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Michael L. Keene, Major Professor

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THOMAS KUHN AND THE EVOLUTION OF COMPOSITION STUDIES: ISSUES IN INTERDISCIPLINARITY

A Dissertation

Presented for the

Doctor of Philosophy

Degree

The University of Tennessee, Knoxville

Carl Joseph Arseneault, CPT, AUSR

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DEDICATION

This dissertation is dedicated to my wife

Gabriele Maria Arseneault

who believed that I could accomplish all that I set out to do and who cheerfully put up with a dining room table full of computer equipment, books, journals, notes, and assorted paraphernalia while patiently listening to my ideas and encouraging me to not give up.

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ABSTRACT

As composition studies has developed, it has shown a remarkable propensity to borrow and adapt ideas from other disciplines, inevitably transforming the ideas in the crucible of practice. While the use of a borrowing may depend initially upon the state and form of the idea in its own discipline at the time it comes to the attention of composition, borrowed ideas then often diverge or develop independently as we reshape them to meet the complexities of composition studies. This dissertation traces the importation and application of Thomas Kuhn's theory of paradigm shifts as an example of borrowing: first, examining Kuhn's theory as it originally developed and gained acceptance within a discipline outside of composition studies; second, tracing the process of borrowing; and third, describing the nature of Kuhn's appearance in composition studies. The current state of Kuhn's theory as we now use it in composition studies is compared with its present position within Kuhn's disciplines of the history and philosophy of science. For some in composition, Kuhn's ideas about the revolutionary nature of science and its paradigmatic structure have simply become a metaphor for describing the various shifts in emphasis within the profession; for others, his ideas accurately describe what goes on in composition studies. The intense look at Kuhn's importation provides an excellent view of the interdisciplinary nature and status of composition studies as scholarly inquiry.

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CHAPTER ONE

INTRODUCTION TO THE PROBLEM AND METHODOLOGY

> Inevitably, any emerging field must find ways of defining itself apart from others or showing how it shares similarities and convergences with others; this is particularly true of an interdisciplinary or cross-disciplinary field such as composition studies.[1]
>
> --Lillian Bridwell-Bowles, 1991

Field? Science? Subdiscipline? Emerging, hybrid, or established discipline? Metadiscipline? Interdiscipline? What is composition studies? In his 1987 The Making of Knowledge in Composition: Portrait of an Emerging Field, Stephen North divides the "field" of composition studies along the lines of what he sees as various communities of knowledge-makers, ranging from practitioners to subdivisions of scholars and researchers. When Louise Wetherbee Phelps, in her 1988 Composition as a Human Science: Contributions to the Self-Understanding of a Discipline, charts an "ecological" view of composition as a "self-organizing system . . . dynamically capable . . . of innovation and unpredictable evolution" (x), she claims the status of both "science" and "discipline" for composition studies. Richard Fulkerson, in his 1990 "Composition Theory in the Eighties: Axiological Consensus and Paradigmatic Diversity," argues that as a "discipline," composition studies "has moved toward homogeneity of purpose within diversity of method" ("Axiological" 410). Gary A. Olson and Irene Gale use "discipline" interchangeably with "field" in (Inter)views: Cross-Disciplinary Perspectives on Rhetoric and Literacy, their 1991 collection of interviews "with major scholars from outside the discipline of rhetoric and composition whose work has had influence on the

field" (ix). Patricia Bizzell echoes North's idea of an "emerging field of composition studies" (Bizzell 9) in the introduction to her 1992 collection, Academic Discourse and Critical Consciousness. In his foreword to Olson's 1994 Philosophy, Rhetoric, Literary Criticism: (Inter)views, Clifford Geertz mentions the "cross-disciplinary" nature of studies in composition ([xi]).

The debate over the status of composition studies as a discipline has its roots in efforts over the past thirty plus years to identify and legitimize a specific province of inquiry into writing and writing pedagogy. These efforts involve claims for greater prestige within the academy as well as validation of composition research as scholarly inquiry. Simultaneously, such efforts reflect the struggle of composition studies to emerge from the political reality of its subordination as a "service" provided by English departments.

Why do the efforts of North, Phelps, Olson and others often seem to fall shy of identifying the boundaries that mark composition studies as a unique field of human inquiry? The answer may lie in the propensity of practitioners, researchers, and theorists in composition studies to lift ideas from other, more established (or more "legitimate") fields of inquiry. The pages of the professional journals that composition studies can perhaps claim as its own--for example, College Composition and Communication, College English, The Journal of Advanced Composition, and The Journal of Research in the Teaching of English--teem with ideas borrowed or "stolen" from other disciplines to meet the perceived needs of a new profession. To increase the prestige of inquiry into writing, many compositionists have been quick to borrow and apply successful theories and methodologies from other disciplines, thereby at least tacitly arguing that the validity of the theories and methodologies in a field outside composition can be transferred intact or with little adjustment to investigating writing process, practice, theory, and pedagogy. Thus, when Lee Odell asserts in his 1983

forward to Stephen Witte and Lester Faigley's Evaluating College Writing Programs, "to establish beyond question the authenticity of composition as a discipline, we must come to some agreement about theory, methodology, and the kinds of questions worth answering" (x), he fails to account for the propensity of the community to look elsewhere, as well as within, for validation.

The Nature of Interdisciplinarity

> [E]very discipline is unique. One must create it, nurse it, and bring it to maturity on its own terms. The melting down of disciplines to make a single new discipline . . . is often a useful endeavor. However, the new discipline must create its own validity and its own laws. . . .[2]
>
> --A. M. Tibbetts, 1963

As the study of composition has developed over the past two centuries, it has shown a remarkable propensity to borrow and adapt ideas from other disciplines, inevitably reshaping or transforming the ideas while testing them in the crucible of practice. When we freely borrow Thomas Kuhn's theories of paradigmatic structure and scientific revolution from the history or philosophy of science and apply them to explain or examine what we do in composition studies, when we appropriate research methodologies such as ethnography from the social sciences to shine light on questions about the composing process, or when we adopt a pedagogical approach from education such as collaborative learning and develop it with a focus on teaching composition, we demonstrate the interdisciplinary, metadisciplinary, and cross-disciplinary nature of composition studies.

In her 1990 Interdisciplinarity: History, Theory, and Practice, Julie Klein explores the nature and consequences of borrowing across disciplines. Klein notes that most knowledge-making communities discriminate one field from another by staking out and defending individual "turfs"--coming to some agreement among members of the community as to the "proper" province of a field. She examines interdisciplinary interaction as it affects the geopolitical structure of discourse, problem solving, and the stability and consistency of subjects and methods. Klein goes on to identify a number of reasons why borrowing occurs across fields: "The more successful a theory is in its ability to describe, explain, predict, and systematize, the more tempting it is to extend its validity outside its original boundaries. As a result, the most successful theories tend to become 'obsessive paradigms' for other fields of knowledge" (Klein 86).

Writing more than a decade before Klein, Richard Lloyd-Jones argues in a condensed version of his keynote address to the 1977 Conference on College Composition and Communication (CCCC) that composition has to borrow:

> In order to know more, we will have to draw into our work the scholarship of many disciplines; we will have to recognize ourselves in the interdisciplinary scholars--with all of the attendant embarrassments that come from trying to speak in fields far from one's own base. But if we do not try to be in the center of all knowledge, to report the view from the center of how disciplines interact, we deserve our present *basic* position, that is, our traditional place in the damp cellar of the house of intellect. (Lloyd-Jones, "Center" 28)

Lloyd-Jones's remarks carry particular weight in the composition community because of his position as the chair of CCCC at the time of his address. He claims a central place for teaching and investigating writing that synthesizes knowledge and unites people.

Simultaneously, he asserts that "we draw from the wisdom of other disciplines and in making it ours, transform it by combining it in new ways" (Lloyd-Jones, "Center" 27).

Treating borrowings and field development from Klein's interdisciplinary stand may provide some new insights into the constitution of composition studies as valid academic inquiry. Doing so may also require a better understanding of the distinctions we make between what constitutes a "field," a "discipline," or a "science." Too often, proponents of composition studies or other areas of academic inquiry have used these terms interchangeably, claiming the status of "discipline" or "science" as if such a characterization as one or the other makes some form of academic inquiry more valid. These distinctions demand a set of hierarchical relationships establishing "science" as the acme of all academic knowledge. For my purposes, I define the "field" of composition studies as a broad, shared interest in knowledge about writing theory, practice, and pedagogy. Scholars working in the "field" of composition studies do so within the framework of commitments to the more established disciplines in which they were trained; for example, many of those working in composition initially trained for careers as literary scholars, not as compositionists. This is changing as composition studies matures, gaining adherents who have trained formally in rhetoric and composition.

By "discipline," I mean an academically accepted, already established branch of knowledge or learning, one that has acquired enough prestige within the academy to garner a share of its limited resources and a stake in the making of knowledge. The struggle to establish composition studies as valid inquiry has also been a struggle to share the limited resources allocated to the study of a discipline of English, which many have viewed as primarily the study of literature rather than writing. To a great extent, I believe that the struggle is succeeding. For example, we can see that success in the growing number of graduate programs focusing on the study of composition theory, practice, and pedagogy.

Finally, to me, a "science" implies even a greater commitment to a particular methodology, a systematized way of establishing knowledge through empirical study, observation, and experimentation. The academy has traditionally viewed knowledge-making in the sciences as more valid because science aims at systematically determining the fundamental nature of the subject it studies--usually the natural or physical world. Many attempts at validating composition studies have included emphasis on empirical study and the use of the scientific method at the expense of the complex variables affecting human nature. If the study of the way humans compose and teach composing is a "science," it is a "soft" science, one deeply affected by the individual and social complexity of human nature.

Any examination of interdisciplinary borrowing runs afoul of the distinctions we make between "field," "discipline," and "science" as we talk about crossing the sometimes murky borders that exist between different areas of inquiry. Political necessity often drives much of the distinction. We use such terms to define our institutional allegiances and establish our prestige vis-à-vis others; thus, it becomes important to rank one's own professional connection higher to claim a greater share of prestige.

Interdisciplinarity and the Nature of Composition Studies

> [T]here has been a remarkable transformation in our notions of what constitutes the scholarly enterprise as we have brought definition, clarity, and substance to a discipline that is still variously articulated. Colleagues have reinvented and refined traditional modes of inquiry and created new paradigms, efforts that have

given rise to a matrix of innovative mechanisms for quantitative and qualitative analyses, for theoretical and pedagogical designs. . . .

In essence, we have reached out abstractly and concretely across boundaries of knowledge and experience to enhance our understanding of the written word as a human phenomenon.[3]

--Jacqueline J. Royster, 1994

Throughout this dissertation, I focus primarily on the appropriation of Thomas Kuhn's theory of paradigms and revolution from the history and philosophy of science as an example of interdisciplinary borrowing, and secondarily on a seemingly intertwined issue--the drive towards disciplinarity in composition studies. As this historical survey shows in the case of borrowing Kuhn's theory, one cannot separate his appropriation by composition scholars from calls for establishing composition studies as a valid academic discipline. Kuhn seems inexorably linked to calls for disciplinarity. When scholars such as Richard Young, Patricia Bizzell, and Maxine Hairston claim the existence of a paradigm in composition inquiry, or others such as N. L. Gage, James Kinneavy, and Janet Emig suggest the preparadigmatic nature of the discipline, they tacitly borrow the prestige of science offered by applying Kuhn's theory to composition studies.

Whether directly borrowed from the history and philosophy of science and used as an analogy, appropriated as borrowed by another discipline and applied to composition, or based on an earlier borrowing by compositionists such as Kinneavy or Young, use of Kuhn seems politically motivated. Gage (1963) argues for devoting greater attention to the "logistical paradigms" that govern the personnel, financial, and institutional support for composition research--issues intimately connected with academic prestige, disciplinary status, and tenure. When Bizzell (1992) criticizes Hairston (1982) for

using Kuhn to enthrone empiricism as the principal research methodology within the discipline, she does so from a position of having already offered her own version of Kuhn (Bizzell 1979). By seeking parallels in the process "paradigms" used in the field of design to describe the efficacy of the writing-as-process paradigm in composition, Charles Kostelnick (1989) harnesses Kuhn's application in another field to support the process movement in composition studies.

Similarly, counterattacks on Kuhn's appropriation by composition studies are no less politically motivated. When Robert Connors (1983) reviews and rejects the "tacit" conclusions of earlier borrowers of Kuhn, finding fault with both the pre- and post-paradigmatic views of composition studies, he makes a political argument against borrowing too much from the sciences. While attacking Young (1978) for misapplying Kuhn's theory to composition and for making a "power play" promoting a particular research agenda, North (1987) seems to modify some of Kuhn's ideas to fit his own agenda, the deconstruction of the emerging discipline of composition studies by modes of inquiry and methodological communities. Such efforts reflect the political nature of the debate over disciplinarity in composition studies. If composition studies can claim status as a discipline, its adherents can claim academic prestige, garner a proportionally greater share of institutional resources (tenure positions, research funding, sabbaticals, and so on), and have a greater say in institutional decisions (curriculum, hiring, and allocation of resources for example). Seen in light of the politically charged call for disciplinarity, the process of appropriating ideas from outside of composition studies takes on greater importance as a means of helping to justify scholastic validity for composition inquiry.

From an examination of historical material such as journal articles, book reviews, and textbooks, acquiring an actual sense of the process of borrowing across disciplinary boundaries remains difficult. How and when a potential appropriation first came to the

attention of the compositionist borrowing it cannot be gleaned from archival research. Few of the early compositionists examined in this dissertation mention how and when the materials they borrowed first came to their attention; indication of the source, such as attending Richard Young's 1975 presentation at the Buffalo Conference on Researching Composing or listening to Maxine Hairston's address at the 1978 Conference on College Composition and Communication (CCCC), does not necessarily translate into a statement of first awareness or exposure to Kuhn's theory. Instead, the materials I examined provide more of a record of the sources of borrowings and an understanding of their spread throughout the discipline as abetted by our own institutions such as CCCC and publications such as College Composition and Communication (CCC) and College English (CE). Historical research thus shows only part of the process by which Kuhn's ideas have spread to the point where they have acquired the status of tacit knowledge within composition studies.

While an intense examination of a single borrowing such as Kuhn's theory may not reflect the dynamic nature of composition studies in total, it does suggest one major route by which ideas cross the border into composition studies, and it highlights how one of our own institutions, CCCC with its attendant journal College Composition and Communication (CCC), helps shape the nature of those borrowings. Many of the borrowings receive first exposure at CCCC's annual meetings or other major conferences and later show up as journal articles or essays in various anthologies. When embraced by some of the conference's leading stars and promulgated through presentations, chairperson speeches, and eventually publication in our journals, borrowings such as Kuhn's theory take on a life of their own, engendering debate and furthering our knowledge about composition theory and praxis.

Institutions such as CCCC play a key role in shaping borrowings through providing a venue for the exchange, debate, and promotion of ideas. CCCC evolved out of the

recognition that scholars interested in composition needed a forum of their own, an organization devoted more to their interests. The panels, forums, workshops, and caucuses at different conferences as well as at the annual CCCC present numerous opportunities to meet others in the profession and share ideas or pass on practitioner "lore" (as Stephen North might remark). As composition studies has grown, the need for avenues to present scholarship and to develop professionally has increased, providing additional indication of the drive towards specialization and disciplinarity. Further, the diversity of the institution's membership translates into a wide variety of interests--linked to some aspect of writing theory, practice, or pedagogy--that boil together in the cauldron of CCCC.

Similarly, support for journals such as CCC has meant increased opportunities to publish composition scholarship. In some ways, the submission and review policies as well as editorial decisions act to "regulate" the importation of ideas. As a profession, we value the blind review of submissions and the exchange of ideas that often occurs with other professionals during the process. At the same time, editorial decisions such as the one that grouped Hairston's "Winds of Change" with Emig's "Inquiry Paradigms" essay in the February 1982 issue of CCC can make significant difference in the reception of a borrowing or its interpretation; a check of citations shows that Hairston's interpretation of Kuhn and call for revolution nearly drowned out Emig's analysis of inquiry paradigms. By providing opportunities to publish ideas and soliciting responses to those ideas, CCC shows an openness to borrowing from other disciplines and allowing its readers to engage in debate over the efficacy of those borrowings. Columns such as "Counterstatement" and "Staffroom Interchange" promote the exchange and discussion of ideas in print, thus reaching a wider audience than a room full of compositionists at CCCC. The pages of CCC thus reflect the trends in composition scholarship and point out

possible directions for future research while providing a venue for the exchange and debate over ideas, especially those imported from other disciplines.

Tracking the spread of a single borrowing through the pages of CCC and to other sources can only reveal part of the nature of composition studies today. In the case of Kuhn, we can see that his appropriation clearly accompanies a political move to establish composition studies as a legitimate academic discipline, worthy of a slice of institutional resources such as research funding and jobs, and possessing sufficient prestige to place composition scholars on a par with their more "established" colleagues in literature, literary criticism, rhetoric, and elsewhere. While all of us may not support Kuhn's appropriation or interpret his ideas in the same way, we have nevertheless added his terminology to our disciplinary vocabulary and used the forums offered by CCCC and CCC to debate his application.

The uses of Kuhn in the pages of CCC and in various works cited in CCC present a portrait of a discipline in the making, a discipline concerned with the issues spawned by interdisciplinary inquiry into writing theory, practice, and pedagogy. As such, composition studies has emerged as something distinct from literary studies, but not completely severed from its deep roots in rhetoric. The history of composition's evolution as a valid subject of academic scholarship is filled with debate over its status versus literary studies. Early arguments such as George Bramer's "Comp. vs. Lit." (1977) and Steven Mailloux's "Literary Criticism and Composition Theory" (1978) carve out seemingly competing positions for composition studies as a separate discipline or as a subdiscipline of English. Later participants in the debate such as Stephen North (1987) and Louise Wetherbee Phelps (1988) continue to contend that composition is a separate entity; North, for example, sees an "emerging field" characterized by methodological plurality, while Phelps claims status for composition studies as a

"human science," independent of literary studies, but still very much connected to rhetoric.

Composition studies' deep disciplinary roots in rhetoric show in our various attempts to tie the modern history of composition teaching to the classical history of rhetoric. Many historical treatments of composition studies rely on establishing and promoting those ties. In one sense, the effort constitutes an attempt to redistribute some of the prestige of the older discipline to the younger. In another sense, rhetorical theory has much that informs writing practice and pedagogy. Our modern historical connections with rhetoric can be seen in arguments such as Virginia Burke's (1965) promotion of grounding composition inquiry in rhetoric, Young's (1978) concern for derailing "current-traditional rhetoric," North's (1987) identification of the community of Rhetoricians and rhetorical theory's influence, and Carol Berkenkotter's (1991) call for better articulation of our rhetorical practices. Phelps (1988) sees rhetoric as a separate, yet closely related discipline. These connections suggest a broader discourse community of composition studies that includes rhetoric.

Our stylization of the community throughout the modern history of composition studies (North dates that birth around 1963) as "Rhetoric," "Rhetoric and Composition," and "Composition Studies"--terms often used interchangeably--has usually resulted in a blurring of disciplinary lines that merge rhetoric and composition, yet separate them between theory and practice. Indeed, I submit that increased academic interest and validation of composition inquiry has created a renewed interest in rhetoric. In our constant quest to validate composition inquiry and inform our theory and practice, we often turn to those historical roots for inspiration and ideas. The yoking of theory to praxis in composition studies reflects our disciplinary concern with theory and practice as well as our continuing interest in the teaching of writing. In this light, rhetoric and composition are not competitive fields of play segregated by rigid boundaries. For

composition studies, the connection between theory (rhetoric) and praxis (composition) represents a wedding of ideas and practices, an intellectual union characterized by free exchange across a permeable boundary.

A close examination of the spread of Kuhn's theory seems to confirm the permeability of the boundaries of composition studies. Regardless of the vocabulary used to describe the transfer of ideas between disciplines (appropriations, border crossings, borrowings, and thefts for example) and the verbs used to describe that activity (appropriating, borrowing, shopping, browsing, stealing, raiding, and so on), the process implies some movement across boundaries. Whether we "borrow from," "shop in," or "raid" another discipline or field for ideas that we may use in our own inquiry, such activity requires a tacit acknowledgment of flexible disciplinary boundaries. Something exists that lies "outside" of the professional sphere of interest that constitutes composition studies, but picturing that sphere as encompassing anything that concerns writing theory, practice, and pedagogy suggests that we may range broadly across the boundaries that others use to designate their own "turfs." Compositionists often construe that interest in writing as licensing intrusion into every space where writing is done and promoting the acquisition of any ideas that might apply to composition theory, practice, and pedagogy.

But the blurring of disciplinary borders to permit borrowing does not translate into any desire to disestablish composition studies. Instead, that blurring has been matched by the competing desire to stake out composition's own disciplinary turf. In the struggle to establish composition studies as an independent academic discipline, Kuhn's contention that the acquisition of a paradigm "is a sign of maturity in the development of any given scientific field" (Kuhn, Structure 11) has helped push the search for paradigms within composition as well as its characterization as preparadigmatic. Perhaps the greatest incentive for borrowing Kuhn's ideas lies in that drive for legitimization of composition

studies as a valid academic discipline. Scholars such as Young (1978) and Hairston (1982) seize upon Kuhn's concept of paradigm on one level as explanation or analogy of advance within composition studies, thereby claiming implicitly the status of discipline for composition. Possession of a paradigm is the hallmark of a mature discipline, a science, according to Kuhn. Using Kuhn's meaning of paradigm in the sense of a community's adherence to a particular set of habits or a disciplinary matrix, such as the much-maligned current-traditional paradigm, functions to lift an emerging discipline out of its murky beginnings as a subfield of English; if we have a paradigm or paradigms, then composition studies must be a discipline following a Kuhnian line of logic.

Similarly, using Kuhn's idea of the "preparadigmatic state" offers a way for borrowers to make claims about why composition studies may lack commitment to a paradigm; the fragmented nature of inquiry into writing theory, practice, and pedagogy may simply reflect the competition of a number of schools for dominance. Claiming that composition studies is in a preparadigmatic state, about to emerge by establishing a first paradigm, serves the same function as the other primary use of Kuhn. According to this view, we are simply not fully developed yet, but coalescing in that direction. When Kinneavy (1971), Bizzell (1979), Fulkerson (1990), and others find evidence of composition studies' preparadigmatic state, they are still moving towards legitimization, arguing for research into composition studies as valued academic inquiry. Both positions involve what Klein would refer to as staking out a turf for composition studies within the larger academy of knowledge. Berkenkotter reinforces the idea with her 1991 "Paradigm Debates, Turf Wars, and the Conduct of Sociocognitive Inquiry in Composition" essay in CCC. Finding a paradigm or paradigms for composition inquiry thus equates to confirming composition studies as a mature, legitimate, academic discipline, establishing its status as worthy of the rights, resources, and prestige accorded to those disciplines already recognized within the academy.

Many borrowers of Kuhn, especially in the early eighties, use his ideas as evidence to support the claim of disciplinehood for composition studies. Few, with the notable exception of Robert J. Connors (1983), bother to explore Kuhn's reception in his own field, and Connors does so only to use that reception to attack both the pre- and post-paradigm versions of Kuhn's theory in composition. Sole reliance on one interpretation of Kuhn or another (Young 1978 and Hairston 1982 seem to be those most often preferred) thus involves the danger of borrowing someone else's appropriation without a thorough understanding of the original source. Connors is correct in going back to the history and philosophy of science where Kuhn's ideas developed and checking on the reception of those ideas. Too often we borrow from other disciplines without trying to "go native" as Kuhn would say and learn the language of that discipline before applying the borrowing to our own.

If knowledge in composition studies does grow through selective raiding of other disciplines for ideas, then we should find evidence of theft and borrowing readily imbricated throughout theory and practice within the field. We do not have to look far to find such evidence. Examination of CCCC programs clearly illustrates the variety of interdisciplinary borrowings that many see as influencing their work within composition studies. Indeed, in the late seventies and eighties, when Kuhn was being most actively borrowed and his application debated, many of those borrowings first appeared as papers read at CCCC or other conferences and later published in some form in composition's major journals or in essay collections such as Cooper and Odell's 1978 Research on Composing: Points of Departure. CCCC roundtables such as the 1993 "Paradigm Wars and Peace Dividends: Issues, Answers, and Action Beyond the Disciplinary Front" chaired by Carol Berkenkotter show an interest in exploring theories imported from other disciplines. A review of the five most recent years of CCCC programs (1990-1994) provides topics ranging from the use of personality

instruments such as Myers-Briggs to the application of curriculum theorists such as John Dewey, the continuing integration of technology in the writing classroom, performing qualitative research, and so on. The variety indicates the extent to which composition studies has grown in its exploration of writing theory, practice, and pedagogy.

Investigating Border Crossings and Borrowings

> [T]he composition teacher must not allow himself to be browbeaten into believing that his discipline is the stepchild of all other disciplines.[4]
>
> --Patrick W. Shaw, 1974

The goal of this dissertation is to answer this question: What does the study of a single borrowing tell us about the structure of (and, hence, the nature of) composition studies? As specific objectives, I examined one example of borrowing and attempted to identify the criteria, if any, that led to the appropriation of that idea from another discipline and the application or transformation of the idea to fit composition studies. The example of borrowing--the theory of paradigmatic structure developed by Thomas Kuhn in the disciplines of history and philosophy of science--perhaps exemplifies the variety of appropriated ideas and illustrates what is happening in composition studies. At what point do developments in other disciplines appear in our own? What do differences between the original and the appropriated versions tell us about composition studies as valid academic inquiry? What do these borrowings tell us about the nature of the boundaries of composition studies? I explored answers to these questions as I examined the development, appropriation, and adaptation of Thomas Kuhn's theory and compared

our version with its concurrent development in its original disciplines of history and philosophy of science.

While the use of such a border crossing or borrowing may depend initially upon the state of the idea and its form in its own field at the time it comes to the attention of composition, the borrowed idea then often diverges or develops independently as we reshape it to meet the complexities of composition studies. I attempt to trace the development of just this one borrowing: first, to examine Kuhn's theory as it originally developed and gained acceptance within the history and philosophy of science, disciplines outside of composition studies; second, to trace the process of borrowing; and third, to describe the nature of the borrowing's appearance in composition studies. Further, I compare the current state of Kuhn's theory as we now use it in composition studies with the present position of the idea as it has continued to develop within its original area of inquiry.

To meet these objectives, I looked intensely at the last three decades of College Composition and Communication (CCC) with a focus on what this publication tells us about the example of borrowing Kuhn's theory. To a lesser extent, I also examined College English (CE) and other journals when articles from them were referred to in CCC. Additionally, I reviewed a number of composition theory and research texts, attempting to chart the attention devoted to Kuhn's ideas. I wanted to identify the extent of institutional influence on the conscious appropriation of ideas from other disciplines and the application of such ideas in the composition classroom.

I divide my examination of borrowing Kuhn's theory into seven chapters. In Chapter Two, I address issues connected with calls for interdisciplinarity, showing that the history of such calls has accompanied the maturation of composition studies. Next, I examine the development of Kuhn's ideas and their initial reception within his own disciplines of history and philosophy of science in Chapter Three. Here my emphasis

rests in attempting to trace the development of Kuhn's theory, moving from early articulations of his ideas through the publication of his 1962 work, The Structure of Scientific Revolutions. In Chapter Four, I cover early criticism of his ideas by scholars within the history and philosophy of science and Kuhn's responses to their remarks. In Chapters Five and Six, I explore borrowings of Kuhn as they appear in CCC and in the various research reports, major texts, and collections of essays referenced in the journal. I focus on the way borrowers of Kuhn promote his work as part of their larger arguments or attack other uses of Kuhn as inappropriate. Chapter Seven returns to science studies and looks at the reception of Kuhn's theory today. Finally, in Chapter Eight, I analyze some contemporary borrowings of Kuhn by compositionists, speculating on the nature and influence of appropriating Kuhn in general and exploring the implications of interdisciplinarity for the future of composition studies.

As we will see, the primary appropriations of Kuhn, whose work as borrowed mostly concerns disciplinarity, appear in arguments about the status of composition studies as a discipline. Major works such as North's 1987 The Making of Knowledge in Composition and Phelps's 1988 Composition as a Human Science argue about the status and prestige of composition studies while drawing upon or accounting for Kuhn's theory. Thus, this dissertation develops along two tracks: the appropriation of Kuhn and the push to establish composition studies as valid academic inquiry.

Methodological Considerations

> Composition research, then, is not highly developed. If researchers wish to give it strength and depth, they must reexamine critically the structure and techniques of their studies.[5]
>
> --Richard Braddock, Richard Lloyd-Jones, and Lowell Schoer, 1963

The methodology used in this dissertation is primarily historiographic and is subject in many ways to the availability of resource materials stretching back across a period of over thirty-five years. I have concentrated on published sources even though much of the scholarly work I review appeared much earlier in conference presentation form than in print. Richard Young, for example, delivered his "Paradigms and Problems: Needed Research in Rhetorical Invention" at the 1975 Buffalo Conference of Researching Composing, but the published version of his presentation did not appear until 1978. Donald C. Stewart uses the unpublished 1975 version of Young's paper in a 1978 analysis of composition texts of in "Composition Textbooks and the Assault on Tradition." I place great dependence upon the available CCC journal materials as a reflection of the state of composition studies. In fact, there are important ways in which this dissertation is a study of an evolving community, one comprising the "field" or "discipline" of composition as made manifest by CCC. My primary research included a key word search of the journal materials with a concentration on direct references to Kuhn or significant elements of his ideas such as paradigms, crises, shifts, and revolutions. Rather than simply depend on works cited pages, bibliographies, and footnotes or endnotes, I examined each article in CCC from 1962 to 1994 looking for evidence of Kuhn's ideas. Tracing bibliographical references established further connections with related non-

journal materials, such as texts and research reports, as well as other journals such as College English. I also conducted key word searches of CD-ROM data bases such as ERIC and MLA with the intent to isolate uses of Kuhn's theory as it applied directly to composition studies.

Using CCC as a reflection of composition studies in general, I focused on identifying sources within the journal that made use of Kuhn's ideas or referenced his work, attempting to trace such references to other sources. As it would be impossible to include all of the sources in composition that cite Kuhn or use his ideas indirectly, my review necessarily involved some choices. I have concentrated on those materials immediately available within the pages of CCC. When those materials referred repeatedly to Kuhn's use in sources outside of CCC such as James Kinneavy's 1971 A Theory of Discourse and Patricia Bizzell's 1979 article in CE, "Thomas Kuhn, Scientism, and English Studies," I added those sources to my examination. Still, the scope of the project precluded investigating every potential citation in the pages of CCC for evidence of Kuhn.

As I identified sources, I arranged and analyzed them chronologically in an attempt to come to some understanding of the historical pattern of Kuhn's appropriation by composition. The selected borrowings of Kuhn appear as close to chronological order as possible. I seek to describe the spread of Kuhn's ideas within composition studies, letting that description unfold in a pattern that parallels efforts to import and apply Kuhn to our inquiry into theory, practice, and pedagogy. Such a methodology focuses on providing a sense of the evolution of the composition community in its own words as seen principally in the pages of CCC.

Pursuing bibliographic references in CCC and searching the CD-ROM data bases revealed the widespread application of Kuhn's ideas in other fields as well. The sheer volume of findings makes any claim to have discovered all of the uses of Kuhn in composition studies alone suspect; as such, I express no claim of this nature. In my

examination, I consciously excluded items such as book advertisements in CCC and freshman composition textbooks even though they represent potential sources of data; I concentrated instead on those theoretical and practical materials oriented toward appropriating or rejecting Kuhn's ideas. Thus, the materials I cite within my text reflect a significant portion of the borrowings of Kuhn, but not all.

In addition to denying any claim of full discovery, I acknowledge that the definition and usage of terms such as "rhetoric," "rhetoric and composition," and "composition studies" to designate specific areas of inquiry are deliberately vague in many cases. The ambiguity reflects much of the struggle to establish inquiry into writing and the teaching of writing as valid academic scholarship. Indeed, many of the writings to which I refer use such terms interchangeably, in doing so often displaying the ideological or methodological commitments of their users. Inquiry into writing and writing instruction has ties that some trace back to the classical rhetoricians of Greece and Rome; others such as Stephen North and Susan Miller claim a specific birthplace or date of inquiry into composition that has a distinctly American flavor rooted in efforts to establish first English, then composition as legitimate disciplines.

Like the interchangeability of terms, the widespread appearance of sexist language in much of the early material I cite, especially material from the history and philosophy of science, represents the conventions of the period I examine. The use of such language reflects the tenor of the mid-century and gradually attenuates as the conventions changed over time. Rather than sacrifice any accuracy in description, I have retained the language as reflective of the period.

Finally, as my primary methodology is historiography ("archival research" as some would call it), much of my analysis of interdisciplinary borrowing remains descriptive. Compared to the way traditional English studies deals with quoted material, the methodology of historiography depends more upon the presentation of historical sources

chronologically in context. For this dissertation, that translates into the inclusion of more direct quotes to capture not just the flavor, but in fact the substance of the community's evolution. I am interested in what others in a profession variously characterized as "English," "rhetoric," "rhetoric and composition," and "composition studies" say about borrowing and how they borrow. I see elasticity as one of the greatest strengths of a profession interested in the theory, practice, and pedagogy of writing. I assert that composition has matured as a profession, a field, a discipline, and perhaps even as a human science within an environment of pluralism and diversity that encourages borrowing. To abandon our "selective raiding" of other fields for some single composition paradigm means stepping away from a position that has encouraged the multiplicity of voices informing our inquiry.

CHAPTER TWO

ESTABLISHING COMPOSITION STUDIES AS LEGITIMATE INQUIRY

> Our particular field has largely developed by selectively raiding others. Rather than condemn theoretical infidelity, however, we should pinpoint how composition scholars "rewrite" their precursors as well as reproduce them.[6]
>
> --John Schilb, 1991

Treatments of composition studies in the sixties and seventies range from outright condemnation and dismissal of composition studies as inquiry too concerned with praxis or pedagogy to its characterization as a critical subcomponent of some larger discipline such as English studies, a tripod combination of literature, literary criticism, and composition. Some argue for the need to base composition studies in some more established, more legitimate discipline such as rhetoric, the classical study of oral and written discourse. Still others argue that composition studies is at some point along the spectrum between emergence or acceptance as a discipline in its own right. Implicit in these arguments is a debate over the definition of composition studies as well as the role that interdisciplinary borrowing plays.

The publication of a number of essay collections and histories of the English profession during the latter half of the seventies raised additional issues connected with the validity of composition studies as an independent discipline. Many of these works stress the debt that composition theory, practice, and teaching owe to other fields. Cognitive psychology, linguistics, speech communications, curriculum development, and science studies are only a few of the disciplines to which compositionists turn to inform or influence their inquiry.

As we move into the eighties and the first half of the nineties, our professional writings continue to reflect the dynamic nature of composition studies, the profession's move towards legitimization as a discipline, and its continued dependence on interdisciplinary borrowing as a source of inquiry and inspiration. During that time, a number of works appear that attempt to place the discipline in perspective, investigating the profession's structure or suggesting a framework for understanding composition studies as well as pointing to the value of interdisciplinary borrowings. Authors such as North, Phelps, Fulkerson, Lindemann and Tate, and Olson make claims about the status of composition studies as a field or discipline, often pointing to informing concepts from outside of composition studies. Still others, such as Richard Lloyd-Jones, argue for a more metadisciplinary position for the profession. Evidence of interdisciplinary borrowing abounds in the attempts to legitimize the discipline.

Many of these later writers share a perception about the fluidity or ambiguity of the discipline's borders. That ambiguity seems to facilitate importing ideas from other fields such as anthropology, cognitive psychology, philosophy, and even the natural sciences. In the interdisciplinary profession of composition studies, compositionists actively browse in other disciplines for ideas and theories to use in our own. Thus the shared perception of permeable borders acts to encourage interdisciplinary borrowing.

Examining a number of treatments throughout the last thirty-five years provides snapshots of the profession's development and maturation process while suggesting the role that interdisciplinary borrowings play. I present these treatments in chronological order to show the linear thread of interdisciplinarity that has accompanied the evolution of composition studies. If we define composition studies as an area of inquiry into writing and writing pedagogy, these treatments show some of the diverse efforts to situate such inquiry either as part of some larger whole or as independent in its own right.

The Sixties and Early Seventies: Early Calls for Disciplinarity

> [A]nyone venturing into the fields related to composition must arbitrarily make the choice of his wanderings.[7]
>
> --James L. Kinneavy and C. Robert Kline, Jr., 1976

In tracing the development of composition studies, many historians of composition tend to draw attention to its beginnings in the rhetoric and composition courses taught at Harvard in the nineteenth-century and follow those beginnings through the evolution of the university system, two world wars, Sputnik, and the turbulent sixties. Others see composition's development as inquiry rooted deeply in classical rhetoric. The sixties brought an increasing awareness of the possibility of teaching and research of writing as a separate field of inquiry. One scholar among many, Stephen North, even dates "the birth" of composition studies, citing 1963 as the date that "marks its emergence as a nascent academic field" (15) and injecting a distinctly unique American flavor.

Early calls for disciplinarity reflect the fragmented nature of composition studies as it struggles to become a separate field in the sixties and seventies. Richard Braddock, Richard Lloyd-Jones, and Lowell Schoer equate composition research with "chemical research as it emerged from the period of alchemy," calling composition research "not highly developed," and thus pointing out the confused nature of composition studies as a field of inquiry (5); their solution is to borrow the "scientific method" of empirical research from the sciences. Some scholars, such as Virginia M. Burke, attempt to tie composition to an already established field; for example, Burke argues in 1965 for centering rhetoric as the basis of composition studies. Others, such as Carl Klaus, assert that composition studies is emerging out of the fragmentation and chaos, coalescing into a

discipline separate, yet related to English. George R. Bramer addresses the issue of separation by calling attention to the growing dichotomy between teaching composition and teaching literature.

Efforts by scholars such as Braddock, Lloyd-Jones, Schoer, Burke, Klaus, and Bramer reflect the dynamic nature of composition studies in an age of change requiring greater interdisciplinarity on the part of composition practitioners. Their writings highlight the confusion accompanying a professional struggle for academic identity. Their overall interest and concern for writing and the teaching of writing unites them and perhaps offers the best basis for defining composition studies as a field or discipline.

In 1963, the National Council of the Teachers of English (NCTE) published the report of its ad hoc Committee on the State of Knowledge in Composition. Richard Braddock, Richard Lloyd-Jones, and Lowell Schoer's landmark work, Research in Written Composition, summarized the findings of the committee, which had been assigned the task of reviewing "what is known and what is not known about the teaching and learning of composition and the conditions under which it is taught, for the purpose of preparing for publication a special scientifically based report on what is known in this area [written composition]" (Qtd in Braddock, Lloyd-Jones, and Schoer 1). The committee decided to limit in three ways the research they would consider: first, acceptable research would address only written composition; second, the research had to involve an "actual" written component; and third, the researchers had to employ sound "scientific methods" (1). The committee then reviewed more than 1000 bibliographic citations before honing down the list of research to 485 studies that fit its criteria. Five of the studies were selected for closer examination as part of the report.

The chapter of the report detailing the state of knowledge about composition presents an excellent picture of the field in 1963. In the chapter, Braddock, Lloyd-Jones, and Schoer "review what is known about the teaching and learning of written composition, as

distinct from research procedures" (29). They focus on the environmental and instructional factors influencing composition, indicate some rhetorical considerations, and contrast objective tests with actual writing as measures of written composition. In addition, they suggest the influence of other variables such as class size, materials, technology, vocabulary and spelling, handwriting and typewriting, and speech. Finally, Braddock, Lloyd-Jones, and Schoer offer a list of twenty-four fundamental questions--"unexplored territory"--that "indicate areas in which future investigators may wish to direct their efforts" (52). For example, researching "What is the effect on writing of having the student compose his paper for different kinds of readers?" suggests exploration of audience as a factor in composing. Asking "At which levels of maturation *can* particular aspects of writing most efficiently be learned?" suggests investigating the interrelationship of cognitive development and composition. Examining "What is involved in the act of writing?" calls for more attention on process. Pedagogical concerns also occupy a portion of the Braddock, Lloyd-Jones, and Schoer's list as seen in their very first question: "What kinds of situations and assignments at various levels of schooling stimulate a desire to write well?" Other questions address topic selection, rhetorical considerations, the relationship between reading and composition, English as a second language (ESL), linguistics, modeling, discourse forms, formal study of rhetoric and logic, and invention (Braddock, Lloyd-Jones, and Schoer 52-53).

Braddock, Lloyd-Jones, and Schoer assert that composition research "has not frequently been conducted with the knowledge and care one associates with the physical sciences" (5) and argue cogently for more empirically-based research. By focusing on scientifically-based studies, they suggest the application of the empiricist approach to all research in composition. Their suggested methods of research, dependent upon scientific research design and statistical analysis, are indicative of efforts to

legitimatize composition research as scholarly inquiry by duplicating the accepted methodologies and procedures of the sciences.

The net effect of Braddock, Lloyd-Jones, and Schoer's report is to promote empirical research into composition over other forms. Simultaneously, the report lays out specific directions of research that suggest the interdisciplinary nature of work in composition. Braddock, Lloyd-Jones, and Schoer chart a movement away from a simple concern with the pedagogical issues of teaching writing to an interest in cognitive processes, rhetoric, speech communication, socio-linguistics, reading theory, discourse analysis, and ESL.

Virginia Burke, like Braddock, Lloyd-Jones, and Schoer, also sees the efficacy of looking to other disciplines for validation. For Burke, composition studies must borrow legitimacy from rhetoric. In her 1965 CCC article, "The Composition-Rhetoric Pyramid," Burke calls for "more concerted and scholarly efforts to restore rhetoric as the informing discipline in the practice of composition at all levels" (3). Burke proposes reconstruction of "a rhetorical theory suited to today's needs--a flexible theory as the informing discipline for the teaching of composition, and a practice based upon it." She notes: "There is chaos today in the teaching of composition because since the turn of the century composition has lacked an informing discipline, without which no field can maintain its proper dimensions, the balance and proportion of its various parts or its very integrity" (Burke 5).

Burke situates her argument for rhetoric as the informing discipline--not some separate discipline of composition studies--within the pragmatic ends associated with praxis and pedagogy. To Burke, composition studies is a "field," and therefore something less than a discipline. She asserts that "the power of a discipline to identify and maintain a field and to energize practice in it should be self evident." Continuing, Burke writes, "*With a discipline*, some reasonable sequence, moving *from* something identifiable *toward* something identifiable is clearly suggested. . ." (6).

Burke's map of composition studies subjugates the praxis and pedagogy of composition studies under the discipline of rhetoric. While she acknowledges composition's existence as a field of practice, it remains only that--a field borrowing its legitimacy from an already established discipline. Her argument depends on going outside of composition to a closely interrelated discipline, instead of seeking within composition the seeds of its own validation. Whereas Braddock, Lloyd-Jones, and Schoer call for borrowing and privileging research methodologies and techniques from the physical sciences as a means of achieving greater validation for composition research and thus academic inquiry into composition, Burke suggests seating an already established discipline (rhetoric) at the head of the table, borrowing its legitimacy at the expense of continuing to relegate composition to a subordinate position.

Carl Klaus, writing eleven years later in his 1976 "Public Opinion and Professional Belief," takes a very different stand than that of Burke. The mid-seventies found English and composition studies reacting to the hue and cry caused by "Why Johnny Can't Write?" and calls for returning to teaching "basics" in composition. In his response to the question about Johnny's inability to write, Klaus champions composition studies as legitimate, interdisciplinary inquiry. He notes that: "When writing is understood as a process, the study of it necessarily demands an interdisciplinary approach" (337). Klaus argues that inquiry into the writing process requires expertise in multiple disciplines including, to name just a few from Klaus's litany, linguistics in its various forms, cognitive psychology, rhetoric, literary criticism, psychoanalysis, and statistics. Klaus asserts, "If the process is to be wholly understood, if we are to know as much about writing as we possibly can, then we must bring to the study of it as many disciplines as are possible and appropriate" (337). Studying writing from such a perspective means that compositionists become exposed to ideas from other fields that might influence inquiry into composition, thus promoting the tendency to borrow.

Klaus's call for a discipline grounded in interdisciplinary study focuses on the potential for other disciplines to inform inquiry into composition. Writing becomes the central activity around which Klaus proposes to organize such interdisciplinary inquiry.

Klaus decries the continuing tendency to consider composition as less important than literature. He complains that devaluation means "writing teachers will continue to be treated like second-class citizens, like dedicated amateurs, which we are, rather than genuine professionals, which we are not" (340). Echoes of Klaus's complaint resound throughout much of the writing about composition studies during the period, and many in the field repeat similar themes as they argue for increasing specialization in the teaching of composition as well as interdisciplinary borrowing.

"Many college English teachers are assigned to teach composition," writes George R. Bramer in the February 1977 issue of CCC, "but few are trained to do it" (30). Bramer's "Comp. vs. Lit.--What's the Score?" represents another direction of argument about the status of composition studies. Bramer explores increasing specialization in composition vis-à-vis conflict with traditional literature programs.

The basis for Bramer's argument rests in his contention that "a real conflict between 'Lit' and 'Comp' has developed within the discipline of English." Bramer attributes this conflict to "some resistance to the apparent excess of literature courses in the preparation of people who become composition teachers" (30). Bramer makes the point about teacher preparation for composition courses to argue for greater specialization in composition studies.

Bramer's call for increasing specialization in composition reflects the real conflict in English--the emergence of composition studies as a separate, major field of inquiry, related but not subjugated to literature or literary criticism. Bramer does not go very far in supporting the emergence of a separate discipline, but he does suggest that

specialization in teaching writing needs to be allotted greater prestige and certainly needs more attention.

The Late Seventies: Mapping the Field

> Just given the sheer number of assumptions, theories, methods, and questions relevant to composing, achieving a better understanding of our field will be difficult. We will have to raise questions that heretofore have seemed unaskable; we will have to devise new procedures for obtaining answers, and we will have to be patient and allow these new techniques time to yield the answers we seek. Most difficult of all, we must be prepared to accept a provisional understanding of our field with new questions and procedures far outnumbering undisputed facts and proven methods.[8]
>
> --Charles R. Cooper and Lee Odell, 1978

To get the best picture of the state of composition studies during the latter half of the seventies, we need to turn to two collections of essays in addition to the professional journals--Gary Tate's 1976 Teaching Composition: 10 Bibliographical Essays and Charles R. Cooper and Lee Odell's 1978 Research on Composing: Points of Departure. Tate's 1976 Teaching Composition and Cooper and Lee Odell's 1978 Points of Departure provide snapshots of a discipline of composition studies coalescing during the turbulent times following the back to basics movement. Their attempts to map composition studies reflect a growing interest in interdisciplinary inquiry into writing. Simultaneously, the collections demonstrate the breadth of inquiry into composition theory, practice, and pedagogy.

In the 1976 Teaching Composition: 10 Bibliographical Essays, Gary Tate offers ten essays on topics "that seem to [him] central to the teaching of composition today [1976]" (viii). The essays range from traditional issues of invention and style gathered from classical rhetoric to examinations of composition's relationship with other disciplines, especially linguistics, philosophy, education, and speech communication.

Tate's opening chapters center critically on rhetorical issues connected with invention, arrangement, style, and modes of discourse. Richard Young offers a topographical survey of invention. Richard L. Larson is interested in the structure and form of non-fiction prose. Edward P. J. Corbett addresses style while Frank J. D'Angelo writes about the modes of discourse. The remaining chapters consist of essays on basic writing, the media, linguistics, rhetorical analysis, relationships with other fields, and dialects. Each essayist concentrates on the implications that scholarship in these areas holds for the teaching of composition.

The value of Tate's collection, as a whole, rests in the breadth of the subjects on which he has invited commentary. As a snapshot of composition studies in 1976, his work makes a sweeping claim for "the vast scope of the discipline" (Irmscher, "Teaching" 92). Further, it explores the influence of disciplines outside of composition studies and suggests the tendency of compositionists to borrow freely from other disciplines. In some cases, contributors to Tate's collection, especially James L. Kinneavy and C. Robert Kline, Jr., admit the necessity of such borrowings.

Kinneavy and Kline's essay in Tate's 1976 Teaching Composition deserves special mention for its attention to interdisciplinary issues. In "Composition and Related Fields," Kinneavy and Kline examine composition in light of its relationship with other disciplines. Kinneavy and Kline do so from the stand of discourse analysis and note the difficulty that anyone who would venture into fields related to composition must face:

> [B]ecause discourse analysis in one sense is relevant to any discipline and because nearly all disciplines have introspective analyses of the language of their discipline, in an important sense the *fields related* to discourse analysis become coterminous with the areas of scholarship itself. . . . Consequently, anyone venturing into the fields related to composition must arbitrarily make the choice of his wanderings. (Kinneavy and Kline 241)

Their arbitrary choices involve philosophy, speech, and education, for in "many instances, there have been direct applications of work from these disciplines to matters of composition." Further, they assert that in other cases, the application has yet to be made, is in progress, or "is not at all imminent, but seems urgently needed." Kinneavy and Kline state that they intend to incorporate three viewpoints in their essay on composition and related fields--"applications already made, applications in progress, and applications yet to be achieved" (241-42).

Kinneavy and Kline advance a number of reasons for relating composition to speech, education and philosophy. They note historical connections between speech and English departments, suggesting that speech teachers, perhaps more than English teachers, "have been the bearers of the rhetorical standard passed on from the tradition of liberal arts"; they suggest that speech departments are probably conducting more original research in rhetorical issues. They allude to a pedagogical connection with colleges and departments of education, noting that such colleges and departments have assumed "much of the burden of training secondary and college teachers of English in important aspects of their preparation--pedagogical skills, supervision of apprenticeships, research into curriculum planning and effectiveness, etc." (Kinneavy and Kline 242). Inherent in Kinneavy and Kline's argument is the conflation of composition with rhetoric. Much of

the relationship they see between composition and speech communication depends upon similar concern for rhetorical issues.

In the remainder of their bibliographical essay, Kinneavy and Kline focus on education and the teaching of composition. They emphasize "the necessity of looking at the *act of teaching*," calling it "paramount," then proceed to "review some of the major works and ideas which they feel inform the teaching of composition, especially with reference to actual classroom teaching strategies" (Kinneavy and Kline 256). The authors place special emphasis on the teaching of reading, the use of behavioral objectives, curriculum design, and evaluation.

The Kinneavy and Kline essay has value in any examination of interdisciplinarity in composition studies because the authors focus so much on influential writings that come from other disciplines or are heavily influenced by developments in those disciplines. Kinneavy and Kline assert that "rhetorical and literary criticism should use whatever other disciplines can offer" (252). By observing the contributions from other disciplines, Kinneavy and Kline build a case for an interdiscipline of composition studies with a grounding in philosophy and an intense interest in the practical pedagogy of teaching writing.

Appearing in 1978, Charles R. Cooper and Lee Odell's Research on Composing: Points of Departure contains a series of essays reflective of many of the earlier "laundry lists" of research needs in composition theory, practice, and pedagogy published in the pages of CCC and other professional journals during the sixties and early seventies. Like Richard Braddock, Richard Lloyd-Jones, and Lowell Schoer in their landmark 1963 work on needed composition research, the essayists in Cooper and Odell's Points of Departure survey the state of existing composition research and suggest directions for the future. Except for the first chapter, contributions to Points of Departure consist of papers prepared for the Fall 1975 Buffalo Conference on Researching Composing sponsored by

the State University of New York at Buffalo and funded primarily by the Research Foundation of NCTE. The publication of the conference papers gives further indication of the changes taking place within composition studies, especially the increasing attention given to the composing process and the pressure for disciplinary acceptance of composition studies. At the same time, the essays point to the expanding province of inquiry in composition studies and the use of borrowings from other disciplines to enhance research efforts.

In their introduction, Cooper and Odell argue that what "we have needed for decades and what we must have soon is a period of vigorous research on written discourse and the composing process." They attack the notion that "the most important kind of inquiry [is] pedagogical research" and clarify the fallacious assumption that the profession has, in 1978, "an adequate understanding of the term *composition*" (Cooper and Odell xi). Cooper and Odell suggest that testing of the precepts of that assumption has proved its unacceptability.

What new kinds of questions do Cooper and Odell raise in Points of Departure? They provide a list of questions ranging from issues connected with identifying the "syntactic, lexical, and conceptual" features "of successful pieces of discourse" to examining effective pedagogical practices for teaching writing, categorizing discourse into types, investigating the writing process, and detailing the "unique problems, special abilities, or special cognitive/perceptual sensory-motor activities" implicit in successful writing (Cooper and Odell xii). In their list, Cooper and Odell specifically reinforce the necessity of interdisciplinary borrowing of ideas and methodologies. Further, they argue that their contributors "raise questions that invite us to examine, test, and modify our basic assumptions about written composition" (Cooper and Odell xiv).

Points of Departure opens with a chapter by Cooper, Odell, and a collaborator, Cynthia Courts, introducing discourse theory and suggesting ways that researchers

"might test that theory directly by identifying and questioning some of the theory's major assumptions" (Cooper and Odell xiv); the assumptions involve the notion of purpose in discourse and the interrelationships between speaker, subject, and audience. The authors carefully state that it "may not be accurate to speak of these assumptions as a new paradigm; the present state of discourse theory may only be . . . 'preparadigmatic.'" More accurately, they describe the assumptions as perhaps part "of an *emerging* paradigm since different scholars are exploring theories that overlap in interesting and useful ways" (Cooper, et al 1-2). Cooper, Odell, and Courts focus on the tentative nature of research findings in investigations of these assumptions, observing that researchers must be willing to accept the tentativeness of their findings and "be willing to participate in . . . ongoing revision" (Cooper and Odell xiv).

The contributors to Cooper and Odell's Points of Departure expand the province of composition studies as a field of inquiry and suggest the directions further expansion might go. Broadening the exploration of the composing process to involve other, more established disciplines such as biology, psychology, and education, suggests the formulation of composition studies as a valid discipline concerned with not just the pedagogy of teaching writing, but with the process of composing itself. The concern for pedagogy still imbricates the ultimate goal of the research directions suggested in Points of Departure--learn more about the process of composing in order to help us teach better; but as the discipline of composition studies gains in credibility and acceptance, it moves from second-class citizenship under the umbrella of English to true scholarly inquiry in its own right.

Cooper and Odell's Points of Departure also has importance for the development of composition studies as a discipline for another reason; rife with references to paradigms and paradigm shifts, it exposes the professional community to elements of Thomas Kuhn's theories. Cooper, Odell, and Courts repeat James Kinneavy's 1971 claim that

discourse theory may be in a "preparadigmatic" state before they speak of an "emerging" paradigm in discourse theory. In his attack on the current-traditional paradigm in composition, Young offers a full treatment of Kuhn's theories, classifying composition studies as a discipline in crisis. Barritt and Kroll warn about the dangers of following science, especially physics, too closely while championing a diversity of methods and combination of "various research paradigms" (57). Emig characterizes inquiry into the biological and physiological aspects of composing as "preparadigmatic." Their essays suggest a broad concern for identifying the underlying theory or theories informing the field--composition studies' paradigm or paradigms--while setting directions for empirical research.

The search for an informing theory or theories, especially through borrowings from other disciplines, does not come without some trepidation. Barritt and Kroll specifically warn about "the unfortunate tendency to ascribe prestige to disciplines by their ability to approximate the presumed precision of physics" (56). They worry about the operationalizing of research methodology that, like physics, depends upon counting or measuring results, yet ignoring the complexity of the human factor in composing. Barritt and Kroll conclude:

> Research in composing, because it must study complicated human functions, is going to be less "scientific" than physics; but the alternative--reduction of complexity by forcing concepts into behavioral statements--will lead to an understanding not of the original concepts, but only of behavioral ones--and they are often not the same. (Barritt and Kroll 57)

The use of theory from other disciplines matched with the warning about overreliance on methods developed in the hard sciences reflects the continuing drive

toward disciplinarity in composition studies throughout the decade of the seventies. At the same time, it displays the interdisciplinary nature of inquiry into the composition process. Investigations into cognitive development, the reemphasis on invention and discovery, and the increasing attention devoted to the writing process over the traditional concerns for product and style show the field's expansion and dynamic structure. Writings by compositionists throughout the latter half of the seventies reflect an increasing concern for stabilizing the expansion as well as the field's structure.

The Eighties: The Challenge of Disciplinarity

> In a postmodern culture the writer reads what she needs in order to think, to make sense, not in order to know what is fashionable. There can be no canon of theory, any more than of literature. Composition is a field for generalists because it accepts anything with relevance to its praxis; in this light theory--writing--must be judged for what it does with what it learns.[9]
>
> --Louise Wetherbee Phelps, 1988

The decade of the eighties saw a continuing interest in establishing composition as valid academic inquiry, a discipline in its own right and synthesizer of knowledge developed in other fields in regards to writing. Expanded bibliographic sources such as the annual CCCC Bibliography of Composition and Rhetoric series call attention to the increasing amount of scholarship devoted to composition, expanding from the short bibliographies pioneered by Richard Larson in CCC in the seventies. The appearance of major analyses of the profession of composition studies, such as North's 1987 The Making of Knowledge in Composition: Portrait of an Emerging Field and Phelps's 1988

Composition as a Human Science: Contributions to the Self-Understanding of a Discipline, focus attention on understanding its structure as a discipline. At the same time, these studies highlight the influence of outside sources on composition studies.

Writing in the May 1982 issue of CCC, Richard Lloyd-Jones offers a global look at the profession of teaching composition in his article, "What We May Become." Focusing on "scholarship dealing with language, with the needs of society for writing, and with the structures of education," Lloyd-Jones examines the future of composition studies--and "the professional future for members of the CCCC" ("What" 202). He suggests that the boundaries of the profession of composition studies remain unclear, claiming that "the home of scholarship in writing is still philosophy, philology, rhetoric, and literature" and expressing his uncertainties about "where the boundaries should be drawn" (Lloyd-Jones, "What" 203).

Lloyd-Jones wants to draw the disciplinary boundaries as loosely as possible. He argues that, "We must picture ourselves as humanistic scholars building on discoveries of the long past refined by traditional methods, elaborated by new techniques, but still concerned with the largest questions of the nature of humanity." To do so requires borrowings from disciplines outside of composition studies, an idea that Lloyd-Jones heartily endorses when he declares, "Let us steal *ideas* from the social sciences" and "draw on the work of others" ("What" 203). In issuing his call to treat writing "as an essential part of the methods of all disciplines," he argues for a metadisciplinary position for composition studies (Lloyd-Jones, "What" 207). Lloyd-Jones acknowledges that the profession has not yet reached that point, but he expresses hopes that it will.

In the preface to the 1987 The Making of Knowledge in Composition: Portrait of an Emerging Field, Stephen North identifies himself "as a member of the first generation of English teaching professionals whose primary allegiance is to the teaching of writing." He identifies his allies as those "who, by both training and professional choice, see

themselves not simply as knowing something about composition, but whose academic identity derives from their membership in the field they call Composition." He offers a curt summary of the field before the mid-seventies, noting: "Composition, described so often as the 'ghetto' or 'stepchild' of English departments, was something that had to be taught. . . . But it was not perceived as a discipline or a field, as a subject matter suitable for graduate study" (North, Knowledge [iii]). He then describes his own "conversion" experience (North, Knowledge [iv]), an excitement that soon turned to frustration as he realized the ambiguity of the field. North notes his alarm at the field's lack of "fixed or absolute value, no field-wide definition" (Knowledge [v]).

In The Making of Knowledge, North describes the structure of composition studies in terms of modes of inquiry--that is, how knowledge-makers, in their various methodological communities, claim to know what they know. North writes:

> Specifically, it [Knowledge] is concerned with . . . *modes of inquiry*--the whole series of steps an inquirer follows in making a contribution to a field of knowledge--as they operate within *methodological communities*: groups of inquirers more or less united by their allegiance to one such mode, to an agreed-upon set of rules for gathering, testing, validating, accumulating and distributing what they regard as knowledge. (North, Knowledge 1)

North's position calls for an interpretation of knowledge as a "social construct, the truth value of which is a function of a given community's commitment to it." Further, it suggests that the truism that "knowledge consists of theory, which then informs practice--won't serve" (North, Knowledge 377). North notes: "Each mode of inquiry, including practice, can produce its own brand of theory. The issue, therefore, is not how

theory informs practice, but how these different brands of knowledge relate to one another: which can claim what kind of authority, and why" (North, Knowledge 377).

North clusters the overall field into three "rough groupings, held together by the kind of question that drives its constituent communities"; he labels these groups as the Practitioners, the Scholars, and the Researchers. North subdivides the Scholars further into the Historians, Philosophers, and Critics, then subdivides the Researchers into the Experimentalists, Clinicians, Formalists, and Ethnographers. According to North, the Practitioners ask the question, "What do we do?"; the Scholars ask, "What does it mean?"; and the Researchers ask, "What happened (or happens)?" (Knowledge 3).

North makes no claim to be measuring the value of knowledge made in one community against the standards of another. He tries to "describe each methodological community *from the inside*, to account for each community's patterns of knowledge-making in a way that, ideally, its members will recognize and approve." However, North does claim to attempt to paint a portrait of composition "*as a whole*" (Knowledge 5). He warns that the "modes of inquiry are not *the* eight modes in the field, with neat and measurable boundaries that will look the same to any investigator" (North, Knowledge 6). If anything, the boundaries of the communities shift dynamically depending upon where one stands.

Finally, North raises the specter of Rhetoric, noting that many of those he identifies as belonging to one community or another pledge allegiance to Rhetoric first. He notes that the terms, Composition and Rhetoric, "are sometimes used interchangeably: Rhetoric or Composition; and even more often as a pair: Rhetoric and Composition (almost always in that order)" (North, Knowledge 63). North's observation focuses attention on the crisis in scholarly identity growing out of the ambiguity of composition's borders and its interrelationship with rhetorical inquiry. Rhetoric, with its strong classical tradition, already possesses much of the legitimacy that composition

studies seek as an academic discipline. Since much of composition inquiry draws strength from rhetorical theory--investigating, promoting, and applying rhetorical techniques to the theory, practice, and pedagogy of writing--the union between rhetoric and composition seems to mirror the corresponding dual allegiance of theory and praxis.

In addition to his examination of modes of inquiry and methodological communities, North offers an assessment of the current state and the future of the field in his closing chapters on the dynamic nature of inquiry, the "revolution" in composition studies, and the making of knowledge. Perhaps the most central issue that North elucidates concerns composition studies' status as academic inquiry. He notes that the terms, "field," "profession," and "discipline," are used "interchangeably, as if in obedience to some unspoken rule: Characterize Composition as paradigmatic or dialogical, coherent or chaotic as you like, but it is to everyone's advantage to treat it as a legitimate academic discipline" (North, Knowledge 363-64). North asserts:

> No doubt Composition can safely be termed a field, broadly defined as being concerned with the ways in which writing is done, taught, and learned. It may be, too, that those whose academic interests lie within the loose boundaries of this field can be said to form a profession. . . .
>
> But to consider Composition a discipline is to ask a good deal more; in effect, it demands that the term be reduced to a meaning little different from that of the already loosely construed "field." Training in a discipline ordinarily implies preparation for *doing* something, and in Composition, that something has been and in practice largely remains teaching writing. However, the modern version of the field is founded, really, on the subversion of that practical tradition. (North, Knowledge 364)

Further, North is at pains to suggest that as a field or discipline, he sees Composition "fragmenting," and he asserts that it, "as a knowledge-making society is gradually pulling itself apart" (Knowledge 364). He argues: "Methodological sympathies cut across the boundaries of the field, whereas methodological differences--disagreements over how knowledge is made, what knowledge can be--can create insurmountable barriers" (North, Knowledge 365). North foresees such sympathies and differences disrupting Composition as an autonomous academic entity, its parts splitting off and merging with other fields with compatible methodologies.

The picture North paints for what he terms "Composition" is bleak indeed, unless radical change takes place. The alternative to the enfeeblement or demise of Composition that North seems to propose hinges on two radical actions necessary to create a separate discipline of Composition. First, North argues for cutting Composition's ties and breaking out of its "constricting relationship with literary studies, either by taking a larger share of responsibility for knowledge-making in English departments, or by moving outside of such departments altogether." Second, he declares that "it must find some way to establish an internal, inter-methodological peace . . . so that the methodological pluralism that was responsible for creating Composition in the first place can remain its vital core" (North, Knowledge 369). To make peace requires "heightened methodological consciousness" and understanding of the various modes of inquiry practiced by other communities within composition, "a spirit of methodological egalitarianism. . . . where all methods, and all kinds of knowledge, would have to be assumed to be created equal," and third, "the re-establishment of Practice as inquiry" (North, Knowledge 370-71).

North concludes with "pleading the cause of a Composition either fully partners with, or separate from, literary studies." He "would prefer the partnership," but he fears the dark future that he paints. Ultimately, North declares: "sad as it may be, I would rather

take my chances on a fully vital Composition that fails than to settle for one that is never quite free to try"; besides, "anything can happen" (Knowledge 374-75).

North's portrait of an emerging field provides a fresh look at composition studies in the last half of the eighties. Though he denies its existence as a discipline, he admits to the political efficacy of calling it one. But his vision of Composition with a capital C is disturbing. He worries about the fragmentation among various communities. In his opinion, the best hope for the field to coalesce into a discipline most likely rests in its separation from its traditional roots in English, severing its ties with literary studies, yet preserving its plurality of scholarly and research methodology.

At the same time, North's division of composition studies into a series of communities also highlights the overwhelming influence of interdisciplinary borrowing. His communities are divided along methodological lines with many of those methodologies growing out of work in other disciplines. North is cognizant of the many contributions to the field that have arisen out of borrowing ideas and theories as well. His warning about fragmentation is accompanied by the possibility of Composition's various parts deserting and merging with other disciplines, disciplines that share similar methodological commitments.

In her 1988 Composition as a Human Science: Contributions to the Self-Understanding of a Discipline, Louise Wetherbee Phelps expresses a view of composition radically different than that of North. Phelps argues for "a new conception of composition as a discipline linked to many sciences and humanities rather than merely a branch of literary studies; and further defines its link to the broader cultural themes of rhetoric, writing, praxis, and context" ("Jacket"). Like North, she calls for a version of composition that is distinct from literary studies, but unlike North's bleak prediction of fragmentation, Phelps seems to find comfort in composition's diversity in a postmodern world "marked by themes of loss, illusion, instability, marginality, decentering, and

finitude" (5). Phelps sees an "intellectual universe" challenged by postmodern "radical critiques" that cast the shadow of doubt on earlier value systems.

It is precisely the postmodern perspective from which Phelps views composition studies that allows her to weave a fabric of composition within the context of its ecology--its relationships--with other disciplines such as cognitive psychology, philosophy, linguistics, rhetoric, and education as well as with the larger socio-cultural scene. She asserts: "Composition comes to maturity [as a field or discipline] at just the moment when discourse (especially writing) and its interpretation stand at the epicenter of a great change, a fundamental crisis in human consciousness some regard as one of the revolutionary transitions that transform history" (Phelps, Human Science 3). The crisis and change that Phelps sees grow out of her view of the postmodern world. In Phelps's view, our intellectual universe has become "inaccessible to common sense" with the discrediting of the "mechanistic-formalist paradigm" (Human Science 4). In its stead, she poses a human science or psychology of composition based upon behavioral and cognitive investigation into how human beings write.

Characterizing composition "as a human science within a philosophical anthropology," Phelps argues that "composition has no intrinsic methodology, but draws on approaches from across the range of disciplines falling within the natural, critical, and hermeneutical sciences." As such, it is a discipline among disciplines. To her characterization, Phelps adds "praxis as a source of knowledge, both the praxes of composition itself (discourse, teaching, inquiry) and analogous arts of performance or creation (acting, sports, music, building)." While composition favors hermeneutical and dramatistic methods to "make events and processes of literacy and reflection intelligible to their participants," Phelps notes that other methods play a role in "constraining such descriptions and interpretations" (Human Science 77).

Within her framework, Phelps contends that "composition is able to reconstruct its relationship with literature, reinterpret its sometimes parasitical relationships with other disciplines, and participate in complex networks of inquiry" (Human Science 77). She describes two kinds of relationships with other disciplines, noting that her framework also contains criteria for borrowing:

> First, there are the relationships with other specific disciplines which develop knowledge that can contribute to or benefit from the project of composition. Examples are developmental psychology, linguistics, speech studies, education, and literature. The three principles and regions that give composition its structure provide criteria for selecting and using the work of other fields. Composition tends to be a synthesizing force in relation to these disciplines, incorporating their work as information and as alternate perspectives on its own topics and issues. . . . In other words, composition is interested in the practical significance and validity of insights that have their own context of meaning and justification in another discipline. In addition, composition may contribute insights to such disciplines as it develops theories that explicate general properties of discourse and thought (for example, insights bearing on a philosophy of science) or specific processes and structures of discourse. (Phelps, Human Science 77)

The significance of the above passage for borrowing across disciplines is immense. Phelps charts a *raison d'être* for interdisciplinary borrowing and simultaneously provides criteria for making selections from other disciplines. She suggests specific relationships with some disciplines, acknowledging their potential to inform inquiry and praxis in composition as well as the potential of composition studies to add to knowledge

in those disciplines. Within Phelps's view of the first kind of relationship with other disciplines, composition acts as a great synthesizer and integrator of knowledge.

Phelps's second kind of relationship focuses on cross-disciplines or the "relationally constituted genres or foci of inquiry defined by the intersection of interests among separate disciplines." She is quick to point out that "the distinction between disciplines and cross-disciplines is breaking down because new disciplines tend to be interdisciplinary in nature, as in composition, while old disciplines become more and more open systems." Phelps identifies the cross-discipline of rhetoric and the emergent cross-discipline of literacy studies as most important to composition because of their overlap in knowledge and inquiry. But composition, often considered by the others as subordinate to them in Phelps's opinion, brings a uniqueness to those cross-disciplines. It integrates "overlapping topics and concerns in fresh abstractive principles that create a distinctive domain of inquiry"; further, composition coincides with other cross-disciplines that do not overlap with one another such as cognitive science and rhetoric (Human Science 77-78).

Phelps's cross-disciplinary view suggests the borrowing of ideas from disciplines closely related to composition studies rests in a similarity of interests. Cross-disciplines--as foci of inquiry--bring similar interests to bear on the phenomena they study. They may use different methods, yet orient on similar goals. As such, their interests coincide in ways that promote borrowing across disciplinary boundaries.

By framing composition studies in terms of its relationships with other fields, Phelps builds a powerful case for interdisciplinary borrowing. In addition, she attempts to define criteria for the selection and application of borrowings from other disciplines, suggesting the nature of the relationships between inquiry, teaching, and personal literacy. By arguing that composition has no innate methodology of its own, Phelps opens the door to the plurality of methodological commitments that North sees in his own

delineation of composition into a number of discrete communities. Similar interests do not equate to similar methodologies. Like North, Phelps offers a view of composition as informed by inquiry in numerous disciplines outside its boundaries. Phelps acknowledges that those borders remain indistinct and permeable, especially in regard to the relationship of composition studies with cross-disciplines that perform similar inquiry into human practices, activities, and education.

The Nineties: The Topography of Disciplinary Response

> The major concern of most composition specialists is teaching writing well. To do this work effectively, they also attempt to answer broader questions about literacy by studying composing from historical, social, psychological, political, and academic perspectives, often borrowing useful concepts and methods from other disciplines.[10]
>
> --Erika Lindemann and Gary Tate, 1991

North's bleak prediction for the future of composition studies clashes with Phelps's position of composition as a human science. Key to both positions is the issue of methodological plurality--the peaceful coexistence of potentially competing research methodologies and modes of inquiry. These discussions continue in the nineties, coloring attempts to unite composition into a comprehensive discipline.

Richard Fulkerson's 1990 "Composition Theory in the Eighties: Axiological Consensus and Paradigmatic Diversity," proposes "a metatheory about the components necessary to a 'Theory of Composition.'" Fulkerson argues that "Composition Studies has moved toward homogeneity of purpose within diversity of method." Fulkerson's proposed

"disciplinary paradigm" of "a full 'theory' of composition" consists of four points. First, the paradigm of a full theory "necessarily includes a commitment about what constitutes good writing--not necessarily a simplistic one, but some analysis of what we want student writers to achieve as a result of effective teaching" (Fulkerson, "Axiological" 410). Next, such a theory "also necessarily involves a conception of how writers go about creating texts, and perhaps a conception of how they should go about it." Third, the theory "must include some perspective about classroom procedures and curriculum designs suitable for enabling students to achieve the sort of writing one values, a *pedagogical* component." Finally, the theory must involve an epistemology: "our perceptions of how texts are created and of what classroom methods are effective depend upon assumptions about what counts for knowledge." Fulkerson notes that "teaching writing implicitly involves teaching epistemology," and that "North has shown In The Making of Knowledge in Composition" that "our discipline scarcely shares a common epistemology" ("Axiological" 411).

To support his proposal, Fulkerson revisits composition studies since an earlier 1979 article in which he classified four philosophies of composition--expressivism, formalism, mimeticism, and rhetorical. He asserts that three of the four philosophies or axiologies he originally noted--expressivism, formalism, and mimeticism--are in decline, while the star of the rhetorical axiology continues to rise. According to Fulkerson, "Composition Studies now shows the emergence of a significant consensus: the widely-held position today is . . . rhetorical" even though broad "disparities . . . continue to exist about process, pedagogy, and epistemology" ("Axiological" 411).

Fulkerson goes on to note an apparent paradox in his position between his contention that composition studies is more axiologically unified and yet still fragmented. He suggests that the explanation lies in the independence of the four elements of his full

theory paradigm. As Fulkerson writes: "Shared axiology requires neither shared pedagogy nor shared epistemology" ("Axiological" 420).

What is the significance of Fulkerson's view of development in composition studies? He suggests a movement philosophically towards greater unity within the field. He does not ignore the apparent fragmentation and diversity of theory; he simply attributes it to differences in the choices compositionists make as to the best way of teaching composition. Fulkerson sees diversity in terms of different pathways toward the same goal. In addition, he notes the growing concern for the rhetorical process as evidenced by the greater attention in textbooks to issues connected with audience and discourse communities. He offers "a disciplinary paradigm" for a "full theory of composition," suggesting its fourfold nature and pointing out that composition studies has not arrived there yet. We have greater unity as to a sense of direction, but we still follow diverse paths to that goal. Finally, Fulkerson argues that the trend toward rhetorical axiology reflects only a change in value positions. His version of composition studies moves the field from a concern for product, attacked as part of current-traditional rhetoric, to a concern for the purpose, function, and audience of a discourse.

Inherent in Fulkerson's argument is the plurality of methodological choices and disciplinary alliances mentioned by earlier compositionists such as North and Phelps. Fulkerson's talk about "greater unity" is tinged by following "diverse paths" to that goal. Among other things, the choices involve diversity in writing process theory, axiology, pedagogy, and epistemology. Fulkerson notes: "Shared axiology requires neither shared pedagogy nor shared epistemology" ("Axiological" 420). Further, such choices involve commitments to divergent views and relationships with other disciplines such as cognitive psychology. While Fulkerson only speaks of borrowings indirectly, he leaves their potential crossing unhindered at the fuzzy borderline of composition studies.

An Introduction to Composition Studies, edited by Erika Lindemann and Gary Tate, opens with the words, "this book introduces readers to the relatively new academic discipline called, variously, rhetoric and composition, rhetoric, composition, and composition studies." Lindemann and Tate offer nine essays by significant figures in the field, including Andrea A. Lunsford, John T. Gage, and Robert J. Connors. The essayists address a series of fundamental questions about the nature of composition studies, the relationship between rhetoric and composition, the relationship between theory and practice, the recent history of the discipline, the conduct of research, and the role of institutional politics. On a deeper level, the contributors subtly discuss the role of interdisciplinary borrowing in the formation of composition studies. Lindemann and Tate write that their work is for two groups of readers--those who are unfamiliar with the new discipline's "assumptions, history, bibliographical resources, methods of research, and professional activities" and those familiar with composition studies who "are nevertheless uncertain that it deserves the status of a separate discipline." Thus, the editors mean for their work to function both as an introduction to and an argument for an independent academic discipline of composition studies, a discipline distinguished "from other disciplines . . . by its central concern with an *activity*, the act of writing" (Lindemann and Tate [v]).

