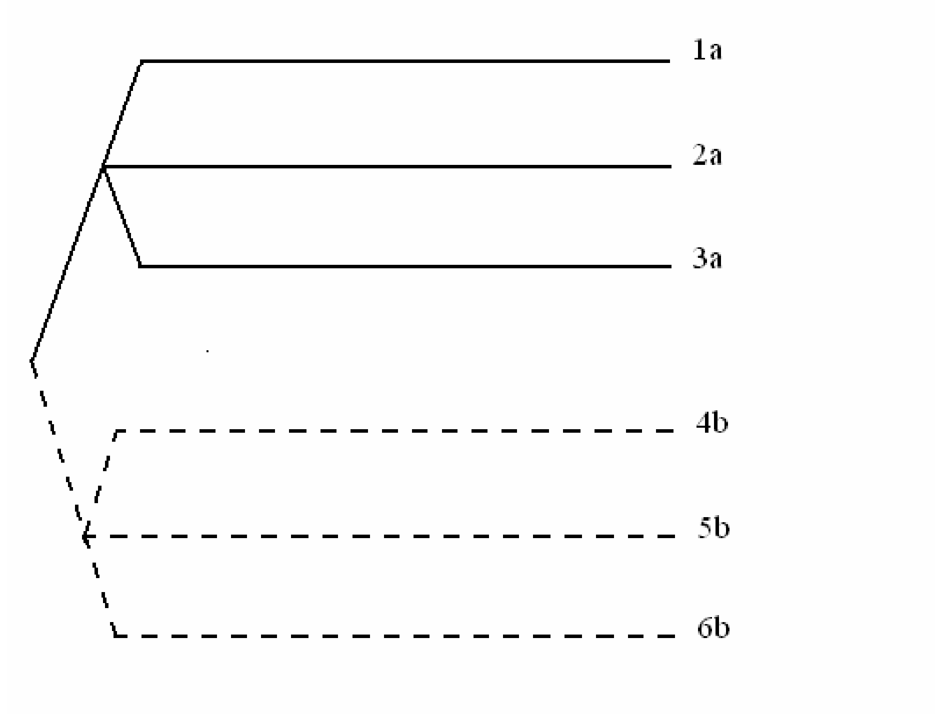

Figure 2.1. In this tree diagram of the debate tradition concerning issue O, the solid lines together represent the debate node which supports position a. The dotted lines represent the node which supports position b.

To make the problem hub of this simple example of a scientific debate tradition explicit, a second issue relevant to these lines of research can be considered. The second issue of this field is X, on which the positions y and z are taken. In this field, research lines 1 and 4 have found support for position y. Lines 2 and 5 have found support for position z. However, lines 3 and 6 have not turned up data relevant to issue X. The lines of research can be denoted in terms of their positions regarding both issues O and X in the following way: 1ay, 2az, 3a, 4by, 5bz, 6b. Debate between those working on these research lines occurs in terms of both issue O and issue X. The composition of the nodes of this debate tradition depends upon which issue is taken to be the problem hub. This difference is illustrated in the contrast between the tree diagrams in Figure 2.2 and 2.3.

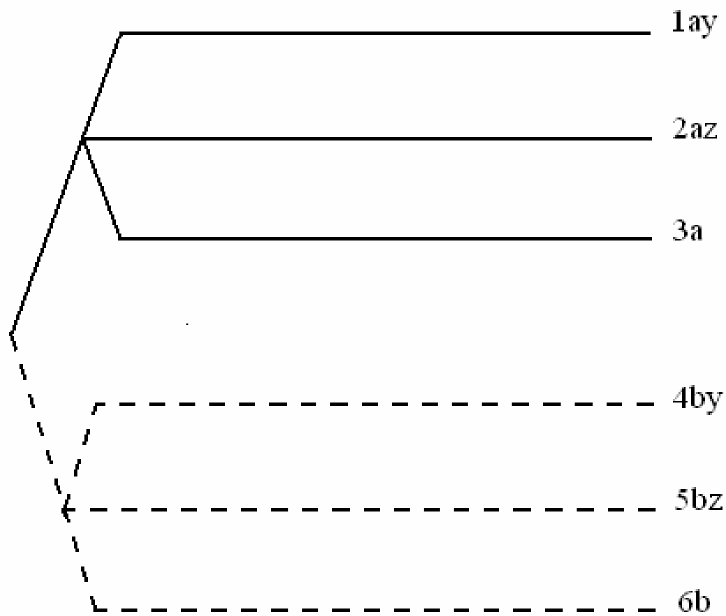

Figure 2.2. In this tree diagram of the debate tradition concerning issue O, the solid lines together represent the node which supports position a. The dotted lines represent the node which supports position b.

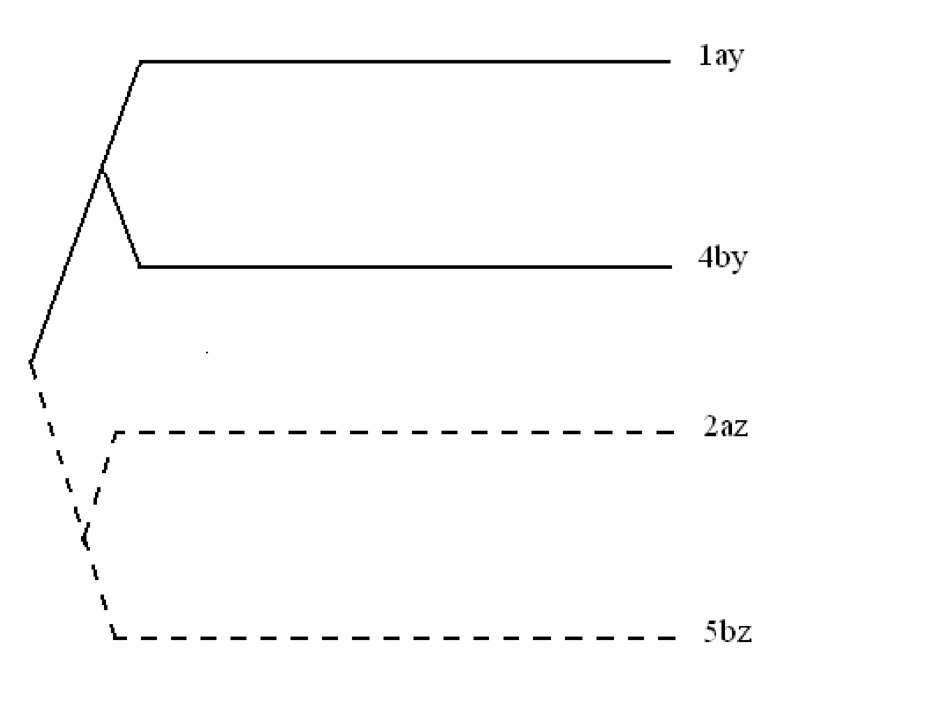

Figure 2.3. In this tree diagram of the debate tradition concerning issue X, the solid lines represent debate node which supports position y. The dotted lines represent the node which supports position z.

Figures 2.2 and 2.3 portray different scientific debates composed of some of the same lines of research. The contrast between these two tree diagrams illustrates how the problem hub of a debate tradition determines the particular way the relevant positions are arranged. A scientific debate tradition's configuration depends on what issues are taken as the central topic of discussion.

The specific way a debate is taken to be configured also depends on the purposes of the person charting the debate. One attempting to contribute to a discussion regarding a particular issue may understand the sets of allies and rivals in a field differently than one approaching a separate issue also relevant to that field. Also, the depth of one's analysis of a debate is related to one's purposes. The decision regarding whether one should take all of the relevant positions into consideration, or instead consider only the key players, depends on the sort of work one is doing. For example, an historian attempting to portray the entirety of a debate may consider a larger number of positions than may a scientist writing a short article which briefly refers to a debate in a literature review.

1c. Branch Quarrels

A debate tradition can be constituted by any number of debate nodes. Also, a debate node itself can be constituted by several opposing positions. To make these points clear, a distinction must be made between the *primary debate nodes* of a tradition, and the *branch quarrels* that constitute those primary nodes. The 'primary nodes' are those major opposing positions of a debate tradition, each relating to the tradition's central defining questions (a.k.a. the problem hub). A 'branch quarrel' is a debate which occurs within one node of a debate tradition.

The primary debate of a tradition is neither necessarily more important nor necessarily more productive than its branch quarrels. A node is 'primary' if it relates to the problem hub of the tradition. 'Branch quarrels' are instead disputes that refer only to issues which exist within a node of the primary debate. It is possible for the content of a branch quarrel to have effects upon the primary debate. If a node of a branch quarrel were to shift in such a way as to contribute to the primary debate, this line of research could be said to *jump tiers*.

Examples of important branch quarrels occur in both case studies in chapters three and four, and also in the fictional example of the paw/hoof debate explored below in section three.

1d. Establishing and Maintaining Debates

It takes work to enter into a debate with potential rivals. It also takes work to maintain an existing debate; participants must continue to gather recognition from rivals even as positions advance and change. In this subsection, I develop a number of concepts for articulating the work involved in building new scientific debate traditions, and keeping existing debate traditions stable and active.

The processes of establishing and maintaining scientific debates have inextricably social characters. One must interact with researchers who agree with one's claims, and also with those who specifically do not. Participants work collectively to make their debate known to a larger audience. To articulate these features of scientific debate traditions, it is useful to revisit the concepts from the field of science studies reviewed in the third section of chapter one. These concepts have been developed by science studies scholars to describe the processes through which a scientific community comes to regard a controversy to be closed. I adjust these concepts for a different use. These concept are modified to help articulate how scientific debates are formed, and how they continue despite changes in the participants' positions.

While appropriating concepts from science studies, I do not wish to commit this dissertation to the position held by some science studies scholars that scientific research is somehow reducible to social processes. My account focuses on practices of experimentation, argumentation, and counter-argumentation; because scientific debate is necessarily a matter of social interaction, the work of science studies proves useful. I first consider (1) the topic of debate establishment, and then (2) the topic of maintaining scientific debates.

(1) Establishing Scientific Debates

It will be useful to appropriate the concepts of 'trials of strength,' 'black-boxing,' 'translation,' and 'obligatory passage points' from the field of science studies in order to articulate the social processes through which scientific debate traditions are formed (all reviewed in chapter one, section three). A 'trial of strength' refers to a dispute in science in which each rival position marshals its forces in the attempt to defeat the others. Science studies scholars consider many different things that can be gathered to win the trial, including the backing of other scientists, newly discovered objects, data, and instruments. When a trial of strength is won and there is no longer any controversy regarding a result, science studies practitioners refer to the result as 'black-boxed.' A 'black-boxed' result is used unquestioningly in further research. The term 'translation' refers to the act of convincing others that it is in their interest to promulgate one's own research results. If one is successful at translation, the results become widely publicized and also remain attributable to one doing the translating. An 'obligatory passage point' refers to a black-boxed result that others feel compelled to cite and use when doing further work. Science studies scholars claim that having one's research results become an obligatory passage point is a high goal of scientists. I suggest that modified versions of these concepts can be used to investigate how scientific debates become established.

The concept of the 'trial of strength' is used by science studies practitioners to emphasize at least two characteristics of scientific research: (1) the fact that it takes work to bring together the variety of resources necessary to be considered the victor a scientific dispute, and (2) the fact that success is in large part determined by the judgment of the larger audience of researchers. The analogy of the 'trial' may not hold as well for the project of creating a debate since the outcome is not a single victor, but discussion between rivals. However, the two aspects of scientific research emphasized by the concept are relevant here. To participate in a scientific debate, a scientist must interact with more than simply her or his lab station; a scientist must interact with those from the opposing debate nodes, and with those who are allies. It requires work to enlist potential allies and rivals for participation in a new debate. Work is also required to convince others in the field that a new debate is important. The others include, for example, journal and book editors, reviewers, and referees, the readership of these publications, grant providers, and the larger community of researchers whose own work may be in smaller ways related to the issues of the new debate. Let us use the term *debate establishment* to refer to the process of setting up a rivalry between positions on a topic in science.

The winner of a trial of strength may have his or her results 'black-boxed,' used unquestioningly in further research. In contrast, the goal in the process of debate establishment is to have one's research become recognized as an important node of a flourishing and productive discussion. When establishing a debate, others are encouraged to scrutinize and challenge one's results, to engage one's position in debate, and to cite one's work in desagreement. The notion of 'translation' also functions differently in the context of debate establishment. Rather than engaging in translation so that a research result can be transmitted through the community, translation in this context involves interpreting others to be rivals and encouraging them to test and challenge one's own position.

If one is exceptionally successful in establishing one's position as a formidable and important node in a tradition of debate, one's position may become instituted as an indispensable voice in the conversation. Legitimate participation in a debate can become defined as challenging a few specific well-established debate nodes. For example, a paper may be rejected by a journal on the grounds that it does not engage the most recognized relevant positions. Let us give the name *obligatory debate node* to a debate node that has become a defining feature of a debate tradition. An obligatory debate node is a position in a debate that a newcomer is obligated to face.[1]

The task of establishing one's position as a node of a new debate can occur through a number of strategies, and within a number of contexts. Two or more researchers could together interpret their positions to challenge one another. Together these researchers could work to generate attention for their disagreement and attempt to establish it as a recognized discussion with their field. Others that would like to participate in the debate could become obligated to engage these original positions.

Also, one could work to incorporate one's position into a debate which is already in existence. In this case, one could petition those occupying the already-established debate nodes, challenge their work, and make a case for inclusion. Those defending the

[1] Endnotes for this chapter are found on page 108.

already-established nodes may welcome the challenger and facilitate his or her project of establishing an alternate node. On the other hand, those occupying the already-established nodes could ignore the newcomer. It might still be possible to become an established debate node; if the larger community that cite, evaluate, and enter the debate acknowledge the newcomer as a node, the newcomer's position could become established despite the failure of the already-established nodes to take notice.

(2) Maintaining Scientific Debates

A scientific debate tradition changes over time. The positions advance, challenge one another, and respond to each others' criticisms. These changes can threaten the integrity of the debate itself, and can threaten the statuses of the individual debate nodes. For example, one position may make a major advance that appears to resolve the debate. Rival positions are then required to make changes to account for this advance, thus maintaining the debate's status as ongoing, and maintaining their own status as viable alternatives. I use the term *debate maintenance* to refer to the changes made throughout the course of a debate's history to keep it coherent as a debate, and to keep it thriving.

Debate maintenance can occur in a number of ways. One way debate maintenance occurs is when one position makes a major advance and its rivals are required to adjust. A typical research advance by one position offers rivals an opportunity to challenge or reinterpret the findings. But a major advance jeopardizes the structure of the discussion itself since it may be taken to close the debate. Rival positions must address this advance in order to maintain their standing as contenders, and to maintain the debate's status as open. To keep the debate active, the major advance may be challenged, reinterpreted, or even adopted and integrated into a rival position.

Another way debate maintenance occurs is in terms of an obligatory node attempting to maintain its status. It takes work for a position to continue to be understood as a defining feature of a discussion. Continuing activity may be required. As rivals advance, those defending an obligatory node may need to demonstrate their node's relevance to those advances.

Without advances, interpretations, reinterpretations, arguments, and counterarguments emerging from the debate process, a tradition of debate may itself become stale in the eyes of the larger relevant professional audience. Another form of debate maintenance occurs in terms of the ways that debate participants maintain their tradition's status as an important discussion within their discipline. How this occurs in each case depends on the nuances of the particular details of the debate, research, topic, and field. For example, in some scenarios a debate may be further sedimented as an institution of a field of study if it remains ongoing and productive without experiencing changes to its structure or tone. In other scenarios, an unchanging ongoing debate may seem stale. The stale debate may need to be rejuvenated by an alteration to its positions or mode. Generally, the production of new findings and new arguments works to maintain debates, providing the larger audience with data and opportunities.

Also, changes to the structures or modes of a debate may require those working within a tradition to engage in maintenance efforts. For example, debate may change in structure if a new position is introduced into the conversation. Already-existing positions may need to develop critiques of, and defenses from, this new position. Alernatively, the mode of argumentation within the tradition could shift. A new kind of data could be introduced, or a new interpretation of the rivalry itself could be advanced. Established positions may need to address these changes to maintain their own viability.

A central project of this dissertation is determining what sorts of relationships can exist between rival positions in a scientific debate, and what it means for those positions to change over time. The notion of 'debate maintenance' refers generally to efforts by rival positions to adjust to the changes which can occur. More refined concepts are developed throughout the dissertation to flesh out this idea. All of the examples of debate maintenance mentioned here are expanded on and conceptualized further in this dissertation.

2. The Relationships Between Rival Debate Nodes

It is now possible to consider the complicated relationships that exist between nodes of a scientific debate. In the first subsection below, I review a debate between developmental psychologists Susan Carey and Elizabeth S. Spelke on the topic of conceptual development in childhood. Many of the concepts introduced above can be instantiated by the details of this example. In the second subsection, I abstract several concepts regarding the ways rival debate nodes relate to one another.

The history of Carey and Spelke's interactions includes a significant shift in the structure of their debate. At first, Carey and Spelke take their two positions to be mutually exclusive. Later, they consider their positions to be compatible. This case provides a concrete and complex example within which we can search for guiding illustrations of the interconnectedness of rival positions.

It is important to note that the debate reviewed below is not**,** by itself, an example of a debate tradition; it is simply a debate between two positions. Examples of full debate traditions are explored in chapter three and four.

2a. Carey and Spelke on Conceptual Development

With figures in its history such as Jean Piaget, Lev Vygotsky, Jerome Bruner, and Eleanor Rosch, the field of developmental psychology analyzes children as they mature. Within the area of 'conceptual development,' researchers investigate children's acquisition of the concepts they use to organize their experiences, and the way these concepts change as children grow. Two major figures in this area are Susan Carey and Elizabeth S. Spelke.

Put generally, Spelke argues that the central concepts through which adults understand the world are present in the minds of infants. These concepts *do not* fundamentally change during development, but do become increasingly 'enriched' over time (e.g. Spelke, 1991; Spelke et al., 1992; Spelke, 1994; Spelke et al., 1994). Carey instead argues that a child's concepts change radically during development. She understands concepts to be embedded within the theories a child has about the world. In her view, concepts shift as a child forms new, more mature theories (e.g. Carey, 1985; Smith, Carey, Wiser, 1985; Carey, 1986; Carey, 1988; Carey, 1991).

Spelke and Carey each at times cite the other's work as a research-guiding contrast to her own. It is the *relationship* between these two positions I wish to analyze here. I suggest that the history of their interactions on this topic can be interpreted to occur in two phases. In the first phase, each researcher puts forward and defends her own theory, and argues against the other's position. In the second phase, the two collaborate to discover the places in which their theories fit together.

Phase #1. During the 1980's, Susan Carey begins a push for an account of conceptual development which holds that a child's concepts undergo radical change during development. She argues that children conceptualize the world through the use of theory-like conceptual frameworks. A child's concepts may radically change when the child develops new theories about the world. Carey articulates this view by combining the study of conceptual change in the history of science with the study of children's understanding of concepts throughout development.

A representative article in this line of research is entitled "On Differentiation: A Case Study of the Development of the Concepts of Size, Weight, and Density," which Carey writes with Carol Smith and Marianne Wiser (Smith et al., 1985). Smith, Carey, and Wiser begin by reviewing the history and philosophy of science, identifying places in which the analogy between conceptual change in science and conceptual change in development holds. They look for guidance in Thomas Kuhn's piece "A Function for Thought Experiments" (1977). Kuhn analyzes the conceptual changes which occurred in the historical shift from Aristotelian to Galileo's physics. He claims that Galileo showed Aristotle to have conflated the concepts of instantaneous and average velocity. In the process of changing his theory of the physical world, Galileo replaced a single, older concept with two, more precise concepts. Carey and her colleagues claim that the type of theory change that children experience during development is similar to this aspect of Kuhn's history of science. A new understanding of the world can force a child to split a 'parent' concept into two new concepts, even though that parent concept functioned perfectly well in the older understanding of the world. They use the term 'differentiation' to refer to a change in a child's understanding (or 'theory') of the world which results in the splitting of a previously held concept into new, more precise concepts (Smith et al., 1985, 179).

Carey and her colleagues investigate their theory with experiments designed to reveal whether young children fail to differentiate concepts that adults are known to hold as separate. In "On Differentiation," they examine whether children differentiate the concept 'size' from the concept 'weight,' and also 'weight' from 'density.' They explain, "we take it as a working assumption that even preschool children possess theory-like conceptual structures. Our success or failure in our effort to reconstruct a portion of their physical theory will allow us to evaluate this assumption" (Smith et al., 1985, 183). Carey and her colleagues claim that the discovery of a specific example of children's failure to differentiate concepts commonly held separate by adults would be an important gain in the effort to understand children's theories about the physical world, and would lend support to their thesis.

Carey and her colleagues infer what concepts children hold by observing their performance on verbal and non-verbal activities. If a child does hold undifferentiated concepts, it should be possible to show that the child never systematically applies the two concepts separately. Also, it should be possible to show that the failure to deploy the two concepts separately leads the child, at times, into confusion. The activities that the children perform in these experiments require them to make comparisons between different objects (e.g. Styrofoam, wood, metal), predict their relative weights, predict how much water they will displace, decide whether the object is heavy for its size, and perform other tasks of this sort. Carey and her colleagues find that, while children do not fail to differentiate between the concepts of size and weight, they *do* systematically deploy the concepts of weight and density in an undifferentiated way.

Based on these findings, Carey and her colleagues claim that a productive way to view the conceptual changes that occur in child development is to understand children to be similar to scientists. A scientist's conceptions of the world are embedded within the theories he or she investigates. Carey and her colleagues posit that children's concepts are similarly embedded within theory-like structures.[2] They argue that their account is the best way to explain the evidence of children's use of particular undifferentiated concepts.

Carey's other works on this topic are similar. She and others claim to uncover further examples of children's failure to differentiate specific concepts, such as the concept 'dead' from the concept 'inanimate,' and also 'air' from 'nothing' (Carey, 1985; 1988; 1991).

Elizabeth S. Spelke's claims instead that from infancy onward, people possess the central concepts they use to understand the world. In this view, concepts gradually 'enrich' as a child matures, rather than change radically as Carey argues. In several works, Carey is the primary figure against which Spelke argues, and Carey's is the central account against which Spelke's theory is structured (e.g. Spelke et al., 1992; Spelke, 1994; Spelke et al., 1994).

A paradigmatic example is Spelke's article "Origins of Knowledge," in which she and her colleagues present a series of experiments representative of the kind Spelke often conducts (Spelke et al., 1994). One of Spelke's central claims is 'the core knowledge thesis.' This is the view that the concepts central to infants' reasoning are also central to adults' commonsense understanding of the world. She interprets the core knowledge thesis to pit her work against Carey's. Spelke and her colleagues claim, "The denial of the core knowledge thesis has been a central feature of arguments that cognitive development brings radical conceptual change" (Spelke et al., 1994, 606). Like Carey, who borrows theoretical structures from Kuhn and others, Spelke understands her own work to fall within a tradition of philosophy, but one which in Spelke's case includes Descartes, Kant, and Chomsky (Spelke et al., 1994, 605).

Spelke and her colleagues begin by reviewing the literature on adults' commonsense conceptions of motion. They argue that support would be provided to their position if it could be shown that infants possess portions of the same reasoning as adults. Two concepts central to adults' understanding of motion are investigated: 'solidity' and 'continuity.' Spelke builds her research on a history of experiments pioneered by Piaget, Renée Baillargeon, and others, in which infants search for hidden objects. In these experiments, researchers clock the amount of time an infant spends watching an object. The infant's 'looking time' is used to interpret his or her interest in an event. In a series of experiments, Spelke and her colleagues show infants moving objects which travel behind a screen and thus out of the infants' view. Several different scenarios can be revealed when the screen is removed, such as one in which the object has moved in a continuous motion or one in which the object has not (or even whether, while occluded, the object appears to have passed through another solid object). It is predicted that an infant should be more interested in instances in which an occluded object appears to have moved in a way different from typical object motion.

Spelke and her colleagues find that infants behave in ways that imply they use several basic concepts of motion central to adults' understanding. They argue that this supports their theory that conceptual development occurs through the enrichment of core concepts present in infancy.

Spelke's article ends by again addressing Carey's position and others like hers. Spelke and her colleagues develop a number of counterarguments. The main argument made against radical conceptual change in development is that it does not explain the offered data. But Spelke and her colleagues also critique Carey's claim that the radical change account gains validity through its resemblance to conceptual change in the history of science. They challenge the analogy between conceptual progression in the history of science and child development, noting for example that according to Kuhn a scientific revolution occurs though much labor and disagreement; the development of commonsense concepts during development, in contrast, seems to come easily.

Carey responds directly to Spelke's works with experiments and counterarguments of her own (e.g. Carey, 1991; Carey, 1992). In her article "Knowledge Acquisition: Enrichment or Conceptual Change?," Carey clarifies her position by arguing against Spelke's (1991). She explains, "I deny Spelke's conjecture that ordinary, intuitive, conceptual development consists only of innate structural principles. The alternative that I favor is that conceptual change occurs during normal cognitive growth" (Carey, 1991, 258). Carey continues to articulate her position, noting the claims to which Spelke disagrees. As before, the main argument Carey makes for her position (and now against Spelke's) is a review of the experimental evidence which shows children to use undifferentiated concepts. Carey further clarifies her position by responding to different versions of Spelke's arguments, and even answering replies that Spelke has not yet offered, but could.[3]

The details of the history of this first phase of Spelke and Carey's debate can be used to instantiate the concepts regarding scientific debate developed in the previous section. The positions of these two researchers are rival 'debate nodes.' Their rivalry exists in terms of a central set of questions. These questions, or 'problem hub,' regard the nature of conceptual development in childhood. These researchers engage in a process of 'debate establishment' as they work to construe their accounts and data as offering challenge to one another. For example, Spelke and her colleagues 'translate' Carey's work, interpreting their own position to be implicitly "denied" by theories such as Carey's. Also, as new findings and arguments are produced within these two debate nodes, each engages 'maintenance' efforts to account for the other's moves. For example, Carey uses Spelke's arguments as an opportunity to further refine her own claims. Though Carey and Spelke's positions are just two in a larger debate tradition regarding conceptual development, it is instructional to conceptualize their interactions in terms of concepts I have offered so far.

The history of this debate does not end here. An important change in the structure of their interactions occurs in what I refer to as the shift from *Phase #1* to *Phase #2* of this debate. It is at this point that the concepts I develop in this dissertation offer an advantage over traditional philosophical accounts of scientific research. A more traditional analysis may treat these two lines of research in isolation, and expect one to triumph over the other. I claim that a great deal is gained through the use of the conceptual tools I develop. These concepts highlight the changing interactions between rival debate nodes. The debate between Carey and Spelke provides a case in point regarding the importance of these changing interactions.

Phase #2. In a recent interview with both researchers, Spelke sums up the relationship between her position and Carey's, explaining that, "I was arguing for enrichment and Susan was arguing for conceptual change. One of us proposed—and we both immediately agreed—that we write a paper about this so that we could get clear on our differences" (Carey and Spelke, 2002, 5). Later in the interview Carey continues, "It turns out that both kinds of development happen" (Carey and Spelke, 2002, 11). In the later period of this debate, which I refer to as *Phase #2*, Carey and Spelke at times join forces to co-theorize about the nature of conceptual development, taking into account both of their histories of research findings. In *Phase #1*, they both understand their theories and findings to be mutually exclusive; in *Phase #2* these are rendered consistent and developed into a combined theory.

Carey and Spelke together report that some aspects of adults' understanding of the world are the result of the enrichment of innate core concepts; others are the result of radical conceptual changes. Together they subscribe to the core knowledge thesis, but also hold that radical conceptual change does, at least at times, occur during development. They say, "we suggest that children's initial cognitive endowment consists of a set of innate core systems of knowledge which have some, but not all, of the properties of later developing intuitive theories and scientific theories" (Carey and Spelke, 1996, 516). This amalgamation of positions leads Carey and Spelke to new research questions not available in the previous theoretical configuration.

They begin the reevaluation of their two positions by first reviewing Spelke's findings. Next they list the interpretations of conceptual development that would be consistent with Spelke's data. Then they consider which interpretations from this list are also consistent with Carey's work. Any conflicting interpretations which remain on the list after both positions are considered are presented as questions for further study.

For example, after reviewing the history of Spelke's research results, the two authors claim that three positions on the issue of conceptual change are possible: what they call 'the strong universality hypothesis,' 'the weak universality hypothesis,' and 'the no universality hypothesis' (Carey and Spelke, 1994, 183-184). 'The strong universality hypothesis' refers to the position that only scientists (who have a metaconceptual sophistication) could overturn core, commonsense, innately acquired principles. If this is the case, commonsense reasoning could be universally found in children and adults. In contrast, the "weak universality hypothesis" states that adults and children overturn commonsense innate principles, but only when raised in a culture which offers them experience with the metaconceptual sophistication of science. A third option consistent with Spelke's findings is the "no universality hypothesis." This states that conceptual change occurs spontaneously during development, and is not the result of factors specific to those cultures which are pervaded by a scientific conception of the world. Next, these researchers consider which of these three ‘hypotheses’ are also consistent with Carey’s research on conceptual change. They claim, “The existence of conceptual change in childhood militates against the strong universality hypothesis” (Carey and Spelke, 1994, 184). More research is required to evaluate the weak universality hypothesis and the no universality hypothesis. Spelke and Carey claim that this example of a new direction for further research demonstrates the value of their amalgamated position.

Carey and Spelke also deploy their combined theory in further debate with others engaged in research on the roles of theory-like structures in child development. For example, they challenge a more radical view held by Alison Gopnik (Gopnik, 1996a; Carey and Spelke, 1996; Gopnik, 1996b). Gopnik provides a sophisticated version of the notion of children’s theorizing, and claims that it can explain both cognition as it occurs in infancy and through development. Spelke and Carey, with their newly-amalgamated view, instead claim that while the ‘theory’ metaphor can coherently account for conceptual change in childhood at least concerning certain topics (e.g. those Carey has studied), a core knowledge understanding of cognition better accounts for infant cognition. Gopnik responds that further empirical work is needed to resolve the matter since, in her view, each side of this debate offers a different coherent interpretation of the current findings. And she cautions, “I would hope that the eavesdropping reader outside the area would conclude that the difficulty in currently finding knock-down evidence that would discriminate our view and Carey and Spelke’s reflects the large amount of overlap between the two substantive views, rather than reflecting their unfalsifiability” (Gopnik, 1996b: 560).

The concepts developed in the previous section draw out particular aspects of this concrete example of scientific research. Where a more traditional conception of scientific debate might focus on the two rival positions of *Phase #1* and the combined position of *Phase #2* each in isolation, the perspective I advance conceives of this history as a shifting array of relationships between these two lines of research. The combination of these two positions in *Phase #2* is due to a shift in how the relationships between these positions are interpreted. In the next subsection, concepts regarding the general interrelations between rival debate nodes are abstracted from this example. In chapter five, I return to this example again and examine it in detail along with the case studies of chapters three and four to conceptualize the different particular ways that the relationships between rival nodes change over time.

2b. The Interrelations of Debate Nodes

The debate between Spelke and Carey provides an instructive example of the interconnectedness of rival debate nodes. A productive rivalry develops between Carey and Spelke's positions in *Phase #1*. Despite their mutual opposition during this phase, these two positions become deeply involved with one another. It is possible to articulate three categories of interrelations existing between Carey's and Spelke's debate nodes: interrelations of *co-validation*, *co-analysis*, and *co-composition*. Insofar as these categories of *general interrelations* abstracted from the example of Carey and Spelke's debate intuitively and empirically apply to debates in science more generally, we have the beginnings of an account of mutual involvement of debate nodes. Below, I explore the notions of 'co-validation,' 'co-analysis,' and 'co-composition.'