While they claim disciplinary distinction for composition studies on one hand, Lindemann and Tate reaffirm the field as an "emerging discipline" on the other ([v]). In such a discipline, compositionists encounter problems that force them to seek and acquire "new ways of thinking about their field, wrestle with terminology, develop better methods of teaching and research, create adequate bibliographical and research sources, and build support for their work among people who may be hostile, sympathetic, or indifferent to it" (Lindemann and Tate [v]). Addressing these needs rests

in "interdisciplinary, collaborative efforts to understand how people learn to write" (Lindemann and Tate vi).

The Lindemann and Tate collection begins with the voice of Andrea A. Lunsford describing the nature of the field in her essay, "The Nature of Composition Studies." Lunsford notes that she wishes "to understand, as contextually as possible . . . where our field comes from, what its typical motives and gestures are; and, perhaps the most important, how its present moment might best be described" (Lunsford 3). Following a brief historical review, Lunsford turns to the "challenges and dangers" facing composition studies in the nineties. First among these is the changing notion of knowledge as composed or constructed rather than as "reflections or givens that are discovered ready made" (Lunsford 8). She notes the "growing consensus among most disciplines" for a "view of reality as a series of composed texts and of composing or constructing as the acts of bringing those realities to consciousness" (Lunsford 9). She argues:

> As a field, we traditionally ask questions about texts, readers, writers, and contexts--and about the dynamic relation among them through which multiplicitous meanings or realities are constructed. Thus composition studies views composing not as a series of discrete skills or a package of processes to be practiced but as the very way we constitute and know our worlds. As such, composition studies stands as a discipline of central importance in a post-modern era. (Lunsford 9)

The changing, dynamic view of the nature of knowledge has significant implications for composition studies, especially in terms of border crossings and borrowings. Lunsford continues:

> Such a position leads composition studies to look well beyond its own borders and challenge divisions between disciplines, between genres, and between media. Thus a scholar of composition may draw on anthropology, linguistics, psychology, philosophy, literary theory, neurobiology, or other disciplines in studying the creation and dissemination of written texts. (Lunsford 9)

The course of drawing upon other fields necessarily results in a blurring of disciplinary distinctions. Accordingly, Lunsford declares: "the questions asked by scholars in composition studies demand a constant pushing against disciplinary barriers, a constant invitation to other fields to add insights and help build satisfactory answers to our questions" (9).

Lunsford's characterization of composition studies--a field focused "on the constructed or composed quality of all experience, of all texts; the pressing against disciplinary, genre, and media boundaries; the move beyond the academy to other forums in the private and public sphere"--represents an invitation to center on "literacy or literacies as the business composition scholars must be about" (10). She couples the study of literacy with composition studies and argues that scholars in composition studies have carved out "an intellectual space" within which questions about literacy and composition may be explored. Lunsford sees "this space, this 'nature' of composition studies, as large and loosely bounded, informed by cross-disciplinary, trans-institutional, multiply mediated, multi-genred, multi-voiced, and radically democratic principles" (11).

Essays by compositionists such as Lunsford and others in the Lindemann and Tate collection reinforce the notion of composition studies as a field or discipline characterized by loose boundaries and metadisciplinary interests. Lunsford ties these interests to inquiry into literacy in a democratic society. She sees composition studies as

informed by and actively seeking knowledge from other disciplines. Her theme echoes throughout the rest of An Introduction to Composition Studies, finding expression in statements such as Lillian Bridwell-Bowles's contention that composition studies is "an interdisciplinary or cross-disciplinary field" (98). Descriptions and contentions like these show the ambiguity of the borders of composition studies and highlight the importance of borrowings as a source of knowledge.

Patricia Bizzell's 1992 collection, Academic Discourse and Critical Consciousness, reflects one scholar's efforts to come to terms with borrowing from other disciplines and to suggest the validity of those ideas in composition studies. Bizzell tackles the issue of borrowing in several of the essays in her collection, examining ideas from other disciplines such as cultural anthropology, the history and philosophy of science, cognitive psychology, education, and literacy studies. Her essays address borrowings of Thomas Kuhn (history and philosophy of science), Paulo Freire (liberation theology), and William Perry (education) among others. Her essays reflect the intimacy of the relationships composition studies maintains with other disciplines.

In her 1979 "Thomas Kuhn, Scientism, and English Studies," the second essay in her collection, Bizzell exploits composition's relationship with the history and philosophy of science. She argues for using Kuhn to facilitate a shift in English studies towards a new paradigm "that allows us to examine the ways in which language sharpens and directs critical analysis of the historical situation in which we and our students and our society find ourselves" ("Scientism" 50). Bizzell contends against blanket acceptance of Kuhn's scientism in the form of an overdependence on empirical evidence. She sees other borrowers of Kuhn, notably Maxine Hairston, as using him to promote particular research agendas that threaten to lead composition inquiry astray. The essay provides a thought-provoking look at some of the debates that interdisciplinary borrowing often evokes. Bizzell responds to what she sees as an inappropriate interpretation of a source

outside of composition studies. In turn, she asserts that the borrowing can help clarify directions in composition research, just not in the way that Hairston and Hairston's supporters use Kuhn.

The essays in Academic Discourse illustrate the debt that Bizzell owes as a composition scholar to sources outside of composition studies. Her development as a scholar reflects in part her ability to utilize knowledge constructed in other fields and apply it to composition theory and praxis. It further displays the importance of reading outside the discipline, browsing for ideas with potential application to composition studies. The permeability or ambiguity of the borders of composition studies allows one to shop in other disciplines, especially those disciplines that inquire into the nature of the phenomena in which composition studies seems most interested--writing theory, practice, and instruction.

Nowhere does the sense of interdisciplinary borrowing shine through more than in two closely related collections of interviews, Gary A. Olson and Irene Gale's 1991 (Inter)views: Cross-Disciplinary Perspectives on Rhetoric and Literacy and Olson's 1994 Philosophy, Rhetoric, Literary Criticism: (Inter)views. In these two works, the editors reprint interviews with leading scholars from disciplines outside of composition studies whose work has informed the field in some way. Originally appearing in the Journal of Advanced Composition, the interviews are offered in Perspectives and Philosophy along with specific responses from leading compositionists.

In the preface to their 1991 Perspectives, Olson and Gale note that the interviews and responses "constitute a dialogue about language, rhetoric, literacy, and other topics relevant to composition theory." They go on to assert: "Because rhetoric and composition is an especially interdisciplinary field, much of its scholarship depends on the work of scholars from other disciplines. The careful application of these scholars' work is essential" (Olson and Gale [ix]).

Perspectives contains interviews of and responses to Mary Field Belenky (psychology), Noam Chomsky (linguistics), Jacques Derrida (deconstructionism and literary theory), Paulo Freire (education), Clifford Geertz (anthropology), Richard Rorty (philosophy), and Gayatri Chakravorty Spivak (cultural criticism). Olson's 1994 Philosophy adds interviews primarily with literary scholars or philosophers such as Donald Davidson (analytic philosophy), Stanley Fish (literary criticism), bell hooks (feminism and cultural criticism), J. Hillis Miller (literary criticism), Jane Tompkins (feminist pedagogy), and Stephen Toulmin (logic and philosophy). Responders to the various borrowings include the many leading names in composition studies such as James Berlin, Patricia Bizzell, Kenneth A. Bruffee, Lisa Ede, Lester Faigley, Andrea Lunsford, John Schilb, James Sledd, and others. Both collections attempt to put the use of borrowings from outside of composition studies into perspective.

The 1991 and 1994 collections offer a dual view of inquiry in composition studies. The range of scholars interviewed reflects the multi-disciplinary nature of composition inquiry. At the same time, the responses of leading compositionists suggest many of the debates that grow out of interdisciplinary borrowings and the validity of applying such borrowings to composition studies. For example, Kenneth A. Bruffee and John Schilb disagree in their responses to Olson's interview with Richard Rorty that appears in Perspectives. Bruffee, a leading advocate of social construction and borrower of Rorty's ideas, attacks the interview as a "disservice" to both himself and Rorty (236). He contends that: "The foundational terms in which the JAC interviewers addressed Rorty, if they're representative, reveal, dismayingly, more about the stage that the conversation in composition studies is currently at than his replies [Rorty's] reveal about either him or me" (Bruffee 237). Schilb, on the other hand, argues that the Rorty interview "suggests the problems in our field's new obsession with 'social construction'" and accuses Bruffee of misleading compositionists "when he intimates Rorty and he think

alike" (Schilb, "Rorty" 238). Both scholars stake out their positions in opposition, partially highlighting the ongoing debate within composition studies over social construction.

Debates over borrowings such as that illustrated by the differences between Bruffee and Schilb range from charges of misinterpretation and misapplication to outright rejection or obsession. Such debates often involve political and institutional forces as well as the theoretical and the pedagogical. Someone borrows from another discipline for a reason; others often have just as valid grounds, at least in their own minds, for supporting, altering, resisting, or rejecting that borrowing.

In the sixties and seventies, composition studies struggles to emerge out of the shadow of literary studies as a field of scholarly inquiry into writing and the teaching of writing. The concern for practice and pedagogy remains at the profession's heart even as composition studies expands its venue from the investigation of products to inquiry into process, theory, and cognition. The pain of weaving a new field of academic inquiry intimately concerned with praxis out of the backing that helped build another discipline, English, shows in the effort to legitimize, perhaps even name the new field, composition studies. The debates over its grounding, its constitution as a field or discipline, and the interdisciplinary nature of its province of inquiry take on paramount importance. During the turbulent days of the sixties and seventies, proponents of composition studies fight to establish an academically respectable field, interested in particular kinds of inquiry, and intimately connected to pedagogy. Simultaneously, they build upon composition's relationship with other disciplines, noting places where inquiry coincides, suggesting arenas where other disciplines may or do influence composition theory and practice, and building a case for interdisciplinary borrowing.

Similarly, efforts by composition scholars in the eighties and nineties, especially attempts to chart the profession's history and trace its beginnings, reinforce the importance of interdisciplinary relationships. Browsing in other fields often yields ideas with potential application to our understanding of writing and teaching writing. Borrowing those ideas provokes debate over their application and serves to stimulate professional inquiry. When we argue about our status as a field, a discipline, or a science, we inevitably make comparisons to other branches of knowledge-making; if we are "like" some more established, academically recognized profession, we can make legitimate claims for our own independent existence within the academy. The perceived ambiguity of our profession's borders thus serves to promote interdisciplinary borrowing as a function of discipline building.

What do these views of various scholars tell us about interdisciplinarity in composition studies today? First, they indicate the extent to which composition studies has become accepted as a separate, valid field of scholarly inquiry and research. They suggest that the profession owes its development in part to the flexibility and permeability of its borders. The broad concern for written composition and its teaching unites and limits the field, but acts simultaneously to open it to the influence of borrowings from other disciplines. Murky borders allow for a wide range of speculation and borrowing from other disciplines in the name of pursuing inquiry into writing theory, practice, and pedagogy.

Finally, these views reflect the extent to which composition studies is indebted to interdisciplinary borrowing. The evolution of composition studies as a separate field or discipline of academic inquiry owes its efficacy in part to a dependence on borrowings. Other disciplines have much to offer informed inquiry into composition theory and practice. Compositionists, characterized by a diversity of commitments and interests, often mine other fields for wisdom that might apply in or inform our own. We are

unabashed about our borrowing or "stealing" of ideas from other fields and making those ideas our own. As John Schilb notes, "Our particular field has largely developed by selectively raiding others" ("Rorty" 238). Many of the leading voices in composition studies even encourage such borrowing, claiming it as a strong suit of a new discipline.

CHAPTER THREE

THOMAS KUHN AND HIS THEORY

> [T]he practitioners of a mature scientific specialty are deeply committed to some one paradigm-based way of regarding and investigating nature. Their paradigm tells them about the sorts of entities with which the universe is populated and about the way the members of that population behave; in addition, it informs them of the questions that may legitimately be asked about nature and of the techniques that can properly be used in the search for answers to them.[11]
>
> --Thomas Kuhn, 1963

In composition studies, much of our knowledge of Kuhn's ideas comes from Structure and the various interpretations of it made by a composition scholars. Few in composition studies have explored the nature of Kuhn's theory in detail. Often we have borrowed him without examining the evolution of his theory or its reception in the disciplines of history and philosophy of science. To fully understand the extent of Kuhn's influence in composition studies, we need a better idea of Kuhnian theory in its original setting; thus Chapter Three follows the pattern of traditional descriptive scholarship as I attempt to set Kuhn's theory within the context of its evolution and articulation. I focus specifically upon the theory's development and its articulation in Kuhn's early essays as well as Structure.

Many of Kuhn's ideas appear in their first form in papers presented at the 1959 Third University of Utah Research Conference on the Identification of Creative Scientific Talent and at the 1961 Oxford, England symposium, The Structure of Scientific Change. In Utah, Kuhn uses "paradigm" in "The Essential Tension: Tradition and Innovation in

Scientific Research" and partially articulates one of the key ideas that form the basis of his 1962 work. At Oxford, Kuhn premieres many of the ideas later to appear in The Structure of Scientific Revolutions, remarking in a footnote to "The Function of Dogma in Scientific Research" that: "The ideas presented in this paper have been abstracted, in a drastically condensed form, from the first third of my forthcoming monograph, The Structure of Scientific Revolutions " ("Dogma" 347). The two papers, taken with the preface to the 1962 edition of Structure, provide an early narrative of the development of Kuhn's ideas, ideas more fully articulated in Structure that provoke wide ranging application and debate in fields far beyond the history and philosophy of science. In the 1962 preface, Kuhn provides his audience with a brief history of the personal inquiry leading up to the publication of his theory. Together, the three works set the context for examining the major elements of Kuhn's theory as it appears in Structure. They further suggest the evolution of Kuhn's theory and trace some of its framing within the context of Kuhn's personal development as a scientist, historian, and philosopher.

In the first half of Chapter Three, I focus on the history provided by Kuhn in the prefaces of the 1962 edition of Structure and his 1977 anthology, The Essential Tension, as well as look at his "Essential Tension" and "Dogma" papers for evidence of the evolution in Kuhn's thought. Examining these prefaces and papers is necessary to any investigation of his influence. These elements, along with the first edition of Structure, provide critical data for comprehending Kuhn's influence in composition studies. To really talk about that influence, we need some sense of the evolution of his ideas, a more detailed look at his theory, and a knowledge of the responses to Kuhn in his own disciplines to give us a better understanding of what we have appropriated by borrowing Kuhn.

In last half of Chapter Three, I concentrate on Kuhn's theory as he articulates it in the 1962 edition of Structure. There, Kuhn offers his fullest explication of his theory of

paradigms, shifts, and scientific revolutions. The 1970 edition of Kuhn's work--covered in Chapter Four--varies little from the first edition with the exception of an additional chapter composed to answer some of Kuhn's most vociferous critics in the disciplines of history and philosophy of science.

Kuhn's Theory: Beginnings

> The great prestige of the natural sciences and, to a lesser extent, that of the older social sciences has repeatedly influenced scholars in the humanities to pattern their research on scientific models.[12]
>
> --Michael Holzman, 1983

To examine the development of Kuhn's theory, it helps to turn to the prefaces of two of his works, his 1962 The Structure of Scientific Revolutions and his 1977 The Essential Tension: Selected Studies in Scientific Tradition and Change. In the preface to The Essential Tension, Kuhn describes the 1947 epiphany that led to his recognition that science advances by change of state, rather than by incremental evolution, the usual image of scientific development. Tension, a collection of Kuhn's essays, sheds additional light on the maturation of Kuhn's ideas. Using the two prefaces as guides, we can follow much of the development of Kuhn's theory on the structure of scientific revolutions.

In the preface to Structure, Kuhn divides the interval leading up to the 1962 publication of The Structure of Scientific Revolutions into three periods. The earliest period of development centers around Kuhn's three year term as a Junior Fellow of the Society of Fellows of Harvard University. Kuhn devoted part of what he refers to as his "transition to a new field of study" to investigating the "history of science proper." He

credits the writings of Alexandre Koyré, Emile Meyerson, Hélène Metzger, and Annelise Maer with showing "what it was like to think scientifically in a period when the canons of scientific thought were very different from those current [in 1962]." Kuhn admits that "their works, together with A. O. Lovejoy's Great Chain of Being, have been second only to primary source materials in shaping my conception of what the history of scientific ideas can be" (Structure vii-viii).

Kuhn goes on to point out that Ludwik Fleck anticipated many of Kuhn's ideas in a 1935 monograph, Entstehung und Entwicklung einer wissenschaftlichen Tatsache [Genesis and Development of Scientific Fact]. In his 1976 foreword to the 1979 English translation of Fleck's Genesis and Development, Kuhn records that Fleck's text helped him to realize the "fundamentally sociological dimension" of the problems Kuhn goes on to discuss in Structure (Foreword viii). Kuhn asserts that, based upon his reading of Fleck's work and upon a remark from another Fellow, Francis X. Sutton, he realized that his ideas "might require to be set in the sociology of the scientific community" (Structure ix). Indeed, the imbrication of community and the history of scientific advance would play a key role in the earliest articulations of his theory and its subsequent publication in Structure.

Writing about the early period in the preface to Tension, Kuhn acknowledges that, "While discovering history, I had discovered my first scientific revolution" (Kuhn, Tension xiii). Kuhn suggests that the discovery grew out of his 1947 research into the origins of seventeenth century mechanics, during which he investigated Aristotle's Physica and works descended from it. Aristotelian ideas about mechanics, though limited, had been necessarily rejected out of hand by Galileo and his followers, an example Kuhn later uses in Structure. Kuhn claims that during his investigation he recognized that "Position itself was . . . a quality in Aristotle's physics, and a body that changed its position therefore remained the same body only in the problematic sense that the child is

the individual that it becomes. In a universe where qualities were primary, motion was necessarily a change-of-state rather than a state" (Kuhn, Tension xi-xii).

Kuhn writes that his reading of Aristotle and Aristotle's successors "seemed therefore to disclose . . . a global sort of change in the way men viewed nature and applied language to it, one that could not properly be described as constituted by additions to knowledge or by the mere piecemeal correction of mistakes" (Tension xiii). A scientific revolution occurred with the Galilean rejection of Aristotelian notions and their replacement. Kuhn's recognition of this event served to structure much of his research into scientific development in subsequent years.

According to the preface to Structure, teaching the history of science dominated the second division of Kuhn's theory building. Between 1952 and 1958, Kuhn presented a number of papers and published a number of writings dealing with "the integral part played by one or another metaphysic in creative scientific research" and "the way in which the experimental bases of a new theory are accumulated and assimilated by men committed to an incompatible older theory" (Structure ix). Works such as Kuhn's 1957 The Copernican Revolution: Planetary Astronomy in the Development of Western Thought focus on factors influencing the development or "'emergence' of a new theory or discovery" (Structure ix).

The Copernican Revolution contains perhaps his earliest definition of the term "paradigm." The book's preface, dated November 1956, refers to "paradigm" in the sense of "the technical facts and theories . . . placed in a historical or philosophical framework where they illuminate the way in which science develops, the nature of science's authority, and the manner in which science affects human life" (Kuhn, Copernican Revolution, ix). Kuhn admits that he did not fully recognize the implications of his concept until the third period leading up to the publication of his 1962 essay.

Kuhn seems to assign greatest importance to the third period--the 1958-1959 academic year he spent at the Center for Advanced Studies in Behavioral Sciences. As a member of a community "comprised predominantly of social scientists," Kuhn confronted "the difference between such communities and those of the natural scientists among whom [he] had been trained" (Structure x). Moving away from an idea of consensus within a community, his observations and inquiry led to his recognition of "the role in scientific research of . . . 'paradigms' . . . universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners" (Kuhn, Structure x).

Kuhn's 1962 Structure grew out of that inquiry and as a response to a solicitation by the editors of the Encyclopedia of Unified Science. He finished the first full draft of Structure between the summer of 1959 and the end of 1960. During that period, paradigms "took on a life of their own, largely displacing the previous talk about consensus" and growing from "exemplary problem solutions" to "the entire global set of commitments shared by members of a particular scientific community" (Kuhn, Tension xix). At the same time, Kuhn tried out some of those ideas in his "Essential Tension" and "Dogma" papers, soliciting feedback from other scholars in his disciplines.

"The Essential Tension," 1959

> [T]he various fields of natural science have not always been characterized by rigid education in exclusive paradigms, but . . . each of them acquired something like that technique at precisely the point when the field made rapid and systematic process.[13]
>
> --Thomas Kuhn, 1959

The Third (1959) University of Utah Research Conference on the Identification of Scientific Talent provided Kuhn with a forum to introduce his notion of paradigm and to suggest its influence on scientific advance. Kuhn's paper appeared in the conference proceedings published in the same year. In "The Essential Tension: Tradition and Innovation in Scientific Research," Kuhn offers a foretaste of what he later develops into a more comprehensive theory in Structure.

"The Essential Tension" opens with Kuhn's declaration that he comes to the conference as "an ex-physicist now working in the history of science." He addresses one aspect of the "image of the scientific process and of the scientist" implicit in many of the working papers distributed to conference participants--the aspect of the scientist as an open-minded, non-prejudicial, divergent thinker (Kuhn, "Essential" 225).

While admitting that "some divergence characterizes all scientific work, and gigantic divergences lie at the core of the most significant episodes in scientific development," Kuhn speculates "whether flexibility and open-mindedness have not been too exclusively emphasized as the characteristics requisite for basic research." He posits that "something like 'convergent thinking' is just as essential to scientific advance as is divergent"; conflict between the two modes "inevitably" arises, and "the ability to support a tension" between the two modes "is one of the prime requisites for the very best of scientific research" (Kuhn, "Essential" 226).

Kuhn grants that the "flexibility and open-mindedness that characterize, or indeed define, the divergent thinker" are necessary elements of scientific revolutions ("Essential" 227), which he defines as the "episodes . . . in which a scientific community abandons one time-honored way of regarding the world and of pursuing science in favor of some other, usually incompatible, approach to its discipline" ("Essential" 226). As evidence of the role of such divergent thinking, Kuhn refers to

another essay he is working on, the essay that later becomes The Structure of Scientific Revolutions. In that essay, Kuhn notes that he argues:

> Contrary to a prevalent impression, most new discoveries and theories in the sciences are not merely additions to the existing stockpile of scientific knowledge. To assimilate them the scientist must usually rearrange the intellectual and manipulative equipment he has previously relied upon, discarding some elements of his prior belief and practice while finding new significances in and new relationships between many others. Because the old must be revalued and reordered when assimilating the new, discovery and invention in the sciences are usually intrinsically revolutionary. (Kuhn, "Essential" 226)

Kuhn suggests that the intrinsic nature of revolutions does require divergent thinking, concluding that unless "many scientists possessed them (flexibility and open-mindedness), there would be no scientific revolutions and very little scientific advance" ("Essential" 227).

Kuhn's conclusion comes with a caveat; he asserts that "flexibility is not enough, and what remains is not obviously compatible." Again drawing upon his draft of Structure, Kuhn emphasizes that "revolutions are but one of two complementary aspects of scientific advance." He argues: "Almost none of the research undertaken by even the greatest scientists is designed to be revolutionary, and very little of it has any such effect." Kuhn classifies such non-revolutionary activity as "normal research," which he characterizes as "a highly convergent activity based firmly upon a settled consensus acquired from scientific education and reinforced by subsequent life in the profession" (Kuhn, "Essential" 227).

Kuhn continues:

> Typically . . . this convergent or consensus-bound research ultimately results in revolution. Then, traditional techniques are abandoned and replaced by new ones. But such revolutionary shifts of a scientific tradition are relatively rare, and extended periods of convergent research are the necessary preliminary to them . . . . [O]nly investigations firmly rooted in the contemporary scientific tradition are likely to break that tradition and give rise to a new one. (Kuhn, "Essential" 227)

Kuhn focuses on this "essential tension" between convergence and divergence; while a scientist must "undertake a complex set of intellectual and manipulative commitments," his success may finally rest upon "his ability to abandon this net of commitments in favor of another" ("Essential" 227).

Turning to the nature of scientific education, Kuhn further argues that "a rigorous training in convergent thought has been intrinsic to the sciences almost from their origin" ("Essential" 228). Kuhn suggests that "the various fields of natural science have not always been characterized by rigid education in exclusive paradigms, but that each of them acquired something like that technique at precisely the point when the field made rapid and systematic progress." As evidence, he uses the example of the properties of light outlined in physics textbooks. Kuhn briefly examines the development of three successive traditions derived historically from Newton's views on optics "by way of two revolutions in optical thought, each of which replaced one tradition of convergent research with another." Kuhn writes: "If we make appropriate allowances for changes in the locus and materials of scientific education, we may say that each of these three traditions was embodied in the sort of education by exposure to unequivocal paradigms" ("Essential" 230).

Looking back in 1977 in the preface to his collection of essays in Tension, Kuhn suggests that the importance of "The Essential Tension" lies in the introduction of the concept of paradigms. Having conceived normal science "as the result of a consensus among the members of a scientific community," Kuhn admits that he had difficulty writing a chapter on normal science in the essay and in his early draft of Structure because "Apparently, the consensus I [Kuhn] had been seeking did not exist" (Tension xviii-xix). He writes that he recognized in 1959 that "no consensus . . . was required" (Tension xix), but he continues to use "consensus" rather than "paradigm" throughout "The Essential Tension" essay. The expansion of his concept of paradigms is reflected in the changes that occur in his theory between the 1959 publication of "The Essential Tension," the 1961 presentation of "The Function of Dogma in Scientific Research," and the 1962 publication of Structure.

Continuing his exemplification of the history of the theories of light, Kuhn charts the existence of what he terms a "preconsensus" phase in the development of optics ("Preconsensus" will be replaced by "pre-paradigmatic" in Structure). He assesses the development of competing schools of theory in the absence of consensus. In his physical optics example, Kuhn claims:

> From remote antiquity until the end of the seventeenth century there was no single set of *paradigms* [my emphasis] for the study of physical optics. . . . As a result, a new man entering the field was inevitably exposed to a variety of conflicting viewpoints; he was forced to examine evidence from each, and there was always good evidence. The fact that he had made a choice . . . could not prevent his awareness of other possibilities. (Kuhn, "Essential" 231)

In tracing "the rise of competing schools," Kuhn calls attention to the idea that the "preconsensus" phase (or "divergent" phase as he also refers to it) is "duplicated in the history of all other scientific specialties, excepting only those that were born by the subdivision and recombination of pre-existing disciplines" ("Essential" 231). He notes that fields such as mathematics, astronomy, dynamics, geometric optics, and parts of physiology acquired "the paradigms that produced a first consensus" well before the Renaissance. He then argues: "Most other natural sciences, though their problems were often discussed in antiquity, did not achieve first consensus until after the Renaissance" (Kuhn, "Essential" 232).

Kuhn then raises issues connected with normal research conducted under the auspices of paradigms. He classifies three sorts of standard problems: first, the attempt "to adjust existing theory or existing observation in order to bring the two into closer and closer agreement"; second, "the extension of existing theory to areas that it is expected to cover but in which it has never before been tried"; and third, the collection of "concrete data . . . required for the application and extension of existing theory." According to Kuhn, the pursuit of these problems "is neither intended nor likely to produce fundamental discoveries or revolutionary changes in scientific theory" (Kuhn, "Essential" 233). Instead, Kuhn posits that researchers are much more likely to aim at the elucidation of "the scientific tradition in which [they were] raised rather than to change it" ("Essential" 234). Kuhn concludes: "Under normal conditions the research scientist is not an innovator, but a solver of puzzles, and the puzzles upon which he concentrates are just those which he believes can be both stated and solved within the existing scientific tradition" (Kuhn, "Essential" 234).

In equating research to puzzle-solving within a set tradition, Kuhn argues that "the ultimate effect of this tradition-bound work has invariably been to change the tradition" (Kuhn, "Essential" 234). He declares: "Again and again the continuing attempt to

elucidate a currently received tradition has at last produced one of those shifts in fundamental theory, in problem field, and in scientific standards to which [Kuhn] previously referred to as scientific revolutions" ("Essential" 234).

In making the claim for basic research under a paradigm, Kuhn avers that, "in the mature sciences [those operating with a consensus or unique paradigm] the prelude to discovery and to all novel theory is not ignorance, but the recognition that something has gone wrong with existing knowledge and beliefs" ("Essential" 235). Commitment and adherence to existing theory provides the motivation to solve the problems of normal research and gives meaning to the search for solutions; such commitment also provides that backdrop for identification of significant anomaly. Kuhn asserts that "the recognition and evaluation of" anomaly as significant depends "upon a firm commitment to the contemporary scientific tradition" ("Essential" 236).

In Kuhn's "The Essential Tension," we see the introduction of such key concepts as paradigm, revolution, normal research, and shifts, all connected with a conception of the basic scientist as a puzzle-solver working within a specific tradition. As a foretaste of what Kuhn eventually develops in Structure, "The Essential Tension: Tradition and Innovation in Scientific Research" provides an excellent early view of Kuhn's theory and prepares the ground for his additional scholarship. The essay helps set the development of his theory into the historical context.

"The Function of Dogma," 1961 (1963)

> Strongly held convictions that are prior to research often seem to be a precondition for success in the sciences.[14]
>
> --Thomas Kuhn, 1961 (1963)

Kuhn's paper for the 1961 Oxford symposium on the structure of scientific change appears in A. C. Crombie's Scientific Change (1963), accompanied by a number of responses. In a footnote, Kuhn writes that his paper is an abstract of the first third of Structure. Kuhn addresses the nature and function of dogma within scientific tradition and forecasts in condensed form much of what later appears in Structure before the publication of his symposium paper.

In his opening paragraphs, Kuhn asserts that, "To be scientific is, among other things, to be objective and open-minded" ("Dogma" 347), at least in appearance. He goes on to argue that individual scientists are often not very open-minded even though the scientific enterprise may be. Kuhn suggests:

> Whether his work is predominantly theoretical or experimental, he [the scientist] usually seems to know, before his research project is even well under way, all but the most intimate details of the result that project will achieve. If the result is quickly forthcoming, well and good. If not, he will struggle with his apparatus and with his equations until, if at all possible, they yield results which conform to the sort of pattern which he has foreseen from the start. (Kuhn, "Dogma" 348)

These convictions extend to the work of others within the same field of scientific endeavor. Kuhn declares that, "unexpected novelties of fact and theory have characteristically been resisted and have often been rejected by many of the most creative members of the professional scientific community." He observes that such preconception and resistance seem "the rule rather than the exception in mature scientific development" and "they characterize the very best and most creative research as well as the more routine" ("Dogma" 348). Such convictions represent "community

characteristics with deep roots in the procedures through which scientists are trained for work in their profession." According to Kuhn: "Strongly held convictions that are prior to research often seem to be a precondition for success in the sciences." Kuhn labels these characteristics collectively as "the dogmatism of mature science" and advances a number of views about their importance and influence over scientific change ("Dogma" 349).

Turning to a theme that shows up again and again throughout his work on scientific revolutions and advancement, Kuhn reminds his audience that "Scientific education inculcates what the scientific community had previously with difficulty gained--a deep commitment to a particular way of viewing the world and of practicing science in it"--in the words of Structure, a paradigm. He continues: "That commitment can be, and from time to time is, replaced by another, but it cannot be merely given up" (Kuhn, "Dogma" 349).

Kuhn argues that the commitment to a particular view of the world has two fundamental influences on research. First, the commitment defines "for the individual scientist both the problems available for pursuit and the nature of acceptable solutions." It constitutes the normal science or puzzle-solving most scientists engage in and details the rules of the game in which they play. Second, the commitment supplies the researcher with "an immensely sensitive detector" for identifying anomalies, unexpected fact and fundamental innovations of theory that function as "responses to a prior breakdown in the rules of the previously established game." Kuhn avers that "no mature science could exist" without the resistance or dogma of commitment, especially as it shows up in scientific education ("Dogma" 349). Kuhn's main point rests in his argument that, "scientific education remains a relatively dogmatic initiation into a pre-established problem-solving tradition that the student is neither invited nor equipped to

evaluate" despite the ability of scientific development to produce significant novelty ("Dogma" 351).

In his discussion of textbooks, Kuhn acknowledges that the educational system they promote is relatively new; it is a product of the early nineteenth century for most sciences with the possible exception of elementary mathematics. He suggests that in the absence of textbooks, "universally received paradigms for the practice of individual sciences" often supplied the elements of scientific education. Kuhn defines paradigms here as the "scientific achievements reported in books that all the practitioners of a given field knew intimately and admired, achievements upon which they modeled their own research and which provided them with a measure of their own accomplishment." He cites classics such as Aristotle's Physica, Newton's Principia, and Franklin's Electricity and works modeled after them as providing for their respective fields with "much of what textbooks now do" (Kuhn, "Dogma" 352).

In citing scientific classics by Aristotle, Newton, Franklin, and others, Kuhn asserts that they serve as paradigms, suggesting that the works possess "something which sets them apart both from some other classics of science and from all the classics of other creative fields." Kuhn sets about identifying those characteristics of paradigms that differentiate these classics from others. First, he notes the "exclusiveness" or incompatibility of one paradigm from another. At any time, a group of practitioners, "if it has a paradigm at all, can have only one" according to Kuhn ("Dogma" 352). By this assertion, Kuhn means that the scientific community chooses between competing models; committing itself to one means simultaneously rejecting the other candidates. Second, Kuhn argues that in choosing a paradigm, "the scientific community commits itself, consciously or not, to the view that the fundamental problems resolved there have, in fact, been solved once and for all" ("Dogma" 353). Commitment to a paradigm does not bring about any fundamental reinterpretation of nature; instead, it brings a commitment

to taking up where others have left off, moving in the direction of greater precision and articulation.

At this point in his essay, Kuhn elaborates on his definition of paradigm:

> It is . . . a fundamental scientific achievement and one which includes both a theory and some exemplary applications to the results of experiment and observation. More important, it is an open-ended achievement, one which leaves all sorts of research still to be done. And, finally, it is an accepted achievement in the sense that it is received by a group whose members no longer try to rival it or to create alternates for it. Instead, they attempt to extend and exploit it in a variety of ways. . . . (Kuhn, "Dogma" 358)

The pattern of development of a mature science usually consists of shifts from one paradigm to another, with the former "swept aside" and replaced by another "quite incompatible" with it (Kuhn, "Dogma" 358). Kuhn suggests that "Advance from paradigm to paradigm rather than through the continuing competition between recognized classics may be a functional as well as a factual characteristic of mature scientific development" ("Dogma" 359).

Given a paradigm, the primary mission of the scientific community is "to bring it [the paradigm] into closer and closer agreement with nature" (Kuhn, "Dogma" 360). Kuhn labels the research growing out of that effort as normal science. He elaborates on two points about normal scientific research. First, he asserts that the problems investigated during periods of normal research are "paradigm-dependent" (Kuhn, "Dogma" 361). Commitment to the paradigm often provides the motivation to seek solutions. Paradigm-based, normal research focuses on meeting anticipated outcomes. The challenge for practitioners "is not to uncover the unknown but to obtain the known"

(Kuhn, "Dogma" 362); thus practitioners of normal science more closely resemble puzzle-solvers than explorers or inventors.

Kuhn admits that his portrait of scientific research "as puzzle-solving or paradigm-matching must be . . . thoroughly incomplete" because of the frequent discovery of "new and unexpected sorts of phenomena" ("Dogma" 364). Out of this admission arises Kuhn's second major point about paradigm-based research--the practice of normal puzzle-solving science, not always successful, "can and inevitably does lead to the isolation and recognition of anomaly" ("Dogma" 365). Kuhn declares:

> That recognition proves, I think, prerequisite for almost all discoveries of new sorts of phenomena and for all fundamental innovations in scientific theory. After a first paradigm is achieved, a breakdown in the rules of the pre-established game is the usual prelude to significant scientific innovation. (Kuhn, "Dogma" 365)

A paradigm provides the practitioner of normal science with the tools "to discriminate essential anomaly from mere failure" (Kuhn, "Dogma" 365), often paving the way for discovery.

The recognition of significant anomaly prepares the way for new theory as well as discovery. When the paradigm on which normal science is based fails to account for such anomaly, the scientific community meets a "crisis." Kuhn notes that he uses "crisis" elsewhere without really elaborating on it here other than to remark that "at times of crisis, one encounters numerous different versions of the paradigm theory" ("Dogma" 367). Kuhn continues: "New theories arise from work conducted under old ones, and they do so only when something is observed to have gone wrong. Their prelude is widely

recognized anomaly, and that recognition can only come to a group that knows very well what it would mean to have things go right" (Kuhn, "Dogma" 368).

Noting that space limitations prevent him from going further, Kuhn acknowledges that his "case for dogmatism must remain schematic" in his essay ("Dogma" 368). He makes one final comment before concluding with a call for more exploration of the characteristics of scientific education and development:

> Though successful research demands a deep commitment to the *status quo,* innovation remains at the heart of the enterprise. Scientists are *trained* to operate as puzzle-solvers from established rules, but they are also *taught* to regard themselves as explorers and inventors who know no rules except those dictated by nature itself. The result is an acquired tension, partly within the individual and partly within the community, between professional skills on the one hand and professional ideology on the other. Almost certainly that tension and the ability to sustain it are important to science's success. (Kuhn, "Dogma" 369)

Ultimately, Kuhn concludes that "the vitality of science depends upon the continuation of occasional tradition-shattering innovations" (Kuhn, "Dogma" 368), novelties and anomalies that result in changes in rules and directions of research.

Kuhn's Theory: <u>The Structure of Scientific Revolutions</u>, 1962

> The study of paradigms . . . is what mainly prepares the student for membership in the particular scientific community with which he will later practice.[15]
>
> --Thomas Kuhn, 1962

In the 1962 edition of The Structure of Scientific Revolutions, Kuhn advances the theory that scientific development progresses by revolution, not by accretion. He analyzes the history and sociology of science using what he identifies--and defines in a variety of ways--as paradigms. In the thirteen sections of his monograph, Kuhn connects scientific change to the idea of professional communities centered around shared language, interests, and ideologies. Kuhn contends that such shared concerns influence the manner in which members of these communities conduct scientific investigation, acquire, promote and define knowledge, and alter their views of what constitutes the domain of science and scientific research. Ultimately, Kuhn attacks the older notion of scientific change--the idea that knowledge builds incrementally--and adapts a view that science advances by revolutionary progress. To accomplish this end, Kuhn develops a theory of paradigm shifts, presenting a picture of the history of scientific discovery and the changes brought about by significant alterations in how scientists view their world.

Kuhn salts Structure with numerous references to many earlier works in the history of science, including his own. His examples spring from more extensive treatments of individual scientific advances offered in works such as his 1957 The Copernican Revolution and James B. Conant's 1950 The Overthrow of the Phlogiston Theory: The Chemical Revolution of 1775-1789. The examples serve to reinforce Kuhn's theory by showing his ideas in application; the ideas represent a bold departure--a paradigm shift, to use Kuhn's own term--from previous historical treatments of science and discovery to a theory emphasizing the influence of community and tradition over the practice of scientific research.

Before proceeding, it is helpful to examine a few of Kuhn's key definitions from the 1962 edition of Structure: normal and extraordinary science, paradigms, scientific crises, scientific revolutions, and incommensurability. Kuhn draws a distinction

between normal science (or research) and extraordinary science (or research). The former follows the traditional practices accepted by a given scientific community; the latter represents a departure from tradition growing out of the inability of traditional practices to resolve anomalous findings. Kuhn's definition of paradigm is particularly thorny because it involves the guidelines that establish the traditions of normal science. The idea of scientific crisis centers on the creation of the opportunity for paradigm debate and shift, while the term "scientific revolution" ultimately marks the replacement of one paradigm with another. Incommensurability grows out of the different world views held by scientists adhering to competing or different paradigms. These definitions are more fully explored below, using Kuhn's own words.

Extraordinary science. Research undertaken during times of scientific crisis "to isolate" and "more precisely structure" fundamental anomalies which the old paradigm cannot adequately explain or predict. It may include scientists turning "to philosophical analysis as a device for unlocking the riddles of their field." It may result in the generation "of speculative theories that, if successful, may disclose the road to a new paradigm and, if unsuccessful, can be surrendered with relative ease" (Kuhn, Structure 86-87).

Normal science. Kuhn's normal science "means research firmly based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practices" (Structure 10). Normal science "is predicated on the assumption that the scientific community knows what the world is like"; such research also represents "a strenuous and devoted attempt to force nature into the conceptual boxes supplied by professional education" (Kuhn, Structure 5).

Paradigms. 1. These are "accepted examples of actual scientific practice--examples which include law, theory, application, and instrumentation together--[that]

provide models from which spring particular coherent traditions of scientific research"; such models share two characteristics: first, their achievement is "sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity," and second, each model is "sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to resolve" (Kuhn, Structure 10). 2. Kuhn also defines paradigms as the "scientific achievements reported in books that all the practitioners of a given field knew intimately and admired, achievements upon which they modeled their own research and which provided them with a measure of their own accomplishment" (Kuhn, "Dogma" 352).

Scientific crisis. A "breakdown of the normal technical puzzle-solving activity" of normal science that signals the failure of a paradigm to adequately explain or predict anomalies (Kuhn, Structure 69). Kuhn argues: "All crises begin with a blurring of a paradigm and the consequent loosening of the rules for normal research"; further, "all crises close with the emergence of a new candidate for paradigm and with the subsequent battle over its acceptance" (Structure 84).

Scientific revolution. A "new set of commitments, a new basis for the practice of science" growing out of "the extraordinary investigations" of anomalies that "subvert the existing tradition of scientific practice"; they are "the tradition-shattering complements to the tradition-bound activity of normal science" (Kuhn, Structure 6). Kuhn further defines scientific revolutions as "those non-cumulative developmental episodes in which an older paradigm is replaced in whole or in part by an incompatible new one" (Structure 91).

Incommensurability. The notion that scientists who adhere to different paradigms have incompatible "ways of seeing the world and of practicing science in it" (Kuhn, Structure 4). Given a shift in paradigms, the world transforms in such a way within the new paradigm that "old terms, concepts, and experiments fall into new relationships

with one another," creating a misunderstanding between adherents of the old paradigm and supporters of the new one. Kuhn argues that communication across the gap created by the transformation "is inevitably partial," thus "the proponents of competing paradigms practice their trades in different worlds" (Structure 149-50).

History and Normal Science

The first four sections of the 1962 Structure lay the groundwork for building a theory of scientific advance around the notion of paradigms, paradigm shifts, and revolutions. In those sections, Kuhn argues for a closer examination of the historical record, moving beyond the stereotypical image of the scientist and attacking the notion of scientific advance as "development-by-accumulation." He describes the nature of normal, or paradigm-driven science, and concludes that most scientific research consists of puzzle-solving more than undirected inquiry; commitment to a paradigm not only involves the selection of acceptable research questions, it implies the framing of the answers to those questions as well.

I. Introduction: A Role for History

In his first section, "Introduction: A Role for History," Kuhn outlines the role that the historical record of research activity plays in his theory. He begins by contending that the image of science "has previously been drawn, even by scientists themselves, mainly from the study of finished scientific achievements as these are recorded in the classics, and, more recently, in the textbooks from which each new scientific generation learns to practice its trade" (Kuhn, Structure 1). Such a view of history is misleading

according to Kuhn; he asserts that a "quite different concept of science" emerges when we examine "the historical record of the research activity itself" (Kuhn, Structure 1).

From Kuhn's point of view, textbooks present a false image of science and scientists by portraying scientific advance as a piecemeal process. Kuhn suggests that the "development-by-accumulation," piecemeal approach causes problems for historians; the historian of science becomes the chronicler of "successive increments and the obstacles that have inhibited their accumulation" (Kuhn, Structure 2). The first task of such a chronicler centers around determining "what man and at what point in time each contemporary scientific fact, law, and theory was discovered or invented"; the second task concerns the description and explanation of the "congeries of error, myth, and superstition" presenting obstacles to scientific advance (Kuhn, Structure 2).

Doubts about the ability of historians of science to adequately answer the questions raised by the development-by-accumulation approach have resulted in what Kuhn terms as a "historiographic revolution in the study of science, though one that is still in its early stages" at the time of the publication of his monograph (Structure 3). Kuhn argues that work by scholars such as Alexandre Koyré implies "the possibility of a new image of science," one that Kuhn sets out "to delineate . . . by making explicit some of the new historiography's implications" (Structure 3).

In the remaining pages of his introductory section, Kuhn introduces the implications of the new historiography of science and previews the direction he will follow throughout the rest of Structure. Sections II-IV describe the nature of normal science and relate it to puzzle-solving. Sections V-VIII detail paradigms, discovery and recognition of anomaly, and the emergence of crises. The final sections of Structure delve into the framework of scientific revolutions and explore Kuhn's notion of incommensurability.

II. The Route to Normal Science

In the second section of The Structure of Scientific Revolutions, Kuhn defines his concept of "normal science," reiterates the influence of textbooks, and introduces his notion of "paradigms." He begins the section by defining what he means by "normal science" and suggesting the extent of normal science's presence in scientific textbooks. Kuhn writes:

> In this essay, "normal science" means research firmly based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practices. Today such achievements are recounted, though seldom in their original form, by science textbooks, elementary and advanced. These textbooks expound the body of accepted theory, illustrate many or all of its successful applications, and compare these applications with exemplary observations and experiments. (Kuhn, Structure 10)

Before textbooks became widely available due to printing, the classical scientific treatises such as Aristotle's Physica, Newton's Principia and Opticks, and Lavoisier's Chemistry among others performed the same function as the later textbooks. The classics "and many other works served for a time implicitly to define the legitimate problems and methods of a research field for succeeding generations of practitioners" (Kuhn, Structure 10). Kuhn argues that such works could do so because:

> [T]hey shared two essential characteristics. Their achievement was

> sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity. Simultaneously, it was sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to resolve. (Kuhn, Structure 10)

The two essentials that Kuhn mentions perform double duty as characteristics of a second major concept introduced in Section II--the paradigm. Kuhn initially defines "paradigms" as "accepted examples of actual scientific practice--examples which include law, theory, application, and instrumentation together--[that] provide models from which spring particular coherent traditions of scientific research" (Kuhn, Structure 10). He states that:

> The study of paradigms . . . is what mainly prepares the student for membership in the particular scientific community with which he will later practice. Because he there joins men who learned the bases of their field from the same concrete models, his subsequent practice will seldom evoke overt disagreement over fundamentals. Men whose research is based on shared paradigms are committed to the same rules and standards for scientific practice. That commitment and the apparent consensus it produces are prerequisites for normal science, i.e., for the genesis and continuation of a particular research tradition. (Kuhn, Structure 11)

While fusing the concept of paradigm to the conduct of normal science, Kuhn acknowledges that his concept "will often substitute for a variety of familiar notions," many of which he will not explore until later in his book. He further stipulates that some research can take place without paradigms, but asserts that adherence to a

paradigm permits "the more esoteric type of research" that marks the maturity of a scientific field (Kuhn, Structure 11). To illustrate the pattern suggested by paradigm acquisition, Kuhn provides the examples of physical optics and electrical research.

In making his argument, Kuhn sets up the pre-paradigm condition as a distinguishing characteristic of an immature field, linking the condition with "a number of creative fields today" (Kuhn, Structure 13). Before the acquisition of "a universally recognized paradigm," science develops as a series of competing schools, each with its own adherents re-creating anew the foundations of knowledge within the discipline and focusing research in directions that seem promising within the scope of the school. Kuhn notes that "History suggests that the road to a firm research consensus"--a tenet of Kuhn's universally recognized paradigm--"is extraordinarily arduous" (Structure 15).

Kuhn goes on to explain that history suggests why the lack of "a firm research consensus" presents difficulty to field development. He writes:

> In the absence of a paradigm or some candidate for paradigm, all of the facts that could possibly pertain to the development of a given science are likely to seem equally relevant. As a result, early fact-gathering is a far more nearly random activity than the one that subsequent scientific development makes familiar. Furthermore, in the absence of a reason for seeking some particular form of more recondite information, early fact-gathering is usually restricted to the wealth of data that lie ready to hand. (Kuhn, Structure 15)

The ready body of data Kuhn points to includes technological development, which he suggests "has often played a vital role in the emergence of new sciences" (Structure 16).

The emergence of a paradigm, in Kuhn's view, directly "affects the structure of the group that practices the field" (Structure 18). He states: "When, in the development of a

natural science, an individual or group first produces a synthesis able to attract most of the next generation's practitioners, the older schools gradually disappear" (Kuhn, Structure 18). Adherents to older schools convert, become isolated, or choose exile; most convert. Since the "new paradigm implies a new and more rigid definition of the field," Kuhn writes that those "unwilling or unable to accommodate their work to it [the new paradigm] must proceed in isolation or attach themselves to some other group" (Kuhn, Structure 19).

The acquisition of a paradigm has other consequences for the group of practitioners beyond simple allegiance or rejection. Kuhn argues:

> [I]t is sometimes just its reception of a paradigm that transforms a group previously interested merely in the study of nature into a profession or, at least a discipline. In the sciences . . . the formation of specialized journals, the foundation of specialists' societies, and the claim for a special place in the curriculum have usually been associated with a groups' first reception of a single paradigm. (Kuhn, Structure 19)

Kuhn's argument becomes a list of the hallmarks of a discipline. He consciously continues to build the structural framework for his theory by calling attention to the picture of the pre- and post-paradigm state.

Adherence to the new paradigm further benefits its supporters by providing a foundation, a starting place from which to pursue and articulate research without first having to lay the groundwork of everything anew. Kuhn notes:

> When the individual scientist can take a paradigm for granted, he need no longer, in his major works, attempt to build his field anew, starting from first

> principles and justifying the use of each concept introduced. That can be left to the writer of textbooks. Given a textbook, however, the creative scientist can begin his research from where it [the textbook] leaves off and thus concentrate exclusively upon the subtlest and most esoteric aspects of the phenomena that concern his group. (Kuhn, Structure 19-20)

Articulation of research under the paradigm also changes. Instead of works addressed to anyone interested in the field, research increasingly appears "as brief articles addressed only to professional colleagues, the men whose knowledge of a shared paradigm can be assumed and who prove to be the only ones able to read the papers addressed to them." Kuhn's emphasis here lies on the shared paradigm; readers of research articles in the field must possess knowledge of the paradigm to properly interpret what they read. Kuhn cites a number of fields--mathematics, electrical research, and "most other fields of physical science" among them--that have "ceased . . . to be intelligible to a generally educated audience" (Kuhn, Structure 20). As a field acquires a paradigm--a specialized fount of accepted knowledge and a shared view of reality--it grows increasingly opaque to those not initiated in the paradigm. The paradigm functions as a guide to the whole group's research and the articulation of its findings.

For Kuhn, the acquisition of a paradigm is a distinguishing attribute of the maturation of a field. "Except with the advantage of hindsight," Kuhn declares, "it is hard to find another criterion that so clearly proclaims a field a science" (Structure 22).

III. The Nature of Normal Science

Next, Kuhn turns to an expanded discussion of the nature of normal science. He writes: "Paradigms gain their status because they are more successful than their

competitors in solving a few problems that the group of practitioners has come to recognize as acute." But Kuhn's notion of paradigm depends not so much on success as on "a promise of success discoverable in selected and still incomplete examples" (Kuhn, Structure 23). According to Kuhn:

> Normal science consists in the actualization of that promise, an actualization achieved by extending the knowledge of those facts that the paradigm displays as particularly revealing, by increasing the extent of the match between those facts and the paradigm's predictions, and by further articulation of the paradigm itself. (Kuhn, Structure 24)

Restated by Kuhn in a different way, "normal-scientific research is directed to the articulation of those phenomena and theories that the paradigm already supplies" (Structure 24).

To more fully illustrate normal or paradigm-driven research, Kuhn attempts to classify the problems at the heart of normal science. He begins with an examination of what he sees as the "three normal foci for factual scientific investigation," though these foci "are neither always nor permanently distinct." The first class consists "of facts that the paradigm has shown to be particularly revealing of the nature of things." Kuhn suggests that by "employing them in solving problems, the paradigm has made them worth determining both with more precision and in a larger variety of situations" (Structure 25). The second, but smaller class consists of "those facts that, though often without much intrinsic interest, can be compared directly with predictions from the paradigm theory"; Kuhn notes that improving the degree of agreement between paradigm and nature or "finding new areas in which agreement can be demonstrated at all presents a constant challenge to the skill and imagination of the experimentalist and observer"

(Structure 26). Finally, Kuhn's third class consists "of empirical work undertaken to articulate paradigm theory, resolving some of its residual ambiguities and permitting the solution of problems to which it had previously only drawn attention" (Structure 27).

Turning to the theoretical problems of normal science, Kuhn suggests that they fall into "nearly the same classes as the experimental and observational." First, a small part of the theoretical work of normal science "consists simply in the use of existing theory to predict factual information of intrinsic value" or the manipulation "of theory undertaken . . . because [it] can be confronted directly with experiment." Kuhn argues that: "The need for work of this sort arises from the immense difficulties often encountered in developing points of contact between a theory and nature" (Structure 30).

A second class of theoretical problems arises when we shift to paradigm articulation and the periods "when scientific development is predominantly qualitative." Kuhn argues that some problems "aim simply at clarification through reformulation" (Structure 32). For example, Kuhn notes that "many of Europe's most brilliant mathematical physicists repeatedly endeavored to reformulate Newton's theory [as expressed in his Principia] in an equivalent but logically and aesthetically more satisfying form." According to Kuhn, "Similar reformations of a paradigm have occurred repeatedly in all of the sciences, but most of them have produced more substantial changes in the paradigm than the reformations of the Principia" (Structure 33).

"More than any other sort of normal research," Kuhn asserts that "the problems of paradigm articulation are simultaneously theoretical and experimental." To paraphrase one of Kuhn's examples, the scientists who design the experiments to distinguish between various theories are generally the same ones who make up the versions being compared; they work both with fact and with theory. Their work produces "not simply new

information but a more precise paradigm, obtained by the elimination of ambiguities that the original from which they worked had retained." Kuhn concludes that "In many sciences, most normal work is of this sort" (Structure 33).

Kuhn wraps up his extended discussion of normal science by reiterating the problems such research addresses. He suggests that the "three classes of problems--determination of significant fact, matching of facts with theory, and articulation of theory--exhaust . . . the literature of normal science, both empirical and theoretical" (Kuhn, Structure 33), but do not quite exhaust the entire body of scientific literature. Some extraordinary problems occur that "emerge only on special occasions prepared by the advance of normal research" (Kuhn, Structure 34). Kuhn insists: "Inevitably . . . the overwhelming majority of the problems undertaken by even the very best scientists usually fall into one of the three categories," thus research "under the paradigm can be conducted in no other way, and to desert the paradigm is to cease practicing the science it defines" (Structure 34). Such desertions occur and play a pivotal role in scientific revolutions.

IV. Normal Science as Puzzle-solving

The practitioner of normal science aims at a known outcome, not *"unexpected* novelty." Normal science does not "aim to produce major novelties, conceptual or phenomenal" (Kuhn, Structure 35). Instead, scientists engage in normal research because the results gained "add to the scope and precision with which the paradigm can be applied" (Kuhn, Structure 36). Kuhn goes on to endow the practitioner of normal research with the sobriquet of "puzzle-solver." A puzzle-solving scientist anticipates the outcome of his work; the shared paradigm of the community suggests that outcome.

Thus the challenge of normal science rests in discovering the way to achieve that outcome:

> Bringing a normal research problem to a conclusion is achieving the anticipated in a new way, and it requires the solution of all sorts of complex instrumental, conceptual, and mathematical puzzles. The man who succeeds proves himself an expert puzzle-solver, and the challenge of the puzzle is an important part of what usually drives him on. (Kuhn, Structure 36)

The direct comparison between the research that characterizes normal science and the tests of "ingenuity or skill" that characterize finding solutions to a jigsaw or crossword puzzle is important to Kuhn, and he pursues it with some vigor throughout the section.

Before moving to his next section, Kuhn qualifies the relationship between puzzle-solving and normal science. He admits that this illumination "may be significantly misleading," and notes that while normal science "is a highly determined activity . . . it need not be entirely driven by rules." He writes: "Though there obviously are rules to which all practitioners of a scientific specialty strictly adhere at a given time, those rules may not by themselves specify all that the practice of those specialists has in common." Kuhn introduces the concept of shared paradigms "as the source of coherence for normal research traditions" for this reason. "Rules," he suggests, "derive from paradigms, but paradigms can guide research even in the absence of rules" (Kuhn, Structure 42).

Paradigm, Discovery, and Theory

Sections V through VIII of Structure detail Kuhn's concept of paradigms, tying that concept to the recognition of anomaly and the emergence of scientific discoveries. Kuhn suggests that recognition of anomaly is a prelude to the emergence of new theory. Such recognition leads to a crisis for normal science that may result in a radical reconstruction of a discipline. A breakdown in normal scientific activity and the emergence of competing explanations for anomaly may result in the overthrow of fundamental elements of a discipline or, as Kuhn labels it, a scientific revolution.

V. The Priority of Paradigms

Kuhn opens Section V with the argument that "the paradigms of a mature scientific community can be determined with relative ease." He suggests that a community reveals its paradigms "in its textbooks, lectures, and laboratory exercises," noting that: "By studying them [textbooks, et al] and practicing with them, the members of the corresponding community learn their trade" (Kuhn, Structure 43). Thus, historians can isolate shared paradigms in both community teaching and practice by analyzing the artifacts of that teaching and practice.

Kuhn warns: "The determination of shared paradigms is not, however, the determination of shared rules" (Kuhn, Structure 43). Determining rules requires comparing "the community's paradigms with each other and with its current research reports. . . . to discover what isolable elements, explicit or implicit, the members of the community may have *abstracted* from their more global paradigms and deployed as rules in their research" (Kuhn, Structure 43). Kuhn acknowledges the greater difficulty posed by the search for rules. "Nevertheless," he admits, "if the coherence of a research

tradition is to be understood in terms of rules, some specification of common ground in the corresponding area is needed" (Kuhn, Structure 44).

In the search for common ground, Kuhn suggests that scientists can "agree in their *identification* of a paradigm without agreeing on, or even attempting to produce, a full *interpretation* or *rationalization* of it." He declares that the lack "of a standard interpretation or of an agreed reduction to rules will not prevent a paradigm from guiding research." Further, Kuhn concludes that the determination of normal science depends more upon "the direct inspection of paradigms" than upon rules; he contends, "the existence of a paradigm need not even imply that any full set of rules exists" (Structure 44).

Kuhn's point, "paradigms *could* determine normal science without the intervention of discoverable rules" (Structure 46), receives further clarification. He recounts the difficulty of discovering rules that guide normal science, and then dissects the nature of scientific education a second time, noting the influence such education exerts on future practitioners. Kuhn asserts that normal science "can proceed without rules only so long as the relevant scientific community accepts without question the particular problem-solutions already achieved" (Structure 47). He argues:

> The pre-paradigm period, in particular, is marked by frequent and deep debates over legitimate methods, problems, and standards of solution, though these serve rather to define schools than to produce agreement. . . . debates like these do not vanish once and for all with the appearance of a paradigm. Though almost non-existent during periods of normal science, they recur regularly just before and during scientific revolutions, the periods when paradigms are first under attack and then subject to change. . . . When scientists disagree about whether the fundamental problems of their field have been solved, the search for rules gains a

function that it does not ordinarily possess. While paradigms remain secure, however, they can function without agreement over rationalization or without any attempted rationalization at all. (Kuhn, Structure 48-49)

VI. Anomaly and the Emergence of Scientific Discoveries

To make his theory about science revolutions work, Kuhn must account for the novelties that develop despite the paradigms which guide normal science. He opens Section VI with a declaration raising the issue of novelty: "Normal science does not aim at novelties of fact or theory and, when successful, finds none. New and unsuspected phenomena are, however, repeatedly uncovered by scientific research, and radical new theories have again and again been invented by scientists" (Kuhn, Structure 52).

How does normal science cope with novelty? Kuhn suggests that "research under a paradigm must be a particularly effective way of inducing paradigm change." According to Kuhn, assimilating novelty "[produced] inadvertently by a game played under one set of rules . . . requires the elaboration of another set" (Structure 52). Assimilation of novelty brings a corresponding change to the field.

As proof of his claim about assimilation, Kuhn offers an analysis of three important scientific discoveries--the discovery of oxygen, the discovery of X-rays, and the invention of the Leyden jar--asserting that the discoveries "are not isolated events but extended episodes with a greatly recurrent structure" (Structure 52). The process of assimilation begins with discovery, developing an awareness that an anomaly exists. Kuhn argues that discovery commences "with the recognition that nature has somehow violated the paradigm-induced expectations that govern normal science" (Structure 53). He contends:

> It then continues with a more or less extended exploration of the area of anomaly. And it closes only when the paradigm theory has been adjusted so that the anomalous has become the expected. Assimilating a new sort of fact demands a more than additive adjustment of theory, and until that adjustment is completed --until the scientist has learned to see nature in a different way--the new fact is not quite a scientific fact at all. (Kuhn, Structure 53)

Kuhn asserts that the three examples he uses share common characteristics evident "of all discoveries from which new sorts of phenomena emerge." He identifies those characteristics as: "the previous awareness of anomaly, the gradual and simultaneous emergence of both observational and conceptual recognition, and the consequent change of paradigm categories and procedures often accompanied by resistance." Citing the work of J. S. Bruner and Leo Postman, Kuhn suggests that evidence exists "that these same characteristics are built into the nature of the perceptual process itself" (Structure 62).

In summarizing Section VI, Kuhn returns to the issue of professionalization that he raises in Section V. He writes:

> In the development of any science, the first received paradigm is usually felt to account quite successfully for most of the observations and experiments easily accessible to that science's practitioners. Further development . . . ordinarily calls for the construction of elaborate equipment, the development of an esoteric vocabulary and skills, and a refinement of concepts that increasingly lessens their resemblance to their usual common-sense prototypes. That professionalization leads, on the one hand, to an immense restriction of the scientist's vision and to a considerable resistance to paradigm change. The science

> has become increasingly rigid. On the other hand, within those areas to which the paradigm directs the attention of the group, normal science leads to a detail of information and to a precision of the observation-theory match that could be achieved in no other way. (Kuhn, Structure 64-65)

Increasing professionalization brings rigidity in paradigm-influenced fields because of the practitioners' focus on the puzzles provided by the pursuit of normal science and the alteration of the process of education that brings new practitioners into a field.

Against the backdrop of increasing professionalization, Kuhn argues that "novelty ordinarily emerges only for the man who, knowing *with precision* what he should expect, is able to recognize that something has gone wrong." Kuhn notes: "Anomaly appears only against a background provided by the paradigm. The more precise and far-reaching that paradigm is, the more sensitive an indicator it provides of anomaly and hence of an occasion for paradigm change." Resistance to novelty and paradigm change has a further role to play. Kuhn explains: "By ensuring that a paradigm will not be too easily surrendered, resistance guarantees that scientists will not be lightly distracted and that the anomalies that lead to paradigm change will penetrate existing knowledge to the core" (Structure 65).

VII. Crisis and the Emergence of Scientific Theories

Kuhn specifically addresses issues connected to paradigm change or shift in Section VII. The assimilation of discovery and the resulting paradigm shift have destructive as well as constructive qualities, occurring as a result of "discarding some previously held standard beliefs or procedures and, simultaneously, by replacing those components of the previous paradigm with others." But shifts in beliefs are prodded by more than just

the assimilation of discovery; Kuhn argues that "destructive-constructive paradigm changes" also result from the invention of new theories (Structure 66).

Kuhn suggests that a profound awareness of anomaly is as much a prerequisite of acceptable changes in theory as it is in the emergence of new sorts of phenomena (Structure 67). He claims:

> Because it demands large-scale paradigm destruction and major shifts in problems and techniques of normal science, the emergence of new theories is generally preceded by a pronounced professional insecurity. . . . that insecurity is generated by the persistent failure of the puzzles of normal science to come out as they should. Failure of the existing rules is the prelude to a search for new ones. (Kuhn, Structure 67-68)

The failure of the existing tradition, a "breakdown of the normal technical puzzle-solving activity" (Kuhn, Structure 69), lies at the core of a scientific crisis or paradigm breakdown. During breakdown, the paradigm flounders upon its increasing inability to explain or predict novelty. While Kuhn does acknowledge the importance of external factors, such as political pressure, changes in philosophy, and other historical elements, as an influence on the timing of a breakdown within a mature science, he chooses not to examine those factors in Structure, suggesting that such an examination rests outside the boundaries of his present monologue.

The breakdown of normal science's ability to predict and explain signals a period of investigation into alternatives that give rise to the emergence of novel theory. Kuhn notes, "Philosophers of science have repeatedly demonstrated that more than one theoretical construction can always be placed upon a given collection of data." At the same time, historians of science have shown that, "it is not even very difficult to invent"

alternatives, especially "during the early developmental stages of a new paradigm" (Kuhn, Structure 76). Kuhn argues that:

> [I]nvention of alternates is just what scientists seldom undertake except during the pre-paradigm stage of their science's development and at very special occasions during its subsequent evolution. 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. (Kuhn, Structure 76)

The emphasis on "the tools a paradigm supplies" allows Kuhn to conclude Section VII with the use of a manufacturing metaphor to describe scientists' reluctance to change. So long as the paradigm provides capable tools, Kuhn suggests that scientists are reticent about the "extravagance" of "retooling," reserving it "for the occasion that demands it." Crises supply ample opportunity and justification for the cost of retooling (Kuhn, Structure 76).

VIII. The Response to Crisis

How do scientists respond to crises? Kuhn delivers a multipart answer. First, he argues that practitioners remain reluctant to "renounce the paradigm that has led them into crisis" even as they consider alternatives. Next, he asserts that "once it has achieved the status of paradigm, a scientific theory is declared invalid only if an alternate candidate is available to take its place." Finally, Kuhn notes that, "The decision to reject one paradigm is always simultaneously the decision to accept another, and the

judgment leading to that decision involves comparison of both paradigms with nature *and* with each other" (Kuhn, Structure 77).

In his elucidation of the scientist's replacement of one paradigm with another, Kuhn argues that counterinstances or anomalies "can at best help to create a crisis or, more accurately, to reinforce one that is already very much in existence"; they cannot invalidate a theory by themselves. When confronting anomaly or novelty, scientists often "will devise numerous articulations and *ad hoc* modifications of their theory in order to eliminate apparent conflict." To affect change, anomalies and counterinstances must function "to permit the emergence of a new and different analysis of science within which they are no longer a source of trouble" (Kuhn, Structure 78).

While a paradigm defines the puzzles that constitute the scope of normal science, "no paradigm . . . ever completely resolves all its problems" (Kuhn, Structure 79). Instead, Kuhn theorizes that "crisis loosens the rules of normal puzzle-solving in ways that ultimately permit a new paradigm to emerge" (Structure 80). In the process, some anomalies grow in importance. "When . . . an anomaly comes to seem more than just another puzzle of normal science," Kuhn notes, "the transition to crisis and to extraordinary science has begun" (Structure 82). Such anomalies receive increasing attention and general recognition in their field. The more the anomaly resists explication within the paradigm, the more likely it is to generate partial or alternate solutions that diverge from the existing paradigm.

Kuhn identifies two universal effects of crisis on the conduct of normal science. First, the onset of crisis brings a corresponding "blurring" of the paradigm and a "loosening of the rules of normal science," thus "research during periods of crisis very much resembles research during the pre-paradigm period, except that in the former the locus of difference is both smaller and more clearly defined." Second, Kuhn concludes that "all crises close with the emergence of a new candidate for paradigm and with the

subsequent battle over its acceptance" (Structure 84). The traditions of normal science must alter as their governing paradigm looses its ability to accurately account for or explain anomaly and counterinstance.

Since normal science in Kuhn's view functions to cumulatively articulate or extend the existing paradigm, a crisis in that paradigm forces the "reconstruction of the field from new fundamentals, a reconstruction that changes some of the field's most elementary theoretical generalizations as well as many of its paradigm methods and applications." The transition from the old to the new paradigm is characterized by "a decisive difference in the modes of solution" to the puzzles being investigated while the two paradigms somewhat overlap. "When the transition is complete," Kuhn argues, "the profession will have changed its view of the field, its methods, and its goals," establishing a new pattern for what constitutes appropriate research (Structure 84-85).

After characterizing "crisis as an appropriate prelude to the emergence of new theories" (Structure 85), Kuhn explores what he terms, "extraordinary research," the attempt to isolate and structure a fundamental anomaly. Kuhn admits wide variations in time often elapse "between the first consciousness of breakdown and the emergence of a new paradigm" (Structure 86). He describes the scientist's research effort:

> Faced with an admittedly fundamental anomaly in theory, the scientist's first effort will often be to isolate it more precisely and to give it structure. Though now aware that they cannot be right, he will push the rules of normal science harder than ever to see, in the area of difficulty, just where and how far they can be made to work. Simultaneously he will seek for ways of magnifying the breakdown, of making it more striking and perhaps also more suggestive than it

had been when displayed in experiments the outcome of which was thought to be known in advance. (Kuhn, Structure 86)

According to Kuhn, the conduct of extraordinary research during periods of crisis may often have other effects. Though he places such effects beyond the limits of his essay, Kuhn writes about one: "By concentrating scientific attention upon a narrow area of trouble and by preparing the scientific mind to recognize experimental anomalies for what they are, crisis often proliferates new discoveries" (Kuhn, Structure 88).

Kuhn avoids answering the question of the nature of the final stage of invention that seems to characterize the fruition of extraordinary research, but he does offer some observations on the nature of the inventor as he closes Section VIII. "Almost always," Kuhn advances, the inventors of new paradigms "have been either very young or very new to the field whose paradigm they change" (Structure 89-90). While he argues that his evidence is circumstantial and calls for more investigation of his contention, Kuhn makes the point explicit that these inventors are often the ones "who, being little committed by prior practice to the traditional rules of normal science, are particularly likely to see that those rules no longer define a playable game and to conceive another set that can replace them" (Structure 90). The transition to the new paradigm they develop constitutes a scientific revolution.

Kuhn concludes Section VIII by noting that sections VI through VIII "have educed numerous criteria of a breakdown in normal scientific activity, criteria that do not at all depend upon whether breakdown is succeeded by scientific revolution." According to Kuhn, revolutions grow out of the "proliferation of competing articulations, the willingness to try anything, the expression of explicit discontent, [and] the recourse to philosophy and to the debate over fundamentals" (Structure 90).

Scientific Revolutions

In the remainder of Structure, Kuhn develops his concept of scientific revolutions and offers an explanation of their influence on scientific progress. He equates revolutions to changes in world view similar to gestalt switches and introduces the concept of incommensurability. He suggests ways in which revolutions operate to change the direction and focus of normal scientific activity, guiding as well as limiting scientific advance.

IX. The Nature and Necessity of Scientific Revolutions

Kuhn further clarifies his concept of scientific revolution in Section IX. He has already indicated that "scientific revolutions are . . . those non-cumulative developmental episodes in which an older paradigm is replaced in whole or in part by an incompatible new one." In Section IX, he points to the parallelism between political and scientific revolutions in order to justify his characterization of a change in paradigm as a revolution: "In both political and scientific development the sense of malfunction that can lead to crisis is prerequisite to revolution." In politics, it is "a growing sense . . . that existing institutions have ceased to adequately meet the problems posed by an environment they have in part created"; in science, it is "the growing sense . . . that an existing paradigm has ceased to function adequately in the exploration of an aspect of nature to which the paradigm had previously led the way" (Kuhn, Structure 91). In both arenas, the disquiet is often limited to a narrow segment of the community; such revolutions "need seem revolutionary only to those whose paradigms are affected by them" (Structure 91-92).

Kuhn makes a second point about the parallel development of politics and science. He asserts: "Political revolutions aim to change political institutions in ways that those institutions themselves prohibit" (Kuhn, Structure 92), replacing those institutions with others. In a similar manner, the choice between competing paradigms in a scientific field in crisis "proves to be a choice between incompatible modes of community life" (Kuhn, Structure 93). Kuhn argues that because paradigms possess that similarity:

> [T]he choice is not and cannot be determined merely by the evaluative procedures characteristic of normal science, for these depend in part upon a particular paradigm, and that paradigm is at issue. When paradigms enter, as they must, into a debate about paradigm choice, their role is usually circular. Each group uses its own paradigm to argue in that paradigm's defense. (Kuhn, Structure 93)

Kuhn goes on to assert that such circular arguments serve persuasion, but they "cannot be made logically or even probabilistically compelling for those who refuse to step into the circle." Kuhn argues that logic and experiment alone "can never" settle the choice between paradigms because choice involves attaining "the assent of the relevant community" (Structure 93). If the two theories are logically compatible, no difference can exist. Kuhn argues that the process of assimilation requires the displacement of the older theory by the second, resulting in the paradigm destruction that is the hallmark of a scientific revolution (Structure 96).

Kuhn continues his argument for the necessity of scientific revolutions with an attack on "the most prevalent contemporary interpretation of the nature and function of scientific theory" (Structure 97). He declares: "That interpretation, closely associated with early logical positivism and not categorically rejected by its successors, would restrict the range and meaning of an accepted theory so that it could not possibly conflict

with any later theory that made predictions about some of the same natural phenomena" (Kuhn, Structure 97). Instead, in an extended discussion of the transition from Newtonian to Einsteinian mechanics, Kuhn argues for a view that grants "that the differences between successive paradigms are both necessary and irreconcilable" (Structure 102).

In establishing that successive paradigms differ in substance, Kuhn also suggests the alterations that such shifts bring to the nature of a discipline. Paradigms "are the source of the methods, problem-field, and standards of solution accepted by any mature scientific community at any given time." Acceptance of a new paradigm "often necessitates a redefinition of the corresponding science." Adherence to the new paradigm means pursuing the new conventions, directions and problems that the paradigm sets. Normal science emerging from a scientific revolution "is not only incompatible but often actually incommensurable with that which has gone before" (Kuhn, Structure 102). Thus, Kuhn concludes: "Changes in the standards governing permissible problems, concepts, and explanations can transform a science" (Kuhn, Structure 105).

As he moves towards a conclusion of Section IX, Kuhn focuses on the prioritization of problems that characterizes paradigm debate. He notes:

> [T]he choice between competing paradigms regularly raises questions that cannot be resolved by the criteria of normal science. To the extent, as significant as it is incomplete, that two scientific schools disagree about what is a problem and what a solution, they will inevitably talk through each other when debating the relative merits of their respective paradigms. In the partially circular arguments that regularly result, each paradigm will be shown to satisfy more or less the criteria that it dictates for itself and to fall short of a few of those dictated by its opponents. . . . since no paradigm ever solves all the problems it

> defines and since no two paradigms leave all the same problems unsolved, paradigm debates always involve the question: Which problems is it more significant to have solved? (Kuhn, Structure 108-109)

Kuhn argues that the answer to that question is always one of values "answered only in terms of criteria that lie outside of normal science altogether" (Kuhn, Structure 109).

X. Revolutions as Changes of World View

In Section X, Kuhn equates a shift in paradigms to a change in the way scientists view the world of their science. He notes:

> Led by a new paradigm, scientists adopt new instruments and look in new places. . . . during revolutions scientists see new and different things when looking with familiar instruments in places they have looked before. . . . paradigm changes . . . cause scientists to see the world of their research-engagement differently. In so far as their only recourse to that world is through what they see and do, we may want to say that after a revolution scientists are responding to a different world. (Kuhn, Structure 110)

This change in world view is an important element in Kuhn's overall theory of scientific revolution. The acceptance of a new paradigm brings a corresponding reinterpretation of former views suggestive of "a switch in visual gestalt" (Kuhn, Structure 110).

"At times of revolution," Kuhn declares, "when the normal-scientific tradition changes, the scientist's perception of his environment must be re-educated--in some familiar situations he must learn to see a new gestalt" (Structure 111). Just as those

novices seeking entry into a particular scientific community must transform their views to see "what the scientist sees" and respond "as the scientist does" (Kuhn, Structure 110), so must the practitioner alter his perception of the research environment "when the normal-scientific tradition changes" (Structure 111). In seeing a new gestalt in the world of research, the practitioner switches to a world "incommensurable with the one he had inhabited before" (Kuhn, Structure 111).

Kuhn illustrates the change in world view further by citing gestalt experiments showing "the nature of perceptual transformations" and by referring to psychological literature (Structure 111). Kuhn suggests that such material "makes one suspect that something like a paradigm is prerequisite to perception itself." He notes, "What a man sees depends upon what he looks at and also upon what his previous visual-conceptual experience has taught him to see" (Kuhn, Structure 112).