Co-validation: Rival debate nodes co-validate. This refers to the way that two (or more) rival positions may together gain recognition through debate. Debating positions explicitly or implicitly acknowledge one another as worthy of engagement; the act of mounting a rigorous opposition to a position can provide an implicit approval of that position, indicating that it is an important node of a debate. Also, as a debate advances, it can generate recognition from the larger community for each of the nodes.

Processes of co-validation can be seen in the history of the establishment of Carey and Spelke's debate. Spelke first offers opposition to Carey's work. She reviews Carey's findings and claims, puts them under careful analysis, and productively sets her own work against them. One effect of this process is to demonstrate the value of Carey's work, showing it to be a helpful and legitimate point of discussion. Next, Carey responds favorably to Spelke's challenge, and a productive debate emerges. Carey's part in the undertaking of creating and continuing the debate provides a degree of validation to Spelke's work. As their debate productively continues and gains popularity, it becomes recognized as an important discussion in the field. This acknowledgment from the larger community of scientists further validates the work of both contributors. So long as these structures, conventions, and interpretations remain in place, mutual participation in the debate can have the effect of co-validating each position.

A popular and vociferous controversy can create publicity. Attention can bring opportunities for publication and other professional activities. One's work gathers citations and review from those working on the other side of the debate. Carey and Spelke's debate is a prime example; each researcher showcases the other's work while contrasting it with her own. Researchers on opposing sides of a debate can work together to create venues for the discussion to take place. An example from the Spelke/Carey debate is a volume edited by Carey which features Spelke, and within which Carey responds to Spelke's charges (Carey, 1991; Spelke, 1991).

In a debate composed of many rival nodes, issues regarding co-validation emerge with regard to the establishment of 'obligatory debate nodes.' For a debate node to become obligatory (that is, a node which one is obligated to engage when participating in the debate), a high level of recognition from the other participants in the debate is required. When someone offers a new position to a debate, relations of co-validation occur as the new position engages the obligatory nodes. By explicitly challenging the obligatory debate nodes, the new position can acquire recognition as one which has something to say about the established points of the conversation. The obligatory debate nodes also become further validated as the defining nodes of the discussion.

Co-analysis: Co-analysis refers to the ways that rival positions of a debate focus on each other in investigating the leading questions of the debate together. This occurs in at least two ways. Firstly, a debate tradition provides the context in which a specific sort of inquiry can occur: the micro-critique of the rival positions. In the case of ongoing research, the definitive details of the object of study are not yet known, and sometimes even the most general features remain a mystery. Despite this, a tradition of debate can provide a context in which positions can be subject to a high level of technical scrutiny.

This is accomplished by evaluating a position in terms of its rivals. Even in areas in which very little is known about the object of study, a rigorous debate can emerge.

Secondly, those defending these scrutinized positions may benefit from the constructive criticism which emerges through the debate process. Rival positions co-analyze as they offer technical critiques of one another's claims or experimental designs.

The debate between Carey and Spelke exemplifies these aspects of co-analysis. In the area of conceptual development, answers to fundamental questions remain wide open. But despite exploring unmapped territory, Carey and Spelke create a context within which their positions can be rigorously appraised; from the perspective of each position in the debate, the other position is examined and challenged. The history of the Carey/Spelke debate reveals instances of constructive criticism. Also, it is this context of technical cross-evaluation that has prompted these two researchers to consider the potential for their positions to be productively amalgamated.

Co-Composition: Parts of one debate node can play important roles in the composition of its rival debate nodes; rival nodes co-compose. One way this occurs is as a position becomes partially defined by its place in an ongoing debate. For example, the specific ways that experiments are designed, claims are posed, and a research agenda is set, can be influenced by a position's rivalry with others. Another way co-composition occurs is as a position adopts some of the content of rival positions. For example, a position could appropriate data, experimental techniques, or even some of the claims of rivals. In this language, the position becomes partially 'composed' by the claims or data it has borrowed.

Examples of co-composition occur in Spelke and Carey's debate. As the debate escalates through *Phase #1*, these positions co-compose to a greater and greater degree. Planting the seeds to create the debate, Spelke claims that any position which includes conceptual change (such as Carey's) necessarily includes a denial of her own claim: the core knowledge thesis. Carey accepts this interpretation of her own work and challenges Spelke's claims. The arguments and counterarguments that ensue bring each researcher to fine tune her own claims. Part of each researcher's project during *Phase #1* becomes the denial and analysis of the other's work, and the countering of the other's criticisms and data. Each researcher's work thus comes to play a significant role in the makeup of her opponent's account, and her opponent's plays a significant role in her own. Their concentrated cross-analysis brings these researchers to discover gaps within their own positions, and to collaboratively work to fill those spaces with a combined theory.

The importance of the debate evolution thesis—my claim that debates often change in important ways through their history—is highlighted by the case of Carey and Spelke's debate. When one is first introduced to this debate, one might be tempted to ask "but who ends up winning the debate, Carey or Spelke?" The analyses of this dissertation reveal the hastiness of this question; it cannot be answered in the terms it is asked. It is not the case that one position has emerged victorious over the other, and that a historian of science ought to focus on the victor's line of research. The history of this research instead reveals that its terms have changed as the debate has progressed. In this example, we see mutual exclusivity change, the positions combine, and their relationships to other positions (such as Gopnik's) adjust. But this is just one example of how a scientific debate may change over time. Scientific debates can 'evolve' in many ways.

3. Debate Evolution

What does it mean for debate traditions evolve? To explore this question, it is helpful to first review a fictional and overly-simple debate tradition. This bare-bones example instantiates the concepts developed so far and offers some new ones. In the first subsection below, I present a fictional debate tradition called 'the paw/hoof debate.' In the second subsection, I define 'debate evolution.' In the third, I distinguish between the 'primary debate' of a tradition, and a 'branch debate.'

3a. The Paw/Hoof Debate Tradition

When a scientist claims that a scientific object of study (call it x) has some quality A, and another scientist claims that x has quality B, it does not follow that x *must* have either quality A or B. It is possible that both scientists are wrong, and x has neither A nor B. It could be the case that both scientists are correct, i.e. that x has both qualities A and B. (In a way, Carey and Spelke had concluded something like 'x has both qualities A and B' by merging their theories and claiming that conceptual development occurs both through enrichment and conceptual change.)

A debate may emerge when there is reason to investigate the option that x has either quality A or B, and not both. For example, imagine scientists studying the topic 'dogs.' A first line of research takes this position: dogs have tails. Where D = 'is a dog' and T = 'has a tail,' this could be represented $(x)(Dx \supset Tx)$. A second line of research takes the position: dogs have fur. Where F = 'has fur,' this could be represented $(x)(Dx \supset Fx)$. There would be no straightforward reason to see the Tx position and the Fx position to be opposed; tails and fur are body parts which serve different functions. If instead the two positions regard 'paws'(P) and 'hooves'(H), there seems reason to engage in debate. The position $(x)(Dx \supset Px)$ may challenge the position $(x)(Dx \supset Hx)$, and vice versa; the terms 'tails' and 'fur' do not refer to the same category of features. 'Paws' and 'hooves' do. 'Paws' and 'hooves' both relate to the thing at the end of an animal's leg, that is, types of animal feet. There also seems to be some tension between them. A debate could emerge between the positions Px and Hx.

A debate tradition consisting of only these elements has a simple structure: two rival debate nodes, Px and Hx. Their rivalry exists in terms of a central question, or 'problem hub,' regarding what sort of feet dogs have. This simple fictional tradition could be charted on a debate tree diagram. See Figure 2.4.

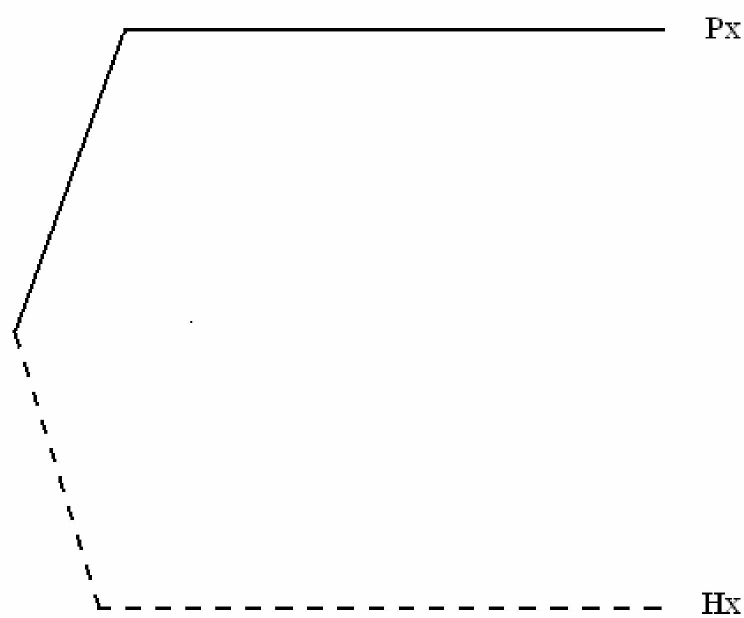

Figure 2.4. In this tree diagram of the paw/hoof debate tradition, the solid line represents the Px node of the debate, and the dotted line represents the Hx node.

However, as the paw/hoof debate tradition advances, and those working within the rival nodes produce new findings and arguments, this simple structure may change.

For example, imagine that as time passes the members of the debate further and further realize that dogs do in fact have paws and not hooves (i.e. that Px is correct rather than Hx). In this case, those researching Hx may decide to shift their focus and attempt to determine whether *both* Px and Hx are somehow correct. This debate has been popular and productive. Many versions of these positions have been considered by the Px and Hx research teams, and also by outside observers such as science reporters, textbook writers, and others. Let us say that simple versions of a combination of Px and Hx have turned out to be false (e.g. that dogs have paws on their front legs and hooves on their hind legs).

The Hx research team has something else in mind. They abandon the pursuit of the simple options of Px, Hx, and the combination of both, and instead grant that Px is correct. Next they explain that Px is the *primary* mechanism through which a dog walks, and wonder if Hx may somehow be a *secondary* mechanism. They suggest that perhaps there are miniature hoof-like structures that can be found within dog paws. This sounds unreasonable to us not living in this fictional world. But remember that in this fictional world the Hx researchers have had at least some reason to continue advancing their position for so long. Perhaps with time, this new version of their work will explain some of their own previous findings. Perhaps not. Either way, the debate which has provoked the discovery of much new knowledge on the topic of dog feet can continue. This is a form of debate maintenance. If the Hx researchers do not altered their position, the debate between their node and the Px node may not remain productive.

What should the Hx research team name this new version of their position? Many practical considerations influence this decision. It could be given a new name entirely since it is indeed a new node of this debate, one which could be held against both Px and the original version of Hx. Yet this new position does rely on the evidence gained through a history of research on the Hx position, and is being advanced by many of the very same researchers who once championed Hx. Let us say that they decide to call the new position 'Hx2.' Internal conflicts, or 'branch quarrels,' may occur between loyalists to the original Hx position and those advancing Hx2. Also, moments of these quarrels could legitimately 'jump tiers' and have effects on the larger debate against Px.

This new debate configuration raises another question: what effects do the changes occurring on the Hx side of the debate have on the Px node? Over time, the Px position has come to (implicitly or explicitly) include clauses that stretch beyond its original simple formulation. For example, this position has come to be regarded as stating more than simply $(x)(Dx \supset Px)$. It has come to also include the denial of the Hx position, stating: $(x)[Dx \supset (Px \bullet \sim Hx)]$. The two positions have thus become co-composed; part of the definition of each is composed by its participation in the debate. Changes may need to be made on the Px side of the debate to account for Hx2. A new position, call it 'Px2,' could be developed to explicitly challenge Hx2. These adjustments to the Px node to account for changes in the Hx node are another example of a form of debate maintenance. It is also possible that some of those defending the traditional Px position may not agree with the changes to Px that come with challenging Hx2. A branch quarrel internal to the Px node could develop between those advancing Px2 and those defending a more pure version of the Px position.

These changes to the paw/hoof debate tradition regarding the Hx2 and Px2 positions can be charted on the tree diagram. With the x-axis marking time, the relative emergence of the Hx2 and Px2 positions can be mapped. See Figure 2.5.

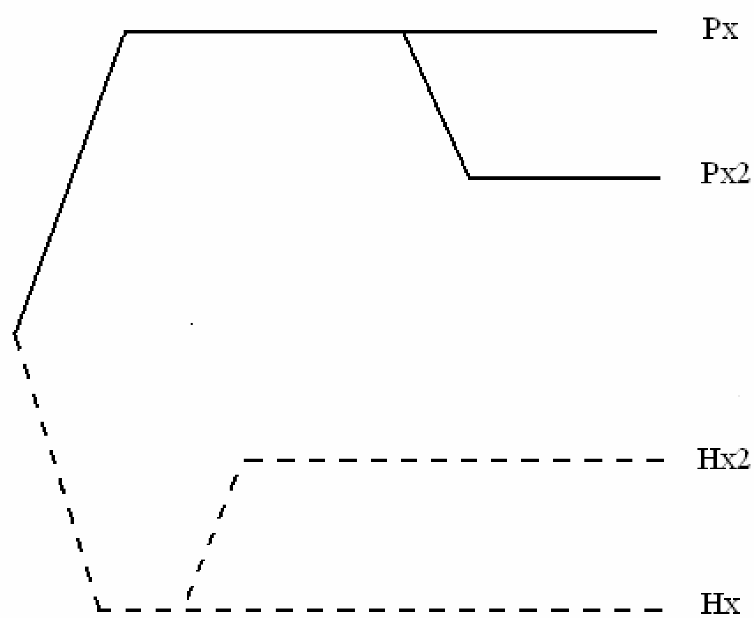

Figure 2.5. In this tree diagram of the Paw/Hoof debate tradition, the solid lines represent the Px node, including the Px2 position. The dotted lines represent the Hx node, including the Hx2 position.

Of course many further changes are possible for the paw/hoof debate. A novel position may enter the debate. Perhaps the new position is significantly different from the Px and Hx nodes, addressing slightly different issues, and utilizing different experimental techniques and technologies. Imagine that this new position regards the issue of dog prints in the snow. Those advancing the new work attempt convince the relevant interlocutors that the snow print evidence bears upon the paw/hoof debate. This new work may combine with the Px node when it is observed that dog prints very much resemble the prints of other animals whose paw structure we know much better.

Maintenance efforts may be required of those working in the Hx node to address this new kind of data.

In chapters three and four, I investigate two entire debate traditions, one from the field of neurobiology, and one from the field of developmental psychology. These two case studies reveal discussions from science that resonate with many of the moves present in the fictional paw/hoof debate. In the next subsection, I consider what it means for a debate tradition to evolve.

3b. Shifting Structures of Debates

I use the term *debate evolution* to refer to the changes that occur to the structures and relationships of a scientific debate tradition over time. This refers to shifts in the composition or the number of nodes in the tradition. This also refers to changes in relationships between rival nodes of the tradition.

The fictional paw/hoof debate tradition provides examples. One instance of debate evolution occurs as the Hx research team, reacting to the continuing success of Px, changes their position in a significant way by offering the Hx2 position. The paw/hoof debate cannot proceed in the way it had after Hx2 emerges. The structure of the debate has changed. There are now two positions within the Hx node: Hx2 and the original version of Hx. The relationship between the Hx and Px nodes also changes. Hx2 does not completely deny Px in the way that Hx had; Hx2 holds that both the Px and Hx accounts are true.

The notion of 'debate maintenance' is an important aspect of debate evolution. As one node advances or adjusts, rival nodes may need to engage in maintenance efforts to keep the debate coherent and active. Since rival debate nodes are deeply interrelated, changes to one node can have effects on others, and can have effects on the relationship itself. For example, in the paw/hoof debate, the Px researchers engage in maintenance efforts as they react to the emergence of Hx2. With the appearance of Hx2, the relationship between the rival nodes changes; the Hx researchers now argue against the Px position in a new way. The debate evolves as the Px researchers respond to this change.

Lakatos's comments regarding changes to the auxiliary claims of a research programme provide an instructional contrast to the notion of debate maintenance. He claims that a research programme can protect its core claims from attack by sacrificing some of its auxiliary claims (the auxiliary claims thus form a 'protective belt'). A programme may drop or alter some of these auxiliary claims to account for the progress of rivals. A similar point is articulated by the notion of debate evolution through debate maintenance. However, as argued above, Lakatos's concepts do not address the relationships between rival programmes with sufficient detail. The examples I explore here and below reveal how those defending a position in a scientific debate do more than simply alter or drop some claims in response to the motions of rivals; debate nodes, and the relationships between them, can evolve in substantial ways.

The shift from Hx to Hx2 in the paw/hoof debate again provides an example. Lakatos's account could correctly cast this shift as a sacrifice made by a degenerating programme in the face of a progressing rival. However, his account cannot describe the changes that occur to the relationships between these rival positions; it cannot completely describe the debate's evolution. Since Hx has taken a different form, the Px researchers are not able to argue against it in the same way they always had. The manner in which the debate is conducted has changed. Where the paw/hoof debate once was constituted as a dispute between Px and Hx, it now concerns whether or not Hx is also correct alongside Px. In the shift from Hx to Hx2, the Hx researchers work to maintain the stability of debate by reasserting their position as a viable contender. The Px researchers then work to maintain the coherence of the debate by adjusting their claims to now argue against the new version of their rival's position.

In chapter five, I expand on the notion of debate evolution. There I consider the ways that debate evolution has occurred in the case studies explored in chapters three and four. The details of these case studies make it possible to articulate a specific kind of debate evolution: changes to the mode of scientific debate. A change in a debate's mode refers to a change in the manner of the rivalry between the positions of a debate. The change to the mode of the paw/hoof debate that occurs with the addition of Hx2 is just one abstract example. Armed with the details of the case studies of the following chapters, in chapter five I articulate a number of different particular modes a scientific debate can take up at a given time in its history. Evolution is then studied in terms of a scientific debate's shifts between modes over time.

There are advantages and disadvantages to the use of the word 'evolution' to refer to the changes that occur to debate traditions over time. One advantage is that the term connotes a sense of change over a particular history. The concrete temporal condition of scientific debates is emphasized. Another advantage is that it connotes a sense of many individuals that interact with one another, and that are also shaped by their environment. This emphasizes the fact that the different individual nodes of a scientific debate advance both by interacting engaging with rivals, and through their research of the world. However, a disadvantage is that the term refers to natural section. I do not intend to reduce scientific debate to a conception of 'survival of the fittest.' I specifically do not intend to associate scientific debate with images of battles, and destruction of rivals. I wish to explicitly distance the notion of 'debate evolution' from these aspects of the term 'evolution.'

Chapters three and four consist of case studies of ongoing debates in science. In chapter five, I show how these case studies conform to the structure of scientific debate traditions defined here. The notions of 'primary nodes,' 'branch quarrels,' 'debate evolution,' 'obligatory debate nodes,' 'debate maintenance,' and 'the problem hub,' will all be instantiated by the concrete details of these studies. This sets the context in chapter five for an account of the particular ways that debates may evolve.

Notes for Chapter 2

1. My term ‘obligatory debate node’ is an adjustment of Latour’s notion of the ‘obligatory passage point’ reviewed in section three of chapter one.

2. For a cutting critique of the use of the metaphor between conceptual development in childhood and the progress of concepts in the history of science (including, and especially, Carey’s use) see Deanna Kuhn’s article “Children and Adults as Intuitive Scientists” (1989). For Carey’s response, see (Sodian, Zaitchick, and Carey, 1991). For an endorsement of the use of the child-as-scientist metaphor from a philosopher of science (including its use by Carey), see Philip Kitcher’s article “The Child as Parent of the Scientist” (1988).

3. In a recent quote, Spelke’s sums up the nature of her debate with Carey. In a piece in *The New Yorker* in which Spelke’s career and body of work are reviewed for a lay audience, while recounting the history of Spelke’s academic posts the writer explains, “in 2001, she accepted an offer from Harvard, in part so that she could work along side the cognitive psychologist Susan Carey, with whom she enjoys ‘productive disagreements’” (Talbot, 2006: 98).

Chapter 3: The Synaptic Vesicle Debate

In this chapter, I review the details of an ongoing debate in the field of neurobiology. Researchers in this field investigate the physical structure of neurons. The particular debate I focus on concerns the nature of neurotransmission, the process through which neurotransmitters are released into the synapse between neurons. Neurobiologists generally agree that neurotransmission occurs through the action of tiny organelles in a neuron's terminal called 'synaptic vesicles.' Synaptic vesicles contain neurotransmitter and release this cargo into the synapse by fusing with the terminal membrane. Despite agreement on this general feature of neurotransmission, vociferous and long-standing controversy over the details of this process has developed. I refer to this controversy as *the synaptic vesicle debate*.

In the first section of this chapter, I review the history of research on synaptic vesicles, exploring how it has provided the context for this debate to emerge. In the second section, I introduce the two major positions of this debate and review the original version of each. The third section addresses the experimental techniques, instruments, data, and claims that make up the content of this debate. In section four, I review the current state of the debate; in the last decade, the two main positions have changed in many ways, from the content of their positions to the methods of their research. In section five, I summarize the history of this debate, taking into account the various changes that have occurred over time.

The synaptic vesicle debate is an example of a 'scientific debate tradition.' In chapter five, I return to this study and show how the definition of the scientific debate tradition fits the details of this case study. There I apply the concepts developed in chapter two (e.g. debate nodes, maintenance, problem hub, primary and branch quarrels) to the features of the synaptic vesicle debate. In that chapter I also analyze the particular concrete ways this debate tradition has evolved, reflecting upon the history provided below.

1. From The Vesicle Hypothesis to The Synaptic Vesicle Debate

The debate I analyze in this chapter opens as another comes to a close. The story begins with the confirmation of a once-controversial theory known as 'the vesicle hypothesis.'

Nobel Prize winner Bernard Katz first proposed that 'synaptic vesicles' are essential to the process of neurotransmission. Synaptic vesicles are tiny, clear, spherical organelles that reside in a neuron terminal and are filled with neurotransmitter. Neurotransmission occurs when these vesicles fuse with the terminal membrane, thus releasing their contents out into the synapse. Katz's ground-breaking hypothesis began with his detailed observations of the synapse (e.g. del Castillo and Katz, 1955; Katz, 1971).[1] He realized that neurotransmitter is released into the synapse in tiny portions which he dubbed 'quantal' units. John Heuser, a major figure in this research, explains,

> Katz was the first person to observe tiny "blips" at the synapse…
> Correlating these electrical blips with the tiny packages of membrane

[1] Endnotes for this chapter are found on pages 156-159.

> (“vesicles”) that had recently been discovered by electron microscopy inside the synapse, Katz realized that his blips couldn’t be individual “hits” of single transmitter molecules on the postsynaptic membrane. He surmised that they must be multimolecular “packets” of nearly uniform size (thus, the term “transmitter quanta”) released by discharge of individual synaptic vesicles (Heuser, 2003, 1248).

This theory of neurotransmission is called ‘the vesicle hypothesis.’ Today, the vesicle hypothesis is widely considered confirmed and has become the standard understanding of neurotransmission. In its day, however, the hypothesis was regarded as quite controversial and was forced to contend with other theories, for example the possibility that neurotransmitter enters the synapse by traveling through selective gates.

The debate I explore in this chapter begins where controversy over the vesicle hypothesis ends; while neurobiologists today agree that synaptic vesicles are responsible for neurotransmission, they disagree about the nature of these tiny organelles. But before this debate can be introduced, it is necessary to first review some of the basic features of neurotransmission.

The term ‘exocytosis’ refers to a secretory vesicle fusing with a membrane to release its contents through to the other side. ‘Synaptic vesicle exocytosis’ is the term for the process through which synaptic vesicles each deposit their quantum of neurotransmitter into the synapse by fusing with the terminal membrane.

A delicate electrochemical balance exists between the inside and outside of a neuron. Slight changes in the electrochemical composition outside or inside the cell trigger neurological events. For example, when a neuron floods a synapse with neurotransmitter, this change to the substance outside of the terminal of the neighboring neuron causes an event to occur in that neighboring cell. The makeup of the cell membrane is an important factor in the maintenance of this electrochemical equilibrium.

The process of synaptic vesicle exocytosis alters the makeup of the cell membrane by increasing its surface area through the addition of the fused synaptic vesicles. The reinternalization of fused vesicles is important for maintaining the membrane's normal surface area, and thus also to the neuron's electrochemical balance. The term 'synaptic vesicle endocytosis' refers to the process through which a synaptic vesicle buds and detaches from the membrane to return into the cell. The term 'vesicle recycling' is also sometimes used to refer to the process as a whole: neurotransmission through exocytosis, and the return of vesicles into the cell through endocytosis.

For the last thirty years, debate has raged over the details of both synaptic vesicle exocytosis and endocytosis, and the dispute continues today. I refer to this controversy as *the synaptic vesicle debate*.

2. The Synaptic Vesicle Debate: The Heuser Model vs. The Ceccarelli Model

There are two major positions in the synaptic vesicle debate: the Hueser model and the Ceccarelli model. The debate begins in the same moments that the vesicle hypothesis became conclusively confirmed; the two major positions of the debate emerge in the very work that proves the vesicle hypothesis (Heuser and Reese, 1973; Ceccarelli et al., 1973). These two articles, appearing in the same issue of the same journal, are often the two cited when one either makes quick reference to the confirmation of the vesicle hypothesis, or to the two positions of the synaptic vesicle debate.[2]

John Heuser, T. S. Reese, and their colleagues advance 'the Heuser model,' the most widely-accepted interpretation of synaptic vesicle exocytosis and endocytosis (e.g.

Heuser and Reese, 1973; Heuser et al., 1979; Heuser and Reese 1981; Miller and Heuser, 1984; Heuser, 1989a). The Heuser model holds that a vesicle collapses completely into the cell membrane as it releases its neurotransmitter during exocytosis. A compensatory endocytotic event is understood to accompany each instance of exocytosis. This involves a new synaptic vesicle budding and separating from the membrane at a spatial location on the membrane separate from the site of exocytosis (see Figure 3.1). Heuser and his colleagues also understand endocytosis to occur at a temporally separate moment, often several seconds after exocytosis. This view of exocytosis and endocytosis is also sometimes referred to as 'the classical model.'

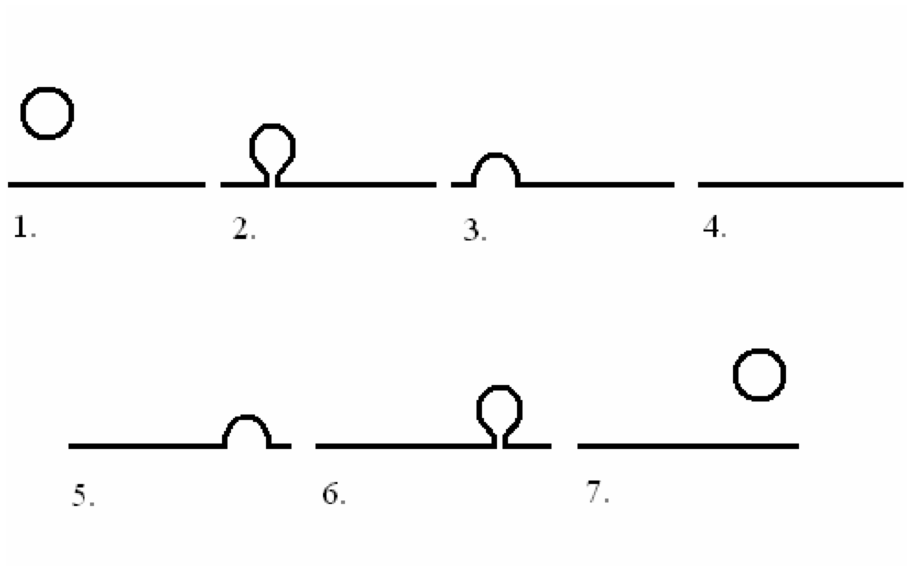

Figure 3.1. This drawing depicts the general processes of exocytosis and endocytosis in terms of the Heuser model. (1) A vesicle close to the membrane. (2) The vesicle fusing with the membrane as neurotransmission begins. (3) The vesicle collapsing and flattening out into the membrane during exocytosis. (4) While steps 1 through 3 take place in a matter of milliseconds, several seconds transpire before synaptic vesicle endocytosis begins. (5) A new dimple forms at a location on the membrane separate from the site of exocytosis. (6) The vesicle buds and pinches from the membrane. (7) The process of endocytosis results in the formation of a new vesicle.

Figure 3.2 is the diagram of the model of exocytosis and endocytosis by Heuser and Reese in their landmark 1973 article. This general diagram structure becomes a ubiquitous tool for displaying conceptions of the process of synaptic vesicle recycling. The diagram is built on, updated, and altered many times by these researchers and others.

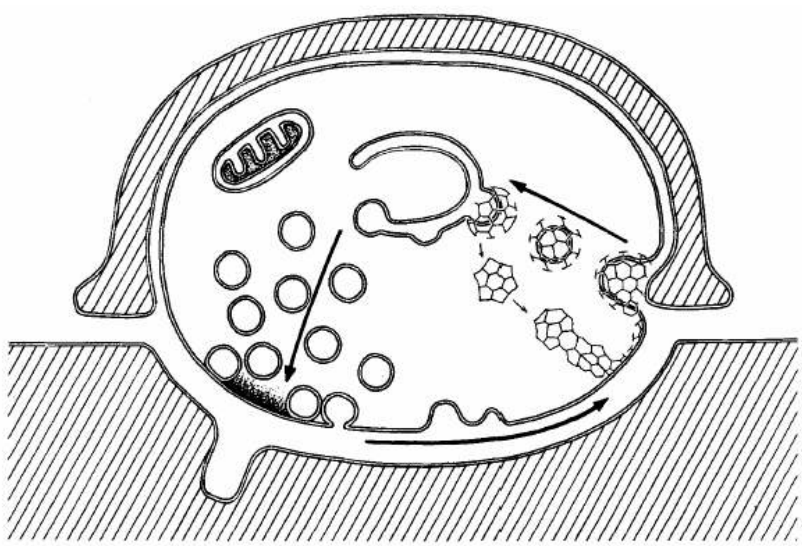

Figure 3.2. This is the diagram of the cycle of exocytosis developed by Heuser and Reese in the original formulation of the Heuser model. On the left, vesicles perch at the nerve terminal membrane. One can be seen undergoing exocytosis. On the right, endocytosis occurs. The soccer-ball-like lines accompanying the vesicles undergoing endocytosis on the right, and also the intermediate docking station structure in the top center, refer to processes discussed in detail in subsection *4a*. (Heuser and Reese, 1973, 339). Reproduced from *The Journal of Cell Biology*, 1973, 57: 315-344. Copyright 1973 The Rockefeller University Press.

The major alternative to the Heuser model is the view of exocytosis held by Bruno Ceccarelli and his students (e.g. Ceccarelli et al., 1973; Ceccarelli et al., 1979; Torri-Tarelli et al., 1985; Ceccarelli et al., 1988; Fesce et al., 1994). In the 'Ceccarelli model' synaptic vesicles are understood to fuse temporarily with the terminal membrane. Next they detach again there in the same location (See Figure 3.3). In this view, exocytosis and endocytosis are tightly coupled, both spatially and temporally. In contrast to the Heuser model which understands vesicles to be destroyed and newly created during the process of neurotransmission, Ceccarelli and his students hold that a vesicle does not lose its identity. This view of exocytosis and endocytosis is sometimes also referred to as 'kiss-and-run.'

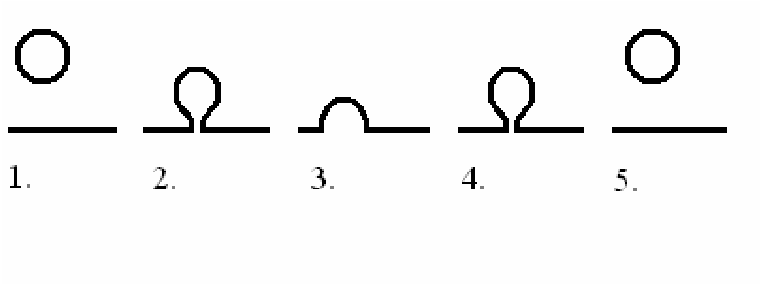

Figure 3.3. This drawing depicts the general process of exocytosis and endocytosis in terms of the Ceccarelli model. In this view, exocytosis and endocytosis are part of the same continuous process, occurring at the same time and at the same location on the membrane.

The debate between these two positions has raged over the last thirty years and continues today. For example, in the introduction to a memorial symposium honoring the late Ceccarelli, the authors refer to Heuser as Ceccarelli's "arch nemesis" (Clementi and Meldolesi, 1989).[3] While evidence has piled up in support of the Heuser model over time, advocates of the Ceccarelli model continue to advance this position. In general, the recent strategy of researchers advancing the Ceccarelli model has changed from arguing that the Heuser model is incorrect to arguing that the Ceccarelli pathway *also* exists (a move analyzed in detail below). Current moves in research on the Ceccarelli model include investigations into the possibility that Ceccarelli-style exo-endocytosis occurs though the mechanism of a molecular structure called a 'fusion pore.' Current research on the Heuser model includes the well-supported theory of 'clathrin-mediated endocytosis,' which holds that endocytosis occurs as budding and pinching vesicles are covered in a cage-like coating by substance called ‘clathrin.’ Both of these current research directions are explored below in section four, including their relationships to the original formulations of the Heuser and Ceccarelli models.

The synaptic vesicle debate regards disagreements concerning many technical issues. Researchers debate about the amount of pressure that a vesicle in the process of fusing can withstand. The nature of the electrochemical balance between the inside and outside of the neuron is under dispute. The duration of the process of endocytosis is an important point of contention. There is disagreement over the effects that adding vesicle material to the membrane during exocytosis has on the cell. Debate also occurs over the comparative ‘efficiency’ or ‘simplicity’ of the two rival models.

However, the *central issue* of the synaptic vesicle debate regards the interpretation of images of vesicles fused to the membrane. A history of technological innovations enables neurobiologists to freeze nerve samples while they are in the process of neurotransmission, thus capturing vesicles in the process of exocytosis. Electron microscopy enables pictures of these fused vesicles to be created. However, instead of bringing a close to the debate, proponents of the Heuser and Ceccarelli models disagree about how these images should be interpreted.

In the next section, I review some of these images and imaging techniques, and examine the competing interpretations offered by each side of the debate.

3. Images of Fused Vesicles Frozen in Time

The original formulations of the Heuser and Ceccarelli models are based on each research group's interpretations of electron micrographs of stained, thin-sliced sections of the nerve terminal which had been labeled with chemical markers such as horseradish peroxidase and dextran (Ceccarelli et al., 1973; Heuser and Reese, 1973). Rather than review the original thin-sliced images central to the work of the early 70's, I focus here on debates over images created through 'freezing' procedures.

Some of the most influential images of the synaptic vesicle debate are created by procedures which instantly freeze a sample; the specific moment of the biological process occurring in the sample at the time of freezing can then be studied under the electron microscope at leisure. But rather than resolve the synaptic vesicle debate, proponents of the Heuser and Ceccarelli models develop rival interpretations of these images. Each side of the debate claims the images support their own position. As Heuser and Reese explain, "Other researchers have witnessed some of the same structural changes at the frog neuromuscular junction or at other synapses but have reached different conclusions" (Heuser and Reese, 1981, 577).[4]

In subsection *3a,* I explore 'quick-freezing,' a process which enables researchers to freeze samples at a precise desired moment. In subsections *3b* and *3c,* I consider two techniques used for preparing neurons for viewing under the electron microscope: freeze-fracture and freeze-substitution. The 'freeze-fracture' technique allows the inner or outer surface of a membrane to be laid out and viewed. The 'freeze-substitution' technique instead allows for the creation of thin-sliced side-views of the cell. Subsections *3d* and *3e* regard two examples of debates over these images: a debate regarding how the process of neurotransmission should appear over time, and a debate over the effects of K^+ on synaptic vesicle fusion.

3a. Quick-Freezing: Introducing the Slam Freezer

The technique called 'quick-freezing' is important to the history of the synaptic vesicle debate. Quick-freezing is accomplished through the use of a device called the 'cryopress' (sometimes nicknamed the 'slam freezer'), invented by Heuser and his colleagues. With this machine, a researcher can freeze a sample at a precise instant. For example, a sample can be quick-frozen at a specific moment after a nerve has been stimulated and neurotransmission has begun (for articles discussing the details of the quick-freezing procedure see Heuser, 1978; Heuser et al., 1979; Heuser, 1981). This is accomplished by using the device to drop (or 'slam') a sample down onto a copper block that has been super-cooled with an extremely cold liquid (see Figure 3.3).

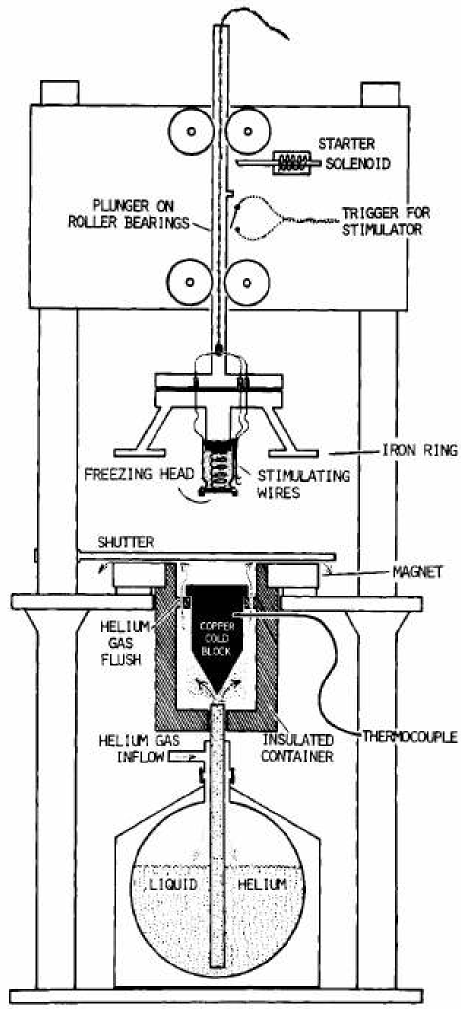

Figure 3.4. This is a diagram of the cryopress appearing in Heuser et al., 1979, 277. Notice the super-cold copper block in black, and the tank of cooling liquid beneath it. Reproduced from *The Journal of Cell Biology*, 1979, 81: 564-580. Copyright 1973 The Rockefeller University Press.

The quick-freezing procedure opens several doors for neurobiological research. The primary freezing technique used before the invention of quick-freezing is called 'quenching.' It involves submerging a sample into cold liquid. But this technique fails to freeze samples fast enough to avoid many specific problems. Heuser explains, "electron microscopists invariably report that ice crystals distort everything in quench-frozen tissue" (Heuser, 1981, 64). Important to the synaptic vesicle debate in particular is that the quick-freezing technique makes it possible to immediately freeze a sample at the precise desired moment. Heuser explains that, "the process of synaptic transmission requires only a few milliseconds, while chemical fixatives require many seconds or minutes to diffuse into a tissue and cross-link its constituents. To avoid such fixation, we built a machine to *quick freeze* nerves" (1981, 65). He reports that quick-freezing, "leads to an exceedingly abrupt if not almost instantaneous arrest of cell function, so it ought to come much closer to identifying the natural lifetime of an event such as synaptic vesicle exocytosis" (Heuser, 1989a, 1063). It has been the quick-freezing technique that has enabled neurobiological researchers to find a precise temporal correlation between the fusion of synaptic vesicles to the terminal membrane and the release of neurotransmitter, thus strongly confirming the vesicle hypothesis.

Of course a limitation of the quick-freezing technique is that since the sample is killed by the freezing procedure and subsequent dissection, each image necessarily comes from an entirely different sample. One motionless image cannot portray all of the aspects of a dynamic process such as exocytosis. To confront this limitation, Heuser and his colleagues create images at different millisecond intervals after stimulation (each, of course, from a different sample). Heuser and his colleagues use this temporal series of images to infer the structural changes that occur to the membrane as vesicles fuse to it while releasing their neurotransmitter (Heuser, et al., 1979; Heuser and Reese, 1981). They report that instances of fused vesicles appear 3ms after stimulation. The vesicle fusions then peak in numbers at 5-6ms, and slowly decrease for the next 50-100ms.

The Ceccarelli school contributes to this effort by creating a quick-freezing device of their own (Torri-Tarelli et al., 1985). Ceccarelli's students later summarize, "The quick-freezing technique, which was originally developed by Heuser et al. (1979), and subsequently improved by Ceccarelli and coworkers (Torri-Tarelli et al., 1985), has provided the best evidence for a temporal correlation between neurotransmitter release and fusion of synaptic vesicles with the axolemma" (Valtora et al., 1989, 1026). These researchers adjust the findings of Heuser and his colleagues, reporting that vesicle fusions begin to appear 2.5ms after stimulation (more on this below in subsection *3d*).

3b. Freeze-Fracture

The technique called 'freeze-fracture' makes it possible to create images of the outside or inside surface of a membrane. Freeze-fractured images are among the most important and most disputed elements of the synaptic vesicle debate. The technique yields images of a large surface of the membrane. If the sample is frozen at the correct moment, images of the membrane surface reveal vesicle fusions.

This technique involves first freezing a sample (for example, through the quick-freezing procedure just reviewed). The frozen sample is cracked and broken (i.e. 'fractured') along specific layers of the membrane so that the surface can be laid out and examined. A metal is then sprayed on the desired surface to create a non-translucent cast. A number of different spraying procedures exist for this purpose, each intended to create specific shadowing effects. Finally, the sample is melted off and the remaining metal cast is studied under the electron microscope.

When a sample is quick-frozen just after stimulation, a freeze-fractured image of the outside surface of the terminal membrane reveals many divots in this plane.

Neurobiological researchers refer to these crater-like structures as ‘dimples,’ and understand them to be synaptic vesicles fused with the membrane (Figure 3.5). Much information is contained in these images. Heuser explains that with freeze-fracture, “Questions of where and how many membrane fusions were occurring finally became experimentally accessible” (Heuser, 1989b, 1068). The dimples are found to occur in specific regions of the membrane dubbed ‘active zones.’ These images of dimples in the terminal membrane are considered the key piece of evidence in support of the vesicle hypothesis since the dimples only appear in samples frozen during neurotransmission. Heuser summarizes, “With this [quick-freezing] technique, we could deliver one single shock to the nerve and freeze it at the peak of the postsynaptic end-plate potential it produced. Then, we could examine the nerve in the electron microscope and count up all the synaptic vesicles that were caught in the act of discharge, and compare their numbers with the quanta that would have produced an end-plate potential of that amplitude. In that way, we managed to show that synaptic vesicle exocytosis occurs at exactly the same moment as transmitter release” (Heuser, 1978, 81).

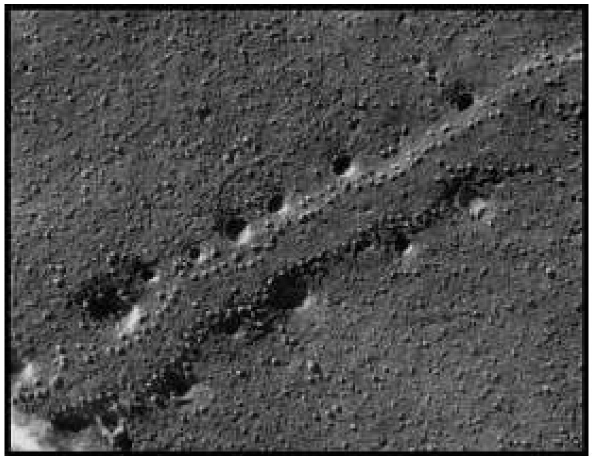

Figure 3.5. This is an example of a quick-frozen, freeze-fractured image of a small portion of the outside surface of the terminal membrane, complete with vesicle fusions at the active zone. Permission received from John Heuser.

Though there is broad agreement that these images provide confirmation of the vesicle hypothesis, there remains disagreement in terms of what I refer to as the synaptic vesicle debate: the debate between the Heuser and Ceccarelli positions. Instead of resolving this debate, these images provide a major context through which the debate takes place. Though both the Heuser and Ceccarelli schools agree that these images reveal synaptic vesicles fused with the membrane, they disagree about what is happening to the vesicles in the picture. The argument regards what exactly the vesicles have been caught in the middle of doing, and what each image would look like had it been created an instant later.

Heuser and his colleagues interpret these images to display vesicles undergoing exocytosis in terms of the Heuser model. They take each dimple to be a synaptic vesicle in the process of widening and flattening out into the membrane (e.g. Heuser et al., 1979; Heuser and Reese, 1981; Miller and Heuser, 1985). Ceccarelli and his students instead interpret these images to show vesicles in the process of temporarily fusing with the membrane: attaching and then detaching (e.g. Ceccarelli et al., 1979; Torri-Tarelli et al., 1985; Ceccarelli et al., 1988; Grohovaz et al., 1989). In their view, these dimples could be in either the process of opening and releasing their neurotransmitter, or closing as they undergo endocytosis.

3c. Freeze-Substitution

Another important technique for creating images of synaptic vesicles fused with the terminal membrane is called 'freeze-substitution.' With this technique, the ice within a frozen sample is first dissolved with subzero acetone. Next the sample is warmed and stained with metals. Finally it is embedded in plastic so the sample can be sliced into thin sections and analyzed.

Since the freeze-substitution technique yields thin slices of the membrane, a different perspective on vesicle fusions is produced. Consistent with the findings of freeze-fracture studies, freeze-substituted images of the membrane include vesicle fusions only when the sample has been frozen during neurotransmission. The fusions displayed in freeze-fractured images are 'side views' of a synaptic vesicle attached to the terminal membrane (Figure 3.6).

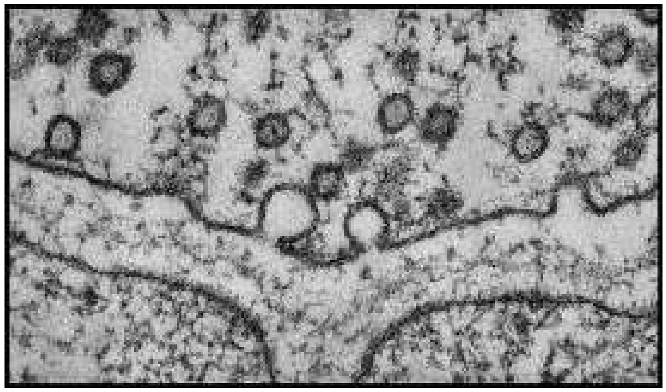

Figure 3.6. This is an example of a quick-frozen, freeze-substituted image of a portion of the terminal membrane, complete with vesicle fusions. Permission received from John Heuser.

In the case of these ‘side views,’ neurobiologists agree that the circular structures attached to the horizontal line are synaptic vesicles fused with the terminal membrane. The term ‘omega shapes’ is sometimes used to refer to instances of rather wide vesicle fusions found in freeze-substituted images. Like the example of the freeze-fractured image in subsection *3b*, there remains disagreement regarding these images in terms of the synaptic vesicle debate. Those endorsing the Heuser model claim that the freeze-substituted images of vesicle fusions display vesicles in the process of exocytosis. They maintain that had the vesicles been able to continue what they were doing, they would collapse into the membrane and flatten out. Heuser and his colleagues expect a side view of a vesicle fused with the membrane to show a wider and wider omega shape. Those endorsing the Ceccarelli model instead interpret these side views to reveal vesicles in the process of temporarily fusing with the membrane, each about to again detach there in the same location.

3d. The Debate Over Fusion Shapes

The synaptic vesicle debate involves many technical controversies. In subsections *3d* and *3e*, I review two examples of technical disagreements with respect to the interpretation of images of vesicles frozen while fused to the terminal membrane. These examples provide important background material relevant to recent moves in the debate reviewed in section four of this chapter. The first of these two examples concerns a debate over how the shape of a vesicle fusion should appear at different moments after stimulation.

In their 1981 paper, "Structural Changes After Transmitter Release at the Frog Neuromuscular Junction," John Heuser and T.S. Reese develop their interpretation of the sequence of events that occur during synaptic vesicle exocytosis, and they present their experimental evidence. As reviewed above, they report that vesicle fusions first appear in images created 3ms after stimulation, peak at 5-6ms, and slowly decrease for the next 50-100ms. Heuser and Reese also investigate the *shape* of vesicle fusions over time. They find that "the vesicle openings enlarged as time passed, although a wide range of sizes occur at every time" (Heuser and Reese, 1981, 570). They add, "At 3-5 ms, when the smallest class of vesicle opening was most prevalent, none were narrower than 20 nm in diameter, and none fractured or etched in an unusual way which might have indicated that they were intermediates in exocytosis" (Heuser and Reese, 1981, 571). Heuser and Reese claim these data obtained from the freeze-fractured images provide confirmation to the Heuser model of synaptic vesicle exocytosis. They explain, "A reasonable interpretation of this rise and fall in the number of vesicle openings, and their tendency to increase in size over time, was that each vesicle opening begins as a small pore in the surface membrane and enlarges until the synaptic vesicle membrane is entirely collapsed into the plasmalemma" (Heuser and Reese, 1981, 570).

This interpretation of the structural changes that a vesicle undergoes during fusion presented in Heuser and Reese's 1981 paper is important to the history of the synaptic vesicle debate for several reasons. First, it represents the backbone of the data that support the Heuser model. Second, in my view, the Ceccarelli school has not offered a complete response to these particular claims. Though the Ceccarelli school has developed a highly refined quick-freezing method, they have not systematically responded to the claim made by Heuser and his colleagues that vesicle fusions tend to widen over time.

In the Ceccarelli school's quick-freezing study, they investigate fusions occurring up to 10ms after stimulation, but no later (Torri-Tarelli et al., 1985). Still, they report that their data add confirmation to their own model. For example, in a later paper, Flavia Valtora, Francesca Torri-Tarelli and others report that in their 1985 study, "no images of vesicles collapsing into the axolemma were observed and the density of vesicle openings was unchanged in preparations quick frozen 2.5, 5 or 10 msec after the stimulus. These findings support the idea that, at least at the early stages in the secretory process, the vesicles can be recovered immediately after their fusion, possibly without even flattening into the axolemma" (Valtora et al., 1989, 1026). The fact that widening and flattening is not seen in the first 10ms is offered as data consistent with the Ceccarelli account. In more recent papers, students of the Ceccarelli school continue to make claims like this based solely on their 1985 study, and with bolder rhetoric.[5]

A striking feature of Heuser and Reese's comments in their 1981 piece is that they take place before the synaptic vesicle debate had become well-established. The authors claim that their findings should settle the debate once and for all. They explain that some of their critics [i.e. the Ceccarelli school], "have argued that we were not witnessing exocytosis [at a specific moment of their studies], but some sort of endocytosis that occurs in the moments after transmitter release." Heuser and Reese continue that "the temporal dissection of the whole process presented in this report, combined with our previous pulse-tracer studies which showed no signs of endocytosis until hundreds of milliseconds after transmitter release (7), will settle this controversy, we hope" (Heuser and Reese, 1981, 569-570). The debate, however, continues today.

The quotes above highlight the fact that the Ceccarelli model has changed in important ways through its history. In its early stages, this model includes the possibility that neurotransmission occurs as a vesicle collapses completely into the membrane during exocytosis, and then undergoes endocytosis by budding and pinching from the membrane at the same location in which it had just flattened out: a sort of reversible fusion. Over time the Ceccarelli model comes to include the possibility that vesicles do not collapse entirely into the membrane, but partially fuse and detach.[6] The contemporary version of this theory, reviewed later in the chapter (subsection *4c*), understands vesicles to temporarily and partially fuse to the terminal membrane through a chemical structure called a 'fusion pore.' It is in writings on this contemporary version of this work that the Ceccarelli model receives its nickname 'kiss-and-run' (more on this later).

The work by Heuser and his colleagues on the shape of fused vesicles over time sets a context intimately relevant to current work. Take as an example Heuser and

Reese's observation that "Apparently, the time resolution of the freezing method used here was not good enough to capture whatever event led up to the 20-nm openings. All we could see was a progressive enlargement of the openings after that time" (1981, 572). These data, both in terms of the timing and the physical details of exocytosis, is important to contemporary 'fusion pore' versions of the Ceccarelli model. Exactly when a fusion pore forms, how long it lasts, and how its physical structure should appear in images, are all burning issues for current research on the Ceccarelli model.

3e. The Debate Over Ectopic Fusions

Another example of a debate regarding images of frozen vesicles revolves around a strange set of findings of the Ceccarelli school in the 1980's. These results still today require complete explanation. This example again involves rival interpretations of the same data. These interpretations set the context for the contemporary accounts of 'clathrin-mediated' endocytosis found in the work of Pietro De Camilli, Kohji Takei and others (to be reviewed in section *4a* of this chapter).

The Ceccarelli school explores the effect of potassium (K^+) on the process of synaptic vesicle exocytosis. Samples which have first been stimulated in a bath of K^+ produce freeze-fractured images of fusions occurring *randomly* all over the membrane surface (not just at the usual 'active zone' fusion cites).[7] They call these randomly-distributed dimples 'ectopic fusions.' Ceccarelli and his students explain, "Our results suggest that K^+ initially activates and later inactivates exocytosis at the active zones, while it slowly activates fusion sites all over the presynaptic membrane" (Ceccarelli et al., 1988, 164). As with previous studies conducted by both the Ceccarelli and Heuser schools that find rough correlation between the number of transmitter quanta released and the number of vesicle fusions found in freeze-fractured images, the Ceccarelli school finds correlation between the total, randomly-dispersed, 'ectopic' dimples and the number of neurotransmitter quanta. They conclude that this correlation shows that all the dimples found in the image should be understood as moments of exocytosis, and that endocytosis should be understood as occurring there at the same time and in the same spatial location of each dimple (Ceccarelli et al., 1979; Ceccarelli et al., 1988; Grohovaz, 1989; and Heuser, 1989b for a response to these claims). They claim, "These data are consistent with the view that endocytosis does not occur as a separate process at short stimulation times, the vesicle being immediately interiorized after a transient opening to the synaptic cleft" (Ceccarelli et al., 1988, 181). In this view, each ectopic dimple appearing in these images portrays a vesicle in the process of temporarily fusing and then detaching.

An important demand on defenders of the Ceccarelli model has been the explanation of the Heuser school's findings that dimples appear away from the active zone several seconds after neurotransmission (e.g. Heuser and Reese, 1981; Miller and Heuser, 1984). The Heuser model interprets these late-forming dimples to be instances of endocytosis. The Ceccarelli school must provide at least some explanation, and they use their ectopic fusion studies to address this issue. They argue that,

> If the occurrence of ectopic vesicle openings were due to the delayed onset of endocytosis, then the density of fusions along the active zones should remain comparable at the different times, as it is the case for the number of quanta secreted during fixation. The observation that the <u>total</u> density of fusions remains relatively constant, while active zone-associated events

> decrease, indicates that both active zone-associated and ectopic fusions represent exocytotic events (Ceccarelli et al., 1988). This result is consistent with the idea that exo- and endocytosis are immediately subsequent steps of a single process (Grohovaz, 1989, 1089-1090).

Put it another way, they suggest that dimples occurring outside of the active zone should be understood as ectopic fusions. The late-occurring dimples which Heuser and his colleagues interpret to be instances of endocytosis, in the view of the Ceccarelli school, should instead be understood as late-onset instances of ectopic fusions.

Another possibility, of course, is that the ectopic fusions are a form of exocytosis that occurs outside of the active zone, and yet one which still requires a compensatory endocytotic event later after neurotransmission. As we shall see in the next section, several major advances occur in terms of understanding the mechanisms involved in endocytosis. The molecular mechanisms involved have been significantly illuminated. It does happen that synaptic vesicles are reinternalized through a process separate from exocytosis. This process however, according to contemporary theories of endocytosis, is one which differs in important ways from Heuser and Reese's original formulation.

Though this important evidence of late-occuring endocytosis has appeared, the synaptic vesicle debate has continued to wage by virtue of essential changes that have been made to the Ceccarelli position.

4. Current Research Directions: Clathrin-Mediated Endocytosis vs. Fusion Pore Theory

Despite the advances reviewed above, and despite the advances reviewed in this section below, the synaptic vesicle debate continues today. However, this debate does not occur in the same terms that had defined it in the past. The Heuser and Ceccarelli models do remain the two major positions of this debate, but they no longer remain identical to the formulations originally developed by John Heuser and Bruno Ceccarelli.

A major shift in our understanding of endocytosis emerges in the 1990's. Researchers discover that endocytosis does in fact occur through a process separate from exocytosis, but does not occur exactly as is posited by the Heuser model. These findings usher in a contemporary era of research focused on uncovering the molecular components of both endocytosis and exocytosis. In subsection *4a*, I review the work of Pietro De Camilli, Kohji Takei, and others, which has substantially increased our understanding of synaptic vesicle endocytosis. In subsection *4b*, I examine how the work of these researchers is considered to be an advancement of the Heuser model.

A major shift in the way exocytosis is researched also occurs during this time. Researchers of the Ceccarelli school and others advance a new version of the Ceccarelli model of exo-endocytosis, positing that a vesicle fuses to the terminal membrane through the mechanism of a chemical structure known as a 'fusion pore.' These researchers bring a variety of forms of evidence to support their fusion pore model. Their efforts, while not yet yielding direct evidence of fusion pore exocytosis, result in an assortment of instances of indirect support and have breathed new life into research on the Ceccarelli model. In section *4c*, I review this fusion pore theory of exocytosis. In *4d*, I examine the ways in which it is considered an extension of the Ceccarelli model.

4a. Clathrin-Mediated Endocytosis and Membrane Invagination

In order to review recent developments in our understanding of endocytosis, a bit more detail of Heuser and Reese's original and developing theory of endocytosis must be addressed:

It is *not* the case (as may have been implied by the oversimplified review of endocytosis presented thus far) that the Heuser model, as originally formulated, portrays the internalization of membrane material to itself result in a fully-formed vesicle (e.g. Heuser and Reese, 1973; Heuser et al., 1978; Heuser and Reese, 1981; Miller and Heuser, 1984). At lease two separate pathways of membrane reinternalization are articulated. Single, synaptic-vesicle-sized spheres are understood to bud and pinch from the terminal membrane while covered in a coating of a substance called 'clathrin.' A clathrin coat appears as a cage-like structure surrounding a vesicle (Figure 3.7).

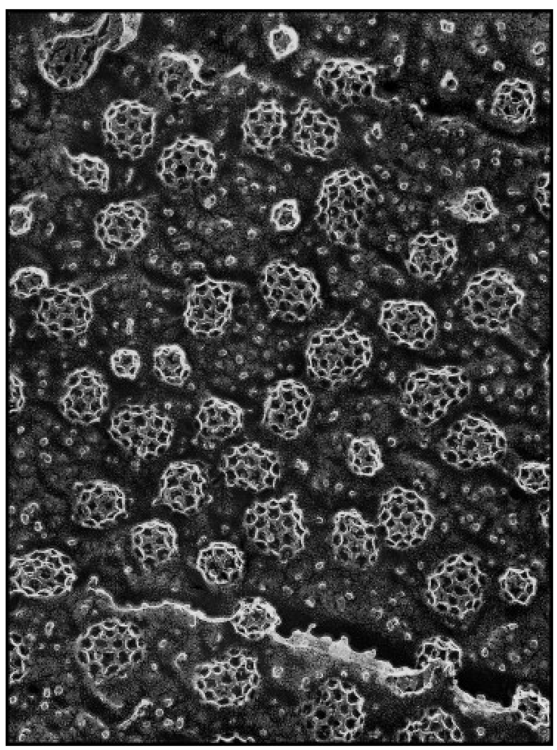

Figure 3.7. Here is an example of a freeze-fractured image of the inner side of the terminal membrane with clathrin-coated pits. Permission received from John Heuser.

Also, deep inward foldings of the membrane occur. These invaginations of the membrane internalize into the cell as large chunks. Clathrin-coated spheres also bud and separate from these internalized portions. The term 'bulk endocytosis' refers to the process through which deep invaginations of the membrane internalize as large chunks.

Heuser and his colleagues further theorize that vesicles shed their clathrin coats and mold together, forming a cistern from which new, fully-formed vesicles finally emerge (see the endosome structure in the top, center area of Figure 3.2).

A major criticism of the Heuser model has been that it represents much too slow a process to account for the intense speed by which vesicle pools are depleted, and thus need to be replaced, during neurotransmission; the Heuser model holds that vesicles must first bud off of the membrane and then dock at an endosome in their journey to become fully-formed units. Defenders of the Ceccarelli model contend that their own, much simpler model yields ready-to-go vesicles at a much faster rate.[8] Ottavio Cremona and Pietro De Camilli explain that the Ceccarelli model, "was first proposed because of the lack of close correlation between the number of clathrin-coated intermediates and the level of nerve terminal endocytotic activity. It provided an attractive explanation for the speed and specificity of SV membrane retrieval under physiological conditions" (Cremona and Camilli, 1997, 323). Heuser and his colleagues, though pushed hard on this issue, hold strong that the evidence shows endocytosis to be able to keep up with the rate of vesicle fusion during exocytosis.

Another challenge for researchers of the Heuser model has been the task of describing the function and sequence of the clathrin-mediation process. Data emerges in the late 80's implying that a budding and pinching vesicle may shed its clathrin coat at a quick rate. A clear and consistent model of the clathrin-mediation process is needed.

Both of these challenges for the Heuser model are addressed in a new version advanced by De Camilli, Takei, and their colleagues. This new understanding of synaptic vesicle endocytosis comes through the efforts of Pietro De Camilli, Kojhi Takei, Ottavio Cremona, Vladimir I. Slepnev, and others (e.g. De Camilli et al., 1995; Takei et al., 1995; De Camilli and Takei, 1996; Takei et al., 1996; Cremona and De Camilli, 1997; Slepnev and De Camilli, 2000; De Camilli et al., 2001; Wenk and De Camilli, 2004; for a supportive review see Morgan et al., 2002). There are two main adjustments to the Heuser model are present in this new version: (1) a new, fully-formed vesicle emerges from the one-step process of a clathrin-coated vesicle shedding its coat, and (2) clathrin-coated vesicles sometimes stalk off of the membrane on a tubular invagination made of a substance called 'dynamin.'