Turning from an extended discussion of the fundamental perception changes accompanying paradigm shifts to matters of interpretation, Kuhn addresses a potential criticism of his claims. He notes that some "will surely want to say that what changes with a paradigm is only the scientist's interpretation of observations that themselves are fixed once and for all by the nature of the environment and of the perceptual apparatus" (Structure 119). Kuhn declares this view to "be neither all wrong nor a mere mistake." Instead, he argues that the view "is an essential part of a philosophical paradigm initiated by Descartes and developed at the same time as Newtonian dynamics." Kuhn adds: "That paradigm has served both sciences and philosophy well," but recent work in philosophy, psychology, linguistics, and other fields "converge to suggest that the traditional paradigm is somewhat askew" (Structure 120).

In addressing what he terms the "traditional epistemological paradigm," Kuhn insists that "What occurs during a scientific revolution is not fully reducible to a reinterpretation of individual and stable data" (Structure 120). He argues:

"Rather than being an interpreter, the scientist who embraces a new paradigm is like the man wearing inverting lenses. Confronting the same constellation of objects as before and knowing that he does so, he nevertheless finds them transformed through and through in many of their details" (Kuhn, Structure 121). Kuhn does not intend for his remarks to be construed that scientists do not interpret; he readily admits that they do. "But," as he notes in examples including Galileo, Aristotle, and Franklin, "each of these interpretations presupposed a paradigm. They were parts of normal science, an enterprise that . . . aims to refine, extend, and articulate a paradigm that is already in existence." Further, "Given a paradigm, interpretation of data is central to the enterprise that explores it" (Kuhn, Structure 121).

According to Kuhn, interpretation "can only articulate a paradigm, not correct it." Normal science, unable to correct a paradigm, "leads only to the recognition of anomalies and crises" which "are terminated, not by deliberation and interpretation, but by a relatively sudden and unstructured event like the gestalt switch" (Kuhn, Structure 121). These switches are born of intuitive leaps incongruent with interpretation. Kuhn states:

> Though such intuitions depend upon the experience, both anomalous and congruent, gained with the old paradigm, they are not logically or piecemeal linked to particular items of experience as an interpretation would be. Instead, they gather up large portions of that experience and transform them to the rather different bundle of experience that will thereafter be linked piecemeal to the new paradigm but not the old. (Kuhn, Structure 122)

Kuhn is not completely willing to relinquish the view that "sensory experience is fixed and neutral" and that "theories [are] simply man-made interpretations of given

data." While giving that view qualified support in the absence of any alternatives, Kuhn declares that it "no longer functions effectively." He claims that attempts to make it function by introducing "a neutral language of observations" seem equally "hopeless" (Kuhn, Structure 125).

Kuhn develops his attack on interpretation and neutral language by insisting that "the operations and measurements that a scientist undertakes in the laboratory are not 'the given' of experience but rather 'the collected with difficulty.'" He asserts that "more clearly than the immediate experience from which they in part derive, operations and measurements are paradigm-determined." They function as "concrete indices to the content of more elementary perceptions, and . . . are selected for the close scrutiny of normal research only because they promise opportunity for the fruitful elaboration of an accepted paradigm." Scientists choose those manipulations "relevant to the juxtaposition of a paradigm with the immediate experience that the paradigm has partially determined." Thus, practitioners of different paradigms "engage in different concrete laboratory manipulations" (Kuhn, Structure 125). In denying the efficacy of a neutral language of observations usable by all practitioners, Kuhn suggests that developing such a language "still depends exclusively upon a theory of perception and of the mind" (Structure 125). So far, attempts to do so have fallen short. "No current attempt to achieve" an actual language of observation, he writes, "has yet come close to a generally applicable language of pure precepts" (Kuhn, Structure 126).

Kuhn suspects that scientists "are right in principle as well as in practice" when they treat objects of perception "as the fundamental ingredients of their immediate experience" (Structure 126-27). According to Kuhn, "Except when all the conceptual and manipulative categories are prepared in advance . . . both scientists and laymen sort out whole areas together from the flux of experience" (Structure 127). Paradigms help to sort and "determine large areas" of that experience. As a result, scientists can only

begin "the search for an operational definition or a pure-observation language" after experiencing (Kuhn, Structure 128).

After a revolution, a scientist "is still looking at the same world," but sees that world differently, dismissing many old measurements and manipulations as "irrelevant" and replacing them with others (Kuhn, Structure 128). Vocabulary and laboratory instruments remain pretty much what they were before the revolution so that "postrevolutionary science invariably includes many of the same manipulations, performed with the same instruments and described in the same terms, as its prerevolutionary predecessor." Kuhn argues that if such enduring manipulations change at all, they do so "either in relation to the paradigm or in their concrete results" (Structure 129).

XI. The Invisibility of Revolutions

Kuhn addresses the closure of scientific revolutions in Section XI. As the source of authority on the outcome of each scientific revolution, he cites "principally textbooks on science together with both the popularizations and the philosophical works modeled on them" (Kuhn, Structure 135). Kuhn argues that these sources "systematically" disguise "the existence and significance of scientific revolutions" (Structure 135). He begins by noting the commonality that textbooks, popularizations, and philosophical works display:

> They address themselves to an already articulated body of problems, data, and theory, most often to the particular set of paradigms to which the scientific community is committed at the time they are written. Textbooks themselves aim to communicate the vocabulary and syntax of a contemporary scientific language. Popularizations attempt to describe these same applications in a language closer

> to that of everyday life. And the philosophy of science, particularly that of the English-speaking world, analyzes the logical structure of the same completed body of scientific knowledge. . . . All three record the stable *outcome* of past revolutions and thus display the bases of the current normal-scientific tradition. (Kuhn, Structure 136)

The dependence upon texts is a recurrent theme for Kuhn. He suggests that "both the layman's and the practitioner's knowledge is based on textbooks and a few other types of literature derived from them." A significant part of his argument about the invisibility of scientific revolutions lies in the need to rewrite such texts "whenever the language, problem-structure, or standards of normal science change." Kuhn states that textbooks, as the "pedagogic vehicles for the perpetuation of normal science," must "be rewritten in the aftermath of each scientific revolution, and, once rewritten, they inevitably disguise not only the role but the very existence of the revolutions that produced them" (Structure 136).

Rewriting texts to account for scientific revolution begins with "truncating the scientist's sense of his discipline's history" and then supplying "a substitute" for what has been eliminated. According to Kuhn, textbooks characteristically "contain just a bit of history" as introduction or in reference "to the great heroes of an earlier age," but usually "refer only to that part of the work of past scientists that can easily be viewed as contributions to the statement and solution of the texts' paradigm problems" (Structure 136-37). Kuhn insists: "Partly by selection and partly by distortion, the scientists of earlier ages are implicitly represented as having worked upon the same set of fixed problems and in accordance with the same set of fixed canons that the most recent revolution in scientific theory and method has made seem scientific." The net effect of

such revision is that "science once again comes to seem largely cumulative" (Kuhn, Structure 137), a notion that Kuhn consistently attacks.

Revision has an additional effect. If, as Kuhn argues, post-revolutionary science texts operate by misconstructing history, those "misconstructions render revolutions invisible" (Structure 139). Kuhn suggests that texts contribute to an "overwhelming" impression that:

> [S]cience has reached its present state by a series of individual discoveries and inventions that, when gathered together, constitute the modern body of technical knowledge. From the beginning of the scientific enterprise, a textbook presentation implies, scientists have striven for the particular objectives that are embodied in today's paradigm. (Kuhn, Structure 139)

Still, Kuhn declares that the building block metaphor that such misconstruction seems to evoke--the idea that "scientists have added another fact, concept, law, or theory to the body of information supplied in the contemporary science text"--does not match "the way a science develops" (Kuhn, Structure 139).

In Kuhn's view of scientific development, "the whole network of fact and theory that the textbook paradigm fits to nature has shifted." The fit of theory to fact occurs by "transforming previously accessible information into facts, that for the preceding paradigm, had not existed at all." According to Kuhn, theories: "do not evolve piecemeal to fit facts that were there all the time. Rather, they emerge together with the facts they fit from a revolutionary reformulation of the preceding scientific tradition, a tradition within which the knowledge-mediated relationship between the scientist and nature was not quite the same" (Kuhn, Structure 140). Textbook presentation, that is "pedagogic form," Kuhn concludes: "More than any other single aspect of science . . . has determined

our image of the nature of science and the role of discovery and invention in its advance" (Structure 142).

XII. The Resolution of Revolutions

At this point in his essay, Kuhn backtracks to discuss the paradigm replacement process. Section XII takes up the issue of testing competing paradigms in times of crisis and revolution. Kuhn repeats his theory that a practitioner engaged in normal science "is a solver of puzzles, not a tester of paradigms" (Structure 143). Kuhn argues that "paradigm-testing occurs only after the persistent failure to solve a noteworthy puzzle gives rise to crisis" and "only after the sense of crisis has evoked an alternative candidate for paradigm." For Kuhn, "testing occurs as part of the competition between two rival paradigms for the allegiance of the scientific community" (Structure 144).

The testing or verification of paradigms unexpectedly parallels the contemporary philosophical theories of probabilistic verification and of Karl R. Popper's approach of falsification according to Kuhn. The former grows out of the effort to test the probability of a theory against existing evidence. However, Kuhn suggests that "probabilistic theories disguise the verification situation as much as they illuminate it." Given his earlier argument advanced in Section X against a neutral scientific language, Kuhn finds that in any comparison of theories and of widespread evidence, "the theories and observations at issue are always closely related to ones already in existence." Using probabilistic verification, paradigm testing echoes "natural selection: it picks out the most viable among the actual alternatives in a particular historical situation" (Kuhn, Structure 145).

Unlike probabilistic verification, Popper's approach "emphasizes the importance of falsification." According to Kuhn, Popper "denies the existence of any verification

procedures at all." Instead, Popper focuses on "the test that, because its outcome is negative, necessitates a rejection of an established theory." In disagreement, Kuhn cites his own ideas concerning the anomalous experience as fulfilling the role Popper assigns to falsification; anomalous experiences, "by evoking crisis, prepare the way for a new theory." To Kuhn, "anomalous experiences may not be identified with falsifying ones," and Kuhn even doubts the existence of the latter (Structure 145).

Kuhn attacks both probabilistic verification and falsification for trying "to compress two largely separate processes into one" (Structure 146). He asserts:

> Popper's anomalous experience . . . evokes competitors for an existing paradigm. But 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. Furthermore, it is in that joint verification-falsification process that the probabilist's comparison of theories plays a central role. (Kuhn, Structure 146)

Kuhn asserts that seeing the process in two stages may enable one to "begin explicating the role of agreement (or disagreement) between fact and theory in the verification process." The question becomes one of "which of two actual and competing theories fits the facts *better*" (Structure 146).

Kuhn continues his argument about the process of establishing a new paradigm when he returns to the issue of incommensurability raised earlier in Structure. The most fundamental reason for incommensurability rests in the idea that practitioners of competing paradigms operate in different worlds. Kuhn argues:

> Practicing in different worlds, the two groups of scientists see different things when they look at the same point from the same direction. Again, that is not to say that they can see anything they please. Both are looking at the world, and what they look at has not changed. But in some areas they see different things, and they do see them in different relations to each other. (Kuhn, Structure 149)

Operating in different worlds means that some observations or laws that seem intuitively obvious to one school "cannot even be demonstrated" to the other. For the groups to communicate fully, "one group or the other must experience the conversion" that Kuhn calls a paradigm shift; they must make the transition between incommensurables. Reinforcing this idea, Kuhn declares that "the transition between competing paradigms cannot be made a step at a time, forced by logic and neutral experience. Like a gestalt switch, it must occur all at once (though not necessarily in an instant) or not at all" (Structure 149).

For Kuhn, "The transfer of allegiance from paradigm to paradigm is a conversion experience that cannot be forced." He suggests that, "Lifelong resistance, particularly from those whose productive careers have committed them to an older tradition of normal science, is not a violation of scientific standards but an index to the nature of scientific research itself" (Kuhn, Structure 150). Kuhn connects the resistance to change to the conduct of normal science. Using normal science, "the professional community of scientists succeeds, first, in exploiting the potential scope and precision of the older paradigm and, then, in isolating the difficulty through the study of which a new paradigm may emerge" (Kuhn, Structure 151).

Kuhn proceeds to explore the process of conversion from the older paradigm to the newer, settling for "a very partial and impressionistic survey" (Structure 151) of the rhetorical arguments "that prove particularly effective in the battles over paradigm

change" (Structure 152). He writes pointedly about two of these arguments. First, Kuhn asserts: "Probably the single most prevalent claim advanced by the proponents of a new paradigm is that they can solve the problems that led the old one to crisis," especially when such claims are "legitimately made." Kuhn notes that this argument is perhaps the most effective, especially "if the new paradigm displays a quantitative precision strikingly better that its older competitor" (Kuhn, Structure 152-53). A second persuasive argument rests in drawing evidence from other parts of the field where a new paradigm may exhibit the ability to predict outcomes "entirely unsuspected while the old one prevailed" (Structure 153). Arguments "based upon the competitors' comparative ability to solve problems" and the ability of the new paradigm to predict "are ordinarily the most significant and persuasive" according to Kuhn, but they are not the only points of persuasion (Structure 154).

Kuhn concludes that the issue really driving the debate over competing paradigms is "which paradigm should in the future guide research on problems many of which neither competitor can yet claim to resolve completely" (Structure 156). He insists: "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" (Kuhn, Structure 156-57). The paradigm is a guide, suggesting which problems the community should focus on as well as directions for the resolution of those problems.

XIII. Progress through Revolutions

Kuhn's final section of The Structure of Scientific Revolutions addresses issues associated with his notions about the progress shown by science. First, he argues that to "a very great extent the term 'science' is reserved for fields that do progress in obvious ways." He draws a comparison between his notion of science and "the recurrent debates

about whether one or another of the contemporary social sciences is really a science," then suggests that these debates "have parallels in the pre-paradigm periods of fields that are today unhesitatingly labeled science" (Structure 159). The key is that "we tend to see as science any field in which progress is marked" (Kuhn, Structure 161). Kuhn argues:

> [T]he members of a mature scientific community work from a single paradigm or from a closely related set. Very rarely do different scientific communities investigate the same problems. In those exceptional cases the groups hold several major paradigms in common. Viewed from within any single community, however, whether of scientists or of non-scientists, the result of successful creative work *is* progress. (Kuhn, Structure 161)

While fields outside of the sciences show progress as well, Kuhn argues that one fundamental difference between those fields and the sciences appears in "the absence at most times of competing schools that question each other's aims and standards" (Structure 162). Kuhn reiterates that "the reception of a common paradigm [frees] a scientific community from the need to constantly re-examine its first principles," allowing the community to concentrate on the phenomena that concern it (Structure 162-63). Further, the nature of the scientific community also plays a role; scientists, according to Kuhn, are not so much concerned with the lay reception of their work as they are with its reception by colleagues already sharing a set of values and beliefs determined by a common paradigm. The professionalization that scientists undergo as part of their entry into the community tends to insulate them from problems and audiences outside of their field of concern.

Moving towards a final conclusion, Kuhn suggests that scientific advance follows a process similar to Darwin's theory of evolution. Kuhn remarks:

> [T]he resolution of revolutions is the selection by conflict within the scientific community of the fittest way to practice future science. The net result of a sequence of such revolutionary selections, separated by periods of normal research, is the wonderfully adapted set of instruments we call modern scientific knowledge. Successive stages in that developmental process are marked by an increase in articulation and specialization. And the entire process may have occurred, as we now suppose biological evolution did, without the benefit of a set goal, a permanent fixed scientific truth, of which each stage of development of scientific knowledge is a better exemplar. (Kuhn, Structure 171)

Kuhn concludes that, "Any conception of nature compatible with the growth of science by proof is compatible with the evolutionary view of science" that he develops. He carefully avoids answering the question of why such an evolutionary process "should work" (Structure 172).

Kuhn's early essays and his 1962 edition of Structure stimulate the debate within the history and philosophy of science over the nature of scientific progress. Kuhn's critics attack his definitions, his framework, his characterization of normal science, and his logic. While championing his cause, Kuhn's disciples spread his message throughout various fields outside of the history and philosophy of science, reaching across disciplinary boundaries into the social sciences, the humanities, education, and composition studies. Many fields add Kuhnian terminology such as "paradigm" and "paradigm shift" to their descriptive vocabularies, seeing the terminology as convenient

labels for disciplinary activities. Kuhn generates what perhaps can best be labeled as a crisis within his own and allied fields by positing a view of scientific progress that accounts for the role of shared values and beliefs within scientific communities. His theory offers borrowers a way to account for the maturity of a discipline, providing them political ammunition for claiming the mantle of disciplinehood within the academy.

CHAPTER FOUR

THE RESPONSE OF HISTORY AND PHILOSOPHY OF SCIENCE

> Turn now to paradigms and ask what they can possibly be. My original text [Structure] leaves no more obscure or important question. One sympathetic reader, who shares my conviction that 'paradigm' names the central philosophical elements of the book, prepared a partial analytic index and concluded that the term is used in at least twenty-two different ways. . . .[16]
>
> --Thomas Kuhn, 1969 (1970)

How well did Kuhn's colleagues studying the history and philosophy of science receive The Structure of Scientific Revolutions? What influence did Kuhn's work exert in those areas of inquiry from its earliest manifestations in the late fifties and early sixties through the next two decades? The early criticism of the ideas presented in the "Essential Tension" and "Dogma" essays and the 1962 edition of Structure led to Kuhn's response in a number of additional essays in the sixties and seventies addressing specific arguments raised against his theory; further, the criticism led to the 1970 enlarged, second edition of Structure as well. Kuhn's critics found fault with his definitions, with his pattern of scientific advance, and with his interpretation of that pattern. His supporters actively sought evidence in the natural sciences while borrowers of Kuhn spread his message to the humanities, social sciences, composition studies, and elsewhere.

Rather than organize my review of the response of the disciplines of history and philosophy of science to Kuhn's theory thematically, I have chosen to follow the historical pattern in which much of that response appeared. Major criticism of Kuhn's

ideas often came in the form of specific responses to various works of Kuhn's followed up with Kuhn's reply to that criticism. We see the pattern (Kuhn's work, specific criticism leveled against it by scholars in history or philosophy of science, and Kuhn's reply to that criticism) repeated in responses to Kuhn's "Dogma" essay in A. C. Crombie's Scientific Change, to Kuhn's "Logic of Discovery or Psychology of Research" appearing in Imre Lakatos and Alan Musgrave's Criticism and the Growth of Knowledge, and in the second edition of Structure, where Kuhn tackles some of his most stringent critics in "Postscript--1969." In each of his replies, Kuhn further refines his theory, attempting to derail his most vociferous opponents while pointing out areas of apparent agreement.

These responses and Kuhn's replies appear in detail here because few of our borrowings in composition studies have made use of any of the disciplinary criticism already available in Kuhn's own fields. With few exceptions in the next chapter, critics responding to Kuhn's ideas in composition studies have chosen to "discover" their own reasons for attacking borrowings of Kuhn, usually couched as accusing the borrower of misinterpreting Kuhnian theory or finding fault with the rationale used to apply Kuhn to composition inquiry. Looking closely at the disciplinary reception of Kuhn in the history and philosophy of science thus reveals a better picture of the validity of borrowing a theory intact from another discipline without examining that discipline's response to it.

Initial Receptions

> For not only is Kuhn's paradigm, in my view, a fundamental idea and a new one in the philosophy of science, and therefore one which deserves examination, but also, although Kuhn's whole general view of the nature of scientific revolutions

depends upon it, those who attack him have never taken the trouble to find out what it is.[17]

--Margaret Masterman, 1970

Kuhn's paper at the 1961 Oxford symposium, "The Function of Dogma in Scientific Research," generated some of the earliest criticism and support for his inquiry. In A. C. Crombie's 1963 Scientific Change, a number of scientists and philosophers including A. Rupert Hall, Michael Polanyi, and Stephen E. Toulmin comment on their initial reception of Kuhn's theory. Kuhn replies to their criticisms, hinting at things to come in Structure and setting out some of the implications of his view of paradigms, science, and revolutions.

The publication of Structure in 1962 opened Kuhn's theory up for greater debate. His colleagues in the history and philosophy of science were divided, and Structure generated a number of critiques and reviews. Writing in Philosophical Review in 1964, Dudley Shapere, for example, reviews Structure and finds Kuhn's notion of paradigm suspect, but admits that Kuhn does contribute to the investigation of scientific thought.

At the 1965 International Colloquium in the Philosophy of Science, held in London, other critics took Kuhn to task over his definition of paradigm, his characterization of normal science, and his sociological bias. The Colloquium devoted volume four of its proceedings, Imre Lakatos and Alan Musgrave's 1970 Criticism and the Growth of Knowledge, to a critical discussion of Kuhn's ideas. Just as in Crombie's earlier collection, Kuhn responded to his critics in the Lakatos and Musgrave volume. Several of the essays appearing in the Lakatos and Musgrave collection were extensively revised between the 1965 Colloquium and the 1970 publication; for example, the editors note that Kuhn's opening remarks in the collection are essentially unchanged from the 1965 paper he presented, while his final response to critics which concludes the volume was

completed in 1969. The volume provided Kuhn with an opportunity to expound upon his ideas and answer criticism too.

The critical interpretations and Kuhn's responses provide a look at the debates within the history and philosophy of science over the validity of Kuhn's theory. Briefly, much of the criticism of Kuhn's ideas follows four important lines: confusion over his notion of paradigm, debate about his characterization of scientific education, attacks upon his conception of normal science and its influence on scientific advance, and questions about the nature of scientific revolutions. These themes, with some variation, occur repeatedly throughout the disciplinary responses to Kuhn's work within the history and philosophy of science. The nature and, at times, the virulence of the early criticism Kuhn's ideas met in the philosophy of science tends to run counter to the rampant spread of his ideas into other disciplines.

Recorded in A. C. Crombie's 1963 Scientific Change, much of the earliest criticism and support for Kuhn's inquiry comes as response to his 1961 Oxford symposium presentation. A. Rupert Hall and Michael Polanyi offer commentaries in reply to Kuhn's theory as presented in "The Function of Dogma in Scientific Research." B. Glass, Stephen E. Toulmin, and E. F. Caldin add to the discussion. Their remarks in Scientific Change provide a foretaste of the debate that breaks out over Kuhn's theory with the publication of Structure.

Discussion of Crombie's Scientific Change would be incomplete without analyzing Kuhn's response to his critics, especially as his replies to the questions about the paradigm, dogma, nature of science, and scientific education mirror later responses to such criticism by others. Kuhn himself presents a final reply to both critics and supporters of the ideas expressed in his "Dogma" paper. His remarks are presented as part of each section detailing the criticisms. The criticism and response begin with A. Rupert Hall's commentary.

Attacking Kuhn's Dogma: Hall and Polanyi

Hall opens his commentary on Kuhn's "Dogma" essay by restating Kuhn's definition of paradigm and offering a simplification of it as a framework for thought. He goes on to identify a number of points of agreement with Kuhn, especially Kuhn's contention that most scientists contribute to scientific advance not by disturbing fundamentals, but by merely confirming them, or revealing "fresh entailments from them" (372). While Hall agrees with many points of Kuhn's theory (paradigms as a feature of developed science, the resistance or reluctance of scientists to exploit the breakdown of older theories, and Kuhn's conception of normal research) and even congratulates him for offering "a stimulating paper" (375), agreement with these points still does not resolve the paradox that Hall suggests also exists in Kuhn's contention that "science is both dogmatic and devoted to discovery." Hall asks: "How is the intellectual freedom which is essential to originality in science to be reconciled with the necessity of adhering to a certain code, to established themes and recognized practices?" (372)

For Hall, Kuhn's answer is twofold and problematic. First, Kuhn suggests that science does advance "within the terms of the existing accepted framework; it is perfected and extended in its application to the facts of experiment and observation." Second, when the framework cannot accommodate the accumulation of significant "'anomalies' and 'discoveries' . . . and when--a crucial condition--a scientist has appeared with sufficient ability to offer an adequate alternative--the framework itself is replaced." Under close examination, Hall asserts, Kuhn's "concept of a rigid, textbook authenticated paradigm or intellectual framework, to which all scientists are firmly dedicated, as set out in the first half of his [Kuhn's] paper, begins to dissolve somewhat in the second." Hall writes that Kuhn modifies his original claim to recognize that such

acceptance or adherence "is rarely or never perfect . . . never complete," because, "No scientific concept or system of scientific reasoning has ever been regarded as *absolutely* sacrosanct" (372-73).

According to Hall, the most obvious reason for restricting the dogmatism of science rests in "the fact that no natural science constitutes a purely formal system. There is always a measure of doubt to be exploited or repressed about what the facts of nature are." Second, "there has never yet been a single unified intellectual framework for the whole of science." Hall writes: "We find rather--in the science of the past as well as in that of the present--a whole series of paradigms . . . that are often overlapping and interpenetrating. Consequently, there exist areas of inconsistency." In his criticism of Kuhn, Hall makes the point that "the paradigm or intellectual framework is somewhat less than monolithic." If so, he argues, "we may begin to wonder whether reluctance to change it, beyond narrow limits, is not as much due to the lack of human capacity as the weight, or inertia, of scientific dogma." Hall wants to assert that "the limit upon change and originality is not set by anything in the nature of science, but by our own capacities." Ultimately, dogma "becomes less a necessity than an apology for weakness" --human weakness (Hall 373-74).

While grateful for Hall's agreement with much of his overall thesis, Kuhn writes that many of Hall's criticisms stem from the brevity of Kuhn's text, especially Hall's impression that Kuhn takes "science to be a 'monolithic' enterprise dominated by a single paradigm." He asserts that Hall fails "to dissolve" Kuhn's thesis "about the essential role played by individual paradigms in the research efforts of various scientific specialties." Kuhn notes: "Historians of science, including Mr. Hall, have recently been particularly successful in showing how the way to a paradigm-change is prepared by weaknesses disclosed and concepts prepared within the prior tradition" ("Discussion" 387-88).

Kuhn uses the opportunity to respond to Hall's commentary by emphasizing the points of agreement between them.

Like Hall, Polanyi also opens his commentary by stating his agreement with Kuhn on certain points. In his response to Kuhn's 1961 paper, Polanyi blatantly states that he wishes "to join forces" with Kuhn and argues that he "could not agree more" with Kuhn's assertion "that the dependence of research upon a deep commitment to established beliefs receives the very minimum of attention." Polanyi writes: "I have tried in vain to call attention to this commitment for years." Further, Polanyi believes that Kuhn's effort should help establish the idea of dependence upon commitment "so firmly that its vast implications will have to be faced by all" (375).

Polanyi differs from Kuhn on the issue of the maturity of a science. The mature stage in Kuhn's framework is characterized "by the collective commitment of all research workers to the same paradigmatic discoveries" (Polanyi 377). Kuhn contends that the early stage of science, what Kuhn later refers to as pre-paradigmatic, "is 'less than scientific' by contrast to the later, mature condition of science." Polanyi, on the other hand, states: "I think we might rather call the earlier stage an unsystematic and the later a systematic pursuit of science." Polanyi suggests that such an interpretation "allows us to observe that science may remain unsystematic, and indeed may *become* unsystematic, at any time in countries where the authority of scientific opinion has either not yet been established or, having once been established, has subsequently been weakened or destroyed" (378). Polanyi means for his warning to serve as a reminder of a social implication that he claims Kuhn ignores.

> The collective commitment of scientists to a set of beliefs and values must be safeguarded through institutional channels controlled by scientific opinion. In

> fact, the very holding of common beliefs inevitably creates an authority which in its turn assures the continued holding of these beliefs. (Polanyi 378)

After discussing the missed social implication, Polanyi elaborates on some disagreements with Kuhn. First, he rejects Kuhn's classification of scientific discovery into two types: "those which merely articulate accepted scientific beliefs by extending their application . . . and others which change our very beliefs about the nature of things, thus establishing a new paradigm to guide further discoveries." Calling the differences between the two "a matter of degree" (378), Polanyi continues:

> Admittedly a discovery which involves considerable conceptual reform requires a different kind of originality from that which enlarges an existing intellectual track into a broad avenue. But both are equally free creations of the mind, and at the same time the scope of both is equally circumscribed by the range of hidden possibilities that are accessible from the intellectual position already reached. (Polanyi 379)

In reporting, supplementing, and amending Kuhn's account "of the process of scientific discovery," Polanyi argues that Kuhn's expression in the "Dogma" essay "leaves open the main questions concerning the nature of scientific method and the foundations of scientific knowledge" (379). By casting the difference between "conceptual reform" and elaboration of "an existing intellectual track" as a "matter of degree," Polanyi asserts that "scientific originality includes a capacity to doubt accepted beliefs" (380). Between these two degrees, a number of questions arise for Polanyi:

> Is there any rule for distinguishing between the two? Or for that matter, any rule by which scientists can distinguish their own failure to apply the current framework of scientific beliefs from the presence of an essential anomaly incompatible with these beliefs--or any rule by which scientists can ascertain whether an anomaly, once established, is to be regarded as a challenging problem, or rather as a basic paradox intrinsic to a new framework of science? (Polanyi 380)

Polanyi concludes that, "There are of course no such rules" (380).

Of all of Kuhn's critics in Crombie's work, Polanyi is the most sympathetic to Kuhn's ideas. Kuhn suggests that Polanyi's most important criticisms lie beyond the scope of the "Dogma" presentation, and, as such, Kuhn uses Polanyi's comments "first, to extend the area of agreement he [Polanyi] has already mapped and, then, to indicate some of the residual, but . . . significant, differences that separate his view of scientific development from my [Kuhn's] own" ("Discussion" 391).

First, Kuhn welcomes Polanyi's comments on "the constructive functions of paradigms" ("Discussion" 391). He agrees with Polanyi about the absence of rules "for distinguishing an essential anomaly from mere failure and also that some anomalies are recognized for many years without 'inducing any scientist to reconsider current theories with which they conflict.'" Kuhn focuses on characterizing "the state of the scientific community within which a new theory is invented and accepted" ("Discussion" 392). Thus, his paper can only be seen, as Polanyi notes, "as a fragment of a larger work," and Kuhn already identifies it as such.

Kuhn does disagree with Polanyi on a few points, most of which grow out of implications of either Kuhn's presentation or Polanyi's commentary. Kuhn speaks of commitment to a paradigm as "the particular way of doing science that is sanctioned by a

given sector of the scientific community at a specified period in time." He asserts that "such a commitment need not be elicited . . . by community authority alone"; the sanction of an accepted paradigm is pragmatic as well as authoritarian as the paradigm "embodies a vast amount of accumulated experience." Such a view creates discord with Polanyi's contention that scientists commit to 'the holding of the premises of science.' According to Kuhn, "The commitment of every scientist therefore includes an important element of the historically accidental, the temporarily local, and, thus of the arbitrary." While admitting that some of Polanyi's premises exist, Kuhn doubts "that commitment to such premises is alone sufficient to make a man a scientist. Commitment to the local, arbitrary, and temporary is also required." Commitment means a "closing of the mind, one that inhibits, at the same time that it permits, original research" ("Discussion" 393).

Kuhn's strongest disagreement lies with Polanyi's contention that "advances within a paradigm are no less 'revolutionary' than those that demand a change in concepts." Kuhn adamantly preserves the distinction he sees between normal and revolutionary scientific advance. He refuses to accept Polanyi's blurring of the two and recasts Polanyi's own example to show how it supports Kuhn's hypothesis about the reconstruction of ideas demanding "simultaneous destruction of a prior professional commitment" ("Discussion" 394).

Finally, Kuhn responds to Polanyi's notions about "the individual's commitment to the premises of science." Kuhn asserts that the community, not the individual, decides whether one's "discoveries or theoretical inventions shall become part of the body of established science." Kuhn takes the position that the "professional community . . . has and sometimes exercises the privilege of declaring [the scientist] deviant" (Kuhn, "Discussion" 394-95). The social implications of Kuhn's view are thus more important than Polanyi allows.

Other Responses to Kuhn's Dogma: Glass, Toulmin, and Caldin

In addition to the commentaries by Hall and Polanyi, B. Glass, Stephen E. Toulmin, and E. F. Caldin also offer responses in Crombie's Scientific Change to Kuhn's "Dogma" paper. Some of the opposition that Polanyi refers to in his commentary seems evident in the modifications, disagreements, and reservations of three.

Glass's contribution to the discussion of Kuhn's "Dogma" paper rests in his contention that Kuhn's "emphasis needs modification" in three respects. First, calling himself "a practicing scientist--a geneticist--rather than a historian or philosopher of science" who agrees with the main thrust of Kuhn's thesis, Glass argues for greater clarification of "the relationship of the paradigm to the scientist's *basic assumptions* and to his use of *hypothesis* and *theory.*" Second, Glass calls upon Kuhn to place more emphasis "on the effect of the rapid growth of science on the frequency of the periods of crisis or overturn of the older bodies of concepts, and on the consequent shortening of the periods during which particular paradigms hold sway" (381).

In his final and perhaps sharpest comment on Kuhn's theory, Glass disagrees completely with Kuhn's emphasis on the influence of paradigms over textbooks and education. Glass concludes he is "appalled to think that, if Mr. Kuhn is right, we should go back to teaching paradigms and dogmas, not as merely temporary expedients to aid us more clearly to visualize the nature of our scientific problems, but rather as part of the regular, approved method of scientific advance" (Glass 382). As seen in the concluding statement to his discussion, Glass's disagreement with Kuhn seems most likely to come from a misapplication of Kuhn's contentions about textbooks and scientific education. Kuhn does not argue for such an application in his "Dogma" paper; he merely observes that scientific textbooks and science education seem to be organized around paradigms.

Further, Glass's statement of disagreement offers some support for Kuhn's position by suggesting that science may have once been taught the way Kuhn posits.

Kuhn sees Glass's criticism as an opportunity to make two additional points not explicit in the original "Dogma" essay. First, he asserts that Glass's remarks about the distinction "between dogmas and paradigms both fits and furthers" Kuhn's inquiry (Kuhn, "Discussion" 390). Kuhn indicates that he is willing to favor something like "commitment to a paradigm" in the place of "dogma" ("Discussion" 392). In addition, Glass effectively brings out "the existence of ideological factors which make a scientist *exploit* a paradigm rather than simply *slumber* within it," a point that Kuhn admits to only hinting at in his paper ("Discussion" 390).

Stephen E. Toulmin is much harsher than Glass in criticizing Kuhn's position. In his opening statement on the "Dogma" paper, Toulmin asserts that Kuhn "has . . . presented his central position in a needlessly paradoxical form." Toulmin argues that clarifying Kuhn's work involves "giving up the suggestion that science necessarily involves dogmatism, *and* going in more detail into the precise *mode d'emploi* of the 'paradigms' of which he [Kuhn] speaks" (Toulmin, "Discussion" 382). He contradicts Kuhn's assertion that scientists "necessarily rely . . . on *preconceived ideas.*" Toulmin insists that Kuhn is mistaken, arguing that "the points to which . . . Kuhn draws our attention do nothing to imply a lack of objectivity and open-mindedness on the part of scientists--they are simply points of logic" ("Discussion" 383).

Toulmin feels "obliged" to draw a distinction about the ambiguity of "such phrases as 'preconceived ideas'" ("Discussion" 383). He argues:

> One may have preconceived ideas in the sense of holding prejudged (prejudiced) beliefs; or alternatively in the sense of employing preformed concepts. Now the objectivity and open-mindedness we demand of the scientist is this--that he

> should not be influenced in the laboratory or the field by prejudices about the answers he will find to the questions he asks there--he must leave nature to answer his questions for herself, without prompting. (Toulmin, "Discussion" 383)

Toulmin insists that such prejudices inappropriately influence the results and interpretation of empirical study. In this instance, Toulmin comes down clearly on the side of the ideal image of the scientist as objective, neutral, and open-minded--the opposite of Kuhn's view.

But Toulmin, though he leaves "nature to answer our questions for herself," also admits that it is the scientists "who frame the questions," and these questions "are formulated in terms of certain concepts antecedent to the investigation in question." Toulmin admits: "However objective and open-minded a scientist may be, his questions necessarily involve the use of preformed concepts," that is reliance on "preconceived ideas" without the involvement of dogmatism. Toulmin labels these concepts as "the common coin of scientific discussion in any given period, the basis of common understanding between scientists, and of their language." He writes that "they are essential if there is to be any agreement between scientists about what questions are relevant, sensible, even intelligible." Toulmin reemphasizes that "the 'collective commitment' of scientists to such preformed concepts or paradigms (if we accept the word) accordingly implies in no way attachment to a dogma or anything of the sort" ("Discussion" 383).

Like Hall, Toulmin has trouble with Kuhn's use of paradigm, noting that Kuhn uses the term:

> to refer *either* more narrowly to a particular set of basic concepts . . . which are determined by an influential theory and in terms of which questions arising in the subsequent application and extension of the theory have to be framed; *or alternatively*, and more widely, to a whole masterpiece of science--a book, not a set of principles--by which a great man . . . imposes on his followers a certain mode and style of expression and exposition. (Toulmin, "Discussion" 384)

He calls this dual use unfortunate, "for though reliance on paradigms in the narrower sense may be proper and necessary, the example set by paradigms in the wider sense may indeed lead to prejudice." Further, he warns against confusing "the proper role of new theories, in fixing and clarifying our notions, with the inadvertent effect they may have, of prejudicing our beliefs." Toulmin argues strongly that any use of the term, paradigm, must be restricted "to the narrower signification--to refer to the basic forms of concept introduced by a new framework of fundamental theory, i. e. to the scientists' 'preconceived ideas,' in the innocent and indispensable sense of that phrase" ("Discussion" 384).

Finally, Toulmin rails against Kuhn's concept of the sciences "as having a 'pre-paradigm' stage." Given his redefinition of paradigm as "preformed concepts," Toulmin states that even in the earliest days of science, "men approached nature with prior notions and preformed ideas as much as in later ages: their paradigms may have been greater in number and less consistent than they became later" when transformed by some unifying theory. He concludes that "the idea of a naive pre-pre-conception [sic] stage in science is surely a myth" (Toulmin, "Discussion" 384).

Toulmin attacks Kuhn by specifically leveling charges against the concepts of dogma and paradigm. While he dismisses the former and calls for a restricted view of the latter, Toulmin unwittingly acknowledges that Kuhn's concepts do contain hints

concerning the "collective commitment," "common coin of scientific discussion," and "common understanding between scientists" (Toulmin, "Discussion" 383). Toulmin completely disagrees with Kuhn on the image of the scientist, the prejudicial nature of commitment to a paradigm, and the existence of a pre-paradigmatic stage in the maturation of the sciences. These essential objections will find later expression in other's criticisms of Kuhn.

In response to Toulmin's objections, Kuhn notes that Toulmin's ideas "may lead to a correspondingly more useful clarification" of Kuhn's own ideas. Dismissing "idiosyncrasy from consideration," Kuhn writes that Toulmin himself insists that, "The scientist will still require . . . 'preformed concepts' in order to undertake research" (Kuhn, "Discussion" 389). Kuhn continues:

> But *with respect to future research* those concepts--ones which the individual acquires principally through education--are necessarily "prejudged" as well as "preformed." They are, that is, the still operant results of a past selection made by the profession from a number of competing alternative bases for research. (Kuhn, "Discussion" 389)

Kuhn argues that neither history nor logic supports Toulmin's feeling that "because the need for *some* sort of preformed concepts is 'logical' . . . it can make no difference which *particular* set of concepts the profession in fact chooses to embrace." Referring to Toulmin's nature metaphor, Kuhn avers that "the most significant episodes in scientific development . . . often prove to be occasions when nature's refusal to answer old questions prepares the way for new ones that can emerge only through fundamental conceptual reform" (Kuhn, "Discussion" 389). He asserts that the necessity of some

preformed concepts "for the continuation of research does not neutralize the particular selection made by the profession" ("Discussion" 390).

Kuhn also responds to Toulmin's contention about the confusion between a narrow and a broad sense of paradigm, dismissing Toulmin's examples. He admits that Toulmin is right "that there is far more to be said about which portions of particular scientific achievements come to serve as paradigms, in which periods, and for whom." But Kuhn contends that those determinations "are problems for extended historical investigation only part of which has already been carried through and none of which is reflected in my [Kuhn's "Dogma"] paper" ("Discussion" 390).

Kuhn's response to Toulmin's criticisms reflects the same thread of argument present in Kuhn's assessment of Hall. Kuhn focuses more on where both critics agree with him than on the points of contention, emphasizing the brevity of his paper as one factor contributing to the confusion or misinterpretation he sees in his critics' views. Kuhn attempts to defuse Toulmin's criticism in much the same way as he does Glass's and Hall's--by pointing out similarities as evidence of Kuhn's own position.

Following in the footsteps of Glass and Toulmin, Caldin--"a practicing physical chemist, doing experimental research of the normal kind"--expresses four reservations about parts of Kuhn's theory, especially Kuhn's use of dogma and the structure implicit in Kuhn's sense of paradigm. Caldin accepts Kuhn's version of normal scientific advancement only partially; he agrees that research is "normally done within a stable theoretical framework" and that a commitment exists to "general hypotheses" that seem to characterize "the normal progress of a developed science." Then, Caldin, like Toulmin, proceeds to discount dogma. He declares: "Scientists are not committed even to conservatism, in the objectionable sense. Rather, they are concerned with a tradition, which develops and can be corrected" (Caldin 384-85).

As for the structure of the scientific theory implicit in Kuhn's sense of paradigm, Caldin asserts that the "tradition of a developed science is not a single hypothesis; it is a combination of hypotheses, with a hierarchical structure. Below the most general hypotheses . . . are subsidiary hypotheses . . . below these again are more specific hypotheses." He argues that the "task of science, outside times of crisis, is to find a self-consistent set of subsidiary hypotheses, to account for a mass of observations" (385).

To account for most scientists' commitment to general assumptions, Caldin suggests that "their understanding of phenomena normally comes about by combining these fundamental hypotheses with adjustable subsidiary ones. . . . obviously the most economical way to make progress" (385-86). He notes that "no one wants to adopt new basic hypotheses before they are seen to be improvements of the old," thus "when anomalies arise, one tries first to adjust the subsidiary hypotheses." Since this "situation continually recurs in research," Caldin asserts, "The tradition is developed and corrected" (386).

Kuhn chooses to let Caldin's remarks pass without response, perhaps because Caldin's position seems to support Kuhn's ideas about normal research--what Kuhn will later term normal science. Caldin's brief discussion confirms Kuhn's identification of such research as puzzle-solving. Additionally, Caldin's account of resistance to new hypotheses follows Kuhn's notion of resistance to paradigmatic change to a certain extent; Caldin seems to agree that one goal of the puzzle-solving activity of normal research is the further articulation of the paradigm, or in Caldin's case, the adjustment of the subsidiary hypotheses to develop and correct the tradition. We also see in Caldin's position a similar concern for scientific education that suggests Kuhn's theory is wrong. Dismissing the dogmatic nature of science that Kuhn argues exists also means scrapping his views on the conduct of scientific education.

The remarks of various scholars in Scientific Change represent the opening salvo of criticism that greets the more extensive publication of Kuhn's theories in Structure. Hall, Polanyi, Glass, Toulmin, and Caldin respond to only a fragment, roughly the first third of Kuhn's draft of Structure, but they do so in ways that signal some of the recurrent themes of later critics, especially the way in which Kuhn uses and defines the central concept of paradigms. As early respondents to Kuhn's ideas, they provide valuable insight into the development of Kuhnian theory and its reception within the disciplines of history and philosophy of science.

Finding Fault with Kuhn's Structure: Shapere

In his review of Kuhn's Structure for the journal, Philosophical Review, Dudley Shapere questions the value of Kuhn's notion of paradigm and attacks Kuhn's work as confusing. While he admits that Kuhn's work "is bound to exert a very wide influence among philosophers and historians of science" (383), he charges Kuhn with relativism and argues that Kuhn's thesis "that shared paradigms are (or are behind) the common factors guiding scientific research appears to be guaranteed, not so much by a close examination of actual historical cases, however scholarly, as by the breadth of definition of the term "paradigm" (Shapere 385). He suggests that Kuhn's interpretation of scientific development--a cycle of normal or paradigm driven science, crisis, revolution, and normal science driven by a new paradigm--"places a heavy burden indeed upon the notion of paradigm" (Shapere 384). By striking at what he sees as Kuhn's most central concept, Shapere attempts to overthrow much of Kuhn's theory.

Shapere begins his attack on the concept of "paradigm" by reviewing the numerous ways in which Kuhn seems to define and use the term (Margaret Masterman in a later

article identifies twenty-one separate usages). He concludes that Kuhn's definition is deliberately expansive:

> The term "paradigm" thus covers a range of factors in scientific development including or somehow involving laws and theories, models, standards, and methods (both theoretical and instrumental), vague intuitions, explicit or implicit metaphysical beliefs (or prejudices). In short, anything that allows science to accomplish anything can be a part of (or somehow involved in) a paradigm. (Shapere 385)

The ambiguity of Kuhn's definition makes it suspect. Even worse, according to Shapere, Kuhn argues that paradigms cannot be adequately formulated. Shapere charges:

> It would appear that, in Kuhn's eyes, the concepts, laws, theories, rules, and so forth that are common to a group are just not common enough to guarantee the coherence of the tradition; therefore he [Kuhn] concludes that the paradigm, "the concrete scientific achievement" that is the source of that coherence, must not be identified with, but must be seen as "prior to the various concepts, laws, theories, and points of view that may be abstracted from it." (Shapere 386)

Shapere goes on to note the paradoxical nature of Kuhn's claim that paradigms "cannot be described adequately in words," but are open to "direct inspection." He writes: "In most of the cases he [Kuhn] discusses, it is the theory that is doing the job of posing problems, providing criteria for selection of data, being articulated, and so forth." Shapere speculates that Kuhn uses the theory in many of his examples "because it is as near as he [Kuhn] can get in words to the inexpressible paradigm." Ultimately, Shapere

suggests that "one begins to doubt that paradigms are open to 'direct inspection,' or else to be amazed at Professor Kuhn's eyesight" (Shapere 386-87).

A second difficulty also arises for Shapere when considering paradigms; how does one distinguish "between different paradigms and different articulations of the same paradigm?" (387). Shapere concludes that such distinctions are "at best a matter of degree" (388), writing:

> The distinction between paradigms and different articulations of a paradigm, and between scientific revolutions and normal science, is at best a matter of degree, as is commitment to a paradigm: expression of explicit discontent, proliferation of competing articulations, debate over fundamentals are all more or less present throughout the development of science; and there are always guiding elements which are more or less common, even among what are classified as different "traditions" (Shapere 388)

Not content simply to discredit Kuhn's notion of paradigm, Shapere goes on to show that Kuhn's notion "affects adversely his [Kuhn's] analysis of science." Shapere notes that much of Kuhn's concept of "paradigm change entails 'changes in the standards governing permissible problems, concepts, and explanations'" or what Shapere sees as changes in meaning. He suggests that Kuhn argues that "concepts and meanings change from one theory (paradigm) to another despite the retention of the same terms." While this argument is crucial to Kuhn's own attack on the positivist view of science as cumulative, "and that therefore earlier sciences are derivable from later," Kuhn fails to offer a "clear analysis of 'meaning' or, more specifically, no criterion of change of meaning; consequently it is not clear why he classifies such changes as changes of meaning rather than, for example, changes of application" (Shapere 389-90).

Citing Kuhn's example comparing Newtonian and Einsteinian physics, Shapere objects to Kuhn's dependence on changes in meaning on a number of grounds:

> Thus it is not so much Kuhn's conclusion that is objectionable as, first, the fact that it is based, not on any solid argument, but on the feature of meaning dependence which Kuhn has built into the term "paradigm" (scientists see the world from different points of view, through different paradigms, and therefore see different things through different paradigms); and second, the fact that this feature leads him to a distorted portrayal of the relations between different scientific theories. For Kuhn's term "paradigm," incorporating as it does the view that statements of fact are . . . theory-laden, and as a consequence the notion of . . . meaning variance from one theory or paradigm to another, calls attention excessively to the differences between theories or paradigms, so that relations that evidently do exist between them are in fact passed over or denied. (Shapere 390-91)

Shapere essentially criticizes Kuhn for obscuring the differences between alternatives by holding that they are incommensurable.

The issue of incommensurability provides Shapere with additional ammunition to counter Kuhn's arguments. He questions Kuhn's notion of progress as the replacement of one incommensurable paradigm by another, declaring that:

> The logical tendency of Kuhn's position is clearly toward the conclusion that the replacement is not cumulative, but is mere change: being "incommensurable," two paradigms cannot be judged according to their ability to solve the same

> problems, or deal with the same facts, or meet the same standards. (Shapere 391)

In other words, Shapere asserts that we cannot judge the acceptability of one paradigm over another because their incompatibility, in Kuhnian terms, will not allow us to communicate in the same language, much less discuss problems, facts, or standards; the nature of those problems, facts, and standards has altered with the change in paradigm. Kuhn's position thus leaves in obscurity the reasoning for favoring one paradigm over another. Kuhn argues "that the decision of a scientific group to adopt a new paradigm is not based on good reasons; on the contrary, what counts as a good reason is determined by that decision" (Shapere 392).

Finally, Shapere turns his attention to the confusion and relativism he sees present in Kuhn's theory. He attacks Kuhn for failing to highlight the reasons for scientific change, charging Kuhn also with obscuring "the existence of such reasons" (Shapere 393). Further, he states:

> [Kuhn] does bring out, through a wealth of case studies, many common features of scientific thought and activities which make it possible to and, for many purposes, revealing to speak of "traditions' in science; and it points out many significant differences between traditions. But Kuhn, carried away by the logic of his notion of paradigm, glosses over many important differences between scientific activities classified as being of the same tradition, as well as important continuities between successive traditions. (Shapere 393)

Kuhn's position, from Shapere's point of view, leads to relativism "with regard to the development of science" (Shapere 393).

Shapere concludes that Kuhn's relativism is the "logical outgrowth of conceptual confusions" (393). These confusions spring from Kuhn's blanket use of paradigm. Shapere harshly charges:

> [Kuhn's view] is made to appear convincing only by inflating the definition of "paradigm" until that term becomes so vague and ambiguous that it cannot easily be withheld, so general that it cannot easily be applied, so mysterious that it cannot help explain, and so misleading that it is a positive hindrance to the understanding of some central aspects of science; and then, finally, these excesses must be counterbalanced by qualifications that simply contradict them. (Shapere 393)

While he finds some aspects of Kuhn's book worthy of attention, Shapere argues that Kuhn must provide "more careful scrutiny of his tools of analysis" (394).

Shapere's objection to Kuhn's theory establishes the track that several of Kuhn's later critics choose. By discrediting Kuhn's central notion of paradigm, Shapere hopes to derail Kuhn's entire theory. Kuhn's concept of normal science depends upon the acceptance of the paradigm, and he remarks that Shapere offers the "most thoughtful and thorough negative account" of the term, paradigm ("Second Thoughts" 294). If scientific revolutions simply reflect significant changes in paradigm--the replacement of one paradigm with another in a mature science--then discrediting the paradigm means dismissing Kuhn's conception of scientific advance and development. Interpreting Kuhn's sense of paradigm as vague, inadequate, confusing, and unproven casts doubt on his entire agenda.

Disciplinary Criticism

> I do not agree that the history of science supports his [Kuhn's] doctrine (essential for his theory of rational communication) that "normally" we have one dominant theory--a "paradigm"--in each scientific domain, and that the history of science consists of a sequence of dominant theories, with intervening revolutionary periods of "extraordinary" science; periods which he (Kuhn) describes as if communication between scientists had broken down, owing to the absence of a dominant theory.
>
> This picture of the history of science clashes with the facts as I see them.[18]
>
> --Sir Karl Popper, 1965 (1970)

For perhaps the best picture of the early response of history and philosophy of science to Kuhn's ideas, we can turn to volume four of the Proceedings of the International Colloquium in the Philosophy of Science, London, 1965. The volume, Criticism and the Growth of Knowledge, contains analysis and criticism of Kuhn's theory. Edited by Imre Lakatos and Alan Musgrave, Criticism follows the earlier format of Crombie's Scientific Change. Criticism opens with Kuhn's essay, "Logic of Discovery or Psychology of Research?" According to the editors, "Logic" appears in its essentially original form as a presentation at one symposium of the Colloquium. Critical responses from a number of scholars including Stephen Toulmin and Sir Karl R. Popper follow, although I do not present all of those responses here. Finally, the volume concludes with Kuhn's reply to his critics.

Kuhn prepared his reply, "Reflections on My Critics," in 1969 as a response to criticism of both his 1965 "Logic" paper and his 1962 edition of Structure. While I examine Kuhn's "Logic" paper and most of "Reflections" as part of the Lakatos and

Musgrave collection, I defer analysis of parts of Kuhn's "Reflections" until later because of similarities between "Reflections" and two other Kuhn essays written in 1969. Kuhn offers specific responses to his critics in Lakatos and Musgrave's Criticism, but some of his comments in "Reflections" relate better to the modifications of his position that appear in the second edition of Structure.

Lakatos and Musgrave note that the contributions to their collection were prepared over the interim between the 1965 colloquium and the 1970 publication of Criticism. Contributions to the volume, especially those presented here, can best be seen in light of the 13 July 1965 symposium in which Kuhn juxtaposes his view of scientific development outlined in Structure with the views of Popper. Kuhn's 1962 Structure takes center stage in many of the responses, with Kuhn's "Logic" essay serving more as a point of departure for criticizing Kuhn than anything else.

Exploring the "Logic of Discovery or Psychology of Research?"

In "Logic," Kuhn addresses the views of some of his strongest supporters and critics --the followers of Sir Karl Popper. Kuhn analyzes the foundations of support among the Popperians, claiming that his and Popper's views of science "are very nearly identical" ("Logic" 1). Despite the points of agreement that he sees, Kuhn remains wary of a number of locutions used by Popper and treats four of them in "Logic." First, he finds fault with Popper's reliance on the cliché of the scientist in the empirical sciences who 'constructs hypotheses, or systems of theories, and tests them against experience by observation and experiment' (Qtd in Kuhn, "Logic" 4). Kuhn's statements about Popper's "cliché" position on testing of scientific criteria illustrate a key difference in their views of scientific advance: Kuhn claims that such testing is "a standard component" of normal science, while Popper attributes it to what Kuhn would call "extraordinary

science" or the investigation of anomaly during times of crisis in a science. The disagreement further highlights differences between Kuhn's concept of normal science as puzzle-solving and Popper's view that science involves problem solving. In an accompanying footnote, Kuhn contrasts his view with Popper's, noting:

> Sir Karl writes (the italics are his), "Admittedly, our expectations, and thus our theories, may precede, historically, even our problems. *Yet science starts only with problems.* Problems crop up especially when we are disappointed in our expectations, or when our theories involve us in difficulties, in contradictions." I [Kuhn] use the term "puzzle" in order to emphasize the difficulties which ordinarily confront even the very best scientist are, like crossword puzzles or chess puzzles, challenges only to his ingenuity. He [the scientist] is in difficulty, not current theory. My point is almost the converse of Sir Karl's. (Kuhn, "Logic" 5)

Kuhn goes on to suggest that Popper characterizes "the entire scientific enterprise in terms that apply only to its occasional revolutionary parts." He concludes that Popper has mistaken Kuhn's idea of normal science "for an intrinsically uninteresting enterprise." Kuhn asserts that "it is normal science . . . rather than extraordinary science which most nearly distinguishes science from other enterprises" (Kuhn, "Logic" 6).

Addressing another point of difference with Popper's views, Kuhn notes that Popper has traced the origin of the philosophical tradition of acquiring knowledge to the Greek philosophers, the men who, Popper claims, "encouraged critical discussion both between schools and within individual schools" ("Logic" 6). Kuhn asserts that:

> [It] is precisely the abandonment of critical discourse that marks the transition to a science. Once a field has made that transition, critical discourse recurs only at moments of crisis when the bases of the field are again in jeopardy. Only when they must choose between competing theories do scientists behave like philosophers. (Kuhn, "Logic" 6-7).

What signals the emergence of a field or profession as a science instead of condemning it to Popper's graveyard of "pseudo science" or relegating it to the status of practical art or craft? Restating Popper's criteria for demarcation, Kuhn writes: "For a field to be a science its conclusions must be *logically derivable* from *shared premises*" ("Logic" 9). In addition to shared rules, he asserts, the field must have puzzles to solve. Using Popper's example of astrology as an example, Kuhn avers that some fields share theory that is "only adequate to establish the plausibility of the discipline and to provide a rationale for the various craft rules which [govern] practice" ("Logic" 8). Fields such as astrology lack the research puzzles "to challenge and then to attest the ingenuity of the individual practitioner"; they may test predictions and recognize that their predictions sometimes fail, but they do not and cannot engage in the sorts of activities that Kuhn claims "normally characterize all recognized sciences" ("Logic" 10).

According to Kuhn, Popper acknowledges that despite his claims for testing, many theories are replaced before they are ever tested; Popper admits that on occasion, "tests are not requisite to the revolutions through which science advances" ("Logic" 10). Kuhn notes that this is not true of puzzles. Ultimately, Kuhn concludes in disagreement with Popper that: "With or without tests, a puzzle solving tradition can prepare the way for its own displacement. To rely on testing as the mark of science is to miss what scientists mostly do and, with it, the most characteristic feature" of science ("Logic" 10).

The second of Popper's locutions that Kuhn finds fault with lies in the thesis of Popper's 1963 work Conjectures and Refutations that *"we can learn from our mistakes"* [Popper's emphasis] (Qtd in Kuhn, "Logic" 10). As part of his disagreement with Popper on this point, Kuhn lists the characteristics of mistakes:

> A mistake is made, or is committed, at a specific time and place by a particular individual. That individual has failed to obey some established rule of logic, or of language, or of the relations between one of these and experience. Or he may instead have failed to recognize the consequences of a particular choice among the alternatives which the rules allow him. (Kuhn, "Logic" 11)

Kuhn argues that, "The individual can learn from his mistake only because the group whose practice embodies these rules can isolate the individual's failure in applying them" ("Logic" 11). Kuhn asserts:

> In short, the sorts of mistakes to which Sir Karl's imperative most obviously applies are in the individual's failure of understanding or of recognition within an activity governed by pre-established rules. In the sciences such mistakes occur most frequently and perhaps exclusively within the practice of normal puzzle-solving research. (Kuhn, "Logic" 11)

Unfortunately, Popper's view of science "obscures even the existence of normal research," looking exclusively to revolutionary or extraordinary episodes of science (Kuhn, "Logic" 11). Kuhn argues that, "Like 'testing,' 'mistake' has been borrowed from normal science, where its use is reasonably clear, and applied to revolutionary episodes, where its application is at best problematic." Kuhn has problems with "the prevalent

impression that whole theories can be judged by the same sort of criteria that one employs when judging a theory's individual research applications" ("Logic" 12).

The third series of locutions that Kuhn objects to concern elements of proof--Popper's ideas of "falsification" and "refutation" as they highlight "the asymmetry of a generalization and its negation in their relation to empirical evidence." Kuhn notes that asymmetry plays a similar role in Structure, "where a theory's failure to provide rules that identify solvable puzzles is viewed as the source of professional crises which often result in the theory's being replaced." Popper defines "falsification" or "refutation" as "what happens when a theory fails," and he suggests, at least according to Kuhn, that "invoking these terms implies the ability to compel assent from any member of the relevant professional community." But such notions also involve the presupposition "that a theory is cast, or can without distortion be recast, in a form which permits scientists to classify each conceivable event as a confirming instance, a falsifying instance, or irrelevant to the theory" (Kuhn, "Logic" 13-14).

Kuhn responds to the presupposition by arguing that scientific knowledge often grows by challenging observations or adjusting theories to fit the results of experimentation. The presupposition "requires that we first produce the class of all logical consequences of the theory and then choose from among these, with the aid of background knowledge, the classes of all true and of all false consequences." The degree that Popper calls for in testing and refuting a theory using a measure of verisimilitude cannot be reached "unless the theory is fully articulated logically and unless the terms through which it attaches to nature are sufficiently defined to determine their applicability in each possible case." Kuhn notes that in practice, this rarely ever happens--"no scientific theory satisfies these rigorous demands, and many people have argued that a theory would cease to be useful to research if it did so." Here, Kuhn mentions without elaboration that he has introduced his own concept of "paradigm" to "underscore the dependence of scientific

research upon concrete examples that bridge what would otherwise be gaps in the specification of the content and application of scientific theories" ("Logic" 16). Ultimately, Kuhn concludes that "no exclusively logical criteria can entirely dictate the conclusions" that one must draw from research ("Logic" 19).

In his "Logic" essay, Kuhn focuses on the "criteria with which scientists determine the validity of an articulation or an application of an existing theory," declaring that logical criteria "are not by themselves sufficient to determine the choice between competing theories" ("Logic" 19). He attacks Popper for transferring characteristics of normal research to extraordinary research and for ignoring normal science entirely. According to Kuhn, Popper "has sought to solve the problem of theory choice during revolutions by logical criteria that are applicable in full only when a theory can already be presupposed" ("Logic" 19). Thus, Popper's position still leaves too many unanswered questions about the nature of scientific progress.

In addressing potential answers to some of these questions, Kuhn closes by focusing on a fourth locution of Popper's--Popper's rejection of a psychology of knowledge in favor of an empirical logic of knowledge. Popper "positively asserts that his concern is with the logical spurs to knowledge rather than with the psychological drives of individuals." In so doing, Kuhn notes that Popper denies "the methodological relevance of an *individual's* source of inspiration or of an individual's sense of certainty." However, Kuhn asserts that such a position does not translate well into a rejection "of the common elements induced by nurture and training in the psychological make-up of the licensed membership of a *scientific group.*" Kuhn declares: "We shall not, I suggest, understand the success of science without understanding the full force of rhetorically induced and professionally shared imperatives" ("Logic" 22). In Popper's refusal to see these "social-psychological imperatives," Kuhn again finds evidence of "the gestalt switch" that separates him from Popper.

Popper's Response to Kuhn's "Logic"

Popper responds to Kuhn's criticisms in "Normal Science and Its Dangers," reiterating some of the very points of congruence and incongruence with Kuhn's views as expressed in "Logic" and in Structure. At the same time, he offers some cogent criticism of his own for Kuhn's definition of normal science. Popper, like Kuhn, first takes the opportunity to identify points of agreement, before launching into his caveat about normal science.

Popper opens his critique of Kuhn by declaring that though Kuhn has misunderstood or misinterpreted him in places, they are very near in agreement. Popper goes on to compliment Kuhn as understanding Popper better than most other critics and to note that Kuhn's two main criticisms of Popper's work "are very important." The first of these criticisms concerns Popper's omission of Kuhn's normal science in favor of extraordinary or revolutionary science. While not admitting that the distinction between normal and extraordinary science or research "is as sharp as Kuhn makes it," Popper still willingly concedes that normal science, "in Kuhn's sense, exists" (52).

Although willing to admit its existence, Popper detests Kuhn's normal science and regards it "as a danger." He attributes it to poor teaching, declaring repeatedly that "the normal scientist," in his view, "has been taught badly" (Popper 52). Popper sees that scientist as "the victim of indoctrination" and further defines the "normal" scientist as described by Kuhn: "He has learned a technique which can be applied without asking why. . . . As a consequence, he has become what may be called an *applied scientist*, in contradistinction to what I should call a *pure scientist.* He is, as Kuhn puts it, content to solve puzzles" (Popper 53). Popper takes Kuhn's use of "puzzle" to mean a concern for the routine application of what one has learned. Using Kuhn's description of puzzle "as a

problem in which a dominant theory . . . is applied"--a paradigm in Kuhnian terminology, Popper argues: "The success of the 'normal' scientist consists, entirely, in showing that the ruling theory can be properly and satisfactorily applied in order to reach a solution to the puzzle in question" (53).

Popper reiterates his acknowledgment of the existence of normal science in the sense of Kuhn's definition. But in calling attention to the danger of such an attitude, Popper also believes that "Kuhn is mistaken when he suggests that what he calls 'normal' science is normal" (53). Especially troubled by what he sees as Kuhn's typology of scientists and scientific periods, Popper disagrees with Kuhn's analysis of a number of historical facts as well as Kuhn's characterization of science.

According to Popper, gradations between Kuhn's "normal scientist" and "extraordinary scientist" must exist. Further, the typology of scientists and periods "needs qualification" (54). Popper argues that Kuhn's "schema of 'normal' periods, dominated by *one* ruling theory (a paradigm in Kuhn's terminology) and followed by exceptional revolutions, seems to fit astronomy fairly well," but it does not describe the pattern of other sciences such as "the evolution of the theory of matter; or of the biological sciences since, say, Darwin and Pasteur" (Popper 54). Popper uses Kuhn's term, paradigm, "in a sense different from Kuhn's usage: to indicate not a *dominant theory,* but rather a *research programme* [sic]--a mode of explanation which is considered so satisfactory by some scientists that they demand its general acceptance" (55).

While acknowledging that he finds "Kuhn's discovery of what he calls 'normal' science most important," Popper is unwilling to admit that the history of science supports Kuhn's doctrine (55). Popper asserts: "This [Kuhn's] picture of the history of science clashes with the facts as I see them" (Popper 55). His strident objection to

Kuhn's vision of the history of science represents a key difference in the shape of their frameworks for scientific advance.

Turning to Kuhn's "Logic" paper, Popper notes that Kuhn uses "a logic of discovery" which differs radically from Popper's. Popper believes that "science is essentially critical; that it consists of bold conjectures, controlled by criticism, and that it may, therefore, be described as revolutionary." He contrasts his position with Kuhn's belief "in the domination of a ruling dogma over considerable periods" and lack of belief that "the method of science is, normally, that of bold conjectures and criticism" (Popper 55). Further, he charges Kuhn with making logical arguments, not psychological or historical. Popper sees Kuhn as suggesting that "the rationality of science presupposes the acceptance of a common framework," rationally dependent upon "something like a common language and a common set of assumptions" and requiring agreement on "fundamentals" for rational discussion and rational criticism. According to Popper, Kuhn's thesis is "widely accepted and fashionable"-- it is "the thesis of *relativism.* And it is a *logical* thesis" (Popper 56).

Popper's main point is that "a critical discussion and a comparison of the various frameworks is always possible." Kuhn suggests that language changes with the paradigm, thus rendering communication incommensurable across incompatible frameworks. Popper calls it "dogma--a dangerous dogma--that the different frameworks are like mutually untranslatable languages" (56). Although difficult, discussion does occur between people "brought up in different frameworks." Further, Popper sees the clash of competing cultures as stimulating "some of the greatest intellectual revolutions." Thus Popper declares that in science, "a critical comparison of the competing theories, of the competing frameworks, is always possible"; to deny this possibility "is a mistake." Popper argues: "In science (and only in science) can we say that we have made genuine progress: that we know more than we did before" (57).

In making such an argument, Popper necessarily attacks Kuhn's turn toward sociology and psychology as "surprising and disappointing." He asserts that the difference between his ideas and Kuhn's remains fundamentally one of logic. He responds to Kuhn's "Logic" paper by asserting that Kuhn's own "mistaken" arguments "go back to the thesis that the scientist is *logically forced* to accept a framework, since no rational discussion is possible between frameworks" (57). Ultimately, Popper concludes that "while the Logic of Discovery has little to learn from the Psychology of Research, the latter has much to learn from the former" (58).

Watkins's Assault on Normal Science

If Popper's work outside the colloquium can be seen as ignoring Kuhn's ideas about normal science and assigning some of its attributes to revolutionary episodes of scientific advance, then John Watkins directly assaults Kuhn's characterization in "Against 'Normal Science.'" Watkins notes that his response to Kuhn's "Logic" paper is "as much about Kuhn's book [Structure] as about the paper" (25). Though Watkins admits that Kuhn's work has met critical acclaim, he asserts that Kuhn's definition of normal science, seen especially in light of Popper's view, reflects an incorrect view "of scientific normalcy as a closed society of closed minds" (27).

Watkins addresses his discontent with Kuhn from a Popperian point of view, bringing out the "*larger* conflicts" between the positions of Kuhn and Popper. He considers Kuhn's idea of normal science "from a methodological point of view"--a methodology that, as Watkins understands it, "is concerned with science at its best, or with science as it should be conducted, rather than with hack science." Finally, Watkins rebuts Kuhn's claims that normal science "constitutes the essence of science" (27).

Watkins attacks Kuhn first for his comments about testing as puzzle-solving, rather than as testing of theories. He notes that Kuhn argues for normal science as "the normal condition of science" and for extraordinary science as "an abnormal condition." According to Watkins's understanding of Kuhn, "within Normal Science . . . the genuine testing of prevailing theories is rendered, in some rather mysterious psychological-cum-sociological way, impossible" (28). Such a view leads to the following fundamental disagreement between Kuhn and Popper:

> [The] condition which Kuhn regards as the normal and proper condition of science is a condition which, if it is actually obtained, Popper would regard as *un*scientific [sic], a state of affairs in which critical science had contracted into defensive metaphysics. Popper has suggested that the motto of science should be: *Revolution in permanence!* For Kuhn, it seems, a more appropriate maxim would be: *Not nostrums but normalcy!* (Watkins 28)

Watkins goes on to accuse Kuhn of reversing himself on the issue of Popper's emphasis "on the asymmetry between the falsifiability and the non-verifiability of scientific generalization" (28). He notes that Kuhn, in "Logic," adds that asymmetry plays a role in Structure, then argues that Kuhn actually refers to Popper's thesis in Structure in order "to *dismiss* that thesis as unrealistic, on the ground that in Normal Science there is no falsification of theories, while in Extraordinary Science the evidence which is taken as falsifying the paradigm which is being ushered out will *also* be taken as *verifying* the new paradigm which is being ushered in" (Watkins 29).

Continuing his attack, Watkins notes that Kuhn fails in Structure to advance any "demarcation-criterion for science," only setting aside Popper's falsifiability-criterion. In "Logic," Kuhn advances the puzzle-solving of normal science as a criterion

instead. Watkins restates Kuhn's alternative as: "Normal Science (in which there is not really any testing of theories) is genuine science; Extraordinary Science (in which genuine testing of theories does occur) is so abnormal, so different from genuine science, that it can hardly be called science at all" (Watkins 29). Thus, though Kuhn may claim that his and Popper's "line of demarcation . . . frequently coincide" because puzzle-solving is easily mistaken for testing, in reality, Kuhn and Popper define genuine science quite differently--what is science for one is hardly that for the other (Watkins 29).

At the heart of Watkins's rejection of Kuhn's characterization of testing as an unreliable "mark of a science" lies adherence to Popper's view that "the mark of a scientific theory is not that it has actually been tested but that it is test*able* [sic], the more testable the better (all things being equal)" (29-30). According to Watkins, Kuhn's insistence that anomalies and unsolved puzzles always exist means that the difference "between supporting, and failing to support, a puzzle-solving tradition is merely one of *degree*: There must be a critical level at which a tolerable turns into an intolerable amount of anomaly." The inability to identify that critical level, Watkins continues, results in "the sort of criterion that can only be used retrospectively: it entitles us to declare, *after* a paradigm-switch has occurred, that empirical pressure on the old paradigm *must* have been pretty intolerable" (30). Watkins interprets these conclusions to indicate that:

> [A] dominant theory may come to be replaced, not because of growing empirical pressure (of which there may be very little), but because a new and incompatible theory (inspired perhaps by a different metaphysical outlook) has been freely elaborated: a scientific crisis may have theoretical rather than empirical causes. (Watkins 31)

If this is so, then science would involve more "free thinking . . . than Kuhn supposes" (Watkins 31).

Watkins proceeds to argue against Kuhn's characterization of normal science, asserting that it is incapable of "giving rise to Extraordinary (or Revolutionary) Science" (31). He asks a series of rhetorical questions designed to demonstrate that while Kuhn may say that he agrees with Popper's rejection of scientific progress by accumulation or accretion, Kuhn's dependence upon normal science as "an orderly, undramatic, step by step" process mirrors progression by accretion (Watkins 32). In other words, Watkins accuses Kuhn of contradicting himself through an over dependence upon normal science.

Watkins suspects that one reason for Kuhn's dependence on normal science may lie in "sheer quantitative considerations." In "Logic," Kuhn attributes most of the work done to 'basic science' and notes that the extraordinary science preferred by Popper rarely occurs. Watkins also sees problems with Kuhn's demarcation-criterion excluding "certain intellectual disciplines that few of us want to call scientific." While Kuhn dismisses astrology, Watkins asserts that it closely fits Kuhn's definition of normal science: "The work is done under the aegis of a stable body of doctrine which is not discredited, in the eyes of astrologers, by predictive failures" (Watkins 32-33).

An even better reason for Kuhn's dependence on normal science, Watkins asserts, rests in Kuhn's sense of the scientific community as analogous to a religious community. Watkins argues:

> For [Structure] contains many suggestions, some explicit, others implicit in the choice of language, of a significant parallelism between science, especially Normal Science, and theology. Kuhn writes of a scientific education as a "process

> of professional initiation" which "prepares the student for membership in the scientific community. He says that "it is a narrow and rigid education, probably more so than any other *except perhaps in orthodox theology* [Watkins's emphasis]." He [Kuhn] also says that a scientific education involves the re-writing, in text-books, of history backwards, and that this indicates "one of the aspects of scientific work that most clearly distinguishes it from every other creative pursuit *except perhaps theology* [Watkins's emphasis]." (Watkins 33)

Watkins notes Kuhn's description of "the personal process of repudiating an old paradigm and embracing a new one. . .as a 'conversion experience'" dependent upon faith. Watkins suggests, then, "that Kuhn sees the scientific community on the analogy of a religious community and sees science as the scientists' religion." Such a position may account for Kuhn's elevation of normal science over extraordinary science, "for Extraordinary Science corresponds, on the religious side, to a period of crisis and schism, confusion and despair, to a spiritual catastrophe" (Watkins 33).

Watkins challenges the Kuhnian pattern, declaring that "a new paradigm never could emerge from Normal Science as characterized by Kuhn." He bases his objection upon "the possibility of the emergence of a new paradigm at the end of a period of Normal Science" and focuses attention "on the *very first* scientist to take up a new paradigm." To prove his challenge to Kuhn's cyclic pattern, Watkins returns to a number of Kuhn's theses about paradigms, relabeling the theses somewhat sarcastically as "Paradigm-Monopoly," "No-Interregnum," "Incompatibility," "Gestalt-Switch," and "Instant-Paradigm." Under Kuhn's "Paradigm-Monopoly" thesis, "It is in the nature of a paradigm to enjoy a monopoly in its hold on a scientist's thinking"; in Watkins's words, a scientist "cannot, while under the sway of one paradigm, seriously entertain a rival paradigm." Under the "No-Interregnum" thesis, a scientist "abandons one paradigm only to embrace a new

one"; little or no time exists between the rejection of old and the adherence to the new. The "Incompatibility" thesis involves the clash between the old and new paradigms with Watkins's reading of Kuhn's idea of incommensurability to mean that a "new paradigm will be incompatible with the paradigm it supersedes" (34). The "Gestalt-Switch" thesis label grows out of Kuhn's endorsement of the idea that "a scientist's change-over from an old paradigm to a new one must be pretty swift and decisive" (Watkins 35), a "conversion" experience that Kuhn even equates to 'a relatively sudden and unstructured event like the gestalt switch' (Qtd in Watkins 35).

Finally, under the "Instant-Paradigm" thesis, Watkins argues that the new paradigm enjoys no pre-history as implied by the "Gestalt-Switch" thesis. The scientist's "*switch* to the new paradigm must be regarded as the very same thing as his *invention* of the new paradigm," assuming that the new paradigm "was invented inside the scientific community and not imported from extra-scientific sources." In examining these five theses, Watkins concludes that a new paradigm is one that "is immediately powerful enough to induce our scientist to turn against the well-articulated and unrefuted paradigm that has dominated his scientific thinking hitherto," and it is sufficiently "large and definite enough for its striking potentialities to be fairly apparent to its inventor" (35).

Having built a straw man out of some of Kuhn's theses, Watkins knocks it down. He declares that the "Instant-Paradigm" thesis is "barely credible on psychological grounds" and "must be rejected" (Watkins 36). If it is rejected, then at least one of the other theses upon which it is grounded, excluding the "Gestalt-Switch" thesis, must also be rejected. Watkins argues that the "Incompatibility" thesis, when severed from Kuhn's notion of incommensurability and applied historically, is in accord with Popper's views. He writes that scientists should accept the "Incompatibility" thesis, asserting: "Popper says, in effect, that major theoretical advances in science *ought* to have a revolutionary

character; and Kuhn says, in effect, that they *do* have a revolutionary character" (Watkins 37).