(1) In contrast to the original version of the Heuser model of synaptic vesicle endocytosis, the results of the work De Camilli and colleagues show endocytosis to be a one-step process. They explain, "We suggest a model in which the vesicle cycle involves a single coat-mediated budding reaction mediated by clathrin and dynamin" (Takei et al., 1996, 1248). They reject the understanding that a vesicle must first bud and pinch from the membrane via clathrin-coating, and then must *also* make the intermediate step of docking at an endosome in route to becoming fully-formed. Instead they claim that a complete, new vesicle if formed by the very shedding of its clathrin coat.[9] The one-step, clathrin-mediated vesicle creation occurs both as a vesicle buds directly from the cell membrane, and also as a vesicle buds from the chunks of membrane internalized through

bulk endocytosis. They explain, “The predominance of one of these pathways [regular or bulk endocytosis] over the other may depend on the physiological state of the terminal. After massive nerve terminal stimulation, formation of deep infoldings and then vacuoles may be enhanced to compensate for the large increase in the surface area of the nerve ending” (Takei et al., 1996, 1248). Of course the “large increase in the surface area” they refer to is that of vesicles fusing and collapsing into the terminal membrane during exocytosis as per the Heuser model. De Camilli and his colleagues make these claims on the basis of morphological observations of electron micrographs, and also through analyzing advancements in our understanding of the dynamics and the molecular structure of the terminal membrane.

(2) The second major part of this new understanding of endocytosis is the observation that vesicle internalization often occurs as the clathrin-coated bud extends into the cell on a stalk made of a substance called ‘dynamin.’ Deep, tubular invaginations of dynamin protrude into the cell from the membrane with a clathrin-coated vesicle riding atop. These dynamin invaginations can sometimes branch, internalizing several clathrin-coated buds. At other times, a vesicle may ride atop without retaining a clathrin coat. These clathrin-coated, tubular, dynamin invaginations are observed to extend both directly from the membrane, and also from membrane chunks internalized through bulk endocytosis.

Several of the problems associated with the original formulation of the Heuser model are addressed by these two major advances in the understanding of endocytosis. For example, the quick, one-step character of this new account of endocytosis addresses the challenge that the Heuser model is too slow to explain the rate at which synaptic vesicles are replaced. Members of the Ceccarelli school explain, “Although the rates of such a [clathrin-mediated, Heuser model] pathway initially appeared to be too slow for neurons, later studies have shown that the process is much faster at synapses (time constants between 20 and 30 seconds) than in other secretory cells. Moreover, synaptic vesicles can regenerate directly from the plasmalemma, the trip to the endosome being unnecessary” (Fesce and Meldolesi, 1999, E3).

This one-step version of the Heuser model forms the basis of much of the contemporary work on endocytosis. The current era of research largely investigates the molecular structures of the terminal membrane, synaptic vesicles, and other related parts. This research builds on the sophisticated accounts of the roles of clathrin and dynamin provided by De Camilli, Takei, and their coworkers. Avenues of research include articulating, for example, the ‘SNARE’ complex of proteins in the terminal membrane, its ‘lipid by-layer,’ dynamin building partners, regulatory and signaling mechanisms, accessory proteins, membrane deforming proteins, protein recruitment mechanisms, and many other equally technical aspects of endocytosis.

4b. Is De Camilli’s model of Endocytosis an iteration of the Heuser Model?

A task for this dissertation is to determine the relationship between the Heuser model (in its original formulation) and these new developments in research on endocytosis. Important questions include: Is De Camilli’s model of endocytosis an iteration of the Heuser model? Should this understanding of endocytosis instead be understood to constitute a separate position in the synaptic vesicle debate?

The findings of De Camilli, Takei, and their colleagues are relevant to many issues in the study of endocytosis. Of those, at least two areas of controversy are directly relevant to the debate between the Heuser and Ceccarelli models: (1) these findings directly challenge the 'two-step' aspects of the Heuser model, suggesting instead that vesicles are formed through the 'one-step' process of clathrin mediation. (2) these findings do not correspond to the Ceccarelli model of exocytosis and endocytosis.

(1) In terms of the one-step vs. two-step understanding of endocytosis, it is clear that De Camilli and his colleagues disagree with the original formulation of the Heuser model. The finding that fully-formed vesicles are produced by the one-step clathrin-shedding process directly challenges specific aspects of the Heuser school's account.

In several of their papers, De Camilli and his colleagues explicitly juxtapose the Heuser model and their new findings to make clear their differences with regard to this issue (e.g. De Camilli and Takei, 1996; Takei et al., 1996; Cremona and De Camilli, 1997; De Camilli et al., 2001). This juxtaposition is cast in different ways from article to article. For example, in some presentations these researchers contrast two models of endocytosis, referring to their own model with names such as 'the single-vesicle-budding-step model,' and referring to the original version of the Heuser model with names such as 'the two-step model.' (De Camilli and Takei, 1996; Cremona and De Camilli, 1997; De Camilli et al., 2001). In one instance, it is even suggested that both models could possibly coexist, each serving a slightly different function (De Camilli and Takei, 1996). In another article, however, these researchers present findings as part of a tradition of work on the Heuser (or 'classical') model explaining that, "Based on our results, we propose a modification of the 'classical model' of synaptic vesicle recycling"

(Takei, 1996, 1248). In terms of this issue of the two-step verses one-step formation of synaptic vesicles, De Camilli and his colleagues clearly disagree with the original formulation of the Heuser model. Whether these researchers present their own model as separate from the Heuser model, or as a modification to the classical model, depends on the context of the presentation.

In the blurb of research history offered on their laboratory website, John Heuser and his colleagues present the findings of De Camilli and Takei as convincing supplementations to their own model (for the website, see footnote 8). It is also the case that when contemporary writers make passing reference to the Heuser model, many also cite the work of De Camilli and his colleagues along with reference to the Heuser school.

(2) Though De Camilli and his colleagues find it useful at times to contrast their findings with the original version Heuser model, their results clearly do not constitute an advancement of the Ceccarelli model. In some presentations of their findings, these researchers review the Ceccarelli model—or as it is now also known 'the kiss-and-run model'—as a subject of controversy. The writings of De Camilli and his colleagues have section headings such as "Clathrin Coats Verses 'Kiss and Run'" or "Clathrin-Mediated Endocytosis verses 'Kiss-and-Run'" (De Camilli and Takei, 1996; De Camilli et al., 2001). In cases when the Ceccarelli model is acknowledged, De Camilli and his colleagues make clear that their findings do not apply to the Ceccarelli model. In general, the clathrin coat findings are considered to add articulation to and support for the Heuser model, and challenge to the Ceccarelli model.

However, De Camilli, Takei, and their colleagues do not hold that their account is necessarily mutually exclusive with the contemporary account provided by the Ceccarelli school. These researchers allow for the possibility that the Heuser and Ceccarelli pathways co-exist. This posturing is due, for the most part, to the details of the contemporary formulation of the Ceccarelli model reviewed below.

4c. Fusion Pore Kiss-and-Run

In the mid 90's, the students of Bruno Ceccarelli begin advancing a modified version of the Ceccarelli model. Researchers such as Riccardo Fesce, Flavia Valtora, Jacopo Meldolesi, Fabio Grohovaz, and others have continued work on the Ceccarelli model by instituting a major shift in this theory. They claim that synaptic vesicle exocytosis may occur through a molecularly-defined structure called a 'fusion pore' (Fesce et al., 1994; Fesce and Meldolesi, 1999; Valtora et al., 2001). In this view, synaptic vesicle exocytosis occurs as a vesicle temporarily fuses to the membrane through a fusion pore structure, and then detaches again without ever flattening out. Endocytosis in this model is simply the closure of the fusion pore.

A central consistent feature of the Ceccarelli model is its claim that vesicles fuse temporarily with the terminal membrane during neurotransmission and then detach in the same location. However, Ceccarelli and his colleagues have developed a variety of accounts regarding exact how this process occurs. A primary version of this model claims that exocytosis occurs through a sort of reversible fusion; the fusing vesicle collapses into the membrane and then that same vesicle material immediately buds and pinches from the membrane there in the same location. As reviewed above (e.g. in the quotes in subsection *3d*., and in footnote 5), these researchers also consider the possibility that vesicles do not collapse entirely, but instead fuse partially with the membrane. In contrast to these two versions of the Ceccarelli model, the contemporary fusion pore version of this model is a significant step in a different direction. A fusion pore has a specific molecular structure, one different from the collapsing and flattening accounts of the Heuser model and earlier versions of the Ceccarelli model. As seen in the previous subsection regarding the clathrin and dynamin studies of De Camilli and his colleagues, the field of synaptic vesicle research has come to focus more and more on the molecular structures and mechanisms of synaptic vesicle exocytosis and endocytosis. The fusion pore theory provides the Ceccarelli model with content that engages this contemporary focus.

The terms ‘kiss-and-run’ and ‘classical’ are coined by the Ceccarelli school in this era of study. As the Heuser model has grown increasingly more and more confirmed, the researchers of the Ceccarelli school create these two titles to emphasize the difference between the generally-accepted ‘classical’ Heuser interpretation, and their own ‘new’ position that synaptic vesicle exocytosis may operate via a fusion pore. These researchers retrofit the fusion pore theory and kiss-and-run terminology to their own history of work on the Ceccarelli model. In the contemporary literature, work on the Ceccarelli model from any part of its history is often referred to as ‘kiss-and-run.’

Part of the fusion pore/kiss-and-run position includes acknowledging the fact that the Heuser model has gained a significant amount of support. In a significant shift of position, the kiss-and-run researchers set to prove that fusion pore exocytosis *also* occurs. They investigate what factors cause the kiss-and-run pathway to be utilized. While researchers from both the Ceccarelli and Heuser schools have considered this prospect at times in the history of this discussion, the fusion pore/kiss-and-run version of the Ceccarelli model is explicitly cast in terms of the possibility that both pathways operate.

The first years of research on the fusion pore/kiss-and-run version of the Ceccarelli model primarily involve highlighting work which shows fusion pore-type exocytosis to occur in non-neuronal secretory cells, and arguing by analogy that this pathway may also exist for synaptic vesicles (Alvarez de Toledo et al., 1993; Neher, 1993; Fesce et al., 1994; Alés et al., 1999; Fesce and Meldolesi, 1999; Valtora et al., 2001). More recently, there begins to appear indirect evidence which shows the fusion pore/kiss-and-run pathway to be utilized by synaptic vesicles in certain situations (e.g. Gandhi and Stevens, 2003; Aravanis et al. 2003; Rizzoli and Betz, 2003; Staal et al., 2004; Wightman, 2004).[10] In the remainder of this subsection I review these two phases of fusion pore research: (1) The First Phase of Fusion Pore Theorizing, and (2) Recent Breakthroughs.

(1) The First Phase of Fusion Pore Theorizing:

In this first phase in the history of the fusion pore/kiss-and-run model, members of the Ceccarelli school articulate what it means for synaptic vesicle exocytosis to occur through the mechanism of a fusion pore (Fesce et al., 1994; Fesce and Meldolesi, 1999; Valtora et al., 2001). They base their claims on recent findings that show this pathway to be operative in secretory processes in other types of cells (i.e. non-neuronal cells).

Advances are made in the 1990's regarding techniques called 'capacitance measurements,' used for monitoring the electrochemical balance of the cell membrane. This balance relates to the membrane's structural makeup. One researcher, Erwin Neher, explains, “Because the plasma membrane is, electrically speaking, a capacitor that is proportional to surface area, a change in capacitance can be observed when vesicular membrane is inserted into the surface membrane during exocytosis” (1993, 497). The fusion of synaptic vesicles to the membrane changes its total surface area. These changes have effects on the delicate electrochemical balance between the inside and outside of the cell. Capacitance measurements read changes to this balance, and therefore enable researchers to infer changes to the membrane’s surface area due to vesicle fusion. Capacitance measuring has become more and more precise, now enabling monitoring of individual cells (called ‘single-cell’ capacitance measurements). Researchers couple the use of capacitance measurements with another technology new to these investigations: patch-clamp amperometry. This technique uses carbon fiber microelectrodes to measure the release of chemicals from a specific area of the cell.

A series of experiments which use these techniques on mast and chromaffin cells (i.e. not neurons) in mice reveal that a secretory vesicle often undergoes exocytosis via a flickering fusion pore (Alverez de Toledo et al., 1993; Neher, 1993). Eva Alés and her colleagues repeat these experiments in rat chromaffin cells, increasing the levels of calcium ions in the extracellular fluid to better mimic the conditions present in neurotransmission. They find that these changes shift the primary mechanism of exocytosis to kiss-and-run, and conclude that the presence of calcium plays a critical role in the determination of which pathway is primary (Alés et al., 1999). Fesce, Valtora, and their colleagues in the Ceccarelli school base their original formulations of the fusion pore/kiss-and-run theory of synaptic vesicle exocytosis on these studies.[11]

The short articles, “Neurotransmitter Release: fusion or ‘kiss-and-run’?,” “Peeping at the Vesicle Kiss,” and “Synaptic Vesicles: Is Kissing a Matter of Competence?” are the foundational texts for research on the kiss-and-run/fusion pore model (Fesce et al., 1994; Fesce and Meldolesi, 1999; Valtora et al., 2001). In these papers, Riccardo Fesce, Flavia Valtora, Jacopo Meldolesi, and Fabio Grohovaz coin the ‘kiss-and-run’ and ‘classical’ terminology, and link this theory to a history of research on the Ceccarelli model. Though admitting that the Heuser model has much support, they remind readers that there has been a rich history of controversy on this subject. According to Fesce and his colleagues, “The idea [of the Heuser model] has gained increasing acceptance, such that it is described in recent textbooks and review articles as the only mechanism. Coated pits and vesicles certainly do exist and most likely operate at the nerve terminal; however, a series of combined morphological and physiological studies has provided evidence that an alternative ‘kiss-and-run’ pathway predominates under conditions of mild-to-moderate activation” (Fesce et al., 1994, 1). They review their own history of research on the Ceccarelli model (sections 1, 2, and 3 of this chapter). Next they claim that the patch-clamp amperometry studies reviewed above support the Ceccarelli model. They explain that in Bruno Ceccarelli’s time, “there was no information about the possible molecular mechanism for transient opening of the synaptic vesicle to the synaptic cleft. The direct demonstration of mediator release through transiently open fusion pores in non-neuronal secretory systems now brings support for this hypothesis and clarifies the possible molecular mechanisms involved” (Fesce et al., 1994, 3).

The Ceccarelli school's 1999 essay repeats these claims that the Ceccarelli/kiss-and-run pathway is neglected in textbook presentations of synaptic vesicle exocytosis. In 2001, they report that the kiss-and-run mechanism is gaining recognition. They explain, "Lately, interest about this rapid mode has grown considerably, and numerous results have appeared in support of its existence and physiological role. Kiss-and-run has become part of the current nomenclature, and its underlying concept has become widely, although not generally, accepted" (Valtora et al., 2001, 324). The term 'kiss-and-run' is not used consistently by all that research this model. Valtora and his colleagues continue, "Indeed, at the present time, the definition of kiss-and-run itself is open to various interpretations: to many scientists, the criterion is rapidity (recycling times shorter than 1s), whereas others focus on the prompt reversal of exocytosis, with no collapse and intermixing of the vesicle and the plasma membranes (the mode rather than the speed of the fusion event)" (Valtora et al., 2001, 326-327). Like the original versions of the Heuser and Ceccarelli models before it, the fusion pore/kiss-and-run model of exocytosis has grown into a general research scaffold with several appendages and levels, not all exactly consistent with one another.

(2) Recent Breakthroughs:

Recent studies report evidence that synaptic vesicles in certain situations do engage in kiss-and-run exocytosis. For example, the work of Gandhi and Stevens, and also that of Aravanis and colleagues, may show kiss-and-run exocytosis occurring in the hippocampus cells of rat brains (Gandhi and Stevens, 2003; Aravanis et al., 2003; and for a review see Rizzoli and Betz, 2003). In these studies, researchers make use of pH sensitive fluorescent dyes which light up when they are exposed to the chemicals outside of the cell. These dyes can be attached to synaptic vesicles in a number of ways. When anchored to a synaptic vesicle, they become visible for the brief time that the vesicle is fused with the terminal membrane.

In both studies, researchers find results suggesting that most of the fusion events witnessed are instances of kiss-and-run exocytosis. Aravanis et al. achieve this by loading synaptic vesicles with the florescent dye. They discover many vesicles to retain a portion of their florescent dye after exocytosis. They conclude that vesicles open transiently and close again, rather than collapse completely into the membrane (Aravanis et al., 2003).

Ghandi and Stevens instead attache a florescent dye marker to synaptic vesicles. They can measure the amount of time a vesicle is open by clocking how long the marker is lit (but they cannot measure the amount of content a vesicle deposits, as did Aravanis et al.). They find three separate temporally-defined pathways, fast (400-860ms), medium (8-21 sec), and an indefinite path (at least 45 sec, the uppermost length of time studied) (Ghandi and Stevens, 2003). Important here is the fact that their study also includes the use of protein buffers; two different proteins, of different sizes, were added to the solution outside of the cell. One is too big to pass through the opening of a fusion pore. One is small enough to enter. Ghandi and Stevens report, “In this study, we conclude that the differential entry of buffer into the vesicle lumen (Tris enters and HEPES does not) reflects the presence of a selective fusion pore” (Ghandi and Stevens, 2003, 612). Silvio O. Rizzoli and William J. Betz (major figures in this field, e.g. see footnote 10) review this research. They consider the fact that these results contradict previous work on synaptic vesicle exocytosis, such as the quick-freezing and freeze-fracture studies performed by the Heuser school (reviewed in section 3). Rizzoli and Betz explain, "For decades, the collapse and slow retrieval of synaptic vesicles has been confirmed repeatedly. Yet in the present work a large fraction of events involved a kiss-and-run mechanism, not vesicle collapse. Why the discrepancy? The explanation may lie in the different types of synapse studied" (2003, 592). The freeze-facture work of the Heuser and Ceccarelli schools reviewed in section three concerns the frog neuromuscular junction. These studies instead examine the comparatively smaller neurons in rats' hippocampal region.

Another recent example of research in this new tradition is the work of Ronald G. W. Staal and his colleagues. Utilizing advances in carbon fiber electrode amperomety, they study neurotransmitter release in dopaminergic neurons in the midbrains of rats. Like the two studies above, these neurons are quite small. The results reveal a specific series of spikes in the line graphs, which these researchers take to imply that the synaptic vesicles under study are releasing their transmitter through a flickering fusion pore (Staal, et al., 2004). In a review of this work entitled "Synaptic Vesicles Really do Kiss and Run," R. Mark Wightman and Christy L. Haynes link these findings to others reviewed above and also to the traditional arguments for kiss-and-run (such as the efficiency and simplicity of the mechanism).

The examples of the progression of research on the fusion pore kiss-and-run theory in this subsection reveal an increasingly complex history, and a still-uncertain future. While evidence for the position grows, this evidence remains of an 'indirect' nature and is not yet generally seen to be as reliable or convincing as the evidence accumulated for the Heuser model. The general kiss-and-run research trajectory opens many doors. Researchers are beginning to identify several factors which may be involved in determining which pathway may be utilized in different situations. The differences between various types of neurons are beginning to come into better resolution in terms of this issue. The kiss-and-run research itself is also beginning to develop debates between its own proponents, engendering new derivative terms such as 'kiss-and-hold' and 'kiss-and-stay.'

4d. Is Fusion Pore Kiss-and-Run an iteration of the Ceccarelli Model?

As was the case for our review of De Camilli and Takei's model of clathrin-mediated endocytosis in subsections *4a* and *4b*, it is important to explore the relationship between the fusion pore/kiss-and-run model and the original version of the Ceccarelli model of exocytosis.

The researchers coining the kiss-and-run terminology clearly understand this fusion pore account to be the next step in the Ceccarelli model research enterprise. Fesce, Valtora, and the others (all members of the Ceccarelli school) actively present the fusion pore mechanism to be the contemporary focus of research on the Ceccarelli model. The discipline has accepted this move, and researchers often cite current work on fusion pore exocytosis along side older references to Ceccarelli's work. The 'kiss-and-run' terminology developed to refer to the fusion pore theory is also retroactively applied to the research on the original formulation of the Ceccarelli model. For a historian, this can tricky to keep track of. But the general adoption of this naming convention is evidence of the success of Fesce, Valtora, and the others in their efforts to link the fusion pore studies to the earlier investigations of the Ceccarelli model.

It is important to note that despite these terminological conventions, there have been essential shifts in the positioning of the Ceccarelli/kiss-and-run research over time. Two structural changes are important to elaborate here. One, the Ceccarelli school in this more recent era of fusion pore theorizing no longer understands the Heuser and the Ceccarelli positions to be inherently mutually exclusive. Two, this new era of research is defined by the understanding that synaptic vesicle exo-endocytosis occurs via the specific fusion pore mechanism, rather than a sort of reversible fusion.

An interesting aspect of this first change is that *despite* the fact that the Ceccarelli school has changed their stance from seeing the Heuser and Ceccarelli models as opposing readings of the same data to seeing them as representing two mechanisms which possibly both work within the same nerve terminal, these writers *maintain* aggressive opposition to much of the Heuser model research. There remain questions concerning which mechanism is primarily utilized in synaptic vesicle exocytosis. There also remains the project of determining which pathway operates under what physiological conditions, and in which types of neurons. Seeing the two mechanisms as compatible thus opens the door to new debates over which model dominates under different conditions.

The Ceccarelli school's change to the fusion pore account from the original version of the Ceccarelli model brings important consequences to the larger synaptic vesicle debate. Valtora and his colleagues write, "In theory, it is possible that only one mode of synaptic vesicle exocytosis exists and that kiss-and-run recycling occurs when the fusion pore has remained open only briefly, so that neurotransmitter release has occurred through incomplete fusion of the vesicle. However, it is also possible that kiss-and-run is a molecularly defined process" (Valtora et al., 2001, 327). Whether Ceccarelli-type exocytosis occurs through reversible fusion, rather than through a molecular process specific to the fusion pore theory, remains an issue for the Ceccarelli school. The introduction of the fusion pore model leaves open the possibility that the former Ceccarelli model position was in fact the correct one. An intra-kiss-and-run debate could emerge regarding these two possibilities.

I suggest that issues arise regarding the relationship between the data collected during the earlier phase and later (fusion pore) phase of research on the Ceccarelli model. The Ceccarelli school works hard to associate the current fusion pore studies with the freeze-fracture and other studies they had conducted with Bruno Ceccarelli in the 70's and 80's. But it is not clear that the fusion pore theory is similar enough to the original version of the Ceccarelli model to be able to receive support from evidence found for the original version. The papers which I have identified as 'foundational texts' of the fusion pore/kiss-and-run theory claim their position to be supported by the quick-freezing and freeze-fracture morphological studies previously conducted by the Ceccarelli school (Fesce et al., 1994; Valtora et al., 2001). However, the recent research reviewed at the end of the previous subsection shows the fusion pore mechanism to operate in neuron types *other* than the kind examined by the Ceccarelli and Heuser schools in the freeze-fracture and other studies (i.e. the frog neuromuscual junction). I suggest that it at best remains an open question whether the studies supporting the original version of the

Ceccarelli model also support the fusion pore version. It is not clear that the images of vesicle fusion ‘dimples’ produced in those studies display fusion pores.

Nevertheless, it is clearly the case that the original version of the Ceccarelli model provides the basis and inspiration for the contemporary fusion pore model, and by convention remains linked to current research.

5. The Changing Structures of the Synaptic Vesicle Debate

In the fifth and final chapter of this dissertation, I revisit the details of the synaptic vesicle debate. In the first section of chapter five, I show how the synaptic vesicle debate is an example of a scientific debate tradition, and apply this framework of concepts (e.g. debate nodes, problem hub, debate maintenance) to the features of this case study. The changes to the positions, structures, and relationships of this debate are analyzed in detail in the second section of that chapter.

For that project, it is helpful to identify first the key shifts that occur in the structure of the synaptic vesicle debate through its history. As with most general claims made to cover a large and diverse body of individuals, some of the generalizations I develop in this subsection do not adequately account for every particular moment of this history. The complexity of this story does not allow a historian or philosopher to uncontroversially cleave its content into simple temporal and logical moments. The utility of these generalizations stems from their capacity to highlight aspects of this history relevant to much of the subject under study.

It is fair, though not entirely unproblematic, to characterize the synaptic vesicle debate as one that begins in the early 1970's, and continues for roughly the first twenty years as a debate between the original formulations of the Heuser and Ceccarelli models. Its structure undergoes fundamental changes in the 1990's. These changes include the changes to the Heuser model brought by the clathrin and dynamin studies of De Camilli and colleagues. They also include the emergence of the fusion pore/kiss-and-run model as the dominant understanding of the Ceccarelli model. What are the constitutive features of the debate in this early phase and current phase? What important changes occur in the shift from one to the next?

One important constitutive feature of the synaptic vesicle debate in the 1970's and 80's is that theorists from both sides of the debate generally take the Heuser and Ceccarelli models to be mutually exclusive interpretations of the same set of findings. The two positions of the debate are understood to offer conflicting, comprehensive accounts of the same general data (e.g. stained images, freeze-fractured and quick-frozen images). This feature determines much of the structure of the way the debate is conducted in this early phase. If the researchers working on one of the two positions of the debate offer new evidence to support their position, researchers from the other side are implicitly set up with a challenge; evidence supporting one interpretation also implicitly (or explicitly) offers a challenge to the opposing interpretation. In these scenarios, the experimental results of one camp are often reinterpreted by members of the other camp. Data generated by either side of the debate can potentially be interpreted to support or deny either side. These data advance the debate, bringing greater articulation to both sides, but do not in themselves settle it.

The controversial images yielded by the assortment of freezing techniques reviewed in section three are a case in point. Both sides of the debate take part in the creation and analysis of images of synaptic vesicles frozen in the terminal membrane. Heuser and his colleagues' invention the quick-freezing technique makes it possible to create images of the membrane just milliseconds after stimulation. Yet despite generating solid evidence in support of the vesicle hypothesis, these images do not themselves resolve the synaptic vesicle debate; they introduce a new technical terrain in which the debate continues to flourish.

Technical disagreements over these images amass during this early period regarding issues such as vesicle fusion shapes, ectopic fusions, and the frequency of the appearance of clathrin-coated pits. Over time, the Heuser model becomes more and more generally accepted. This ascending general acceptance comes with growing evidence for the Heuser school's claims that endocytosis occurs long after exocytosis, at a separate location on the membrane, and involves clathrin coating.

The later phase of the synaptic vesicle debate is marked by substantial changes in the content of the two rival models, in the sorts of investigations conducted, and in the ways the debate itself is engaged.

One major change is to the contents and structures of the two rival models. The work of Pietro De Camilli, Kohji Takei, and their colleagues updates the Heuser model in significant ways. This work challenges the original formulation of the Heuser model in terms of the number of steps a vesicle must undergo during endocytosis to become fully-formed. Though De Camilli, Takei, and their colleagues cast this work in opposition to the original version of the Heuser model in terms of this particular issue, this work is generally seen to be an advancement of the Heuser model.

The relationship between the fusion pore/kiss-and-run model to the original version of the Ceccarelli model is similar. The kiss-and-run research differs from the original version of the Ceccarelli model in significant ways. For example, the kiss-and-run account holds that exo-endocytosis occurs through the mechanism of a fusion pore, where the original Ceccarelli model explained exo-endocytosis as a kind of reversible fusion. But despite this difference between the kiss-and-run account and the original version of the Ceccarelli model, this work is consistently cast as the current iteration of the Ceccarelli model.

Those working on these models acknowledge these changes, and allow for the fact that some also working within their side of the debate may not agree. Thus this later phase of the debate is not simply defined as a rivalry between two positions, but as one cluster of generally similar positions in debate with a rival cluster of generally similar positions.

Another major change to the synaptic vesicle regards the relationship between the Heuser and Ceccarelli models. Where the earlier phase features an understanding that these rival positions are mutually exclusive, the later phase features an understanding that the rival positions may both explain something different occurring during neurotransmission. In the foundational articles advancing the fusion pore/kiss-and-run version of the Ceccarelli model, the Ceccarelli school suggests that the Heuser pathway exists, and that the Ceccarelli pathway may also exist (though they continue to deflate evidence supporting the Heuser model). Research on this model often considers what circumstances may initiate reliance on the Ceccarelli pathway, such as the quantity of particular chemicals outside of the cell, or the particular type of neuron in question.

This change in the relationship between the rival positions of this debate has implications for another major change: the kind of investigations conducted. Research on synaptic vesicles focuses more and more on the specific molecular structures and mechanisms involved in exocytosis and endocytosis. The Ceccarelli school develops its account in terms of the fusion pore, a defined chemical structure. Work on the Heuser model continues with the study of clathrin cages, dynamin tubular invaginations, and the other molecular structures of synaptic vesicle endocytosis. Each side of the debate has remained a rival to the other in terms of these molecular-level accounts. Yet it is not the case that each new study conducted by one side offers challenge and opportunity to the other, as was the case with the images of vesicles frozen to the membrane from the 80's. For example, a new clathrin study offered in support of the Heuser model does not automatically challenge the kiss-and-run account, since contemporary positioning allows for the possibility that fusion pore-type exo-endocytosis may also occur. Also, it is no longer the case—as it was with frozen image studies in the 1980's—that data created by one side offer an opportunity for re-interpretation by the rival side. A new clathrin study offered in support of the Heuser model does not lend itself for reinterpretation by the kiss-and-run theorists for use in their own account. The rival sides of the synaptic vesicle debate do, however, continue to criticize each other's accounts. Debate remains over which account is primary, which dominates in which circumstances, and whether each new advance is open to criticism.

In sum, despite years of progress, hundreds of articles, advances in experimental techniques, the invention of new technologies, essential changes to each position, significant conceptual advances, and piles of data, the two major positions of the synaptic vesicle debate remain versions of the Heuser and the Ceccarelli models.

Notes for Chapter 3

1. The importance of Bernard Katz to the field of research explored in this chapter should not be underestimated. An introductory text for the field explains, "It is rare that a scientific field of such magnitude can be traced back to a single scientist. One such case is the development of the physiology of synaptic transmission by Sir Bernard Katz" (Kelly, 1999, 7).

2. When one casually cites this debate, these two articles are often used to represent the two opposing theories concerning the nature of synaptic vesicles. According to *ISI Web of Science* citation index, the Ceccarelli school's piece has been cited over 400 times, and Heuser and Reese's piece over 1300 times (Heuser and Reese, 1973; Ceccarelli et al., 1973).

3. The "arch nemesis" reference is presumably in good fun. Heuser is a contributor to the volume (Heuser, 1989b).

4. In a project related to this dissertation, I analyze the competing interpretations of quick-frozen, freeze-fractured and freeze-substituted images in terms of insights from the philosophy of technology, specifically Don Ihde's philosophy of imaging (e.g. Rosenberger, 2005; Rosenberger, forthcoming).

5. In their important 1994 paper "Neurotransmitter Release: Fusion or 'kiss-and-run'?," the students of the Ceccarelli school explain, "in neuromuscular preparations quickly frozen a few milliseconds after the delivery of a single stimulus, many profiles of vesicles are seen in close apposition to the plasma membrane or open to the synaptic cleft… a neck, occasionally as narrow as the opening of a fusion pore (Fig. 1), always connects the interior of the vesicle with the extracellular space. By contrast, typical 'omega shapes' of synaptic vesicles caught while collapsing into the axolemma are rarely seen[6]" (Fesce et al., 1994, 1). The superscript number six is a reference to Torri-Tarelli et al., 1985. In another paper, these researchers report that the kiss-and-run model is "corroborated by the observation of clear images of vesicles fused with the presynaptic membrane, but no wide open omega-shaped vesicle profiles, in neuromuscular preparations quick frozen 2-10 ms after delivering a single shock to the motor nerve" (Fesce et al., 1996, 18). The Torri-Torelli et al., 1985 paper itself, however, does not report on the shape of vesicles in any detail.