Accepting the "Incompatibility" thesis leads Watkins to the other two and his ultimate conclusion. The "Paradigm-Monopoly" thesis says that a scientist's thinking is, "at any one time, dominated by *one* paradigm"; the "No-Interregnum" thesis holds that the "scientist's professional thinking is *always* paradigm-dominated" (Watkins 37). Watkins finds these two theses bound together. He maintains:

> [Since] it takes time--a matter of years rather than of hours--to develop a potential new paradigm to the point where it may challenge an entrenched paradigm, heretical thinking must have been going on for a long time before paradigm change can occur. This means that it is not true that a reigning paradigm exercises such a monopolizing sway over scientists' minds that they are all unable to consider it critically, or to toy with (without necessarily embracing) alternatives to it. It means that the scientific community is not after all, a closed society whose chief characteristic is "the abandonment of critical discourse." (Watkins 37)

Watkins thus challenges a number of key concepts expressed by Kuhn in Structure and further articulated in "Logic" in an effort to clarify Kuhn's position vis-à-vis Popper's. While acknowledging that many of Kuhn's ideas coincide with Popper's, Watkins finds fault with the notion of the dogmatic nature of paradigm-driven normal science. He adheres closely to Popper's contention of what science ought to be--that is, revolutionary in nature--and discounts Kuhn's normal science as "hack science" (27). Watkins argues against the puzzle-solving of normal science and seems to deny the

efficacy of Kuhn's notion of paradigm, asserting that Popper's testing-criterion and view of extraordinary science is correct.

Toulmin's Response to Kuhn's "Logic"

Unlike Watkins, Toulmin chooses to address Kuhn's remarks in "Logic" not as a critique of Popper's views, but "as a further installment in the development of Kuhn's analysis of scientific change" ("Distinction" 39). By insisting "on the 'revolutionary' character of some changes in scientific theory," Kuhn has "compelled many people to face for the first time the full profundity of the conceptual transformations which have, at times, marked the historical development of scientific ideas" ("Distinction" 39). Toulmin notes that Kuhn's original position can only serve as a starting point because of his dependence upon "built-in rhetorical exaggeration" in his 1961 "Dogma" essay.

Toulmin objects to Kuhn's use of the term "revolution" as "an explanatory *concept."* He declares that while revolution has some usefulness as a "descriptive *label,*" its usefulness for explanation is limited. He contrasts Kuhn's use of the term with the use that political scientists have made of it, noting that as explanation, "the difference between normal and revolutionary change in the political sphere turned out after all to be only one of degree." Toulmin argues that Kuhn's use of the term appears "to demand similar qualifications," especially in differentiating "between the kinds of change taking place during 'normal' and 'revolutionary' phases of scientific development . . . [as], at the intellectual level, absolute." He suggests that Kuhn goes too far in Structure in making such a claim, "implying the existence of discontinuities in scientific theory far more profound and far less explicable than any which ever in fact occur" (Toulmin, "Distinction" 41).

Toulmin goes on to critique Kuhn as appearing to withdraw in his "Logic" paper from the position distinguishing between normal and revolutionary science established in Structure. He argues that the net effect of Kuhn's apparent withdrawal is to "demolish entirely his original distinction between 'normal' and 'revolutionary' phases" ("Distinction" 41). He suggests that Kuhn has moved away from "the original 'normal'/'revolutionary' dichotomy" in which the "occurrence of a 'scientific revolution' (it appeared) threw attempts at communication so completely out of joint that incomprehension was *guaranteed.*" Kuhn continues to insist "that the development of scientific ideas involves, at times, changes so drastic that they introduce profound conceptual incongruities between the ideas accepted by successive generations of scientists." Toulmin agrees that "No theory of scientific growth and development would be adequate which did not recognize, and do justice to these intellectual discontinuities." But Toulmin argues that the problem with Kuhn's earlier accounts--both the 1961 "Dogma" essay and the 1962 Structure--rests with Kuhn's insistence that such revolutionary discontinuities are absolute, creating "a situation in which there [is], inevitably, complete incomprehension at the theoretical level between supporters of the older and newer versions of scientific thought" ("Distinction" 43).

Using as an example a supporter of the older Newtonian dynamics and one supporting the newer Einsteinian version, Toulmin avers:

> This incomprehension was inescapable because, when it came to organizing their experience, the two men shared no common language, no common viewpoint, nor even a common *gestalt.* In consequence, neither the [older] Newtonian language nor the [newer] Einsteinian language would suffice to explain either point of view to supporters of the other. (Toulmin, "Distinction" 43)

Toulmin again reduces Kuhn's distinction between normal and revolutionary science as "rhetorical exaggeration" in the same manner as Kuhn's earlier use of dogma. Continuing with his example of physicists during the transition period between Newtonian and Einsteinian thought from 1890 to 1930, Toulmin insists that some evidence of the complete breakdown in scientific communication for which Kuhn argues would manifest itself in the "experience of the men in question" and that we would be able to document that experience. Toulmin concludes, "If the conceptual change involved in the transition was as deep as Kuhn claimed, these physicists at any rate appeared curiously unaware of the fact" ("Distinction" 44). They provide reasons for changing their positions, but do not attribute those changes to Kuhn's "conversion" or "gestalt switch." Toulmin states:

> Accordingly, one may concede that the development of scientific thought does involve important conceptual discontinuities, and that the conceptual systems which displace one another within a scientific tradition may often be based upon quite different, even incongruous principles and axioms; but we must beware of going all the way with Kuhn's original "revolutionary" hypothesis. For the displacement of one system of concepts by another is itself something that happens for perfectly good reasons, even though these particular "reasons" cannot themselves be formalized into still broader concepts, or still more general axioms. For what is presupposed by both parties in such a debate--both those who cling on to the older view, and those who put forward a new one--is not a common body of principles and axioms: rather it is a common set of "selection procedures" and "selection rules," and these are not so much "scientific principles" as "principles "constitutive of science." (Toulmin, "Distinction" 44)

Toulmin goes on to assert that "the occurrence of a 'scientific revolution' no longer amounts to the dramatic interruption in the 'normal' continuous consolidation of science: instead, it becomes a mere 'unit of variation' within that very process of scientific change." He argues that the core of Kuhn's theory distinguishing between normal and revolutionary change depends upon acceptance of Kuhn's criterion of absoluteness. Taking away that criterion for recognizing that revolutionary change has occurred destroys "the distinctive element in Kuhn's theory." Toulmin declares that "once we acknowledge that *no* conceptual change in science is ever absolute, we are left with only a sequence of greater and lesser conceptual modifications differing from one another in degree" ("Distinction" 45).

In effect, Toulmin argues for a new theory of scientific advance, one that "will have to go beyond . . . Kuhn's concept of 'revolutions'" ("Distinction" 45). He states:

> Like Professor Kuhn, I believe that this new theory--when we have it--will have to be based in part on the results of new empirical studies of the actual growth and development of science; and that, as a result, it will tend to bring the logic of science closer together with its sociology and psychology. Yet it will remain as important as ever . . . to avoid *identifying* the logical criteria for appraising new scientific hypotheses with generalizations about the actual practice of scientists, taken either individually or collectively as professional groups. (Toulmin, "Distinction" 45)

He calls for treating "Kuhn's small scale 'micro-revolutions'" as units of variation, not "as units of effective *change* in scientific theory." Additionally, Toulmin suggests treating scientific change "as a special case of a more general phenomenon of 'conceptual evolution,'" one involving three major evolutionary aspects: quantity, direction, and

selection criteria. He writes that the *"quantity* of innovation going on in a given field at any time can be distinguished from the *direction* in which this innovation is predominantly tending." Further, "both of these can be distinguished, in turn, from the *selection criteria* determining which variants are perpetuated within the tradition" (Toulmin, "Distinction" 46).

Ultimately, Toulmin is unwilling to explore the full ramifications of "an 'evolutionary' theory of scientific change," one that contrasts with Kuhn's views on revolutionary change ("Distinction" 46). He concludes that if "the "micro-revolutions" of Kuhn's present position are the units of all scientific innovation, then the idea of "scientific revolution" will have to follow that of "political revolutions" out of the category of explanatory concepts and into that of mere descriptive labels" (Toulmin, "Distinction" 46). Thus, Toulmin takes a position against Kuhn's theory as an adequate explanation of scientific development and argues for a reductionist view of Kuhnian theory.

Toulmin's position on Kuhn's theory as "provisional" continues a theme Toulmin introduced in his criticism of Kuhn's 1961 "Dogma" essay. He insists that Kuhn's position has continued to evolve, showing movement away from "rhetorical exaggeration" for effect towards a new theory of scientific development that may unite the logic, sociology, and psychology of scientific advance. Toulmin is more interested in an evolutionary view of science, a view he suggests is contrary to Kuhn's dependence upon revolutionary change.

Masterman and Kuhn's Concept of Paradigm

While Watkins and Toulmin attack Kuhn outright in Criticism, Margaret Masterman offers perhaps the strongest defense of Kuhn's theory during the early period following

its 1962 publication in Structure. In "The Nature of a Paradigm," Masterman elaborates on Kuhn's notion of paradigm and demonstrates the urgency, as well as philosophical importance, of discovering "what a Kuhnian paradigm is" (60). As a point of departure, she accepts Kuhn's sense of normal science, arguing:

> That there is normal science--and that it is exactly as Kuhn says that it is--is the outstanding, the crashingly obvious fact which confronts and hits any philosophers of science who set out, in a practical or technological manner, to do any scientific research. It is because Kuhn--at last--has noticed this central fact about all real science (basic research, applied, technological, are all alike here), namely that it is normally a habit-governed, puzzle-solving activity, not a fundamentally upheaving or falsifying activity (not, in other words, a *philosophical* activity [Popper's view]), that actual scientists are now increasingly reading Kuhn instead of Popper: to an extent, indeed, that, in new scientific fields particularly, "paradigm" and not "hypothesis" is now the "O. K. word." (Masterman 60)

Given the existence of normal science as Kuhn describes it, Masterman turns to what she sees as the two "vital difficulties" arising out of Kuhn's theory--"his conception of verification in experience (or the absence of it)" and "his conception of paradigm." She focuses on the second point and notes that Kuhn's critics "have never taken the trouble to find out" what Kuhn's sense of paradigm is even though "Kuhn's whole general view of the nature of scientific revolutions depends on it" (Masterman 61).

Masterman acknowledges that Kuhn has contributed to the confusion by using his term "in not less than twenty-one different senses in his [Structure], possibly more, not less" (61). She goes on to list the ways Kuhn describes a paradigm in his book,

classifying those twenty-one senses of "paradigm" into three main groups: the metaphysical paradigms or metaparadigms, the sociological paradigms, and the artefact [sic] or construct paradigms. The first group, used mainly by philosophical critics of Kuhn, consists of usages such as a set of beliefs, a myth, a successful metaphysical speculation, a standard, or a map. Masterman writes that Kuhn clearly has "a metaphysical notion or entity, rather than a scientific one" in mind. The sociological group involves the sense of "paradigm," for example, as "a universally recognized scientific achievement"; the group depends upon some sense of community for recognition. Masterman argues that the third group is more concrete, consisting of textbooks, tools, and instrumentation in one sense (artifacts) and in other senses linguistically, illustratively, and psychologically as constructs (65).

Masterman chooses to address the sociological group of paradigms first, arguing that: "Seen sociologically (as opposed to being seen philosophically) a paradigm is a set of scientific habits." She notes that the only explicit definition of a paradigm which Kuhn ever provides "is in terms of . . . habits"--lumped together under the label of "a concrete scientific achievement." She quotes Kuhn's definition of normal science from page ten of Structure: "Normal science is research based upon one or more past scientific achievements that some particular community acknowledges for a time as supplying the foundation for its further practice" (Qtd in Masterman 66). Masterman then repeats the two key characteristics that Kuhn assigns to these achievements as paradigms: first, they are "sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity," and second, they are "sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to solve" (Qtd in Masterman 66). Achievements with these attributes appear prior to theory according to Masterman's interpretation of Kuhn. She argues that, "for Kuhn, something sociologically describable, and above all, concrete, already

exists in actual science, at the early stages, when the theory is not there" (Masterman 66).

Kuhn's non-equation of paradigm with theory is important. Masterman remarks that Kuhn "never, in fact equates 'paradigm,' in any of its main senses [the three groups that she identifies], with 'scientific theory'" (66-67). She goes on to explain:

> For his metaparadigm is something far wider than, and ideologically prior to, theory: i.e. a whole *Weltanschauung*. His sociological paradigm, as we have seen, is also prior to theory, and other than theory, since it is something concrete and observable: i.e. a set of habits. And his construct-paradigm is less than a theory, since it can be something as little theoretic as a single piece of apparatus: i.e. anything which can cause actual puzzle-solving to occur. (Masterman 67)

Masterman's analysis allows her to conclude that Kuhn is saying something "new" about science, a view not necessarily shared by many of Kuhn's critics. Further, she concludes from her own examination of Structure that much of the criticism of Kuhn is misplaced or "false." She suggests that "the very differences between Kuhn's 'new image' of science (or, as I [Masterman] shall from now on call it, the 'paradigm view' of science) and all other philosophies of science" known to her is "causing Kuhn's book to be so widely read" (67).

What is it about Kuhn's paradigm view of science that Masterman finds so successful? For one, she suggests that it establishes "the scientificness [sic] of science." She proceeds to outline what she sees as "the distinctive and revolutionary logical characteristics of Kuhn's paradigm," stripping it of its "sociological environment" and looking at it "generally and philosophically." She derives these logical characteristics

"from the paradigm's basic property," which she calls "concreteness or 'crudeness'" (Masterman 67).

Masterman continues by exploring the philosophic consequences of Kuhn's insistence on normal science and the influence of paradigms. She claims: "Philosophically speaking, a paradigm is an artefact [sic] which can be used as a puzzle-solving device, not a metaphysical world view" (68). She is unwilling to be satisfied with "a purely sociological sense" of Kuhn's paradigm, a definition of which she suggests "is bound to be circular." Accordingly, she argues that the "primary sense of 'paradigm,' clearly, has got to be a philosophic one; and the paradigm has got to exist prior to the theory." For evidence, Masterman points to new scientific fields and notes that "a very great deal of high-powered scientific activity" aims at making "the right choice of the moment when it will be worth the labor to construct it." She argues that the alternative to constructing a theory is "'just going on as we are now'; that is, with some trick, or embryonic technique, or picture, *and an insight that this is applicable in this field."* Further, she declares: "And it is this trick, plus this insight, which together constitute a paradigm" (Masterman 69). Masterman asserts: "Thus the real problem in getting a philosophy of new science, is to describe philosophically the original trick, or device, on which the sociological paradigm (i.e. the set of habits) is itself founded" (70).

According to Masterman, it is the sense of the construct or artefact [sic] paradigm, rather than the metaphysical or metaparadigm, that is the most fundamental when considering function within Kuhn's normal science. She emphasizes bluntly in italics: *"For only with an artefact* [sic] *can you solve puzzles."* She argues that Kuhn "has a steady, literal and very concrete idea of what he means by the puzzle-solving activity of normal science" (Masterman 70). According to Masterman's reading of Kuhn:

> A normal scientific puzzle always has a solution which is guaranteed by the paradigm, but which it takes ingenuity and resourcefulness to find. Typically, the solution is known beforehand, as with any other puzzle, but the step-by-step route to it is not. The normal scientist is a puzzle-solving addict; it is in this puzzle-solving--not just vague "problem-solving," but puzzle-solving--that normal science prototypically consists. And a puzzle is always an artefact [sic]. (Masterman 70)

She concludes that "for any puzzle which is really a puzzle to be solved by using a paradigm, this paradigm must be a construct, an artefact [sic], a system, a tool; together with the manual instructions for using it successfully and a method of interpretation of what it does" (Masterman 70).

Masterman moves away from her discussion of normal science to address what Kuhn says about the preparadigm stage of science. Here she disagrees with Kuhn and posits instead three relevant states: non-paradigm science, multiple-paradigm science, and dual-paradigm science. Masterman defines non-paradigm science as "the state of affairs right at the beginning of the process of thinking about any aspect of the world, i.e. at the stage where there is no paradigm" (73). She reports that Kuhn sees all facts as equally relevant at this stage and repeats Kuhn's claim that 'though the field's practitioners were scientists, the net result of their activity was something less than science' (Qtd in Masterman 74). Masterman goes on to provide details of Kuhn's description of the stage, concluding that: "In short, non-paradigm science is barely distinguishable, if at all, from 'the philosophy of' the relevant subject" (74).

Multiple-paradigm science contrasts sharply with the earlier non-paradigm stage. At multiple-paradigm stage, too many paradigms compete for domination. Masterman suggests that such a stage can be seen currently in the psychological, social and

information sciences. The stage is characterized by paradigmatic sub-fields supporting narrow views. Masterman suggests that:

> Here, within the sub-field defined by each paradigmatic technique, technology can sometimes become quite advanced, and normal research puzzle-solving can progress. But each sub-field as defined by its technique is so obviously more trivial and narrow than the field as defined by intuition, and also the various operational definitions given by the techniques are so grossly discordant with one another, that discussion on fundamentals remains, and long-run progress (as opposed to local progress) fails to occur. (Masterman 74)

Long-run progress according to Masterman only occurs "when someone invents a deeper, though cruder paradigm, which gives a more central insight into the nature of the field, though restricting it and making research into it more rigid, esoteric, precise." Masterman suggests that "multiple-paradigm science is full science, on Kuhn's criteria; with the proviso that these criteria have to be applied by treating each sub-field as a separate field" (74).

Dual-paradigm science, for Masterman, characterizes the stage implied by Kuhn's concept of scientific crisis and paradigm shift. Kuhn argues that during this period "there are always two competing paradigms struggling for mastery" (Masterman 74). Masterman's dual-paradigm stage shares many of the characteristics of Kuhn's pre-paradigm science, only more narrow and restricted to the competing schools.

Masterman suggests that the reason for Kuhn's failure to "distinguish non-paradigm science from multiple-paradigm science, and therefore . . . to connect multiple-paradigm science with dual-paradigm science, is due partly to confusion." She asserts about Kuhn that, "having said that there can be a sort of scientific research without

paradigms, he then adds, 'or at least without any so unequivocal and binding as the ones named above' (p. 11), as though these two states of affairs were identical." Masterman further admonishes Kuhn for not paying enough heed to technology, "which exists plentifully and sometimes excessively in multiple-paradigm science, but only non-relevantly, if at all, in non-paradigm science" (75).

Masterman returns to her earlier point that "a puzzle-solving paradigm . . . has got to be a 'concrete way of seeing.'" She notes that Kuhn "repeatedly compares the switch from one paradigm to another to the operation of 're-seeing' and ambiguous gestalt-figure"; two alternative "ways of seeing" exist. Masterman posits that Kuhn is really making an assertion about the use of an "artefact," not about its nature. She writes: "that being a picture of one thing, it is used to represent another." Kuhn's "artefact" must be used analogically as "an organized puzzle-solving gestalt which is itself a 'picture' of something, *A*, if it is then to be applied, non-obviously, to provide a new 'way of seeing' something else, *B* ." In other words, Kuhn's "way of seeing," his paradigm, "is a concrete 'picture' of something, *A*, which is used analogically to describe a concrete something else, *B*" (Masterman 76-77). Masterman explains: "That is, the trick which, as I said earlier, starts off every new science, is that a known construct, an artefact [sic], becomes a "research vehicle," and at the same moment, if successful, it becomes a paradigm, by being used to apply to new material, and in a non-obvious way" (Masterman 78). She asserts that Kuhn's paradigm "thus has two kinds of concreteness, not one: the concreteness which it brought with it through being a 'picture' of *A*, and *the second concreteness which it has now acquired, though becoming applied to B*" (Masterman 78).

"If a paradigm has got to have the property of concreteness," as Masterman concludes, "this means that it must either be, literally, a model; or, literally, a picture; or, literally, an analogy-drawing sequence of word-uses in natural language; or, some

combination of these" (79). She favors drawing a paradigm as a "crude analogy," which she further defines as possessing a series of logical characteristics: "a crude analogy is finite in extensibility"; "it is incomparable with any other crude analogy"; and "it is extensible only by an inferential process of 'replication'" (Masterman 79).

Masterman's delineation of logical characteristics leads into a discussion of extensibility and Kuhn's description of anomaly and crisis. She finds Kuhn's description of "a paradigm breaking down, by the emergence within it of anomaly which deepens into crisis . . . at once illuminating and realistic, when applied to a new science" (Masterman 82). She concludes that the paradigm eventually fails or "goes bad on you, if it is stretched too far, producing conceptual inconsistency, absurdity, misexpectation, disorder, complexity and confusion, in exactly the same way as a crude analogy does, if pressed to far, say, in a poem." Masterman credits Kuhn with being the first philosopher of science to describe "this deterioration" or failure of a paradigm or governing theory (Masterman 83).

Ultimately, Masterman concludes that historians of science cannot backtrack to the time before Kuhn and his immediate predecessors began raising objections to the accretion image of science. She urges that: "The history of science, by its nature as part of the history of ideas, has got to be a discipline which helps scientists to get a deeper insight into the real nature of their own science." To retreat from Kuhn's paradigm image of science is to run the risk of disaster; to go forward requires re-examining "what is true of analogy in light of what Kuhn has shown to be true of paradigms" (Masterman 88).

The greatest value of Masterman's criticism of Kuhn's work rests in her characterization of paradigms, her examination of Kuhn's notion of normal science, and her division of Kuhn's preparadigmatic state into non-paradigm, multiple-paradigm, and dual-paradigm science. She addresses the confusion that Kuhn raises by his own

inexact use of the term, "paradigm," and suggests that its significance lies not in its sociological sense--which critics of Kuhn such as Watkins and Toulmin seize upon--but in its concrete sense as a construct or artefact [sic] that can be used as a puzzle-solving device. She reiterates the importance Kuhn places upon normal science, criticizing others for not acknowledging or accepting the significance of his recognition of normal science. Normal science happens; it is the usual form in which science advances over long periods of history.

Masterman's analysis of Kuhn's preparadigmatic state and its place in the history of science logically extends the meaning of "preparadigmatic" by deconstructing it and characterizing the additional stages of scientific advance. Her stages of multiple-paradigm and dual-paradigm science seem to better define Kuhn's characterization of competing schools before communal commitment to a single paradigm. She ably describes the clash between an older paradigm and a newer paradigm struggling to replace it in a mature science in crisis, acknowledging the correctness of Kuhn's views. At the same time, Masterman suggests the provisional nature of Kuhn's observations and identifies some of his biases. While she does not agree with everything that Kuhn hypothesizes, Masterman supports much of his position and acknowledges that the history of science has changed significantly as a result of Kuhn's contributions.

Feyerabend's Thoughts on Kuhn's Ideology

Paul Feyerabend takes a direction different from that of Masterman. In "Consolations for the Specialist," Feyerabend disagrees with Kuhn's theory and argues against the general ideology that he believes informs Kuhn's thinking. He accuses Kuhn of professing an ideology that can "only give comfort to the most narrowminded and most conceited kind of specialism" (Feyerabend 197). He attacks Kuhn's ideology as tending to "inhibit the

advancement of knowledge" and "to increase the anti-humanitarian tendencies which are a disquieting feature of much of post-Newtonian science" (Feyerabend 197-98). Given his association with Kuhn in 1960 and 1961 and his disagreement with Kuhn's ideology, it is hardly surprising that Feyerabend's criticism of Kuhn is severe. He draws upon Kuhn's 1961 "Dogma" essay as well as Structure for much of his criticism. To cast doubt upon Kuhn's ideas, Feyerabend uses rhetorical questioning to the extent that his essay reads more like a litany of questions than a comment on Kuhn's theory.

Feyerabend opens his attack on Kuhn by declaring that his writings are ambiguous. He asserts that Kuhn's work can be interpreted as supporting both methodological prescriptions "which tell a scientist how to proceed" and a description "of those activities which are generally called 'scientific'" (Feyerabend 198). He argues that Kuhn must have intended to be ambiguous in order "to fully exploit its propagandistic potentialities" (Feyerabend 199). He declares:

> He [Kuhn] wants on the one side to give solid, objective, historical support to value judgments which he just as many other people seem to regard as arbitrary and subjective. On the other side he wants to leave himself a safe line of retreat: those who dislike the implied derivation of values from facts can always be told that no such derivation is made and that the presentation is purely descriptive. (Feyerabend 199)

Thus Feyerabend claims that Kuhn deliberately obscures his meaning, allowing a reading of his work that provides "a historico-scientific justification for the ever growing need to identify with some group" (199).

Next, Feyerabend denigrates Kuhn's notion of puzzle-solving as a criterion of science and registers his opposition to Kuhn's conception of normal science. To make his point,

he reduces Kuhn's conception of normal science to absurdity, comparing it with "organized crime." Feyerabend writes: "Every statement which Kuhn makes about normal science remains true when we replace 'normal science' by 'organized crime'; and every statement he has written about the 'individual scientist' applies with equal force to, say, the individual safebreaker" (Feyerabend 200).

By making such a comparison, Feyerabend implies that Kuhn's theory is too general in one sense to apply to science because it can be applied to almost anything--even organized crime. Kuhn is guilty of not taking the aim of science into account in his discussion of normal science.

Next, Feyerabend addresses the structure and function of normal science and the relationship of normal science to revolutions. He implies that Kuhn's account of normal science--what Feyerabend calls "the pedestrian activity associated with a 'mature' science" or "the *exclusive choice* of one particular set of ideas, the monomaniac concern with only one single point of view" (201)--influences the substantiality and content of scientific ideas negatively. Further, Kuhn's definition of scientific revolution depends upon the acceptance of normal science--a view with which Feyerabend completely disagrees.

Feyerabend charts three major difficulties with Kuhn's concept of revolutions. First, he argues that Kuhn cannot establish the desirability of his brand of revolutions. According to Feyerabend: "Revolutions bring about a *change* in paradigm. But following Kuhn's account of this change, or 'gestalt-switch' as he calls it, it is impossible to say that they have led to something better" (Feyerabend 202). Since pre- and post-revolutionary paradigms are incommensurable, Feyerabend argues that one cannot assert that one is better than the other.

Second, Feyerabend asserts that the nature of the relationship between normal science and revolutions, if closely examined, reveals structural flaws that "we do not

want to condone" (Feyerabend 202). He attacks Kuhn for making an argument for the tendency of a scientist to support or defend a particular theoretical view even in the face of evidence to the contrary--an idea that Feyerabend refers to as the principle of tenacity. Under Kuhn's theory, "scientists must develop methods which permit them to retain their theories in the face of plain and unambiguously refuting facts, even if testable explanations for the clash are not immediately forthcoming" (Feyerabend 205). If the principle of tenacity is correct, scientists do tend to cling tenaciously to their theories.

Feyerabend returns to normal science in his third point about revolutions. He questions the existence of normal science, raising the suspicion that "normal or 'mature' science, as described by Kuhn, *is not even a historical fact."* Feyerabend argues that normal periods of science in the Kuhnian sense, "if they ever existed, cannot have lasted very long and cannot have extended over large fields either" (207). Anomaly exists even during periods governed by a single paradigm. If the majority of scientists follow the paradigm, it is the minority, engaged in proliferating alternatives that accommodate anomaly, that spur revolutions. If so, Feyerabend asserts that Kuhn's account "which *temporally separates* periods of proliferation [extraordinary science] from periods of monism [normal science] altogether collapses" (Feyerabend 208). Feyerabend concludes that the puzzle solving of Kuhn's normal science is not "responsible for the growth of our knowledge"; instead, he suggests that responsibility belongs to "the interplay of various tenaciously held views." Accordingly, "it is the invention of new ideas and the attempt to secure for them a worthy place in the competition that leads to the overthrow of old and familiar paradigms" (Feyerabend 209). When Kuhn confers "some dignity even on the most boring and pedestrian part of the scientific enterprise" --that is, normal science--he is wrong. If Kuhn is wrong about normal science, then he must also be wrong about scientific revolutions.

Feyerabend is not content with simply declaring that Kuhn is wrong. He goes on to attack Kuhn's values. Feyerabend insists that "the happiness and the full development of an individual human being is now as ever the highest possible value." He argues that Kuhn's conception of a normal or mature science, by its dependence upon a single paradigm, is too dogmatic, too authoritarian, and too narrowminded; it leads to a closing of the mind. Feyerabend thus determines: "Such an enterprise [normal science] is not only ill-conceived and non-existent; its defence [sic] is also incompatible with a humanitarian outlook" (210).

Feyerabend's criticisms echo those of Watkins and Toulmin. He attacks Kuhn's notion of normal science. In discrediting that notion, he also discredits Kuhn's whole historical conception of science as a linear series of normal periods interrupted by occasional changes in paradigm. It seems that Feyerabend's attack is more rhetorical and dependent upon casting doubt on Kuhn's ideas than on effectively rebutting them. He offers little proof beyond the series of rhetorical questions that he raises. Further, his attack seems to shows much of his own ideological bias, one that promotes a pro-humanity, rather than a pro-science, approach to the advancement of knowledge and focuses on individual potential rather than institutional or community sociology.

In many ways, Feyerabend's attacks grow out of his initial disagreement with Kuhn's theory conceived during his association with Kuhn just prior to the publication of Structure. In "Consolations for the Specialist," Feyerabend notes that in 1960 and 1961, he enjoyed "the good fortune of being able to discuss with him [Kuhn] various aspects of science" while Kuhn was a member of the philosophy department at the University of California in Berkeley (197). Those opportunities led Feyerabend to question both the tenets of Kuhn's theory and its ideological underpinnings. He finds both wanting.

Kuhn Reflects on His Critics

The criticisms of scholars such as Watkins, Toulmin, Feyerabend and Masterman serve as a springboard for Kuhn's reply in "Reflections on My Critics," the final essay in Lakatos and Musgrave's Criticism and the Growth of Knowledge. Written in 1969 about the same time as his "Postscript--1969" for the upcoming second edition of Structure, "Reflections" contains specific replies to some of Kuhn's critics' most salient points. Section six of Kuhn's "Reflections" essay suggests some of the continuing evolution of his theory within the debates of his own profession. I consider the suggestions in section six more specifically in my next section, "Adding to Theory," in conjunction with two other Kuhn essays, "Second Thoughts on Paradigms" written earlier in 1969 and "Postscript--1969" written after "Reflections" in the same year.

Kuhn opens his response in "Reflections" with the declaration that much of Criticism "testifies to what I [Kuhn] have described as the gestalt-switch that divides readers of my [Structure] into two groups." Kuhn asserts the collection of essays in Criticism "provides an extended example of . . . partial or incomplete communication--the talking-through-each-other that regularly characterizes discourse between participants in incommensurable points of view." Kuhn goes on to suggest that the collection also "provides a developed example of a minor culture clash, of the severe communication difficulties that characterize such clashes, and of the linguistic techniques deployed in the attempt to end them." He remains "convinced" that much of the fire expressed by his critics in Criticism "is frequently misplaced" and therefore addresses a number of those criticisms directly ("Reflections" 231-32).

Kuhn divides the criticisms in the Lakatos and Musgrave collection, excluding Masterman's, into three groups which he labels "method, normal science, and mob rule" ("Reflections" 234). He bases the first group primarily on his critics' perceptions of

methodological differences, for example--"logic versus history and social psychology; normative versus descriptive." Although Kuhn dismisses such methodological differences, preferring instead to write about "substance rather than method," he still devotes a section of "Reflections" to addressing the methodological issues raised by Popper, Watkins, and others in the collection ("Reflections" 233).

Many of Kuhn's disparagers in Criticism claim that history and social-psychology "are not . . . a proper basis for philosophical conclusions." Kuhn agrees that such fields "are weak reeds from which to weave a philosophy of science" ("Reflections" 235), but asserts that the observations of historians and sociologists still have relevance. Calling himself an "unrepentant Popperian" ("Reflections" 236), Kuhn notes that Popper's own writings "are crowded with historical examples and with generalizations about scientific behavior" ("Reflections" 235). Kuhn goes on to seek out similarly structured arguments used by his critics. If others make claims using material and arguments structured similar to Kuhn's, they have no place criticizing Kuhn for doing so.

In another defense against methodological criticism, Kuhn willingly admits that some of his theory is "irreducibly sociological, at least at this time" ("Reflections" 237). Specifically, he summarizes the problem of theory-choice:

> [Take] a *group* of the ablest available people with the most appropriate motivation; train them in some science and in the specialties related to the choice at hand; imbue them with the value system, the ideology, current in their discipline (and to a great extent in other scientific fields as well); and, finally, *let them make the choice.* If that technique does not account for scientific development as we know it, then no other will. There can be no set of rules of choice adequate to dictate desired *individual* behaviour [sic] in the concrete cases that scientists will meet in the course of their careers. Whatever scientific

process may be, we must account for it by examining the nature of the scientific group, discovering what it values, what it tolerates, and what it disdains. (Kuhn, "Reflections" 238)

Kuhn suggests that those critics who dismiss the sociological base of his position fail to appreciate the nature of the group as opposed to the individual mind. He argues that groups share ideals and commitments that affect behavior, but the effects of such a shared ideology are not uniform. Kuhn notes:

> Given a group all the members of which are committed to choosing between alternative theories and also to considering such values as accuracy, simplicity, scope, and so on while making their choice, the concrete decisions of individual members in individual cases will nevertheless vary. Group behaviour [sic] will be affected decisively by shared commitments, but individual choice will be a function also of personality, education, and the prior pattern of professional research. (Kuhn, "Reflections" 241)

Kuhn asserts that such variability is a strength of his theory especially when considering problems "of crisis and of theory-choice" ("Reflections" 241).

The second and most apparent set of criticisms that Kuhn identifies refers to differences over Kuhn's concept of the nature and function of normal science. In response to much of the criticisms of this group, Kuhn notes that a large part of Criticism "calls forth some of the oddest rhetoric: normal science does not exist *and* is uninteresting." Kuhn concludes that casting away the concept of normal science means questioning the existence of revolutions. He argues that: "Revolutions through criticism demand normal science no less than revolutions through crisis" (Kuhn, "Reflections" 233). Noting that

most of his critics agree to the existence of revolutions, Kuhn asserts: "By their nature revolutions cannot be the whole of science: something different must necessarily go on in between." He repeats Popper's observation that 'scientists *necessarily* develop their ideas within a definite theoretical framework' (Qtd in Kuhn, "Reflections" 242). Kuhn declares that "revolutions demand such frameworks, since they always involve the rejection and replacement of a framework or some of its integral parts." Therefore Kuhn's vision of normal science "is precisely research within a framework"--the opposite of revolutions. He concludes that if revolutions exist, so must normal science ("Reflections" 242).

As part of his discussion of normal science, Kuhn differentiates between the arts and philosophy, the mature or developed sciences, and "proto-sciences" ("Reflections" 244). Historians, according to Kuhn, have often portrayed the arts and philosophy as "developing through a succession of traditions punctuated by revolutionary alterations of artistic style or taste or of philosophical viewpoint or goal" ("Reflections" 243). He notes: "The relation of pre- and post-revolutionary practice in these fields is not what we have learned to expect from the developed sciences." While these fields may not meet Popper's demarcation criteria, "they could nevertheless progress as the sciences do." Kuhn also acknowledges the existence of other fields or "proto-sciences--in which practice does generate testable conclusions but which nonetheless resemble philosophy and the arts rather than the established sciences in developmental patterns." The arts, philosophy, and the proto-sciences "lack some element which, in the mature sciences, permits the more obvious forms of progress" ("Reflections" 244-45).

Kuhn does not pretend to offer any prescription for transforming a proto-science into a mature one. In fact, he agrees that: "If, as Feyerabend suggests, some social scientists take from me the view that they can improve the status of their field by first legislating agreement on fundamentals and then turning to puzzle-solving, they are badly

misconstruing my point" (Kuhn, "Reflections" 245). But Kuhn does not preclude the transition of a proto-science to maturity. He concludes:

> Each of the currently established sciences has emerged from a previously more speculative branch of natural philosophy, medicine, or the crafts at some relatively well-defined period in the past. Other fields will surely experience the same transition in the future. Only after it occurs does progress become an obvious characteristic of a field. And only then do those prescriptions of mine which my critics decry come into play. (Kuhn, "Reflections" 245)

For a field to mature into a science, Kuhn posits it must meet four conditions. First, it must meet Popper's criteria of demarcation, that is, "for some range of natural phenomena concrete predictions must emerge from the practice of the field." Second, Kuhn argues that the field must consistently achieve what passes for "predictive success" for some interesting sub-class of phenomena. Third, the predictive techniques used "must have root in some theory which, however metaphysical, simultaneously justifies them, explains their limited success, and suggests the means for their improvement in both precision and scope." Fourth, "the improvement of predictive technique must be a challenging task, demanding on occasions the very highest measure of talent and devotion" ("Reflections" 245-46).

Taken together, Kuhn's four conditions form a description of "a good scientific theory" as well as the basis for one of his disagreements with Popper. Kuhn claims that once a field has "such a theory in hand the time for steady criticism and theory proliferation [two Popperian notions] has passed" ("Reflections" 246). Members of the particular scientific community can now practice normal science in the Kuhnian sense --performing puzzle-solving and "mopping up operations."

Given a theory that allows the practice of normal science, Kuhn declares that scientists need not attempt to solve the puzzles supplied by the theory. He asserts:

> They could instead behave as practitioners of the proto-sciences must: they could, that is, seek potential weak spots, of which there are always large numbers, and endeavor to erect alternate theories around them. Most of my present critics believe they should do so. I disagree but exclusively on strategic grounds. (Kuhn, "Reflections" 246)

But puzzle-solving is the central activity of Kuhn's normal science. Kuhn reinforces the importance of puzzle-solving within the sciences: "In the developed sciences, unlike philosophy, it is the technical puzzles that provide the usual occasion and often the concrete materials for revolution. Their availability together with the information and signals they provide account in large part for the special nature of scientific progress" (Kuhn, "Reflections" 247).

Criticism of Kuhn also extends to what Kuhn sees as the failure of some of his critics to recognize in Kuhn's "historical examples the detailed functions of the breakdown of normal science in setting the stage for revolutions" ("Reflections" 247). Kuhn states categorically that, "if there are revolutions, then there must be normal science" (Kuhn, "Reflections" 249). Responding especially to criticism by Toulmin, Kuhn argues for two sorts of scientific change:

> One of them, normal science, is the generally cumulative process by which the accepted beliefs of the scientific community are fleshed out, articulated, and extended. It is what scientists are trained to do, and the main tradition of

> English-speaking philosophy of science derives from the examination of the exemplary works in which that training is embodied. (Kuhn, "Reflections" 250)

He agrees with Toulmin that both normal science and revolutionary science "interpenetrate: revolutions are no more total in science than in other aspects of life" (Kuhn, "Reflections" 250). Further, Kuhn admits the difficulty of discriminating "normal from revolutionary episodes" (Kuhn, "Reflections" 251). He writes that making discriminations "demands close historical study" that "few parts of the history of science have received." Kuhn contends: "One must know not simply the name of the change, but the nature and structure of group commitments before and after it occurred. Often, to determine these, one must also know the manner in which the change was received when first proposed" (Kuhn, "Reflections" 251).

Some of Kuhn's critics do acknowledge the existence of normal science. Watkins, for instance, refers to it as "hack science"; Popper simply calls it bad science. But when they do admit the existence of normal science in the Kuhnian sense, Kuhn records that his critics often have trouble with his concept of crisis and its function or role in the process of scientific change. The criticism is mostly a matter of focus. Kuhn continues to place importance on the role of crisis as a departure from normal science; many of his critics have dismissed Kuhn's normal science in favor of Popper's attention to revolutionary science.

In "Reflections," Kuhn clusters the third set of criticisms around issues related to shifts in paradigms or changing traditions of normal science. This cluster focuses on "the nature of change from one normal scientific tradition to another and of the techniques by which the resulting conflicts are resolved." Kuhn categorically rejects "the charges of irrationality, relativism, and the defence [sic] of mob rule" leveled by his critics in the third category ("Reflections" 234). He suggests that such charges arise primarily out of

his "description of the procedures by which scientists choose between competing theories" (Kuhn, "Reflections" 259). Kuhn even accepts partial responsibility for some of the misunderstanding, but insists that his critics shoulder their part of the burden.

Kuhn seems particularly troubled by the charge of relativism which he restates as arising out of misunderstanding. He avers that his "view of scientific development is fundamentally evolutionary." Further, he sees scientific development as, "like biological evolution, unidirectional and irreversible," arguing, "One scientific theory is not as good as another for doing what scientists normally do" (Kuhn, "Reflections" 264). Kuhn suggests that much of the charge of relativism rests in his unwillingness to take a "step, or kind of step, which many philosophers of science wish to take"--that of comparing theories "as representations of nature, as statements about 'what is really out there.'" Kuhn argues against the notion of one theory being "a better approximation of the truth" ("Reflections" 265).

In the section six of his "Reflections" essay, Kuhn turns to a closer examination of incommensurability and paradigms, identifying these two concepts as the central issues that differentiate him from most of his critics. Kuhn offers further explication of his concept of paradigm and responds to Masterman's analysis of his usage of the term. To reduce confusion over his definition of "paradigm," Kuhn isolates the "two quite different ways in which the term [paradigm] is deployed in [Structure]" ("Reflections" 234). He uses the clarification to further explore his differences with Popper and attack the Popperian notion of a semantically neutral observation language shared by competing theories ("Reflections" 235).

Kuhn points out that "point-by-point comparison of two successive theories demands a language into which at least the empirical consequences of both can be translated without loss or change." In the Popperian sense, some basic vocabulary exists "consisting entirely of words which are attached to nature in ways that are

unproblematic and, to the extent necessary, independent of theory." But Kuhn and Feyerabend agree that no such vocabulary "is available" ("Reflections" 266). Instead, Kuhn argues that though many theories use the same vocabulary, the meanings--especially "the ways in which some of them attach to nature"--somehow change. "Successive theories," according to Kuhn, "are thus, we say, incommensurable" ("Reflections" 267).

Many of Kuhn's critics decry his choice of "incommensurable," insisting that Kuhn "cannot mean it literally since men who hold different theories do communicate and sometimes change each others' views." Popper, for example, declares: "It is just a dogma--a dangerous dogma--that the different frameworks [of competing theories] are like mutually untranslatable languages" ("Reflections" 267). Kuhn attacks these objections by exploring the difficulties of translation between languages. He argues that, "Translation, in short, always involves compromises which alter communication." Translation is difficult and far from ideal because "languages cut up the world in different ways, and we have no access to a neutral sub-linguistic means of reporting." Kuhn insists that learning to translate a language or theory means "learning to describe the world with which the language or theory functions" ("Reflections" 268).

Kuhn regards scientific communities "as language communities," noting the parallels between the two ("Reflections" 270). He states:

> In learning either a science or a language, vocabulary is acquired together with at least a minimal battery of generalizations which exhibit it applied to nature. In neither case, however, do the generalizations embody more than a fraction of the knowledge of nature which has been acquired in the learning process. (Kuhn, "Reflections" 270)

In his comparison, Kuhn focuses on the process of language-nature learning. He asserts that linguistically, scientific and language communities "acquire laws of nature together with a knowledge of meanings," yet the relationship of words to other words "can function only if we already possess some vocabulary acquired by a non-verbal or incompletely verbal process" (Kuhn, "Reflections" 270). Kuhn points to ostension "or some elaboration of it, the direct matching of whole words or phrases to nature," as that process ("Reflections" 271).

Kuhn's reference to ostension serves as the transition from a discussion of incommensurability to a clarification of paradigm. He notes: "When I speak of knowledge embedded in terms and phrases learned by some non-linguistic process like ostension, I am making the same point that my book aimed to make by repeated reference to the role of paradigms as concrete problem solutions, the exemplary objects of an ostension" (Kuhn, "Reflections" 271). Kuhn better articulates that point by repeating Masterman's conclusion that paradigms 'can function when the theory is not there' (Qtd in "Reflections" 271). He goes on to acknowledge that his point is often missed partly because of his own use of the term in multiple ways.

In an attempt to ease the confusion over his definition of "paradigm," Kuhn offers a new term, the "disciplinary matrix," as a possible substitution. Given the isolation of individual communities of specialists, Kuhn uses "disciplinary matrix" to refer to what members of a community share "that [enables] them to solve puzzles and that [accounts] for their relative unanimity in problem-choice and problem-solutions." His new term is "'disciplinary,' because it is common to the practitioners of a specified discipline," and "'matrix,' because it consists of ordered elements which require individual specification." Kuhn argues: "All the objects of commitment described in my book as paradigms, parts of paradigms, or paradigmatic would find a place in the disciplinary matrix, but they would not be lumped together as paradigms, individually or

collectively." Among the ideas that would fall under the "disciplinary matrix," Kuhn lists shared symbolic generalizations, shared models, shared values, and other elements. Under other elements, he includes "concrete problem solutions, the sorts of standard examples of solved problems which scientists encounter first in student laboratories, in the problems at the ends of chapters in science texts, and on examinations." Referring specifically to "concrete problem solutions," Kuhn goes on to declare: "If I could, I would call these problem-solution paradigms, for they are what led me to the choice of the term in the first place. Having lost control of the word, however, I shall henceforth describe them as exemplars" ("Reflections" 271-72).

In an accompanying footnote, Kuhn notes that redefining paradigms in the dual sense of disciplinary matrix and exemplars means depriving himself of "the phrases 'pre-paradigm period' and 'post-paradigm period' when describing the maturation of a scientific specialty" (Kuhn, "Reflections" 272). He continues:

> In retrospect that seems to me all to the good, for, in both senses of the term, paradigms have throughout been possessed by any scientific community, including the schools of what I previously called the "pre-paradigm period." My failure to see that point earlier has certainly helped to make a paradigm seem a quasi-mystical entity or property that, like charisma, transforms those infected by it. Note, however . . . that this alteration in terminology does not at all alter my description of the maturation process. The early stages in the development of most sciences are characterized by the presence of a number of competing schools. Later, usually in the aftermath of a notable scientific achievement, all or most of the schools vanish, a change which permits a far more powerful professional behaviour [sic] to the members of the remaining community. (Kuhn, "Reflections" 272)

Kuhn may change his meaning of paradigm, but he does not abandon his basic framework of scientific advance by embracing the new terminology of "disciplinary matrix" and "exemplar."

Kuhn adds to his description of exemplars as he returns to the issues associated with language and translation. He writes:

> Ordinarily problem-solutions of this sort [exemplars] are viewed as mere applications of theory that has already been learned. The student does them for practice, to gain facility in the use of what he already knows. Undoubtedly that description is correct after enough problems have been done, but never, I think, at the start. Rather doing problems is learning the language of a theory and acquiring the knowledge of nature embedded in that language. (Kuhn, "Reflections" 272)

The acquisition of the language helps new practitioners to gain the symbolic formulations "demanded by individual problems" (Kuhn, "Reflections" 272-73). Exposure to "exemplary problem-solutions teaches them to see different . . . situations as like each other." Accordingly, "Seeing problem-situations as like each other, as subjects for the application of similar techniques, is also an important part of normal scientific work" (Kuhn, "Reflections" 273). Until students learn to recognize the similarity-dissimilarity relationships--tacit parts of "a language-conditioned or language-correlated way of seeing the world"--they will "not see a world at all" (Kuhn, "Reflections" 274). Students have to absorb the components of the discipline's paradigm to share the world of the practitioners of that discipline.

Theory-choice is tied to translation in Kuhn's conception of normal science. He contends:

> One of the things upon which the practice of normal science depends is a learned ability to group other objects and situations into similarity classes which are primitive in the sense that the grouping is done without an answer to the question, "similar with respect to what?" One aspect of every revolution is, then, that some of the similarity relations change. Objects which were grouped in the same set before are grouped in different sets afterwards and *vice versa*. . . . Since most objects within even the altered sets continue to be grouped together, the names of the sets are generally preserved. Nevertheless, the transfer of a subset can crucially affect the network of interrelations among sets. . . . When such a redistribution of objects occurs, two men whose discourse had proceeded for some time with apparently full understanding may suddenly find themselves responding to the same stimulus with incompatible descriptions or generalizations. Just because neither can say, "I use the word element (or mixture, or planet, or unconstrained motion) in ways governed by such and such criteria," the source of the breakdown in their communication may be extraordinarily difficult to isolate and by-pass. (Kuhn, "Reflections" 275)

These communication breakdowns force practitioners to find ways to translate the language of each other's theories "and simultaneously to describe the world in which that theory or language applies" (Kuhn, "Reflections" 277). Kuhn concludes that, "In the absence of a neutral language, the choice of a new theory is a decision to adopt a different native language and to deploy it in a correspondingly different world" ("Reflections" 277).

Kuhn's response to his critics in "Reflections" presents an opportunity to clear up some of the confusion generated by the multiple uses he makes of the term "paradigm" in the first edition of Structure. By redefining "paradigm" as "disciplinary matrix" and "exemplar," he synthesizes some of the criticism of his original theory. Further, he does so without really disturbing the basic frame of his theory or abandoning any of the significant positions he takes on normal science, revolutions, communities, or his cycle of scientific advance.

The combination of Kuhn's essays and the comments of his critics shows a theory capturing a great deal of attention within the professional communities Kuhn inhabits. Responding to his critics forces Kuhn to further explicate his ideas and to modify some of his concepts. At the same time, key figures in the history and philosophy of science, such as Sir Karl Popper, Stephen Toulmin, and Paul Feyerabend, feel compelled to address Kuhn's ideas. Borrowings of Kuhn in other fields occur as his 1962 Structure becomes better known, but it is not until the publication of his 1970 second edition, enlarged by the addition of his "Postscript--1969" but otherwise essentially unchanged, that his influence increases substantially beyond the history and philosophy of science.

Adding to Theory

> [I]f there are revolutions, then there must be normal science.[19]
>
> --Thomas Kuhn, 1969 (1970)

In the interim between the 1962 publication of Structure and the 1970 second edition, Kuhn's theory evolves in response to criticism within the history and philosophy of science. Attacks on the definition of paradigm, the constitution of normal

science, and the characteristics of scientific revolutions force Kuhn to craft his addition to the 1970 edition of Structure as a response to criticism. Without retreating far from his original position, Kuhn offers further clarification of his theory in "Postscript--1969," the extra chapter he adds to the second, enlarged edition of Structure. "Postscript" shares a number of points with two of Kuhn's works written earlier in 1969, "Second Thoughts on Paradigms" published in 1974 and parts of the 1970 "Reflections on My Critics," especially section six.

I treat the three works--"Second Thoughts," parts of "Reflections," and "Postscript"--together because they are so nearly alike in framework and content. In all three, Kuhn addresses his critics, redefines paradigms, and modifies the framework of his theory somewhat. Kuhn writes that he prepared "Second Thoughts" for a conference in March 1969, retraced "some of the same ground" in "Reflections" later that year, and finally prepared "Postscript" for inclusion in the second edition of Structure (Essential Tension xx). Though Kuhn does respond to specific points in Lakatos and Musgrave's Criticism, he predicates much of that response, especially in section six and less so in section four of "Reflections," on ideas abstracted from "Second Thoughts." These same ideas appear in "Postscript." Kuhn thus links the three works inexorably together. Taken as a whole, the three illustrate the evolution of Kuhn's ideas: he clarifies his concept of paradigms, he modifies his sense of the preparadigmatic stage in the development of a discipline, and he renews his emphasis on the influence of scientific communities.

"Second Thoughts on Paradigms" and Other Essays, 1969

Near the conclusion of his "Reflections" essay, Kuhn notes that much of the evidence and additional explication of the modifications to his theory appear in "Second Thoughts."

The precursor to "Reflections" was originally scheduled for a 1970 publication in Frederick Suppe's The Structure of Scientific Theories, in press at the time Kuhn composed "Reflections." Kuhn admits that he abstracted much of "Second Thoughts" in arriving at his conclusions in "Reflections" ("Reflections" 274). Although Suppe's volume does not appear until 1974, four years after the publication of Kuhn's second edition of Structure and the appearance of "Reflections," Kuhn's "Second Thoughts" plays a critical role in recording the evolution of Kuhn's theory. Using "Second Thoughts" as a baseline, I explore the parallel development of "Reflections" and "Postscript."

In "Second Thoughts," Kuhn revisits the concept of paradigm to clarify the theory he offers in Structure. He begins the essay by observing the success of Structure, concluding "regretfully . . . that it can be too nearly all things to all people" (Kuhn, "Second Thoughts" 293). At the heart of the Structure's "excessive plasticity" is the concept of paradigm, "a word that figures more than any other, excepting the grammatical particles, in its pages" (Kuhn, "Second Thoughts" 293-94). Kuhn refers to Masterman's identification of at least twenty-one different uses of the term in Structure and realigns these uses into two major groups, which he later terms "disciplinary matrix" (referring to a global sense of paradigm as "embracing all the shared commitments of a scientific group") and "exemplars" (specific scientific achievements which are a subset of the first) (Kuhn, "Second Thoughts" 294). He repeats these definitions of paradigm in the other two essays.

Kuhn ties his dual sense of "paradigm" closely to his conception of scientific community, arguing: "A paradigm is what the members of a scientific community, and they alone, share." He further explains: "Conversely, it is their possession of a common paradigm that constitutes a scientific community of a group of otherwise disparate men" ("Second Thoughts" 294). To make the dual definition of paradigm work, Kuhn argues

for the recognition of the independent existence of scientific communities, acknowledging that his argument at present is somewhat circular.

In all three works, the realignment of the paradigm and the attention focused on scientific communities affects Kuhn's characterization of what he originally termed the "preparadigm period" in the 1962 version of Structure. Kuhn defends his framing of early scientific advance in a footnote to "Second Thoughts" and "Reflections" and in the text of "Postscript." He recounts his earlier position of the preparadigm period as involving "a number of competing schools, each claiming competence for the same subject matter but approaching it in quite different ways," followed by "a relatively rapid transition, usually in the aftermath of some notable scientific achievement, to a so-called postparadigm period characterized by the disappearance of all or most schools." While he retains the pattern, Kuhn now modifies that position to allow even the preparadigm schools to possess paradigms, stating: "Whatever paradigms may be, they are possessed by any scientific community, including the schools of the so-called preparadigm period" ("Second Thoughts" 295). This modification seems a direct answer to many of Masterman's observations about paradigms and her characterization of multiple paradigm science as a stage occurring before a discipline matures in the Kuhnian sense by adherence to a single dominant paradigm.

Kuhn elucidates further in "Postscript" on the possession of paradigms by the competing schools that exist in the pre-paradigm period. He asserts that normal puzzle-solving research is not possible until after the transition to maturity. In Kuhn's modified view, the nature of the paradigm changes upon transition to something "that identifies challenging puzzles, supplies clues to their solution, and guarantees that the truly clever practitioner will succeed" ("Postscript" 179). In other words, Kuhn argues that normal science as he envisions it cannot exist until the emergence of a single

dominant paradigm that allows the community to take certain foundational ideas and knowledge for granted.

Returning to issues connected to his sense of community in "Second Thoughts," Kuhn offers a new definition of a "scientific community":

> A scientific community . . . consists of the practitioners of a scientific specialty. Bound together by common elements in their education and apprenticeship, they see themselves and are seen by others as the men responsible for the pursuit of a set of shared goals, including the training of their successors. Such communities are characterized by the relative fullness of communication within the group and by the relative unanimity of the group's judgment in professional matters. To a remarkable extent the members of a given community will have absorbed the same literature and drawn similar lessons from it. Because the attention of different communities is focused on different matters, professional communication across group lines is likely to be arduous, often gives rise to misunderstanding, and may, if pursued, isolate significant disagreement. (Kuhn, "Second Thoughts" 296)

Kuhn later repeats this paragraph with only slight variations in both "Reflections" and "Postscript."

In section four of "Reflections," he designates the community as a unit for empirical analysis composed of "the practitioners of a given specialty, men bound together by common elements in their education and apprenticeship, aware of each other's work, and characterized by the relative fullness of their professional communication and by the relative unanimity of their professional judgment." Further, he argues: "In the mature sciences the members of such communities would ordinarily see themselves and be seen

by others as the men exclusively responsible for a given subject matter and a given set of goals, including the training of their successors" (Kuhn, "Reflections" 253). In "Postscript," Kuhn adds that members of a community are seen as "uniquely responsible" ("Postscript" 177), rather than "exclusively responsible" ("Reflections" 253) for shared goals. In a footnote to "Second Thoughts," he emphasizes the shared source materials--the literature and lessons he mentions in his text--"often provide significant clues to community structure" (Kuhn, "Second Thoughts" 296). Having referred to works such as Newton's Principia as paradigms in Structure, Kuhn acknowledges that he now describes them "as particularly important *sources* [my emphasis] of the elements in a community's disciplinary matrix" ("Second Thoughts" 296).

Kuhn's account of scientific communities, which he basically repeats in "Reflections" and "Postscript," suggests that the best scientists claim membership in multiple communities at the same time "or in succession" ("Second Thoughts" 297). Indeed, Kuhn himself at various points in his own career claims membership in a historian of science community, a philosophy of science community, and perhaps even a sociology of science or knowledge community or some combination of all three. Membership in various communities thus tends to change as an individual's as well as a group's interests shift.

Given the identification of a community, Kuhn asks rhetorically: "What shared elements account for the relatively unproblematic character of the professional communication and for the relative unanimity of professional judgment?" His first edition of Structure "licenses the answer 'a paradigm' or 'a set of paradigms,'" one of Kuhn's two main senses of paradigm. Kuhn defines this sense of "paradigm" as a "'disciplinary matrix'--'disciplinary' because it is the common possession of the practitioners of a professional discipline and 'matrix' because it is further composed of ordered elements of various sorts, each requiring further specification," a definition

appearing in the other essays as well. Kuhn continues: "Constituents of the disciplinary matrix include most or all of the objects of group commitment described in the book [Structure] as paradigms, parts of paradigms, or paradigmatic" ("Second Thoughts" 297).

Kuhn goes on to discuss three of the elements specifically in "Second Thoughts": symbolic generalizations, models, and exemplars. In "Reflections," he adds "shared values" to the list, but concentrates on exemplars, weaving the other three terms into his discussion. In "Postscript," the elements emphasized change slightly again; Kuhn retains generalizations, substitutes new terminology "shared commitment to . . . beliefs" for "models," discusses shared values, and continues expanding on the concept of paradigms as exemplars ("Postscript" 184).

In "Second Thoughts," Kuhn defines "symbolic generalizations" as "those expressions, deployed without question by the group, which can be readily cast in some logical form" ("Second Thoughts" 297); in "Postscript," Kuhn alters that definition slightly to "deployed without question or dissent by group members," but otherwise uses the same definition ("Postscript" 182). Kuhn conceives of the expressions as "the formal, or the readily formalizable, components of the disciplinary matrix," his first revised sense of paradigm. "Models," in Kuhn's design, "provide the group with preferred analogies or, when deeply held, with an ontology"; under this definition, models exist as a range of extremes from "heuristic" to "the objects of metaphysical commitment." Kuhn's "exemplars" consist of "the concrete problem solutions, accepted by the group as, in quite the usual sense, paradigmatic" ("Second Thoughts" 297-98).

Kuhn suggests in "Second Thoughts" that we require an understanding of the operation of generalizations, models, and exemplars "to understand how a scientific community functions as a producer and validator of sound knowledge" ("Second Thoughts" 298), More concerned with exemplars in "Reflections" and faced with space limitations, he

avoids explicitly making such a claim. Kuhn changes terminology again in "Postscript," suggesting that deconstructing the disciplinary matrix into its components will help "clarify the nature of my [Kuhn's] present approach" ("Postscript" 182). He increases from three elements to four "main" components, adding the shared values from "Reflections" to his list. Kuhn uses the three accounts focusing on elements or components to set up his discussion of exemplars.

What does Kuhn have to say about symbolic generalizations? First, they are often expressed in symbolic form such as $f = ma$ or in words such as 'action equals reaction' (Kuhn, "Second Thoughts" 298), examples he repeats in the other essays. Second, community members "routinely deploy" such expressions "without felt need for special justification" and without challenge from fellow members of the community. Kuhn argues about the importance of the behavior, noting "for without a shared commitment to a set of symbolic generalizations, logic and mathematics could not routinely be applied in the community's work" ("Second Thoughts" 298). Third, Kuhn asserts that "the power of a science increases with the number of symbolic generalizations its practitioners have at their disposal" ("Second Thoughts" 299).

Kuhn adds to the descriptive list in his discussion in "Postscript." He writes that such generalizations often have a dual function; they serve "in part as laws but also in part as definitions of some of the symbols they deploy." Kuhn's distinctions in both "Second Thoughts" and "Postscript" are critical since Kuhn suspects "that all revolutions involve, among other things, the abandonment of generalizations the force of which had previously been in some part that of tautologies" (Postscript" 183-84).

Determining the shared set of symbolic generalizations becomes a discussion of the process of scientific formalism in "Second Thoughts." Kuhn suggests that symbolic generalizations in the sciences "are not so much generalizations as generalization-sketches, schematic forms whose detailed symbolic expression varies from one

application to the next" ("Second Thoughts" 299). He concludes that symbolic expressions, as "the common possession of the members a scientific community . . . provide the group with an entry point for logic and mathematics," although "these tools" apply not to the shared generalization, but some formalist version of it ("Second Thoughts" 300). Kuhn declares:

> Any account of the cognitive apparatus of a scientific community may reasonably be asked to tell us something about the way in which the group's members, in advance of *directly* relevant empirical evidence, identify the special formalism appropriate to a particular problem, especially a new problem. That clearly is one of the functions which scientific knowledge does serve. It does not, of course, always do so correctly; there is room, indeed need, for empirical checks on a special formalism. The deductive steps and the comparison of their end products with experiment remain prerequisites of science. (Kuhn, "Second Thoughts" 301)

Kuhn's discussion of models in "Second Thoughts" centers around the determination of analogies or models, rather than correspondence rules, that allow scientists to "attach symbolic expressions to nature" ("Second Thoughts" 310). Using examples that later appear in "Reflections" and "Postscript"--Newton's Second Law, Ohm's law, and one of Galileo's experiments--Kuhn speculates that "an acquired ability to see resemblances between apparently disparate problems plays in the sciences a significant part of the role usually attributed to correspondence rules." He notes: "Once a new problem is seen to be analogous to a problem previously solved, both an appropriate formalism and a new way of attaching its symbolic consequences to nature follow. Having seen the resemblance, one simply uses the attachments that have proved effective before." Kuhn

thinks that the ability "to recognize group-licensed resemblances is . . . the main thing students acquire by doing problems" ("Second Thoughts" 306). For Kuhn, those concrete problems and their solutions form the exemplars, the community's standard examples.

While Kuhn uses his discussion of analogies or models as the point of departure to discuss exemplars in "Second Thoughts," he ignores most of the discussion in "Reflections" and substitutes shared commitments and shared values as main components in "Postscript." I will do the same before examining Kuhn's discussion of exemplars further.

In "Postscript," Kuhn identifies shared commitments to particular beliefs as the second main component of a paradigm in the sense of disciplinary matrix. These commitments are essentially "beliefs in particular models," including heuristic ones, that function as a group to supply the community "with preferred or permissible analogies and metaphors." Kuhn argues: "By doing so they help to determine what will be accepted as an explanation and as a puzzle-solution"; on the other hand, the commitments "assist in the determination of the roster of unsolved puzzles and in the evaluation of the importance of each" ("Postscript" 184).

According to Kuhn, values "are more widely shared among different communities than either symbolic generalizations or models." Shared values "function at all times" and become especially important during times of crisis or when community members have to "choose between incompatible ways of practicing their discipline." Kuhn identifies values about predictions as those "most deeply held," noting issues of accuracy, the preference of quantitative over qualitative evidence, and consistent application among others. But communities also appeal to values when "judging whole theories" in Kuhn's view. He suggests that such values "must . . . permit puzzle-formulation and solution; where possible they should be simple, self-consistent, and plausible, compatible, that is, with other theories currently deployed." Kuhn does not

limit shared values to simply prediction and theory choice; he acknowledges the existence of others. Further, Kuhn admits to a weakness in his original Structure; he does not pay enough attention to values when discussing crisis and theory choice ("Postscript" 184-85).

Kuhn goes on to declare that values, more than any other element of the disciplinary matrix, "may be shared by men who differ in their application." He notes that while judgments of accuracy remain "relatively . . . stable from one time to another and from one member to another," great variation occurs in "judgments of simplicity, consistency, plausibility and so on." Accordingly, as context and situation differ when values "must be applied, different values, taken alone . . . often dictate different choices" ("Postscript" 185). Kuhn summarizes: "In short, though values are widely shared by scientists and though commitment to them is both deep and constitutive of science, the application of values is sometimes considerably affected by the features of individual personality and biography that differentiate the members of the group" (Kuhn, "Postscript" 185).

Kuhn's second major sense of paradigm--that is, paradigm as exemplar--functions as the fourth element of the disciplinary matrix in Kuhn's "Postscript" and the "third sort of cognitive component" of the matrix in "Second Thoughts." In "Second Thoughts," these concrete problem solutions comprise "a community's standard examples" ("Second Thoughts" 306). In "Reflections," they become "the sorts of standard examples of solved problems which scientists encounter first in student laboratories, in the problems at the ends of chapters in science texts, and on examinations" ("Reflections" 272), a definition echoed in "Postscript."

In all three works, Kuhn credits the "sense of paradigm as standard example" ("Second Thoughts" 307) with leading him to choose the term in the first place and laments having "lost control of the word" ("Reflections" 272). According to Kuhn, most

of his readers have chosen to focus on paradigm in the sense of disciplinary matrix, even though Kuhn finds paradigm in the sense of exemplar as more appropriate. Kuhn finds himself now forced to substitute "exemplar" for "paradigm" when focusing on a community's standard examples.

Kuhn underscores the importance he attaches to exemplars, both in terms of their effect on the structure of a science and the influence that exemplars wield on initiation into a community. In "Second Thoughts," Kuhn remarks that "exemplars (and also models) are far more effective determinants of community substructure than are symbolic generalizations" ("Second Thoughts" 307). He insists that "the science student, confronted with a problem, seeks to see it as like one or more of the exemplary problems he has encountered before" ("Second Thoughts" 308). Similarly, he writes in "Postscript": "More than other sorts of components of the disciplinary matrix, differences between sets of exemplars provide the community fine-structure of science" ("Postscript" 187). In Kuhn's view, "doing problems is learning consequential things about nature" ("Postscript" 188). In "Reflections," Kuhn asserts, "doing problems is learning the language of a theory and acquiring the knowledge of nature embedded in that language" ("Reflections" 272). Exposure to these exemplary concrete problem solutions teaches initiates into a scientific community to recognize similarities in problem situations. More importantly, seeing "problem-situations" as similar to one another forms a key part of "normal scientific work" ("Reflections" 273).

At the point of recognizing similarity relationships provided by a knowledge of a community's exemplars, the three works tend to diverge. Kuhn digresses into a discussion of the nature of data and sense perception in "Second Thoughts." In "Reflections," he continues to focus on similarity relationships and language. In "Postscript," Kuhn moves from similarity relationships on to tacit knowledge and the

intuition still emphasizing the nature of exemplars before turning to incommensurability, revolutions, relativism, and the nature of science.

In "Reflections," Kuhn gives further consideration to the recognition of sets of shared examples, changes in those sets, and the influence of those examples and language on the way we see the world. Kuhn notes his own research, an attempt to computer model both criterion-learning and similarity-learning. He argues that "we have too long ignored the manner in which knowledge of nature can be tacitly embodied in whole experiences without intervening abstraction of criteria or generalizations." Kuhn theorizes: "Given examples of what a scientific theory does and being bound by shared values to keep doing science, one need not also have criteria in order to discover that something has gone wrong or to make choices in case of conflict." He believes similarity-recognition to be especially effective when identifying anomaly or choosing between theories. For Kuhn, part of "the practice of normal science" depends upon "a learned ability to group objects and situations into similarity classes," grouping done without even answering "the question, 'similar with respect to what?'" Thus, he asserts: "One aspect of every revolution is, then, that some of the similarity relations change" ("Reflections" 275).

According to Kuhn, a change in similarity relations--that is, the redistribution of objects or subsets of objects from one group to other groups and *vice versa*--"affects the network of interrelations among sets" ("Reflections" 275). Redistribution does not necessarily result in the renaming of the various sets; many names are preserved. But the spread of similarity-changes does influence communication. Kuhn writes:

> When such a redistribution of objects among similarity sets occurs, two men whose *discourse* [my emphasis] had proceeded for some time with apparently full understanding may suddenly find themselves responding to the same stimulus with incompatible descriptions or generalizations. (Kuhn, "Reflections" 276)

Kuhn notes that the source of the resulting breakdown in communications "may be extraordinarily difficult to isolate and bypass" ("Reflections" 276).

Kuhn emphasizes the alteration in "discourse" in the longer passage above. While Kuhn focuses on the change wrought within a single scientific community that causes its potential fragmentation or a paradigm shift, he provides further evidence to scholars in composition studies to promote the concept of a discourse community built upon shared language. Following Kuhn's lead, composition scholars will focus greater attention on the role of such discourse communities, especially in shaping writing theory, practice, and pedagogy in the eighties and nineties.

The change in discourse that Kuhn alludes to and the resulting breakdown in communications "are likely evidence that the men involved are processing certain stimuli differently, receiving different data from them, seeing different things or the same things differently" ("Reflections" 276). Kuhn admits the possibility that those involved in a communication breakdown may discover the source of their differences, usually variance in the definitions of terms the participants shared before the crisis, and work through them. To renew communication, each participant has to find "a way to translate [the] other's theory into his own language and simultaneously to describe the world in which that theory or language applies." Without some movement towards translation, Kuhn argues, the process of theory-choice would be difficult to describe. Essentially, Kuhn concludes, "the choice of a new theory is a decision to adopt a different native language and to deploy it in a correspondingly different world"--a choice that Kuhn continues to equate with a "conversion" experience ("Reflections" 277).

In "Postscript," Kuhn returns to his insistence upon the nature of scientific knowledge as "embedded in theory and rules" and his focus on paradigms as "shared examples" of problems would-be scientists learn to solve during their initiation into a

community. He declares that "at the start" of that education process "and for some time after, doing problems" translates into "learning consequential things about nature"; the exemplars provide the "empirical content" of the theories and rules that the scientist acquires through learning. Doing "exemplary problems" helps student scientists develop a knowledge of the similarity sets that constitute "a time tested and group-licensed way of seeing." Through practice of these shared examples, the student learns to view "the situations that confront him as a scientist in the same gestalt as other members of his specialists' group" (Kuhn, "Postscript" 187-89).

Kuhn attaches great importance to the role "acquired similarity relations" play in governing community behavior. He writes: "Scientists solve puzzles by modeling them on previous puzzle-solutions, often with only minimal recourse to symbolic generalizations" (Kuhn, "Postscript" 189-90). Students acquire the language of the community "together with concrete examples of how words function in use." Borrowing Michael Polanyi's terminology, Kuhn states that initiates learn the 'tacit knowledge' of the scientific community "by doing science rather than by acquiring rules for doing it" (Kuhn, "Postscript" 191).

Kuhn points out another criticism leveled against him as he credits tacit knowledge over rules; some critics have accused him of "trying to make science rest on unanalyzable individual intuitions rather than logic and law" ("Postscript" 191). He responds by pointing out his concern for intuitions that "are tested and *shared* [my emphasis] possessions of the members of a successful group," not individual possessions. Initiates assimilate these shared intuitions as part of the education process they undergo to become members of a scientific community.

To answer the charge of "unanalyzable," Kuhn refers to his own research investigating the properties of such shared intuitions using computer models and notes that he provides more information on his research project in "Second Thoughts." He

avers that just the mention of his own research "should make" his "most essential point":

> When I speak of knowledge embedded in shared exemplars, I am not referring to a mode of knowing that is less systematic or less analyzable than knowledge embedded in rules, laws, or criteria of identification. Instead I have in mind a manner of knowing which is misconstrued if reconstructed in terms of rules that are first abstracted from exemplars and thereafter function in their stead. Or, to put the same point differently, when I speak of acquiring from exemplars the ability to recognize a given situation as like some and unlike others that one has seen before, I am not suggesting a process that is not potentially fully explicable in terms of neuro-cerebral mechanism. Instead I am claiming that the explication will not, by its nature, answer the question, "Similar with respect to what?" That question is a request for a rule, in this case for the criteria by which particular situations are grouped into similarity sets, and I am arguing that the temptation to seek criteria (or at least a full set) should be resisted in this case. (Kuhn, "Postscript" 192)

Kuhn simply opposes "a particular sort of system" ("Postscript" 192).

Kuhn's remarks about exemplars and tacit knowledge serve as the basis for further clarification of his concept of incommensurability as it relates to choice between successive theories. He suggests that the superiority of one theory over another cannot be proved through debate given that only partial communication occurs. Referring back to both sections X and XII of Enlarged and sections V and VI of "Reflections," Kuhn repeats that both sides of the conflict may share the same vocabulary, but use those terms differently. As a result, Kuhn insists that, "each party must try, by persuasion, to

convert the other" ("Postscript" 198). Here Kuhn attacks some of his critics in philosophy for misinterpreting incorrectly that:

> [The] proponents of incommensurable theories cannot communicate with each other at all; as a result, in a debate over theory-choice there can be no recourse to *good* reasons; instead theory must be chosen for reasons that are ultimately personal and subjective; some sort of mystical apperception is responsible for the decision actually reached. (Kuhn, "Postscript" 198-99).

Misconstructing Kuhn's theory allows these critics to a label it as "irrational," a charge that Kuhn calls mistaken ("Postscript" 199).

Responding specifically to the misconstruction, Kuhn maintains that: "Debates over theory-choice cannot be cast in a form that resembles logical or mathematical proof," because such proof requires the prior stipulation of "premises and rules of inference." The existence and acceptance of such premises and rules allows parties in the debate to "retrace their steps one by one" against "prior stipulations," eventually forcing one or the other to admit making "a mistake" or violating "a previously accepted rule" ("Postscript" 199). Kuhn remarks:

> Only if the two discover instead that they differ about the meaning or application of stipulated rules, that their prior agreement provides no sufficient basis for proof, does the debate continue in the form it inevitably takes during scientific revolutions. That debate is about premises, and its recourse is to persuasion as a prelude to the possibility of proof. (Kuhn, "Postscript" 199)

Kuhn proceeds to argue that reasons for theory choice such as "accuracy, simplicity, fruitfulness, and the like" function "as values"--values that can be "differently applied, individually and collectively, by men who concur in honoring them" ("Postscript" 199). He asserts that "no systematic decision procedure" exists "which, properly applied, must lead" individual group members to the same decision; instead, "it is the community of specialists rather than its individual members that makes the effective decision" (Kuhn, "Postscript" 200). Kuhn contends that we "must understand. . .the manner in which a particular set of shared values interacts with the particular experiences shared by a community of specialists to ensure that most members of the group will ultimately find one set of arguments rather than another decisive." Kuhn states categorically that the process is "persuasion" ("Postscript" 200).

To explore how persuasion succeeds, Kuhn delves further into the issue of incommensurability. Kuhn repeats nearly word for word his earlier contention in "Reflections" ("Reflections" 275-76) about normal science's dependence on "the ability, acquired from exemplars, to group objects and situations into similarity sets," the nature of the change in "similarity relations" that occurs during revolutionary periods of science, and the "incompatible descriptions and generalizations" that result from viewing the same stimuli as "different." The changes first "become evident in communication." However, simply defining "troublesome terms" does not offer resolution for the breakdown in communications that occurs ("Postscript" 200-1).

Continuing to repeat ideas presented in "Reflections," Kuhn stipulates that "participants in a communication breakdown can . . . recognize each other as members of different language communities and then become translators." He provides a brief outline of how the process works, beginning with isolating troublesome "terms and locutions," sharing "everyday vocabularies," and "discovering" how others respond to the same stimuli. Thus, Kuhn concludes, "Each [members of different communities] will have

learned to translate the other's theory and its consequences into his own language and simultaneously to describe in his language the world to which the theory applies" ("Postscript" 202).

According to Kuhn, translation acts as a "potent tool" for persuasion and conversion ("Postscript" 202), but persuasion does not always lead to conversion. For Kuhn, "To persuade someone is . . . to convince him that one's own view is superior and ought therefore supplant his own." Kuhn admits that such a change in conviction can occur without "anything like translation," noting that "each language community can usually produce . . . concrete research results that, though describable in sentences understood in the same way by both groups, cannot yet be accounted for by the other community in its own terms." Yet, "for some" involved in the debate over theory-choice, usually those "just entering the profession," Kuhn observes that, "results alone will be decisive" in choosing which theory to support ("Postscript" 203).