6. The quote by Valtora et al., 1989 appearing two paragraphs above is an example of this.

7. Since it is the dunking of a sample into a bath heavy with potassium for various amounts of time that causes stimulation in these studies, these are not examples of the quick-freezing technique, but rather chemical fixing.

8. A concise review of the evolution of the classical model, from its origin in the early 70's to its current form advanced by De Camilli, Takei, and others, appears on John Heuser's website, www.heuserlab.wustl.edu.

9. It was not unreasonable for Heuser and his colleagues to misinterpret their own findings to suggest that after shedding their clathrin coats, a vesicle must take the intermediary step of forming a cistern, i.e. visiting a sorting endosome. This is how some processes similar to synaptic vesicle endocytosis occur in other sorts of secretory cells. Also, it is easy to interpret a large chunk of membrane, internalized through the process of bulk endocytosis, as something else.

10. Another important avenue of recent research has been the study of different vesicle pools present in the nerve terminal. This research has yielded much data relevant to the synaptic vesicle debate, particularly in terms of the specific locations of exocytosis and endocytosis on the terminal membrane. These results have brought reviewers to consider circumstances in which both the Heuser and Ceccarelli pathways operate (for recent reviews of this body of research see Wilkinson and Cole, 2001; Rizzoli et al., 2003; Rizzoli and Betz, 2005).

11. It is important to note that the technological advances in capacitance measurement, amperometry, and florescent dye studies have also been used to find evidence for the Heuser model in non-neuronal cells (e.g. Neher and Marty, 1982; Finnegan et al., 1996). In this section, I focus on the way these advances have contributed to the kiss-and-run position because these studies provide the backbone of kiss-and-run research.

Chapter 4: The Theory-of-Mind Debate

> And one day Julie sat down at a desk next to me and put a tube of Smarties on the desk, and she said, "Christopher, what do you think this is here?"
>
> And I said, "Smarties."
>
> Then she took the top off the Smarties tube and turned it upside down and a little red pencil came out and she laughed and I said, "It's not Smarties, it's a pencil."
>
> Then she said, "If your mummy came in now and we asked her what was inside the Smarties tube, what do you think she would say?" because I used to call Mother *Mummy* then, not *Mother*.
>
> And I said, "A pencil."
>
> That is because when I was little I didn't understand about other people having minds. And Julie said to Mother and Father that I would always find this very difficult (Haddon, 2003, 115-116).

The above quote comes from Mark Haddon's novel *The Curious Incident of the Dog in the Night-Time* (2003). The narrator is an autistic boy and in the passage above he recounts a story from when he was twelve years old. His failure to correctly answer the question, and also the terms he uses to describe the experience (including the Smarties tube), are references to the research that is the subject of in this chapter. This research occurs in the field of developmental psychology and involves a task which children of a specific age have been shown to have difficulty with, and which children of a slightly older age do not. Findings also show that those with autism continue to have trouble with tasks such as this well past the normal age.

With founding figures in its history such as Jean Piaget and John H. Flavell, the field of developmental psychology studies children as they mature. In the area of cognitive developmental psychology, researchers investigate children's ability to reason, solve problems, and understand their world. Susan Carey and Elizabeth S. Spelke, the two researchers whose debate was reviewed in chapter two, are both members of this field. This chapter concerns another area of research in developmental psychology: the investigation of children's ability to understand that others have separate minds and experience the world from their own separate perspectives. This area of study is often referred to as 'child's theory of mind.' The 'child's theory of mind' experiments I review are some of the most influential and cornerstone findings of the discipline.

There is a long-standing disagreement over how these findings should be interpreted. A large number of rival accounts of these data have been developed in this literature. These accounts are subject to a correspondingly large amount of scrutiny from rivals. I refer to this disagreement as *the theory-of-mind debate*.

The theory-of-mind debate is an example of a scientific debate tradition. In chapter five, I return to this study to show how the notion of the scientific debate tradition fits the details of this discussion. In that chapter I demonstrate that the concepts associated with the scientific debate tradition (e.g. debate nodes, problem hub, branch quarrels, etc.) apply to the theory-of-mind debate. I also reflect on the history provided below to articulate the specific ways this tradition has evolved.

In the *first* section of this chapter, I review the experiments and data central to this area of research. In the *second* section, the many interpretations of this data which have been developed are explored. In the *third* section, I observe the manners in which this debate has been conducted. The *final* section provides a general summary of the movement of this debate as it has changed over time.

1. The Crucial Evidence: False-Belief Tasks

Since the pioneering work of Jean Piaget, developmental psychologists work to understand how the cognitive abilities of children mature over time. One major trajectory of research in this field is the attempt to understand how and when children develop the ability to conceptualize the fact that other people perceive the world from their own separate and limited perspectives. Or, as researchers put it, they investigate the development of children's conception of other minds. These studies are referred to as 'child's theory of mind.'[1] Child's theory of mind research is based in a set of influential experiments that provide some of the most foundational evidence of the field of developmental psychology. The subject of this chapter is the long-standing and popular dispute that has arisen over the interpretation of these findings. I refer to this dispute as *the theory-of-mind debate*.[2]

In this section, I review the experiments which make up the backbone of data in this area of research and provide the templates for the scores of related studies that have been performed. The central evidence in child's theory of mind comes from experiments referred to as 'false-belief tasks.' These experiments involve tests of a child's ability to understand the notion that another person holds an incorrect belief about the world. Subsection *1a* regards the first of two major types of false-belief tasks called 'unexpected transfer tasks,' in which a child predicts the behavior of a person that is under a false impression regarding the location of a desired object. Subsection *1b* regards the second major type of false-belief task called 'the deceptive box task,' in which a child predicts the behavior of one who looks into a box with contents that differ from the box's label. Subsection *1c* reviews another category of experiments relevant to the theory-of-mind

[1] Endnotes for this chapter are found on pages 192-194.

debate: tests of a child's understanding of the distinction between appearance and reality. Subsection *1d* explores the influential versions of false-belief and appearance/reality tasks which study the abilities of children with autism. Subsection *1e* reviews a further related task in which children sort cards based on two separate parameters. Subsection *1f* regards a recent version of a false-belief task performed on infants.

1a. The Unexpected Transfer Task

The sizable literature regarding child's theory of mind builds from a small number of key experimental designs which researchers have replicated with an exceptional quantity of creative alterations. The central kind of experiment on this topic is called the 'false-belief task.' False-belief tasks test a child's ability to conceive of another person as holding an incorrect belief about the world.

This line of reasoning began not within developmental psychology, but within primatology and philosophy. Primatologists David Premack and Guy Woodruff claim to explore whether or not chimpanzees possess a "theory of mind" (1978). In their commentaries on Premack and Woodruff's work, three philosophers suggest that this question can be answered by developing an experiment that determines whether or not chimps can correctly predict the behavior of one who is looking for an object that has been moved in one's absence (Bennett, 1978; Dennett, 1978; Harman, 1978).

This general insight has been adopted by developmental psychologists for the purpose of studying children, providing the logic which underpins a tradition of successful and prolific 'false-belief' experimentation. The false-belief tasks developed over the years have been altered in numerous ways to account for the various extraneous variables that could influence the findings. In 2001, Henry M. Wellman, David Cross, and Julanne Watson conduct a large scale meta-analysis of false-belief task experiments, reviewing 178 separate studies. This tradition of research and debate includes more than just these false-belief studies; add to this the many studies performed after 2001, the multitude of studies which relate to this topic but are not specifically 'false-belief tasks' (e.g. appearance/reality tasks reviewed in subsection *1c*), studies with an exclusive autism focus, and recent related brain imaging studies.[3] Also add the countless developmental psychology articles which, while not offering new data, propose new theories, advance existing theories, and contest the rival theories which make up the theory-of-mind debate.[4]

One main version of false-belief inquiry is 'the unexpected transfer task.' In a landmark article entitled "Beliefs About Beliefs: Representation and Constraining Function of Wrong Beliefs in Young Children's Understanding of Deception," Heinz Wimmer and Josef Perner construct the experiment proposed by the responders to Premack and Woodruff but with children (rather than chimps) as their objects of study (1983).

In Wimmer and Perner's study, a child is presented with a story of a little boy named Maxi who puts a bar of chocolate in a specific kitchen drawer. Maxi leaves for a short time and in the interim his mother arrives, borrows the chocolate, and returns it to a different drawer. This child is then asked in which drawer Maxi will expect the chocolate to be when Maxi returns to the kitchen.

Wimmer and Perner find that most 6-year-old children correctly observe that Maxi would look for the chocolate in the drawer in which he had left it. In contrast, they also find that many 4 and 5-year-olds incorrectly assume that Maxi would believe the chocolate was in the drawer in which his mother had moved it. This general research strategy, and the developmental trend it has uncovered, has inspired much of the work in this area of study. However, the common contemporary understanding of the trend is that children 3 years of age and younger have trouble understanding false-belief, and children 5 years of age and older do not. The theory-of-mind debate is over how to explain this developmental trend.

1b. The Deceptive Box Task

The second major version of false-belief task is called 'the deceptive box task,' introduced by Joseph Perner and his colleagues in an article entitled "Three-Year-Olds' Difficulty With False Belief: The Case For a Conceptual Deficit" (Perner, Leekam, and Wimmer, 1987). Here a child is presented with a box of candy which he or she is familiar, specifically a tube of 'Smarties.' When the child is first asked what he or she expects to be in the Smarties tube, he or she replies "candy."

Next, the experimenter reveals that the candy box actually happens to contain pencils. The child is then asked what another person who has not yet seen the box open would expect to find inside. Children's responses to this question again reveal the developmental trend shown by the unexpected transfer task. Children of 5 years of age correctly expect another person to assume there to be candy within the candy box, where children of 3 years instead understand others to expect pencils.

An advantage of the deceptive box task over the unexpected transfer version is that the extraneous variables regarding children's abilities of imagination and story comprehension are greatly reduced; the child experiences parts of the story rather than imagines Maxi's experiences. This influential experimental design is adopted by many researchers, and has been replicated with populations of non-Western children (e.g. Avis and Harris, 1991).

Alison Gopnik and Janet W. Astington develop the influential next step of this research with their article "Children's Understanding of Representational Change, and Its Relation to the Understanding of False Belief and the Appearance Reality Distinction" (1988). In the final moment of the exercise, rather than ask the child what another person would expect to find inside the box, the experimenters ask the child what he or she him or herself had originally (i.e. in Stage 1) expected to be in the candy box. Provocatively, they find results which further confirm this developmental trend.

1c. Appearance/Reality Tasks

A related set of tasks that influence child's theory of mind research (yet are not considered 'false-belief tasks') investigate children's ability to understand the distinction between appearance and reality. These tasks provide further confirmation of the developmental trend identified above.

The seminal study in the tradition of appearance/reality tasks is introduced by John H. Flavell and his colleagues in a 1983 article entitled "Development of the Appearance-Reality Distinction." In this study, children are presented with a stone and also a sponge which looks very much like a stone. Next it is revealed that the sponge is in fact a sponge, despite its stone-like appearance. When asked what the sponge appears to be, 3-year-olds take it to look like a sponge, where older children take it to look like a stone. Another popular version of this sort of task developed by Paul L. Harris and D. Gross involves facial expression and emotion (1988). They find that young children are unable to understand that one with a happy expression may, in reality, feel unhappy. The results of the appearance/reality tasks have also been shown to hold in non-Western contexts (e.g. Flavell et al., 1983). These and other appearance/reality tasks contribute to developmental psychologists' claims that a clear trend can be identified in children's ability to understand other minds.

1d. Autism Versions

A widely influential variation of the false-belief and appearance/reality tasks involves tests on people with autism. The ground-breaking study on this topic is Simon Baron-Cohen and his colleagues' 1985 piece "Does the Autistic Child Have a Theory of Mind?" These researchers largely repeat the structure of Wimmer and Perner's 1983 Maxi study, but investigate the behavior of autistic children. Baron-Cohen et al. find that 12-year-old children with autism fail the task, where their control groups of children with

Down's syndrome and also 4 ½-year-olds with neither autism nor Downs syndrome instead come to the correct answer.

Joseph Perner and his colleagues also conduct a version of the pencil box false-belief task with autistic children, and Baron-Cohen conducts a corresponding version of the rock/sponge appearance/reality task (Perner et al., 1989; Baron-Cohen, 1991). This research further confirms that those with autism continue to have trouble with tasks regarding child's theory of mind well beyond the years in which clinically normal children do. These studies serve as a spring board to further research in the study of autism. Also, these findings relating to autism play a major role in the theory-of-mind debate.

1e. The Dimensional Change Card Sort

'The dimensional change card sort' is a recently-developed experimental technique for studying these issues (DCCS) (e.g. Frye et al., 1995; Zelazo et al., 2003; Müller et al., 2005). With the DCCS, children sort cards with pictures on them. The pictures have more than one parameter. For example, cards may have one of two colors (red or blue) and also one of two shapes (a rabbit or a boat). In this example there would be four cards: a blue rabbit, a red rabbit, a blue boat, and a red boat. Children first sort the cards in terms of one of the parameters. For example, they could be first asked to make a pile of red cards and one of blue cards (or asked to make piles of rabbits and boats). Next the children are asked to re-sort the cards in terms of the second parameter.

As Zelazo et al. report, "3- to 4-year olds typically exhibit inflexibility on this task. In contrast, by 5 years of age, children typically perform well" (2003). As with the tasks described above, various versions are performed over an assortment of studies to account for extraneous factors in the experimental design. The results of these studies fit the general findings of the false-belief and appearance/reality experiments in that a significant developmental shift is found in the preschool years. The results of DCCS studies have been shown to correlate highly to false-belief work.

1f. Variations and a Recent Infancy Version

Researchers continuously interrogate the experimental designs of these tasks in attempts to eliminate or explain the roles of extraneous variables. There are also challenges to the basic designs (e.g. Jenkins and Astington, 1996; Astington, 2001). Also, as is usually the case in the study of developmental trends, researchers investigate whether the trend can be shown to begin at an earlier age. One example of this trajectory of research is the comparison of 'where children look' to 'what they verbally report' as they engage in these tasks. A recent set of studies on this topic reveals evidence suggesting that young children may look to the correct answer of false-belief tasks despite verbally delivering the incorrect answer (e.g. Clements and Perner, 1994; Garnham and Ruffman, 2001; Ruffman et al., 2001; and for a neural imaging study which relates to this research see Saxe and Kanwisher, 2003).

In a recent study published in *Nature*, entitled "Do 15-Month-Old Infants Understand False Belief?," Kristine H. Onishi and Renée Baillargeon take this line of thinking to the next level (2005; for commentary see Perner and Ruffman, 2005). They test whether infants look to the correct answer of a false-belief task by devising a non-verbal version of the exercise in which infants' 'looking times' are measured. This is a similar methodology to Elizabeth S. Spelke's work reviewed above (chapter two, section two). In 'looking time' studies, infants are understood to pay more attention when what they see occurs in a way different to their expectations. Onishi and Baillargeon build their experimental design on studies which show that infants can be trained to expect an actor they are watching to possess a 'goal object,' an object the actor tends to reach for. When the actor appears before an infant and reaches for something other than the goal object, thus departing from the infant's expectations, the infant clocks a longer looking time. To test infants' understanding of false-belief, Onishi and Baillargeon present infants with an actor with a goal object who places the object under one of two boxes. In some trials, the infant observes the goal object as it is moved from one box to the other without the knowledge of the actor. Through analysis of looking times, these researchers find that infants expect the actor to look for the goal object in the box in which they had left it, not in which the object actually now is. Onishi and Baillargeon conclude that, despite three-year-olds' performances on verbal false-belief tasks, infants hold a rudimentary and implicit theory of mind.

2. The Rival Theories

With the basic experimental designs and data at issue in the theory-of-mind debate considered, the various theoretical positions which have been taken up in respect to these findings can be reviewed. Due to the quantity of rival positions that make up this debate, I review them here in a list-like format.

While a large number of different theories have been offered to explain the developmental trend revealed by child's theory of mind research, it is possible to group many of these positions into related clusters. This what the members of the debate themselves often do as they engage one another; while several researchers may together hold one of the major positions of the debate, they each may also hold disagreements with one another regarding different versions of that major position. One of the most popular and influential positions of the theory-of-mind debate is the 'theory-theory,' reviewed in subsection *2a*. Subsection *2b* addresses another major position called 'simulation.' Subsection *2c* concerns 'modularity' theory, a position which may rival the first two in reputation. Many other theories have been developed in this literature. Several of these are reviewed together in subsection *2d*. In the final subsection, *2e*, I review position called 'executive function' which has entered recently and has garnered much attention.

2a. Theory-Theory

One of the most influential accounts of the child's theory of mind data is a position called 'theory-theory.' This position is advanced by figures such as Alison Gopnik, Henry M. Wellman, and others (e.g. Wellman, 1990; Perner, 1991; Gopnik and Wellman, 1992; Gopnik, 1993; Gopnik and Wellman, 1994; Bartsch and Wellman, 1995; Gopnik, 1996a; Gopnik, 1996b; Gopnik and Meltzoff, 1997; Wellman, Cross, and

Watson, 2001).[5] In this view, a child develops a theory-like understanding of the notion of mind. This theory-like understanding adjusts as the child has experiences in the world.

This account is often cast in terms of a metaphor between child development and the development of knowledge in science. The degree to which a child's theory-posing is held to be like a scientist's theory-posing depends on the variation of theory-theory considered. Theory-theory accounts also differ from one another in terms of their views on exactly how the metaphor between science and child development works; some accounts hold that what *results from* development is a theory like those of scientists, while other accounts hold that the very *processes* of theorizing and theory change in development are like those of science. For example, Alison Gopnik and Andrew Meltzoff claim, "The central idea of this theory [theory-theory] is that the processes of cognitive development in children are similar to, indeed perhaps identical with, the processes of cognitive development in scientists" (1997, 3).

Defenders of theory-theory account for the developmental trend in terms of a child's advancing theories. In this view, the developmental shift in a child's abilities revealed by false-belief tasks occurs as a better theory regarding others' minds replaces the older theory during these years of a child's life. In their article "Why the Child's Theory of Mind Really Is a Theory," Gopnik and Wellman explain, "We argue therefore, that the transition from 2 ½ to 5 shows all the signs of being a theory change. While initially the theory protects itself from counter-evidence, the force of such counter-evidence eventually begins to push the theory in the direction of change" (1992, 158). That is, a child possesses one theory of mind, but, as the child has experiences during the time period identified, he or she develops a new theory which accounts for the minds of others differently.

Defenders of theory-theory offer detailed accounts of what it means for a child to possess a theory. They develop lists of features to define the notion of a child's 'theory' (e.g. Gopnik and Wellman, 1994; Gopnik and Meltzoff, 1997). Gopnik and Meltzoff, for example, offer a list of "structural" features of theories, the "functions" theories play, and an account of the "dynamics" of theories (1997, 32-41).

It is important to keep track of an ambiguity in this literature regarding a distinction between the version of theory-theory I have summarized here, and another major position in the theory-of-mind debate: the modularity theory. Reviewed below (in subsection *2c*), the modularity position holds that the child's theory of mind data are best accounted for by changes in biological structures of the brain, what are called 'modules.' However, at many moments of this literature, the module in this account is referred to as a 'theory,' and modularity theory is referred to as a type of theory-theory. When modularity theory is described in these terms, the modularity version is often distinguished from the version of theory-theory reviewed above by referring to the two positions as 'nativistic' and 'non-nativistic' theory-theory (more on this in *2c*).

2b. Simulation

The second major position of the theory-of-mind debate is 'simulation theory.' This position is advanced in various ways by figures such as Alvin I. Goldman, Robert Gordon, Paul Harris, and Jane Heal (e.g. Gordon, 1986; Heal, 1986; Goldman, 1989;

Harris, 1989; Goldman, 1992; Gordon, 1992; Harris, 1992; Harris, 1994; Heal, 1994; Gordon, 1996; Heal, 1996; Nichols et al., 1996; Gallese and Goldman, 1998; Goldman, 2000; Goldman, 2006). In this view, a child develops a theory of mind through a process role-playing and copying the actions of others. To understand others' minds, a child reflects upon his or her own experience and uses that experience as a model (i.e. a simulation) for what others may experience.

An important aspect of this theory is the claim that a child reflects on his or her own motivations and emotions during the process of simulation. Simulation theorists argue that anticipating the actions of others crucially includes the consideration others' behaviors in terms of one's own experiences. In this view, the explanation of the developmental trend lies in a child's ability to empathize with others; during the preschool years, a child comes to recognize that others behave in a way similar to him or herself. The child then looks within him or herself to learn what it means for others to have minds.

While simulation theorists together offer their account as an alternative to the theory-theory position and other positions of the theory-of-mind debate, there are also disagreements within simulation theory literature. For example, one significant quarrel regards the degree to which introspection must be conducted in an explicit and purposeful way. Robert M. Gordon, for example, has worked to distinguish his version of simulation from those offered by Harris and Goldman (Gordon, 1996). Gordon explains that simulation theories such as Harris's and Goldman's hold that simulation occurs as a child recognizes his or her own mental states and imagines the behaviors of others to occur in terms of similar states. Gordon instead offers an account which involves a child answering an ascending series of questions, based on his or her own experience, that do not require extensive introspection, imagination, or self concept at each step. These two types of simulation accounts are distinguished as 'online' and 'offline' theories.

Simulation theory, like the other major theories presented here, continues strongly today. This theory even expands into neurological research. Vittorio Gallese and Alvin I. Goldman attempt to draw connections between neurological studies on 'mirror neurons' and the process of simulation. These neurons have been shown in macaque monkeys to activate both when the monkey performs an action, and also when the monkey watches another perform the same action. Gallese and Goldman write, "Our conjecture is that MNs [mirror neurons] represent primitive, or possibly a precursor in phylogeny, of a simulation heuristic that might underlie mind-reading [theory of mind]" (1999, 498). This is just one example of the way simulation theory remains on the front lines of current research.

2c. Modularity

The 'modularity theory' is widely interpreted to be the third of the three major positions of the theory-of-mind debate. Forwarded by figures such as Simon Baron-Cohen, Alan Leslie, and Brian J Scholl, modularity theory holds that a child develops a theory of mind through the maturation of a specific part of the brain (e.g. Fodor, 1992; Leslie, 1992; Leslie, 1994; Baron-Cohen, 1995; Baron-Cohen and Swettenham, 1996; Segal, 1996; Scholl and Leslie, 1999; Scholl and Leslie, 2001). In this view, the concepts operative in a child's development of a theory of mind, such as 'belief' and 'desire,' are innate and are acquired through physiological maturation. Scholl and Leslie summarize, "It has been suggested that this capacity—termed a 'theory of mind' (ToM)—arises from an innate, encapsulated, and domain-specific part of the cognitive architecture, in short a module" (1999, 131). As innate, the module exists in our brains from the time we are born and is triggered as a child reaches a certain point in development. As encapsulated, the development of the module occurs independently from others. As domain-specific, the module relates only to a specific cognitive capacity.

Some view the modularity theory to be first introduced by philosopher Jerry Fodor as an extension of his philosophical work into the false-belief task literature (Fodor, 1992). Fodor's proposal has collected much response, and has inspired elaborated versions of modularity. An important aspect of modularity theories is their heavy focus on data regarding the difficulty that people with autism have in correctly completing false-belief tasks. While all accounts of child's theory of mind are pressed to explain the data regarding autism, modularity theorists treat these data centrally. For example, Baron-Cohen and his colleagues, the researchers who first discovered connection between theory of mind and autism, advance their own version of modularity theory (e.g. Baron-Cohen, 1995; Baron-Cohen and Swettenham, 1996).

A well-elaborated version of the modularity theory is called 'the Theory of Mind Mechanism' (ToMM), advanced by Leslie and others (e.g. Leslie, 1992; Leslie, 1994; Scholl and Leslie, 1999). The ToMM version of the modularity theory is distinguished by its account's high level of specificity. Early versions of the ToMM account attempt to explain the child's theory of mind data by postulating that two separate modules are responsible; one activates later than the other, superseding it. A more recent version claims that certain more general and global abilities (executive functions) must be in place before the relevant modules can activate properly (a position referred to as ToMM/SP).

As mentioned above, questions remain regarding whether modularity theories should be considered a type of theory-theory. Paul Carruthers, for example, begins an article with the clarification, “Let me begin by pinning my colors to the mast: I am a theory-theorist” (1996b, 22). Then he begins a later paragraph, “I also believe—to pin my colors to another mast—that at least the core of this folk-psychological theory is given innately, rather than acquired through a process of theorizing, or learning of any sort” (Carruthers, 1996b, 22). Carruthers holds that the developmental trend at issue in this literature is best explained by mechanisms innately present in children, what he calls a “nativistic” version of theory-theory. Yet in terms of the way I define the positions here, Carruther’s position appears to have more affinity with modularity theory than with theory-theory. The simulation theorist Alvin I. Goldman makes a similar observation, but in regards to Alan Leslie’s claims that modularity is a type of theory-theory. Goldman writes, “But although Leslie himself classifies his approach (in these passages) as a version of TT, that does not make it so. Leslie and his collaborators clearly offer a cognitive science theory of mentalizing, but a cognitive science theory of mentalizing is not necessarily a TT” (2000, 177).

2d. Further Alternatives

Though simulation, modularity, and theory-theory are the three positions commonly held to be the central contenders in the theory-of-mind debate, an overabundance of further positions have been offered by developmental psychologists. I only review a selection of these positions here, not a comprehensive list. This should provide a general impression of the variety of positions defended in this literature. Some examples of theories regarding the children's theory of mind data are as follows:

- Janet Astington argues that the development of language is central to the reasoning abilities required for completing false-belief tasks (e.g. Astington, 2001; Astington and Baird, 2005).

- Huttenlocher and Smiley emphasize the importance of a child's differently developing conception of his or her own mental states, and also his or her conception of the mental states of others (e.g. Smiley and Huttenlocher, 1989).

- Shaun Gallagher, working from the phenomenological tradition in philosophy, suggests that a child's theory of mind develops out of the everyday embodied experience of interacting with others (2001).

- Ulrich Müller and his colleagues have suggested that false-belief data are best explained in terms of a child's ability to understand false statements (Müller et al., 1998).

- Sometimes referred to as the 'social constructivist' view, several theorists have suggested that a child's theory of mind develops through participation in cultural dealings, and interacting within one's shared community (e.g. Bruner, 1990; Feldman, 1992; Hobson, 1991).

- Kathrine Nelson claims that a theory of mind develops as a child differentiates his or her own perspective from a congregation of others with similar by not identical views, what Nelson calls "entering a community of minds" (2005).

- As with both Astington and the 'social constructivists' above, Charles Fernyhough develops another position based on the work of Vygotsky. He suggests that the developmental trend results from "dialogic" interactions between people in which others' perspectives are internalized (Fernyhough, 1996).

- Jill G. de Villiers advances 'the linguistic determination theory.' In this view, language development does not simply facilitate false-belief task performance; it is causally involved in the development of a theory of mind (e.g. de Villiers, 1995; de Villiers and de Villiers, 2000; de Villiers and de Villiers, 2003; de Villiers, 2005).

- Several researchers hold that the development of conversational abilities is involved in a child's ability to successfully complete false-belief tasks (Lewis and Osborne, 1990; Siegal and Beattie, 1991; Siegal, 1997).

2e. Executive Functioning: a recent addition

A theory called 'executive function' (EF) has recently emerged as a prominent contender in the theory-of-mind debate. Figures such as David Zelazo, Douglas Frye, Stephanie M. Carlson, and Ulrich Müller are among those advancing this position (Frye et al., 1995; Perner and Lang, 1999; Carlson and Moses, 2001; Zelazo et al., 2003; Müller et al., 2005). Research on executive function examines a child's ability to follow rules, and considers the implications of these abilities for a child's capacity to consciously control his or her actions. Like other positions of this debate, this theory provides a new interpretation of theory of mind data. EF is also influential in areas of study in the field of developmental psychology outside of child's theory of mind.

EF accounts explain the developmental trend at issue in the theory-of-mind debate in terms of a child's developing ability to consciously follow rules. In this view, a child's abilities of control, planning, and coordination determine his or her performance on false-belief tasks. Though this general explanation of the developmental trend is agreed upon by EF theorists, they are not of one mind in terms of the details of this account. As we have seen within the theory-theory, simulation, and modularity literature, EF too contains intra-theory debates in which researchers disagree about the nature of EF itself. The varieties of EF include positions such as the Cognitive Complexity and Control theory (a.k.a. CCC theory), working memory theories, inhibition accounts (stressing one's ability to control one's attention), and redescription accounts.

Like the way that modularity theory focuses centrally on autism studies, EF focuses centrally on the Dimensional Change Card Sort (DCCS) tasks reviewed in subsection *1e*. The DCCS task is advanced by the EF researchers (along with, apparently, an agenda of propagating acronyms). As Carlson writes, "Indeed, I anticipate that the DCCS could come to serve as a marker task for executive function in the preschool period in much the same way that the false-belief task has done for theory of mind" (2003). In any case, it is clear that the development of a task which is connected to, yet not reducible to, false-belief and appearance/reality tasks is an important contribution to child's theory of mind research.

3. The Proliferation of Competing Theories

The theory-of-mind debate is composed of a number of rival positions with complex relationships between them. The history of this debate is straightforwardly characterized by a proliferation of positions; each offers a new take on the increasing stock of data, and each critiques the already-offered theories.

A full-scale study in the ascent of the theory-of-mind debate to its current status as a foundational discussion of the discipline exceeds the scope of dissertation. However, it is helpful to review a few telling details regarding the increasing influence of this body of research. For example, the publication history of the Robert S. Siegler's textbook *Children's Thinking* provides helpful indicators (1986). *Children's Thinking* is a well-regarded upper-level survey and introduction to the field of developmental psychology. The differences between the various editions of this book reflect the evolving trends of this field. The first two editions of *Children's Thinking* (1986 and 1991) contain no references to child's theory of mind research. However, the third edition contains a review of child's theory of mind research, including the work of many of the figures reported on above, the false-belief and appearance/reality tasks, and the autism studies (Siegler, 1998). The fourth and most recent edition expands on this section, adding a full-page cartoon depiction of the Wimmer and Perner 1983 'Maxi' study and a 'theory of mind' entry in the subject index (Siegler and Alibali, 2005). Insofar as the importance of theory-of-mind debate to the field of developmental psychology is reflected in the progression of editions of this book, this discussion eventually emerges as one worthy of considerable textbook treatment, and is one which continues to maintain this status.

Another indication of the escalation of the influence of the theory-of-mind debate can be inferred from citation history of an article central to this literature. The *ISI Web of Science* citation index reports that Wimmer and Perner's seminal 1983 'Maxi' study has been cited by others 842 times.[6] Though there are many factors that determine how this particular citation index registers the way this article has been cited over the years, its data identify a general citation trend. Of the citations noted by the index, 9% of those citations were made from 1984-1990, 17% from 1991-1995, 31% from 1996-2000, and 42% of from 2001-2007.[7] These citation data show a steady increase of citations into the mid-1990's, and a clear continuation of the high level of citation into the present. Insofar as the citations made to the Wimmer and Perner piece reflect influence of false-belief task experimentation and the theory-of-mind debate, this influence has steadily increased since the publication of the study into the mid-1990's, and remains high (for the exact data, see footnote 7).

The project of this section is to determine how this debate has been configured and characterized over the years. While no resolution or consensus has yet been reached, it is certainly the case that changes have occurred to the debate through its duration. How have the positions of the debate transformed over time? How has the structure of their interactions developed? I claim that two general changes can be observed.

First, as revealed by the previous section, the theory-of-mind debate is characterized by a continuous proliferation of theories regarding the nature of this developmental trend. New theories come in many forms. Some are novel and creative interpretations of the false-belief data. Others bring conceptual structures from outside the discussion (and sometimes from outside the field) to provide the basis for a new position. Or a new position may emerge from an attempt to modify an existing theory (or theories). Also, despite the abundance of positions that have been developed, three of these positions—theory-theory, simulation, and modularity—have become comparatively well-developed and well-recognized.

Second, the history of this debate is characterized by a continuous critical exchange between those defending the various theories. There has been a climate of constant scrutiny of both the well-established and the minor positions. The defenders of the various positions of this debate often criticize the rival positions. This scrutiny occurs in terms of constructive comments, blistering criticisms, and also in terms of attempts to reconcile differences. Below, I explore the contrast between these different types of interactions. The next subsection focuses on the critical cross-analysis that characterizes the debate. The following subsection considers attempts to reconcile the rival positions.

3a. Criticism Volleys

The theory-of-mind debate has developed into an industry of micro-critique between its various positions. A deeply constitutive feature of this literature is that consistent and ongoing cross-analysis occurs between those holding each of the various positions. The most well-established positions—theory-theory, modularity theory, and simulation theory—receive criticism on a constant basis from all sides. But the figures defending these three positions also themselves administer copious critique to all comers, and especially to the other two major positions. Critiques come in many forms, such as the challenge that a theory fails to fit existing data in specific ways, the identification of conceptual problems within an account, or the claim that a theory cannot account for new data.

The history of Wellman's research provides example. In his seminal 1990 work *The Child's Theory of Mind*, he puts forward his version of theory-theory. In the process he addresses theoretical stances in the literature that can be interpreted to be in contention with his own. His handling here is not yet as systematic and fully-formed as future treatments of the debate will be; only Harris's version of simulation is critiqued in detail. Contrast this with his 1995 *Children Talk About the Mind,* which he writes with Karen Bartsch, in which a chapter is exclusively devoted to confronting many different positions. Included are critiques of different versions of simulation, Leslie's ToMM, positions which regard children's conception of self and other, Fodor's nativist proposals, positions considering the roles of larger cognitive abilities (executive functions), Perner's work on the nature of representation, and even these authors' own previous claims.

Wellman sees a value in these criticisms beyond the refinement of his own position. In *Children Talk About the Mind*, during their continuing critique of Harris's account, Wellman and Bartsch claim that the criticisms they have offered have resulted in better work in this research over all. They explain, "Based on several challenges to a simulation account (e.g., Gopnik, 1993; Gopnik and Wellman, 1992), Harris has increasingly detailed how a simulation theory would apply to the development of children's understanding of mind, how it would explain why children are adept at certain attributions of mental states before others (Harris, 1992; 1994)" (Bartsch and Wellman, 1995, 175-176).

Also consider Wellman's influential survey of false-belief task studies, which he writes with David Cross and Julanne Watson (2001). After a systematic statistical analysis of 178 studies, they argue that the general results support the theory-theory position, rather than a number of other positions they review. The issue of the journal *Child Development* in which this article appears also contains a number of responses from defenders of other positions in the theory-of-mind debate (e.g. executive function, ToMM) who each instead interpret the survey results to be compatible with their own view.

Another piece of this history which exemplifies the quantity of cross-analysis that has characterized this debate is the energetic reception which the modularity theory has received in its climb to its current status as a central position. The systematicity with which Alison Gopnik, defender of theory-theory, has critiqued this position is demonstrative. In her article "Theories and Modules: Creation Myths, Developmental Realities, and Neuarth's Boat," Gopnik sets the context for her critique by stating, "In previous work, I have outlined the distinctive features of a theory-formation account in some detail, and have contrasted it with the predictions of a simulation account (Gopnik & Wellman, 1992). In this paper, I will focus instead on the contrast between this account and modularity accounts" (1996). Gopnik's reasoning is that the simulation theory has been well-battled already; she can now focus on the modularity theory as the major newcomer (other works in with a primary focus on modularity include Gopnik and Meltzoff, 1997, in which an entire chapter is dedicated; and Gopnik, 2003). In other places, the modularity theory is critiqued by theory-theorists along side critiques of other theories (including simulation). Through the course of the debate with modularity theory, those holding a (non-nativistic) theory-theory position develop an account which holds some innate-though-replaceable knowledge to exist in infancy, but do not accept that this knowledge is structured in modular form.

This climate of constant rigorous cross-analysis deeply characterizes the history of the theory-of-mind debate. However, a counter current has also developed in the history of this discussion in which some researchers attempt to reconcile the differences between the rival positions. In the next subsection I explore this counter current.

3b. Attempts at Reconciliation

"Fortunately, there is no need to choose among these explanations. Probably, all are correct" (Siegler, 1998, 244).

A number of figures in the theory-of-mind debate, such as Janet W. Astington, Joseph Perner, Peter Carruthers, and George Botterill, approach the theory-of-mind debate with a perspective open to the possibility that several of the rival positions developed may be together correct (e.g. Astington, 1993; Astington, 1996; Botterill, 1996; Carruthers, 1996a; Perner, 1996; Botterill and Carruthers, 1999; Nichols and Stitch, 2003). Their work amalgamates rival accounts. These efforts vary in terms of their methods for combining rival accounts, in terms of their reasons for doing so, and in terms of which of the rival theories are to be invited into the amalgamation.

One approach toward reconciling the rival positions is to emphasize the fact that the phenomena at issue in the theory-of-mind debate are intensely complicated, and to claim that that each of the rival theories brings something different and crucially important to the discussion. These amalgamator accounts vary in terms of sophistication. Some simply state the reasons why it is imperative to incorporate the rival theories. Others explain that with adjustments to particular claims, the rival theories need not be interpreted to be mutually exclusive. Others detail how, with certain specific changes to the particular theories, a concrete combined theory can be created.

Another strategy for amalgamating rival positions is to argue primarily for one position, and then relate how the rival theories must also offer some part of the solution. Astington's work in *The Child's Discovery of the Mind* fits this category (1993). She explains,

> Enculturation, Nativism, Theory development, Domain-general development—how are we to decide among these alternative explanations? There is some sense in which each explanation provides part of the answer. I have expressed the view that children are developing a theory of mind, but other processes must also play significant roles. Theory formation, whether for children or scientists, always takes place

> within a particular culture, from which knowledge is acquired. And innate structures or abilities must be there at the beginning to provide a starting state that is later transformed by experience—the argument is really over how much is there and what kind of structure it is. Obviously information-processing abilities are also needed; without a certain memory capacity certain theories of mind might not be possible… Equally obviously, there can be no theoretical explanations without experience to explain, and such experience will be introspective evidence. Thus all these processes are part of the cause, the debate concerns their relative importance (Astington, 1993, 174-175).

Astington then reviews the different positions of the debate with more detail. Her view above contains several amalgamating elements, including an indication of why each theory must be an important part of the full explanation of child's theory of mind.

The work of George Botterill and Peter Carruthers is an example of an account that integrates the claims of the rival positions of the theory-of-mind debate in a technical manner. In their book *The Philosophy of Psychology*, they develop what they call a "hybrid view" (1999). Botterill and Carruthers advance a nativistic version of theory-theory, providing a modular account of the 'theories' that determine a child's understanding of other minds. In developing this view, these authors consistently contrast it with simulation theory. Despite arguing against simulation, their position is 'hybrid' because it provides a role for simulation in the acquisition of a theory of mind. They write, "So far our approach has been resolutely anti-simulationist. Yet we do want to acknowledge that there is a place for simulation as an enrichment of the operation of theory. Note that sometimes the only way of dealing with a mind-reading question is to use your own cognitive resources" (Botterill and Carruthers, 1999, 89). Botterill and Carruthers do not simply emphasize the importance of simulation; they articulate its specific function in terms of their account. In this view, the simulation process 'enriches' a child's a theory of other minds. They report, "Superficially, we think that what simulation is involved in, is the process of *inferential enrichment*—that is, that we can input the 'pretend beliefs' of another person into our own system(s) of inferential processing, and then use the output of derived beliefs for attribution to the other" (Botterill and Carruthers, 1999, 89-90). In this view, simulation-type inferences *only* work to enrich a child's theory of mind. They are not involved in the generation of the child's understanding of others' minds.[8]

Due to these kinds of reconciliatory moves, the general rules for participating in the debate have shifted. Carruthers's review of Goldman's 2006 book *Simulating Minds* is revealing. He explains,

> The book does have one major flaw, however. This is that Goldman sets up the dialectic in such a way that simulation theory gets to win out over the opposition provided that simulation plays *some* role in mind-reading; and conversely theory-theory and modular approaches both lose if mind-reading turns out *not* to be *entirely* theory-driven, or *entirely* modular. This asymmetry in treatment is unwarranted. One reason is that many writers who call themselves "modularists" or "theory-theorists" now accept that mind-reading involves *both* simulation *and* modules and/or theory (Carruthers, 2006).

This quotation exemplifies the fact that at this current point in the debate's history, it is not enough for defenders of a major position to simply argue their proposed mechanism plays *some* role in the development of child's theory of mind. Enough attempts have been made to amalgamate rival theories that one advancing a position is required to contend with the established positions, and also to contend with the prospect that positions may be combined in some way. It is now the case that one defending a position in this debate must consider whether or not one's account operates in exclusivity, and if so, how.

4. The Changing Structures of the Theory-of-Mind Debate

In the next chapter of this dissertation, I revisit the details of the theory-of-mind debate. In the first section, I show how the theory-of-mind debate is an example of a scientific debate tradition, and apply its framework of concepts (e.g. debate nodes, problem hub, debate maintenance) to the features of this case study. The changes to the positions, structures, and relationships of this debate are analyzed in detail in the second section of that chapter. For that project, it is helpful to first develop a general summary of the changes that have occurred in the structure of the theory-of-mind debate through its history.

The theory-of-mind debate evolves in a way primarily characterized by an increase in the number of positions, and a growing and changing exchange between those positions. A large number of theories are developed to account for the developmental trend identified by the false-belief tasks. Some positions offer novel understandings of these data; some creatively amalgamate existing theories; some import perspectives from other conversations. Among this proliferation of positions, three are regarded as central to this debate. Each of these three well-established positions shares a complex relation to the other two. There are also significant intra-position debates between those holding different versions of the same position.

The three major positions that rise to the status of being considered 'well-established' in this literature are modularity, simulation, and theory-theory. The rise of these positions is significant to the definition of the theory-of-mind debate itself. What it means to do work on the topic of child's theory of mind includes responding to these well-established positions. As the conversation about these three positions develops through the history of this debate, the theory-of-mind debate emerges as a recognized discussion within the discipline.

Several events set the context for the theory-of-mind debate to develop; the false-belief task is invented and refined, autism versions arrive, and appearance/reality tasks are applied to the topic. Theory-theory and simulation emerge as positions with regard to child's theory of mind in the 1980's as the basic experimental data are discovered. Some accounts of this history take simulation theory to emerge as a response to the popularity of theory-theory positions in the larger developmental psychological literature (e.g. *Mind and Language* Introduction, 1992; Carruthers and Smith, 1996; Stone and Davies, 1996). In any case, the theory-of-mind debate becomes sedimented as an established discussion as major representatives of the theory-theory camp (e.g. Gopnik, Wellman) and the simulation camp (e.g. Goldman, Gorman, Harris) engage one other systematically in the late 1980's and early 1990's.

Though reference to a possible innate-knowledge-type interpretation of child's theory of mind data existed in the literature, modularity theory does not emerge as a central contender until the early 1990's. Versions of the modularity theory, especially Fodor's account and Leslie's ToMM account, garner much response. The modularity theory takes its place as an influential position by the mid-1990's.

Each of the three central positions receives a great deal of analysis and criticism through the duration of this history.

Each of the three central positions is analyzed and criticized continuously through the history of this debate. These three positions are critiqued by those reviewing the work on this topic, by those developing new positions, and especially by others defending the rival central positions. The three central positions also gather critique from members of their own camp as intra-position debates develop. Those who maintain the three central positions further elaborate their claims in response to criticism, defend their positions, and comment critically on other positions. Part of what it means to hold a well-established position in this debate is that one's position receives considerable critical engagement. If an ascending position (such as executive function) is to gain a status comparable to the three well-established theories, it must become so definitive that responsible participation in the discussion is defined by engaging it.

The positions of this discussion (both major and minor) have been straightforwardly criticized through a number of approaches. Critics evaluate a theory's conceptual coherency. Positions are accused of failing to adequately account for existing data. Positions are assessed in terms of their consistency with new data as they emerge. Those holding popular positions can find full-time employment countering, adjusting for, and reinterpreting these challenges.

I have also suggested that attempts are sometimes made to amalgamate theories in this discussion; there are efforts to develop positions that include parts of rival theories. These efforts occur in many ways. Some downplay the conflict between rival theories by reinterpreting them. Some adjust rival theories so that parts of each can be combined. Some simply emphasize the way that each different theory brings something different and essential to discussion regarding child's theory of mind. These efforts at amalgamation themselves open the doors for further critical engagement. Some are prompted to defend pure non-amalgamated positions. Some contrast different amalgamations. Others accept a certain level of amalgamation, but defend one of the positions of the debate as primary.

In sum, the evolution of theory-of-mind debate has generally been characterized by a proliferation of positions regarding the child's theory of mind data. Within these positions, three have attained a central status. Engaging in the debate is at least partially defined as confronting these central positions. As well as increasing in number, the positions in this debate have grown and shifted in terms of their relations to one another. The primary relationship between rival positions is one of technical cross-examination. A counter current of attempts to bring together rival positions is also evident.

Notes for Chapter 4

1. Helpful reflection on the use of the term 'theory of mind' appears in (Astington, 1998).

2. Unlike the synaptic vesicle debate of chapter three, there exists a vast amount of secondary review and reflection in the developmental psychological literature regarding child's theory of mind research and debate. Helpful summaries and histories include (e.g. Astington, 1993; Mitchell, 1996; Mitchell, 1997; Flavell, 2000; Flavell, 2004; Siegler and Alibali, 2005). Also, many summaries of the key experiments and various debate positions occur in books and articles which themselves are active partisan contributions to the debate. Davies and Stone have edited a collection of seminal papers in this work, and refer to this discussion as "the theory of mind debate" (1995). What is original to

this case study here is the tracking of these positions as they become a debate, and how they, and their relations to one another, change over time as the debate progresses.

3. Examples of brain imaging studies that relate to child's theory of mind research include (e.g. Fletcher et al., 1995; Gallese and Goldman, 1998; Vogeley et al., 2001; Gallagher et al., 2003; Gallagher and Frith, 2003; Saxe and Kanwisher, 2003).

4. One could add reflections on this research by philosophers (e.g. Kitcher, 1988; Sheets-Johnstone, 2000; Gallagher, 2001; Bishop and Downes, 2002).

5. The theory-theory (as it relates to the subject of child's theory of mind) is often grouped with other versions of theory-theory which relate to other topics on developmental psychology, such as children's understanding of physics and biology (e.g. Carey, 1985; Carey, 1988; Keil, 1989). The work of Susan Carey, reviewed in section two of chapter two, is a major representative of this work.

6. Note that the *ISI Web of Science* index primarily keeps track of citations made by articles appearing in a restricted list of journals.

7. The citation rates of Wimmer and Perner's 1983 false-belief study according to the *ISI Web of Science* citation index, accessed on 5/5/2007:

2007-(as of May)-25 times
2006-67 times
2005-52
2004-56

2003-61
2002-44
2001-50
2000-59
1999-57
1998-60
1997-34
1996-54
1995-37
1994-35
1993-29
1992-15
1991-31
1990-15
1989-15
1988-16
1987-10
1986-14
1985-2
1984-4
1984-1990: 76 =9.0261%
1991-1995: 147 =17.4584%
1996-2000: 264 =31.3539%
2001-2005: 263 =31.2351%
2001-2007: 355 =42.1615%

8. Botterill and Carruthers and clear and forceful regarding this point. They write, "Theory-theory can quite happily make this much in the way of concession to simulation. What theory-theory should not concede is that simulation is needed for anything other than inferential enrichment, going from already attributed goals to further sub-goals. In particular, theory-theory should deny that our conceptions of mental state types are provided by simulation, as well denying it any role in the prediction of action from intention and the prediction of intention from desire" (1999, 90-91).

Chapter 5: The Dynamics of Scientific Debate Traditions

In chapter two I developed the notion of 'scientific debate tradition' to conceptualize the structures of scientific debate. A first task for this chapter is to show how the case studies of chapters three and four instantiate this concept. Next, I abstract concepts regarding the dynamics of changing scientific debates from these concrete histories. I finish this chapter with a discussion of the implications of my account of scientific debate for philosophical views of scientific progress.

In the first section of this chapter, I review the features of the notion of the scientific debate tradition and consider the ways that the synaptic vesicle debate and the theory-of-mind debate exhibit these features. Section two consists of the core contribution of this chapter: the identification of specific ways that scientific debate traditions change over time. I develop concepts which denote different modes (or 'modalities') of relation that may occur between rival debate nodes. In the third section, I contrast the claims of this dissertation with other accounts of scientific progress from the philosophy of science.

1. Two Examples of Scientific Debate Traditions

In chapter two I developed the notion of the scientific debate tradition and its associate framework of concepts. The term 'scientific debate tradition' refers to the rival positions of a scientific debate, the relationships between these positions, and the ways these positions and relationships change over time. I use the term ‘debate nodes’ to refer to the rival positions within a debate tradition. A debate tradition also possesses a ‘problem hub,’ which refers to the central issues that guide the debate. The problem hub of a tradition determines the particular configuration of the tradition’s nodes; a collection of positions regarding a general topic may bear different relations to one another depending on what particular issue is the debate’s central guiding question.

Some nodes of a scientific debate tradition are distinguished as ‘primary nodes.’ Primary nodes are those that stake a position with respect to the guiding issues (or ‘problem hub’) of the debate. A ‘branch quarrel’ is a debate between positions that together subscribe to a primary node. The rival positions of a branch quarrel are called ‘branch nodes.’ The terms ‘primary’ and ‘branch’ should not connote different levels of importance; they simply indicate the configuration of the work with respect to the problem hub of the tradition. It is possible for the claims of positions of branch debates to ‘jump tiers’ and have effects on the primary debate. Positions staked in the primary debate can also jump tiers and have effects on the quarrels within the branches.

I identify three ‘interrelations’ shared by rival nodes of a scientific debate tradition. One is called ‘co-validation.’ This refers to the way a position both receives recognition from its rivals and provides recognition to its rivals through participation in a debate. An example is when one implicitly acknowledges a rival position as worthy of engagement by offering critique of that position. A second interrelation between positions of a scientific debate tradition is called ‘co-analysis.’ This refers to the way that, in some respects, rival positions can be interpreted as collaborating in the effort to investigate the object of study. For example, a position may receive constructive criticism from rivals. A third interrelation is named ‘co-composition.’ This refers to the way that a position’s identity and content become partially defined through participation in the debate. For example, a position can become defined by its contrast with rivals. A position might also adopt some of its rivals’ successful claims or data.

I also comment on the processes of establishing a debate, maintaining a debate, establishing a position within a debate, and maintaining a position. The term ‘obligatory debate node’ refers to a position which has become established within a debate tradition in so strong a way that others working within the tradition are obligated to engage it. As a debate advances, the participants may be required to engage in ‘maintenance’ efforts to keep the discussion active, or to keep it coherent as a debate. For example, if there are two obligatory nodes in a debate tradition and one makes a major breakthrough, members of the opposing node may be required to engage in maintenance efforts in response. This maintenance may include reinterpreting or critiquing their rival’s breakthrough. If successful, they can maintain the debate’s status as active, and maintain their own position’s place as an obligatory node of the discussion.

In subsections *1a* and *1b*, the details of the case studies of chapters three and four are used to instantiate this framework of concepts associated with the notion of the scientific debate tradition.

1a. The Synaptic Vesicle Debate Tradition

In chapter three, I explored the synaptic vesicle debate, a discussion in the field of neurobiology over the nature over the nature of neurotransmission. I claim that this discussion is an example of a scientific debate tradition.

The two primary nodes of this debate tradition are the Heuser and Ceccarelli models. Branch quarrels occur within each of these two primary nodes. For example, De Camilli and Takei's challenge the original version of the Heuser model is an example of a quarrel within the Heuser branch. The debate between the various interpretations of kiss-and-run is an example of a branch quarrel within the Ceccarelli branch.

The synaptic vesicle debate tradition possesses a problem hub. It is defined by the attempt to determine the nature of synaptic vesicle exocytosis and endocytosis. This problem hub can be made explicit through the use of 'debate tree' diagrams. A debate tree for the synaptic vesicle debate tradition can be charted with two primary nodes and also branch nodes stemming from each primary node. See Figure 5.1.

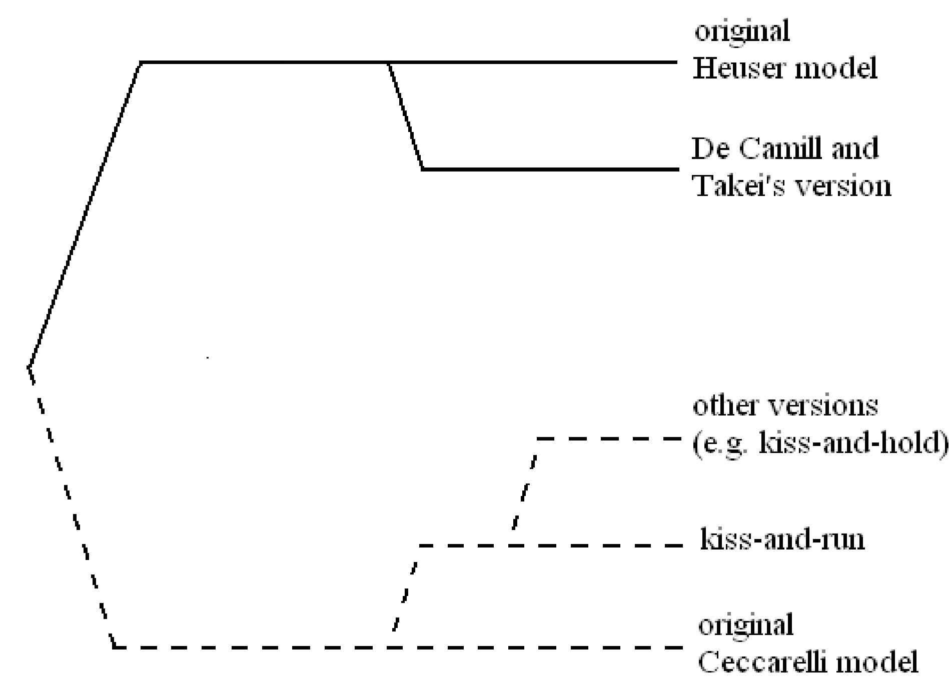

Figure 5.1. In this tree diagram of the synaptic vesicle debate tradition, the solid lines represent the Heuser model debate node. The dotted lines represent the Ceccarelli model node of the debate.

Several features of the history of the synaptic vesicle debate tradition are charted on this debate tree diagram. The two primary nodes are marked by the solid and dotted lines. The branch quarrels of these nodes are also charted. The x-axis of the diagram roughly marks the progression of time from left to right. The diagram captures the fact that the debate began as a dispute between the original versions of the Heuser and Ceccarelli models. The relative emergence of the other versions of these models is reflected by the stemming branch nodes in the diagram (e.g. the various versions of kiss-and-run emerge after the general kiss-and-run model is developed by Ceccarelli's students).

A tree diagram of the synaptic vesicle debate tradition can also be used to demonstrate that the problem hub of this tradition significantly configures the relationships between the various nodes. Imagine a debate comprised of this same research but which regard a different central issue: whether or not re-forming endocytotic vesicles require a stop at an intermediate endosome. The problem hub of this fictional 'endosome debate tradition' would arrange the positions of this discussion differently. The original version of the Heuser model (i.e. pre-De Camilli and Takei) understood the process of endocytosis to involve a vesicle first pinching and separating from the membrane, and next visiting an endosome in order to become fully-formed. In their alternate version of the Heuser model, De Camilli and Takei claim that fully-formed vesicles emerge from the very process of pinching and separating from the membrane; in this view, no stop at an endosome is required. Also, the various versions of the Ceccarelli model claim that vesicles do not visit an endosome during endocytosis. A debate tree diagram can be used to chart the relationships between these positions with regard to the endosome issue. See Figure 5.2.

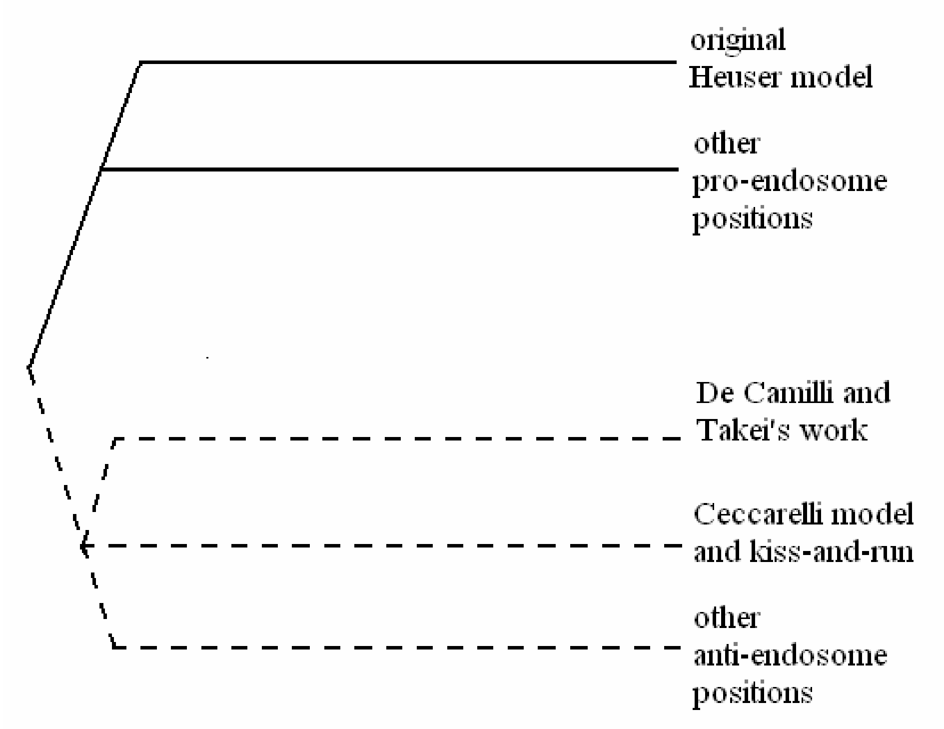

Figure 5.2. In this tree diagram of the fictional 'endosome debate tradition,' the solid lines represent the primary node which contains positions that hold a 'pro-endosome' position, i.e. that an endosome visit is necessary for vesicle re-formation. The dotted lines represent the primary node of the debate which contains positions that hold an 'anti-endosome' position, i.e. that an endosome visit is not necessary for the re-formation of vesicles.

The contrast between Figures 5.1 and 5.2 makes the problem hub of the synaptic vesicle debate tradition explicit; if another issue were to be used to guide the organization of this work, the positions of the debate would be configured differently. The example of the fictional endosome debate tradition would elevate the quarrel between the original Heuser model and De Camilli and Takei's work to the level of the primary debate. In the endosome debate tradition, the central issue of the synaptic vesicle debate would be refigured as a branch quarrel between the De Camilli and Takei position and the Ceccarelli model.

The two primary nodes of the synaptic vesicle debate tradition—the Heuser and Ceccarelli models—are also examples of obligatory debate nodes. Responsible participation in the discussion regarding synaptic vesicle fusion often appears to require acknowledging these two positions. The current major forms of these positions, i.e. the kiss-and-run model and the work of De Camilli and Takei, may also attain an obligatory debate node status—if not wrestle this status away from the original Ceccarelli and Heuser versions.

The three kinds of general interrelations between rival nodes—co-analysis, co-validation, and co-composition—are shared between the rival nodes of the synaptic vesicle debate. The rival positions co-validate in the varying degrees that each recognizes the other as an important debate participant or research contributor. An example of this recognition is the new titles that the Ceccarelli school uses to refer to the positions of this debate: 'the kiss-and-run model' for their fusion pore version of the Ceccarelli model, and 'the classical model' for the Heuser model.

These rival nodes co-analyze in several ways. The two positions offer criticism to one another. It also occurs in instances in which these positions accept some of their rival's claims and build on them. In some instances, the work of one position is guided by moves made by the other. For example, the Ceccarelli school's work on ectopic fusions (reviewed in chapter 3 subsection *3e*) is in some ways a response to the Heuser school's work on late-forming dimples far from the site of fusion. Heuser, in turn, comments on the ectopic fusion work.

The two primary nodes of the synaptic vesicle debate also co-compose; each position has part of its identity and content wrapped up in its place in the debate. As these rival accounts adopt one another's claims, each position contributes to the content of the other. For example, in the foundational formulations of the kiss-and-run model, the students of Ceccarelli acknowledge that the Heuser pathway exists. They claim that the kiss-and-run neurotransmission also sometime operates via the kiss-and-run pathway. Also, the work of De Camilli and Takei accepts at least the possibility that the kiss-and-run pathway also exists (take, for example, the question marks in Figure 3.7).

The synaptic vesicle debate evolves through the course of its history. Changes occur in terms of the content of the views expressed by its participants, in terms of the number of branch nodes that stem from the primary nodes, and in terms of the relationships between the rival camps. These changes occur despite the fact that the debate has not yet come to a close. The goal of this chapter is to find ways to talk about how these changes happen both in the synaptic vesicle debate tradition and in scientific debate traditions more generally. It is almost time to address these changes in the relationships between rival nodes of scientific debate traditions with greater specificity. However, it important to first put the theory-of-mind debate into the context of the notion of the scientific debate tradition so that examples of debate evolution can be derived from its history as well.

2a. The Theory-of-Mind Debate Tradition

In chapter four, I explored the theory-of-mind debate, a discussion in the field of developmental psychology over the nature of children's understanding of others' minds. I claim that this discussion is also an example of a scientific debate tradition.

The notions of 'primary nodes,' 'branch quarrels,' and 'obligatory debate nodes' are useful for sorting out the large number of positions that have been staked in this debate. It is important first to identify which of the primary nodes of this debate are obligatory debate nodes. The 'primary' nodes are those positions which refer to the central defining issues of the tradition (i.e. the problem hub). But only those primary nodes which have become so essential to the debate tradition to have become part of the tradition's very definition are obligatory debate nodes. Only those three most well-established positions—theory-theory, simulation, and modularity—maintain an obligatory status in practice. Responsible participation in this debate tradition appears to minimally include engaging these three well-developed, well-established positions.

There are branch quarrels within the primary nodes of this tradition that have played very important roles in this history. Branch quarrels exist in all three of the obligatory nodes and some of the non-obligatory primary nodes. For example, the simulation node contains an ongoing productive branch quarrel regarding 'online' and 'offline' positions. The executive function position is also a primary node which contains a number of branch nodes, such as the Cognitive Complexity Control theory, and the inhibitory control versions. There are also instances in the history of the theory-of-mind debate tradition in which branch positions have jumped tiers with significant impact on the primary debate. For example, Leslie's ToMM version of modularity both contributes to intra-modularity branch debates and defends modularity in the primary debate of the tradition.

The theory-of-mind debate tradition can be charted on a debate tree diagram. This diagram can be used to depict graphically the relative historical emergence of the nodes of this debate. It can also be used to chart the relationships between various primary and branch nodes. In Figure 5.3, the tree portrays only the following four positions: theory-theory, simulation, modularity, and executive function. In Figure 5.4, the tree is expanded to include many of the branch quarrels that make up these four primary nodes.