Kuhn further acknowledges that stating arguments in vocabulary "that both groups use in the same way" may not have much effect until "a very late stage in the evolution of opposing views," especially for those individuals already established within the profession. Most will need the time involved in the translation process "to understand how a statement previously opaque could seem an explanation to members of the opposing group." Unfortunately, while translation may aid communication and assist persuasion, Kuhn asserts: "For most people translation is a threatening process, and it is entirely foreign to normal science" ("Postscript" 203).

Translation does not necessarily mean conversion to a new theory. Kuhn observes that, "To translate a theory or worldview [sic] into one's own language is not to make it one's own." Acceptance of the other theory or view requires going "native," discovering "that one is thinking and working in, not simply translating out of, a language that was previously foreign." In other words, it requires making the gestalt switch that Kuhn

argues occurs "at the heart of the revolutionary process." At some point, the scientist simply finds himself operating in the new language, having "slipped into the new language without a decision being made" (Kuhn, "Postscript" 204). Concluding, Kuhn declares:

> Good reasons for choice provide motives for conversion and a climate in which it is more likely to occur. Translation may . . . provide points of entry for the neural reprogramming that, however inscrutable at this time, must underlie conversion. But neither good reasons nor translation constitute conversion, and it is that process we must explicate in order to understand an essential sort of scientific change. (Kuhn, "Postscript" 204)

As he moves toward a final review of the nature of science, Kuhn returns once more to the issue of relativism raised by some of his critics throughout the sixties, especially Popper and Shapere. His concept of translation, taken in the sense of considering "the proponents of different theories" as "like the members of different language-culture communities," does suggest the "relativistic" nature of his position somewhat. Restating his assertion that the "practitioners of the developed sciences are . . . fundamentally puzzle-solvers," Kuhn argues that his critics "have failed to see" that "at times of theory-choice . . . the demonstrated ability to set up and to solve puzzles presented by nature is, in the case of value conflict, the dominant criterion for most members of a scientific group" ("Postscript" 205). Given normal science as the fundamental condition of scientific practice and advance, Kuhn calls himself a "convinced believer in scientific progress," scientific development that "is a unidirectional and irreversible process" ("Postscript" 206). Though many of these same critics attack his notion of normal

science as well, Kuhn maintains that the description of his position as relativistic "seems . . . wrong" ("Postscript" 207).

In his closing statements about the nature of science, Kuhn discusses two additional "reactions" to his original Structure as not "quite right." First, he addresses the accusation that he drifts between "the normative and the descriptive" in his explication of scientific development and the activities of practitioners of science. Kuhn argues that he presents "a viewpoint or theory about the nature of science" that "has consequences for the way in which scientists should behave if their enterprise is to succeed," thus he has "a legitimate basis" for such drift. "Conversely," he writes," one set of reasons for taking the theory seriously is that scientists, whose methods have been developed and selected for their success, do in fact behave as the theory says they should." Kuhn's "descriptive generalizations" support his theory "precisely because they can also be derived from it, whereas on other views of the nature of science they constitute anomalous behavior" ("Postscript" 207-8).

Speaking about the second reaction to his theory, Kuhn questions those outside of the natural sciences who "read" his "main theses as applicable to many other fields as well" ("Postscript" 208). He notes his puzzlement over such extensions, writing:

> To the extent that the book [Structure] portrays scientific development as a succession of tradition-bound periods punctuated by non-cumulative breaks, its theses are undoubtedly of wide applicability. *But they should be, for they are borrowed widely from other fields* [my emphasis]. Historians of literature, of music, of the arts, of political development, and of many other human activities have long described their subjects in the same way. Periodization in terms of revolutionary breaks in style, taste, and institutional structure have been among their standard tools. (Kuhn, "Postscript" 208)

While he can "conceivably" claim to have made a "contribution" of "the notion of paradigm as a concrete achievement, an exemplar," Kuhn states that his intent has been to explicate how scientific development is "strikingly different," despite close resemblances to development in other fields. In both editions of Structure, Kuhn attempts to isolate features of the scientific community, "none necessarily unique to science but in conjunction with setting the activity apart" ("Postscript" 208-9).

Kuhn closes his "Postscript" by calling for continued investigation into the community structure of science and "by underscoring the need for similar and, above all, for comparative study of the corresponding communities in other fields" ("Postscript" 209). He offers a series of potential research questions focusing on initiation into communities, socialization, goal and value setting processes, and treatment of anomaly. Finally, he concludes: "Scientific knowledge, like language, is intrinsically the common property of a group or else nothing at all. To understand it we shall need to know the special characteristics of the groups that create and use it" ("Postscript" 210).

Kuhn's "Second Thoughts," "Reflections," and "Postscript," taken together, show the movement in Kuhn's thought more on issues raised by his critics, than on any general drift away from the position originally developed in Structure and restated essentially unchanged in his enlarged, second edition. His "Postscript--1969," as the principal addition to the second edition, serves to clarify some of his initial points and grows out of positions developed earlier in the year in "Second Thoughts" and "Reflections." Kuhn does not abandon the basic tenets of his original position, choosing instead to call for more investigation of the communal nature of scientific development. While he claims to be puzzled by the attention his theory has attracted in fields outside of the sciences--suggesting that he has borrowed from many of them--he notes the need for comparative

studies of other fields to enhance knowledge about the nature of the scientific communities.

Changes to Kuhn's Second Edition of Structure, 1970

The 1970 edition of Structure varies little from the original until one reaches Kuhn's thirty-six page, "Postscript--1969." In a footnote to "Postscript," Kuhn alerts us to changes in two passages in the original work; these concern the description of Newton's Principia on pages 30-33 and the response to crises on page 84. Otherwise, Kuhn admits that he makes no attempt to systematically rewrite his work, "restricting alterations to a few typographical errors plus two passages which contained isolable errors" ("Postscript" 174) and adding the postscript.

The first of the "isolable errors" involves his description of Newton's Principia on pages 30-33. Here, Kuhn seems to perform fine-tuning more than anything else. Kuhn's second change involves a complete revision of a single assertion about the closure of scientific crises. In his 1962 version, Kuhn argues that "all crises close with the emergence of a new candidate for paradigm and with the subsequent battle over its acceptance" (Structure 84). In the 1970 edition, Kuhn amends the original assertion to read that "all crises close in one of three ways": first, normal science may prove "able to handle the crisis-provoking problem"; second, problems that resist "even apparently radical new approaches" may simply be labeled and set aside for a future generation with more developed tools to solve; or, a new paradigm may arise (Enlarged 84). Kuhn no longer limits the closure of scientific crises to competition between paradigms.

The real substance of Kuhn's expansion in the second edition occurs in his "Postscript--1969" as already seen. In it, Kuhn addresses some of the criticism of his original work, elaborating upon his concept of paradigm and admitting, as he does in

"Reflections," that some aspects of the "initial formulation" of his theory "create gratuitous difficulties and misunderstandings." Kuhn welcomes the chance to eliminate some of those misunderstandings, "to sketch needed revisions, to comment on some reiterated criticisms, and to suggest directions in which [his] own thought is presently developing" ("Postscript" 174). In any event, rather than completely revising Structure to account for the direction in which his thought has developed, Kuhn adds "Postscript" in an attempt to buttress his theory.

Prior to the publication of the second edition of Structure, disciplinary criticism of Kuhn developed along four major lines within the history and philosophy of science: confusion over the concept of paradigm, disagreement with Kuhn's characterization of scientific education, dismissal of normal science, and attacks on Kuhn's conception of science as revolutionary. Several critics found fault with Kuhn's most basic concept, his notion of paradigm, calling into question the validity of his entire theory. The ambiguity of Kuhn's initial definition of the term allowed a wide latitude of interpretation. Critics such as Shapere attacked that notion of paradigm as too broad; it allowed the paradigm to be too many things to too many people. Supporters of Kuhn's position argued in the other direction, seeing the latitude of the definition as a strength and applying the concept of paradigm in the broadest of senses. Still, others simply attempted to redefine Kuhn's concept using other terms such as "basic assumptions" or "stable theoretical framework." Responding to such criticism compelled Kuhn to rein in some of the ambiguity of the paradigm in the second edition of Structure and limit his definition to two major senses: disciplinary matrix and exemplar.

Similarly, Kuhn's critics in the history and philosophy of science divided over his characterization of the nature of scientific education as the teaching of paradigms and the promulgation of dogma. Some begrudging admitted that Kuhn's characterization might

hold true at times, attributing those times to poor teaching and equating that teaching to indoctrination. Others dismissed Kuhn's characterization as completely wrong. The entire line of criticism followed the premise that Kuhn's observations about scientific education and textbooks did not reflect the ideals of a scientific method more closely aligned with the Kuhnian concept of extraordinary science; teaching dogma--indoctrinating would-be scientists in some overarching paradigm--could be construed as stifling curiosity.

While several of Kuhn's critics debated his description of the nature of scientific education, others railed against his definition of the normal activity of science as puzzle-solving. They countered Kuhn's emphasis on puzzle-solving by pointing to Kuhn's companion concept of extraordinary science as the more accurate description of the scientific enterprise. Opponents such as Popper regarded Kuhnian normal science as a danger, declaring that such science was not normal or represented some lesser applied or "hack" science. Under this line of attack, the scientist always questions the ruling dogma or paradigm, challenging the underlying critical framework and stimulating scientific advance through the competition of ideas, not the confirmation of the existing paradigm.

Questioning Kuhn's definitions, his description of scientific education, and his concept of normal science cast greater doubt on the efficacy of his characterization of scientific advance by revolution rather than accretion. Some found Kuhn's dependence upon a gestalt switch between incommensurable positions especially vulnerable. They warned against acceptance of Kuhn's revolutionary hypothesis, suggesting that acceptance of Kuhn's position depended upon accepting the criterion of the absoluteness of incommensurable views. Proponents of the fourth line of criticism often reduced the importance of Kuhnian revolutions to mere variation.

The 1970 expanded version of Structure met with similar criticism from historians and philosophers of science as well as members of an emerging field of inquiry, variously termed either the sociology of knowledge (or of science) or science studies. Neither Kuhn's "Reflections" nor "Postscript--1969," both appearing in 1970, changed many of the objections already registered against his theory by earlier critics. For instance, Shapere, in "The Paradigm Concept," a 1971 review in Science, panned Kuhn's second edition of Structure as more positivistic and confusing than the first. The appearance of "Second Thoughts" in 1974 did not appear to contribute to changing the opinions of critics either. Many continued to reject his characterization of normal or basic research as the most essential type of science. Others within Kuhn's community continued to attack his notion of paradigm, finding fault with his original definitions of paradigm, shift, crisis, revolution, and normal science as well as with his new characterization of paradigm as both "disciplinary matrix" and "exemplary scientific achievements." Yet, as members of his own profession spiritedly debated many of Kuhn's central concepts, those outside of the history, philosophy, and sociology of science--especially those in the social sciences, humanities, and education--were "discovering" Kuhn and applying his ideas to explain their own fields.

In their 1977 Paradigms: An Annotated Bibliography with Special Reference to Research on Teaching, N. L. Gage, Alexis L. Mitman, and Dale H. Schunk list over 180 sources drawn from the general sciences, the social and behavioral sciences, psychology, educational research, and teaching reflecting the spread of Kuhn's theory. Gage, perhaps one of the earliest borrowers of Kuhn, already claims as early as 1963 that Kuhn's ideas have application well beyond the realm of the history of science and introduces those ideas into education research and the teaching of English. The disciples of Kuhn, preaching his gospel in other fields with relentless vigor, fan the winds of revolution often without seeking any deeper than Kuhn's own words in their "bible," The Structure

of Scientific Revolutions. In composition studies, like many other fields outside of the natural sciences, importers of Kuhn appear; most rely on the 1970 second edition of Structure as their guide, accepting and interpreting Kuhn without resorting to his other works or those of his critics within his own professional community.

CHAPTER FIVE

THOMAS KUHN AND COMPOSITION STUDIES, 1963-1981

> We are the ones at the center who reach to all other disciplines and to all other people. We synthesize knowledge and unite people. By our force, we draw from the wisdom of other disciplines and in making it ours, transform it by combining it in new ways.[20]
>
> --Richard Lloyd-Jones, 1978

If Kuhn's ideas inflamed debate as well as ardent support within the history and philosophy of science, what response did those ideas invoke elsewhere? The 1962 publication of Structure opened a Pandora's box of questions for other disciplines as Kuhn's ideas circulated. While the community studying the history and philosophy of science argued about the validity of Kuhn's interpretation of scientific development and advancement, other fields of inquiry began looking at his theory and suggesting that it applied to scholarly work within their own fields. By 1977, N. L. Gage, Alexis L. Mitman, and Dale H. Schunk state in their report, Paradigms: An Annotated Bibliography with Special References to Research on Teaching, that "the paradigm is almost universally recognized to be the intellectual root of research or scholarship in any field" (1). Their report consists of a partially annotated bibliography of over 180 entries addressing scholarship on Kuhn in a number of fields.

When did Kuhn's ideas cross the boundary into composition studies? Who assisted the border crossing? How did importers interpret Kuhn's ideas, and for what ends did they utilize those interpretations? Answering these questions requires exploring works spanning over three decades. The exploration begins with the 1963 San Francisco

Conference on Research Design and the Teaching of English. The conference, part of a series of conferences on problems within a broader context of a discipline of English that included composition only as a subcomponent, concentrated upon research into pedagogical issues but also provided a venue for the introduction of Kuhn's ideas.

Early Borrowings of Kuhn's Work, 1963-1970

> If the research worker has a successful paradigm, it yields positive results, and we witness a steady accretion in our power to understand, predict, and improve the kind of learning that teachers bring about.[21]
>
> --N. L. Gage, 1963 (1964)

The borrowing and use of Kuhn's ideas in the initial period following the publication of Structure seems somewhat sparse. Though borrowings from the history and philosophy of science appear in CCC, few of those borrowings involve Kuhn's ideas. The period is heavily influenced by the growing emphasis on empirical research spurred by Braddock, Lloyd-Jones, and Schoer's 1963 NCTE research report, Research in Written Composition. The report emphasizes the use of quantitative methodologies borrowed from the sciences to help analyze texts, explore the composing process, and theorize about the practice and the teaching of writing. By favoring the scientific method and treating scientifically conducted studies in composition favorably, Braddock, Lloyd-Jones, and Schoer encourage borrowing from the sciences as a means for achieving greater validity for research into an emerging discipline.

A year after the 1962 publication of Kuhn's essay, N. L. Gage, Professor of Education and Psychology at Stanford University, presented "This Side of Paradigms: The State of

Research on Teaching English" at the November 25-27, 1963 San Francisco Conference on Research Design and the Teaching of English. The San Francisco Conference, planned by the National Council of Teachers of English (NCTE) was the fourth in a series of conferences concerning issues of research in the teaching of English. In his opening address, David H. Russell declared that the purpose of the conference was not to develop "another *list* of needed studies in the teaching of English," but instead, to explore "in some depth the process of investigating problems in English" (1). Russell went on to set some objectives for the conference:

> We want some patterns, if patterns are possible, for empirical and other approaches to the study of English teaching. One of our purposes, accordingly, is that of producing some models, some suggestions for typical ways in which problems can be analyzed and attacked. (Russell 2)

After noting that "Interdisciplinary is still a fashionable word" (2), Russell summed up his hopes for the conference by asking attendees to "tackle the questions of model studies, varied methods of research, interdisciplinary approaches, and the role of the design expert" (3).

Gage's paper was one of four that introduced the conference and served to indicate the scope of the material and the problems in research that attendees should address. In his paper, Gage reviews issues concerning research on the teaching of English and uses Kuhn's concept of paradigms as an organizing principle for mapping out potential conflicts. Gage states in his second paragraph:

> In particular, Thomas Kuhn, in a 1962 volume entitled The Structure of Scientific Revolutions, has made much of the importance of paradigms. By one of

> his definitions [Kuhn's], a paradigm consists of the "universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners" of science. . . . Having a good paradigm makes it unnecessary for scientists to examine first principles and to justify themselves continually. It is symptomatic of the state of research on the teaching of English. . .that we should reexamine our first principles. (Gage, "This Side," 22)

Rather than use Kuhn's examples of paradigms and normal science from the physical and biological sciences, Gage applies Kuhn's principles to the field of psychology and offers the exemplars of factor analysis, conditioning theory, and Freudian theory. He notes that each of these diverse movements in psychology "has set a pattern for a host of conceptual, theoretical, and methodological developments that has kept several generations of scientists busy in the kind of mopping up operation that, in Kuhn's view, constitutes the work of normal science" (Gage, "This Side," 23). In the field of scientific work in education, Gage suggests that:

> [P]erhaps the paradigm of evaluation, developed by Ralph Tyler, qualifies as such a system of ideas. . . . It seems fair to say that a whole generation of measurement workers in education has devoted itself to the application, refinement, extension, and elaboration of this paradigm enunciated and promulgated by Tyler in the early 1930's. (Gage, "This Side," 24)

Using exemplars from his own fields helps Gage justify applying Kuhn's work to researching the teaching of English, and ultimately to what becomes composition studies. As a professor of education and psychology, Gage is more concerned with researching the behavior of practitioners or teachers in the classroom than in the content of English

itself; in Gage's paper, one can almost substitute any field for English. Significantly, Gage does not set out to identify any paradigm in English other than what he sees applying to the types of research needed and being conducted. While he asks, "Do we have paradigms in this field [research in the teaching of English]?" he notes that:

> It may well be that we have nothing in this field at present by name of paradigm. That is, we have nothing comparable to the powerful systems of ideas that Kuhn dealt with in the natural sciences. We may well be in a pre-paradigmatic stage of development where no scientific achievements have yet compelled the kind of assent that marshals a whole army of research workers behind it. But if we do have paradigms, their state of health badly needs examination. Making this examination may hasten the arrival of better paradigms. (Gage, "This Side," 24)

Using Kuhn's ideas, Gage seeks to organize research into the teaching of English along three lines; he wants "to divide the overall notion of paradigm into three parts: substantive, methodological, and logistical" (Gage, "This Side," 24), noting later that each part may also function as a kind of paradigm on its own and does not "exist in isolation" (Gage, "This Side," 25). Gage's tripartite division reflects his analysis of the research process. He focuses on the potential of each part to affect research on teaching English. Gage defines the three parts:

> By "substantive," I mean the concepts, the educational and psychological variables, and their relationships, that constitute the subject matter of the field, its way of abstracting and focusing on a certain set of the infinite variety of phenomena in the natural world. For example, methods of teaching and types of achievement are substantive concerns of research on teaching.

By "methodological," I mean the ways of measuring variables, and determining relationships between them, namely, the tests and statistical methods, the ways of collecting and analyzing data, that are used in research. In research on teaching, methodological concerns deal with schemes of observation, rating, judging, testing, experimenting, and with ways of correlating, analyzing variance, factor analyzing, and so on.

Finally, by "logistical," I mean the whole host of concerns with men and money, organization and administration, that are entailed in getting research planned, executed, interpreted, communicated, applied, and approved. (Gage, "This Side," 25)

Gage argues that "Kuhn was right in viewing paradigms as tightly intertwined systems of concepts, theories, instruments, and methods." Gage acknowledges that his reduction of Kuhn's ideas into three separate parts "does violence to the realities of research life" and that he discusses "them in this way only for the sake of reducing complication" (Gage, "This Side," 25).

Gage begins his discussion of "the substantive paradigms" with a question, "What are the variables and the theories of learning and teaching that have shaped research on teaching?" Lacking "the background for more than an illustrative attempt to identify these substantive paradigms," Gage admits that: "The answer must be put in general terms because paradigms refer to larger conceptions that govern scientific effort and not to the specific forms they take in any given research project" (Gage, "This Side," 25). As Gage sees it:

[C]urrent paradigms for research on teaching English have as one of their bases a distinction between composition and literature, between production of writing

> and the assimilation, or appreciation, of writing. Within each of these major rubrics of concern, further distinctions seem to prevail. Within composition, influential distinctions are made between various kinds of writing, such as expository or literary. Further distinctions are made between levels of analysis of writing--i.e., between the sentence, paragraph, and the whole theme, between grammar and usage, between clarity and emotive power, between denotation and connotation. (Gage, "This Side," 25)

Stated almost exclusively in terms of student production or expected achievement, the distinction between composition and literature reinforces the San Francisco Conference's, as well as Gage's, orientation towards the study of writing under the rubric of a larger discipline of English.

To conclude his discussion of substantive paradigms, Gage asserts:

> Now, these paradigms of learning and teaching, or others like them, seem to me to shape the research that gets done, even when the investigator is unaware of them. If the research worker has a successful paradigm, it yields positive results, and we witness a steady accretion in our power to understand, predict, and improve the kind of learning that teachers bring about. (Gage, "This Side," 27)

Turning to methodological paradigms, Gage is less forthcoming. He wants "to call attention to the methodological paradigms underlying experiments on teaching methods or correlational studies of teachers' personality traits," yet fails to offer anything explicit beyond generalities. He suggests that too much faith is held "that what a teacher does makes a difference," a generality that "is unwarranted" (Gage, "This Side," 27-28).

Applying Kuhn's ideas about paradigms to the logistical problems of research on teaching English, Gage compares the realm of research as it existed before the nineteenth century with the development of the modern university and subsequent developments in research after Word War II. World War II saw "the old-fashioned autonomous professor paradigm" shifting with the development of "the university-affiliated but autonomous research laboratory," the organization of scientific societies, and the growth of professional journals (Gage, "This Side," 29).

In briefly mapping the growth of the university and the dominance of the professor as scientist or researcher, Gage claims that he "merely" tries "to illustrate" his conception of "logistical concerns and paradigms" as they relate to "the logistics of research on the teaching of English." He suggests that "the logistical paradigms may still be sufficiently unsettled to allow for rational planning." Gage then asks a series of questions addressing his view of key logistical concerns within a broad framework consisting of finding the necessary researchers, organizing teachers of English "to foster the proper conduct and communication of research," and thinking about paradigms of interdisciplinary collaboration between English and educational research (Gage, "This Side," 30). Gage states that "research on teaching English will become a serious enterprise only to the extent that solutions to these kinds of problems [the questions he raises] will be found" (Gage, "This Side," 31).

N. L. Gage's application of Kuhn to research on teaching English represents an early attempt to impose Kuhnian theory on composition studies. Like Kuhn's own broad, multi-dimensional, dynamic definition of "paradigm," Gage's definition sways and bends to meet his purposes. Using examples from "the natural sciences, psychology, and education," he suggests the potential shape of paradigms within composition research, calling attention to the need to clarify them (Gage, "This Side," 31). As an education researcher, not a compositionist, Gage focuses solely on the implications of Kuhn's ideas for research and

avoids addressing the larger question of identifying the paradigms underlying the study of composition and English. His broad treatment of Kuhn and interpretation of paradigm-driven normal science serve as a platform to ask questions about the interdisciplinary collaboration between English teachers and education researchers.

Gage's talk also speaks to the permeability of composition studies' borders. Gage's primary interests lie in educational research and psychology. He attended the San Francisco Conference as a research specialist on teaching, not a specialist in composition or English. Further, Gage's conference paper later becomes a source for others borrowing Kuhn's theories and applying them to composition research. Thus, Gage's "This Side of Paradigms: The State of Research on Teaching English" forges an important early link between Kuhn in his own field and Kuhn's reinterpretation in composition studies.

Gage's early use of Kuhn to promote examination of issues related to research on teaching English does not transfer over to College Composition and Communication (CCC). Throughout the sixties and the first part of the seventies, CCC displays an intense interest in pedagogy and is the site of internecine warfare among the linguists, compositionists, and other elements composing its readership. The journal pays little heed to Gage or the results of the San Francisco Conference except for a few reports of quantitative or statistical research of direct interest to CCC's readers or calls for research into particular problems or concerns. Albert R. Kitzhaber, for example, writes about research needs in "4C, Freshman English, and the Future," noting:

> [I]f we in the English teaching profession are serious about wanting to make radical improvements in the teaching of composition . . . we shall have to start asking fundamental questions about the act of writing, and asking them in a way

> that will make it possible to get answers based on more than hunches and personal theories and long-established custom. . . . (Kitzhaber 133)

While much is made in the sixties of inquiring into principles guiding composition, only one author acknowledges the potential influence of "paradigms." In a 1968 article, "A Paradigm of Discovery," E. M. Jennings harnesses one term from science, "paradigm," in its sense of a model, and argues for investigation into creativity, invention, and discovery. But Jennings's use of the term fails to suggest knowledge of Kuhn's work. Instead, Jennings depends upon Arthur Koestler's 1964 The Act of Creation.

In The Act of Creation, Koestler expresses a number of ideas similar to as well as competing with Kuhn's interpretation of scientific advance. Koestler, for example, in a chapter on the evolution of ideas and the history of science, writes:

> The various phases in the historic cycle [of science] correspond to the characteristic stages of individual discovery: the periods of creative anarchy to the period of incubation; the emergence of the new synthesis to the biosociative act. It may emerge suddenly, sparked off by a single individual discovery; or gradually, as in the history of electromagnetism, where a series of individual discoveries acted as "links." Each revolutionary historic advance has a constructive and destructive aspect: the thaw of orthodox doctrines and the resulting fertile chaos. . . . Lastly, the process of verification and elaboration of individual discoveries is reflected in the map of history as a consolidation of a new frontier--followed by the development of a new orthodoxy, a hardening of the collective matrix--until it gets blocked and the cycle starts again. (Koestler 253)

While Koestler states elsewhere in his work that the progress of science is evolutionary, his conclusions about the cycle of scientific inquiry suggest the shifting paradigms and revolution that Kuhn wrote about two years earlier. The use of Koestler fits with the interest in discovery and creation that is the crux of Jennings's article.

Writing two years after Jennings, Walker Gibson cites James B. Conant's 1952 work, Modern Science and Modern Man--a source probably familiar to Kuhn as Kuhn dedicated the first edition of The Structure of Scientific Revolutions to Conant--in a deconstruction of "the metaphor of exploration and discovery, the writer as map-maker." In his 1970 article, "Composing the World: The Writer as Map-Maker," Gibson defines the metaphor as the view that "the writer 'explores' his environment, he charts it, and his success can be measured by his degree of accuracy in reflecting the actual landscape" (255). Gibson illustrates the difficulties and breakdown of the map-maker metaphor using the analogy with science provided by Conant's work.

According to Gibson, Conant compares eighteenth and nineteenth century scientists with early fifteenth and sixteenth century explorers and map-makers; after each successive voyage of discovery, the investigators brought back better knowledge and produced more accurate maps. In Conant's view, the eighteenth and nineteenth century scientists labored under a fundamental misconception by imagining "themselves as the equivalent of the early explorers and mapmakers" (Qtd in Gibson 255). Conant suggests that the investigators believed that those working in laboratories could, like the early explorers beginning with imperfect knowledge and the aid of imperfect maps, present more and more accurate accounts of reality through a series of successive approximations. Conant asserts that these scientists held the view that "Given time and patience, it was assumed that truth would be ascertained. By the same token there must be a truth about the nature of heat light and matter" (Qtd in Gibson 256). Conant writes:

> [I]n view of the revolution in physics, anyone who now asserts that science is an exploration of the universe must be prepared to shoulder a heavy burden of proof. To my mind the analogy between the map-maker and the scientist is false. A scientific theory is not even a first approximation to a map; it is not a creed; it is a policy--an economical and fruitful guide to action by scientific investigators. (Qtd in Gibson 256)

The passages from Conant suggest much of Kuhn's theory about the revolutionary nature of science and the influence of paradigms on research and normal science. Conant, one of Kuhn's mentors, speaks of revolutions, and without using Kuhn's terminology, comments on the policy commitments suggested by adherence to paradigms.

Like Jennings's use of Koestler, Gibson's use of Conant represents an importation from the history and philosophy of science, but suggests another missed opportunity to borrow Kuhn. While Jennings and Gibson depend upon authorities from the history and philosophy of science to validate arguments for particular positions, most other authors writing in CCC during the decade continue the debate about the nature and structure of English, cry for research into various topics, offer pedagogical tips, report teaching successes or failures, or reveal the results of their individual experiments, surveys, and inquiries.

Borrowing Kuhn in the Early Seventies, 1971-1974

> The field of discourse is still in what Kuhn, a notable historian of science, has called the preparadigm period. That is, there has not yet been erected a

> comprehensive system of the discipline which has received some general acceptance and which could serve as a framework for research, further speculation, innovation, even repudiation.[22]
>
> --James L. Kinneavy, 1971

The initial period following the publication of Kuhn's work shows a limited effort by compositionists to borrow from the history and philosophy of science. Some scholars turn to these disciplines for validation of inquiry into composition. Using the views of scientific advance held by Conant and Koestler to validate particular positions within composition represents a claim to legitimization by showing how closely the pursuit of knowledge in composition mirrors scientific inquiry. The desire to establish inquiry into writing practice and pedagogy as academically valuable points (for us) to the potential usefulness of Kuhn's ideas. Gage's direct citation of Kuhn remains an isolated incident, awaiting discovery within channels more accessible to compositionists.

James L. Kinneavy exploits one of those channels in his textbook, A Theory of Discourse: The Aims of Discourse (1971). Kinneavy relies on Kuhn's 1963 essay, "The Function of Dogma in Scientific Research," as well as Kuhn's 1962 version of Structure to help frame A Theory of Discourse, perhaps the earliest textbook applying Kuhn's ideas to composition and discourse theory. Kinneavy equates discourse with composition and argues for the establishment of a theory of discourse that will reduce or eliminate "the present anarchy of the discipline" (1). Early in his book, Kinneavy avers:

> The present anarchy of the discipline of what is commonly categorized as "composition" . . . is so evident as to scarcely require proof.
>
> Composition is so clearly the stepchild of the English department that it is not a legitimate area of concern in graduate studies, is not even recognized as a

> subdivision of the discipline of English in a recent manifesto put out by the major professional association (MLA) of college English teachers, in some universities is not a valid area of scholarship for advancement in rank, and is generally the teaching province of graduate assistants or fringe members of the department. (Kinneavy 1)

For Kinneavy, the chaotic nature of the field and its lack of legitimacy points to the need for some unifying direction as well as validation of composition studies as scholarly inquiry. To highlight the chaos, Kinneavy simply cites the confusion over composition course content; he makes note of "such governing concepts as expository reading and writing, a survey of logic, new rhetoric, grammar (traditional, structural, or generative), reading in linguistics, semantics, [and] beginning literary analysis" (2). Kinneavy claims:

> Such a rich variety on the surface might suggest an underlying poverty. It does suggest one clear hypothesis: there is no definite concept of what the basic foundations of composition are. There is even the uneasy suspicion that there is nothing more to composition *as* composition than what could and should be covered in an adequate high school course. (Kinneavy 2)

But Kinneavy proceeds to discount the poverty hypothesis and dismiss the "uneasy suspicion," arguing that "the field of composition--or discourse as it will presently be termed--is a rich and fertile discipline with a worthy past . . . an exciting present, and a future that seems as limitless as either linguistics or literature" (2).

Kinneavy goes on to note the tentative nature of his own theory; he recognizes that "little experimental evidence can be cited as conclusive" as to imply a settled theory of discourse or composition (2). Still, he believes that:

> [B]y synthesizing some of the neglected areas of the past and much going on in the present within a framework suggested by modern philosophy and linguistics . . . composition can very legitimately carve out a respectable domain in the field of English.
>
> The advantages of such a theory seem immeasurable. By constructing norms parallel in a sense to the techniques of new criticism, the analysis of writing other than literary and the composition of such works also can be improved. Teachers . . . can be trained more efficiently. Literature itself can emerge more intelligible in its own right. (Kinneavy 2)

At this point in his opening, Kinneavy borrows Kuhn and makes the claim that the field of composition studies, or "discourse" as Kinneavy defines it, is still preparadigmatic in nature. He thus connects his borrowing of Kuhn to the issue of the disciplinarity in composition and relies on Kuhn's 1963 essay, "The Function of Dogma in Scientific Research," for much of the supporting evidence of the claim to preparadigm status. According to Kinneavy:

> The field of discourse is still in what Kuhn, a notable historian of science, has called the preparadigm period (Kuhn, ["Dogma"] 354). That is, there has not yet been erected a comprehensive system of the discipline which has received some general acceptance and which could serve as a framework for research, further speculation, innovation, even repudiation. Kuhn sketches the pattern of

> development of some other disciplines before the advent of a workable paradigm, and the sketch looks very much like a portrait of the field of composition at the present time. Each person begins his work anew from different foundations, the same ground is covered again and again with inconclusive results, there is no way to discriminate among research "accidents" and "essential novelties" (Kuhn, ["Dogma"] 355-56), and there are no systematic commitments which can motivate serious innovation (Kuhn, ["Dogma"] 349). (Kinneavy 2-3)

To bolster his claim, Kinneavy briefly describes one of Kuhn's exemplars, the study of electricity. Kinneavy quotes a passage from Kuhn's "Dogma" essay, noting the inability of electricians before 1740 to "distinguish consistently between electrical and non-electrical effects, between laboratory accidents or essential novelties, or between striking demonstrations and experiments which revealed the essential nature of electricity." Sometime between 1740 and 1780, Kuhn argues that the study of electricity "achieved a paradigm, possession of which enabled them to take the foundation of their field for granted and to push on to more concrete and recondite problems" (Qtd in Kinneavy 3). Kinneavy states that Kuhn considers the achievement of such a paradigm as a clear criterion proclaiming a field as a science. Kinneavy adds: "No paradigm is irrevocable; in fact, the later development of the sciences is from paradigm to paradigm, often within the same generation. But some initial order seems essential if a discipline is to emerge from folk wisdom" (3).

By putting his opening argument in Kuhnian terms, Kinneavy sets composition studies clearly in a preparadigmatic state characterized by great confusion and disagreement. He concludes that the "present anarchy" and chaos in composition results from the lack of a paradigm--the lack of a foundation--from which to orient research

and advancement in the field. Kinneavy hopes to lay some of that foundation through his examination of discourse.

Before continuing further, Kinneavy takes a few moments to briefly define essential terms in his argument. He mentions "rhetoric" in its sense of "the whole field of the uses of language," but restricts its usage, declaring that he will not use it "in the sense of a general science or art of communication" (Kinneavy 3-4). He avoids the use of the term, "communication," suggesting that its stress on skills *as* skills (of speaking, listening, writing, and reading) and connection "with an important theory of electric and electronic signal transmission" renders it inappropriate for describing discourse theory (Kinneavy 4). While admitting that "composition" is the term most frequently used to cover many of the kinds of problems he addresses, Kinneavy finds fault with "composition" as an adequate description because:

> [I]t rarely refers to oral communication and this book intends no such exclusion; in etymology and in practical usage it embodies almost solely a "process" connotation which is not desirable in this instance; and . . . it is frequently associated with the too sterile and static disciplines of high school and freshman composition courses: finally, the term is not used in the frameworks of philosophy and linguistics which structure the present [Kinneavy's] approach. (Kinneavy 4)

Ultimately, Kinneavy settles on "discourse" as the most important term for framing his argument and theory. He uses discourse in the sense of referring "to the full text (when feasible) of an oral or written situation; it does not denote necessarily a rational or logically coherent content; the discourse can be directed to any aim of language or refer to any kind of reality" (Kinneavy 4). To Kinneavy, a "theory of discourse will then

comprise an intelligible framework of different types of discourse with a treatment of the nature of each type, the underlying logic(s), the organizational structure of this type, and the stylistic characteristics of such discourse" (5).

In the remainder of his book, Kinneavy sets out to erect such a framework using the "only available criteria" of "the history of theory and practice and a survey of current theory and practice" combined with an attempt to comprehensively synthesize "the valid historical and theoretical components of the discipline of English" (5). He offers a brief history of college education in English discourse, citing major historical trends and important movements. At the same time, he disclaims any attempt to offer "an exhaustive coverage" (5).

Kinneavy proceeds to outline the foundation of his own theory for the structure of English study, basing that theory upon the communication triangle. He relates the triangle to the arts, media, modes, and aims of discourse, defining each in turn and specifying the nature of the relationship. It is in his attention to the aims of discourse that much of Kinneavy's theory rests; indeed, The Aims of Discourse is the subtitle of his textbook.

In his taxonomy of discourse types, Kinneavy divides the aims of discourse by the way each type stresses the various components of the communication process. He sees four major classes of discourse: reference, with its subclasses of informative, scientific, and exploratory discourse; persuasive; literary; and expressive. Kinneavy devotes an entire chapter to analyzing each class of discourse. When analyzing reference discourse, Kinneavy returns to Thomas Kuhn's work to help explain the differences between the informative, scientific, and exploratory subclasses.

In his first reference to Kuhn in chapter three, Kinneavy mentions the views of J. Robert Oppenheimer concerning the difficulty in translating scientific writing into conventional lay discourse that sacrifices none of its scientific integrity in the

translation. According to Kinneavy, Kuhn states that Oppenheimer's view is seconded by others (Kinneavy 74). Briefing citing Kuhn here calls attention to Kuhn's point deploring "the widening gulf that separates the professional scientist from his colleagues in other fields" (Kuhn, Enlarged 21) and, indeed, the average lay person who does not share any of the accepted paradigms that form the foundation for understanding a particular field of inquiry.

As Kinneavy investigates the nature of scientific discourse in chapter three, Kuhn becomes one of a long list of users distinguishing between exploration and scientific proof. Kinneavy suggests that several ambiguous terms "on occasion synonymous with science (and information and exploration) . . . can refer to discourse which makes no pretense at reporting reality or proving anything" (78). For Kinneavy, informative or scientific discourse refers indiscriminately to process, event, structure, idea, etc. The use of Kuhn, among others, reinforces Kinneavy's distinction between demonstration and discovery in reporting information and research:

> Demonstration, as a process of proof, has a much longer history than the term "demonstrative" speech. The logic of *demonstration*, in the sense of effective proof for a hypothesis, has long been distinguished from the logic of *discovery.* The tradition of such a distinction extends back to Bacon, Descartes, Galileo, and Newton, and is still a very live issue. . . . It is the basic distinction between scientific and exploratory discourse. "Demonstration," in this sense, is synonymous with "confirmation" and "justification" (Kuhn, [Structure] 8-9). (Kinneavy 78)

The passage referred to in Kuhn speaks of Kuhn's own acknowledgment that he "may even seem to have violated the very influential contemporary distinction between 'the

context of discovery' and 'the context of justification'" in his own use of history to make conclusions about the nature of science (Structure 8). Kuhn remarks that such distinctions: "Rather than being elementary logical or methodological distinctions which would thus be prior to the analysis of scientific knowledge . . . now seem integral parts of a traditional set of substantive answers to the very questions upon which they have been deployed" (Kuhn, Structure 9). He goes on to stipulate that such distinctions are "parts of a theory" and subject "to the same scrutiny regularly applied to theories in other fields." Kuhn notes: "If they [the distinctions] are to have more than a pure abstraction as their content, then that content must be discovered by observing them in application to the data they are meant to elucidate" (Structure 9).

As further evidence of the distinction between exploration and scientific proof, Kinneavy charts Kuhn along with other theorists drawn from throughout history in a series of figures. First, Kuhn's ideas from both Structure and the "Dogma" essay appear on a table (Kinneavy's Figure III, 1) developed by Kinneavy to compare various distinctions between exploration and scientific proof made by significant "users" (98). Kinneavy's Figure III, 1 begins with Plato's distinction between exploration as "opinion" and scientific proof as "knowledge by dialectic," and extends through the various distinctions made by such notables as Aristotle, Cicero, Newton, Descartes, Kant, Hegel, and Dewey (only part of Kinneavy's entire list). In his explanation of the figure, Kinneavy repeats Kuhn's distinction between exploration in its "context of discovery" and scientific proof within the "context of justification" (Kinneavy 98). He notes that Kuhn objects to exploration in his "Dogma" essay.

Expanding his discussion of the distinction and the appropriateness of separating exploratory discourse from scientific discourse, Kinneavy declares:

> "Exploratory" seems surrounded by fewer misleading connotations than most of the others [the terms referred to in Kinneavy's chart] and is also applicable to discourse. It has been objected to by Kuhn, who feels that it carries connotations of the explorer's haphazard, random, and nonstructured meandering without a goal; he objects to this since nearly all modern research is not of this type (Kuhn, ["Dogma"] 363). (Kinneavy 99)

Further, Kinneavy's Figure III, 1 presents "some rough parallelisms in several dialectical procedures" (100). He writes:

> [M]any violent dissimilarities have been abstracted from in order to focus . . . on similarities. And even some of the similarities are uneasy in the indicated categories. Yet the synthesis seems worthwhile, as a tentative formulation. . . . The formulation pictures only the exploration half of the full cycle of what is usually called the "scientific method." Exploration leads to a testable hypothesis which scientific proof then demonstrates as tenable or not. (KInneavy 100)

Kinneavy represents the relationship of exploration to hypothesis and scientific proof in a second figure (Kinneavy's Figure III, 2) by drawing a continuum between exploration and scientific proof with the hypothesis being tested lying midway between the other two (100).

In his Figure III, 3, Kinneavy compares Kuhn's theory with the theories of six others, outlining the structure of their views on exploration (101). Kinneavy examines the theorists on six points: preliminary knowledge of the field, cognitive dissonance, focus of problem, search for new model, composition of new model, and hypothesis or

tentative explanation. Of the seven theorists listed in the original figure, Kinneavy writes that Kuhn's position is perhaps the most explicit of those listed in Figure III, 3.

An abbreviated version of Figure III, 3 appears below as Table 1 of this dissertation; in it, I compare only three of the seven theorists--Plato, Hegel, and Kuhn--Kinneavy uses in his original figure in A Theory of Discourse.

Table 1. Abbreviated Examination of Kinneavy's Figure III, 3: Some Similarities in Designs of Exploration.

Person	Plato	Hegel	Kuhn
Preliminary Knowledge of Field	Wonder	Analytic Thesis	Initial Acceptance of Current Dogma
Cognitive Dissonance	Division	Instability	Anomalies
Focus of Problem	In Rumor, then Conjecture, then Thought;	Dialectical Thesis	Crisis
Search for New Model	Definition, Hypothesis, Deduction, Elentric	By Antithesis	Search for New Paradigm
Imposition of New Model	Testing by Induction, Doubt	Thesis vs. Antithesis	
Hypothesis: Tentative Explanation		Synthesis	

Comparing three of seven users from Kinneavy's more extensive Figure III, 3 reflects the differences that Kinneavy seeks to illustrate in his differentiation of exploration and scientific proof. (Adapted from James L. Kinneavy's A Theory of Discourse: The Aims of Discourse, Englewood Cliffs: Prentice-Hall, 1971. 101.)

Kinneavy chooses to use Kuhn's position as the example to explicate the details of Figure III, 3. He describes Kuhn's position using points established in the "Dogma" essay, writing:

> In the first stage [Preliminary Knowledge of the Field], possibly the most categoric [sic] position is taken by Kuhn who designates the current theories by the term "dogma," for the process of becoming aware of them is almost like an indoctrination (Kuhn, ["Dogma"] 350). This acceptance of current paradigm serves several purposes: it helps a discipline move forward and prevents recurrent starts from bald rudiments each time, and it frames the field of the discipline; and thus serves as an idea source for the routine research of the discipline; but especially it serves as the norm from which deviations are observable. Without a norm, there are no abnormalities (Kuhn, ["Dogma"] 365). . . .
>
> Once the current dogma is known, it establishes a framework of consistency. Inferences from it should prove valid and observed facts relevant to it should be consonant with the dogma. The establishment of these inferences and congruent sets of observed facts constitutes the normal process of research (Kuhn, ["Dogma"] 360). They do not challenge the dogma; they extend, apply, and confirm it. (Kinneavy 102)

At the "Cognitive Dissonance" stage of Kinneavy's Figure III, 3, inconsistencies arise in the dogmatic system (Kinneavy 102). Kinneavy states:

> But it may happen that certain inferences deduced from the dogmatic system seem inconsistent with prior axioms or inferences in another part of the system. Or

certain observed facts do not seem consistent with what the system would have predicted they should be. A dissonance between the dogma and these inferences and observations is created. This disagreement creates wonder, instability, or discomfort. (KInneavy 102)

During the third and fourth stages of the process Kinneavy outlines in his table, the dissonance becomes more acute as the number of anomalies rises and eventually forces a search for a more appropriate model.

> If enough of these anomalies exist, they may converge into a crisis which calls into question some features of the dogma itself (Kuhn, ["Dogma"] 367). The problem has now crystallized and demands some solution. This stage of the process is called the focus of the problem, for the issue has emerged from indeterminacy and can be stated as an alternate thesis to the dogma.
>
> Since the dogma no longer provides a framework for solution, a new framework must be sought elsewhere. (KInneavy 102-103)

Part of the search for the new pattern involves negation of the old dogma as a component.

> But mere negation is often not enough. The positive aspect of the exploratory process, once the issue has become clear, consists in the search for a new theory, model, or paradigm. The new model or paradigm, coming from without the confines of the dogmatic camp, almost invariably strikes an intrusive and nonconformist note. . . .

> Since the current concepts about the given reality are unsatisfactory and may be unclear, the tendency is to search for a somewhat similar issue which is presently clear and satisfactory and see if the pattern of the familiar can be imposed on the unsatisfactory and unclear. This familiar and clear pattern imparted from elsewhere is the new model. It can be defined as a similarly structured pattern of design which helps to explain, describe, or even predict a given reality. . . . The model, imported from the already familiar, clear, explained, better organized, and proven in the secondary domain, carries with it the possibility of transferring its own clarities, explanations, structures, and proofs into the primary domain of the given reality to be explained. . . . This projection of the known to explain the unknown gives rise to the fertility of the model. . . . (Kinneavy 103)

Kinneavy depends noticeably upon Kuhn in his explanation of exploratory discourse. By setting up the stages of his Figure III, 3 as he does in the chapter on reference discourse, Kinneavy can compare similarities with other major theorists, thus lending further credence to Kuhn's ideas about scientific advance. At the same time, Kinneavy also makes an exploratory voyage of his own in an attempt at coalescing elements of a discipline characterized, in Kinneavy's view, by anarchy.

If composition, as a domain of scholarly inquiry, is "preparadigmatic" as Kinneavy claims early in his book, then it has not yet reached Kuhn's state of "Initial Acceptance of Current Dogma" because the field has not yet come to some agreement on just what that "dogma" is; for Kinneavy, composition studies is still floundering in a chaotic sea of conflicting theory. He acknowledges that we profess a number of certain practices, yet members of the profession do not even agree on those.

Kinneavy's use of Kuhn in 1971 represents a strong move toward disciplinehood and a defense of borrowing simultaneously. As noted in the passage above, the search for a new paradigm often involves going outside the given provenance of a field to find "clear and satisfactory" patterns elsewhere that may prove helpful in explaining, describing, or predicting events. Kinneavy does just that by borrowing Kuhn's ideas to help pattern composition studies' push toward disciplinarity. To Kinneavy, Kuhn's theory about the process of paradigm-governed advances in science offers an attractive structure for explicating what is happening in composition studies.

The second edition of Kuhn's The Structure of Scientific Revolutions appeared in 1970, offering a second opportunity for articulation and importation of Kuhn's theory into composition studies. The first to come near to seizing that opportunity in CCC seems to have been Thomas M. Sawyer in a December 1972 article, "Rhetoric in an Age of Science and Technology." Sawyer draws upon the philosophy of science and on Kuhn to focus attention on the relationship between rhetoric, science, and technology, to call attention to the need to train more rhetoricians, and to come to the aid of scientists "by helping them and their students to communicate their knowledge to the public at large" (398).

Sawyer calls upon Kuhn in an examination of one of the rhetorical characteristics of the deductive systems that scientists establish governing inquiry. A scientist "often takes it for granted that his reader understands the major premises with which he began" (Sawyer 396). Asserting that these premises form the paradigm in a developed science, Sawyer writes:

> As Thomas Kuhn has pointed out in his The Structure of Scientific Revolutions, the book which explains the paradigm, the basic premises, has [sic] tended to disappear in the developed sciences. Those working in the field don't need it; they

already take the premises for granted. It is the report of the experiment which tests the deduced conclusion which matters to them. It is only the layman who needs to have the chain of deductions leading up to the experiment mapped out for him. (Sawyer 396)

Sawyer's citation of Kuhn applies directly to the sciences and what scientists do, and not so much to composition studies. His use of Kuhn, like other early uses in CCC, stops short of applying Kuhn's theory directly to composition inquiry. Sawyer makes a plea for greater participation in the communication of scientific discovery by suggesting that rhetoricians can act as the lubricant between the scientist and the layman. In Sawyer's article, Kuhn's sense of paradigms simply serves to address the nature of scientific activity--exactly what Kuhn intended it to do. Sawyer makes no attempt to apply the idea of paradigms or paradigm shifts to composition studies.

Borrowing Kuhn, 1975-1981

We need to develop experimental designs and identify analytical methods especially adaptable to our own discipline. While we must borrow from other disciplines to do this important work, we cannot depend upon others to have the required sensitivity to the complexities of the composition process that we have.[23]

--Vivian I. Davis, 1979

The 1970 publication of the second edition of Structure brought more interest in the applicability of Kuhn's ideas to composition studies. Increasingly, a number of compositionists, such as Frank D'Angelo in his 1976 "The Search for Intelligible

Structure in the Teaching of Composition," join in the attempt "to identify the underlying principles that constitute the discipline of composition" ("Search" 143). Although D'Angelo theorizes that "composition does have an underlying structure which gives unity and coherence to the field," and he calls for identifying those "significant principles and concepts in the field which will make intelligible everything we do" ("Search" 143), he does not refer to Kuhn or the concept of paradigm in his CCC article--even though D'Angelo sounds like he is echoing Kuhn! But others do refer to the concept of paradigm as Kuhn uses it, and in their uses, we can trace much of the spread of Kuhn's ideas.

The use of "paradigm" and related words increases as the last half of the decade progresses. For example, in a chapter in Ohmann's 1976 English in America: A Radical View of the Profession, Wallace Douglas describes the modern orator in terms of a new "paradigmatic image . . . of a man thinking, of a man whom 'increased knowledge' has made 'more contemplative'" (Douglas 116). Ohmann himself talks about "ghostly paradigms" when referring to work in freshman textbooks in 1965 (140). Alan J. Chaffee, in his 1977 "The Ghostly Paradigm in Composition," appearing in CE, uses "paradigm" in its sense of model or pattern to describe Socratic dialogue and suggest its efficacy as a transition between informal conversation and formal argument. When Leslie T. Whipp characterizes the back to basics movement in composition with a "Biblical paradigm" in his May 1979 article, "The Teaching of English and Social-Class Relationships," he uses the term in its sense of model or exemplar without any reference to Kuhn (145). Whipp notes that the use of such a "paradigm" suggests some techniques for surviving with the back to basics movement. In the 1979 "Paradigms as Structural Counterparts of *Topoi*," D'Angelo offers a definition of paradigm as "a *principle of forward motion* that enables a writer to move his or her thinking in an orderly manner from the beginning of an essay to its conclusion" ("Paradigms" 42). In their 1980

"Staying Viable," a humorous glossary of terms currently in vogue, Ann E. and Warner B. Berthoff define *paradigm* as "conventional wisdom; a model of a process; rebirth; idea; unexamined structure; assumption; presupposition; ideology" (85).

Besides the emphasis on paradigm as a model or structure, others add Kuhn's sense of paradigm shift. In "A Cognitive Process Theory of Writing," Linda Flower and John R. Hayes use *paradigm* and point to the idea of a shift when they describe their cognitive process theory of writing "as a *major departure* [my emphasis] from the traditional paradigm of stages . . . of completion which reflect the growth of a written product . . . organized in a *linear* sequence or structure" (367). In her "Essay Writing and Teachers' Responses in Nineteenth-Century Scottish Universities," Andrea A. Lunsford uses Kuhnian terminology when she characterizes a description of an 1840 class in Aberdeen, Scotland as "paradigmatic" and contrasts it with a change in "paradigm" in "modern composition classes" (435).

Robert J. Connors's use of "paradigm shift" is the most specific; Connors writes in his 1981 article, "The Rise and Fall of the Modes of Discourse," that the publication of John Genung's The Practical Elements of Rhetoric "really marks the paradigm shift," the shift in teaching composition "from a concrete, form-based model rooted in literary high culture [the belletristic forms of treatises, biographies, and fiction] to a more pliable abstract model [the modes] that seemed adaptable to anything which a rising young American might wish to say" ("Rise and Fall" 447). Connors returns again to the idea of shift as he charts the passing of the modes and their condemnation to "a shadowy half-life in the attic of composition legends," noting their replacement by other systems such as Frank D'Angelo's "discourse paradigms." He goes on to question whether or not these "paradigms will become as rigid, abstract, and useless as did their progenitors, the modes" (Connors, "Rise and Fall" 453). These uses and other more significant ones suggest a growing familiarity with Kuhn's terminology and perhaps his ideas.

The late seventies, especially 1978-1979, saw oblique and occasionally direct references to Kuhn's idea of paradigms. In her 1975 CCC article, "Interdisciplinary Studies, the Latest Experimental Rage," Joan Baum demonstrates a knowledge of Kuhn. Unlike Sawyer, her use is more subtle than direct and rests in the conclusion of her argument against blindly using inter-disciplinary courses to teach composition. Baum concludes her discussion of interdisciplinary courses by mixing allusions to Kenneth Burke and Thomas Kuhn in the same sentence. When she claims, "There are grammars of motive as well as scientific procedure, paradigms of verbs as well as revolutions" (33), Baum equates the contributions of both men in the ongoing debate over teaching composition. Without mentioning any names, she shows a knowledge of their work and positions herself relative to her contemporaries. Baum indirectly alludes to Kuhn's growing influence in composition.

James Kinney drops Kuhn's name in the December 1979 CCC article, "Classifying Heuristics," to support a claim that, "Most significant advances in knowledge have come through an initially intuitive or empirical process" (354). Kinney writes: "The rationalist construct of modern physics or analytical mathematics, for example, are *post facto* operations explaining the intuitive flash of an Einstein or an experienced fact that was inexplicable by the previous rationalist paradigm--as Thomas Kuhn has pointed out" (Kinney 354). But Kinney, like many others, goes no further, choosing simply to use Kuhn as an example without explanation.

By the turn of the decade, many of the other references in the late seventies and early eighties indicating knowledge of Kuhn's work come as citations of Richard Young's essay, "Paradigms and Problems: Needed Research in Rhetorical Invention" from Cooper and Odell's 1978 collection of essays, Points of Departure. Lisa S. Ede cites Young in her 1979 attack on the current-traditional paradigm's focus on product over process at the expense of attention to the role of audience. Aviva Freedman and Ian Pringle mention

Young's definition of "current-traditional rhetoric" in their 1980 article, "Writing in the College Years: Some Indices of Growth." While Freedman and Pringle also examine different "models" for evaluating abstraction in student compositions and suggest that the current-traditional framework distorts the teaching of process by omission (322), they never take the next step of using Kuhn's ideas to support their own preliminary findings or justify their call for additional research. David Tedlock mentions that most opposition to the case approach to composition that he advocates comes from composition instructors grounded in "the heart of what Richard E. Young calls the 'Current-Traditional Paradigm'" (257). These brief references suggest the growing awareness of Kuhn's ideas spread by access to Young's essay.

Without referencing Young or Kuhn, at least one author briefly mentions Kuhn's concept of paradigms in the sense of the breakdown leading to crisis. In his 1981 review of the English as a second language textbook, Principles of Language Learning and Teaching, John C. Schafer makes a direct comparison between the status of language learning and teaching and that of composition theory and teaching practice. Schafer concludes: "In both fields an old paradigm has broken down, a number of exciting research ventures are underway, and teachers are hammering together their own eclectic theories of language acquisition or composing" (228). The brief space of the book review precludes Schafer from pursuing his thought any further.

Increasing attention to scholarly inquiry in composition also spurs the development of a number of professional resources such as Richard L. Larson's CCC series of selected bibliographies of research and writing about teaching composition. Between 1975 and 1979, Larson's annual selected bibliography of composition materials mirrors bibliographies already available in more established disciplines. Initially, Larson focuses solely on research and classroom experiences, excluding "textbooks, reviews of books, and items dealing with the preparation or in-service training of teachers"

("1973" 187), but Larson's bibliographies highlight much of what is happening in composition research in the seventies. The 1973-74 bibliography contains 57 items, the 1975 bibliography lists 66, and the 1977 bibliography consists of 86 entries, with subsequent bibliographies growing even larger. Larson's 1976 bibliography contains references that use Thomas Kuhn's work, referring to several essays in Gary Tate's 1976 Teaching Composition: Ten Bibliographical Essays, but never directly cites Kuhn or uses his name in any of the annotations. The 1977 bibliography, published in the May 1978 edition of CCC, contains an item using "paradigm" in its title, but not applying to Kuhn's use of the term. The 1978 bibliography, totaling 98 entries, refers to essays in Cooper and Odell's Research on Composing: Points of Departure, especially Richard Young's "Paradigms and Problems: Needed Research in Rhetorical Invention" which offers one of fullest early treatments of Kuhn's ideas.

Larson became editor of CCC in 1980, and responsibility for compiling the annual bibliography passed to others. The growing volume of works in composition and rhetoric meant that CCC could no longer act as a venue for the annual selected bibliography that Larson had begun. For example, Erika Lindemann's 1984-1985 bibliography, published in 1987 as the Longman Bibliography of Composition and Rhetoric 1984-1985, contains over 3800 titles and suggests the strength of the discipline of composition studies. Her CCC Bibliography of Composition and Rhetoric 1990 lists 1849 items published during the 1990 calendar year.

In one sense, the increase in professional publications such as bibliographies functions as growing evidence of the maturity of composition studies as a discipline. Though the early bibliographies show little of Kuhn's direct influence, they do reflect the gradual spread of his theory in the late seventies.

In addition to such brief indicators that knowledge of Kuhn's theory seems to be spreading, the period from 1975 to 1981 contains a number of more significant uses or

treatments of Kuhn's ideas, especially Richard Young's presentation at the 1975 Buffalo Conference on Researching Composing.

At the Buffalo Conference, many attendees including Young made extensive use of Kuhn's theories. The polished essays of many of the presenters appear later in Charles R. Cooper and Lee Odell's 1978 Research on Composing: Points of Departure. The papers, as they appear in Cooper and Odell's collection, suggest the widening influence that Kuhn's theory exerts in the seventies following the publication of the enlarged edition of Structure. Cooper and Odell's Points of Departure is full of references to paradigms and paradigm shifts. Three out of ten chapters in the Cooper and Odell book, treated here, include references to Kuhn directly or indirectly. A fourth, written by Young, provides one of the fullest early applications of Kuhn's theories and merits special attention.

In chapter one of Points of Departure, Cooper, Odell, and their co-author, Cynthia Courts, reference James Kinneavy's 1971 claim about the "preparadigmatic" state of discourse theory, then argue that discourse theory has reached the stage of an "emerging" paradigm. Referring to contrasts with earlier assumptions by others about "the qualities of 'good writing,'" Cooper, Odell, and Courts state:

> It may not be accurate to speak of these new assumptions as a new paradigm; the present state of discourse theory may only be, as James Kinneavy (1971) claims, "preparadigmatic." There is no single set of terms and no single well-established, widely shared body of knowledge that constitutes modern discourse theory. But we may at least speak of an *emerging* paradigm since different scholars are exploring theories that overlap in interesting and useful ways. . . . we shall discuss two of the major assumptions of this emerging paradigm. . . . (Cooper, et al 2)

Cooper, Odell, and Courts never mention Kuhn by name, nor make direct reference to Kuhn's publications in their works cited. They do refer to Kinneavy's earlier use of Kuhn without extensively exploring Kinneavy's claim. Cooper, Odell, and Courts' use of the term paradigm suggests a meaning of a "single set of terms and . . . single well-established, widely shared body of knowledge" (2). They assert that "different scholars . . . exploring theories that overlap in interesting and useful ways" constitutes evidence of an emerging paradigm (Cooper, et al 2), some agreement on direction; their "emerging paradigm" is closely connected to the advances in modern discourse theory.

Without mentioning Kuhn, Loren Barritt and Barry Kroll champion a combination of "various research paradigms" (57) in chapter four. They seem to use "paradigm" in the sense of a specific methodology for conducting research. At the same time, they warn about placing too much dependence upon hard science, such as physics, for answers to problems growing out of complex human behavior. Though not targeting Kuhn per se, Barritt and Kroll complain about the tendency in cognitive-developmental psychology and other fields to borrow ideas and methods from science that fail to accurately account for the complicated nature of human behavior and interaction. They caution against attempts to reduce such complexity to empirical or mathematical terms, thus admonishing borrowers to remember that the composing process is intimately linked with human action.

In chapter five, Janet Emig uses "preparadigmatic" to characterize inquiry into the biological and physiological aspects of composing. She argues about the need for "new vantages . . . especially if our ambition is to attempt sketching and then constructing a model or models of the writing process. Establishing the essential components and structures of the process is, of course, requisite to that effort" (Emig, "Hand" 59-60). Emig's concern for models parallels Kuhn's emphasis on paradigms. Further, Emig, like Cooper, Odell, and Courts, refers to Kinneavy's work on discourse theory. She writes of

Kinneavy's work and that of others: "Even with the most recent work, one has the impression of being in the presence of an inquiry that Thomas Kuhn (1970) would characterize as being in the preparadigmatic stage" (Emig, "Hand" 70). Emig does not yet see any single paradigm "emerging" to dominate the field; the field is still young, engaged in sorting out and establishing positions or points of view.

Each of the essays in Points of Departure suggests a concern for identifying the underlying theory or theories that inform or shape the field of composition studies. With the exception of Young (covered below), the authors only hint at the possibilities of Kuhn's influence by suggesting assessments of the state of composition studies or various theories within the field. By claiming the study of the composing process is "preparadigmatic" or pointing out the presence of an "emerging paradigm," the authors actually evaluate the maturity of composition studies as a discipline. In the cases of Cooper, Odell, and Courts, of Barritt and Kroll, and of Emig, composition studies remains immature, engaged in the development process.

For Young, composition studies possesses greater maturity in the Kuhnian sense. Young's Buffalo Conference presentation on Kuhn, later referred to unpublished in work by Stephen North and Donald C. Stewart, becomes Chapter 3 of Cooper and Odell's 1978 book. In "Paradigms and Problems: Needed Research in Rhetorical Invention," Young argues for research into rhetorical invention and makes a cogent case for applying Kuhn's theories to composition. Young offers perhaps the first full treatment of Kuhn's theory of paradigms and shifts, the revolutionary nature of scientific advance, and the conduct of normal science as it might apply to composition studies.

Young opens with a rhetorical question concerning the argument over what kinds of research are needed in invention. His answer depends upon the acceptance of a proposition concerning the nature of composition studies in general, couched within the

broader theoretical framework of Kuhn's theories of discipline building and paradigms. In his opening paragraph, Young asserts:

> [S]ince the beginning of the century, the teaching and researching of composition have been guided by what Thomas Kuhn (1970) has called a "paradigm," a system of widely shared values, beliefs, and methods that determines the nature and conduct of the discipline. A paradigm determines, among other things, what is included in the discipline and what is excluded from it, what is taught and not taught, what problems are regarded as important and unimportant, and, by implication, what research is regarded as valuable in developing the discipline. It is what accounts "for the relative fullness of . . . [our] professional communication and the relative unanimity of . . . [our] professional judgments" (Kuhn, 1970, p. 182). For those working within a discipline, a paradigm is an eye to see with. (Young, "Paradigms" 29-30)

While acknowledging that "it is not difficult to find evidence for the contrary position that . . . no generally shared system of beliefs . . . has guided work in the discipline," Young goes on to argue that "a reasonable case can be made for the proposition that for several decades the members of the discipline have shared a remarkably stable system of beliefs, a system that Daniel Fogarty (1959) has called 'current-traditional rhetoric'" ("Paradigms" 30).

Young buttresses his application of Kuhn with additional commentary in footnotes that explicate Kuhn's work and strengthen Young's argument for the acceptance of current-traditional rhetoric as an underlying paradigm of composition studies despite the failure of some to accept the notion of current-traditional rhetoric. In his first

footnote, Young defines and uses "paradigm" in Kuhn's sense of "disciplinary matrix," noting that:

> Although "paradigm" has several meanings in Kuhn's work, I am treating it as synonymous with what he [Kuhn] calls a "disciplinary matrix"--"'disciplinary' because it refers to the common possession of the practitioners of a particular discipline; 'matrix' because it is composed of ordered elements of various sorts, each requiring further specification" ([Kuhn, Structure] p.182). (Young, "Paradigms" 29)

While he goes on to acknowledge that a question exists about the legitimacy of applying Kuhn to other disciplines since "Kuhn's work is an effort to account for deep and rapid changes in the sciences," Young asserts that "the similarities between scientific disciplines and other disciplines, including our own, are substantial enough to make its [Kuhn's work] use here at least tenable" ("Paradigms" 29).

Young uses Kuhnian theory to explore the concept of current-traditional rhetoric and examine the implications for composition research of accepting the paradigm's existence. He writes: "If we assume that such a [current-traditional] paradigm does exist and if we examine it through the lens of Kuhn's theory, then some recent developments in rhetoric take on a special meaning, one which has strong implications for the conduct of research" (Young, "Paradigms" 30).

Young goes on to describe the current-traditional paradigm in terms of its overt features and our tacit theoretical knowledge about composition. For Young, many of these features are the outgrowth of "the vitalist assumptions, inherited from the Romantics." He declares that:

> The overt features . . . are obvious enough: the emphasis on the composed product rather than on the composing process; the analysis of discourse into words, sentences, and paragraphs; the classification of discourse into description, narration, exposition, and argument; the strong concern with usage (syntax, spelling, punctuation) and with style (economy, clarity, emphasis); the preoccupation with the informal essay and the research paper; and so on. Vitalism, with its stress on the natural powers of the mind and the uniqueness of the creative act, leads to the repudiation of the possibility of teaching the composing process, hence the tendency of current-traditional rhetoric to become a critical study of the products of composing and the art of editing. Vitalist assumptions become most apparent when we consider what is excluded from the present discipline that had earlier been included, the most obvious and significant exclusion being the art of invention. (Young, "Paradigms" 31)

Noting that the "overt features of the paradigm have provided the content and organizational principles for hundreds of anthologies and composition texts for three generations," Young argues that the "striking similarity [of many texts] is . . . a symptom of a widely shared paradigm." Echoing Kuhn, Young asserts that textbooks "elaborate and perpetuate established paradigms; they are one of the principal vehicles for the conduct of a discipline in a stable state." Textbooks serve as "particularly valuable source[s] of information about the paradigm" ("Paradigms" 31). Young stresses the existence of the paradigm by further citing bibliographies of research such as the one in Braddock, Lloyd-Jones, and Schoer's Research in Written Composition. According to Young, such bibliographies, like many of the anthologies and texts, reflect the "distinctive features" of the current-traditional paradigm; they focus on research

devoted to problems of application, especially pedagogy, rather than a concern for theories that might challenge the accepted tradition.

The general thrust of Young's section on the current-traditional paradigm is to establish the paradigm's existence within the framework of Kuhnian theory. He suggests that the stability of the established paradigm is breaking down because of its exclusion of invention and its attention to product over process. Such instability ultimately leads to crisis.

Young devotes the next section of the chapter to explicating the conflicts that cast the continued viability of the current-traditional paradigm in doubt and suggest its instability. These conflicts center upon the failure of the paradigm "to provide effective instruction in . . . the 'prewriting stage' of the composing process and in the analytical and synthetic skills necessary for good thinking" (Young, "Paradigms" 33). Young attributes "the failure to develop effective means for cultivating the skills of invention. . . . to efforts to respond to the problem in terms of the current-traditional paradigm" ("Paradigms" 34). He concludes that attention to the paradigm has meant inattention to invention and its ranking as relatively unimportant; research into invention does not fit comfortably into the puzzles the current-traditional paradigm defines as important.

Young labels the tendency "to regard the problem as relatively unimportant" as not "surprising" ("Paradigms" 34); the paradigm acts as a filter, coloring the community's view of what problems are important. Citing Kuhn in a footnote, Young emphasizes the insulating nature of a paradigm; a paradigm can isolate a community "from those socially important problems that are not reducible to the puzzle form, because they cannot be stated in terms of the conceptual and instrumental tools the paradigm supplies" (Qtd in Young, "Paradigms" 34).

To further explicate the lack of priority attached to investigating invention, Young relies on Kenneth Burke.

> We all see the world "through" our beliefs, and that is the problem of most current-traditionalists. Training in a paradigm develops particular scholarly and pedagogical capacities, but it also develops particular incapacities. As Kenneth Burke (1954) remarked, "A way of seeing is also a way of not seeing" (p.49). (Young, "Paradigms" 34)

In his third section, Young expands on the notion of the discipline in a state of crisis. Paraphrasing Kuhn, Young maps the ways in which Kuhn believes that a discipline develops:

> One way a discipline develops, Kuhn (1970, pp. 23-34) says, is by gradual elaborations, clarifications, and applications of the paradigm. We can find such changes in the current-traditional paradigm if we review our professional literature, though the changes have been disturbingly slow. . . . Kuhn says, a discipline also develops . . . through a process set in motion by an awareness of serious problems in the established paradigm. The stages of the process go something like this. A paradigm acquires wide support by demonstrating its superior ability to solve problems generally acknowledged by those in the discipline to be acute and fundamental; once it is established, research is directed primarily toward its articulation and application. New problems arise, however, which those committed to the paradigm cannot solve adequately, and a crisis develops, accompanied by a sense of uncertainty and insecurity in the profession. The response to the crisis is typically the development of new theories which are

> able to provide more adequate solutions. A new paradigm emerges from the inquiries and controversies of the crisis state and with it another period of relative stability (Kuhn, 1970, pp. 66-76). (Young, "Paradigms" 35)

The explication of Kuhn's ideas forms a central part of Young's "rationale for a program of research" ("Paradigms" 35). Assuming the existence of the current-traditional paradigm, Young uses Kuhn to set up the idea of a discipline in a crisis state. According to Young, "if we see the problem discussed as creating a crisis in our discipline and, in so doing, stimulating proposals for the formal arts of invention, we can make . . . sense out of the recent and rapidly growing interest in the composing process and the numerous proposals for controlling it" (Young, "Paradigms" 35).

Young notes that the past fifteen years (circa 1963-1978) have seen a growing "shift in attention from composed product to the composing process . . . occurring at the same time as the reemergence of invention as a rhetorical discipline." For Young, "Invention requires a process view of rhetoric; and if the composing process is to be taught, rather than left to the student to be learned, arts associated with the various stages of the process are necessary" ("Paradigms" 35). Young suggests that these developments are important because they meet "a long-standing need unmet by the current-traditional paradigm. . . [and] are incompatible with some of the paradigm's basic features. They are challenges to the continuing viability of the paradigm" ("Paradigms" 36).

After reviewing four "recently proposed arts of invention" that he sees as incompatible with the current-traditional paradigm--classical invention, Burke's Dramatistic Method, Rohman's Prewriting Method, and Pike's Tagmemic Invention--Young shifts his argument toward specific areas of needed research addressing paradigm change. He states:

> I have been arguing that, if examined through the lens of Kuhn's theory, our discipline appears to be in a crisis state. . . . [A] crisis state calls for research quite different from that carried on during periods dominated by a single, stable paradigm. Research carried on under the influence of the current-traditional paradigm has been . . . directed toward elaborating and applying the paradigm. Research appropriate to the present situation, however, must be directed toward determining the adequacy of the present paradigm and the proposed alternatives. (Young, "Paradigms" 39)

Young suggests that "the existence of a persistent problem in the current-traditional paradigm does not in itself provide a basis for repudiating it" ("Paradigms" 39). Rejection of the current-traditional paradigm during a crisis will not occur "unless there is an acceptable alternative to take its place" (Young, "Paradigms" 39). Again, Young depends upon Kuhn's words, noting: "The decision to reject one paradigm . . . is always simultaneously the decision to accept another. . ." (Qtd in Young, "Paradigms" 39). Members of the community must necessariiy judge and decide between the existing paradigm and competing alternatives.

Using Kuhnian theory allows Young to set up his own agenda for needed research into theories of invention. First, he poses two questions, again reflecting Kuhn's influence, central to any evaluation of competing theories of invention:

> 1. Does it do what it claims to do? That is, does it provide an adequate account of the psychological processes it purports to explain? And does it increase our ability to carry out these processes more efficiently and effectively?

2. Does the theory provide a *more adequate* account of the processes and *more adequate* means of carrying them out than any of the alternatives? ("Paradigms" 40)

These questions reflect Kuhn's point that paradigms grow out of the ability of a theory to adequately address issues connected with a problem, shape research into that problem by drawing the boundaries of normal science, and promote incremental fine tuning of the theory by application.

In addition to research into theories of invention, Young also calls attention to the need for metarhetorical research--establishing "the criteria by which we determine the adequacy of any theory." As an example, he points out that "Our conception of the composing process . . . influences strongly our criteria for determining what is an adequate theory of invention" (Young, "Paradigms" 42). Young writes:

> If . . . we assume that the composing process begins after the identification of the thesis and ends with a finished discourse, as is often the case with current-traditional rhetoric, then invention . . . need only involve finding relevant things to say about the thesis. However, if the composing process begins with the perception of a social problem and ends with changes in an audience's beliefs and behaviors . . . if it is carried out within a rhetorical situation, then classical invention is required, or some other method with similar functions. (Young, "Paradigms" 42-43)

The need for metarhetorical research translates into a clarion call for greater investigation into the composing process.

Young then directs attention to the third and final part of his research agenda--the need for more historical research. In the same vein of Kuhn's investigations into the history and philosophy of the sciences, Young suggests that "studies of earlier theories can offer us contrasts to present theories, thus enabling us to perceive more clearly what present theories are not and, hence, what they are." Young claims that such comparative studies are "especially valuable during periods of crisis when belief systems must be scrutinized with uncommon care" He goes on to note Douglas Ehninger's contention that studies of past theories serve to "increase our awareness of the difficulties and dangers involved in creating a new paradigm" ("Paradigms" 45). Young writes: "A new paradigm redraws the boundaries of the discipline, adding to it and leaving out. The gains it brings come at a cost"; thus, conducting "historical studies help[s] us understand better the kind of disciplinary change we appear to be undergoing, for it has happened several times in the past" (Young, "Paradigms" 45).

To Young, a great deal of the value of historical studies rests in the historian's ability to view revolutions in paradigms in a light different from that of advocates of competing theories. He notes: "When seen through the historian's eyes, revolutions are more likely to appear as stages in the growth of a discipline. Such a perspective suggests the possibility of a metarhetoric in which all theories of rhetoric, past and present, participate" (Young, "Paradigms" 46). Further, such studies lead to a better understanding of the discipline as a whole and the paradigms that influence it.

Young concludes his chapter with a strident cry to conduct more research. He suggests that "no systematic decision-making procedure . . . can dictate the choice of one theory rather than another"; the profession must depend upon informed choice growing out of "informed debate." To debate and choose "requires that we be clear about our criteria for judgment, that we agree on the meaning of our terms, [and] that we have

evidence to support claims about the adequacy of one or another of the theories" (Young, "Paradigms" 47).

Inherent throughout all of Young's claims is the structure provided by using Kuhn's work to frame the debate about needed research. Kuhn's ideas about paradigms lend support to Young's contentions about current-traditional rhetoric, crisis and competition, and the state of the discipline. Young uses Kuhnian theory to promote his own agenda of research while attacking the elements of the current-traditional paradigm that he argues inadequately address the composing process.

In addition to using Kuhnian theory to promote a specific research agenda, Young's 1978 chapter in Points of Departure also escalates the debate about the state of composition studies as a discipline in Kuhnian terms. Whereas Gage in 1963 suggests that the greater discipline of English may be in a preparadigmatic state, Young offers evidence of an established paradigm under attack in a smaller, more defined discipline--a discipline at the crisis stage of development. In his initial footnote, Young cites Gage's 1963 use of Kuhn, calling Gage's application of Kuhn ""valuable" while noting that Gage considers composition as a subdiscipline of some larger, wider whole. When Gage "argues that the discipline as a whole is in a 'preparadigmatic' stage, a judgment that seems . . . reasonable [to Young]," Young suggests that Gage fails to account for composition studies as a "more orderly, coherent, and directed enterprise" ("Paradigms," 29).