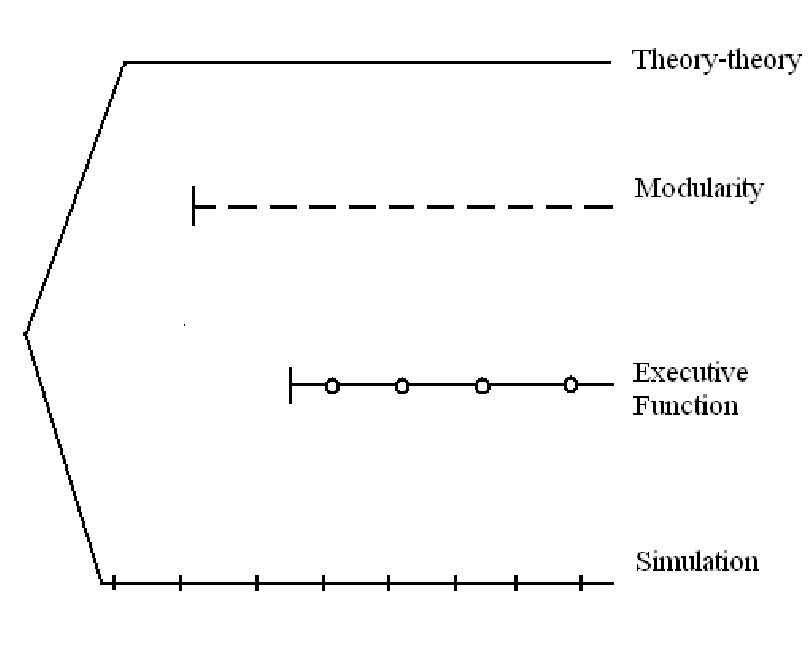

Figure 5.3. In this tree diagram of the theory-of-mind debate tradition, the solid line represents the theory-theory node, the dotted line represents modularity, the line dotted with circles represents executive function, and the line with vertical dashes represents simulation.

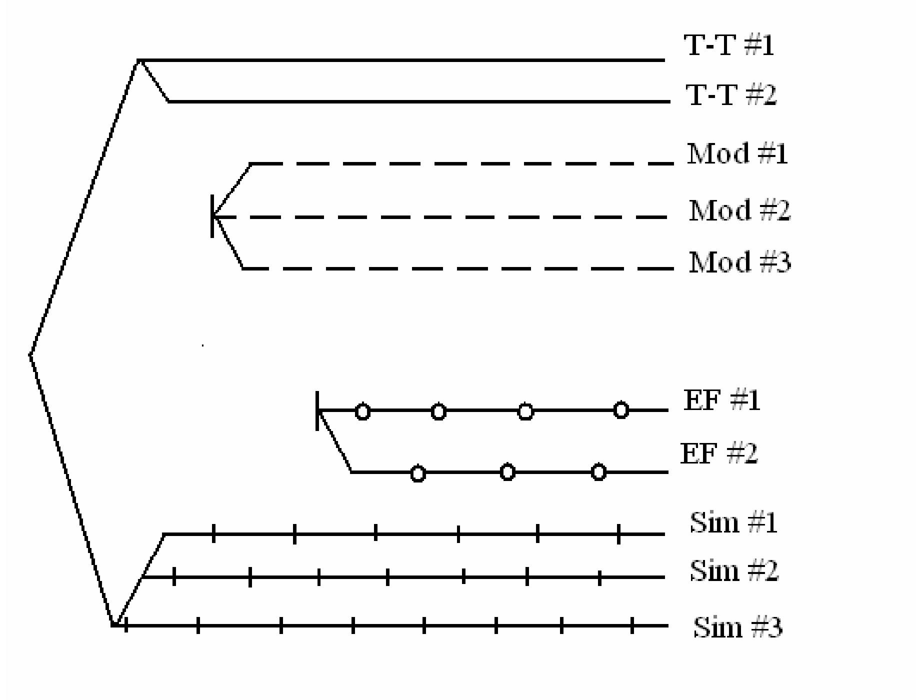

Figure 5.4. In this expanded debate tree, lines are added to represent several of the positions which constitute the branch quarrels of the various primary nodes. The positions are listed in the text below.

The contrast between the two debate trees appearing in Figures 5.3 and 5.4 graphically displays the organization of various positions with respect to the guiding issues of the tradition, and the guiding issues of the branches. Figure 5.3 depicts the debate in terms of the relative temporal emergence of four important primary nodes: theory-theory, modularity, executive function, and simulation. The x-axis denotes the passage of time from left to right. In this depiction, the tradition emerges as a debate between simulation and theory-theory, modularity emerges more recently, and executive function emerges more recently still.

Figure 5.4 depicts the same four primary nodes as 5.3 and adds several of the branch positions within those nodes. Just as Figure 5.3 includes only a select number of primary nodes, Figure 5.4 includes only a selection of important branch nodes. They are as follows:

T-T #1: Wellman's version of theory-theory
T-T #2: Gopnik's version of theory-theory
Mod #1: Fodor's version of modularity theory
Mod #2: Baron-Cohen's version of modularity theory
Mod #3: Leslie's ToMM version of modularity theory
EF #1: inhibitory control versions of executive function
EF #2: the Cognitive Complexity Control theory version of executive function
Sim #1: Gordon's version of simulation
Sim #2: Goldman's version of simulation
Sim #3: Harris's version of simulation

The problem hub of the theory-of-mind debate tradition is roughly defined as the project of addressing the developmental trend revealed by the false-belief tasks and other related experiments. This problem hub significantly configures the relationships between the various nodes of this debate. This fact can be highlighted by imagining other discussions to which the positions of this tradition are relevant. For example, consider the much larger and more general discussion within psychology regarding whether a person's character is primarily the result of upbringing and environment or instead inborn (say, genetic) features: i.e. the nature/nurture debate. This large scale debate includes consideration of many positions on many topics, including the various positions regarding child's theory of mind. Simple versions of the three obligatory debate nodes of the theory-of-mind debate tradition belong to different sides of the nature/nurture debate. Both simulation and theory-theory are part of the 'nurture' node. Modularity is part of the 'nature' node. With this wider-angle view of the field of psychology, the simulation and theory-theory accounts appear as rival positions of a branch quarrel included within the 'nurture' primary node of the nature/nurture debate.

The nodes of this debate tradition share the three general interrelations I identify as co-validation, co-analysis, and co-composition. Co-validation is exemplified by the rigorous and continuous way that those defending the various debate nodes cross-analyze one another. Considering the large number of positions staked within this discussion, one's position must be widely interrogated by others in order to become well-recognized. The fact that strongly-sedimented obligatory debate nodes have emerged in the cacophony of primary positions in this discussion reveals the high level of engagement that these few well-established nodes have received. The commotion that the tradition as a whole generates brings attention not only to those obligatory positions but also to the tradition as a whole; this large-scale debate constitutes a major conversation within the field of developmental psychology. Contributors to this discussion benefit from the already-established audience for this work, and the widely-recognized concepts and data.

The interrelationship of co-analysis is also evident within this tradition. There is a sense in which the works of the rival nodes the tradition advance together in the attempt to increase our understanding of child development. New experimental results are interpreted through the various rival accounts. New ideas and data are rigorously cross-analyzed. The debate process productively refines positions.

The nodes of this debate tradition also share an interrelationship of co-composition. The status of being part of this debate has specific effects on the identities and contents of the various nodes. An example of positions' identities becoming informed by participation in the debate is the effort to differentiate modularity accounts from theory-theory accounts; though some proponents of modularity use the term 'theory' centrally in their account, these theorists call their position a 'nativistic' account, rather than 'non-nativistic' (i.e. non-modularity theory-theory). The identities and claims of these nativistic versions of modularity emerge from, and are explicitly marked by, their contrast with the non-nativistic theory-theory position. There are also instances in the history of this debate tradition in which research results offered by one position are reinterpreted and absorbed by other positions. In these cases, the absorbing position has part of its content born in the history of the larger discussion. In addition, there are several efforts made to combine opposing views, preserving the best parts of each; these amalgamated positions have their substance and their identity deeply informed by the debate context.

2. Modalities of Scientific Debate

With the concept of 'the scientific debate tradition' now instantiated by the two case studies of chapters three and four, the context is set for a more focused examination of the relationships shared by rival positions. It is also time for an analysis of how these relationships change. In this section, I develop a series of concepts for categorizing the specific changes that occur within ongoing scientific debate traditions.

I use the term *debate modality* to refer to a scientific debate tradition's particular configuration of nodes, and also the particular character of the relationships between them.[1] If the tradition undergoes a change in the composition of its nodes, or in the kind of relationship shared between them, the tradition has experienced a shift in its 'modality.' A debate tradition can be tracked in terms of historical shifts in debate modality. Above, I have used the term 'debate evolution' to refer to the changes that

[1] Endnotes for this chapter are found on pages 245-250.

occur in debate traditions over time. Debate evolution occurs as a tradition experiences changes in its modality.

It is helpful to differentiate between two different types of debate modality: *morphological modalities* and *relational modalities*.

With the term 'morphological modality,' I refer to a scientific debate tradition's particular composition of nodes, and their particular structural configuration. A given morphological modality is defined by the positions that make up the debate, and their statuses as primary and branch nodes. A shift in a debate tradition's morphological modality (or 'morphology') occurs with any change to the debate's shape, such as the addition of a new node, the combination of two formerly opposed nodes, the fracture of one node into two (or more), the jumping of tiers by a branch node to the primary debate, or any number of occurrences. An evolving debate tradition may experience rapid shifts from one morphological track to another, or may continue indefinitely—in slump or in productivity—within the same morphology. Because the project of cataloguing possible morphological modalities requires the review of a large survey of scientific debate traditions, and in this dissertation I investigate only two major case studies, in the first subsection below I remark only briefly on this topic.

With the term 'relational modalities,' I refer to the particular character of the relationships between the nodes of a scientific debate tradition. A given relational modality is defined by the specific conventions of the rivalry between the nodes of a debate. A shift in a debate tradition's relational modality (or 'relationality') occurs with any change in the way the rival nodes relate to each other, i.e. change in the manner in which the debate is conducted. In what follows, I identify a number of specific relational modalities that can characterize debate traditions. These concepts are abstracted from the case studies of previous chapters. The list I develop does not comprehensively account for all modes of rivalry possible for scientific debate traditions; the empirical data from which these concepts are abstracted is too limited to create a comprehensive list. However, these concepts are enough to provide an adequate general impression of what it means for the relationships between the nodes of an ongoing scientific debate tradition to change.

Subsection *2a* considers morphological modalities. *2b* considers relational modalities. I conclude this section with a third subsection on the potential practical value of the project of cataloguing debate modalities.

2a. Morphological Modalities

The morphological modality of a scientific debate tradition is defined by the particular primary and branch nodes that exist at a give time. Over time, a scientific debate tradition may experience radical changes to its morphology—shifts from one morphological modality to another. Examples of morphological shifts include the addition or subtraction of nodes, the combination or division of nodes, and a node's jumping from one branch to another (such as a branch node that has effects on the primary debate). Shifts in a debate tradition's morphological modality can be charted on debate tree diagrams.

It is important to note that a shift to a debate tradition's morphological status does not necessarily correspond to the generation of new data, the establishment of widely-accepted facts, or even a sense of important progress in the minds of the debate participants. A shifting morphology may coincide with a flourishing and productive debate, or it may reflect the fact that its participants are desperately searching for a source of reinvigoration. Also, an unchanging morphology may coincide with stagnation, or it may reflect a structure which has remained consistently productive over time.

Of the few examples of debates and debate traditions reviewed in this dissertation, several instances of shifts in morphological modality can be identified. For example, the history of the theory-of-mind debate tradition is rife with shifts to its morphological modality, mostly in the form of a proliferation of new nodes (both primary and branch). Also, the case of the debate between Spelke and Carey from chapter two provides an example of a morphological change in which two nodes combine and together enter into further debate with others. In contrast, the history of the synaptic vesicle debate tradition remains relatively static in terms of morphology, continuing in a modality involving two major primary nodes. However, some morphological changes have occurred to the synaptic vesicle debate tradition, such as the addition of the kiss-and-run theory to the Ceccarelli node, and it rise to become the main representative of that node within the primary debate against the Heuser model.

2b. Relational Modalities

The relational modality of a scientific debate tradition refers to the character of the relationships between its nodes at a given time. It is the nature of the rivalry between nodes. It is the structure of the interactions between the participants of a debate. It is the mode of the debate.

In what follows in this subsection, I identify number of specific relational modalities. They are grouped in *pairs*. These pairs represent structures of debate between which a tradition has the potential to shift back and forth. I identify three pairs of relational modalities: *amelioration* vs. *productive controversy*, *identical-data* vs. *discordant-data*, and *intervolved* vs. *independence-oriented*. I define these pairs of relational modalities one at a time, and consider the ways the three case studies explored above—the theory-of-mind debate tradition, the synaptic vesicle debate tradition, and the Carey/Spelke debate—each instantiate these pairs of concepts.

Scientific debates are complex enough that they can take up several different modalities at once. This is case for the examples I explore below. Examples of all three pairs of relational modalities are identified in each of the three case studies. Also, though these concepts are arranged into pairs, and though scientific debate traditions generally oscillate between the two modalities of a pair, a tradition may take up both modalities of a pair at once. For example, below I explore instances in which a debate tradition takes up a particular relational modality in a primary way, and yet also retains the other modality of the pair as a secondary aspect of the discussion.

Pair #1. amelioration vs. productive controversy

With this complimentary pair of relational modalities, I refer to two associated types of rivalry: one in which debate participants stress the incompatibility of rival nodes, and one in which they consider the potential for overlap between the rival accounts.

A scientific debate tradition has a modality of *amelioration* if some participants attempt to reconcile the disagreement between opposing nodes. In this modality, some participants in the debate try to resolve differences within the tradition, or try to reinterpret positions to show them to not be in opposition. An example of an ameliorative move is an effort to show that nodes which have been established in opposition are actually each addressing different (though related) issues. The nodes could be reinterpreted not as rivals, but as each constituting a compatible part of a larger project. Another example is the development a new position which combines parts of rival positions.

A scientific debate tradition instead has a modality of *productive controversy* if those involved attempt to further differentiate nodes, and attempt to intensify the dispute between them. In this modality, the rival positions of a debate tradition become more entrenched and more refined. Their dispute is further cultivated. For example, participants in a debate characterized by productive controversy emphasize the differences between the rival nodes.

The two relational modalities of ‘amelioration’ and ‘productive controversy’ are a ‘pair’ because *each sets the context for efforts to shift to the other modality*. For one to consider the possibility that the nodes of an established scientific debate should be ameliorated, the debate must first be recognized as a productive controversy; an effort to ameliorate a rivalry is one which attempts to disrupt the established order of a discussion, an order defined by entrenched dispute. Correspondingly, the amelioration of aspects of a tradition's rivalry creates opportunities for new productive controversies to develop.

The two concepts of amelioration and productive controversy operate in both of the chapter-length case studies offered in this dissertation, and also in the debate between Carey and Spelke reviewed in chapter two. I review examples of these concepts from each of these three cases:

The Theory-of-Mind Debate

Of the scientific debates reviewed in this dissertation, the theory-of-mind debate tradition is most significantly constituted by issues regarding amelioration and productive controversy. A central characteristic of this debate is the way the major nodes receive constant criticism and become deeply entrenched. The continuous cross-examination and refinement of these positions reveal this tradition to be in a modality of productive controversy. Figures such as Alvin I. Goldman, Alison Gopnik, and Henry Wellman engage in systematic efforts to address challengers and address changes to the others' positions. An abundance of forums (journal issues, article collections, monographs, conferences, etc.) provide spaces in which the dispute has been furthered. The obligatory debate nodes are well-recognized; this recognition comes from constant challenge, defense, and articulation. The mode of productive controversy so strongly characterizes this discussion that the general topic of child's theory of mind itself has become almost inextricably linked to this debate.

A growing modality of amelioration also develops within this discussion as a reaction to the success of its obligatory debate nodes at entrenching and popularizing themselves. Several researchers attempt to bring together the ideas of rival positions that have otherwise developed in contrast with one another. A variety of amalgamating strategies are utilized. There are efforts to advance specific amalgamated positions that incorporate parts of opposing positions and attend to inconsistent elements. There are also more general calls for an appreciation of the importance of each position. Though ameliorative efforts grow in prominence and influence, the debate has not shifted from a primary modality of productive controversy to one that is primarily ameliorative. A general agenda of amelioration is instead a strong 'side current' of this debate.

It is important to note that this 'side current' of amelioration is having effects on the primary 'productive controversy' mode of this debate. The ameliorative efforts change the conventions of the rivalry between positions. One defending an established position is now pressured to articulate not only the support for one's position, but also why one's account provides a complete account of the issues.

The Carey/Spelke Debate

The debate between Carey and Spelke over the nature of conceptual development in children also exemplifies the productive controversy/amelioration issue. In what I have identified as *Phase #1*, their debate has a modality of productive controversy. In this first phase of the relationship between their two positions, each researcher works to define her own position based on her own data, and each critiques of the other's claims. Each also develops responses to the challenges raised by the other.

In *Phase #2*, these two researchers reinterpret their work to not be in dispute, and they develop a new combined position. This combined position takes some aspects of conceptual development to fall under Spelke's account and other aspects to fall under Carey's. The work which leads up to this morphological change can be interpreted as ameliorative.

This ameliorative context offers the possibility for a new modality of productive controversy to emerge; a debate over which aspects of development fall under Carey's account and which fall under Spelke's account. Since their combined view holds that their original positions are not inherently incompatible, and also holds that each accounts for some concepts that a child holds, it remains to be determined which concepts develop by virtue of which mechanism. If this new debate were to emerge, it could have a modality of productive controversy.

The Synaptic Vesicle Debate

The history of the relationships between the nodes of the synaptic vesicle debate tradition also reveals issues regarding productive controversy and amelioration. The early history of this debate is characterized clearly by a modality of productive controversy. In both the earliest time of the debate in which disagreement concerned stained images, and the next phase in which the discussion turned to the interpretation of quick-frozen images, the major questions regard whether Ceccarelli's account or

Heuser's account correctly explains neurotransmission. The data from this time period are read and reread in terms each account. As these cross-analyses continue, the positions become more refined, more entrenched, and more deeply defined by their contrast with one another.

As the Heuser model becomes the dominant position of the debate, the Ceccarelli school begins to introduce ameliorative moves into the discussion. For example, in their own version of the quick-freezing studies, the Ceccarelli school considers the possibility that a Ceccarelli-type mechanism is utilized by synaptic vesicles during the first few milliseconds of fusion and that a Heuser-type mechanism may take over after this time.

De Camilli and Takei's clathrin and dynamin studies extend the dominance of the Heuser node of the debate into the 1990's and beyond. These moves are a 'maintenance' effort, keeping the debate active and open. Responding to the Heuser model's continued status of wide acceptance, the students of Ceccarelli advance further ameliorative moves as they develop the kiss-and-run model. In the foundational articles of the kiss-and-run model, these authors grant that the Heuser pathway exists and explore the possibility that a kiss-and-run pathway also exists.

Insofar as this debate tradition has entered an ameliorative modality, a context has been set for a new contemporary modality of productive controversy to develop. For example, one contemporary issue of the synaptic vesicle debate regards which kinds of neurons operate via which fusion pathway. A debate with a modality of productive controversy could develop over this issue.

Pair #2. identical data vs. discordant data

With this complimentary pair of relational modalities, I refer to two associated kinds of rivalry: one in which debate occurs over a single set of data, and one in which rivals bring different sets of data to the discussion which are interpreted to be inconsistent with one another.

A scientific debate tradition possesses an *identical-data* modality if its participants address the same general set of data. Participants of a debate tradition in this mode offer rival accounts based on the same evidence. A debate possesses a *discordant-data* modality when its different nodes each address somewhat different or completely different sets of data, yet interpret these different sets of data to offer challenge to the others' claims.

These two modalities of 'identical data' and 'discordant data' are identified as a 'pair' to highlight the way that the instantiation of one of these modalities sets the context for the debate to shift to the sibling modality; a debate over the nature of a single set of data primes the conversation for the possibility that a separate sort of data may be brought to the discussion. A debate over rival sets or kinds of data creates the possibility for the various positions to expand their accounts to address all of the data—thus shifting the tradition back to an identical-data modality.

To define these tracks further, I review the ways they are instantiated in the examples of scientific debate explored in this dissertation. They are as follows:

The Carey/Spelke Debate

The history of the debate between Carey and Spelke is an example of a change from one of these modalities to the other. In *Phase #1* of their debate, Spelke and Carey each work with a separate set of data; each researcher's work refers to children of different ages, and each regards data obtained with different experimental techniques. As they engage in debate, both researchers interpret the two different sets of data to be incompatible with one another; each understands her own data to challenge the other's claims, and defends her own claims from the critiques leveled by the other. These characteristics of *Phase #1* of this debate reflect a modality of discordant-data. Carey and Spelke's discussion shifts out of a discordant-data modality since in *Phase #2* they reinterpret the different sets of data to be consistent and to support their new combined position.

Though in this dissertation I do not explore the larger debate tradition in which Spelke and Carey's projects reside, I have reviewed one deployment of their combined theory in *Phase #2*: a disagreement with Alison Gopnik's theory-theory perspective. According to Gopnik, the data at issue in her debate with the combined Carey-Spelke position is open to multiple interpretations. This debate has an identical-data modality; both Gopnik and the combined Carey-Spelke position develop accounts of the total data offered by everyone, i.e. an identical set of data.

The Synaptic Vesicle Debate

The history of the synaptic vesicle debate tradition also provides an example of the identical data/discordant data issue. I claim that a shift from one of these modalities to the other occurs with the shift from the earlier ‘Heuser model vs. Ceccarelli model’ phase to the later ‘kiss-and-run vs. De Camilli-Takei clathrin studies’ phase of this debate.

The debate has an identical-data modality in the earlier period; in this phase, the central focus of the debate resides in whether the Heuser model or the Ceccarelli model correctly interprets the quick-frozen images (e.g. Figures 3.4 and 3.5). Each side offers a different interpretation of the same images. During this phase of the debate, each side creates images of synaptic vesicle fusion and interprets these images to support their own position. Each time one side introduces new images into the discussion, the other side is challenged to develop its own account of those data.

This debate shifts to a discordant-data modality in its later phase. This occurs as the Ceccarelli school begins to work on non-neuronal cells, and more recently on different types of neurons. It also occurs as De Camilli, Takei, and others investigate clathrin cages, dynamin invaginations, and other molecular structures involved in synaptic vesicle endocytosis. Each side now focuses on different types of data. But the debate continues. Each side interprets its own findings as conflicting with the account provided by the other side. And each side’s account is challenged by the findings of the other.

The Theory-of-Mind Debate

The theory-of-mind debate tradition is a bit trickier to define clearly in the language of identical data and discordant data. Though there has not been a large-scale shift in this history from one of these modalities to the other, the issue raised by this pair of modalities is relevant to this debate. I suggest that in general, the theory-of-mind debate tradition is primarily characterized by an identical-data modality. Also, there are pockets of a discordant-data mode of engagement.

Through its history, the primary modality of the theory-of-mind debate tradition is one of identical data; the primary positions of the debate present themselves as if they best explain *all* of the data gathered by everyone working in the tradition. In practice this occurs in many high-profile ways. Researchers introduce new data which they claim support their own position and challenge others' positions. Rivals respond with 'maintenance' efforts. These include counterarguments, challenging data that does not support their own position. Or these rivals reinterpret and appropriate the new data. Proponents of the most well-established primary nodes tend to be confident that their accounts are able to handle any new data that come along.

Yet in practice it is also the case that some positions of this tradition focus on certain types of data and experimental methods. Insofar as this is the case, these parts of the tradition possess a more discordant-data mode. For example, the modularity theory is most identified with work on autism. Though modularity theorists may claim that their position best accounts for all the data of the tradition, and its rivals may claim to better account for autism data, there is still a sense in which modularity's core support lies in autism studies. Another example is the executive function node and its development of the Card Sort studies. While the Card Sort data is an important contribution to the greater discussion, these studies are born within this claims and vocabulary of this node, and these data are primarily examined by defenders of the executive function account.

Pair #3. intervolved vs. independence-oriented

With this complimentary pair of modalities, I refer to two associated kinds of rivalry: one in which rival positions are taken to have developed separately and without debt to one another, and one in which positions acknowledge contributions made by rivals.

A scientific debate tradition possesses a relational modality of *independence orientation* when its members do not regard their rivals to bring essential contributions to the discussion. This attitude may be stated explicitly, or may be revealed by participants' practices. In contrast, a debate tradition possesses an *intervolved* modality when its members regard their own positions to be indebted to the work of rivals. Of course, acknowledging rivals' accomplishments does not necessarily build consensus; the accomplishments of rivals may be acknowledged so that they can be confronted, addressed, or possibly appropriated.

In both debates with independence-oriented modalities and those with intervolved modalities, rivals are acknowledged and engaged. The difference between these modalities concerns the value rivals place on one another's work. Participants in a debate tradition with an intervolved modality take the debate process to further enrich the exploration of the issues. The accomplishments of rival positions, or the critiques they have leveled, are acknowledged to influence one's own position. In contrast, participants in a debate tradition with an independence-oriented modality take their own work to develop separately from the efforts of rivals. One engages rivals, but only in attempt to dispatch them.[2]

It is helpful to contrast the issues raised by the amelioration/productive controversy pair and the intervolved/independence-oriented pair of relational modalities. The amelioration/productive controversy pair refers to the degree to which debate participants understand the rival positions to be incompatible with one another. Are attempts made to render rival positions consistent? Or do rival nodes instead stress their differences, and further entrench their positions? In contrast, the intervolved/independence-oriented pair refers to the degree to debate participants understand rival positions to contribute to one another's work. Do participants claim to be provoked or even influenced by rivals' ideas, data, or criticisms? Or do participants instead claim autonomy, and dismiss rival positions entirely?[3]

These two tracks are identified as a 'pair' because the instantiation of one sets the context for a debate tradition to potentially shift to the other. A debate tradition in a modality of independence orientation sets the context for the debate to possibly shift to one in which the rival positions begin to influence one another. As a debate progresses, the formerly independent nodes may grow more involved in one another's work. A debate tradition in a modality of intervolution sets the context for the debate participants to possibly begin to demonstrate their autonomy. Intervolved nodes may eventually attempt to gain independence from their rivals, dismissing their contributions as old news, and defending their own account as the only valuable position.

Examples of this pair of tracks occur in the case studies explored in this dissertation. They are as follows:

The Synaptic Vesicle Debate

The history of the synaptic vesicle debate is an example of a scientific debate tradition which experiences shifts in modality between intervolution and independence-orientation. As this debate begins in the 1970's, the two rival models develop largely independently of one another. As each begins to engage the other in debate, the other's model is denied whole sale. For example, Heuser and his colleagues introduce the quick-freezing technique as one which should "settle this controversy" (chapter 3, subsection *3d*). In this early stage of the synaptic vesicle debate tradition, the modality is one of independence-orientation.

By the end of the 1980's the situation changes; by this point, the members of the two nodes each recognize their rival as making important contributions. The Ceccarelli school struggles to account for the clathrin-coated pits which Heuser and his colleagues claim to be synaptic vesicles undergoing endocytosis. Heuser and his colleagues are faced with accounting for the ectopic fusions investigated by the Ceccarelli school. This transition in the relationships between these rival nodes is a shift from an independence-oriented modality to an intervolved one.

This intervolved status deepens as these researchers more openly consider the possibility that both of the pathways investigated by these two positions may operate during neurotransmission. For example, as the students of Ceccarelli develop the kiss-and-run version of the Ceccarelli model, they claim that both the kiss-and-run and the Heuser accounts together describe the process of neurotransmission. The debate has become so intervolved that the kiss-and-run position appropriates not only the Heuser school's data, but also its claims.

However, this intervolved status sets the context for the debate to shift to back to an independence-oriented mode; though De Camilli, Takei and their colleagues engage the kiss-and-run version of the Ceccarelli model, their work moves forward in a way that is largely separate from the kiss-and-run work. Contemporary kiss-and-run research also advances in a way which is largely independent of the clathrin studies of De Camilli and Takei. While each relates to the other's general model as a foil, they no longer significantly engage the current experimentation of the other (due, at least in part, to issues regarding discordant-data considered above).

Carey/Spelke Debate

The issue raised by the intervolved/independence-oriented pair of modalities is also relevant to the history of the debate between Carey and Spelke. As the debate begins, it possesses a mode of independence-orientation. Though in *Phase #1*, Carey and Spelke take their positions to challenge to the other, they each develop these positions separately, basing their positions on separately developed sets of data. As *Phase #1* continues, the two researchers begin to delineate their own positions in terms of the other's comments. During this phase, the debate modality becomes more and more intervolved. For example, through the process of debate each position becomes helpfully articulated in terms of its opposition to the other.

The Theory-of-Mind Debate

In general, the theory-of-mind debate tradition becomes more and more intervolved through its history. There are several aspects of this scientific debate tradition which indicate that its nodes become more and more intervolved over time.

Firstly, from the beginning of this debate, researchers take seriously the critiques offered by rivals. They develop defenses and counterarguments, or at times adjust their accounts. As the debate grows into an enduring dialogue, the tradition grows intervolved; the criticisms received are (at times) put to constructive ends, and criticisms leveled can be (at times) taken as potential contributions to others' efforts at developing their own positions.

Secondly, the intervolution of this tradition can be understood to increase as the theory-theory, simulation, and modularity positions graduate into obligatory debate node status. The establishment of obligatory debate nodes does not necessarily lead a debate tradition into an intervolved modality. However, in the case of the theory-of-mind debate, these nodes are so well-established that interlocutors tend to do more than acknowledge them; they define their own positions with reference to them. For example, if one does not claim a child must possess a 'theory' to understand others' minds, one may be obliged to explain why this is the case. If one's account includes reference to innate knowledge, one may be obliged to address whether or not this innate knowledge is modular.

Thirdly, this debate moves further into a mode of intervolution when ameliorating moves are introduced. Some researchers work to combine the positions of this debate, or to appreciate the contributions of all of the rival positions. Due to these efforts, one advancing a single position is now faced with the contention that one's own work offers only part of the answer to the question of child's theory of mind. One defending a 'pure' position must engage in 'maintenance' efforts to avoid being appropriated or relegated by the ameliorative moves. An account can certainly remain independent in this context, but it is pressured to prove that independence, rather than assume it.

2c. The Value of Debate Evolution for the Study of Science

I suggest that the notion of the scientific debate tradition and its associated concepts, and the notion of debate evolution in particular, are useful for the study of science. There are three ways this framework of concepts may be useful. (1) These concepts elucidate specific aspects of scientific debate. (2) Discussions in science can be compared and contrasted in terms of these specific aspects of scientific debate in potentially productive ways. (3) These concepts set the stage for further analysis of the changing relationships of scientific debates.

(1) The notion of the scientific debate tradition is abstract enough to apply to a variety of scientific debates. Yet it is specific and developed enough to identify and organize significant, though commonly-overlooked, aspects of these debates. In particular, the notion of the scientific debate tradition is useful for conceptualizing the ways that the relationships between the rival positions of a scientific debate have evolved over time. For example, above I have used this notion to locate and articulate specific changes that

have occurred over time in the modes of debate operating in the case studies of this dissertation. This was accomplished by first demonstrating the case studies to be examples of scientific debate traditions. Next, the debate evolution occurring in these histories were conceptualized in terms of shifts in relational modality.