Young carries his criticism of Gage further by arguing that composition studies, "if examined through the lens of Kuhn's theory . . . appears to be in a crisis state"--and not in the "preparadigmatic" stage that Gage claims for the larger discipline of English. The criticism is necessary because it bolsters Young's own argument for the existence of the current-traditional paradigm, asserts that the paradigm is under attack, and suggests the volatile nature of research needed to explore the established paradigm and its competing theories.

Young's reliance on Kuhn and Gage suggests an intimate knowledge of Kuhn's work and some thoughtful application of that work to the enterprise of composition research. While Young retains the label of rhetoric as the overall umbrella under which he works, he clearly discusses issues central to composition studies as a distinct discipline. By calling Gage's work "valuable" and debating with him simultaneously, Young enters a discussion about the stage of development of the discipline, a discipline that Young perhaps already accepts as mature and certainly undergoing a period of crisis when looked at using Kuhnian theory. Young's view counters those of the other contributors to Points of Departure who insist that composition studies remains preparadigmatic or that a paradigm is only now emerging. The state of composition studies in terms of the presence or absence of an informing theory or paradigm continues at the center of the debate about applying Kuhn's ideas to describe what is happening to the field.

Stephen North likewise turns to Kuhn's theory for help to explain the discipline's response to developments in composition pedagogy. In a 1978 article in CCC, "Composition Now: Standing on One's Head," North harnesses Kuhn to argue for the development of a student-centered curriculum. He uses the metaphor of "standing on one's head" to describe the state of composition inquiry, suggesting that:

> [L]earning, high education, has to be [like the metaphor] in an age and in a field in which "interdisciplinary" takes on a tone of ironic understatement. For it isn't only that one has to be familiar with a variety of disciplines (psychology, linguistics, rhetoric, calligraphy, biology, stylistics) but also that within each of those disciplines, several approaches need to be mastered and held together. (North, "Standing" 177)

North continues exploring the metaphor by describing the process by which a theory becomes predominant within a field. His description follows near to Kuhn's version of paradigm conflict and revolution in the sciences. North states:

> I was saying why education now must mean standing on one's head. It used to be . . . that you could put two or three seeming truths into the arena, they'd battle it out, and a winner would emerge by which you could confidently carry on everyday operations. Now, I know it's never been all that neat . . . but the pattern is still with us. Someone develops a theory that dictates a practice, and that theory/practice approach is tested against all other theory/practice approaches, usually one at a time, and the conflict is good. The supremacy of one or another of these approaches (or some synthesis thereof) is demonstrated by its adoption in practice. (North, "Standing" 178)

North reinforces the comparison to conflict over theories and practices in the scientific disciplines by comparing himself to Kuhn's scientist. He suggests the allegiance that many practitioners in the discipline profess to whichever theory dominates the teaching of writing.

> In the manner of Kuhn's practical scientist, the folks who have to go out and perform in the real world every day behave as though the reigning theory/approach were the only, the right, one.
>
> Of course, I'm not talking about scientists here, but about teachers, especially teachers of writing. (North, "Standing" 178)

North goes on to chart the current pattern he argues that practice in the teaching of writing follows:

> Some theory/approach is developed--often out of frustration with the failure of the reigning grammar/rhetoric system--and is flung into the arena with that old gladiator/dowager via the journals, makes a small temporary dent, then is filed away as theoretically nice but useless in the classroom. There is usually some resistance . . . but it is most often isolated and on paper. (North, "Standing" 178)

North's description seems to be a thinly veiled application of Kuhn's ideas. In Kuhn's view, the dominant paradigm continues until conflict reaches a crisis and the old pattern is no longer able to adequately answer the questions raised.

At this point, North shifts gears and moves on to the main thrust of his argument. He suggests that for "a long time, the pedagogy that was the product of this pattern really could hold up in the teaching of college writing," noting that to the average or better student attending college, "education has always meant conforming to the teacher's standards." North claims that this usually required "a compromise of about 70-30%, with the student giving the most ground" (North, "Standing" 178). He asserts: "A competent student will learn certain skills almost in spite of the teacher present, so long as he is able to shift to seeing the world the way the teacher does or at least to behaving as though he does. And this has been true in writing as in anything else. . . . The student unable to conform is the loser. . . " (North, "Standing" 178).

As colleges and universities have undergone a corresponding shift towards larger numbers of non-traditional students, North contends:

> Probably three-fourths of our population will be going to college at one time or another in their lives, and among these there will be many who never have learned the art of conforming, of compromising. I don't know what effect this will have on the pedagogy of the other disciplines; in writing, it is simply going to force the development of the student- centered curriculum that has been talked about for so long. (North, "Standing" 178)

North argues essentially for a reversal of the older pattern. According to North, the shift to a student-centered curriculum means that the "70-30% compromise gets turned around, and the teacher's role is defined by the student" (North, "Standing" 178).

To meet North's goal, composition teachers will have "to shift to seeing the world the way the student sees it, to understand writing as he understands it before [teachers] can offer much instruction"--ultimately, to stand on their heads and open up (North, "Standing" 178-179). For a composition teacher, North's call means:

> [You must be] prepared to stand on your head a half-dozen, a dozen times a day; *not* [sic] turning your back on any theory/approach and damning the confusion as chaotic and anarchical, but getting lost in its fertility; getting inside the student's head . . . assuming his potential, then offering him as many ways as you know, as he needs, to get him to write. (North, "Standing" 179-180)

In one sense, North's call can be interpreted as contributing to a crisis state in the discipline. The increasing diversity of students brings an increasing diversity in writing problems. As a result, "the traditional ways of looking at student writers will become, are becoming, inadequate" (North, "Standing" 180).

North concludes that the "main challenge for writing teachers is a sort of a technological one: to take the great mass of theory and shape it, refine it pedagogically to help one student write better" (North, "Standing" 180). In essence, he argues against the traditional pattern, or paradigm as Kuhn would probably refer to it. North hides Kuhn in the background, using his work as more of a metaphor for what is happening in composition. His argument could be made without Kuhn, and, indeed, North never once mentions Kuhn's concept of paradigm or paradigm shift specifically. Instead, he relies on pathos, personal anecdote, and a rough approximation of Kuhn's ideas concerning the development and shifting of paradigms within a discipline. If, as North states, "the conflict is good" when one theory/practice approach battles the others for supremacy, the crux of North's main argument seems to rest with fueling conflict within composition studies.

While North turns to Kuhn directly, Donald C. Stewart places his confidence in Young's interpretation of Kuhn. Stewart uses the unpublished 1975 paper version of Young's 1978 chapter in Research on Composing: Points of Departure to support a February 1978 review of two contemporary composition textbooks and to frame the analysis of composition texts in his May 1978 article in CCC, "Composition Textbooks and the Assault on Tradition." Both Stewart's review and his analysis depend upon Young's application of Kuhn and conception of a current-traditional paradigm in the teaching of writing.

In his review of John and Francine Hardaway's Writing Through Reading and Jo R. McCuen and Anthony C. Winkler's From Idea to Essay: A Rhetoric, Reader, and Handbook in the February 1978 issue of CCC, Stewart begins with a summary of Young's 1975 paper. According to Stewart: "Richard Young . . . says that current composition theory and practice are dominated by a paradigm which Daniel Fogarty has called 'current-traditional rhetoric.' Young uses "paradigm" here as "disciplinary matrix," a term he

derives from Thomas Kuhn's Structure of Scientific Revolutions" (Stewart, "Writing" 65). Stewart borrows Young's ideas to frame the review. In it, he focuses on Young's arguments about the features of the current-traditional paradigm, the assumptions behind that paradigm, and the inability of the paradigm to address problems in the discipline, stating:

> Now there is nothing wrong with a paradigm dominating and giving stability to a discipline, so long as the paradigm articulates and generates solutions to the problems of the discipline. The question, then, is whether or not the current-traditional paradigm in rhetoric and composition still serves that function. I think it does not. It has been under assault from several directions for the last fifteen years, primarily for its failure to deal adequately with the composing process. (Stewart, "Writing" 65)

In a short, four column book review, it is difficult for Stewart to present more than a brief taste of Young's interpretation of Kuhnian theory, a theory Stewart's audience exhibits little awareness of in 1978 as analysis of CCC reflects. But Stewart uses more than half of his review to present the bare skeleton of Young's interpretation and to mention a number of other works "outside the bounds of the current-traditional paradigm" ("Writing" 65). He then proceeds to base his review of Writing Through Reading and From Idea to Essay on how well these books fit or disagree with that paradigm. Stewart notes:

> In reviewing the texts cited at the beginning of this review, I have, quite obviously, asked myself these questions: (1) to what extent does each exemplify or depart from the current-traditional composition paradigm; (2) despite my

"Christensen's fundamental purpose: developing dynamic structures that would reveal the mind thinking, not having already arrived at its conclusions and delivering its information cold." Stewart credits Christensen as "one of the first to shift the focus from product to process and to turn our attention more and more closely to the mystery of the composing process itself" (Stewart, "Writing" 66).

Stewart concludes his review of the two texts by stating: "Regrettably, too few texts coming out, even in 1977, have caught up with the research into the composing process. The lag actually seems to be increasing. Why? Probably because too few teachers of composition have caught up with it themselves" (Stewart, "Writing" 66).

Examining the two textbooks in light of the current-traditional paradigm forces Stewart to explore and define that paradigm. He does so by offering a summary of Young's paper marshalling Kuhn's definition of paradigm in the sense of "disciplinary matrix." His charges, delivered in 1978, indicate the growing credence attached to the idea of the current-traditional paradigm or current-traditional rhetoric. At the same time, he deplores the lag between research into the composing process and the application of that research, attributing it to the lack of recognition teachers of composition give to research.

Stewart goes even further in his application of Young's borrowing of Kuhn to argue about the efficacy of the current-traditional paradigm; Stewart uses Young's version of Kuhn and the current-traditional paradigm to frame an analysis of 34 handbooks, rhetorics, and readers. Stewart reports his findings in "Composition Textbooks and the Assault on Tradition," appearing in the May 1978 issue of CCC.

As a means of establishing the context of his report, Stewart acknowledges the strident, sometimes raucous debate in the popular press about "Why Johnny Can't Write." He declares:

> reservations about the adequacy of the current-traditional paradigm, do those texts which exemplify it have redeeming features which deserve mention; (3) to what extent do those which depart from the current-traditional paradigm reveal awareness of composition as a process and of the varied assaults upon the current-traditional paradigm's failure to deal adequately with invention? (Stewart, "Writing" 66)

Stewart attacks the Hardaways's' From Idea to Essay as an example of a text "firmly rooted in the current-traditional paradigm" ("Writing" 66). He accuses the authors of grossly oversimplifying "the complexities of modern composition pedagogy" by asserting in their preface that: "the teaching of writing has "traditionally . . . swung between two polar approaches," one of which "fearlessly prescribes to the student the rhetorical techniques and strategies used by professional writers," the other of which attempts "to stimulate the student's interest" (Stewart, "Writing" 66).

Stewart goes on to assess the part of the Hardaway text dealing with grammar; he pans it as "astonishingly innocent of the influence of modern linguistics," noting: "I cannot believe that knowledgeable linguists would find much to cheer them in this part of the book." He concludes that From Idea to Essay "is current-traditional in every aspect, neither better nor worse than hundreds of books like it which have appeared in the past twenty or thirty years" (Stewart, "Writing" 66).

While he also finds the elements of the current-traditional paradigm quite evident in McCuen and Winkler's Writing Through Reading, Stewart points to something he calls significant--their "awareness of significant research and theory in the composing process over the past fifteen years" expressed in their "practical application of principles of sentence structure and addition as articulated by Francis Christensen." Stewart goes on to express his regret that the authors do not aggressively pursue

> During the past two or three years, article after article has appeared in the popular press advocating a return to the basics in the teaching of English composition. For example, Newsweek's December, 1975, issue decorated its cover with "Why Johnny Can't Write" and then proceeded to explain *why* [sic], in terms which were, for those of us who try to keep abreast of work in composition theory and practice, simplistic and outdated. How ironic that the writer of the Newsweek article was reaching an audience of millions, while Richard Young of Michigan, one of the country's truly knowledgeable people in composition theory and practice, was reaching only a few hundred with a paper delivered at the 1975 Buffalo Conference on the Composing Process. (Stewart, "Composition" 171)

While paying tribute to Young's work, Stewart points out Young's thesis that a current-traditional paradigm dominates theory and practice in composition. Stewart cites Young's use of paradigm "to mean a 'disciplinary matrix,' a term he derives from Thomas Kuhn's Structure of Scientific Revolutions" ("Composition" 171). He quotes Young's definition of "paradigm," repeating Young's assertion that 'For those working within a discipline, a paradigm is an eye to see with' (Qtd in Stewart, "Composition" 171).

Extensive quotation from Young's unpublished paper in the opening paragraphs of the report allows Stewart to establish a foundation for exploring the presence of the paradigm in the composition and rhetoric texts he examines. Within those quotations and Stewart's own words lie an acknowledgment of Kuhn's ideas. Stewart simply repeats his own contention from his book review in the preceding issue of CCC:

> Now there is nothing wrong with a paradigm dominating and giving stability to a discipline, so long as the paradigm articulates and generates solutions to the

> problems of the discipline. The question, then, is whether or not the current-traditional paradigm in rhetoric and composition still serves that function. I think it does not. It has been under assault from several directions for the last fifteen years, primarily for its failure to deal adequately with the composing process. (Stewart, "Composition" 172)

Inherent in Stewart's repetition is acceptance of Kuhnian theory, at least as interpreted by Young; for Stewart, Kuhn's ideas about paradigms accurately reflect conditions within composition studies. Further, Stewart's work suggests acceptance of the existence of the current-traditional paradigm, again as articulated by Young. He attacks the viability of the paradigm to answer questions about invention and the composing process.

As part of his report, Stewart reviews the textbooks for acknowledgment of the four theoretical stances on invention that Young mentions in his unpublished paper. Stewart writes:

> My own judgment is that of the four, the topics of classical rhetoric and Burke's pentad [topics one and three as mentioned by Young] are the heuristic systems most familiar to those textbook writers and teachers who are concerned with providing students with help in the invention process. But I find virtually no evidence that the authors of most textbooks which now include sections on pre-writing are at all familiar with Rohman and Wiecke's report . . . The Construction and Application of Models of Concept Formation [topic two in Young's article]. There is no question, however, that these men were addressing the inadequacies which Young identifies in the current-traditional paradigm. (Stewart, "Composition" 172)

Stewart goes on to remark about the fourth topic mentioned by Young:

> Equally difficult to find is any evidence that Pike's tagmemic theory has been understood or adopted. The only text which presents it fully is Rhetoric: Discovery and Change . . . by Young, Becker, and Pike. As far as I am aware, the book has not been widely adopted, and its theories have scarcely been incorporated into even a small minority of texts on the market. (Stewart, "Composition" 172)

Stewart moves from the four theories of invention Young mentions to other "assaults on the failure of the current-traditional paradigm to deal with the composing process" (Stewart, "Composition" 172). He refers Robert Zoellner's 1969 introduction of behaviorism into the writing process, calling Zoellner's approach "still so original and so fundamental and so far beyond the boundaries of the current-traditional paradigm that most English teachers regard it with intense hostility" (Stewart, "Composition" 173). Writing about Winston Weathers' 1976 article in Freshman English News, "Grammars of Style: New Options in Composition," Stewart concludes that Weathers "offers an alternate grammar of style. . . . [that] is not primarily rational, logical, or ordered," features attributable to the current-traditional paradigm ("Composition" 174). Weathers calls for coming "to composition with options, openmindedness, adaptability" (Qtd in Stewart, "Composition" 174).

Following his analysis of Zoellner and Weathers, Stewart asks the key question investigated in his research: "To what extent have these assaults upon prominent features of the current-traditional paradigm in rhetoric and composition been reflected in widely used composition textbooks?" From a compiled list of 52 texts with sales of

over 100,000 copies, Stewart reports that he reviewed the latest edition of 34 handbooks, rhetorics, and readers published between 1970 and 1977. He notes that all the texts reviewed had "been revised recently enough to be touched by the work . . . mentioned, except for the Weathers' essay, and the 1977 titles . . . eleven, could even have responded to that" (Stewart, "Composition" 174). After carefully searching the "appropriate sections of each book for indications that their authors were at least aware of the new approaches to the composing process, writing behavior, and stylistic options" which Stewart addresses in the opening half of his article, Stewart summarizes:

> Regrettably, only seven of the 34 contained any appreciable awareness of the work of people like Corbett, Rohman, Burke, Pike, Zoellner, or Weathers. The other 27, and some are the products of people with enormous reputations as literary scholars, were strictly current-traditional in their discussions of invention, arrangement, and style. (Stewart, "Composition" 174)

According to Stewart, textbook publishers and college English departments share the blame for inhibiting the spread of composition research findings. The publishers, while "they serve English teachers and their students . . . do not exist to lose money, however. They have to make it, and in our market system, they make it by selling the customer what he/she wants." Speaking bluntly, Stewart argues: "Until very recently, composition research and teaching have not been considered intellectually respectable by those in power in college English departments in this country. For many, they still aren't" (Stewart, "Composition" 175).

Part of Stewart's argument about textbooks can be traced back to Kuhn's contention that texts reflect the dominant paradigm of a scientific field and are slow to change. Similarly, using Young's interpretation of Kuhn, Stewart's assertion about "those in

power" reflects Kuhn's contentions about supporters of the dominant paradigm; the paradigm determines inclusion and exclusion in the field, dictates pedagogical practices, establishes the relative importance of research problems, and evaluates the value of research. If "those in power" fail to attach any importance to composition research, they are unlikely to see the need for alternatives or changes to the dominant paradigm.

Stewart closes on a crusading note against the status quo represented by the current-traditional paradigm. He asserts that,

> English teachers are going to continue to perpetuate the current-traditional paradigm, no matter how outdated it becomes, with no more success than they have had, and they will receive a continuing barrage of complaints about their inability to teach all the Johnnies and Janies to write well. But you will not find me sustaining the status quo. I'm crusading for alternatives to the current-traditional paradigm and will continue to do so until either my vocal chords, or my pen, or both, give out. (Stewart, "Composition" 176)

Without Young's interpretation of Kuhn, Stewart's attack on the current-traditional paradigm and his argument about textbooks flounders. He depends upon Young's interpretation to build a case for the existence of a current-traditional paradigm dominating composition studies. Accepting the existence of that paradigm, he can investigate textbooks to see how well they meet its features or take into account new directions in research that suggest the paradigm's inadequacy. Kuhn's theory of paradigms, as interpreted by Young, thus forms the cornerstone of Stewart's attack.

Another borrowing of Kuhn's concept of paradigms appears in 1978, Steven Mailloux's "Literary Criticism and Composition Theory." Mailloux briefly refers to Kuhn, then uses the concept of paradigms to frame a description of emerging "areas of

shared theory and practice," areas Mailloux sees uniting literary criticism and composition studies (267). Kuhnian theory represents a convenient method to categorize the emerging paradigms shared by literary criticism and composition theory. After noting the disparity and debate over the distinction between rhetoric and poetics, Mailloux states:

> In fact, recent trends in literary criticism suggest that a rapprochement may be taking place between literary and composition theory; shared paradigms are now emerging. What I would like to do in this essay is to outline these areas of shared theory and practice.
>
> Observation and interpretation always proceed within accepted paradigms, whether in medical research, composition study, or literary criticism. (Mailloux 267)

Mailloux accepts outright much of Kuhn's ideas concerning the effect of established or "accepted paradigms" on the promotion and conduct of normal science. By equating medical research, composition study, and literary theory, he extends the application of Kuhn from the hard sciences to the softer subjects of rhetoric and poetics without presenting any evidence; he apparently expects his readers to accept Kuhnian theory just as he does. In a footnote, he refers specifically to pages 10 and 187-191 in the second edition of Kuhn's Structure, but leaves it up to the reader to seek further clarification from Kuhn. In the first reference, Kuhn covers the route to normal science, describing in more detail what Mailloux summarizes in one sentence. The second reference in the footnote, pages 187-191, addresses paradigms as shared examples.

Mailloux describes a number of paradigms in literary criticism in his effort to tie rhetoric and poetics together. He begins with New Critical formalism, calling it the

dominant paradigm in American literary criticism from the late thirties to the early sixties. The dominant paradigm of New Criticism held "little of interest to rhetoricians. It not only ignored audience (a central concern of rhetoric) but also actively discouraged talk about readers through its condemnation of the 'affective fallacy'" (267). In other words, the dominant paradigm in literary criticism had nothing to offer composition studies. But challengers to the dominant paradigm have begun to emerge that, according to Mailloux, do have something to offer composition.

Mailloux switches from New Criticism, a "model of art as aesthetic product," to challengers which focuses on "a model of art as communicative process" and allows practice of both the intentional and affective fallacies (267). He suggests that the recent approaches of textual-biographical and reader-response criticism begin to reflect the "overlap of rhetoric and poetics most clearly" (Mailloux 267). Such approaches "view art as process not product" (Mailloux 268).

The textual-biographical perspective changes from a focus on product to a focus on acquiring a "total knowledge of the author's composing process (including extant forms of the text) and the relevant biographical forms that affect that process." The rhetorical approach of reader-response criticism changes the focus to the response of the audience to the text. Mailloux notes with special emphasis: *"Whereas the textual-biographical critics view literature as a series of acts by the authors, reader-response critics view it as a series of acts by the reader* " (268). This change in view away from product is a key point in Mailloux's argument.

Given the acceptance of paradigm theory and his own observations about the state of literary criticism, Mailloux can focus on issues of rapprochement between criticism and composition. He writes: "Whereas the old New Critical paradigm was hostile to any synthesis of rhetoric and poetics, the new paradigm which views literature as a

temporal act of communication provides much encouragement for a rapprochement between literary criticism and composition theory" (Mailloux 268-269).

Mailloux moves on to an analysis, admittedly oversimplified, "of the composing process based on the traditional rhetorical categories--invention, arrangement, and style." He acknowledges that he is simply providing "a clear framework in which to examine some additional parallels between literary criticism and composition," parallels he sees present primarily in reader-response approaches. Making the assertion that the "goal of the composition teacher is to give the student writer alternative choices at every stage of the composing process," Mailloux provides examples of the parallels. For invention, he cites the teaching of brainstorming and heuristic techniques and notes that "Literary critics often use heuristics in their analysis of literary texts"; Mailloux turns specifically to the work of Stanley Fish and Kenneth Burke as specific exemplars. For arrangement or form, Mailloux insists that "the work of Fish and Burke encourages the composition teacher to place a strong emphasis on the structure of the reader's response," reflecting a growing concern for the effect of audience on the composing process (269).

When Mailloux turns to the third area of his simplified version of the composing process, style, he argues that literary and composition theory interact once more. Mailloux uses W. Ross Winterowd's division of stylistics into pedagogical and aesthetic categories, noting that: "'*Pedagogical stylistics* . . . deals with teaching students to develop style'; *aesthetic stylistics* refers to the study of style within literature." The former becomes "a matter of providing students with syntactic alternatives," while the latter "becomes a method of analyzing a text in terms of alternative choices among available structures" that can provide "added perspective in using pedagogical stylistics" (Mailloux 270-271).

Returning to the issue of paradigms in his conclusion, Mailloux reinforces his notion of the connection between literary criticism and composition theory. He boldly concludes that: "In terms of their controlling paradigms and in light of shared models for invention, arrangement and style, rhetoric and poetics are becoming more closely related in current theory and practice." For Mailloux, this translates into justification for considering literary criticism and composition theory as "parts of one homogeneous discipline of English." Further, he insists that "literature specialists have the *potential* to be the best qualified teachers of composition," although he admits that "not just any English professor can teach writing, even if he *has* accepted the model of literature as a communicative act." Mailloux acknowledges the existence of "a growing empirical and rhetorical body of knowledge that all serious teachers of composition must master." Ultimately, he suggests that "A synthesis of rhetoric and poetics will go a long way toward curing the English Department's split personality" (Mailloux 271).

Mailloux's conclusion strikes at the heart of one of the issues constantly appearing in the professional journals in composition studies, especially throughout the seventies--the frequently heard complaint that most of those teaching composition were trained to teach literature, not writing. While he acknowledges the increasing specialization in composition, he asserts the interconnection between literary criticism and composition and argues for a synthesis of rhetoric and poetics as a partial cure for the distancing occurring between literary and composition theory.

Mailloux's use of Kuhn represents a departure from the usual application to promote changes in the direction of research in composition studies or to define or attack the current-traditional paradigm in composition studies and alternatives to it up to 1978. He accepts the existence of paradigms, then sets about briefly describing the paradigms in literary criticism before focusing on the overlap between poetics and rhetoric. Though joining others in his use of Kuhnian theory as a framework for argument,

Mailloux charts a unique direction by setting out to establish connections, "shared exemplars," between what he sees as diverging subcommunities under the larger umbrella of English. By demonstrating shared theory and paradigms, he hopes to heal the growing split.

Patricia Bizzell has little interest in the split between literary criticism and composition. Unlike the majority of articles in the March 1979 issue of College English, which address literary studies and criticism under the legend of "Paradigms of the Discipline," Patricia Bizzell's "Thomas Kuhn, Scientism, and English Studies" focuses on the rhetorical implications of applying Kuhn to the broad discipline of English studies, a field seeming to incorporate rhetoric and composition studies under its rubric. Bizzell suggests that Kuhn's concepts have "increasing interest for English studies because new demands on our composition courses, along with new developments in literary theory, have contributed to a hot debate over the premises of our discipline" ("Scientism" 764). In interpreting and applying Kuhn as she does, Bizzell attacks the views stated by Maxine Hairston in an address to the 1978 convention of CCCC. Ultimately, Bizzell argues for a picture of English or composition studies as dependent upon the domain of rhetoric.

Bizzell begins her argument for employing Kuhn by defining his use of *paradigm* to mean "a comprehensive theoretical model that governs both the view of reality accepted by an intellectual community and the practice of that community's discipline." She immediately counterattacks Hairston's position that deliberation over the constitution of English or composition studies as a discipline represents "the sort of profound revolution in accepted thinking that accompanies a new paradigm, rather than as an unrelated group of local disagreements over critical tastes and pedagogical methods" (Bizzell, "Scientism" 764). Bizzell complains about Hairston's views:
"Hairston wants to dignify our debate as a paradigm debate because she fears, with good reason, that its beginnings in literary theory and composition pedagogy have allowed too

many practitioners in English studies to regard it [the debate over the premises of the discipline] as tangential to their main business" (Bizzell, "Scientism" 764).

According to Bizzell, Hairston uses Kuhn's "paradigm" in a comprehensive sense to buttress "her own candidate for our new paradigm by an appeal to empirical evidence." Such an appeal is immediately refuted by Kuhn's own claim that "a paradigm determines the identification and interpretation of 'empirical evidence' in a given discipline" (Bizzell, "Scientism" 764). Bizzell notes:

> "Empirical evidence" makes sense only when considered in light of a paradigm; therefore, empirical evidence cannot be imported to establish a paradigm above debate. Hairston and others (Janet Emig and E. D. Hirsch, for example) have sought, however, to establish a paradigm based on such evidence, under the misapprehension that only a paradigm so established can raise English studies to the status of a truly rigorous discipline. (Bizzell, "Scientism" 764)

In contesting Hairston's use of paradigm, Bizzell asserts that Kuhn takes a position contrary to Hairston's. Bizzell declares:

> Kuhn argues that a paradigm is established, even in the natural sciences, not because of compelling empirical evidence, but because of a rhetorical process that delimits the shared language of the intellectual community governed by the paradigm. Indeed, he suggests that he has derived his concept of "paradigm" for the sciences from a study of the theoretical models that govern the humanistic disciplines. In following Kuhn, we should not be misled into a scientistic faith in empirical evidence as compelling. Instead, the special province of our new

> paradigm may be indicated in his analysis of the ways in which any paradigm is constituted by language. (Bizzell, "Scientism" 764).

Bizzell's warning against the dangers of "scientism" is accompanied by a claim that places any new paradigm securely within the pale of rhetoric.

Before going further, Bizzell acknowledges some of the confusion Kuhn creates in his own application of paradigm to describe the state of a discipline. Citing the second edition of Structure, Bizzell suggests that Kuhn creates "a misleading impression that English studies might be more aptly described as being in a 'pre-paradigm state,' in which practice has not yet become sufficiently rigorous to allow" one model to dominate a discipline ("Scientism" 765). Bizzell quotes Kuhn's description of the discourse of physical optics before Newton's unifying paradigm, focusing on the lack of a "common body of belief," the need of each writer "to build his field anew from its foundations," and each writer's freedom of choice "of supporting observation and experiment." Kuhn explains that the dialogue of resulting texts "was often directed as much to members of other schools as it was to nature." The pattern Kuhn describes "is not unfamiliar in a number of creative fields today, nor is it incompatible with significant discovery and invention. It is not, however, the pattern of development that physical optics acquired after Newton" (Qtd in Bizzell, "Scientism" 765).

Bizzell argues that Kuhn's term, "creative fields," includes English studies while his phrase, "discovery and invention," refers to "the rhetorical nature of the development of knowledge in any discipline." Bizzell admits that in the passage she quotes, "Kuhn seems to be contrasting the internal debates within 'creative fields,' to which he condescends, with the presumed disposition of natural sciences to examine nature directly and discover its truth." However, she asserts that "the main thrust of Kuhn's entire argument counters this view" ("Scientism" 765). As she interprets Kuhn:

> His [Kuhn's] historiography of science has been controversial in his own field precisely because he questions its traditional view of science as a discipline that progresses ever closer to accurate description and prediction of the reality of phenomena. . . . Kuhn concludes that there are no "scientific discoveries," if by that we mean isolable events performed by geniuses. Instead, the change in perspective traditionally labeled a discovery by historians of science typically comes about over a long period of time, and as a result of community effort. Furthermore, there is no "progress" in science, if by that we mean an ever-closer approach to truth about the phenomena of reality, because science does not deal in "truth." but rather in the cataloguing of sense perceptions and generalizations about them. (Bizzell, "Scientism" 765)

According to Bizzell, Kuhn "argues that a paradigm enforces a particular ideology, or way of seeing the world." She avers that Kuhn is "acutely alive to the limitations imposed by a paradigm, limitations that may not only restrict free inquiry, but vitiate the social usefulness of the discipline" (Bizzell, "Scientism" 766). She again quotes from Kuhn's second edition, calling attention to Kuhn's claim that "one of the things a scientific community acquires with a paradigm is a criterion for choosing problems that, while the paradigm is granted, can be assumed to have solutions" (Qtd in Bizzell, "Scientism" 766).

Bizzell turns to Kuhn's "Postscript--1969" for further evidence of her interpretation. She notes that Kuhn claims to have adapted his ideas about paradigm-driven science "from the very humanistic disciplines that are attempting to dignify themselves as paradigm-governed communities under his [Kuhn's] aegis." She recounts that Kuhn is puzzled by the effort of such communities "to achieve paradigm-governed

status" (Bizzell, "Scientism" 766). Kuhn remarks in his "Postscript" that his originality in portraying "scientific development as a succession of tradition-bound periods punctuated by non-cumulative breaks" rests in just that--applying the description to science; the humanistic disciplines, such as literature, music, arts, and political development "have long described their subjects in the same way" (Qtd in Bizzell, "Scientism" 766). Bizzell applies Kuhn's words to rhetoric and composition, concluding that "historians of and within English studies and other humanistic disciplines are already guided by a paradigm which Kuhn finds superior to the dominant one in the history of science" ("Scientism" 766).

In making her argument, Bizzell turns attention to the status of English studies as a discipline and suggests that it is undergoing a paradigm shift, "one of those breaks from a tradition-bound period Kuhn sees when he looks at the histories of many intellectual activities" ("Scientism" 766-67), rather than existing in a pre-paradigmatic state. She calls for a closer examination of Kuhn as we become "constructively self-conscious about our paradigms" and work "to hasten the emergence of a new one [paradigm] to meet our current problems more effectively" (Bizzell, "Scientism" 767). For Bizzell, such an investigation must include a greater focus on Kuhn's concept of "community."

Bizzell declares that Kuhn's concept of "'community'. . .may ultimately be more useful to English studies than his concept of 'paradigm.'" She notes, "Kuhn emphasizes in his 'Postscript' that any paradigm, or way of seeing and acting on reality, must be understood as the joint product of a community, whether it be a community bound together for study or for survival." Writing about the paradigm, Kuhn concludes: "it has been transmitted through education; it has, by trial, been found more effective than its historical competitors in a groups' current environment; and, finally, it is subject to change both through further education and through discovery of misfits with the environment" (Qtd in Bizzell, "Scientism" 767). Bizzell notes that the "paradigm is

so deeply implanted in the members of the community that it achieves the status of tacit knowledge, invisible so long as its effectiveness is not challenged" ("Scientism" 767).

In suggesting the inseparable link between Kuhn's sense of "community" and his notion of "paradigm," Bizzell reports that Kuhn "uncovers the crucial operation of 'shared language' in binding the community together under the paradigm." Bizzell argues that Kuhn "sees that the language of the paradigm is the substance of the paradigm, so that the sudden failure of the language to unite the community often initiates a paradigm shift." She goes on to quote Kuhn's explanation of the process of abandoning the old paradigm in favor of the new one as evidence of the importance Kuhn places upon shared language, noting that 'a particular set of shared values interacts with the particular experiences shared by a community of specialists to ensure that most members of the group will ultimately find one set of arguments rather than another decisive' (Qtd in Bizzell, "Scientism" 767). Condensing four pages of Kuhn's second edition of Structure to a single brief paragraph (quoted in part here), Bizzell illustrates Kuhn's acknowledgment of the power of shared language:

> Briefly put, what the participants in a communications break-down can do is recognize each other as members of different language communities and then become translators. . . . [but] To translate a theory or world view into one's own language is not making it one's own. For that one must go native, discover that one is thinking in and working in, not simply translating out of, a language that was previously foreign. (Qtd in Bizzell, "Scientism" 767)

Bizzell's interpretation of Kuhn and the connection he makes between the concepts of community and paradigm allows her to suggest two applications of Kuhn's theories to English studies: one, that the "current unsettled condition" of English studies can be

understood "as a state of paradigm shift," and two, that the special province of English studies once the paradigm shift is complete may rest in "the study of language as the product of a community, reflecting the community's shared values, its historical situation, its cultural traditions, and so on" ("Scientism" 768). Bizzell's applications depend upon the acceptance of "Kuhn's account of the relationship between a community and its paradigm" ("Scientism" 767).

Bizzell's first application requires an awareness of "the traditions of the community of English studies, the needs of the larger community that includes our students, and the politics and culture of the even larger historical community in which we all live." Such an awareness is important in understanding "the means used by the proponents of the current competitors for paradigm status" and "in shaping the outcome of our debate, as any empirical evidence adduced and interpreted by competing schools of thought." Bizzell insists that "Kuhn's analysis provides a powerful insight into the ways ideas shape our lives as our lives shape our ideas" ("Scientism" 767-68).

Addressing her second application, Bizzell relates that "Kuhn's analysis of the importance of language to the relationship between a community and its paradigm may suggest the intellectual work . . . of English studies." She takes a moment to indicate some of the inadequacies of the current paradigm under assault, arguing that "the current dominant paradigm seems founded in a conception of language as a system of references to a knowable reality external to it." Many practitioners of academic discourse are "unaware of the entire constellation of premises by which under its current paradigm the English studies community understands the operations of language" (Bizzell, "Scientism" 768). Bizzell calls this lack of awareness problematic:

> Faced with the new problem of explaining what had been tacit knowledge, many in English studies are now attempting to understand how we ourselves gained this

> knowledge. Many of those working on the new paradigm are turning to the study of language as the product of a community. . . . In other words, we are turning in the direction Kuhn indicates, toward the study of language as a social product and embodiment of ideology. As we uncover the ways we learned to be self-conscious about language, we will be better able to pass this knowledge along to our uninitiated students. But more important, we may develop a new paradigm to place the study of this process of self-consciousness in the center of our discipline; and we will begin studying the process whereby all paradigms exercise their influence through language. (Bizzell, "Scientism" 768)

Bizzell's emphasis on the development of a new paradigm leads her to Perelman and Olbrechts-Tyteca's The New Rhetoric. She states that Perelman and Olbrechts-Tyteca's "theoretical analysis of the function of language in determining standards of reasonableness . . . may help to advance our new paradigm." According to Bizzell, Chaim Perelman and L. Olbrechts-Tyteca "discuss language in terms reminiscent of those Kuhn uses to describe the protocols binding together a paradigm-governed scientific community" ("Scientism" 768-69). Bizzell quotes from The New Rhetoric (quoted in part here), noting that Perelman and Olbrechts-Tyteca argue along Kuhnian lines:

> All language is the language of a community, be this a community bound by biological ties, or by the practice of a common discipline or technique. The terms used, their meaning, their definition, can only be understood in the context of the habits, ways of thought, methods, external circumstances, and traditions known to the users of the terms. . . . Language . . . is linked to a social and historical situation which fundamentally conditions any distinction one might wish to draw

between judgments of reality and value judgments. (Qtd in Bizzell, "Scientism" 769)

Bizzell goes on to assert:

> [Perelman and Olbrechts-Tyteca] cannot draw a distinction "between judgments of reality and value judgments"--that is, between empirical evidence and ideology. . . . One must attempt to uncover the "social and historical situation" of a community to understand its language, for that language expresses "certain definite positions"--an ideology or paradigm--that cannot be taken simply as the "reflection of an objective reality." (Bizzell, "Scientism" 769)

Bizzell uses Perelman and Olbrechts-Tyteca's remarks to bolster her own interpretation of Kuhn. She argues that their analysis reinforces Kuhn's position that "a paradigm gains ascendancy not because it is proved true, but because 'preceding argumentation' [Perelman and Olbrechts-Tyteca's term] within the community has persuaded most of its members that it is a reasonable choice." Further, Bizzell contends that Perelman and Olbrechts-Tyteca's work extends Kuhn's argument, moving it beyond the sense of a paradigm as provisional, "established by reasonable debate" and "redeeming his [Kuhn's] account of the transition to a new paradigm from the charge that community members can only submit to it passively" (Bizzell, "Scientism" 769).

Bizzell sees the domain of English studies under the new paradigm as "the study of the ways in which a language community's methods of argumentation establish standards for thinking and acting together that are 'neither compelling nor arbitrary,' but reasonable--in other words, the domain of rhetoric" ("Scientism" 769-70). Her argument for applying Kuhn thus becomes an instrument for contending that the true

future of English studies lies in the study of rhetoric. Bizzell concludes that "Thomas Kuhn is of value to English studies only if he does not lead us into scientism," but rather "toward a new paradigm that allows us to examine the ways in which language sharpens and directs critical analysis of the historical situation in which we and our students and our society find ourselves" ("Scientism" 771).

Bizzell's argument reinforces earlier efforts to apply Kuhn to composition studies by suggesting the utility of Kuhn's paradigmatic structure even though much of Kuhn's theory remains "controversial in his own field" ("Scientism" 765). At the same time, Bizzell cautions against abusing his ideas, relating Kuhn's own confusion over attempts by scholars in the humanities to apply theory and ideas that he claims to have drawn from them in the first place. In her brief examination of shared language's impact on a community, she attacks the position, as expressed by Maxine Hairston, that the revolution in the teaching of writing can be supported solely by empirical evidence; Bizzell sees such a position as "undermining" the idea of "the individual as a reasonable being interacting critically with reality" ("Scientism" 771). Bizzell's reliance on Perelman and Olbrechts-Tyteca's The New Rhetoric strengthens her position by providing a theoretical basis for her argument about the importance of rhetoric to English studies.

Bizzell's essay is especially important as an early expression of Kuhnian theory because of the position she takes on disciplinehood. By contending that the real domain of English studies is rhetoric, Bizzell promotes composition studies as an established discipline undergoing a paradigm shift. She suggests that "our interest in Kuhn can facilitate this shift, both by helping us to understand the kind of debate we are experiencing, and by indicating a productive field of research for our community, under a new paradigm" (Bizzell, "Scientism" 771). By arguing against overdependence on

empirical evidence at the expense of a community's shared language, values, social cohesion, and historical situation, she reaffirms the humanistic nature of the discipline.

Rather than following Bizzell's lead and offering her own interpretation of Kuhnian theory, Lisa S. Ede relies on Young's version of Kuhn. Ede opens her October 1979 CCC article, "On Audience and Composition," with the maxim: "Consider your audience." She declares that it has become "one of the most quoted and least understood of what might . . . be called composition commonplaces" (Ede 291). In arguing that "teachers of written composition should place greater emphasis on the role of audience," Ede refers to Wallace Douglas's earlier contention about the "new paradigmatic image of the rhetorician" as a "man thinking" (Ede 292) and uses Kuhn's sense of paradigm as interpreted by Richard Young in his essay in Cooper and Odell's 1978 Points of Departure. She notes that: "In general, there seems to be . . . a growing dissatisfaction with what Young calls the "current-traditional paradigm," which emphasizes product over process, style and usage over content, editing over invention--and which tends to ignore the instrumental, or audience-centered, purposes of composition" (Ede 292). Both works suggest the need to place greater emphasis on the role audience in composition.

In the remainder of her essay, Ede concentrates on offering two possible ways to encourage students to set the context for their writing in the composition classroom: "the teacher can provide the context and audience for the students, or the teacher can stimulate the students to create them [context and audience] for themselves." Ede prefers the latter and encourages her students "to create their own context, their own rhetorical situation" (294).

The references to Douglas and Young serve to help Ede frame her argument that not enough attention is given in many composition classrooms to context and audience formulation. She never refers to Kuhn directly. Instead, she voices agreement with

Young's remarks about dissatisfaction with the current-traditional paradigm, which she attacks for ignoring audience.

The influence of Young's 1978 essay in Points of Departure also clearly shows in Barry M. Kroll's March 1980 article in CE, "Developmental Perspectives and the Teaching of Composition." Kroll begins with a direct reference to Young's essay, noting that Young "calls for more historical research in the field of composition-rhetoric, particularly investigations of the development of contemporary approaches to the teaching of writing." Kroll remarks: "Drawing upon the work of Thomas Kuhn, Young asserts that our approaches to composition teaching are based on sets of tacit assumptions, and that these assumptions form a paradigm" (Kroll 741). Like Stewart, Kroll quotes Young's definition of paradigm as "a system of widely shared values, beliefs, and methods that determines the nature and conduct of the discipline" (Qtd in Kroll 741). But Kroll goes a step further than Young and addresses up front the "problematic" use of Kuhn's "paradigm." In a footnote, Kroll refers to Kuhn's 1974 essay, "Second Thoughts on Paradigms," in which Kuhn tackles the problem of consistent usage of the word. Kroll prefers "to use the term 'perspective' . . . in much the way Young uses 'paradigm': to denote a disciplinary orientation derived from a larger world view" (Kroll 741).

Even though he differs slightly from Young in his choice of terminology, Kroll, quoting in places from Kuhn's Enlarged edition of Structure, argues:

> Young's introduction of the Kuhnian concept of paradigms to the field of composition teaching should stimulate at least two kinds of inquiry. First, we need to explore further the nature of the controlling paradigms in our discipline. Young has discussed one plausible candidate--"current-traditional" rhetoric--for the major paradigm, but we need additional proposals from other

> perspectives. Second, we need to reconsider the relevance of Kuhn's theory of the development of the physical sciences for the discipline of composition-rhetoric. Kuhn himself suggests that scientific development is "strikingly different" from the development of other fields, and that a key element of this difference is the "relative scarcity of competing schools in the developed sciences." One of the distinguishing characteristics of the contemporary social sciences, for example, is the sustained coexistence of two or more well-defined but competing paradigms. (Kroll 742)

While Kroll distinguishes what he calls "composition-rhetoric" as a discipline, he does so from a perspective that posits the existence of more than one "controlling," perhaps even "competing," paradigm, a characteristic he attributes to "the contemporary social sciences."

Continuing his thought, Kroll proposes that "the field of composition teaching has been influenced by two dominant but opposing paradigms, two theoretical perspectives from which to approach the teaching of writing." He defines the two approaches--the interventionist, or "nurture" perspective and the maturation or "nature" perspective--in terms of human development and each perspective's conception of "the main tasks of education." He goes on to argue that many of the approaches to teaching composition can be described in terms of "nurture" or "nature" and belong to one or the other of these two "dominant developmental perspectives" (Kroll 742).

Kroll focuses on defining the particular attributes of each perspective or paradigm and applying those criteria to the teaching composition courses. He demonstrates the influence of adherence to one perspective or the other in terms curriculum, classroom environment, and instructional technique, casting each perspective against historical changes in the way composition has been taught and noting the "resulting shift from a

school-centered pedagogy to a child-centered curriculum" (Kroll 746). Although the interventionist perspective has tended to dominate college writing programs, Kroll suggests that the rise of the more student-centered, maturationist perspective in college composition programs can be seen in the attention to two central concepts--personal writing and writing as artistic expression.

According to Kroll, "interventionism and maturationism have provided competing perspectives on which to base the teaching of composition" and "have coexisted uneasily in composition pedagogy during this century" (747). Their coexistence may suggest that composition studies is still in a "pre-paradigmatic state." Kroll argues:

> If we assume that these perspectives are disciplinary matrices analogous to Kuhn's paradigms, then the presence of two opposing perspectives can be seen as problematic for our discipline. Without substantial agreement concerning values, methods, and goals there can be little progress toward validating specific principles or pedagogies. Perhaps we are in a pre-paradigmatic state of competing schools. Or perhaps we are waiting for a "revolution" in which one of the perspectives unifies our field--if Kuhn's pattern for the development of scientific fields applies to our discipline. (Kroll 747)

But Kroll does not stop with just questioning the application of Kuhn's pattern to composition. Instead, he proceeds to suggest an "evolutionary" alternative--"the resolution of conflicting views through an 'emergent synthesis'" (747), a synthesis of "nurture" and "nature" distinguished by "its own strong conceptual roots" (748). Kroll's candidate for the emerging perspective is "interactionism," the perspective that development is "a dynamic interaction between individual and environment, between

internal and external influences" (748). Calling Jean Piaget the "chief spokesman" for the third perspective, Kroll elaborates:

> In every encounter with the world, the environment is transformed in the knowing act, but the knower is transformed as well. Developmental progress results from a synthesis of two seemingly contradictory but interdependent tendencies: continuity (the individual's conservation of organized ways of acting) and discontinuity (the individual's differentiation of ways of acting in order to meet new, problematic situations). The result is an organism which has maintained its coherence while extending its range and level of functioning. (Kroll 748)

In Kroll's eyes, the "interactionist" perspective champions "the aim of education" as "the development of higher levels of active intelligence--those forms of reflective thinking which require not only the acquisition of knowledge but also the attainment of intellectual discipline and conceptual skills" (748).

Under Kroll's third perspective, "teachers and students share responsibility for learning." Kroll's approach suggests the collaborative nature of the learning enterprise; the teacher acts as a resource, posing "meaningful, challenging problems" and helping "students acquire the skills or knowledge necessary to solve those problems." The student retains individual responsibility for "active participation," working with the teacher "in a spirit of cooperation" (Kroll 748). Kroll cites the work of John Dewey as indicative of the interactionist perspective.

Kroll returns to his earlier thought that the interactionist perspective may hold promise "as a theoretical perspective which can synthesize opposing elements in the major approaches to composition teaching." He declares that it "is able to synthesize the

divergent approaches [of interventionism and maturationism] while maintaining its own integrity" (Kroll 750), and he suggests ways in which it can lead to such a synthesis by integrating "elements of maturation and intervention" (Kroll 751). Concluding, Kroll argues that:

> [T]he newly emerging interactionist perspective offers its own dialectical view of human development, thereby synthesizing a number of divergent approaches in the teaching of composition. Thus, interactionism promises a unified perspective which could both support a comprehensive pedagogy and sustain a rigorous program of research in the field of composition. (Kroll 752)

At the beginning of his article, Kroll seems to favor applying Kuhn to the field of composition studies. One gets the sense from his writing that composition, or at least composition-rhetoric, is a discipline with "controlling" paradigms. However, Kroll quickly abandons that notion, opting instead to borrow Kuhn's sense of a pre-paradigmatic state, if Kuhn's pattern applies at all! Substituting "perspective" for "paradigm" helps Kroll distance himself in some ways from Kuhn, but never completely. Using Young's sense of paradigm--"to denote a disciplinary orientation derived from a larger world view" (Kroll 741)--allows Kroll to further distance himself from applying Kuhn's pattern exactly. He ably borrows Kuhnian terminology, then recasts it to fit the shape of his own ideas, finding ample circumstantial evidence in general observations about the history of the profession.

In one sense, Kroll's borrowing depends upon the secondary source of Young's 1978 "Paradigms and Problems: Needed Research in Rhetorical Invention." Kroll derives his sense of paradigm from that essay. However, Kroll is not content to just accept Young's interpretation of Kuhn; Kroll seeks additional elucidation on the concept of paradigms by

turning to sources of Kuhn's work other than Structure. Few other users of Kuhn seem to have done this during the early stages of importing Kuhn into composition studies.

The decade immediately following the 1962 publication of Kuhn's work saw only limited explicit use made of his ideas in composition studies. Gage (1963) introduces portions of Kuhn's theory to an audience consisting of key players in a larger discipline of English. Kinneavy (1971) borrows Kuhn to support greater emphasis on discourse theory. Sawyer (1972) simply notes Kuhn's influence on explaining progress in the natural sciences. Between the three lie a number of others borrowing from the fields of history and philosophy of science without borrowing Kuhn specifically. Yet, the growing emphasis on empirical study spurred by Braddock, Lloyd-Jones, and Schoer's 1963 report--an emphasis on applying the scientific method to inquiry into writing--helps create a climate ripe for additional borrowing from history and philosophy of science, especially borrowing of Kuhn. Many construe borrowing from the already established disciplines of the sciences as borrowing the validity of those sciences as well.

During the late seventies and early eighties, borrowers of Kuhn appropriate principally his ideas about paradigms, shifts, and communities. While they suggest that Kuhn's theory offers composition studies an excellent tool for analyzing its own structure, a political decision underlies many of those borrowings as well. By claiming to possess a paradigm, that composition is undergoing a shift in paradigms, or that composition is in a preparadigmatic state, users of Kuhn's ideas simultaneously make claims about the status of composition as a discipline or a science. Some see Kuhn's statements about disciplinehood as ammunition for their own claims about the status of composition studies. If we can perceive composition studies as possessing a paradigm, the hallmark of a mature science in Kuhn's definition, we can claim much of the maturity that Kuhn contends comes with possession and adherence to a paradigm. On this level,

borrowings of Kuhn become inherently political regardless of the argument his theory is used to justify. Perceiving the field as undergoing a paradigm shift away from some sort of current-traditional rhetoric becomes evidence to suggest a revolution as well as reinforcing a sense of maturity. If we only perceive the field as preparadigmatic, a term that Kuhn seems to abandon in "Second Thoughts on Paradigms," "Reflections on My Critics," and "Postscript--1969," we can argue for a structure or perception of the field as emerging and still moving towards maturity; in other words, composition studies is not "there" yet, but is rising out of some tangle of competing schools of thought on writing theory, practice, research methodology, and pedagogy. These same political arguments become even more evident throughout the remainder of the eighties and into the nineties.

CHAPTER SIX

THOMAS KUHN AND COMPOSITION STUDIES, 1982-1989

> It seems natural that composition theorists, especially in their emerging intellectual and institutional struggles toward respectability, would look to shore up their credibility by borrowing accepted ideas and methodologies from established disciplines.[24]
>
> --John Clifford, 1986

The onset of the eighties brings an expansion of the debate over Kuhn's application to composition studies. Richard Larson mentions in his October 1981 editor's note that the February 1982 issue of CCC "will focus on paradigms within which we plan our teaching" in composition ("Editor's" 250). In that February 1982 issue, Janet Emig "identifies some of the assumptions and implications of a good deal of current research ('inquiry') about writing" in her "Inquiry Paradigms and Writing" (Larson, "Notes" 17). Maxine Hairston expounds upon her 1978 address to CCCC, virtually drowning out Patricia Bizzell's 1979 interpretation of Kuhn and igniting throughout the decade a blaze of counterarguments and position statements about Kuhn's utility and applicability.

Many of the book reviews appearing in CCC give further indication of Kuhn's influence on composition studies. For instance, three reviewers in the February 1982 issue of CCC address works using Kuhn and paradigms or shifts; one reviewer in 1983 talks of paradigms. S. Michael Halloran, writing in 1982, disagrees with the view expressed in Aviva Freedman and Ian Pringle's Reinventing the Rhetorical Tradition (1980) that "rhetoric is a discipline in something approximating Kuhn's sense of that term" (95). In his review of Donald McQuade's collection, Linguistics, Stylistics, and

the Teaching of Composition (1979), Lester Faigley credits Frank D'Angelo with making an important point about the "conceptual patterns [that] may underlie all discourse" in D'Angelo's "Paradigms as Structural Counterparts of *Topoi*." Charles Moran attacks Harvey S. Wiener's The Writing Room: A Resource Book for Teachers of English (1981) as being "simultaneously alive in two different worlds" and claims that the book is "living proof" that "our field is in the throes of a paradigm shift" ("Room" 103). Jack Selzer refers to Kuhn in a 1982 review of Writing in the Professions (1981). Selzer cites Hairston's "Winds of Change" article, noting that she "analyzes the 'new paradigm for teaching writing,'" and he suggests that Professions represents one of the "new group of books that will codify the new paradigm and extend its influence" (337).

Other reviewers throughout the decade also rely on Kuhnian terminology or theory. Writing in 1986, Linda H. Peterson characterizes some of the essayists in Writing and Reading Differently: Deconstruction and the Teaching of Composition and Literature (1985) as using deconstruction "to reassert the 'new paradigm' in writing," generating support for "process pedagogy" (358). A year later, Stephen P. Witte and Richard L. Larson, in separate reviews of George Hillocks, Jr.'s Research on Written Composition: New Directions for Teaching (1986), address issues connected to "the dispute over research methodologies and paradigms" spurred by Hillocks's examination of empirical research (Witte 206) and his "secondary agenda" of refuting "Emig's criticism of the 'positivist' research paradigm" (Larson, "New Directions" 210). The references to Kuhn in such reviews reflect efforts to account for the implications of Kuhn's theory.

Uses of Kuhn's terminology and concepts throughout the period, often without direct attribution to him, suggest the general adoption by many compositionists of at least part of his theory--paradigms, shifts, crises, and revolutions--to explain what is happening to composition studies. "Paradigm" becomes an accepted part of composition terminology. For example, Jeanne Fahnestock describes A. L. Becker's paragraph taxonomies as

"paradigms of expository paragraphs" (Fahnestock 400); Leonard Moskovit uses "paradigm" in a quoted exemplar in his article on clarity (Moskovit 456); and Peter Elbow speaks of paradigms for speech and writing in his 1985 "The Shifting Relationships between Speech and Writing" (Elbow 286). John Clifford, in a 1983 review of Cognitive Processes of Writing, remarks that social scientists "are . . . working inside a different paradigm than English professors" ("Cognitive" 99).

Elsewhere in the professional literature of rhetoric and composition, compositionists speak of shifts in paradigms, again often without any mention of Kuhn's contributions. In "Multiple-Choice and Holistic Essay Scores: What are They Measuring?" her 1982 call for the development of evaluation procedures designed to address the growing emphasis on the writing process, Betty Bamberg notes: "Much has been written about the 'paradigm shift' from writing-as-product to writing-as-process" (406). Bamberg, like so many others, never mentions Kuhn; she simply picks up on the terminology some compositionists are using to validate or explain the move from product to process. Likewise, Marilyn S. Sternglass opens her introduction to Bruce T. Petersen's 1986 collection, Convergences: Transactions in Reading and Writing, without referring to Kuhn:

> It has become acceptable, even fashionable, to speak of the paradigm shifts that have occurred in reading (from a skills model to a psycholinguistic model) and in writing (from a product orientation to a process orientation). What has been examined less frequently is the paradigm shift that has been occurring in the relationships among reading, writing, literature, and literacy. The relationships among these language processes have undergone shifts from being treated as parallel processes . . . or interactive processes, to being treated as transactive processes. (Sternglass 1)

Essayists in the Petersen collection explore the convergences growing out of research in transactional studies, promoting the triumph of process over product. Such uses of Kuhnian concepts of paradigm and shift serve to demonstrate the interpenetration of Kuhn's theory into composition studies.

Others familiar with Kuhn's theory focus on the crisis they perceive in composition studies, casting their reflections on the identity of the profession in Kuhnian terms. William F. Irmscher's call for reintegrating "the literary and critical values of our discipline" ("Identity" 87) in his essay, "Finding a Comfortable Identity," supports a view of a separate discipline of composition studies seen no longer "solely as a practitioner's art, but as a subject for scholarly study" ("Identity" 81). Irmscher sees that discipline in crisis, primarily for what he perceives as an overreliance on "controlled experimental studies producing statistical evidence" ("Identity" 82) and the imitation of other disciplines through borrowing "without fully considering the context and bounds of our own discipline" ("Identity" 84). Although he agrees that "we should continue to borrow insights from other disciplines," Irmscher argues that the narrow focus of experimental studies, a methodology borrowed from the sciences, ignores the fundamental nature of inquiry into the complex human process of writing ("Identity" 85). Further, he believes:

> We need to reassert the humanistic nature of our own discipline, which in this context means its concern for the individual as a human being, not as a quantity or specimen. We need to adopt procedures based upon shared assumptions. If we accept the idea that the paradigm of a discipline represents the unspoken assumptions by which it functions, then we may find more implicit

understanding about composition than explicit statement. (Irmscher, "Identity" 85)

In his essay, Irmscher cites works by Emig and Hairston, prominent early borrowers of Kuhn. Arguments such as Irmscher's underscore Kuhn's influence on the perception of the disciplinary identity of composition studies.

Kuhn's consideration of the influence of textbooks on the education of members of professional communities receives more attention as the trend to study the historical evolution of composition gains momentum. In 1983, Mike Rose refers specifically to Kuhn's ideas about a discipline's textbooks when writing: "textbooks are the repository of our knowledge on a given subject at a given time. That knowledge is usually knowledge of a certain kind: facts, data, information, theories. Not so much of how to do but knowledge of what is known or is currently surmised" (Rose, "Speculations" 211). Sharon Crowley, without citing or mentioning Kuhn, but referring to others who do, talks about "current-traditional compositional textbooks" in her 1985 examination of invention in the nineteenth century; her discussion of the role of textbooks closely parallels Kuhn's while dependent upon the acceptance of the notion of some current-traditional paradigm or rhetoric (59). In his 1986 "Seeing the Text," Stephen A. Bernhardt speaks of the "characterization of expository texts . . . enshrined in the handbooks of our trade" as seeming "sufficiently influential within our discipline to be considered the norm, the product we encourage in our practice within the 'current-traditional paradigm'" (68). Bernhardt cites Young's 1978 essay in Points of Departure. Analyzing problems connected with the preparation of bibliographies of composition research, Patrick Scott notes in his 1986 "Bibliographical Problems in Research on Composition" that "Kuhn makes it one of the characteristics of a new research paradigm that much of the publication in a field is intended to consolidate and

disseminate the new approach" (170). When Robert J. Connors, in his 1986 "Textbooks and the Evolution of the Discipline," details the influence of composition textbooks and the evolution of composition studies as a discipline, he echoes Kuhn without citing him, referring instead to Stewart's 1978 "Composition Textbooks and the Assault on Tradition" and C. H. Knoblauch's 1984 address, "Neither Current, Nor Traditional, Nor a Paradigm."

Others carry on the same tradition of textbook examination, building upon earlier users of Kuhn ideas. Kathleen E. Welch addresses the ideological aspects of textbook composition in her 1987 "Ideology and Freshman Textbook Production: The Place of Theory in Writing Pedagogy." Welch defines ideology "as a shared system of belief, an interconnecting set of values that informs attitude and behavior" (271)--a definition very similar to Kuhn's disciplinary matrix. Later, Welch notes the strong dependence of current-traditional structure on the revelation that "the need for a paradigm, any paradigm, even a theory-conscious one, comes before any consideration of real thinking about how people write" (275); Welch notes that the dependence "has the characteristics of ideological belief" (275). In 1989, W. Ross Winterowd cites the earlier work of Stewart (1978), Connors (1986), and Welch (1987) in "Composition Textbooks: Publisher-Author Relationship." While reiterating Stewart's charge that publishers have ignored "the theoretical revolt" in composition theory to produce books still steeped in the current traditional paradigm (139), Winterowd offers an examination of the potential hazards of dealing with publishers. These uses suggest the extent of acceptance of Kuhn's comments upon the dogmatic nature of textbooks and the role of those textbooks in writing education and research.

Other parts of Kuhn's theory, principally his notions about change and professional communities, find articulation in CCC and books during the eighties. Irvin Hashimoto, in his 1985 discussion of the limitations of structured heuristic procedures, refers to

Kuhn's conclusions about "the difficulties people have in shifting perspectives, *changing* their conceptual structures, and recognizing other ways to see issues and problems than the ones they are accustomed to" (75). Hashimoto uses Kuhn as an example, writing, "Thomas Kuhn, for instance, suggests that for scientists, such shifts 'cannot be forced' and that shifts in perspective will occur, if they occur, 'a few at a time . . . until the last holdouts have died'" (75). Richard C. Freed and Glenn J. Broadhead, in their 1987 "Discourse Communities, Sacred Texts, and Institutional Norms," argue for continued research into discourse communities. They suggest that the move towards disciplinarity has also been a move towards establishing composition studies as an autonomous discourse community. In their contention, Freed and Broadhead refer directly to Kuhn as well as some of his principal borrowers, Hairston and Young. They assert:

> For both overtly and tacitly, these communities establish paradigms that discourses adhere to or, often at their risk, depart from (see, for example, Kuhn; Hairston; and Young). The paradigms reign like prelates and governments reign: they set an agenda and attempt to guarantee its meeting, often rewarding those who do and discouraging those who don't. They legislate conduct and behavior, establishing the eminently kosher as well as the unseemly and untoward. (Freed and Broadhead 156)

Freed and Broadhead use "paradigm" in Kuhn's sense of disciplinary matrix, tying it to a definition of community discourse. About the same time, North, in his 1987 The Making of Knowledge in Composition, examines composition studies from the standpoint of communities, berating and finding fault with Young's 1978 interpretation of Kuhn.

The writings of Emig, Fulkerson, Hairston, North, and others reflect the evolution of Kuhn's theory within composition studies. Many adopt his terminology as it becomes

fashionable, often divorcing that terminology from its original source or relying solely on interpretations of Kuhn offered by compositionists such as Kinneavy, Young, or Hairston. Still others continue the debate over the appropriateness of using Kuhn in composition, focusing on the interpretation of his theory by other compositionists and the assessment of the theory's implications for inquiry into writing. For example, Robert J. Connors investigates borrowings of Kuhn in his 1983 CE essay, "Composition Studies and Science." Connors reviews appropriations by Gage, Kinneavy, Young, Bizzell, Hairston, Emig and others and treats the disciplinary response of the philosophy of science to Kuhn in the process; Connors is one of the few in composition studies to do so. Fulkerson, in his 1984 "Kinneavy on Referential and Persuasive Discourse: A Critique," revisits James Kinneavy's 1971 A Theory of Discourse in a critique of Kinneavy's theory and "four-part paradigm" ("Critique" 43); he notes that Kinneavy, in places, "is heavily indebted to Kuhn's discussion of the nature of scientific revolutions" ("Critique" 45). With increasing frequency, articles in CCC reference secondary sources on Kuhn developed within composition studies, especially treatments by Kinneavy, Young, Hairston, and Emig.

A growing debate over the efficacy of borrowing from science, especially cautions about an overemphasis on the scientific method and empirical research, accompanies the appropriation of Kuhn and others from the history and philosophy of science. Warnings about too much emphasis on "empirical" approaches to writing research, a descent into what Michael Holzman refers to in "Scientism and Sentence Combining" as "scientistic rather than scientific" (73), appear at the same time as calls for more scientific emphasis (such as George Hillocks, Jr.'s 1986 Research on Written Composition: New Directions for Teaching). Holzman, while declaring that the "practice of the natural sciences . . . is exemplary for all disciplines which have as their goal the achievement of objective knowledge" (73), nevertheless argues:

> Humanistic knowledge achieves intersubjective validity in a manner different from, but also no less real than, that by which valid knowledge in the sciences is found. The humanities are the sciences of man; the nature and the extent of possible humanistic knowledge are themselves subjects of study by humanists. The humanities are fundamentally unlike the natural sciences, which seek to approach as close as possible to absolute knowledge. (Holzman 78)

Although Holzman aims directly at borrowing "methodologies for social scientific research," his words have equal applicability for borrowings from the history and philosophy of science (79). Likewise, Ralph F. Voss admits the ability of scientific methods to bring "us greater, more precise knowledge of the field of composition" than before achieved and that "scientific methods will continue to reveal more knowledge to us," but he remains cautious about the "science consciousness" shown by too many in composition (279); Voss distinguishes between the precision available with laboratory science and that of the social science research methodologies used by many compositionists.

With the movement from product to process already well underway in the early eighties, calls for borrowing Kuhn or acknowledgments of his usefulness in composition studies accompany the rise of two other principal movements in the latter half of the eighties: writing across the curriculum and attention to the role of discourse communities. Elements of Kuhn's theory, especially his attention to the role of communities in establishing and governing the shared language, habits, values, and attitudes that constitute membership in a specialty, support both movements. In Kuhn's eyes, membership in individual communities means adherence to the particular paradigm or paradigms accepted by that community. Each community shares a language

incompatible or incommensurable with the language of communities that do not share the paradigm. Such ideas play out within both the writing across the curriculum and the discourse community movements.

Inherent in the writing across the curriculum movement is the idea that different disciplines compose differently. In other words, the various disciplines use language in ways unique to the particular discipline; they possess particular conventions for presenting information. For example, engineers do not write the same way as chemists or sociologists. The nature, the content, and even the tools of the disciplines vary considerably. The disciplines may possess different ideas about what constitutes knowledge and the nature of evidence for that knowledge. In Kuhnian terms, each discipline adheres to a paradigm of its own making.

Simultaneously, the idea of discourse communities also draws strength from Kuhn's theory. Adherents to this movement focus on the nature of the discourse shared by community members, seeing initiation into the community as a process of learning to communicate in the shared discourse. Thus, we see efforts to identify the constitution of various discourse types tied to the communities that these discourses serve. The language and community elements of Kuhn's theory function to buttress arguments for discourse communities.

By the end of the decade, concern for the writing across the curriculum and discourse community movements occupies more attention within the pages of CCC; direct references to Kuhn and his work begin to diminish. Still, Kuhn's work achieves the status of tacit knowledge within composition studies. Familiarity with the major tenets of Kuhn's theory and his language become part of the scholarly training and background of most compositionists even if only to allow them to enter into the debate over applying Kuhn to the profession.

The Winds of Revolution, 1982-1986

> [C]omposition studies are finally coming to constitute a genuine discipline and are no longer a mere purblind drifting on the current of unexamined tradition.[25]
>
> --Robert Connors, 1985

Fueled by the increasing emphasis on using paradigms to describe various directions of thought in composition studies, CCC devoted much of its February 1982 issue to discussing the paradigms in the teaching of writing. Maxine Hairston's "The Winds of Change: Thomas Kuhn and the Revolution in the Teaching of Writing," patterned in part on her 1978 address to CCCC, overshadows Janet Emig's important, but less cited "Inquiry Paradigms and Writing." In the years following the publication of their essays, the debate over the applicability of Kuhn's ideas to theory, practice, and pedagogy within the discipline of composition studies widens

In her "The Winds of Change: Thomas Kuhn and the Revolution in the Teaching of Writing," Hairston reiterates her 1978 call for a revolution in writing pedagogy. While not the first to propose Kuhn's application to composition studies, Hairston sets out one side of the debate over Kuhnian theory by adopting "the perspective of Thomas Kuhn to organize her analysis of developments in the teaching of writing" (Larson, "Note" 17). Hairston's article elicits a number of responses and counter-statements throughout the decade of the eighties, emphasizing the increasing importance attached to supporting or denying Kuhn's application to teaching as well as researching composition. For Hairston, Kuhn's work offers an excellent opportunity to analyze the direction that the teaching of writing is taking in the early eighties.

Hairston explains the essence of Kuhn's work on the change within scientific communities and notes the potential influence of applying Kuhn's theories to composition. She begins by summarizing Kuhn:

> Kuhn hypothesizes about the process by which major changes come about in scientific fields, and conjectures that they probably do not evolve gradually from patient and orderly inquiry by established investigators in the field. Rather, he suggests, revolutions in science come about as the result of breakdowns in intellectual systems, breakdowns that occur when old methods won't solve new problems. He calls the change in theory that underlies this kind of revolution a *paradigm shift.* (Hairston, "Winds" 76)

Hairston then states up front that she believes "we are currently at the point of such a paradigm shift in the teaching of writing, and that it has been brought about by a variety of developments that have taken place in the last 25 years" ("Winds" 76). She follows up this assertion with an extended definition of Kuhn's thesis and an attack on the current-traditional paradigm and its proponents.

Hairston reduces the essence of Kuhn's theory of paradigm shifts to a few brief paragraphs, presented here in attenuated form:

> When a scientific field is going through a stable period, most of the practitioners in the discipline hold a common body of beliefs and assumptions; they agree on the problems that need to be solved, the rules that govern research, and on the standards by which performance is to be measured. They share a conceptual model . . . a paradigm, and that paradigm governs the activity in their profession.

Students who enter the discipline prepare for membership in its intellectual community by studying that paradigm.

But paradigms are not necessarily immutable. When several people working in a field begin to encounter anomalies or phenomena that cannot be explained by the established model, the paradigm begins to show signs of instability. . . . [W]hen enough anomalies accumulate to make a substantial number of scientists in the field question whether the traditional paradigm can solve many of the serious problems that face them, a few . . . will devise a new model. And if enough scientists become convinced that the new paradigm works better . . . they will accept it as the new norm.

This replacement of one conceptual model by another one is Kuhn's *paradigm shift*. . . . Such shifts are usually disorderly and often controversial, and the period in which they occur is apt to be marked by insecurity and conflict within the discipline. (Hairston, "Winds" 76-77)

Hairston completes her summary of Kuhn's theory by focusing on the disruption which Kuhn notes that paradigm shifts induce. The shifts "occur only when the number of unsolved problems in a discipline reaches crisis proportions and some major figures in the field begin to focus on the unsolved problems" (Hairston, "Winds" 77). Citing Richard Young's 1978 essay, "Paradigms and Problems: Needed Research in Rhetorical Theory," Hairston suggests that resistance to the change in paradigms, especially changes in textbooks which depend upon the older paradigm for their validity, stems from vested interest. As the new generation of scholars accepts the new paradigm, older practitioners are forced reluctantly into adherence as well. Hairston remarks, "Those who cling to the old paradigm lose their influence in the field because the leaders in the

profession simply ignore their work. When that happens, the paradigm shift is complete, and the theory that was revolutionary becomes conventional" ("Winds" 77).

Hairston acknowledges that her reduction of Kuhnian theory to a few paragraphs is necessarily "sketchy and too simple," but she asserts, "I think it accurately reflects the key points in his theory" (Hairston, "Winds" 77). (Indeed, Robert J. Connors later compliments Hairston's reduction as particularly "well done" ("Science" 3).) Continuing, Hairston notes that though Kuhn "did not claim or even suggest that his model for scientific revolution could or should apply to the social sciences or humanities," she believes that:

> [C]omposition theorists and writing teachers can learn from Thomas Kuhn if they see his theory of scientific revolutions as an analogy that can illuminate developments that are taking place in our profession. Those developments, the most prominent of which is the move to a process-centered theory of teaching writing, indicate that our profession is probably in the first stages of a paradigm shift. (Hairston, "Winds" 77)

The entire thrust of Hairston's opening pages rests in summarizing Kuhn sufficiently to provide the structure for her real argument--a polemic against the current-traditional paradigm and a championship of an emerging process-oriented approach to teaching composition. If her readers are to accept and apply Kuhn's idea of paradigm shift to composition studies, Hairston has to demonstrate the ability of Kuhnian theory to explain change within composition studies. Further, she has to offer evidence of its validity even if she only calls for using it as an "analogy." She cannot simply throw out Kuhnian theory as an explanation for change without articulating what composition is changing from and what it is changing into.

After her interpretation of Kuhn, Hairston proceeds with the next step, articulating the principal characteristics of the paradigm under assault. For this articulation, she relies on Young's listing of the features of the current-traditional paradigm, buttressed by an article by James Berlin and Robert Inkster as well as her own observations. Young offers a list of overt features of the paradigm:

> . . .the emphasis on the composed product rather than on the composing process; the analysis of discourse into words, sentences, and paragraphs; the classification of discourse into description, narration, exposition, and argument; the strong concern with usage . . . and with style; the preoccupation with the informal essay and the research paper; and so on. (Qtd in Hairston, "Winds" 78)

Hairston notes that Young supplements his list by identifying an underlying "'vitalist' attitude toward composing: that is, the assumption that no one can really teach anyone else how to write because writing is a mysterious creative activity that cannot be categorized or analyzed" (Hairston, "Winds" 78).

To Young's list of features, Hairston integrates those identified by Berlin and Inkster in their 1980 article in Freshman English News, "Current-Traditional Rhetoric: Paradigm and Practice." According to Berlin and Inkster:

> [T]he traditional paradigm stresses expository writing to the virtual exclusion of all other forms, that it posits an unchanging reality which is independent of the writer and which all writers are expected to describe in the same way regardless of the rhetorical situation, that it neglects invention almost entirely, and that it makes style the most important element in writing. (Hairston, "Winds" 78)

To the combined list of features, Hairston adds her own observations:

> I would make three other points about the traditional paradigm. First, its adherents believe that competent writers know what they are going to say before they begin to write; thus their most important task when they are preparing to write is finding a form into which to organize their content. They also believe that the composing process is linear, that it proceeds systematically from prewriting to writing to rewriting. Finally, they believe that teaching editing is teaching writing. (Hairston, "Winds" 78)

Hairston waves her extended listing of the features of the current-traditional paradigm like a red flag. While she admits that the "vanguard of the profession have by and large adopted the process model for teaching composition," Hairston suggests that the majority of college composition practitioners continue to teach the traditional paradigm (Hairston, "Winds" 78). Thus, she progresses to an analysis of those practitioners who adhere to the paradigm and to condemnation of the administrators that allow or suggest that they do so "out of necessity, apathy, and . . . a benighted and patronizing view of the essential nature of composition courses" (Hairston, "Winds" 79). Hairston complains:

> I think . . . that the people who do most to promote a static and unexamined approach to teaching writing are those who define writing courses as service courses and skills courses; that group probably includes most administrators and teachers of writing. Such a view, which denies that writing requires intellectual activity and ignores the importance of writing as a basic method of learning, takes away any incentive for the writing teacher to grow professionally. People

> who teach skills and provide services are traditionally less respected and rewarded than those who teach theory. . . . Consequently there is no external pressure to find a better way to teach writing. (Hairston, "Winds" 79)

Hairston concedes that many of practitioners work hard at "trying to teach writing from an outmoded model," the current-traditional paradigm, and end up frustrated at the lack of success. She concludes: "But the writing teacher's frustration and disenchantment may be less important than the fact that if they teach from the traditional paradigm, they are frequently emphasizing techniques that . . . research has largely discredited" (Hairston, "Winds," 80).

Hairston turns again to Kuhn for an explanation of the inability of some practitioners to perceive the need and direction of change. According to Hairston, Kuhn notes that "the paradigm that a group of professionals accepts will govern the kinds of problems they decide to work on, and that very paradigm keeps them from recognizing important problems that cannot be discussed in the terminology of their model." In other words, "The traditional paradigm hides these problems" (Hairston, "Winds" 80).

Hairston uses Kuhn to further explain that textbooks contribute to the inability of practitioners to perceive problems outside of the traditional paradigm. Standard textbooks, according to Kuhn, reinforce and represent "the accepted authority" in a discipline, constituting "a major block to paradigm shift." Hairston suggests that many, "though certainly not all, of the standard textbooks in rhetoric and composition for the past two decades [1960s and 1970s] have been product-centered books that focus on style, usage, and argumentation." As proof, she refers to Donald Stewart's 1978 analysis of rhetoric texts, "Composition Textbooks and the Assault on Tradition." Hairston reiterates that Stewart found only seven of thirty-four texts examined "showed any awareness of current research in rhetoric"; the remainder followed the current-

traditional paradigm. Textbook change is conservative. Publishers are motivated by profits, and "they tend to direct the appeals of their books to what they believe the average composition teacher wants, not to what those in the vanguard of the profession would like to have" (Hairston, "Winds" 80).

Hairston's discussion of the current-traditional paradigm and its effects on practitioners turns into an exploration of the signs of change and crisis that seem to mark the transition period to a new paradigm. Hairston sees "in the current state of our profession enough evidence of insecurity and instability to suggest that the traditional prescriptive and product-centered paradigm that underlies writing instruction is beginning to crumble." Noting that the pressure creating the instability comes "from both inside and outside of the profession," Hairston suggests that the roots of the instability and change in theory probably evolved beginning "in the middle 1950s, from intellectual inquiry and speculation about language and language learning that was going on in several fields, notably linguistics, anthropology, and clinical and cognitive psychology" ("Winds" 80).

Hairston briefly mentions only a few of the developments leading up to the present instability she sees, concentrating on Noam Chomsky's transformational grammar, Francis Christensen's generative rhetoric, Carl Roger's criticism of behaviorist psychology, and the results of the 1966 Dartmouth Conference that "deëmphasized [sic] the formal teaching of grammar and usage . . . and emphasized having children engage directly in the writing process in a non-prescriptive atmosphere" (Hairston, "Winds" 81). According to Hairston, these shifts in theory combined with other concrete forces put pressure on writing teachers to adapt to the growing needs of expanding writing programs, and they helped to create an intellectual climate for change during the 1960s and 1970s.

Noting the climate for change, Hairston goes on to ask rhetorically, "But why should the paradigm begin to break down just now?" (Hairston, "Winds," 81) She attributes the answer to Kuhn, writing:

> He [Kuhn] says, ". . .external conditions may help to transform a mere anomaly into a source of acute crisis." I believe that the external conditions which have hastened the crisis in the teaching of writing are open admissions policies, the return to school of veterans and other groups of older students who are less docile and rule-bound than traditional freshmen, the national decline in conventional verbal skills, and the even larger number of high school graduates going on to college as our society demands more and more credentials for economic citizenship. Any instructional system would come close to collapse under such a strain. (Hairston, "Winds" 82)

Hairston describes the systemic response to the external pressures fomenting the crisis as "following the pattern that Kuhn describes in his book." She asserts that "our first response to crisis has been to improvise *ad hoc* measures to try to patch the cracks and keep the system running" (Hairston, "Winds" 82). For Hairston, these patches have included the creation of writing labs, individualized and computer-assisted instruction (both of which passed quickly according to Hairston), experiments with expressive writing, and sentence combining (which Hairston predicts will turn out to be an *ad hoc* measure like the others).

While crediting such patches as having "proved temporarily or partially useful," Hairston argues that, "none, however, has answered the crucial question: what is the basic flaw in the traditional paradigm for teaching writing? Why doesn't it work?" (Hairston, "Winds" 82). To answer that crucial question, Hairston returns to Kuhn and

notes: "Someone who cares has to ask that question [What is the basic flaw?] before the revolution can start because, as Kuhn points out, 'novelty ordinarily emerges only for the man who, knowing *with precision* what he should expect, is able to recognize that something has gone wrong'" (Hairston, "Winds" 82).

Hairston credits Mina Shaughnessy as the person in the teaching of composition with the insight to ask the questions, "What went wrong?" and "Why?" She goes on to offer some of Shaughnessy's reasoning as evidence of the need "to try to move behind students' written products and find out how those products came into being" (Hairston, "Winds" 84). Though Shaughnessy was not the first directing attention to process--Janet Emig, Charles Stallard, and James Britton and his colleagues share some of the credit--Hairston insists that:

> [Shaughnessy] was the first to undertake a large-scale research project whose goal was to find practical ways to teach the new students of the seventies to write. Her example, her book, and her repeated calls for new research in composition have undoubtedly been important stimuli in spurring the profession's search for a new paradigm. (Hairston, "Winds" 84)

The attribution to Shaughnessy serves to emphasize Hairston's contention that "we are poised for a paradigm shift" and to set up discussion of the emerging paradigm (Hairston, "Winds" 85). Hairston lists the twelve features of the new paradigm she sees emerging for the teaching of writing:

> 1. It focuses on the writing process; instructors intervene in students' writing during the process.

2. It teaches strategies for invention and discovery; instructors help students to generate content and discover purpose.

3. It is rhetorically based; audience, purpose, and occasion figure prominently in the assignment of writing tasks.

4. Instructors evaluate the written product by how well it fulfills the writer's intention and meets the audience's needs.

5. It views writing as a recursive rather than a linear process; pre-writing, writing, and revision are activities that overlap and intertwine.

6. it is holistic, viewing writing as an activity that involves the intuitive and non-rational as well as the rational faculties.

7. It emphasizes that writing is a way of learning and developing as well as a communication skill.

8. It includes a variety of writing modes, expressive as well as expository.

9. It is informed by other disciplines, especially cognitive psychology and linguistics.

10. It views writing as a disciplined creative activity that can be analyzed and described; its practitioners believe that writing can be taught.

11. It is based on linguistic research and research into the composing process.

12. It stresses the principle that writing teachers should be people who write. (Hairston, "Winds" 86)

Without further elaboration on the twelve features of the new process paradigm, Hairston moves to recent events that she suggests will speed the new paradigm's adoption by the profession and from thence to her conclusion. First, she notes efforts by the University of Iowa's Writing Institute to train freshman composition directors as encouraging the spread of the new paradigm. Second, Hairston points out the increase in

graduate programs in rhetoric, programs that allow students to specialize in composition instead of literature, long the mainstay of most English Department graduate programs. Third, she calls attention to increased in-service training for graduate assistants, the group most often tasked with responsibility for teaching writing courses; the assistants are receiving more training in process-centered approaches to teaching writing. Fourth, process-oriented textbooks on composition have appeared. Finally, and perhaps most importantly, Hairston notes that "college composition textbooks are changing. . . . The publishers sense change in the wind" ("Winds" 87).

A change in the wind does not mean that the direction of change will be sustained, as Hairston is quick to point out in her concluding arguments. She writes:

> But no revolution brings the millennium nor a guarantee of salvation, and we must remember that the new paradigm is sketchy and leaves many problems about the teaching of writing unresolved. As Kuhn points out, new paradigms are apt to be crude, and they seldom possess all the capabilities of their predecessors. So it is important for us to preserve the best parts of earlier methods for teaching writing: the concern for style and the preservation of high standards for the written product. I believe we also need to continue giving students models of excellence to imitate. (Hairston, "Winds" 88)

Hairston finishes by again relying on Kuhn to provide the essence of her argument:

> Kuhn contends that "the transition between competing paradigms cannot be made a step at a time, forced by logic. . . . Like the gestalt switch, it must occur all at once (though not necessarily in an instant) or not at all." He says, however, that, "if its supporters are competent, they will improve it [the paradigm], explore

> its possibilities, and show what it would be like to belong to the community guided by it." I see this last opportunity as the challenge to today's community of composition and rhetoric scholars: to refine the new paradigm for teaching composition so that it provides a rewarding, productive, and feasible way of teaching writing for the non-specialists who do most of the composition teaching in our colleges and universities. (Hairston, "Winds" 88)

By closing so strongly with Kuhn, Hairston reinforces the influence his work holds over her entire argument. Although she suggests using Kuhn's theory of paradigm shift as an "analogy" for what is happening in composition studies, she really applies it! She laces and intertwines Kuhn's theory throughout her entire argument for the shift from the current-traditional paradigm to a writing-as-process paradigm. She cites the work of others who have also suggested that Kuhn's theories may have application to composition studies, principally Richard Young and those who refer to his work like Donald Stewart. Kuhn's theories offer a convenient framework for attacking traditional views within composition studies and offering cogent replacements.

It should be of no surprise that Hairston's use of Kuhn in her attack on the current-traditional paradigm provokes subsequent debate in the Counterstatement column of CCC and elsewhere. Her liberal use of Kuhn provides her opponents with fodder to fuel their arguments against her. Questioning the validity of applying Kuhn's ideas to composition studies enables opponents to cast doubt on Hairston's entire argument. Various critics enter the debate by suggesting that she has misinterpreted Kuhn, misapplied his ideas, or misjudged the state of the field. Others, such as North, who condemns Hairston for dismissing writing centers ("Training" 441), simply disagree with some of her conclusions.

Appearing in the same 1982 issue of CCC, Janet Emig's "Inquiry Paradigms and Writing" received less attention than Hairston's landmark article. Emig uses the cloak of Kuhn's ideas to explore the nature of evidence and academic inquiry in composition studies while she addresses the limitations of empirical research or the Scientific Method; simultaneously, she proposes broadening composition research methodologies to include phenomenological research methods such as case study and ethnography. To frame her argument over the nature of evidence and inquiry, Emig relies on the 1970 second edition of Structure.

Emig opens with a number of questions about the nature of evidence; her responses to these questions serve to establish her argument about inquiry in general. She writes: "What in the universe constitutes evidence? And how do we perceive/select, gather/arrange, and then value/judge entities and processes to fulfill our definitions?" Emig declares, "Our responses concerning the nature, organization, and evaluation of evidence reveal our inquiry paradigms, both those we elect to inhabit, and those we may even help to create" (Emig, "Inquiry" 64).

Emig proceeds to define what she means by "inquiry paradigm." She chooses the term "*inquiry* rather than *research* . . . because its connotations are less parochial and more generous" (Emig, "Inquiry" 64). Emig suggests, "The generic term *research* suffers from conceptual synecdoche in that, for many, the part has become mistaken for the whole: the single species of empirical research is treated as the entire genus" ("Inquiry" 64). Writing about her concept of "paradigm," Emig notes: "As to *paradigm*--a term as useful as well as currently modish--I will, like so many others, pluck one definition from Thomas Kuhn and call a paradigm an explanatory matrix. An inquiry paradigm then is the explanatory matrix for any systematic investigation of phenomena" (Emig, "Inquiry" 64). Kuhn, of course, uses "disciplinary matrix" which Emig seems to reinterpret as "explanatory."

Citing general examples, Emig acknowledges that:

> There is not necessarily a one-to-one correspondence between a given academic discipline and a given inquiry paradigm. Within a single academic field or discipline, there can be several . . . inquiry paradigms active and working, as there can be several . . . academic disciplines deploying a single inquiry paradigm. (Emig, "Inquiry" 64)

For example, the field of belletristic studies includes scholars following historical, structuralist, and psychoanalytic inquiry paradigms "with contrasting, even warring, assumptions and methods." Many researchers in the sciences follow what Emig tags as a positivistic inquiry paradigm shared with "most evaluators of research in state, federal, and private funding and granting agencies"; the second example bothers Emig because "it means that believers in a single mode of research guard almost all entries into monies and to influence" (Emig, "Inquiry" 64-65).

After establishing her case for using inquiry paradigms, Emig refines her concept and lists five characteristics.

> To qualify as an inquiry paradigm, an endeavor must be informed, I believe, by 1) a governing gaze; 2) an acknowledged, or at least conscious, set of assumptions, preferably connected with 3) a coherent theory or theories; 4) an allegiance to an explicit or at least a tacit intellectual tradition; and 5) an adequate methodology including an indigenous logic consonant with all of the above. (Emig, "Inquiry" 65)

By governing gaze, Emig means "a steady way of perceiving actuality." She notes: "We see what we elect to see. We have . . . a gaze that is governed--by our expectations, which are in turn governed by our experiences, and what we have decided cognitively to make of them." Emig's emphasis rests on the contention that "most of us have a preferred way of perceiving" and that despite the "idiosyncratic features that mark our intellectual biographies," perhaps no more than three governing gazes exist: positivistic, phenomenological, and transactional/constructionist (Emig, "Inquiry" 65).

Emig moves from an examination of positivistic and phenomenological gazes to the other characteristics she poses for inquiry paradigms--a set of assumptions, theory, intellectual tradition, and methodology. She connects all of these to her criticisms of empirical research by suggesting that the positivistic view dominates inquiry into composition, exerting undue influence over not only the direction and methods used to conduct inquiry, but over decisions about resources, publication, and other support. The dominance she suggests echoes the normal science that Kuhn posits in his paradigm shift theory. Communities of scholars are united by their adherence to particular sets of assumptions, theories, and traditions that seem to offer an explanation of the phenomenon studied as well as limiting the scope of inquiry into that phenomenon.

Ultimately, Emig concludes that the value of making her "elaborate . . . characterization of inquiry paradigms," especially as they pertain to composition, rests in identifying "mature paradigms and fully realized inquiries" to use as benchmarks to evaluate "unexamined" and "impoverished or immature inquiries" ("Inquiry" 73). Further, it allows her at attack the existing positivistic paradigm held by many decision makers. Emig argues:

> [D]espite the avowed national commitment to pluralistic evaluation, there can be no real possibility of contrastive assessment if evaluators literally do not know

> that more than the positivistic paradigm exists; if they have no criteria by which to evaluate a range of paradigms; and if, undemocratically, they do not acknowledge the legitimacy of alternate world views. For what is involved in paradigm construction and inquiry enactment is, I have tried to show, no less than how we choose to perceive the world and how we elect to define what is distinctly human about human life. (Emig, "Inquiry" 73)

Adherence to a single form of inquiry blinds as much as it reveals, especially when the phenomena is decontextualized.

Like most early users of Kuhn, Emig takes the concept of paradigm and builds upon it. Kuhn's definition of paradigm within the context of a disciplinary or explanatory matrix serves as a foundation to construct a larger argument, in Emig's case, one attacking the inadequacy of the positivistic world view expressed through The Scientific Method. Without explicitly identifying herself with the phenomenologist gaze until citing her own use of the case study methodology, she justifies use of the research methodologies of case study and ethnography. Kuhn's work on paradigms provides a convenient framework to characterize and analyze particular world views.

"Levels of inquiry" and "leading paradigms," rather than "inquiry paradigms," play a role in two essays contained in Peter Mosenthal, Lynne Tamor, and Sean A. Walmsley's 1983 collection, Research on Writing: Principles and Methods. In their "Levels of Inquiry in Writing Research," Carl Bereiter and Marlene Scardamalia refer to Emig and Kuhn. Mosenthal's "On Defining Writing and Classroom Writing Competence" places significant reliance on Kuhn in an argument repeatedly charging that composition studies lacks paradigmatic unity. Both essays cite Kuhn or early users of his ideas in composition studies, such as Emig, Cooper and Odell, and Young.

In their discussion of inquiry in writing research, Bereiter and Scardamalia identify six levels of inquiry that they expect will "eventually" intertwine and form "a coherent, integrative program" (20). They predict that some combination of the six levels--reflective, empirical variable testing, text analysis, process description, theory-embedded experimentation, and simulation--will emerge as a "new dominant approach" to composition research (Bereiter and Scardamalia 20).

As yet, Bereiter and Scardamalia believe that "There is no dominant movement but rather a variety of new tendencies stimulated . . . by the collapse of empiricism" (20). This belief leads them to the issue of shifting paradigms in behavioral science and the status of composition research. They suggest:

> Within behavioral science as a whole, a shifting of paradigms seems to be taking place. But writing research is still in a preparadigm stage (Emig, 1978; Kuhn, 1970). There is no consensus about what needs explaining, what kind of inquiry will lead to explanation, or how one would judge whether something was or was not an explanation. Until there is a paradigm, one cannot have much useful methodological or theoretical controversy. And in fact we know of little theoretical controversy within writing research proper. Controversy that does occur is imported from other domains, such as sociology or pedagogy, where paradigms are better developed. (Bereiter and Scardamalia 21)

Bereiter and Scardamalia go to almost painful lengths to insure that their scheme of inquiry levels is not misconstrued as a potential paradigm for composition research. They warn:

> A research paradigm is not something that is proposed and agreed on. It emerges as investigators come to recognize common problems, to discover common ways of talking about them, and to agree on the relevance of certain kinds of data. The Levels of Inquiry scheme is, accordingly, not a paradigm nor even an outline of one. *It is a framework for the kind of interaction that should lead to a paradigm.* (Bereiter and Scardamalia 22)

Bereiter and Scardamalia conclude that their levels of inquiry scheme has some practicality for composition research, especially in its indication of an interdisciplinary approach. They argue strongly for "having experts in different disciplines engaged in coordinated research toward common objectives" (Bereiter and Scardamalia 22).

The 1983 "Levels of Inquiry in Writing Research" raises again the status of composition studies vis-à-vis Kuhn's theory. Is composition studies still preparadigmatic in nature? Bereiter and Scardamalia answer that question with a definite yes. While they seem to accept composition studies as a legitimate field of inquiry, perhaps even as a discipline, they believe that it has not yet acquired any dominant paradigm. Further, they are careful to say that they are not proposing any paradigm of their own; they are only setting out a "framework for . . . interaction" that might help researchers discover a paradigm (Bereiter and Scardamalia 22).

In his own essay, "On Defining Writing and Classroom Writing Competence," Peter Mosenthal, like Bereiter and Scardamalia, cites Kuhn's 1970 second edition of Structure. Mosenthal asserts that research into "how children acquire writing competence in classroom lessons . . . has provided few adequate answers" because the research "has been conducted in the absence of any leading paradigms of writing" (26). He reiterates Kuhn's point that "in the absence of any leading paradigm, all findings

generated by studies are equally true or relevant" (Mosenthal 26), then quotes a supporting passage from Kuhn: "In the absence of a paradigm, all of the facts that could possibly pertain to the development of a given science are likely to seem equally relevant. As a result, early fact gathering is a far more a nearly random activity than one that subsequent scientific development makes familiar" (Qtd in Mosenthal 26).

Mosenthal goes on to state:

> When there are no mutually agreed-upon criteria for establishing descriptive and operational definitions and bridge principles, there are no criteria against which one can test competing hypotheses of writing and writing acquisition (Kuhn, 1970 [and others]); hence, there are no criteria for separating relevant from irrelevant facts. (Mosenthal 27)

Since Mosenthal, in an endnote, defines a "paradigm" as "a mutually agreed on set of criteria that enable one to separate relevant from irrelevant facts," his contention that writing research lacks a "leading paradigm" is significant (Mosenthal 65). Like Bereiter and Scardamalia, Mosenthal is asserting that composition research exists in a preparadigmatic state. The field lacks the necessary criteria to measure or test the adequacy of its facts--its "descriptive definitions . . . assumed to be true or that have been shown to be true by the use of operational definitions and bridge principles" (Mosenthal 27). Further, "because there are no leading paradigms in writing research, there tends to be no consensual basis for selecting procedures and criteria to formulate operational definitions of writing and classroom writing competence." Sounding remarkably like Kuhn describing the preparadigmatic state in the natural sciences, Mosenthal argues that without explicit criteria, "all operational definitions of writing

and classroom writing competence will continue to remain equally viable and the facts based on these definitions will be equally relevant" (29).

Mosenthal sets out to consider the possible criteria "for formulating descriptive and operational definitions . . . and for formulating . . . principles that relate to these . . . definitions" (Mosenthal 27). In the remainder of his essay, he proposes "a full context model of writing and classroom writing competence," discusses the criteria for defining his terms, and outlines his descriptive and operational definitions and bridge principles (Mosenthal 27).

In his proposal and discussion, Mosenthal continually repeats his charge that composition lacks a leading paradigm. He notes that Kuhn has suggested that "paradigms supply criteria for selecting examples and features to be used in formulating descriptive definitions" (Mosenthal 28). The "paradigms one adopts to define a phenomenon" also usually specify the bridge principles one uses to indicate the relationship between features and examples used to describe phenomenon and the procedures and criteria used to operationally define that phenomenon. As a result, Mosenthal admits that "it is difficult, if not impossible, to determine which set of criteria for relating descriptive and operational definitions is the best criteria" (30).

While admitting the difficulty of choosing criteria, Mosenthal sets great importance on the criteria of "good science" as he evaluates the inadequacy of his various definitions and finds fault with his own model. "Good" science serves two functions: "it orders and simplifies the complexity of the world so that states and events can be made interpretable, and . . . it enables one to predict future states and events based on known cause-and-effect relationships" (Mosenthal 34). Mosenthal wants to apply these two functions as criteria for judging the adequacy of various research efforts in composition studies.

According to Mosenthal, the function of simplification is obtained by most researchers through partial specification--that is, they begin "by constructing a partially specified descriptive definition of writing and, in some cases, classroom writing competence" (Mosenthal 35). Using a partially specified descriptive definition has a cascade effect; it means that the bridge principles and the operational definitions will also be partially specified. Partial specification allows researchers to restrict the context and variables influencing the writer and limit research "to a particular type of writing in terms of meaning source and writing process" (Mosenthal 37).

The point Mosenthal emphasizes about partial specification rests in repeating his initial charge:

> An important function of paradigms is to provide established guidelines for conducting partial specification to formulate definitions of some concept. Given that there are no well established paradigms in writing research, one is faced with the problem of identifying criteria for selecting different partially specified definitions of writing and classroom writing competence. (Mosenthal 38)

Mosenthal appeals "to different ideologies" as the solution for identifying such criteria (Mosenthal 38). He divides the different ideologies into academic, utilitarian, romantic, cognitive development, and emancipatory, specifying the unique influence that each exerts over the partial specification of definitions and the inherent weaknesses of each.

The absence of paradigms forces researchers to address two problems from Mosenthal's perspective: first, "deciding which ideology provides the best framework for defining writing and classroom writing competence," and second, after selecting an ideology, "determining which features and examples, procedures and criteria, and . . .

principles provide the best representation for understanding these terms relative to the assumptions of one's selected ideology" (Mosenthal 50-51). One potential solution lies in appealing to an acknowledged "authority in writing research," someone who has come "to be regarded as the authority . . . of how these terms should be defined" (Mosenthal 51). Mosenthal comes very close to describing the concept of normal science when he asserts that accepting the mantle of discipleship means accepting the definitions of the authority without challenge and attempting to confirm and extend those definitions and assumptions. Mosenthal declares:

> In time, enough disciple researchers will have abided by different authorities' assumptions so that distinct paradigms of writing research will have emerged. These paradigms may become dissociated with the authority and may become known only by the name of an authority's original theory, for example, behaviorism or cognitivism, or the paradigm may become synonymous with the name of the authority, for example . . . Piagetism. This is the way most paradigms are formed. (Mosenthal 51)

Mosenthal's essay represents one form of a number of essays and articles that begin appearing in number in the eighties. These works are riddled with references to paradigms, sometimes redefining Kuhn's term slightly, and occasionally even citing Kuhn directly. More often, the works appeal to others' interpretations of Kuhn, especially Young, Hairston, and Emig, and, to a much lesser extent, Kinneavy.

Another work appearing in the early eighties, Stephen P. Witte and Lester Faigley's Evaluating College Writing Programs, also mentions Kuhn's ideas, though indirectly. Witte and Faigley examine the state of college writing programs, evaluating four in particular, with the intent of establishing a framework for evaluation. In outlining a

useful framework for evaluating college writing programs, Witte and Faigley "are intentionally pluralistic and eclectic, drawing on many sources," including some that depend on Thomas Kuhn (4).

Witte and Faigley desire to construct a framework that addresses two primary concerns: first, the framework must specify the necessary components of evaluation, and second, "it must reflect the interactions between those components." The authors go on to list and elaborate the five "major components of writing programs" (Witte and Faigley 39): cultural and social context, institutional context, program structure and administration, content or curriculum, and instruction. They conclude that "effects in writing programs . . . always result from interactions among the five components," differing in when and where they occur as well as affecting the variety of data available to analyze (Witte and Faigley 41). They note that the evaluator "usually assumes that the first four components of writing program evaluation . . . will remain relatively stable" (Witte and Faigley 41), but the fifth, instruction, is dynamic, constantly changing.

Witte and Faigley's first mention of ideas derived from Kuhn occurs during their discussion of the content or curriculum component. They note that "virtually everything ever said about *what* to teach in writing courses falls under the general rubric of curriculum. Subsumed under this general rubric is considerable diversity, much of which reflects a more-or-less traditional paradigm" (Witte and Faigley 48). In an accompanying endnote, the authors cite Young's 1978 "Paradigms and Problems: Needed Research in Rhetorical Invention), Stewart's 1978 "Composition Textbooks and the Assault on Tradition," Berlin and Inkster's 1980 "Current-Traditional Rhetoric: Paradigm and Practice," and Hairston's 1982 "The Winds of Change: Thomas Kuhn and the Revolution in the Teaching of Writing." Witte and Faigley speculate in that note that:

> [T]he traditional paradigm is not likely to change either substantially or quickly unless and until composition teachers accord textbooks considerably less status than they presently have in the teaching of writing. Textbooks, to be sure, are useful for teaching content in the sense of *what*, but they are of considerably less value in teaching content in the sense of *how*. The reason for this, of course, is that they can only address *how* by converting it to *what*. (Witte and Faigley 92)

Witte and Faigley's language, as well as their attention to other users of Kuhn's ideas, suggests their acceptance of the idea of the current-traditional paradigm. Simultaneously, their comments about textbooks echo Kuhn's analysis of textbook use in the natural sciences.

Continuing in the text of Evaluating College Writing Programs, Witte and Faigley address issues concerning validity in their fourth chapter. They note: "Validity depends upon the appropriateness of the evaluation to the nature of the thing evaluated," and they rely on another's use of Kuhn to explore "the development of and the need for two distinctive approaches to evaluation"--quantitative and qualitative (Witte and Faigley 67). Witte and Faigley write: "Quantitative and qualitative methods embody fundamentally different theories about both the nature of reality and about the nature of knowledge and knowledge acquisitions." They contrast "the two often competing paradigms" using a quotation from Michael Q. Patton, a leading authority on evaluation (Witte and Faigley 69). Patton, partially quoted here, writes:

> Evaluation research is dominated by the largely unquestioned, natural science paradigm of hypothetico-deductive methodology. This dominant paradigm assumes quantitative measurement, experimental design, and multivariate, parametric statistical analysis to be the epitome of "good" science. . . .

> By way of contrast, the alternative to the dominant . . . paradigm is derived from the tradition of anthropological field studies. Using the techniques of open-ended interviewing and personal observation, the alternative paradigm relies on qualitative data, holistic analysis, and detailed description derived from close contact with the targets of study. The . . . natural science paradigm aims at the prediction of social phenomena; the holistic-inductive, anthropological paradigm aims at understanding of social phenomena. (Qtd in Witte and Faigley 69-70)

Witte and Faigley reflect that the central question becomes one of "whether evaluation research ought to represent itself through its methodology as a hard science after the manner of chemistry or physics or as something closer to research in the humanities or the social sciences" (70). Patton suggests that the trend is toward using multiple methodologies, arguing: 'The debate and competition between paradigms is being replaced by a new paradigm--*a paradigm of choices*. . . . [which] recognizes that different methods are appropriate for different situations" (Qtd in Witte and Faigley 70). According to the authors, "Patton's notion of a 'paradigm of choice' is useful because the complexity of writing programs . . . precludes the use of a single approach to evaluation." The "emergence" of a paradigm of choices increases the options available to evaluators although "it does not eliminate the essential and fundamental differences in world view that the choices represent" (Witte and Faigley 74).

Finally, Witte and Faigley argue that analysis of the interaction of the five components they identify and use of multiple methodologies may present the best approach to developing the questions necessary to evaluate the complex nature of college writing programs. They conclude that such programs are dynamic, ever changing "in response to society's demands for a literate populace and in response to an ever-growing

body of knowledge about both the products and processes of writing" (Witte and Faigley 75).

Although Witte and Faigley make mention of Kuhn's terminology, they stop short of establishing a paradigm of their own, preferring instead to suggest the heuristic nature of the laundry lists of questions they offer throughout their analysis of the five components of college writing programs. As indicated by their endnote, they seem to accept the notion of the current-traditional paradigm (Witte and Faigley 92), thus they can be seen in one light as suggesting that composition studies has reached the status of discipline in Kuhn's sense. Their acceptance of Patton's work on evaluation--borrowed from outside of composition studies--certainly lends tacit approval to the concept of paradigms and competition.

Published for CCCC, Witte and Faigley's effort also represents an opportunity to articulate a number of concerns about the state of the discipline of composition studies as a whole. As Lee Odell points out in his Foreword to Witte and Faigley's work, "To establish beyond question the authenticity of composition as a discipline, we must have some agreement about theory, methodology, and the kinds of questions worth asking" ("Foreword" x). Writing for the CCCC Publication Committee of Studies in Writing and Rhetoric, Odell hopes that the "Witte and Faigley text . . . will help us work toward that agreement and, ultimately, toward the establishment of composition as a discipline" ("Foreword" xi). Odell's words may express hope, but they still seem to indicate that he has some reservations about the legitimacy of composition studies.

If we conceive Odell's call for "agreement about theory, methodology, and the kinds of questions worth asking" as establishing a paradigm for composition studies, then the issue of disciplinary legitimacy remains linked to Kuhnian ideas about the maturity of a science, a position that Robert J. Connors disagrees with completely. Connors's warning against accepting the mantle of science for composition studies appears in the January

1983 issue of CE. In "Composition Studies and Science," Connors argues vehemently that: "We are not a science and will not be one in the foreseeable future, and we must beware lest our understandable desire to share in the cachet of science lead us to a barren enactment of imitation science" ("Science" 19).

How does Connors arrive at such a position? By finding fault with the principal early appropriations of Kuhn made by Gage, Young, Emig, Hairston, Bizzell, and others, Connors counters the "tacit" claim those authors make for considering "composition studies as a truly scientific or prescientific discipline" ("Science" 6). He offers a brief summary of Kuhn's "metatheory" and the disciplinary response to it in the history and philosophy of science before launching his assault on appropriations by composition studies.

Connors suggests that compositionists have been interested in borrowing only parts of Kuhn's theory--"the details about the way that scientific paradigms shift" and "Kuhn's conception of 'pre-paradigmatic fields'" ("Science" 3-4). Labeling borrowers of Kuhn in composition "non-science scholars," Connors suggests that their primary reason for importing Kuhn lies in their "yearning toward the power and success of the natural sciences." He notes that Kuhn's explanation of progress in the natural sciences has "inflamed" many in composition as well as other fields, thus he feels the need "to examine the use that composition scholars have made of [Structure]" ("Science" 4).

In addition to noting the temptation to freely borrow Kuhn's terminology, Connors argues that compositionists have used Kuhn in two primary ways divided by their assessment of the state of composition studies. Some, such as Bizzell, Young, and Hairston, view composition as in the midst of a paradigm shift. This view suggests that composition studies already possesses a paradigm; therefore composition studies exists as a mature field. In the second view, composition inquiry has not yet developed enough to claim the status of a mature, paradigm-driven discipline. Under the second view,

scholars such as Gage, Kinneavy, and Emig argue that composition studies is "preparadigmatic" or "emerging." What unites these two views is the belief, wrong in Connors's eyes, that "composition studies can attain a Kuhnian scientific paradigm--either its first or one more desirable than its present one" ("Science" 5).

Connors questions the attainment of that Kuhnian paradigm and the corresponding claim of science for composition studies. Noting that "Kuhnian analogies have recently gained so much popularity that they have become part of the tacit knowledge" of composition studies, he argues that describing composition inquiry in Kuhnian terms does not mean such inquiry meets the "gate criteria" for determining if a discipline shares "the nature of the developed sciences" (Connors, "Science" 6). Using a list he derives from Kuhn's Structure, Connors perceives composition studies fails to measure up as a science when compared against the five general criteria that seem to mark scientific inquiry: establishing points of disagreement over aims, metaphysics and empirical methods, testing, experimentation, and cumulative progress

First, he rejects any claim to science, arguing "there is no manner in which we can claim that composition pedagogy per se has any scientific nature at all or that a 'scientific revolution' can take place in the teaching of writing." Composition's concern for the teaching of writing does not share the "scientific aims of description, explanation, and prediction." Connors concludes that "Scientific research and the aims of teaching are simply not commensurable," thus placing most of what we do in composition studies and most practitioners outside the pale of practicing a "science." Such a conclusion means that we have to examine composition research, that is, look at "the non-teaching activities of composition scholars if we are to find serious defenses for the application of Kuhnian terms" ("Science" 7).

Connors declares that only one category of composition research --experimental and descriptive studies--"seems to have the best claim to scientific status." These studies

share many of the same aims as science, fitting most of the gate criteria. Still, Connors suggests that the relative youth of such research, especially that of the last fifteen years, hardly qualifies it to claim to "have evolved an explanatory structure to guide further research" or suggest "any obvious cumulation." Connors writes emphatically: "We have not yet been able, in Kuhn's terms, to transform the problems that the discipline faces into normal-science puzzles" ("Science" 8).

Is "the field of composition studies," Connors then poses, "a rigorous scientific discipline in the midst of a paradigm shift" as scholars such as Young, Bizzell, and Hairston have proposed? For Connors, "The answer is pretty obviously no." Connors appropriates Kuhn at this point to argue that composition possesses no "extant paradigm" in the Kuhnian sense. Even "the empirically-oriented areas" of composition studies, Connors suggests, still seem to treat all facts as equally relevant ("Science" 8-9).

Taking such a position on Kuhn's concept of paradigm allows Connors to further undermine attempts by Zoellner, Young, Bizzell, and Hairston to claim an extant paradigm for composition inquiry. These scholars neglect Kuhn's second sense of paradigm, exemplars, concentrating instead on the "disciplinary matrices" all fields possess--their "commitments to beliefs in particular models, values, techniques, that are shared by all members of a disciplinary community." Connors readily admits that "composition studies has such 'metaphysical paradigms,' as do" other fields, but asserts that the lack of "exemplars" argues against any consideration of composition studies as a science ("Science" 9).

Connors relates the absence of exemplars to the lack of agreement "on what are to be the methodology and conditions for puzzle solving within our field." He cites disagreements among leading composition researchers such as Janet Emig and Nancy Sommers over composition's lack of "a clearly articulated theoretical base" as evidence of his point, indicating that our continuing interest in pedagogy and classroom practice

may be at fault ("Science" 9). Referring to Sommers's 1979 "The Need for Theory in Composition Research," Connors interprets Sommers's main point as: "practitioners of composition research cannot engage in true Kuhnian normal-science puzzle-solving because we have not the framework to determine which problems may be soluble puzzles, which remain insoluble problems, and which have no meaning" (Connors, "Science" 9-10). Sommers never mentions Kuhn nor uses his terminology, but her observations taken in combination with remarks in Kuhn's second edition of Structure suggest to Connors the inability of composition studies to identify the puzzles worth solving. Connors notes that composition studies "can offer problems in plenty, but it cannot offer the puzzles that are the central components of Kuhnian normal science" ("Science" 10).

Having dealt with the perspective of considering composition studies as paradigm driven, Connors turns to criticism of those who view composition studies as "a field in a preparadigmatic state, a proto-science of a sort waiting for its first genuine exemplars." He suggests that the efforts of Gage, Kinneavy, and Emig in claiming a preparadigmatic status for composition are "more persuasive. . . . since we cannot predict the future and for all we know a complete composition-studies paradigm may emerge tomorrow from completely unsuspected sources." Taking the concept of "proto-science" from Kuhn's "Reflections" essay, Connors argues that composition has more in common with the social-scientific fields than the natural sciences, thus we must view composition inquiry from its relationship with the social sciences, beginning with our borrowings from them ("Science" 10).

According to Connors, we have borrowed "all of the genuinely scientific apparatus of composition studies"--apparatus such as empiricism, controlled research, and experimental methodology--"from a branch of educational research that in turn has derived its methods from those of clinical psychology" ("Science" 10). By reviewing our

debt to interdisciplinary borrowing, Connors means to suggest that to determine if "the most rigorous end" of composition studies, research, shows "evidence of being a preparadigmatic science, we must look more closely into the nature of cumulation and certainty in the social sciences, especially cognitive and behavioral psychology," which have informed much of the research into writing practices ("Science" 10-11). Connors chooses to review psychology as a precursor to his continued examination of the claims for preparadigmatic status.

According to Connors, psychology has less readily adopted Kuhnian ideas of scientific maturity and made "far stronger logical cases . . . for the non-scientific status of psychology than for its identity as science" ("Science" 11). In his view, theoretical and cognitive psychologists continue to have trouble with the tenets of the experimental methodology borrowed from the physcial sciences because of the complexity of human mind and the variability of human behavior; human complexity and variation makes explanation and prediction difficult. He dismisses behavioral psychology as well, rejecting it as "theoretically sterile" ("Science" 16). Such observations lead Connors to conclude:

> Methodologically rigorous, carefully controlled, and technically advanced though some aspects of psychologically-based research into composition may be, they do not make composition studies a mature scientific field with a paradigm of its own, and they do not even show conclusively that it is a preparadigmatic field--at least not one whose first paradigm is anywhere in sight. (Connors, "Science" 17)

Ultimately, Connors believes that his assessment "of the roots and branches" of psychologically-based composition research shows "that Kuhn's terms, applied analogically as a claim for the essentially scientific or prescientific nature of the

discipline leads us only to bind alleys or to unrealistic expectations" ("Science" 17). He suggests that while Kuhn directed his theory against "the logical-positivist theory of science as constantly accumulating neutral knowledge, in practice Kuhnian disciples in non-scientific fields have tended to *promote* positivism within their own disciplines" (Connors, "Science" 18). Thus Connors warns against "scientism," the continued push to place "methodology at the heart of rhetorical education and [tilt] composition studies toward the sort of mechanistic concerns with neutral 'techniques' that we wish in our best moments to transcend." He declares that composition scholars "should not in our search for provable knowledge forget that the essential use of all knowledge is in aiding humanity in the search for consensually-arrived-at truth" (Connors, "Science" 19).

In rejecting science, Connors also makes an even more expansive claim for discourse. He suggests that the "universe of discourse is larger than the universe of science." No matter how attractive or "seductive" Kuhn's normal science may seem, rhetoricians have the task of trying "to solve problems and not puzzles" (Connors, "Science" 20). While not dismissing the value of empirical research, Connors cautions that composition studies should remain "humanistic" and avoid imitating science.

Connors uses his review of composition's "roots" in psychologically-based research as an ax to sever any claims that composition studies might have to status as a science, "human" or otherwise. Although he values empirical research, he discards Kuhn's theory as an unhelpful way of viewing composition studies. To reject a view of composition studies as a scientific or proto-scientific discipline translates into demonstrating that compositionists have incorrectly applied Kuhn's ideas. In Connors's view, we neither possess a paradigm nor come anywhere near to establishing one; therefore compositionists such as Kinneavy, Young, Bizzell, and Hairston wrongly introduce Kuhn into composition studies. They may desire to establish composition inquiry as academically valuable, but claiming the status of science ultimately fails to do so.

Connors's attack on appropriating Kuhn also represents one of the few occasions where a compositionist reviews and incorporates the disciplinary criticism of Kuhn in an analysis of his influence outside of the philosophy of science. Relying primarily upon the criticism in Shapere's reviews of Kuhn and the essays in Lakatos and Musgrave's Criticism and the Growth of Knowledge (covered in chapter six of this dissertation), Connors asserts that "Kuhn's work has been largely rejected or bypassed" in the philosophy of science ("Science" 2). In a long footnote, Connors explores the disciplinary responses of critics such as Dudley Shapere, Sir Karl Popper, and Stephen Toulmin. What Connors sees as Kuhn's rejection by his own discipline becomes one of Connors's tacit arguments for not applying Kuhn to composition studies. Unfortunately, Connors's analysis of the disciplinary response of the philosophy of science does not go far enough and remains one-sided. He virtually ignores Kuhn's influence on the history of science and on science studies.

Following Connors, Richard Fulkerson also reviews another's borrowing of Kuhn. Fulkerson's "Kinneavy on Referential and Persuasive Discourse: A Critique" appears in the February 1984 issue of CCC. In "Critique," Fulkerson recounts Kinneavy's reliance on Kuhn's theory and frames Kinneavy's own arguments using the Kuhnian terminology of "paradigm" in its sense of "model." Fulkerson notes Kinneavy's proposal in the 1971 A Theory of Discourse of a "four-part paradigm" dividing all discourse into four types including referential and persuasive. He differentiates between Kinneavy's theory--"'the aim of a discourse determines everything else,' specifically the discourse's structure, its logic, and its style"--and the paradigm. Fulkerson writes: "To the extent that the theory is sound, Kinneavy has given us a powerful taxonomical, pedagogical, and interpretive tool." But Fulkerson goes on to state his dissatisfaction and the main point of his review, maintaining "that Kinneavy fails to provide compelling support for the

theory and that a number of his [Kinneavy's] specific assertions fail to hold up under examination" ("Critique" 43).

Much of Fulkerson's dissatisfaction grows out of the failure of Kinneavy to demonstrate empirically that "a given discourse aim leads to certain traits." He accuses Kinneavy of "merely" deducing "from the work of others" rather than providing sufficient evidence in the form of more discourse analysis ("Critique" 44). The examples Kinneavy uses only illustrate, not demonstrate his theory according to Fulkerson.

After making such general criticisms, Fulkerson pursues "some serious weaknesses" in Kinneavy's work, especially in Kinneavy's discussion of the three types of reference discourse: exploratory, informative, and scientific ("Critique" 45). He points out evidence of Kuhn's influence upon Kinneavy in a discussion of exploratory discourse.

> For him [Kinneavy], exploration is a sort of discourse that reflects the breakdown of some accepted view and the attempt through discourse to find a new way of viewing reality. The presentation is heavily indebted to Kuhn's discussion of the nature of scientific revolutions. In structure, such discourse *may* follow the chronological cycle of (1) acceptance of a traditional paradigm, (2) awareness of dissonance between observed reality and the paradigm, and (3) attempts to use a new model with a better fit to the observed reality. But then again, it may not. (Fulkerson, "Critique" 45)

Fulkerson concludes that, "For Kinneavy, exploratory discourse must actually engage in the process of discovering the new paradigm; it is not a recounting of such a discovery after the fact." While Fulkerson finds Kinneavy's description of exploratory discourse

"interesting and theoretically consistent," he argues that Kinneavy's version "has limited applicability for composition teachers or critics of discourse" ("Critique" 46).

Fulkerson pounds away at Kinneavy's conception of informative and scientific discourse, charting empirical examples that offer refutation, or that at least contradict Kinneavy's classification scheme. He notes that Kinneavy's work is useful "for the questions it raises" and more applicable "for rhetorical critics than for teachers" ("Critique" 52). Ultimately, Fulkerson declares that "Kinneavy has provided a complex set of hypotheses to be tested, used, and modified through rhetorical criticism," rather than "a finished taxonomy" of discourse types ("Critique" 54). Fulkerson concludes:

> Even though the theoretical description of the structure, logic, and style of each type of discourse is highly questionable, the emphasis on purpose and the intuitive paradigm of possible purposes for discourse can serve to make us better teachers of discourse (even if we do not try to build curriculum around either paradigm or theory). . . . Yet even though Kinneavy's paradigm has the distinct virtue of providing a governing teleological perspective on writing, a perspective which makes sense intuitively, his theoretical descriptions of the individual types are frequently unsatisfactory when compared to the reality of specific pieces of discourse. (Fulkerson, "Critique" 54)

In terms of a borrowing of Kuhn, Fulkerson's reexamination of Kinneavy's theory more than a decade after the publication of A Theory of Discourse seems to reflect, at a minimum, an acceptance of Kuhnian terminology. Fulkerson equivocates on the nature of exploratory discourse as following the cyclic nature of Kuhn's scientific revolutions, but he does not rule out the possibility. He simply notes that most readers would define exploratory discourse differently, tying it more to personal writing or rhetorical

invention. Fulkerson attacks Kinneavy's theory on the basis of the inadequacy of its examples and lack of evidence; he does not do so from any view of disagreement with those aspects of the theory borrowed from Kuhn.

In Fulkerson's "Critique," we see the seeming acceptance of one theorist's view of Kuhn in composition. Fulkerson does not fault the borrowing, nor gainsay it. He uses the terminology of the borrowing --that is, "paradigm"--in an exploration of the framework behind Kinneavy's theory.

Unlike Fulkerson's essay, "Measuring teachers' attitudes toward writing instruction" appears in Lillian S. Bridwell and Richard Beach's 1984 collection, New directions in composition. The authors of the essay, Anne R. Gere, Brian F. Schuessler, and Robert D. Abbot, address "the paucity of knowledge" found in research on teacher effectiveness. They report the results of survey research they conducted into teacher effectiveness, then turn to Barry M. Kroll's 1980 CE article, "Developmental Perspectives and the Teaching of Composition," in the discussion of their analysis. Dividing their target population into two groups based upon survey responses, they conclude that the attitudes of the two match two "paradigms" in composition instruction that Kroll identifies as "nature" and "nurture." Gere, et al write:

> According to Kroll, two competing theories of human development--"nature" and "nurture"--underlie most composition instruction. Interventionists, following the nurturing principle that environment provides the essential source of development, emphasize the content and agent of instruction, text-centered evaluation, comprehensive error marking, and tightly controlled assignments. . . .
>
> Maturationists, on the other hand, follow the "nature" principle that organisms contain the seeds of their own development. They emphasize personal

> writing. . . . conceptualize writing as artistic expression, focus on the process of composing, and try to enable students to experience that process rather than to impart specific knowledge or skills as the interventionists do. For the maturationist, evaluation is context-centered. (Gere, Schuessler, and Abbot 354-56)

Gere and her colleagues extract Kroll's conclusions and apply them to differentiate between the nature and nurture "paradigms" they see in their own data. Their effort is empirically-based and dependent upon statistical analysis and interpretation of the results. It reflects the drive to use empirically scientific methodologies to validate a correlation between composition theory and subgroups of survey items identified by statistical evaluation. Gere, Schuessler, and Abbott conclude that such surveys "can contribute to the development of theories about the relationship between teacher attributes and student achievement" (356).

Gere, Schuessler, and Abbot's survey study suggests the acceptance of Kuhn's concept of paradigm. They refer to the work of another compositionist, arguing that their findings provide evidence of that compositionist's theory. Kroll, in turn, draws upon Young's interpretation of Kuhn. Gere and her colleagues do not attempt to define paradigms or suggest Kuhn's work in any way, but their use of Kroll reveals their exposure to Kuhn's ideas.

If Gere, Schuessler, and Abbot's work suggests acceptance of Kuhn's ideas, Thomas E. Blom's response to Maxine Hairston's 1982 "The Winds of Change" essay indicates the dissenters' view. Both Blom's response and Hairston's reply to it appear in the December 1984 Counterstatement column of CCC. Blom launches a polemic against "a few process advocates who use the pages of scholarly journals and the apparatus of scholarship for 'unscholarly'--one might say 'oracular'--ends" (Blom 490). He rails against Hairston

in particular. Blom's criticisms and Hairston's reply illustrate the depth of the debate over importing Kuhn into composition studies.

Blom singles out Hairston for misapplying Kuhn and jeopardizing "the real and positive values of the very pedagogy [she seeks] to promote" (Blom 490). He notes:

> Hairston proposes that our profession is in the throes of a revolutionary paradigm shift, but in so doing, she resorts to the oracular potentiality of an antiquated archetype--the dynamics, structure, and imagery of which impede serious inquiry into the art of teaching.
>
> The archetype uses reductive polarization to stimulate feeling and thereby depress analytical thinking. The principle of "revolution" is the source of the polarity in Hairston's essay: she generates its persuasive force by insisting on the diametrical opposition of what she calls "outmoded" product- and "new" process-centered theories of composition. (Blom 490)

Blom accuses Hairston of using "the lens of Thomas Kuhn's theory of scientific revolution" to create "an aura of scholarly authenticity," one that fails to account for Kuhn's contention about the dissimilarity between hard science and social science (490). Blom cites Kuhn's own words from the 1962 Preface, reprinted in the 1970 second edition, as proof of the dissimilarity between the two groups of fields.

> Somehow, the practice of astronomy, physics, chemistry, or biology normally fails to evoke the controversies over fundamentals that today often seem endemic among, say, psychologists or sociologists. Attempting to discover the source of that difference led me to recognize the role in scientific research of what I have since called "paradigms." These I take to be universally recognized scientific

> achievements that for a time provide model problems and solutions to a community of practitioners. (Qtd in Blom 490)

Blom calls the dissimilarity substantive and further attacks Hairston for spinning "words to steer clear of Kuhn's significant distinction." While acknowledging that Hairston admits "that Kuhn 'did not even claim or suggest that his model for scientific revolution could or should apply to social sciences,'" Blom argues "Kuhn obviously believes that his model *cannot* be applied to fields other than hard science." Blom goes on to assert that Hairston has misapplied as well as misinterpreted Kuhn's work, creating discrepancies in her "analogy." If the discrepancies render Hairston's analogy untenable, "then her argument must be invalid" (Blom 490).

Blom offers a brief counter proposal for interpreting progress in composition pedagogy:

> Suppose that progress in pedagogy takes not the form of revolutionary paradigm shifts between dichotomous alternatives, but rather the form of complex, ongoing Hegelian dialectics in which manifold theses and antitheses contained within mutually inclusive systems of thought continuously seek resolution. If such is the case, then Hairston's reductive view distorts the multi-dimensional activity it claims to clarify, and thereby impedes, rather than spurs, understanding and progress. (Blom 490-91)

In other words, Blom adamantly claims that Hairston is wrong to apply Kuhnian theory to describe or promote changes in composition studies.

Going even further, Blom charges Hairston with attempting to "impose dubious cognitive imperatives on the mass mind" and suggesting that she is unscrupulous, unable

to "resist the temptation to sway others or be . . . swayed by the persuasive stratagems of soapbox, pulpit, and billboard" (Blom 491). Noting that Hairston succumbs to the temptation, Blom writes with increasing scorn:

> [H]er essay is a manifesto: it couples reductive polarization with an oracular stance. Unlike Kuhn, the historian of science, Hairston dons the robes of a paradigmatic seer: she writes in the prophetic mode: her style is apocalyptic, coercive. . . . Finally, and most significantly, her essay smacks of sermon literature. Like an evangelical preacher, Hairston's plays . . . upon an absolute dichotomy between process- and product-oriented teachers: the saved and the damned. (Blom 491)

Increasingly virulent in tone, Blom denounces Hairston for seeking to force the unconverted to accept the new paradigm of process. He attacks her for portraying "product-oriented teachers as dehumanized human beings so staggeringly incompetent and ineffectual as to constitute a national scandal" and for implying "that colleagues realize their potential as humans and as teachers when they adopt a process-centered approach to composition." Blom finds Hairston's description of college writing teachers "morally, ethically, and professionally objectionable" (Blom 491), and he accuses her of using it as "a deplorable rhetorical device" to enable her "to distract her readers--and perhaps even herself--from her inability to produce credible evidence demonstrating either the failure of the pedagogy she scorns or the success of the one she applauds" (Blom 492). In short, Blom labels Hairston's scholarship as "unsound" and distorted.

As evidence of scholarly distortion, Blom argues that Hairston has misinterpreted Mina Shaughnessy as well as Kuhn. Blom censures Hairston for putting words into

Shaughnessy's mouth, noting that Shaughnessy never asks the questions that Hairston attributes to her nor views "her field in terms of paradigmatic conflict" (Blom 492). Blom continues:

> However much Hairston may wish to enhance the stature of her argument by forging an alliance with Mina Shaughnessy, we must consider the price at which such enhancement is purchased: Hairston's misrepresentation interferes with the accurate comprehension, assimilation, and transmission of Shaughnessy's valuable, pioneer work. (Blom 493)

In his final criticisms of Hairston, Blom holds her up as an example to avoid. He stridently declares:

> Unlike hard scientists, who weigh, measure, and test to demonstrate the validity of their conclusions, social scientists seek consensual validation. The latter must accordingly resist not only the temptation to invent, distort, and conceal facts, but also the temptation to practice rhetorical sleight-of-hand, neglect common sense, and abuse the rules of responsible scholarship.
>
> Accordingly, we may not ask Hairston for *hard* evidence that an "outmoded" paradigm causes student writers to fail or that a "new" paradigm causes them to succeed. But we may surely ask Hairston to present facts accurately and to avoid dubious arguments and rhetorical strategies which have no place in the pages of influential professional journals. Our colleagues and our students deserve better from us than a pedagogy based on bogus archetypes, buzzwords, sophistry, and fervor. (Blom 493)

Blom's agenda throughout his counterstatement seems to rest not only on dismissing Hairston's use of Kuhn, but on completely discrediting her as a scholar and accusing her of practicing sophistry. He dismisses the application of Kuhn, writing that even Kuhn does not suggest applying his theory to the social sciences. He sees in other parts of her article a blatant attempt to preach revolution and subvert others to the writing-as-process movement. Blom rails against her for failing to provide hard evidence and misinterpreting or twisting what evidence she does cite. The "lens of Thomas Kuhn's theory of scientific revolution" may provide Hairston with an "analogy" for what is happening in composition, but Blom denounces it as completely inaccurate and inapplicable.

Hairston, in her brief two page reply to Blom's denunciation, chooses "not to attempt to refute Mr. Blom's accusations about [her] view of the profession" ("Reply" 493) nor deny that she is "a zealot in the cause of teaching writing as process" ("Reply" 494). She states simply: "Obviously he and I see a different reality" (Hairston, "Reply" 493). Unapologetically, she adds that she does preach revolution and subversion to the writing-as-process movement. But Hairston rejects outright Blom's contention that "Thomas Kuhn's theories cannot legitimately be used to help us understand change within fields other than science." She notes that "many writers in many fields use Kuhn's concept of the 'paradigm shift' as an analogy to explain new trends that they perceive in their disciplines." She cites other authors who, like her, have "found Kuhn's theory rich, thought-provoking, and exciting" and have "adapted his ideas to their own fields." If she has any apologies to make, Hairston remarks that she should have cited Robert Zoellner, another author who used Kuhn in an early article and was an early champion of the process movement ("Reply" 494).

Blom's counterstatement perhaps represents the most toxic response to the application of Kuhn's ideas to composition studies. He denounces it as unsound

scholarship and accuses Hairston of misapplying as well as misinterpreting Kuhn. By refusing to dignify Blom's accusations with more than a brief refutation, Hairston lets her article speak for itself and allows others to judge its merit. If others have seen some application of Kuhn to their respective fields, why not apply Kuhnian theory to composition studies? Hairston certainly will not win over someone as acerbic as Blom, but if she can use Kuhn's theory to provoke thought, excite debate, and stimulate discussion, especially as it relates to promoting the process paradigm over the current-traditional paradigm, she will continue to preach "revolution and subversion" ("Reply" 494).

Jon Harned uses Kuhn's "paradigm" in its sense of "exemplar" when he considers Alexander Bain's 1866 English Composition and Rhetoric. Harned explores the intellectual milieu within which Bain crafted his theory, focusing on theory's underpinnings in associationist psychology and tying its prestige to changes occurring in the American system of higher education. In the opening paragraph of "The Intellectual Background of Alexander Bain's 'Modes of Discourse,'" Harned declares:

> Bain's textbook brought about what we now, thanks to Thomas Kuhn, refer to as a paradigm shift, sweeping away these belletristic schemes [the established literary forms such as orations, history, romance, treatises, sermons, and the like] and substituting five forms--Description, Narration, Exposition, Persuasion, and Poetry--that, with the exception of Poetry, have survived up to the present in Freshman Composition and are known in the trade as the Modes of Discourse. (Harned 42)

Harned suggests the acceptance of Bain's work and the popularity of the modes led to a shift away from rhetoric in the latter half of the nineteenth century. He notes that "Bain

is now often held responsible for the impoverishment of rhetoric" (Harned 42), confirming indirectly that Bain's ideas damaged rhetoric as discipline. Simultaneously, Harned talks about "another paradigm shift" currently taking place, although he leaves identity of that shift in doubt (Harned 42). He is not interested in examining the ongoing shift; he concentrates on elucidating the theoretical background of Bain's work instead.

Harned limits his use of Kuhn to the opening paragraph of his essay. From there, Kuhn disappears, never to return in Harned's discussion. By using Kuhn in this way, Harned may simply pick up on the fashion of the times. He seems to accept the viability of paradigms and paradigm shifts without question. He accords Bain's textbook the status of exemplar, yet never explains Kuhn's ideas or definitions in any way. As such, his use of Kuhn represents an unquestioning acknowledgment of Kuhn's applicability to rhetoric and composition. Kuhn's ideas provide the framework from which Harned makes an historical examination of a key work in composition's development within the academy. By claiming the status of exemplar for Bain's English Composition and Rhetoric, Harned establishes the importance of conducting his own analysis.

Inquiry and Knowledge, 1987-1989

> A field that presumes the efficacy of a particular research methodology, a particular inquiry paradigm, will collapse inward upon itself.[26]
>
> --Stephen P. Witte, 1987

The 1987 publication of Stephen North's The Making of Knowledge in Composition: Portrait of an Emerging Field signals perhaps the acme of borrowing and responding to Kuhn in composition studies. North's segregation of various communities within

composition studies by their modes of inquiry, especially his delineation of the researcher community, depends heavily upon a knowledge of Kuhn's theory as well as the uses that other compositionists have made of that theory. North rejects Young's 1978 interpretation of Kuhn without dismissing Kuhn's ideas entirely, but North does find Kuhn's concept of paradigm confusing.

In the late eighties, other compositionists such as Stephen Witte, Richard Fulkerson, Charles Kostelnick, Linda Flower, and John Schilb continue the trend in appropriating Kuhnian terminology, sometimes mentioning Kuhn, but more often not. Few of the articles and reviews in CCC from 1987 to 1989 cite or mention Kuhn although some uses of Kuhnian terminology appear. A cursory examination of Richard L. Larson's 1987 "Selected Bibliography of Scholarship on Composition and Rhetoric, 1986" provides no direct evidence of Kuhn; neither Kuhn's name nor closely associated terminology such as paradigm appear in any of the annotated entries. Appearing the following year, Larson's "Selected Bibliography of Scholarship on Composition and Rhetoric, 1987" lists an article in Pre/Text (Bizzell's "Foundationalism and Anti-Foundationalism in Composition Studies" later reprinted in her 1992 anthology, Academic Discourse and Critical Consciousness), two essays in Gary Tate's 1987 Teaching Composition: Twelve Bibliographic Essays (D'Angelo's "Aims, Modes, and Forms of Discourse" and Young's "Recent developments in Rhetorical Invention"), two articles in CCC (Irmscher's "Finding a Comfortable Identity" and Freed and Broadhead's "Discourse Communities, Sacred Texts, and Institutional Norms"), and two books (North's The Making of Knowledge in Composition and Gere's Writing Groups: History, Theory, and Implications).

Many of the articles appearing in CCC during the late eighties that mention paradigm or other language often associated with Kuhn do not make use of Kuhn. For example, Mike Rose uses "paradigm" once in his "Narrowing the Mind and Page: Remedial Writers and

Cognitive Reductionism," but his article has nothing to do with Kuhn ("Narrowing" 276). Without mentioning Kuhn, Richard Fulkerson uses "paradigm" throughout his 1988 "Technical Logic, Comp-Logic, and the Teaching of Writing," his examination of the limitations of Toulmin logic and the value of borrowing stasis theory "from classical rhetoric" ("Comp-Logic" 450). As mentioned at the beginning of this chapter, Winterowd's 1989 "Composition Textbooks: Publisher-Author Relationships" refers to the current-traditional paradigm and cites other borrowers of Kuhn, but does not include any direct mention of Kuhn. In "Cognition, Context, and Theory," Linda Flower explores "some ways we might use research--observational research, specifically--to create a well-supported, theoretical understanding" of the interaction of cognition and context in the writing situation (283) while simultaneously noting that many of the techniques of observation research in which she has an interest "operate out of different paradigms, with different immediate goals and values" (284). Like Rose, Fulkerson, and Winterowd, Flowers uses some of the terminology and shows the influence of Kuhn's ideas.

Some of the authors in CCC who do mention Kuhn do nothing more than drop his name. For example, John Schilb notes in his 1989 essay, "Composition and Poststructuralism: A Tale of Two Conferences," that:

> When composition scholars have turned to poststructuralism, they have tended to consult the writings of Stanley Fish, Richard Rorty, and Thomas Kuhn--partly because they are relatively accessible, and partly . . . because they provide a theoretical justification for maintaining a service relationship to the traditional disciplines, now construed as "discourse communities" into which students need to be socialized. (Schilb 436)

Schilb goes on to refer to other compositionists who appropriate Kuhn, referring to Stephen North's focus on communities and modes of inquiry, Louise Wetherbee Phelps's use of hermeneutics, and C. H. Knoblauch and Lil Brannon's account of paradigm shifts (436). For Schilb, as for many others in composition, Kuhn's ideas have become part of the accepted background, part of the "disciplinary matrix" in composition to use Kuhn's term, with which one should be familiar.

Essays in CCC by Charles Kostelnick and by Richard J. Murphy, Jr. reflect still another trend in the late eighties--borrowing Kuhn's ideas as they appear in other disciplines that have appropriated those ideas from the history and philosophy of science. Kostelnick's comparison of the design process paradigm developed in fields such as architecture and engineering with the writing process paradigm in composition reflects an attempt to explain change within composition studies by analogy. Murphy borrows from education research in his "On Stories and Scholarship" appearing in the December 1989 CCC Staffroom Interchange column; he cites Jerome Bruner's 1985 "Narrative and Paradigmatic Modes of Thought" as well as rhetoric and composition works such as James Moffett's 1968 Teaching the Universe of Discourse, James Britton's 1970 Language and Learning, and North's 1987 The Making of Knowledge in Composition (470). The essays by Kostelnick and Murphy exemplify a tendency to borrow Kuhn as interpreted in other disciplines and tacitly use those borrowings as further evidence of his application to composition studies.

Book reviewers in CCC also retreat from using Kuhn to analyze various works, often limiting their remarks about Kuhn or paradigms to uses in the works reviewed. In her 1988 review of Anne Ruggles Gere's Writing Groups: History, Theory, and Implications, Beverly L. Clark mentions Kuhn as one of the thinkers that Gere invokes in her work (96). In another 1988 review, Diana George mentions the use of Kuhn in David Bartholomae and Anthony Petrosky's 1987 Ways of Reading: An Anthology for Writers.

Greg Myers's December 1988 review of several texts from the discipline of sociology of knowledge cites sources as well as reviews books that draw upon Kuhn, but these works from another discipline represent additional potential borrowings. In the separate 1989 reviews of North's The Making of Knowledge in Composition by James C. Raymond, Richard Larson, and Richard Lloyd-Jones, the word "paradigm" appears only once (in Raymond's review), and Kuhn does not even warrant any mention. Patricia Bizzell's review of Bennett A. Rafoth and Donald L. Rubin's 1988 The Social Construction of Written Communication lists Kuhn along with Clifford Geertz, Richard Rorty, Kenneth Bruffee, and Stanley Fish as the scholars composition theorists "are accustomed" to think of in connection with the concept of "social construction" ("Construction" 483). Bizzell attacks Rafoth and Rubin's work as potentially flawed for not taking the views of such scholars into account.

Uses of Kuhn in the late eighties indicate the extent to which scholars in composition have adopted many of Kuhn's ideas, recasting those ideas as their own. With the notable exception of North, most compositionists now seem to see those ideas as common property, especially the terminology Kuhn developed and defined in his own inquiry. In making various arguments, many compositionists cite borrowers of Kuhn, but few depend upon Kuhn's own words for evidence.

In one sense, a shift in the debate over using Kuhn occurs as we reach the end of the decade. The debate moves from applying Kuhn in composition studies to broader questions of research, theory, and practice. Some, such as Flower, ask, "What does a single integrated theory of writing look like? What is the best way to discover such a theory or paradigm? Can we find it?" They suggest the efficacy of one approach over others, for example, preferring empirical research as the ultimate way in which to test possible theory candidates. Others such as North proclaim that composition studies must continue to value multiple methodological approaches to understanding writing practice thus

ruling out adherence to a single research paradigm. The more inquirers in composition theory, practice, and pedagogy become exposed to Kuhn's ideas and his terminology, the more most inquirers divorce those ideas from the original context in which Kuhn developed them.

In his 1987 The Making of Knowledge in Composition, North first invokes Kuhn's name in a discussion of the research community within composition studies--a community whose members consist of "those who adopted modes of inquiry geared to lead them to more 'scientific' knowledge" (Knowledge 135). The tie between the Researchers and Kuhn's theory lies in the community's search for patterns. North is especially concerned with the potential ties between Kuhn's theory of paradigms and one subcommunity of researchers North dubs as the "Experimentalists," a group North defines as "those who seek to discover generalizable 'laws' which can account for--and, ideally predict--the ways in which people do, teach, and learn writing" (Knowledge 137).

North notes that the Experimentalists share with two other subcommunities of researchers, the Clinicians and the Formalists, as well as the group he terms the Scholars "the positivist tradition's fundamental faith in the describable orderliness of the universe" (Knowledge 137). According to North:

> [They] the belief that things-in-the-world, including in this case people, operate according to determinable or "lawful" patterns, general tendencies, which exist quite apart from our experience of them, and which are, in addition, accessible to the right kinds of inquiry. . . . [The groups] differ . . . in the kind of access to those patterns that they provide. The Experimental method proposes what amounts to a direct assault on them. In keeping with its natural science heritage, it attempts to systematically control and manipulate the phenomena it

> studies--in short, to experiment with it--in order to uncover those lawful patterns. The emphasis in Experimental inquiry has been what Gordon Allport calls nomothetic: the overall goals are framed in terms of generalization, not particularization. Data collected is valuable not for what it reveals about any particular individual, but as evidence concerning the sought after broader patterns. (North, Knowledge 137)

The inquiry into these "sought after broader patterns" is what North calls "with some reservations, paradigmatic" (Knowledge 147).

North identifies his reluctance to define the nature and structure of the knowledge that Experimentalists produce as paradigmatic. His reluctance "derives from the widespread confusion over this term [paradigmatic] generated by Thomas Kuhn's [Structure], and by all the borrowings of it since" (North, Knowledge 147). North goes on to limit his use of paradigmatic, "invoking at least to some extent its Greek origin in *paradeigma* (a pattern, a model, a plan) and so dampening, though not eliminating, the resonances of its more recent uses" (Knowledge 147).

In exploring the nature of Experimental knowledge-making, North asserts that:

> Experimental knowledge, then is paradigmatic in that its form corresponds to--is patterned after or perhaps more precisely, *is assumed to constitute the pattern of*--that portion of the orderly and accessible world under study: here, that portion defined by an interest in a phenomenon called writing. A fully formed paradigm would thus consist of the complete set of principles necessary to account for the relationships among all the variables of that portion of the world to which the community's powers of observation and measurement provided access. In its earlier stages--and, to be sure, Composition's Experimental

> paradigm fits that description--there are only a relatively small number of accounted-for variables tentatively connected by an even smaller, and very loose set of principles, posited relationships which nevertheless provide the basis for speculation about the larger paradigmatic whole (theorizing), and for framing further inquiry. (North, Knowledge 147)

North emphasizes the general principles and patterns searched for and discovered by the Experimentalists using "a process of trial and error which discounts relationships that seem random and pursues those that seem to exhibit regular, and so potentially lawful, patterns" (Knowledge 147).

In Section V of Knowledge, North again refers to Kuhn in an exploration of "the dynamics of inquiry" as it applies to "The 'Revolution' in Composition," the title of North's Chapter 10. North characterizes that inquiry into composition as "a methodological land rush: a scramble to stake out territory, to claim power over what constitutes knowledge in composition; and to claim, as well, whatever rewards that power might carry with it" (Knowledge 317). He notes that "the concept of paradigm gains most of its potency" from Kuhn's 1962 work and that the paradigm concept is implicit, though not stated, even in Braddock, Lloyd-Jones, and Schoer's 1963 Research in Written Composition.

Chapter 10, "The 'Revolution' in Composition," is the first of the two chapters of Section V that North devotes to dynamic changes in composition theory. North begins by crediting Richard Young's 1978 "Paradigms and Problems: Needed Research in Rhetorical Invention" and Maxine Hairston's 1982 "The Winds of Change: Thomas Kuhn and the Revolution in the Teaching of Writing" as two "particularly influential . . . [but] by no means the only versions" of Kuhn's paradigm shift theory. North goes on to explore variations of and objections to paradigm shift theory, treating Young's 1978 essay "as an

early representative and, at least in its general outlines, widely accepted accounting of Composition's internal turmoil" (Knowledge 318). He asserts that "Young's version of the paradigm shift explanation argues, first, that Composition is and has been a 'discipline,' bound together by this 'shared system of beliefs, values, and methods'; and second, that this 'shared system' began . . . to prove inadequate" (North, Knowledge 320).

North finds fault with Young's paradigm shift explanation while not completely discounting it. He concludes:

> There has unquestionably been a shift in the internal order of the society of Composition, and one manifestation of it has been to give increased attention to all sorts of composition- related issues, including those that interest Young. What is wrong with the paradigm shift explanation, as Robert Connors and C. H. Knoblauch, among others, have pointed out, is its assumption that Composition--if it can even now be called a discipline at all--can really be assumed ever to have had this sort of paradigmatic structure. (North, Knowledge 320)

But North claims that the "*a priori* assumption that [Composition] had (or still has) such a [paradigmatic] structure is bound to lead, as it leads Young, to a misunderstanding of the methodological pluralism" North delineates in Knowledge. North accuses Young of lumping "all modes of inquiry . . . together, epistemological and ontological assumptions notwithstanding, arguing in effect that the dynamics of Composition as a whole are modeled on those of its Experimental community, so that all contributions gain significance paradigmatically" (Knowledge 320).

Although he is unwilling to accept Young's view of paradigm shift as "particularly valuable for what it *says*," North does acknowledge that the theory "does reveal a good deal by what it tries to *do*" (Knowledge 321). "That is," North declares:

> Young uses it in the context of Research on Composing: Points of Departure to provide a platform from which to launch his particular research agenda, a use which amounts to a kind of propaganda: an account of Composition's developmental dynamic articulated for some political purpose. In terms of this study [North's], it might be described as a power play, an attempt by one methodological community of a cluster of communities to assert its dominance over others. (North, Knowledge 321)

According to North, these sorts of power plays have been "most characteristic of the emergent field of Composition." For North, Young's use of Kuhn's paradigm shift "has a certain fashionable appeal to it," but the "revolution is not best understood, as Young would have it, as a more or less unified response to some shared paradigmatic crisis, but as an inter-methodological struggle for power" (Knowledge 321).

To cull all of North's uses and references to paradigms, paradigm shifts, revolutions, crises, Kuhn, and borrowers of Kuhn would take much too long. He rejects many of the early uses that Kuhn's ideas are put to, arguing that, at least in terms of modes of inquiry and methodological communities, compositionists such as Young and Hairston either misunderstand or misapply Kuhn's ideas to composition studies. It may be convenient to use the terminology of "paradigm" and "shift," but those terms have significantly different meanings in composition if the profession is seen as North sees it in terms of its various methodological communities. Simultaneously, North can be interpreted as arguing for a separate discipline of composition that is dedicated to some

sort of paradigm of methodological plurality united by its attention to practice as inquiry. In this sense, North can be seen as both borrowing and rejecting Kuhn.

Another proponent of methodological plurality, Stephen P. Witte uses Kuhnian ideas, but not Kuhn, in his review of George Hillocks, Jr.'s Research on Written Composition: New Directions for Teaching. Witte finds fault with Hillocks's restriction of valid research in composition between 1963 and 1982 to empirical studies of written composition. While acknowledging the importance of Hillocks's book, especially the compilation of a comprehensive bibliography of empirical research between 1963 and 1982, Witte accuses Hillocks of favoring empirical research methodologies over all others. He notes that one vein of Hillocks's argument involves defending empirical research against assaults by its detractors, notably Janet Emig and Donald Graves. Witte's criticism of Hillocks's book owes much of its efficacy to Kuhnian ideas even though Witte never mentions Kuhn.

In his 1987 review, Witte declares:

> My own view is that the dispute over research methodologies and *paradigms* [my emphasis], which seems to have in large part motivated Hillocks' inquiry, is largely counterproductive, primarily because it diverts attention away from what I think are more critical issues in a field still struggling to define itself and still searching for a theory capable of guiding either research or classroom practice. A field that presumes the efficacy of a particular research methodology, a particular inquiry paradigm, will collapse inward upon itself. If it is ever to develop a comprehensive theory capable of explaining acts of writing or of guiding the teaching of writing and if its findings are ever to contribute significantly to educational policy, research in written composition must be able to use, and use well, a multitude of research tools or methodologies. The field of

> written composition is large enough and vital enough to make good use of both qualitative and quantitative methodologies and to embrace both the logic of discovery and the logic of validation. Moreover, the field of research in written composition must do both, it if is to meet its obligations to itself *and* to the larger social context which sustains it. What the field needs is not more unproductive and even counterproductive bickering about methodologies, but greater recognition of the strengths and limitations of all research methodologies (and there is no perfect research methodology) in providing answers to important questions. Only a true marriage of discovery and validation in research on written composition will allow us to sort out the important from the unimportant questions and to provide answers to those questions we judge the most important. (Witte 206-207)

In Witte's criticism of New Directions, we see a deliberate effort to sustain multiple methodological inquiry within composition studies. In one sense, Witte takes a stand against those who would support one dominant form of research over others. He suggests that the strength of composition studies lies in pursuing multiple forms of inquiry, marrying qualitative and quantitative methodologies, and fusing discovery with validation. These ideas seem to fly in the face of Kuhn's notion of single paradigm-driven science, with which Witte apparently disagrees although he does not rule out such an eventuality. By noting the field's struggle for definition and search "for a theory capable of guiding either research or classroom practice" (Witte 207), he reinforces Kuhn's notion of a preparadigmatic state in the form of Masterman's multiple-paradigm perspective. If anything, Witte argues that any paradigm in the sense of Kuhn's disciplinary matrix must account for a multiple methodological approach to research in composition.

While Witte argues against composition possessing a dominant paradigm, Charles Kostelnick poses the process approach as one. In "Process Paradigms in Design and Composition: Affinities and Directions," Charles Kostelnick scrutinizes the "principal tenets" of the process approach to composition, what Kostelnick calls "the new composition 'paradigm,'" through a comparison with "another field of creative problem-solving--design." Kostelnick argues that the process approach to teaching writing "now has a definable history and . . . rival theories" vying for dominance. He suggests that "examining similar developments in . . . design . . . can shed light on the evolution and future direction of the writing paradigm" (Kostelnick 267). In setting up his discussion, Kostelnick writes:

> Process theories of design broadly encompass visual thinking, both applied and expressive, in a variety of disciplines . . . with theoretical models developed most intensely in architecture and in design pedagogy. As a medium for creativity and communication, design is the natural counterpart to writing, one adapting visual, the other verbal, language to diverse contexts and audiences. (Kostelnick 267)

Rejecting Kostelnick's central premise of the parallelism between design process and the writing process would invalidate his entire argument, so he takes pains to draw attention to the "affinities between the two movements." He speaks of their exploration of creativity, their shift from a linear to a recursive process, and their treatment of "invention as a problem-solving task adapted to a particular audience and context." Kostelnick intends to "show how developments in design process corroborate some of the principal tenets of composition theory and pedagogy, laying the foundation for a cross-disciplinary theory of the creative act." He argues that the juxtapositioning of the two

process movements "can also provide insight into how to reconcile the writing process paradigm with real-world text production" (Kostelnick 267-68).

According to Kostelnick, an examination of the evolution of the "writing and design process paradigms" reveals that the two movements share significant affinities. He proceeds to list and clarify three major affinities he sees that the two process models share. First, he contends that during their development, both models have viewed "creativity as a conscious, goal-directed activity in which problem definition plays a crucial role in the creative act." Second, the evolution of the paradigms displays a shift from application to description, "from devising practical methods to studying how writers and designers actually proceed." Finally, Kostelnick asserts that both paradigms have "drawn on theories external to the discipline (cognitive psychology, the philosophy of science) to redefine issues and to generate problem-solving models" (268).

Borrowing from the epistemology or philosophy of science, proponents of both movements have, according to Kostelnick, appropriated Kuhn and others "for theories about how the shared knowledge and values of a discipline influence . . . thinking" (269), especially invention or creation. Kostelnick suggests that compositionists have "begun to explore," in much the same way as the design theorists, "problem-solving as an epistemological phenomenon grounded in social interaction" (273). He continues:

> Drawing on the work of Kuhn and Popper, design theorists have shown how disciplinary paradigms engender a cognitive framework that enables designers to "pre-structure" problems. Guided by a "pre-existing cognitive map," the designer formulates tentative solutions of "conjectures," which are then analyzed, tested, and modified. . . . This social-cognitive approach to invention parallels . . . composition research in which the production and reception of texts are examined in their social and cultural context. *Context* entails not only the

> situational variables of a single writing task but the shared values, ideas, and norms of environment in which writers and readers make meaning. . . . The collective knowledge of discourse participants "pre-structures" the communication process, engendering reader expectations that guide text production and processing. Both writing and design theorists, therefore, have increasingly treated invention not as an isolated act of an individual but as a social extension of the environment in which the writer or designer communicates. (Kostelnick 273)

Kostelnick asserts that: "Because writing and designing are highly contextual tasks, audience analysis initiates the invention process, clarifying the rhetorical nature of the communication--verbal *or* visual" (273).