The examples of debate evolution uncovered above can be summarized as follows:

The theory-of-mind debate tradition evolves in many ways through its history. Morphologically, the tradition is characterized by a continuous proliferation of primary positions. Among these various positions, three achieve an obligatory debate node status: theory-theory, simulation, and modularity. Influential branch quarrels are established within several of the primary nodes (see Figures 5.3 and 5.4). Importantly, the relationships between the positions of this debate change through its history. These changes occur regarding several issues. Firstly, the tradition remains primarily characterized by a mode of rigorous cross-analysis between rival positions, and by the continuous entrenchment and defense of these positions. However, there is also a secondary current of efforts to find middle-ground and overlap between rival positions. In response to this second current, some defend explicitly non-amalgamated versions of established positions. Secondly, a consistent convention of this debate is that the defenders of primary positions expect their theory to be able to account for all of the data regarding child's theory of mind. Yet, in practice, some positions specialize and focus more on particular sorts of data. Thirdly, the positions of this tradition generally grow more and more involved with one another. While each of the obligatory debate nodes of this tradition were originally created somewhat independently, over time there is an increase in efforts to confront one another, to defend against the others' critiques, and to address one another's successes.

The synaptic vesicle debate tradition also evolves in many ways through history. Morphologically, the debate begins between two positions: the Heuser model and the Ceccarelli model. These original positions are succeeded after two decades of productive dialogue by replacement versions: De Camilli and Takei's version of the Heuser model and the kiss-and-run position of the Ceccarelli school. The debate continues between these successors. The relationships between these positions also evolve over time. This occurs in several ways. Firstly, a productive mode of technical scrutiny and position differentiation slowly develops between the Heuser and Ceccarelli models. After these positions establish themselves as obligatory debate nodes, efforts to consider overlap also develop; examples appear in which rivals engage one another's positions in detail, or even adopt some of the other's claims. Over time, this consideration of overlap opens up new spaces of debate. For example, despite adopting some of the positions of the Heuser model, kiss-and-run research begins to question whether its proposed mechanisms are primary in particular contexts of neurotransmission not previously considered. Secondly, an important change occurs over time in the history of this debate regarding the relationship of each position to the set of data addressed by the other. In general, the Heuser and Ceccarelli models in the earlier moments of this history address the same general sort of data (i.e. first stained images, and later images of quick-frozen vesicle fusions). Each position later begins to develop different but related foci of investigation (e.g. the Heuser model's clathrin cages, and the Ceccarelli school's ectopic fusions). The same mode of debate continues; each side addresses the other's new results. Then an important shift occurs to the mode of this debate with regard to this issue: the current versions of the positions now focus on different sets of data (i.e. kiss-and-run studying different types of neurons, De Camilli and Takei studying clathrin and dynamin). These different sets of data are interpreted to be incompatible with one another. Thirdly, a fluctuation in the history of this tradition occurs in terms of how independently the major nodes operate. In the beginning, the Ceccarelli and Heuser models develop independently. Then, as the debate between them flares up, they become more and more involved with the other in terms of critical analysis and data sharing. Recently, however, as the sorts of data being analyzed have become more divergent, a mode of more independent research has again become dominant.

The example of the debate between Carey and Spelke can be used to illustrate the notion of debate evolution. Morphologically, the debate changes when the two established rival positions merge into a combined position. With this morphological change, several changes in the relationships between these positions also shift. Firstly, during the time while these positions were separate, they develop a mode of intense technical cross-analysis. Through the debate process over time, Carey and Spelke determine that their positions need not be interpreted as inconsistent. Recognizing this, they move to a mode of considering their positions' potential overlap. After the merger of their positions, there is a return to the mode of technical cross-analysis as they challenge the work of Alison Gopnik. Secondly, in the shift from separate to combined positions, there is a shift from an effort to pit two separate sets of data against one another to an effort to interpret both sets of data together at once. Thirdly, these two positions become more and more involved with one another over time. Spelke's position originally emerges in a way that addresses Carey's existing position. In response, Carey begins to define her own position against Spelke's claims. When they reinterpret their positions to be compatible with one another, each position's dependence on the other deepens.

(2) Conceptualizing ongoing debates in science in terms of the notion of the scientific debate tradition may enable productive comparison and contrasts.

Applying this framework of concepts to an ongoing scientific debate highlights certain parts. In particular, the changing relationships between the nodes of the debate are called to light. Applying these concepts to more than one debate facilitates their comparison in terms of the specific features this framework discloses—changing modes of debate. One may recognize new things about one's own work by comparing it with similar histories of debate. This comparison may inspire one to consider new possibilities for the mode of one's own debate.

It may also be use useful to those participating in ongoing scientific debates to be aware of the conceptual relationships between different modes of rivalry. For example, the relational modalities explored above are arranged into 'pairs.' When a debate takes up one of these modes, the potential may be created for the sibling mode to be shifted to. It may be helpful for those participating in ongoing debates to be aware of their debate's contemporary mode, and also aware of the sibling mode to which their debate has the potential to shift.

Scientists surely already make these sorts of considerations, though in a less explicitly formulated way. The notion of the scientific debate tradition may enable scientists to consider these issues in a way that is more systematic and more historically-informed.

(3) Above I have identified and articulated three pairs of 'relational modalities.' This sets the stage for further research into the changing modes of debate in science. This list of six relational modalities is not exhaustive. I am confident that if further case studies of scientific debate traditions are conducted, further kinds of relational modalities will be revealed. The six which have been studied above provide exemplars for the kinds of relational modalities that may be discovered. For example, the fact that the six I identify are grouped into pairs implies that this 'pairing' pattern may continue for further relational modalities that are found. In any case, the notion of the scientific debate tradition provides the context for this empirical work to continue; the search for further kinds of relational modalities requires applying the notion of the scientific debate tradition to further concrete examples of debate in science. The six relational modalities identified above provide model examples for the kinds of further modes of debate that may be identified.

3. The Implications of Debate Traditions for the Philosophy of Science

A final task for this dissertation is to determine how the notion of the scientific debate tradition relates to other work in the philosophy of science. Below, I contextualize my claims within the history of this field, and contrast them with relevant work.

I have developed the notion of the scientific debate tradition as an analytic tool for conceptualizing the structure and evolution of debates in science. This notion includes a framework of concepts (e.g. debate nodes, problem hub, branch quarrels, debate modalities). When this framework is applied to a concrete history of scientific debate, specific features of that history are identified and organized. These concepts highlight the relationships between the rival positions of a scientific debate. In particular, this framework is useful for analyzing the history of changes that have occurred to the relationships between rival positions over time. In this view, the changing relationships between these positions can be helpfully conceptualized in terms of shifting modalities of debate.

Ultimately, the value of this conceptual framework will be determined by its usefulness for analyzing the histories of debates in science, both closed and ongoing. Above, I have applied the notion of the scientific debate tradition to two concrete case studies of ongoing scientific debates. The application of this framework has helped to identify and articulate several ways that these debates have evolved through their histories. These studies provide examples for the kind of work that can be done with these conceptual tools.

Aspects of this depiction of science run counter to most depictions of science cast by philosophers. Some parts of the notion of the scientific debate tradition extend beyond what particular philosophers claim. Other parts develop challenges for philosophers studying specific issues.[4] In what follows, I contrast the notion of the scientific debate tradition with three categories of work done in the philosophy of science. The first subsection below addresses the logical empiricist perspective. The second subsection addresses views on scientific progress, such as those of Lakatos, Kuhn, and Laudan. The third and final section addresses philosophical views on research collectives, such as the perspectives of Ihde and Galison.

3a. For Logical Empiricism, a shift in focus

Though now widely considered defunct, the logical empiricist program dominated the philosophy of science for several decades of the early twentieth century, and its history continues to set the context for contemporary investigations in this field. These philosophers attempted to conceptualize science as using formal logic to precisely articulate of the relationships between empirical observations and theory. In this view, it is possible for basic, purely-observational statements to be made without the context of an overarching theory. Ideally, scientific theories are simply the expression of generalizations regarding these observation statements (e.g. Mach, 1886; Carnap, 1928; Carnap, Hahn, and Neurath, 1929; Ayer, 1936; Reichenbach, 1938; Carnap, 1939; Hempel and Oppenheim, 1948; Hempel, 1962; Hempel, 1965). The logical empiricist program has gathered a number of debilitating criticisms since the days of its reign as the dominant view in the philosophy of science (e.g. Quine, 1951; Goodman, 1955; Putnam, 1962; Achinstein, 1965). Some of these problems are addressed by the 'probabilist' view of science. Emerging from the logical empiricist tradition, this view replaces the strictly logical relations between theory and observation statements with statistical relations (e.g. Carnap, 1950; Hempel, 1965; Hesse, 1974; Franklin, 1990; Earman, 1992).

The notion of the scientific debate tradition goes beyond the logical empiricist tradition by concentrating on the changes that occur to theories through the course of their debates. This focus emphasizes aspects of scientific work that are missed by the logical empiricist account.

Logical empiricists focus on the relationships between a set of data and an individual account of those data. In contrast, the notion of the scientific debate tradition introduces a whole-debate perspective. This perspective widens the focus to include the relationships between rival positions. Versions of logical empiricism and probabilism do understand rivalries to occur in science. These rivalries are described in terms of comparisons of the fit between the data and the competing theories. The notion of the scientific debate tradition instead emphasizes the modes of these rivalries, the way these modes change over time, and the effects that changing rivalries may have on the constituent positions. This view highlights the way that participation in a debate may have important effects on the rival positions, a point missed by logical empiricism.

To illustrate this contrast, consider the history of the Ceccarelli model of the synaptic vesicle debate explored in chapter three. An influential version of the logical empiricist view is Carl G. Hempel's Deductive-Nomological model (e.g. Hempel and Oppenheim, 1948; Hempel, 1962; Hempel, 1965). This model of scientific explanation can be applied Ceccarelli's model of synaptic vesicle exocytosis. In Hempel's view, explanation occurs in science when an object of study can be deductively subsumed by an account which includes a series of scientific laws and also the specific conditions which relate to the object. Hempel's view is 'nomological' since it takes reference to scientific laws to be an essential part of explanation in science. It is 'deductive' since it takes explanation to occur when the thing to be explained can be deduced from a set of relevant laws and particular conditions.[5] The Ceccarelli model can be cast in these terms; a particular exocytotic event is explained by the particular conditions necessary for that event and the laws of the Ceccarelli account. These laws include, for example, the Ceccarelli school's claim that exocytosis and endocytosis are a continuing process which occurs in the same moment and location. Hempel's account suggests the Ceccarelli school should first deduce implications of their model, and then devise empirical tests of those implications. Failures to meet those empirical tests should offer challenge to the Ceccarelli model. It is possible to coherently read much of the work of the Ceccarelli school in Hempel's terms.

However, Hempel's view does not account for all of the ways that the Ceccarelli model develops over time. Some work of the Ceccarelli school is a response to its rival, the Heuser school. The notion of the scientific debate tradition highlights these aspects of this history, aspects missed by Hempel's account. The development of the Ceccarelli school's view comes into better focus when it is considered in terms of its place in an evolving scientific debate tradition. For example, the Ceccarelli school's quick-freezing studies are in part designed to counter the Heuser school's development and use of this technique in support of their own model (Torri-Tarelli et al., 1985). The Ceccarelli school's influential studies of ectopic fusions are developed in part as a response to the Heuser school's evidence of late-forming endocytotic dimples (e.g. Ceccarelli et al., 1988). Importantly, the Ceccarelli model experiences a major shift in the mid-1990's when it becomes the kiss-and-run model (with its fusion pore account). This shift includes several responses to the Heuser school. One crucial change is the kiss-and-run theorists' claim that the Heuser pathway and the kiss-and-run pathway may both be operative during neurotransmission. This change to the Ceccarelli model is centrally addressed by the notion of the scientific debate tradition.

Each of these features of this history become clear only when the Ceccarelli model is analyzed in terms of its place within an evolving scientific debate tradition. These features of this history are obscured by any account of science that does not emphasize these factors.

3b. For Views of Scientific Progress, a shift in context

The issue of scientific progress vexes philosophers. For the logical empiricists, progress occurs as a new better theory subsumes the successes of the previous ones.[6] One effect of the collapse of logical empiricism has been the loss of this intuitive account of scientific progress. The most influential remaining views claim that scientific progress occurs with respect to particular measures. For Kuhn, during periods of 'normal science,' progress occurs as scientists have success addressing the 'puzzles' set out by the paradigm.[7] Lakatos conceives of progress in terms of the novel predictions made by research programmes, and the novel confirmations of those predictions. But according to Lakatos, progress occurs only as a programme advances with respect to rivals. More recent accounts take a somewhat pragmatic perspective, arguing that progress should be conceived in terms of our needs and goals (e.g. Laudan, 1977; Kitcher, 1993; Laudan, 1996). The claims of this dissertation create a novel and specific context for questions regarding scientific progress.

Above, I have developed a framework for charting the changes that occur to the relationships between the positions of a scientific debate. In particular, this framework accounts for these changing relationships between rivals in terms of shifting modalities of debate. I claim that this framework of rival nodes and shifting modalities represents terrain in which the question of scientific progress can be profitably explored. The explorations of this dissertation reveal issues of scientific progress not yet addressed by philosophers of science.

I have been careful thus far to note that a shift in a debate's relational modality or morphological modality does not necessarily imply that progress is occurring. For example, an ongoing debate tradition which does not experience changes to its morphology may progress, or may not. The debate tradition's static morphological structure may be very productive. On the other hand, the discussion may be trapped within a failing morphological scheme with no contributors finding recent success. The same possibilities exist for a debate tradition with a history of shifts in its morphology. The changes to the debate nodes may reflect a progressing debate with creatively shifting positions. Or it may reflect a collection of stagnating rival positions, each looking desperately for a source of reinvigoration.

The same is true for shifts in relational modality; shifts in relationality may reflect a debate with strongly progressing elements, or may reflect stagnating discussions that change without moving forward. A debate which continues in terms of a particular relational modality may have found a scientifically progressive mode of debate. Or the debate may be stuck in a mode which facilitates much chatter, yet is not progressing forward in terms of any of the accounts of scientific progress developed by philosophers.

A debate may shift from one modality to the sibling modality of its pair, or may oscillate back and forth between them. A debate tradition which oscillates between a pair of modalities may be growing and changing in productive ways. Or the mode of the debate may be changing without any productive results in terms of scientific progress.

For example, consider a scientific debate tradition with a modality of productive controversy. The nodes of a scientific debate in this mode engage in rigorous cross-analysis, and further define, defend, and entrench their positions. The word 'productive' in 'productive controversy' refers to the high level critical interaction occurring within a debate that possesses this modality. Yet this 'critical interaction' may or may not occur in a way that generates what philosophers of science call scientific progress. The criticism that is leveled and received may lead these positions to progress in substantive ways, or the criticism itself may constitute substantive progress. Or these activities of micro-critique, position refinement, and argument and counterargument may serve as a crutch; they may provide debate participants with work to do despite the fact that no scientific progress is occurring.

I claim that the notion of the scientific debate tradition raises a specific challenge for those who work on the question of scientific progress: how to address the scientific progress (and lack of progress) that occurs through debate evolution. A comprehensive theory of scientific progress must be able to address this type of change in scientific practice. Specific normative questions are raised for philosophical accounts of scientific progress. When do changes in a scientific debate's morphological or relational modality constitute moments of progress? Is it possible to recognize when morphological or relational shifts constitute a lack of progress? When are maintenance efforts justified?

Addressing this issue is not a simple matter. It requires an account of scientific progress be able to attend to the structures and dynamics of scientific debate I have articulated above. Lakatos's view of scientific progress is an instructive example. My account of scientific debate traditions shares much with his account of scientific research programmes. But my account goes beyond Lakatos's in particular ways.

Lakatos contends that scientific progress occurs in terms of the advances made by scientific research programmes. A scientific research programme includes a collection of theories and the heuristics for investigating these theories.[8] According to Lakatos, a research programme has a 'hard core' of main theories, and also a 'productive belt' of auxiliary theories. In this view, the unit which should be assessed with regard to the issue of scientific progress is the research programme as a whole. A progressing programme is one that continues to predict new facts, find confirmations to these predictions, or both. In contrast, a degenerating programme only develops ad hoc adjustments to its protective belt in response to the successes of other programmes. It is important to note that for Lakatos, scientific progress occurs in terms of the rivalry between programmes; a programme progresses or degenerates only with respect to the progression of a rival programme.

I claim that Lakatos's view of scientific progress must be adjusted in order to be applied to the issues revealed by the notion of the scientific debate tradition. Lakatos's research programmes share a similarity to my own concept of debate nodes; a scientific debate tradition is in some ways like the rivalry between research programmes.[9] However, my account goes beyond Lakatos's by articulating the relationships that exist between rival positions in a scientific debate.[10] For Lakatos, rival programmes relate to one another with regard to their relative progression. The notion of the scientific debate tradition reveals that the relationships of a scientific debate are much deeper.

For Lakatos's account of scientific progress to apply to the issues raised by my account of debate evolution, it must be adjusted to be able to recognize the relationships between rival debate positions. His view of rivalry fails to account for the particular character of a rivalry may take at a given time. His view may be able to account for scientific progress which occurs as a new prediction or finding is made. But it does not have the tools to account for progress which occurs in the form of change to the mode of debate. And it cannot account for degeneration that occurs despite changes to a debate's mode.

3c. For Views of Research Collectives, a sibling concept

Some research in the philosophy of science investigates the structures of groups of researchers and their relationships with instrumentation. Philosophers studying this topic develop concepts to articulate the ways collectives of researchers work within specific conventions and histories, and they consider the philosophical implications. Peter Galison's notion of the 'experimental tradition' and Don Ihde's notion of the 'instrumental trajectory' are model examples of this sort of philosophical research. I take the notion of the scientific debate tradition to be an addition to this assortment of concepts regarding collectives of scientists and instruments.

The notion of the scientific debate tradition does not provide an exhaustive account of the kinds of relations that can occur between those doing scientific research.

It applies only to groups of scientists engaging in debate. Other notions need to be developed, and have been developed, to conceptualize the various collectives of researchers that occur. This raises an issue: what is the relationship between scientific debate traditions and other accounts of research collectives in science?

I claim that the notion of the scientific debate tradition is compatible with both Ihde's notion of the instrumental trajectory and Galison's notion of the experimental tradition. These particular structures of group dynamics and instrumental relations do more than exist along side one another; they overlap in particular ways.

With the notion of the 'instrumental trajectory,' Ihde refers to the ways that the technologies and techniques of scientific practice play a role in determining the course of inquiry. The particular capacities a laboratory technology enables, and the particular possibilities for its further development, are part of what guided the directions that scientific research travels. The material aspects of scientific practice suggest their own vectors of advancement. For example, a magnifying technology such as a microscope suggests that research should proceed through the development of better and better magnification. The notion of the scientific debate tradition relates to these vectors of developing research in specific ways.

It is possible for an entire debate tradition to develop within the bounds of a particular instrumental trajectory. A large-scale movement could develop in the effort to refine a specific research technology for the sake of investigating a certain topic. A tradition of scientific debate could emerge within this movement regarding how these developments should occur. The problem hub would regard the issue of how the technology should be developed. The rival nodes could regard different strategies for how this development should occur. Shifts in the mode of debate could occur throughout the duration of the history of this research.[11] In contrast, it is also possible for an instrumental trajectory to occur within the bounds of a scientific debate tradition. A debate tradition could occur which includes a node that focuses on a specific type of data. Research within this node could be largely influenced by an instrumental trajectory that relates to its favored type of data.

Galison uses the term 'experimental tradition' to refer to a collective of scientists involved in a history of research with specific instruments. An experimental tradition is determined by the researchers' practices regarding training procedures, instrumentation, and strategies for proving their scientific claims. According to Galison, experimental traditions can grow so specialized and instrumentally-embedded that two traditions working on a related topic can become entirely separate. These traditions can develop so independently that they are only able to approach one another's work in a way akin to cross-cultural interaction. Galison develops the notion of the 'trading zone' to refer to the kinds of limited interactions that can take place between traditions. I suggest that the notions of the experimental tradition and the debate tradition can overlap in specific ways.

A tradition of debate could develop within a tradition of experimentation. Since experimental traditions have the potential to be quite large in scale, it is possible for several separate debate traditions to occur within one. The development of a trading zone with another experimental tradition can have effects on one of these debate-traditions-within-an-experimental-tradition. The potentially novel sorts of discoveries instigated by work in the trading zone could lead to a new debate node. Or it could influence the modality of the debate by introducing new kinds of data or techniques.

These relationships between these three concepts regarding research collectives—instrumental trajectories, experimental traditions, and scientific debate traditions—exemplify the sorts that may exist between any accounts of this kind. This poses a challenge for anyone developing an account of the way that groups of scientists interact: to determine the relationship between their own account and the account of scientific debate developed above.

Notes for Chapter 5

1. I use the term 'modality' instead of 'mode' to avoid the rhyming sound and visual similarity between the word 'mode' and the word 'node.'

2. This is the difference between a debate tradition with an independence-oriented mode and 'non-debate.' Independence-oriented debaters do not ignore one another. They argue down or dismiss one another. If they simply ignored one another all together, they may not belong to a debate tradition together at all.

It is possible for a debate tradition to form even if the major nodes of the debate refuse to acknowledge the existence of one other. The audience of the debate could still interpret the positions to be in conflict. Others working on these issues (such an experimentalist with no commitment to either position, or someone summarizing work on the topic) could write about the conflict between these positions.

3. Of the six relational modalities I identify, only two non-pair modalities cannot occur in a debate tradition together, at least not in a simple way. These are independence-orientation and amelioration. Though from different pairs, a debate in one of these modalities appears situated against the debate simultaneously taking up the other mode. A debate in which the participants do not take their rivals' work to be of value cannot also be a debate in which the compatibility of rival positions is explored.

However, since debate traditions can be complex, it is possible for parts of a tradition to be in one of these modalities, and parts to be in the other. In this way these two modalities are similar to those of a 'pair.' But unlike modalities of a pair, the instantiation of one of these modalities does not set the context for the debate to shift to the other.

4. Those working in the philosophy of science attend to a number of different tasks. Consider these three. (1) Some attempt to account for the logic of science without regard for whether actual practice conforms to this ideal. (2) Some attempt to describe actual practice of science, developing tools for conceptualizing the structures or reasoning of scientific work. (3) Others develop normative arguments (based on ideal or actual descriptions of science), describing how science ought to be practiced and evaluated. This dissertation fits into the second category since it does not make abstract normative recommendations. However, the claims of this dissertation do have implications for

those projects that do have an explicit normative component. The subsection below on theories of progress contains some of these implications.

5. This simple review, of course, fails to do justice to Hempel's well-developed account. Also, Hempel's project is importantly different from mine in that he develops and 'ideal' account of science. His goal is to create a simple model of the logic of science, independent of whether this model captures actual practice. He says, "The term 'model' can serve as a useful reminder that the two types of explanation as characterized above [Deductive-Nomological and Inductive-Statistical] are not intended to reflect the manner in which working scientists actually formulate their explanatory accounts. Rather, they are meant to provide explications, or rational reconstructions, or theoretical models, or certain modes of scientific explanation" (Hempel, 1962, 15). Also, I do not intend to address the criticisms his account has received from others.

The point here is to contrast the general claims of logical empiricism with the notion of the scientific debate tradition to demonstrate the particular virtues of the latter account.

6. The central example of this view in the logical empiricist tradition is Ernest Nagel's account of 'intertheoretic reduction' (e.g. 1961).

7. Of course Kuhn's view of 'scientific revolutions' is controversial in terms of the issue of scientific progress. He claims that two paradigms are incommensurable with one another. Typical scientific argumentation cannot guide one to chose between an old

paradigm and its potential replacement. According to Kuhn, paradigms are incommensurable in a number of ways. The standards of proof shift during a scientific revolution. Also, new concepts are identified by the paradigm, and old concepts gain new meanings and relationships. Regarding scientists debating over paradigm change, he says, "Though each may hope to convert the other to his way of seeing his science and its problems, neither may hope to prove his case" (1970, 148). Instead, persuasive arguments are used.

Kuhn's position is often taken to imply that ultimately progress in science has an irrational character, if progress can be understood to occur at all. Kuhn's own comments on the issue are a bit different (they more resemble a Lakatos-type position of progress as growth in knowledge. See Kuhn, 1970, 171). But critics contend that, regardless of Kuhn's own stray remarks, his account leads to a view of scientific progress as irrational or non-existent.

Kuhn's claims regarding paradigm change regard a different level of scientific practice than that addressed by this dissertation. I address the level of work in which everyday scientists engage in substantive analysis of one another's positions. If scientific revolutions do occur in the way Kuhn indicates, the sort of advance they represent does not relate to my investigation.

8. Lakatos's philosophy is reviewed in detail in chapter one, subsection *1b*. The primary work in which his views are advanced is (Lakatos, 1970).

9. Lakatos's account of research programmes of course is not completely consistent with my own account of debate nodes.

Some aspects of Lakatos's account do not inherently contradict claims I have made. For example, his notions of 'hard cores,' 'auxiliary theories,' 'creative shifts,' and 'heuristics' are compatible with the account I have provided. Also, his claims regarding the progression and degeneration of programmes, and a programme's relationship to anomalies (which he shares with Kuhn), are not challenged by my work.

However, insofar as there is a correspondence between his notion of research programmes and my notion of debate nodes, some of his claims are challenged by my account. For example, in my account the most hard core claims of a debate node are subject to change over time. Also, in my account a debate node may contain contradictory claims. These contradictory claims may occur in the form of a branch quarrel.

These points of compatibility and incompatibility are not immediately relevant to my claim that if Lakatos's view of progress is to be applied to the case of debate evolution, his account must be adjusted in the ways I address.

10. In chapter one, I referred to this as 'the debate critique of Lakatos.'

11. Certain features of both case studies of this dissertation can be read to proceed in just this way. For example, the synaptic vesicle debate tradition from the late 70's to the early 90's largely regarded the development of the quick-freezing and other related

imaging processes. This debate tradition during these years could be read, at least in part, as occurring within a large-scale instrumental trajectory.

Glossary of original terms

Amelioration: with ‘productive controversy,’ one of a pair of relational modalities. A debate tradition with an ameliorative modality is one in which rivals consider the potential overlap or compatibility of their positions. See chapter 5, subsection *2b*.

Branch Quarrels: a debate which occurs within a primary node of a scientific debate tradition. See chapter 2, subsection *3c*.

Co-Analysis: the interrelation between nodes regarding the ways that a rivalry between positions is a group effort at addressing issues. For example, one may receive constructive criticism from rivals. See chapter 2, subsection *2b*.

Co-Composition: the interrelation between nodes which regarding the way positions receive some of their identity and content through participation in the debate. See chapter 2, subsection *2b*.

Co-Validation: the interrelation between nodes regarding the way that rival each recognize the other as a valuable point of a discussion. See chapter 2, subsection *2b*.

The Debate Critique of Lakatos: the contention that Lakatos’s account fails to address the deep and changing relationships between rival positions. See chapter 1, subsection *1c*.

Debate Establishment: contrasted with ‘debate maintenance,’ refers to the work of creating a debate. This work includes engaging rivals, and gaining the attention of an audience. See chapter 2, subsection *1c*.

Debate Evolution: the changes which occur to scientific debate traditions, such as to number of nodes (through new nodes arriving, splitting, combining, etc.), or to the sorts of relationships which exist between the nodes. See chapter 2, subsection *3b*. In chapter 5 section 2, debate evolution becomes more precisely defined as changes in ‘morphological’ and ‘relational modality.’

The Debate Evolution Thesis: the claim that scientific debates do not always (or necessarily often) end with one position, in its original version, defeating all others. See chapter 2, subsection *1a*.

Debate Maintenance: contrasted with ‘debate establishment,’ refers to the work of keeping a debate coherent, active, and popular. This work includes adjusting one’s position to account for the progress of rivals, and creating further venues for the debate to continue. See chapter 2, subsection *1c*.

Debate Tradition: see ‘Scientific Debate Tradition’

Debate Modality: refers to the particular set of nodes of a debate tradition, and the mode of the relationships between them, at a give time. Different types of debate tracks can be identified. Two general axes of a debate modality are its 'morphological modality' and its 'relational modality.' Six different relational modalities are identified in chapter 5, section 2: 'amelioration,' 'discordant-data,' 'identical-data,' 'independence-oriented,' 'intervolved,' and 'productive controversy.'

Discordant-Data: with 'identical-data,' one of a pair of relational modalities. A debate tradition with a modality of discordant-data is one in which rivals rely on separate sets of data which they interpret to be inconsistent with one another. See chapter 5, subsection *2b*.

Identical-Data: with 'discordant-data,' one of a pair of relational modalities. A debate tradition with a modality of identical-data is one in which rivals each address the same set of data. See chapter 5, subsection *2b*.

Independence-Oriented: with 'intervolved,' one of a pair of relational modalities. A debate tradition with an independence-oriented modality is one in which positions regard themselves to develop independently from rivals. See chapter 5, subsection *2b*.

Intervolved: with 'independence-oriented,' one of a pair of relational modalities. A debate tradition with an intervolved modality is one in which rival nodes regard one other as bringing something useful to debate. See chapter 5, subsection *2b*.

Jumping Tiers: when the content of a branch quarrel has effects on the primary debate of the tradition. See chapter 2, subsection *3c*.

Morphological Modality: one of the two axes of debate modalities. A morphological modality—or a debate's morphology—refers to the number and configuration of a debate tradition's nodes at a given time. See chapter 5, subsection *2a*.

Obligatory Debate Node: a debate node that becomes established such that others working within the debate tradition are obligated to engage it. See chapter 2, subsection *1c*.

Primary Debate Nodes: a node of a scientific debate tradition which relates to the tradition's central issues (or 'problem hub'). See chapter 2, subsection *3c*.

Problem Hub: the central questions or issues that define and organize a scientific debate tradition. The 'primary debate nodes' address the problem hub. See Chapter 2, subsection *1b*.

Productive Controversy: with 'amelioration,' one of a pair of relational modalities. A debate tradition with a modality of productive controversy is one in which rival nodes engage in intense cross-analysis, defense, and the further refinement and entrenching of their views. See chapter 5, subsection *2b*.

Relational Modality: one of the two axes of debate modalities. A relational modality—or a debate's relationality—refers to the particular mode of debate which exists between the nodes of a debate tradition at a given time. Six different relational modalities are identified in chapter 5, section 2: 'amelioration,' 'discordant-data,' 'identical-data,' 'independence-oriented,' 'intervolved,' and 'productive controversy.'

Scientific Debate Tradition: refers to all of the positions of a scientific debate, and the relationships between those positions, as they evolve over time. The term also refers to the framework of concepts developed in this dissertation. See chapter 2, section 1.

The Synaptic Vesicle Debate: an ongoing debate in the field of neurobiology over the nature of synaptic vesicles, tiny organelles responsible for neurotransmission. Chapter 3 consists of an original case study into the history of this debate. In chapter 5 subsection *1a*, this debate is shown to be an example of a scientific debate tradition. In subsection *2b* of that chapter, the relationships between the rival positions of this debate at different points of its history are explored.

The Theory-of-Mind Debate: an ongoing debate in the field of developmental psychology over the nature of children's developing ability to understand the separate perspectives of others. Chapter 4 consists of an original case study into the history of this debate. In chapter 5 subsection *1b*, this debate is shown to be an example of a scientific debate tradition. In subsection *2b* of that chapter, the relationships between the rival positions of this debate at different points of its history are explored.

Whole-Debate Perspective: a focus that includes the rival positions of a scientific debate, and their developing effects on one another. See chapter 1, subsection *1d*, and chapter 2, section 1.

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