The stress on audience analysis or awareness in both models has another significance. In Kostelnick's view, the placement of the audience "at the center of the rhetorical process" has "debunked the myth of the isolated artist or writer operating in a cognitive or contextual vacuum" (274). Neither can create independent of the social context and construction of knowledge.

Kostelnick reasons that the parallel evolution of design methodology corroborates the "central tenets of the writing process paradigm." He contends: "The concept of designers or writers who consciously oversee their own methods--by organizing complex tasks, using audience analysis to define problems, and exploring solutions recursively--has transformed our understanding of the creative act." Further, he argues that the evolution of both models independently of one another "validates the universality of these principles and makes the parallels between the two movements all the more remarkable" (Kostelnick 274).

Throughout the first half of his article, Kostelnick's use of Kuhn depends upon borrowing him from another discipline, design methodology, that in turn has borrowed Kuhn. Although he cites Hairston's 1982 "Winds of Change: Thomas Kuhn and the Revolution in the Teaching of Writing," Kostelnick never turns to the original Kuhn for any supporting evidence. He simply notes that the process movements in both areas of inquiry draw sustenance from appropriating some of Kuhn's ideas. He never questions the validity of the appropriation; he simply accepts Kuhn's contribution as interpreted by others and uses Kuhn's terminology for his won purposes. His comparison of both process models allows him to infer the possible future of the writing process movement in composition studies.

In the second half of his comparative analysis, Kostelnick focuses on the implications of problems with the design process paradigm. He suggests that even with the important "insights" into creativity that design methodologists have developed, their "discoveries now appear to be more the legacy of a waning movement than the foundation for a sustained disciplinary shift." He identifies two major "flaws" within the design process movement: "(1) the quest for a single all-encompassing model, and (2) the loss of credibility resulting from practical application" (275). Kostelnick implies that the process paradigm in composition studies possesses the same two flaws.

With these statements, Kostelnick finally hints at what appears to be his real agenda--questioning the efficacy of the shift within composition studies to an emphasis on the writing process. If design theorists have tested various models of invention and found them wanting, rejecting the idea that invention can be "neatly circumscribed in a single model," what do their findings suggest for attempts in composition studies to discover "a unified model of *'the* composing process'" (Kostelnick 275)? Kostelnick reasons: "Like the futile quest of design theorists for a single all-encompassing method, the search for a universal composing paradigm is an intellectual and practical cul-de-

sac" (276). A composition process model does not exist to fit all contexts in which writing takes place.

Kostelnick goes on to point out one key difference he sees between design and writing paradigms in his discussion of implications, the accountability that many design theorists and researchers face as practitioners in their field. Design "process methods and theories are continually tested, scrutinized, and then accepted, modified, or rejected" in the disciplines where students of those methods and theories practice. "In contrast," Kostelnick reports, "those of us who teach writing as a service course have minimal contact with the disciplines our students study, much less with the writing environments students will enter after leaving the university" (277). Therefore, compositionists do not face the same application problems that design researchers face; Kostelnick argues that we are not held as accountable as those in design. Thus he questions the commitment of many compositionists to the writing process movement, suggesting that many adopt the process paradigm because "it is compatible with our own writing and teaching experiences or . . . because we accept 'on faith' the validity of its underlying principles and methods" (Kostelnick 277-78).

Kostelnick contends that advocates of design process paradigms face the continuous challenge of having to wed theory with practice in their disciplines. The failure of design process theory has forced the movement "into a series of crises" pointing "to one conclusion: to reorient and solidify a discipline, a paradigm shift must be underpinned by theories that are verifiable and that enable to address the issues it sets out to address." Applying these same two criteria to the writing process paradigm, Kostelnick concludes that the paradigm "still lacks the necessary conceptual bridge between theory and practice to sustain a credible, long-term transformation of the discipline" of composition studies (278).

Ultimately, Kostelnick declares that several of the "common tenets" of the design process and composing process movements "lay the cornerstone for a cross-disciplinary theory of the creative act." While those tenets--"writing and designing as acts of discovery, the recursiveness of invention, the consciousness of experienced writers and designers of their own processes, and the essential role of audience analysis in problem definition"--suggest a foundation for theory building, Kostelnick warns against "the hazards of constructing a working model from these principles" hastily. He favors multiple models, given "the immense diversity of writing that we teach and that our students will do professionally"; according to Kostelnick, "a pluralistic approach--sensitive to a variety of writing tasks and to the social context of the writing environment--would simply be more accurate, functional, and teachable than a single amorphous model" (278).

Although Kostelnick refers directly to Kuhn by name two or three times within his analysis, he does not cite any of Kuhn's works or devote any space to explicating Kuhn's theory; he does not even bother to define any of the terms he uses such as "paradigm," "shift," or "crises," assuming instead that his readers will be familiar with Kuhnian terminology. Kostelnick's argument depends upon the acceptance of Kuhn's ideas in general while rejecting Kuhn's single paradigm as inappropriate for composition; the diversity of contexts within which people write does not allow adherence to any one way of practice. Kostelnick attacks the search for a single unified theory of the composing process, implying that such a search is a misguided mistake given the social context of writing practice. He values Kuhn's ideas most for their contribution to the establishment of a pre-existing cognitive framework, relying perhaps on Kuhn's meaning of paradigm as a disciplinary matrix more than anything else.

The period from 1982 to 1989 represents the peak of borrowings of Kuhn since the 1962 publication of Structure and its 1970 second edition. Before the eighties expire, Kuhn becomes part of the background of thinkers that students of composition studies need to possess to understand many of the debates within the profession. Kuhnian terminology takes on a life of its own, often distanced from its coiner; it becomes fashionable to speak of paradigms, shifts, crises, and revolutions in the theory, practice, and teaching of writing. As the decade progresses, compositionists move away from direct references to Kuhn and citations of his work to dependence upon interpretations of that work by others, even others in disciplines outside of composition. The distance between what Kuhn writes and what others write about him grows. As knowledge of Kuhn's ideas spread within composition studies, users often unconsciously divorce those ideas from their originator.

These tendencies seem to reflect the transformation of Kuhn's thoughts from a direct borrowing to some status as an integral part of composition studies. To actively engage in the debates over inquiry into composition, it helps if one has some familiarity with Kuhn's ideas and terms. Despite attempts by scholars such as Robert Connors to derail Kuhn's influence, a knowledge of Kuhn seems to have garnered a place in the shared values, attitudes, and beliefs that constitute elements of the "discourse community" of composition studies. Within the "disciplinary matrix" of composition inquiry, Kuhn's influence appears widespread.

CHAPTER SEVEN

THOMAS KUHN'S THEORY TODAY

> If . . . some social scientists take from me the view that they can improve the status of their field by first legislating agreement on fundamentals and then turning to puzzle-solving, they are badly misconstruing my point.[27]
>
> --Thomas Kuhn, 1969 (1970)

By the time we reach the eighties, pursuing the implications of Kuhn's work, extending or modifying its meaning, or rejecting its claims spawns an entire new field --the sociology of knowledge, a field interested in the philosophical, social, and cognitive underpinnings of knowledge. Barry Barnes, in the 1982 T. S. Kuhn and Social Science, declares: "Thomas Kuhn's work is widely known, readily intelligible and easily obtained. Its [Structure's] account of scientific research is an accepted point of reference throughout the academic world." In making this statement, Barnes addresses "the sociology of scientific knowledge which takes Kuhn's work as its point of departure." He goes on to assert that "Kuhn has made one of the few fundamental contributions to the sociology of knowledge" (Barnes [ix]-x). Barnes writes:

> It fell to him [Kuhn] to provide, at the time it was most needed in the 1960s, a clear indication of how our own forms of natural knowledge could be understood sociologically. This encouraged the empirical studies of scientific culture which were then beginning in the social sciences. And it inspired a revived awareness of the constitutively social character of knowledge generally. (Barnes x)

In a broader sense, Barnes is less concerned for Kuhn's own field of history of science, writing instead for other sociologists interested in exploring knowledge in the context of society. But his words and those of his colleagues as well as those of Kuhn and others in the history of science speak volumes about the interpenetration of Kuhn's ideas throughout investigations into the nature of knowledge and science. Barnes's characterization of Kuhn's work as "an accepted point of reference throughout the academic world" ([ix]) seems reminiscent of Gage and his colleagues citing the universality of Kuhn's concept of paradigm in their 1977 selected annotated bibliography.

Conversely, Stanley Wallen, writing in his dissertation only six years after the publication of Barnes's book and slightly more than a decade after Gage and his colleagues, "regards Kuhn's program, as opposed to his intention, as an unmitigated failure" and contends that "it constitutes not only an impractical program for the historian [of science] but in fact an impossible one" (154). Wallen echoes many of Kuhn's earlier critics by attacking Kuhn's sense of incommensurability, his description of the conversion experience associated with a paradigm shift, his distinctions between normal and revolutionary science, and his concept of paradigm. Indeed, Wallen draws extensively from the work of Masterman, Popper, Shapere, and Toulmin, some of the most vocal of Kuhn's critics and supporters. Wallen completely discounts Kuhn's theory, arguing that "Kuhn's program, if taken seriously, would make history impossible" (159).

Others in the history and philosophy of science take a view quite different from that of Wallen. Philip Kitcher uses Kuhn's ideas as a starting point in 1993 The Advancement of Science: Science without Legend, Objectivity without Illusions, suggesting that Kuhn revolutionized the history and philosophy of science. In his 1993 Paradigms and Barriers: How Habits of Mind Govern Scientific Beliefs, Harold Margolis likewise builds

upon much of Kuhn's work, giving "an account of the Kuhnian notion of 'paradigm shifts'" with a focus on the role of persuasion, belief, and judgment in scientific development (1) and arguing cogently in favor of Kuhn's notion of incommensurability. Efforts by scholars such as Kitcher and Margolis acknowledge Kuhn's contributions, attempt to place his theory of scientific development in perspective, and build upon the theory's implications.

Kuhn also offers an assessment of the current status of his theory, acknowledging certain developments in his thoughts and suggesting his continued interest in the notion of incommensurability. In his 1991 lecture, The Trouble with the Historical Philosophy of Science, and his 1993 "Afterwords" essay in Paul Horwich's World Changes: Thomas Kuhn and the Nature of Science, Kuhn evaluates the status of the ideas expressed in Structure. At the same time, he continues his tradition of responding to his critics while working for a more refined articulation of his theory.

Chronicling the development and borrowing of Kuhn's theory since the publication of his seminal Structure requires an examination of the reception of Kuhn's theory today in its original and allied fields and a comparison of that reception to composition's appropriation of Kuhn in the nineties. In this chapter, I return to the reception of Kuhn's ideas in his own fields--history and philosophy of science and sociology of knowledge--and focus on the current status of his ideas.

Understanding Scientific Knowledge Today

> In judgment it is fair to say that Kuhn has pointed us in exactly the wrong direction. If we are to understand the apparent universality of science what we must seek are factors that explain how diverse individuals are able to work

together successfully. To focus upon the difficulties as Kuhn has done is no real help, given the total failure on his part to attempt to resolve those difficulties.[28]

--Stanley Wallen, 1988

What has been the response of history and philosophy of science to Kuhn's theory since 1980? Divided. As noted above, Barry Barnes continues to advocate the spread of those theories, while Stanley Wallen concludes that Kuhn's theories ultimately fail. Philip Kitcher differs widely from Kuhn in the 1993 The Advancement of Science: Science without Legend, Objectivity without Illusions, yet remarks that he follows up on some of the hints of Kuhn's work. He adopts and expands many of Kuhn's ideas. In the 1993 Paradigms and Barriers: How Habits of Mind Govern Scientific Beliefs, Howard Margolis attempts "to show how the habits-of-mind claim implies the possibility of the phenomena Kuhn has captured under the notion of 'paradigm shifts'" (3). Building upon earlier criticisms and investigations of Kuhn's work, these authors present a portrait of the contemporary reception of his theory.

Borrowing by the Social Sciences

In T. S. Kuhn and the Social Sciences (hereafter referred to as Social Sciences), Barry Barnes evaluates Kuhn's contributions to the social sciences, "how those contributions have been developed in recent years, and how they might usefully be further developed" ([ix]). Barnes approaches that goal from the standpoint of sociology. He writes that "a discussion of current views in the sociology of scientific knowledge can be developed from an examination of Kuhn's work, even though Kuhn himself is not a sociologist" (Barnes x). Barnes warns that we still understand very little about learning, cognition, and knowledge-making, thus a sociological approach "can only be

part of the larger project of gaining a proper *empirical* understanding of the characteristics of knowledge and cognition" (x). Further, he cautions that his "focus of attention is the knowledge associated with a specific social context," developed through "scientific investigation" and including "among its subject-matter, the knowledge and culture of science" (Barnes xi). Barnes confines much of his investigation to Kuhn's works as they relate specifically to Structure in its 1970 second edition.

Fully cognizant of the criticism of Kuhn's Structure delivered in Lakatos and Musgrave's Criticism and the Growth of Knowledge, Barnes addresses that criticism in his work, especially doubts about Kuhn's sense of "paradigm." Barnes notes that he uses the term "'paradigm' to denote an accepted problem-solution in science, a particular concrete scientific achievement" (Barnes xiv). Barnes thus attempts to "adhere" throughout his work to a definition of "paradigm" in Kuhn's second sense of the word, as an exemplar.

According to Barnes, Kuhn opens himself to criticism from other historians and philosophers of science for invoking sociology by calling attention to the social context within which scientific knowledge develops. Still, Barnes contends, Kuhn's theory, especially "his treatment of the nature of convention, culture, and tradition," provides a "descriptive, naturalistic account of science" (15). Barnes believes that sociological methodologies can be used to achieve a more fuller account of what Kuhn hints at in Structure.

To support his contentions, Barnes spends a great deal of space in Social Sciences explicating Kuhn's theory of advance in the natural sciences and discussing its sociological implications. But Barnes is far more interested in extending Kuhn's work outside the context of science. Barnes records:

> In my judgment the general significance of Kuhn's work lies neither in its specific historical narrative of the development of science, nor in the concepts invented for that narrative, but simply in its explicit discussions of general problems concerning cognition, semantics and culture. Kuhn is important where he examines similarity relations, concrete problem-solutions, and the development of usage and procedure by analogy and direct modeling. Here, our understanding of the conventional nature of knowledge is advanced, as is our understanding of the nature of convention itself. This is material which no one in the social sciences should pass over, even if Kuhn himself has not advertised its general importance. (Barnes 120)

He goes on to "recap how Kuhn seeks to understand a scientific theory," writing that:

> It is by the competent use of paradigms [exemplars] that scientists acquire a sense of the meanings of terms, the implications of laws and generalizations, and hence the content of theories. Theories are not separate from paradigms. On the contrary, a theory can be understood simply as a cluster of accepted problem-solutions. (Barnes 121)

Barnes redefines "theory" as the "cluster of problem solutions" or applications. Thus, he reverses the traditional view of a theory as informing its applications.

Such remarks place Barnes clearly among Kuhn's supporters. He builds a case within Social Sciences for extending Kuhn beyond the boundaries of the natural sciences. While he repeats Kuhn's concerns about such expansions, Barnes clearly favors an empirical application of Kuhn, one that does not grow out of any "intellectual laziness," but takes into account Kuhn's "context of the history of the natural sciences" (120).

Barnes's application of Kuhn to sociology parallels the appropriations made by Young, Bizzell, Emig, and Hairston in composition studies around the same time. He goes further in that he offers a book length study illustrating the relationship of Kuhn's theory to sociology and supporting investigation of Kuhn's ideas within broader realms. Like Kuhn in his "Second Thoughts," "Reflections," and "Postscript" essays, Barnes attempts to account for much of the criticism that Kuhn meets within the history and philosophy of science. Where compositionists in the early eighties seem to have embraced Kuhn's theory with a minimum of explanation and a maximum of faith, often never bothering to seek beyond Kuhn's second edition of Structure, Barnes puts Kuhn's ideas into the perspective of his own discipline, "translating" them, to borrow some of Kuhn's language, into a basis for further sociological research into the nature of communities. Rather than simply apply Kuhn's ideas as a framework for analyzing his discipline, Barnes sees those ideas as initiating further debate about the social construction of knowledge.

Rejecting Kuhn's Theory as Impossible

In his 1989 dissertation, Historiography of Thomas S. Kuhn, Stanley Wallen directly opposes the admiration expressed by Barnes and many other borrowers outside of Kuhn's community of history and philosophy of science. Wallen sets out to deliberately expose the weaknesses of Kuhn's theory through an extended discussion of it. He labels Kuhn's theory a "failure," calling it "impractical" and "impossible" (154), and repeats many of the earlier charges leveled against Kuhn by critics such as Shapere and Popper while denigrating the remarks of Kuhn's supporters. Wallen confines his own chief criticisms of Kuhn to his last chapter, preferring to structure many of his comments in forms that echo many of Kuhn's earlier critics.

Wallen attacks Kuhn on a number of fronts, beginning with an extension of Shapere's refutation of Kuhn's concept of incommensurability. Shapere has called Kuhn's idea "a myth" (Qtd in Wallen 154); Wallen repeats the charge, suggesting that no strict interpretation of incommensurably can exist. Accepting the possibility "that Kuhn only means that there are certain elements in two theories that prevent our comparing them," he further argues that "to deny that such differences are susceptible to analysis is to advocate a complete relativism" (Wallen 155). Wallen contends: "In short, if we were to take the concept of incommensurability, i.e., the existence of factors that we could not resolve by analysis, seriously, then it would be impossible to do history at all" (155). By taking this position, Wallen discredits a key part of Kuhn's theory. The essence of the concept of paradigm shift depends upon the conflict between incommensurable or incompatible views held by members of the community.

As for Kuhn's conception of perception as it relates to incommensurability, Wallen writes that Kuhn "seriously distorts the degree to which different individuals see different things" (155). He offers a step-by-step assault on Kuhn's attempts "to borrow from psychology" to support the concept of the breakdown in communications between adherents of different paradigms (156). Recalling Kuhn's use of the duck-rabbit "figure-ground" experiment as support for the concept of paradigm, Wallen charges that the drawing "is always ambiguous" and that "we can always see two configurations" (156). He notes that "this is <u>just</u> the characteristic that they do not share with paradigms; once the new paradigm is seen the old one can never be seen again" (156). But Wallen's reasoning is flawed by his literal interpretation of Kuhn's point; Kuhn uses the figure-ground experiment to illustrate his contention that different members of the same community may view the same stimuli, yet "see" different things. Kuhn makes the point that the different members do not see two configurations; they see different configurations. The stimuli remain the same, but are interpreted differently.

Next, Wallen mistakenly interprets and likens Kuhn's paradigm "to a gestalt change," referring to what he calls Kuhn's admission that "it [the gestalt change] must occur all at once, although not necessarily all at the same time," as "gibberish" (156). Wallen argues: "By definition a gestalt change must be instantaneous, otherwise our perception system would shut down like an automobile plant for the yearly model change" (156). Here, he chooses to dismiss Kuhn rhetorically after misconstruing Kuhn's point about the recognition that one has shifted paradigms.

Wallen concludes that "Kuhn has pointed us in exactly the wrong direction" to investigate the nature of science (157). He declares: "If we are to understand the apparent universality of science what we must seek are factors that explain how diverse individuals are able to work together successfully" (Wallen 157-58). To Wallen, Kuhn's focus on the scientific community rather than the individual scientist seems an anathema. He dismisses Kuhn's descriptive commentary which suggests that the shared values, beliefs, and language of the scientific community do influence the direction of scientific advance.

After arguing for refocusing on the individual, Wallen proceeds to attack Kuhn's framework of normal and revolutionary science, suggesting that Kuhn has retreated somewhat from the rigidity of the distinction that he makes between the two. He faults Kuhn with going too far to discredit the evolutionary notion of scientific development by accumulation, and he repeats the conclusions of others "that even the most violent revolution in science leaves intact far more than it changes" (Wallen 160). Wallen charges that Kuhn's focus excludes "other, possibly more important aspects" of the history of scientific development (160).

In attacking Kuhn's notion of paradigm and its role in scientific advance, Wallen misinterprets Kuhn's remarks about salvaging the original concept of paradigm. He deliberately plays down Kuhn's redesignation of paradigm in one sense as a disciplinary

matrix and in a second sense as the concrete problem-solutions, the specific scientific achievements that serve as exemplars for a particular community of specialists. Instead, Wallen chooses to label the "idea of one overarching concept governing the way science is conducted" as "a veritable will-o'-the-wisp." He concludes that any attempt to revive Kuhn's concept "is not worth the confusion" (Wallen 160).

Wallen also questions the concept of scientific revolutions, using the analogy of the "American, French, Russian, and Chinese political revolutions" to suggest that such events "have little in common in regard to causes, procedures or results" (161). Criticism of Kuhn's work as descriptive or normative resurfaces in Wallen's remarks. Citing his own investigation of Kuhn's 1957 The Copernican Revolution, Wallen argues that Kuhn has not clarified "the basic elements" that constitute a revolution. Ultimately, Wallen asserts: "The conclusion we are forced to adopt is that while the term "revolution" as a descriptive term covering a wide variety of events has a value in history, the idea of "revolution" in any normative sense is insufficiently developed to possess any like value (Wallen 162).

Kuhn's examples in Structure receive like treatment. Wallen finds that Kuhn has "carefully selected" studies to "support his views," then stipulates that an "equal number could be presented to contradict" those views. Further, Wallen insists that the case studies Kuhn chooses "do not, in fact, support the ideas for which he [Kuhn] has adduced them" (163).

Wallen returns to one of the initial issues he raised against Kuhn's ideas at the beginning of his last chapter--"Kuhn's importation into the history of science of certain concepts from psychology." Wallen chastises Kuhn for repeatedly asserting that "a mental change must always precede a perceptual change" (163). He accuses Kuhn of borrowing "perception theory . . . still in its infancy" and misapplying it to the history of science. According to Wallen: "To postulate that a 'paradigm change' makes . . . new

perception possible is no help at all unless 'paradigm change' means something far more exact than Kuhn has made it" (164).

In his concluding remarks, Wallen contends that Kuhn's 1978 Black-Body Theory and the Quantum Discontinuity, 1894-1912 represents an abandonment of "the theoretical side of the history of science," a movement away from Kuhn's original theory of paradigm, shifts, and revolutions as expressed in Structure (164). He focuses on the absence in Black-Body Theory of ideas most closely associated with Structure--paradigms, crises, normal and extraordinary science--and cites a 1979 review symposium in Isis entitled "Paradigm Lost?" The authors of the symposium, Martin J. Klein, Abner Shimony, and Trevor J. Pinch argue that the ideas expressed in Structure seem conspicuously absent, thus Kuhn's Black-Body Theory does not support Kuhn's earlier views. In light of their remarks, Wallen reduces the value of Kuhn's original theory to that of "devil's advocate," noting that Kuhn's "large scale failure" in Structure serves "to throw a great deal of light on the positions that he [Kuhn] opposed" (165-66).

Wallen's dissertation presents an extreme response to the efficacy of Kuhn's theory in the history of science. He depends upon the criticisms rendered earlier by others for the bulk of his attack on Kuhn. Although writing in the latter half of the eighties, Wallen uses few contemporary sources in his bibliography; he relies almost completely on sources published before 1979, a decade before the completion of Historiography of Thomas S. Kuhn. Further, few of his comments represent any new departures from the criticism that appeared between the 1962 publication of Structure and the 1974 publication of Kuhn's "Second Thoughts" essay. While he finds sufficient material to convince himself that Kuhn's theory does not work in the history of science, he fails to make a case that would prevent those in other disciplines from continuing to borrow Kuhn's ideas.

Modifying Kuhn's Work

In the 1993 The Advancement of Science: Science without Legend, Objectivity without Illusions, Philip Kitcher presents a philosophical and mathematical treatise on the nature of scientific knowledge. As such, Kitcher grounds his inquiry squarely in the philosophy of science. His arguments rest on the modification of Kuhn's work within the realm of what Kitcher calls "Legend"--the scientific concern, in its different versions, with the "discovery of truth . . . valued both for its own sake and for the power that discovery would confer upon us" (3). The Advancement of Science contains an extended explanation of Kitcher's concept of scientific legend and the existence of consensual practices among scientists. To advance his own theory of knowledge-making in science, Kitcher relies on Kuhn for a number of ideas and constantly draws comparisons between Kuhn and others. He dismisses a number of Kuhn's points and reinterprets others in an investigation of legend, endeavoring "to provide a philosophical framework for the study of science which combines the insights of Legend with the insights of its critics" (Kitcher 390).

Kitcher contends that Legend, "for all its difficulties," is "broadly right about the characteristics of science" (390). He seems to summarize these characteristics when he writes: "Flawed people, working in complex social environments, moved by all kinds of interests, have collectively achieved a vision of parts of nature that is broadly progressive and that rests on arguments meeting standards that have been refined and improved over centuries" (Kitcher 390). Within such concluding statements rest the elements of his agreement and disagreement with Kuhn that appear throughout Kitcher's text.

In his text, Kitcher sets his overall goal as illustrating scientific advance, "using the commonsensical ideas that underlie Legend, the insight of Legend's critics, as well as the

contributions of contemporary philosophers, historians, sociologists and cognitive scientists" (8). In part, that goal also involves presenting a more "accurate picture of science, its goals, and its achievements"; he aims "to probe the notions of progress and rationality" (6). Kitcher states: "Legend's legacy is the task of recognizing the general features of the scientific enterprise, most importantly by scrutinizing the apparent progressiveness of science, the seeming individual rationality of individual scientists, and the collective rationality of the scientific community" (Kitcher 10).

Quoting "the famous opening sentence" of Kuhn's Structure--"History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed"--Kitcher attributes Kuhn with beginning "the enterprise of comparing the actual course of science with Legend's ideal of accumulation of truth" (Kitcher 6). In the process, he argues that Kuhn attempts "to steer a middle course between Legend and relativism" (7). Kitcher asserts: "In the wake of Kuhn's work, the practice of science, past and present, is seen as raising doubts about whether the aims beloved of Legend have *actually* been obtained, and these doubts then serve as the basis for wondering whether they are *attainable."* Calling Kuhn's work "seminal," Kitcher continues: "Kuhn did not simply propose that existing accounts of good reasoning in science were wrong, that the idealizations they employed were to simple, or that their vocabulary needed enrichment. Instead, he suggested that the entire project of finding a "calculus of scientific reasoning" ought to be abandoned" (Kitcher 6-7).

Unfortunately, various interpretations of Kuhn's call, certainly unintended by Kuhn, have "opened up a vacuum into which social explanations of scientific behavior could be asserted" (Kitcher 7). Kitcher remarks that Kuhn has continued to insist, much to the chagrin of some inspired by him, "on a set of commitments that scientists share, that cannot be articulated as rules but that function in distinguishing good reasoning from

bad" (7). The issue of sharing implies the sense of community that permeates Kuhn's Structure. Kitcher suggests that Kuhn's work has been helpful in partially exploring the "idea of understanding how social institutions might promote knowledge," though Kuhn does not go far enough (9).

In an extended discussion of Charles Darwin's theories in chapter two of The Advancement of Science, Kitcher examines conceptual shifts. He suggest that the shifts attendant on the debate inspired by Darwin's Origin of the Species are "epistemologically unproblematic" (32). In other words, such shifts "do not threaten to break down communications between Darwinians and their opponents, or even to create a situation in which 'communication . . . is inevitably partial' (Kuhn, Structure 149)" (Kitcher 32). Kitcher notes that "Darwin and his critics had not the slightest difficulty in communicating their ideas to one another" (32). Thus, for Kitcher, the Darwin example seems to counter at least part of Kuhn's notion of incommensurability and call it into question.

In the place of Kuhn's "paradigm," Kitcher introduces the notion of "consensus practices." For Kitcher, a "consensus practice" serves much the same function:

> [It is] constituted by language; an (impersonal) assessment of significant questions; a set of accepted statements with a (partial) justificatory structure; a set of explanatory schemata; a set of paradigms of authority and criteria for identifying authorities; a set of exemplary experiments, observations, and instruments and justificatory criteria; and, finally, a set of methodological exemplars and methodological principles. (Kitcher 87)

Kitcher envisions scientific progress "as a sequence of consensus practices that get better and better with time" (90). Much like Kuhn's paradigms, in their sense of

"disciplinary matrix," Kitcher's consensus practices function as shared ideas to govern the conduct of science. Indeed, Kitcher compares "paradigms" and "consensus practices" in a footnote:

> There are obvious links between my notion of consensus practice and the Kuhnian concept of paradigm. Like Kuhn I take what is shared among scientists to be far more that the acceptance of a set of statements. However, Kuhnian paradigms are intended *both* to offer a richer description of the state of a science at a time *and* to divide the history of a scientific discipline into segments, such that there are significant epistemological differences between the course of science within a segment and the intersegment transitions. My approach bears no commitment to this second function. (Kitcher 87)

Later in The Advancement of Science, Kitcher generalizes the "Kuhnian idea . . . that scientists are bound together by shared values, by proposing that, in our dedication to inquiry, we endorse a shared system of goals" (93). He goes on to compare Kuhn's ideas about conceptualization and perception as expressed in Structure with those of Feyerabend, focusing specifically upon "the relations among scientific languages, and . . . the thesis of conceptual rather than perceptual incommensurability," and attacking Kuhn's notion that "'conceptual incommensurability' . . . engenders problems of communication and of reporting of evidence" (Kitcher 97). He disagrees with Kuhn's views on the notions of scientific truth, suggesting instead that the correspondence theory of truth has "its roots" in everyday scientific practice (Kitcher 129-31). He reviews Kuhn's notion of "the theory-ladenness of observation," labeling it "pessimistic" (Kitcher 223-225), but ultimately sustainable when applied to "the logical empiricist conception of observational evidence and observation language"

(Kitcher 227). Kitcher reexamines "the transformation of puzzles into anomalies," writing that much of his discussion "is deliberately reminiscent" of Kuhn's language without retaining Kuhn's commitment to sharply distinguishing between episodes of normal and revolutionary science (260); Kitcher substitutes his own language and imagery of "escape tree" for Kuhn's "puzzle/anomaly distinction" (262). These cross-currents appear throughout Kitcher's book, especially in his examination and reinterpretation of several of Kuhn's examples from Structure.

In making his appeal for the reinvigoration of "Legend," Kitcher relies on Kuhn's work, especially Structure and Essential Tension, to provide a starting place for the investigation of Legend's role in scientific advance. Unlike Wallen, who roundly condemns Kuhn's theory as impractical and impossible, Kitcher agrees with Kuhn on a number of points and credits Kuhn with initiating some much needed inquiry into the constitution of scientific progress. He may disagree with Kuhn on key issues such as the role of incommensurability or substitute his own language--"consensus practices" and "escape tree"--for that which Kuhn might use, but he writes favorably of Kuhn in general and suggests that Kuhn's theory has made a lasting impression on investigations into the history of science. A knowledge of Kuhn's work seems an essential part of the world that Kitcher paints in The Advancement of Science.

Arguing a Pro-Kuhnian Position

In the 1993 Paradigms and Barriers: How Habits of Mind Govern Scientific Beliefs, Howard Margolis presents a decidedly pro-Kuhnian view of paradigms, shifts, and incommensurability. Margolis sets out to explore "paradigm shifts" as they relate to habits of mind. By "habits of mind," he "suggests entrenched responses that ordinarily occur without conscious attention, and that even if noticed are hard to change" (Margolis

7). According to Margolis, a habit of mind is "an intrinsically invisible (though not undetectable) component" of a Kuhnian paradigm (23), commonly coordinated tacitly across a community (19). Margolis argues that "a Kuhnian paradigm shift comes down to a breaking of some one particular habit of mind," the breaking of the "barrier" of his title (Margolis 2). He asserts that "shifting from one paradigm to another requires retraining intuitions, or reshaping habits of mind" (Margolis 7), especially those habits of mind that affect the evolution of scientific belief, and goes on to contrast two views of Kuhn's paradigm shift to account for changes of scientific habits of mind: the "barrier hypothesis" which equates to the breaking of a habit of mind, and the "gap" view, the notion that "some logically difficult gap" divides "an established theory from its challenger" (Margolis 3). In a series of chapters detailing many of Kuhn's own examples, Margolis presents a strong version of Kuhn's theory, expanding upon Kuhn's original contentions.

Margolis mentions that Paradigms and Barriers builds upon an earlier work, his 1987 Patterns, Thinking, and Cognition: A Theory of Judgment, in which he devotes an entire chapter to an interpretation of Kuhn's paradigm shift theory. In an appendix to Paradigms and Barriers, Margolis notes that Patterns provides "an account of thinking and judgment" which "reduces everything to pattern-recognition" ([201]). In learning to recognize patterns, students of science acquire the habits of mind that allow them to practice Kuhn's normal science and recognize the anomalies that suggest the onset of a revolution, a paradigm shift. Margolis narrows his focus in Paradigms and Barriers to highlight Kuhn's theory and argue that habits of mind form the most essential cognitive element affecting scientific change and belief.

For Margolis, a parallel exists between habits of mind and "physical habits, whose reality and importance for governing behavior no one doubts" (2). He builds a case for the plausibility of "the *identification* of habits of mind with physical habits" and

aggressively pursues "the parallels between the two" (3), focusing on the properties of such habits. These properties include notions of rivalry (the competition between ideas), contagion (the spread of ideas), and taming (the resolution of conflicts between ideas). Acceptance of Margolis's argument demonstrating the connection between physical habits and habits of mind allows him to claim similar authority for habits of mind and extend that legitimacy to his explanation of paradigm shifts.

Margolis contends that his "habits-of-mind claim implies the possibility of the phenomena Kuhn has captured under the notion of 'paradigm shifts' and hence suggests . . . what is going on when we observe the stresses that characterize such shifts" (3). He argues that "ordinarily a particular, identifiable habit of mind is critical for the emergence and contagion of just those new ideas capable of producing the marked symptoms of Kuhnian incommensurability" (Margolis 3). Margolis refers to Kuhn's "incommensurability" in its sense that different members within a community often "talk past each other" (21) and "see the world differently, in some way that is not directly observable (since habits of mind are invisible and operate below the level of consciousness) and is tied to no reasonable definition of where their interests lies" (20). Thus Kuhn's paradigm shift refers to "a special sort of shift of habits of mind" (2), a reconfiguration of patterns of scientific thought that reflects the operation of Kuhn's notion of incommensurability.

Margolis proceeds to develop the barrier hypothesis as "the central feature of radical discovery," constituting a scientific revolution (3). As a barrier, a habit of mind acts as a "deeply entrenched cognitive propensity" (Margolis 6) to discriminate against the recognition of anomaly. Although usually "habits of mind serve as facilitators of effective thinking, not as barriers, just as physical habits ordinarily facilitate effective performance, not impede it" (4), Margolis contends that habits of mind "must also powerfully constrain intuition in ordinary conditions, where there does not seem to be

any rationally puzzling oddity to be explained." To support Kuhn's theory, Margolis tries to show that the interaction of habits of mind "with the characteristic features of scientific communities, can be expected to severely constrain what beliefs can be sustained over time" by such communities (6).

In his first three chapters, entitled respectively "Habits of Mind," "Paradigms," and "Barriers," Margolis details his own interpretation of Kuhn's theory, offering a cogent explication of the relationship between habits of mind and paradigms. He argues that his habits of mind explanation reinforces Kuhn's own growing interest in the notion of incommensurability broached in Structure and often held by Kuhn's critics "to be particularly open to doubt" (Margolis 21). Interested in examining how a "radical change of belief sometimes takes hold in a community," Margolis asserts:
"When scientific ideas change smoothly, we assimilate the episode to the "normal" end of Kuhn's normal-to-revolutionary spectrum. When change occurs in a wrenching way that provides conspicuous evidence of incommensurability . . . we have cases of Kuhn's revolutionary science" (Margolis 21). Such statements suggest Margolis's outright acceptance of Kuhn's framework for scientific advance, especially in relation to Kuhn's sense of scientific community. Indeed, Margolis assumes that in some "straightforward sense . . . scientific knowledge is socially constructed, since science could hardly be pursued except under favorable conditions" (216).

Given his broad acceptance of Kuhn's theory, Margolis argues, sometimes convincingly, that Kuhn's framework of scientific advance and concept of incommensurability works effectively to explain revolutionary revisions of scientific thought. He uses the concept of habits of mind, in the sense of precursors or components of Kuhn's paradigm, to advocate for Kuhn's version of science. Margolis's support perhaps represents the most favorable view of Kuhn within the history and philosophy of science. Rather than attack Kuhnian notions of paradigm, shift, revolutions, and

incommensurability, Margolis offers an extremely favorable schema for understanding Kuhn's theory.

Kuhn, Incommensurability, and Structure Today

> Kuhn's The Structure of Scientific Revolutions . . . has stood for three decades as the paradigm of a revolution in the field of the history of science.[29]
>
> --Robert Rothschild, 1992

While deeply flattered by the critical attention Structure has merited over the three decades since its publication, Kuhn has not remained unaware of potential misapplications and misreadings of Structure, nor has his theory remained static. Now a professor emeritus at the Massachusetts Institute of Technology, he has continued his active interest in the history and philosophy of science. Kuhn's 1991 lecture, The Trouble with the Historical Philosophy of Science, appearing in print in 1992, addresses some of the troubling problems Kuhn acknowledges still need attention. In Trouble, Kuhn mentions the direction his own research is taking him. In his 1993 essay, "Afterwords," appearing in Paul Horwich's 1993 World Changes: Thomas Kuhn and the Nature of Science, Kuhn offers further remarks about the continuing evolution of his thought while again responding to his critics and supporters. Horwich's World Changes consists of contemporary essays on Kuhn reminiscent of earlier collections of Kuhn's critics and supporters such as Lakatos and Musgrave's 1970 Criticism and the Growth of Knowledge. Like Criticism, Horwich's World Changes closes with Kuhn addressing critics and further elucidating on his work. The Trouble lecture and

"Afterwords" essay illustrate the extent of Kuhn's changes to his original theory, especially his increasing emphasis on incommensurability.

Kuhn's Troubles with the Historical Philosophy of Science

In his 19 November 1991 polemic, The Trouble with the Historical Philosophy of Science, a Rothschild Distinguished lecture published in 1992 by the Harvard University Department of the History of Science, Kuhn acknowledges his own contribution to a radical transformation of "the image of science current both inside and, less completely, outside the academy," a change that Kuhn claims "has . . . begun to yield a far more realistic understanding of what the scientific enterprise is, how it operates, and what it can and cannot achieve than was available before" (Trouble [3]). The transformation has included replacing a "philosophical tradition" of science as "a static body of knowledge" with "a dynamical one," an image of science as "an ever developing enterprise or practice" (Kuhn, (Trouble 10). But Kuhn expresses his discomfort about a "by-product" of the transformation--the "damagingly mistaken" views "emphasized and developed by people who often call themselves Kuhnians" (Trouble [3]). In Trouble, Kuhn delivers perhaps one of the most scathing rebukes to borrowers of his theory, describing what he thinks "went wrong," sketching how further damage "might be avoided," and suggesting the direction of his own thoughts (Trouble 4).

Kuhn's description revolves around his own experience as a "theoretical physicist with a strong avocational interest in philosophy and almost none in history" developing into a scholar "devoted to history of science." Like many of his contemporaries, he began with a traditional understanding of science as proceeding "from facts given by observation" (Kuhn, Trouble 4), facts discovered "prior to the scientific laws and theories for which they provide the foundation, and which are themselves, in turn, the

basis for explanations of natural phenomena." One engaged in the human process of interpreting the facts to construct scientific laws, theories, and explanations, performing "something called the scientific method" to discover as "true" as possible "generalizations about and explanations for natural phenomena." Kuhn suggests that attempts "to refine" an understanding of the scientific method drove him and many contemporaries "to observations of scientific life and to history" where they were "considerably disconcerted" by their findings (Trouble 5).

Kuhn summarizes those findings. First, "the supposedly solid facts of observations turned out to be pliable"; that is, different people "apparently observing" the same phenomena arrived at different results (Kuhn, Trouble 5). Further, Kuhn argues that investigators of the history of science found that "the so-called facts proved never to be mere facts, independent of existing belief and theory," thus observation "always left room for disagreement about whether some particular law or theory should be accepted." Third, the historical record also indicated that "individuals committed to one interpretation or another sometimes defended their viewpoint in ways that violated their professed canons of professional behavior." Using these aspects of scientific life as "a plausible starting point," Kuhn writes that he and others began "seeking behavioral clues with which to reform" the prevailing tradition of science (Trouble 6).

Kuhn suggests that investigators of the history of science have focused on "the process by which differences of belief were reconciled *en route* to an ultimate consensus within the group." They ask, "What is the process, that is, by which the outcome of experiments is uniquely specified as fact and by which authoritative new beliefs--new scientific laws and theories--come to be based upon that outcome?" Kuhn attributes such inquiry to the generation following him, noting that they have used historical and sociological perspectives to study the negotiation processes "within a scientific community or group from which an authoritative consensus finally emerges."

Unfortunately, such studies have only deepened "the very difficulty they were intended to resolve" (Kuhn, Trouble 7).

Negotiation, according to Kuhn, "seeks to establish . . . the facts from which scientific conclusions should be drawn, together with the conclusions--the new laws or theories--which should be based upon them" (Trouble 7). It operates in a circular manner, drawing concurrently from its factual and interpretive aspects, "the conclusions shaping the description of facts just as the facts shape the conclusions drawn from them." However, the individual experiences that affect the conclusion making process "are not in principle accessible to reasoned argument or negotiation" according to Kuhn. Thus, many liken negotiations in science to negotiations in other aspects of society, arguing that personal interest governs such negotiations and that "considerations of power and authority" determine outcomes (Kuhn, Trouble 8).

Kuhn agrees that "Interest, politics, power, and authority undoubtedly do play a significant role in the scientific life and its development," but laments that the form taken by many studies of negotiation in science have "made it hard to see what else may play a role as well" (Trouble 8). The strong form of the negotiation movement, Kuhn argues, asserts that only power and interest influence the process: "What passes for scientific knowledge becomes, then, simply the belief of the winners." Kuhn labels the claims of such a strong program "absurd," calling such claims "an example of deconstruction gone mad" (Trouble 9).

Kuhn also finds fault with "the more qualified sociological and historical formulations" striving to replace the strong program and account for the role of "observations of nature" in scientific development. He argues that the more qualified views "remain totally uninformative about that role," failing to account adequately for the way "in which nature enters the negotiations that produce beliefs about it" (Kuhn, Trouble 9).

In the second major section of his lecture, Kuhn moves to a discussion of his current work. For Kuhn, the replacement of a static image of science with a dynamic one has brought the realization that "many of the most central conclusions" drawn from "the historical record can be derived from first principles." He suggests that "an approach from principle yields . . . a very different view of what's at stake in the evaluative processes that have been taken to epitomize such concepts as reason, evidence, and truth" (Kuhn, Trouble 10). For both historians and philosophers of science, the fundamental problem becomes "understanding small incremental *changes* of belief." Kuhn states that the primary question that investigators must ask is: "Why, that is, given the body of belief with which they began, do the members of a scientific group elect to alter it, a process that is seldom a mere addition but ordinarily calls for the adjustment or abandonment of a few beliefs already in place?" Such a question indicates the gap between two philosophical points of view--"the rationality of belief" and "the rationality of incremental change of belief" (Kuhn, Trouble 11).

Kuhn chooses to touch on three of the differences between evaluating belief and evaluating change in belief. First, past "tradition supposed that good reasons for belief could only be supplied by neutral observations, the sort of observations, that is, which are the same for all and also independent of all other beliefs and theories." Unfortunately, few observations exist that can satisfy that criterion in Kuhn's opinion. Kuhn asserts: "From the historical perspective, however, where change of belief is what's at issue, the *rationality* of the conclusions requires only that the observations invoked be neutral for, or shared by, the members of the group making the decision, and for them only at the time the decision is being made" (Trouble 11). According to Kuhn, the observations involved need only be independent of those "prior beliefs . . . that would be modified as a result of the change" (Trouble 11-12). The highest criteria for the rationality of the discussion over change rests in the shared beliefs of the participants.

Changes in community or culture and movement over time do not interfere in the ability of the historical perspective to provide "a basis for reasoned discussions and evaluations of proposed changes in the body of belief current in a given community at a given time" (Kuhn, Trouble 12).

The second difference Kuhn examines concerns the graduated steps by which changes in perspective take place. Kuhn stipulates, "From the historical perspective the changes to be evaluated are always small." All changes "have been prepared for gradually, step by step, leaving only a keystone to be put in place by the innovator whose name they bear." Kuhn records that "Many of the considerations that suggested the nature of the change to the innovator are also the ones that supply reasons for accepting the proposal he or she has made" (Trouble 12).

Finally, Kuhn supplies a "dogmatic assertion" to address the third "effect of shifting from belief to change of belief." Under the previous tradition, investigators generally "evaluated" beliefs "for their truth or for their probability of being true, where truth meant something like corresponding to the real, the mind-independent external world" (Kuhn, Trouble 12). Lack of time forces Kuhn to assert that such a formulation of evaluation "fails to account for essential aspects of scientific development" (Trouble 13).

Assuming truth "to be the goal of evaluations," according to Kuhn, requires indirectly evaluating "a newly proposed law or theory" against reality. Kuhn argues that evaluation means "one must embed it [the newly proposed law or theory] in a relevant body of currently accepted beliefs . . . and then apply to the whole a set of secondary criteria." The secondary criteria include accuracy, consistency, applicability, and simplicity as well as others. Secondary criteria "are equivocal . . . and rarely all satisfied at once" (Kuhn, Trouble 13).

Secondary criteria, although equivocal, increase in significance when applied to a change in belief rather than to belief. When comparing sets of beliefs within the "historical situation," Kuhn notes:

> To ask which of two bodies of belief is *more* accurate, displays *fewer* inconsistencies, has a *wider* range of applications, or achieves these goals with the *simpler* machinery does not eliminate all ground for disagreement, but the comparative judgment is clearly far more tractable than the traditional one from which it derives. (Kuhn, Trouble 13)

Still, Kuhn declares that the accuracy, consistency, applicability, and simplicity of one set of beliefs does not necessarily make that set "any truer" or "more probable" (Trouble 14).

Ultimately, Kuhn argues against the conception of scientific truth as something "like correspondence to reality." He attacks the notion that "successive scientific law and theories grow closer and closer to the truth," asserting the ambiguity of the meaning of the phrase, "closer to the truth." Kuhn suggests that "facts are not prior to conclusions drawn from them and those conclusions cannot claim truth" (Trouble 14). Thus, he concludes that power and interest should not replace evidence and reason as proponents of the strong program have suggested; other influences come to bear as well on scientific advance.

Kuhn shifts gears once again to provide additional "excessively condensed remarks" about his historical or developmental perspective on scientific advance. He observes that human history has seen "the apparently inexorable (albeit ultimately self-limiting) growth in the number of distinct human practices or specialties." He describes this growth as "speciation," though he admits that no precise "parallel" exists "to biological

evolution" (Kuhn, Trouble 15). He sees the "proliferation of specialties" as "especially prominent" in the sciences and perhaps more obvious as well. The substructuring of various professions results in "the elaborate and ramshackle structure of separate fields, specialties, and sub-specialties within which the business of producing scientific knowledge is carried forward" (Kuhn, Trouble 16). Kuhn declares:

> Knowledge production is the particular business of the sub-specialties, whose practitioners struggle to improve *incrementally* the accuracy, consistency, breadth of applicability, and simplicity of the set of beliefs they acquired during their education, their initiation into practice. It is the beliefs modified in this process that they transmit to their successors who carry on from there, working with and modifying scientific knowledge as they go. (Kuhn, Trouble 16)

In essence, Kuhn argues for picturing "human practices in general and scientific practices in particular" as roughly similar to an "evolutionary tree" developing over time. He assigns "'political' characteristics" such as power, authority, and interest to the earliest stage of the evolution of various practices and suggests that "all human practices" share the early characteristics. Kuhn asserts that later in the evolutionary pattern, "at some developmental branching point," other characteristics appear. Identifying the sciences as a major branching involving "several branch points and much recombination," Kuhn characterizes the sciences by their concern for "the study of natural phenomena" and their use of the "evaluative procedures" he has already described--"accuracy, consistency, breadth of application, simplicity, and so on--characteristics that are passed, together with illustrations, from one generation of practitioners to the next" (Trouble 17).

Kuhn concludes that the "trouble with the historical philosophy of science," the topic he initially sets out to explore in his lecture, rises out of its use of the historical record to undermine "the pillars on which the authority of scientific knowledge was formerly thought to rest without supplying anything to replace them." By undermining the notion that "facts are prior to and independent of the beliefs for which they are said to supply the evidence" and the idea that the practice of science reveals "truths, probable truths," or approximations to the truth about a mind- and culture-independent external world," the historical philosophy of science has allowed the traditional authority of scientific knowledge to collapse. Kuhn's solution for the difficulties caused by such a collapse begins with the recognition that those difficulties "are necessary characteristics of any developmental or evolutionary process" (Trouble 18).

Kuhn reiterates the three main aspects needed to reconceptualize scientific advance. First, he asserts that scientists "produce and evaluate . . . change in belief," not belief. Second, evaluation aims to select "simply . . . the better or best of the bodies of belief actually present to the evaluators at the time their judgments are reached." Finally, he calls for "abandoning the view of science as a single monolithic enterprise, bound by a unique method" (Trouble 18). Instead, Kuhn argues that we should look at science as "a complex but unsystematic structure of distinct specialties or species, each responsible for a different domain of phenomena, and each dedicated to changing current beliefs in their domain in ways that increase accuracy and the other standard criteria . . . mentioned" (Kuhn, Trouble 18-19). Under such a vision, Kuhn contends that the essentially plural nature of the sciences "can be seen to retain a very considerable authority" (Trouble 19).

With the shift of gears toward the historical or developmental perspective and in Kuhn's final coda, more than anywhere else throughout the bulk of the Trouble lecture, we see major elements of Kuhn's earlier theory repeated in toned down form. He does not

use "paradigm," "exemplar," or "normal science," yet some sense of those elements remains present. Talk about "sets of beliefs" acquired through scientific education and the passing down of "illustrations" to the succeeding generation seems to reek of "disciplinary matrix" and "exemplar," Kuhn's two major senses of paradigm as expressed in the second edition of Structure and several of his 1969 essays. Practitioners struggling "to improve incrementally" seems a clear, though muted reminder of "normal science." Kuhn seems to retain much of the pattern of scientific education and initiation as he initially mapped it out. In a sense, Kuhn has backed away from some of the language he raises in Structure but not so much from the elements of his original theory.

While Kuhn does not seem to place as much emphasis on the role of paradigms as he once did, his emphasis on his notions of "revolutionary change" and "incommensurability" persists, and, if anything, has grown stronger. He continues to emphasize "revolutionary change," associating "scientific revolutions" intimately with his new concept of "speciation." Kuhn argues that "revolutions directly displace some of the concepts basic to the earlier practice in a field in favor of others" and result in "a narrowing of focus." But practices "permitted by the new concepts" never completely replace the entire field, and pursuit of the "residue" left by the shift "continues as an increasingly distinct specialty," eventually doomed to "die off" (Kuhn, Trouble 19).

Kuhn's notion of "incommensurability" continues to operate to distinguish between specialties, widening the "conceptual disparity between the tools employed in the . . . specialties." Kuhn contends: "Once . . . two specialties have grown apart, that disparity makes it impossible for the practitioners of one to communicate fully with the practitioners of the other" (Trouble 20). Kuhn's image of a communication breakdown remains. Unfortunately, he limits his comments to these necessarily curt and dogmatic statements about the impact of incommensurability upon practice.

The world of scientific practice that Kuhn briefly posits at the end of his lecture is "not independent of mind and culture," nor "a single coherent whole" inhabited by "the practitioners of all the individual scientific specialties." In this new world, "the practitioners of these various specialties practice their trade" within "niches, which both create and are created by the conceptual and instrumental tools" used by those practitioners. For Kuhn, those niches "are as solid, real, resistant to arbitrary change as the external world was once said to be" (Trouble 20).

Kuhn's "Afterwords" about World Changes

Paul Horwich's 1993 World Changes: Thomas Kuhn and the Nature of Science consists of ten essays by historians and philosophers of science examining Kuhn's work and Kuhn's reply, "Afterwords," a response to their criticism and an indication of the current direction of Kuhn's thoughts. Horwich notes in his introduction to the collection that Kuhn's Structure "called into question many of the central elements of the traditional picture" of science and "provided an alternative model of scientific change" that dispensed with traditional views of "the concept of absolute truth, the observation/theory distinction, the determinacy of rational choice, and the normative function of philosophy of science." Kuhn's "radical views" became the focus of much of the debate "not only by philosophers, historians, and sociologists of science but also by large numbers of practicing scientists" (Horwich 1).

Horwich's volume grows out of philosophical and historical papers about Kuhn's "radical views" presented at a 1990 conference at the Massachusetts Institute of Technology. The contributors revised their original papers for inclusion in the Horwich volume to which Kuhn added his "Afterwords" essay. Rather than discuss each of the essays separately, I concentrate on Kuhn's "Afterwords" as one of the most recent

statements available about his original theory and the shifts that theory has undergone over the years. My review of "Afterwords" is necessarily reductionist in that I do not focus on all of his responses to various criticisms in the essays contained in Horwich's book; I simply want to capture the flavor of Kuhn's changes and consistencies with the theory originally published in Structure.

"Afterwords" opens with an acknowledgment of the historical beginnings of Kuhn's theory, especially Kuhn's relationship with C. G. Hempel and Kuhn's disagreement with R. Carnap. Hempel's 1957 Fundamentals of Concept Formation in Empirical Science had a "considerable" role in Kuhn's own "intellectual development" (312). Kuhn points out that Hempel's work contains four "essential elements" of Kuhn's own developed position:

> [S]cientific terms are regularly learned in use; that use involves the description of one or another paradigmatic example of nature's behavior; a number of examples are required for the process to work; and, finally, when the process is complete, the language or concept learner has acquired not only meanings but also, inseparably, generalizations about nature. (Kuhn, "Afterwords" 312)

Further, Kuhn writes that Hempel, in later works, implicitly adopts "a developmental or historical stance" to the philosophy of science through his changes in language, thus he sees Hempel as sharing some of ideas Kuhn articulates (312).

Kuhn refers to language issues a second time when he responds to "Carnap, Kuhn, and the Philosophy of Scientific Methodology," John Earman's essay in World Changes showing the parallels between Carnap and Kuhn. Kuhn maintains that a "deep difference" exists between Carnap's position and his own on the significance of language (313). Both emphasize the "untranslatability" of the language of competing theories. But Carnap sees

the "cognitive importance" of language change as "merely pragmatic" while Kuhn labels it "cognitively significant" (314). According to Kuhn:

> Concerned from the start with the *development* of knowledge, I have seen each stage in the evolution of a given field as built--not quite squarely--upon its predecessor, the earlier stage providing the problems, the data, and most of the concepts prerequisite to the emergence of the stage that followed. In addition, I have insisted that some changes in conceptual vocabulary are required for the assimilation and development of the observations, laws, and theories deployed in the later stage. . . . Given those beliefs, the process of transition from old state to new becomes an integral part of science, a process that must be understood by the methodologist concerned to analyze the cognitive basis for scientific beliefs. (Kuhn, "Afterwords" 314)

Although Kuhn does not employ any of his extensively borrowed vocabulary in this passage--terms such as paradigm, shift, normal science, and revolution--much of the essence of his original theory still shines through as he argues for the cognitive significance of language.

Kuhn notes that critics of Structure have frequently criticized him for attempting "to undermine the cognitive authority of science" because Structure "had little constructive to say about how the transition between stages comes about or what its cognitive significance can be." He promises to provide better responses to these issues, using "Afterwords" as a vehicle to suggest the changes in his position since the publication of Structure. Much of that change concerns his notions of "incommensurability and the nature of the conceptual divide between the developmental

stages separated by what [he] once called 'scientific revolutions'" (Kuhn, "Afterwords" 314).

Kuhn considers incommensurability to be "the central innovation introduced" by Structure. Since first publishing his concept, he admits that efforts "to understand and refine it [incommensurability] have been [his] primary and increasing concern" ("Afterwords" 315). He indicates that the concept still poses a range of problems, the first of which Kuhn addresses as the nature of the different world that exists after a change in concepts.

When considering the nature of the world change, Kuhn refers to Ian Hacking's introduction of "scientific kinds" in "Working in a New World: The Taxonomic Solution" ("Afterwords" 315). Hacking asserts that a "nominalist resolution" exists to the problem of a scientific community's change of world view; he concludes that the "world in and with which we work is a world of kinds. . . . After a scientific revolution, the scientist works in a new world of kinds" (Hacking 306). Further, Hacking defines "scientific kinds" as those proper containments "that some group of scientific specialists finds relevant for its investigations during some period," including "kinds of apparatus and instruments" (283-85). But Kuhn rejects Hacking's taxonomy of "scientific kinds."

Instead of "scientific kinds," Kuhn suggests that "what is required is a characteristic of kinds and kind-terms in general." He indicates that "this characteristic can be traced to, and on from, the evolution of neural mechanisms for reidentifying what Aristotle called 'substances': things that, between their origin and demise, trace a lifeline through space over time." Kuhn uses the term "lexicon" to mean the "mental module" possessed by members of a speech community "that permits us to learn to recognize not only kinds of physical object . . . but also kinds of furniture, of government, of personality, and so on" ("Afterwords" 315). Kuhn mentions that he intends in his next book to move away from the broader idea of "language change" to talk "of change in concepts and their

names, in conceptual vocabulary, and in the structured conceptual lexicon that contains both kind-concepts and their names" ("Afterwords" 316).

For Kuhn, changes in the conceptual lexicon represent changes "in convention rather than fact" ("Afterwords" 318). He argues: "Kind terms supply the categories prerequisite to description of and generalization about the world," and notes that when "communities differ in their conceptual vocabularies, their members will describe the world differently" ("Afterwords" 319). While he disagrees with Hacking somewhat, Kuhn finds enough in his suggestions to use as fodder for a better expression of an issue connected to Kuhn's own sense of incommensurability.

Next, Kuhn turns toward refuting some of the points offered by John Heilbron in "A Mathematicians' Mutiny, with Morals." Kuhn declares that Heilbron's remarks about a historian's vocabulary "seem mistaken" ("Afterwords" 320). Kuhn sketches his own understanding of the historian's methodological practice of building narratives and engaging in "language teaching," that is in setting the stage: "To communicate . . . results to readers, the historian becomes a language teacher and shows readers how to use the terms . . . current when the narrative began but no longer accessible in the language shared by the historian and his or her readers" ("Afterwords" 320). Kuhn points out the inapplicability of modern scientific terms, such as the names of modern scientific fields, "when describing past scientific beliefs"; the "introduction of modern conceptual distinctions" leads directly to misreadings of those beliefs in Kuhn's opinion ("Afterwords" 321-22). If historical "narrative are by their nature intensely selective," Kuhn declares: "Historians are required to include in them only those aspects of the historical record that affect the accuracy and plausibility of their narrative" ("Afterwords" 323).

In a discussion of Jed Z. Buchwald's "Design for Experimenting," Kuhn moves from the language of historical narratives to an admission that he "was wrong to speak of

translation" in his "Postscript" essay in the second edition of Structure. Kuhn writes: "In Structure I spoke of meaning change as a characteristic feature of scientific revolutions; later, as I increasingly identified incommensurability with difference in meaning, I repeatedly referred to the difficulties of translation" ("Afterwords" 324). He now realizes that in "Postscript," he describes the process of language learning, "a process that need not, and ordinarily does not, make full translation possible" (Kuhn, "Afterwords" 324). Comparing literature with science, Kuhn argues: "For both literature and science . . . the difficulties in translation arise from the same cause, the frequent failure of different languages to preserve structural relations among words, or in the case of science among kind-terms" ("Afterwords" 324). Ultimately, "the criteria for determining the reference of scientific terms" depends upon these structural relations (Kuhn, "Afterwords" 324).

The issue of structure also receives more of Kuhn's attention in his discussion of Buchwald's essay. Kuhn asserts that part of the lexicon that members of a particular scientific community share consists of "a set of learned expectations about the similarities and differences between the objects and situations that populate their world" ("Afterwords" 325). Community members may arrive at the same results given their recognition of the various similarities and differences, but do not have to use the same expectations or techniques to arrive there. According to Kuhn: "The lexicons of various members of a speech community may vary in the expectations they induce, but they must all have the same *structure*. If they do not, then mutual incomprehension and an ultimate breakdown of communication will result" ("Afterwords" 325-26). Kuhn contends that when "lexical structures differ, then what appears to be a disagreement about fact (which kind does a particular item belong to?) proves to be incomprehension (the two are using the same name for different kinds)." The participants in the

communications breakdown "fail to share . . . not so much belief as a common culture" ("Afterwords" 326).

Kuhn focuses initially on the role of culture when he moves from his discussion of Buchwald to responding to M. Norton Wise's "Mediations: Enlightenment Balancing Acts, or the Technologies of Rationalism." He suggests that Wise's research is incomplete at present and thus reserves judgment about it. More importantly, Kuhn notes that Wise's look at rationalist scientific culture "involves a damaging category mistake" in the movement from individual to group, a mistake that Kuhn admits to making "repeatedly" in Structure. Kuhn identifies the mistake as treating "groups as individuals writ large or else individuals as groups writ small," especially in talking about "the group mind" or "group interest" or "in attributing to the group a characteristic shared by all or most of its members." Kuhn acknowledges making the egregious error In Structure of repeatedly referring to "gestalt switches as characteristic of the experience undergone by the group." He labels the error as grammatical in nature--"A group would not experience a gestalt switch even in the unlikely event that every one of its members did so." Kuhn uses this admission to argue for more investigation into "ways of understanding and describing groups that do not rely upon the concepts and terms we apply unproblematically to individuals" ("Afterwords" 328).

Kuhn lists two "steps" he has taken towards achieving the understanding for which he calls. The first rests in his characterization of lexicons and lexical structures as integral parts of a scientific community's world view and the notion that community members do not have to arrive the same conclusion through the same route or expectations. The second step concerns Kuhn's "discovery that the puzzles about the relation of group members to group have a quite precise parallel in the field of evolutionary biology: the vexing relation between individual organisms and the species to which they belong." Kuhn suggests that the example of evolutionary biology "contains

important clues to the sense in which science is intrinsically a community activity" ("Afterwords" 329).

In the last half of "Afterwords," Kuhn returns to the notion of incommensurability and the often heard charges of relativism leveled against him. He summarizes the double goal of his inquiry:

> On the one hand, I aim to justify claims that science is cognitive, that its product is knowledge of nature, and that the criteria it uses in evaluating beliefs are in that sense epistemic. But on the other, I aim to deny all meaning to claims that successive scientific beliefs become more and more probable or better and better approximations to the truth and simultaneously to suggest that the subject of truth claims cannot be a relation between beliefs and a putatively mind-independent or "external" world. (Kuhn, "Afterwords" 330)

Kuhn declares that claims of science "zeroing in on truth, getting closer and closer to, the truth. . . . are meaningless" given the consequences of incommensurability ("Afterwords" 330). Kuhn's emphasis remains on the value of a community's lexicon in determining knowledge.

Further, Kuhn argues: "A lexicon or lexical structure is the long-term product of tribal experience in the natural and social worlds, but its logical status, like that of word-meanings in general, is that of convention" ("Afterwords" 330). Kuhn expresses interest in the socialization process by which that "conceptual vocabulary is transmitted from one generation to the next." He asserts that such transmission from generation to generation depends upon "the exhibition of concrete examples," drawn either "by pointing to real-life examples in the everyday world or the laboratory or else by describing these potential examples in the vocabulary antecedently available to the

student or inductee." A knowledge of "the kind-concepts of a culture or subculture" brings with it "the world in which members of the culture live" (Kuhn, "Afterwords" 333-34). Similarly, "changes in lexical structure" bring correspondingly changed "professional practice and a different world within which to conduct it" (Kuhn, "Afterwords" 335).

Following up on his original concept of scientific "revolutions," the episodes leading to fundamental changes in professional worlds, Kuhn reduces the emphasis he originally placed upon such "developmental episodes" in Structure. Though he still sees these revolutionary episodes as transforming, Kuhn relegates their importance to the role of marking increasing speciation among scientific specialties. Kuhn notes that such episodes "are often, perhaps always, associated with an increase in the number of scientific specialties required for the continued acquisition of scientific knowledge." Further, he argues that the empirical evidence "is overwhelming: the development of human culture, including that of the sciences, has been characterized since the beginning of history by a vast and still accelerating proliferation of specialties." Ultimately, "The transition to a new lexical structure, to a revised set of kinds," as Kuhn now characterizes such transformations leading to proliferation, "permits the resolution of problems with which the previous structure was unable to deal" (Kuhn, "Afterwords" 336).

Kuhn's new concept of development by proliferation or speciation, also articulated somewhat in Trouble, continues to support his views on incommensurability. According to Kuhn, "a practice and its world . . . are interdefined; neither component . . . can be known without the other." With the increase in speciation comes the corresponding increase in "the inability of residents" of different specialties "to crossbreed" as well as "difficulty of communication between practitioners of different specialties" (Kuhn,

"Afterwords" 337); in other words, the positions of different specialties become incommensurable.

Finally, Kuhn closes with a restatement of his original point from Structure about scientists as puzzle solvers:

> [W]hether or not individual practitioners are aware of it, they are trained to and rewarded for solving intricate puzzles--be they instrumental, theoretical, logical, or mathematical--at the interface between their phenomenal world and their community's beliefs about it. That is what they are trained to do and what, to the extent they retain control of their time, they spend most of their professional lives doing. (Kuhn, "Afterwords" 338)

Kuhn may abandon the terminology of "normal science" that others found objectionable in his work, but he still retains a definite sense of that science as puzzle-solving.

In the end, Kuhn concludes: "The pursuit of puzzle solving constantly involves practitioners with questions of politics and power, both within and between the puzzle-solving practices, as well as between them and the surrounding nonscientific culture." These interests have always played a large role in human evolution. For Kuhn, "Puzzle-solving is one of the families of practices that has arisen during that evolution, and what it produce is knowledge of nature." Accordingly, "Those who proclaim that no interest-driven practice can properly be identified as the rational pursuit of knowledge make a profound and consequential mistake" (Kuhn, "Afterwords" 339).

Kuhn's "Afterwords" represents perhaps the best statement of where the theory originally expressed in Structure and extensively borrowed by other fields, often without reference to its reception within the history and philosophy of science, stands today. He continues to adhere to many of his original concepts even if he seems to have

abandoned the terminology associated with them. He no longer uses the terminology--the "paradigms," "disciplinary matrix," "exemplars," "normal science," and "shifts"--that filled Structure, although evidence of these terms continue to crop up in his diction. He retains his initial views about the practices of scientific education, stressing more the cognitive training that initiates into the sciences receive. Instead of the translation problems of "Postscript," he now focuses on language learning problems. Kuhn's clear dependence upon some notion of scientific community, or "speech community" as he now refers to it, still shines through. Finally, his notion of incommensurability, rather than scientific revolutions, now occupies the forefront of his inquiry. In short, Kuhn has retreated somewhat, but not very far from his original theory as expressed in the 1962 and 1970 editions of Structure.

If the disciplines of history and philosophy of science credit Kuhn for nothing else, they come to some accord that Kuhn's work has served to spur greater investigation into issues connected to scientific advance that had previously received little attention. The strength of Kuhn's claims forced both opponents and supporters to dig deeper into the constitution of scientific knowledge, the authority claimed for such knowledge-making, and the function of the greater scientific community in governing the individual scientific enterprise. Kuhn's exemplars are subjected again and again to analysis and interpretation of both his critics and supporters.

Kuhn's framework for the scientific enterprise and conclusions about scientific practices merit a wide range of critical attention. At one extreme, we find complete rejection of his ideas and their denigration as "failed," "impractical," and "impossible." At another extreme, we see Kuhn's praises sung and his gospel preached in fields far beyond the boundaries of the natural sciences; his borrowers teem in the social sciences, the humanities, and elsewhere. In yet another direction, we see his theory developed

even further, used to justify "pattern-recognition" and "habits of mind" while still exploring the implications expressed in the original Structure. Finally, we have Kuhn's own thoughts about the direction his continued inquiry has taken him--further and further into issues of language and incommensurability. If, as Rothschild has noted, Kuhn did indeed initiate a revolution in the history of science, that revolution has yet to play out.

CHAPTER EIGHT

IMPLICATIONS FOR INTERDISCIPLINARITY

Writing teachers should know everything; that is understood. [30]

--Charles Moran, 1982

As we see in the last chapter, Wallen's dissertation represents one extreme of views on Kuhn. Similarly, Barnes's work suggests another direction--the appropriation of Kuhn far beyond his original world of the history and philosophy of science. Most of the borrowings of Kuhn by compositionists throughout the eighties and into the nineties fall in the category suggested by Barnes. Concern for the disciplinary status of composition accompanies inquiry into issues connected with discourse communities, writing across the curriculum, the nature of composing and reading as social acts, collaborative learning, and the social construction of knowledge in composition. Exposure to Kuhn's ideas and his interpretation by various borrowers has become an accepted part of the curriculum in many advanced composition courses. Further, Kuhnian terminology has passed from its beginnings as a borrowing from Kuhn's field to being an integral part of the shared language used by the composition community. For example, in "College Composition and Communication: Chronicling a Discipline's Genesis," their 1993 chronicle of CCC's contributions to composition's development as a discipline, Donna B. Phillips, Ruth Greenberg, and Sharon Gibson argue:

> With the growth of rhetoric as a theoretical foundation for scholarship and the emergence of composition specialists, through professional choice or formal graduate programs, the meaning of *composition* [authors' emphasis] has been

> shifting from a course title to a conceptual *paradigm for an emerging discipline* [my emphasis]. (Phillips, Greenberg, and Gibson 461)

Later, Phillips, Greenberg, and Gibson refer to "revolution" as well. Like so many others in composition, they use language closely associated with the appropriation of Kuhn to focus on disciplinary concerns.

Phillips, Greenberg, and Gibson's concern for "disciplinary legitimacy" comes complete with a warning and a rejoinder. They note that with "the end of a revolution can come the end of unity among revolutionaries," a warning against the fragmentation of composition similar to that of North's bleak prediction in his The Making of Knowledge in Composition (Phillips, Greenberg, and Gibson 461). In opposition to that fragmentation, they propose continuing CCC's mission as "a forum for hearing the voices of the many, the few, and perhaps most importantly, the one" (Phillips, Greenberg, and Gibson 461).

At the same time they propose continuing the multi-voiced conversation within composition studies, Phillips, Greenberg, and Gibson suggest some of the influence that borrowers of Kuhn exert. Without giving any indication of the particular works cited, they list Richard Young, Patricia Bizzell, Janet Emig, and Maxine Hairston among the top fifty authors cited within the pages of CCC over the forty year period examined. Each of these authors has made a significant contribution to the appropriation of Kuhn by composition studies.

Contemporary Borrowings of Kuhn in Composition

> While some might consider a plethora of theories evidence that composition has lost its way, or is on the verge of losing itself in a miasma of theory, I'm inclined

> to see the theoretical excursions taken . . . as intellectual sorties, and the sorties as harbingers of an emerging sense among us that our right to govern our intellectual lives depends upon our willingness to theorize writing in the light of practice.[31]
>
> --Linda Brodkey, 1994

As we turn to the most recent period in composition studies, 1990-1994, what do borrowings of Kuhn's work look like? A knowledge of Kuhn has become a necessity for entering into much of the dialogue over inquiry within composition, achieving the status of a commonplace in the education of new compositionists. When seen from the perspective of Kuhnian language, the profession has adapted wholesale the terminology of paradigm and paradigm shift, usually severing its use of that terminology from Kuhn but retaining his sense of paradigm more often as disciplinary matrix than exemplar. Finally, and perhaps most importantly, citation of Kuhn to support discipline building in composition virtually disappears. Instead of applying Kuhn's ideas directly as evidence to buttress claims of disciplinehood for composition inquiry or seeking to cloak composition studies in the mantle of science's legitimacy as an academic discipline, contemporary borrowers tend to place an overall sense of his theory in the context of particular movements such as social construction and discourse communities.

Does Kuhn's importance as a borrowing fade as composition studies moves into the nineties? The pages of CCC still reflect a degree of Kuhn's influence on contemporary composition inquiry. The occasional debates over the applicability of Kuhn's ideas appear less frequently and are often framed within discussions of disciplinary boundaries, discourse communities, writing across the curriculum, poststructuralism, and social construction. To a great extent, Kuhn's ideas now appear couched in ways that suggest

some discipline-wide knowledge of him or at least the writings of his principal borrowers--usually Young, Bizzell, and Hairston.

A sampling of the articles appearing in the pages of CCC during the period 1990-1994 gives an idea of the uses that compositionists now make of Kuhn. For example, James Britton refers to Kuhn and his concept of paradigm shift in a reply to other symposium members in the 1990 "Re-Presenting James Britton: A Symposium." Britton notes that the "center" of John Warnock's commentary in the same essay, the "oedipal moment" Warnock relates when discussing the present state of composition inquiry, functions as "some kind of turning point, or what Thomas Kuhn might have called a paradigm shift: oedipal in the sense that it marks a transition that involves rejection of traditional practices, or the supplanting of father figure(s)" (Tirrell, et al 183). Michael Carter cites borrowings by Young and by Hairston in his argument for construction of a theory of expertise in writing based on "local knowledge." In his 1990 "The Idea of Expertise: An Exploration of Cognitive and Social Dimensions of Writing," Carter discusses the "revolutionary . . . shift" from product to process, asserting that local knowledge has a "crucial role" in any concept of expertise (276). Carter's bibliography contains several works that mention Kuhn. As already noted in chapter two, Richard Fulkerson proposes a four-part "disciplinary paradigm" for a "full 'theory' of composition" in his 1990 "Composition Theory in the Eighties: Axiological Consensus and Paradigmatic Diversity" (410). Like Carter, Fulkerson cites several borrowings of Kuhn including Hairston, Kinneavy, and North.

In 1991, Howard B. Tinberg refers directly to Kuhn and borrowings of him by Berlin and Bizzell in "'An Enlargement of Observation': More on Theory Building in the Composition Classroom." He argues that, "under the influence of Thomas Kuhn, many composition theorists have concluded that the field, historically, may be reduced to a collection of paradigms, with 'revolution' occurring in the shift from one paradigm to

another (Bizzell, Berlin)" (Tinberg 39). Tinberg demonstrates further knowledge of Kuhn through his citation of North's The Making of Knowledge in Composition. Others simply call themselves "Kuhnians" such as Cheryl Geisler in her 1992 "Exploring Academic Literacy: An Experiment in Composing" (49) or continue the tradition of appropriating Kuhn's language. Citing a number of other users of the term, paradigm, Maxine Hairston talks about "a new paradigm for the teaching of writing, one . . . focused on process and on writing as a way of learning" in her 1992 polemic against the politicizing of the composition classroom, "Diversity, Ideology, and Teaching Writing" (179). M. Jimmie Killingsworth cites Hairston's 1982 "Winds of Change" essay in his 1993 "Product and Process, Literacy and Orality: An Essay on Composition and Culture" when he writes about the "dominant paradigm of writing instruction, the so-called 'current-traditional' or 'product-oriented' model" (26) and Hairston's heralding of "the onset of the process revolution" (28). Michael A. Pemberton's 1993 "Modeling Theory and Composing Process Models" also raises questions about paradigms, paradigm shifts, and models while citing the work of Hairston and others. Pemberton distinguishes between models, theories, and paradigms, arguing:

> A model, for example, will attempt to describe a select assortment of data by suggesting an underlying pattern; a more general theory will characterize the operations and predictions of several illustrative models; and a paradigm will--theoretically--encompass all the theories, models, data, and methodologies that are jointly held by a given interpretive community. (Pemberton 43)

Articles such as Pemberton's also call the notion of paradigm into question. In a footnote, Pemberton writes that Robert J. Connors, in a 1983 article in College English, has pointed out that Kuhn's "concept of paradigm has fallen into some disrepute, even in the

domain of the physical sciences" (55). Connors's "Composition Studies and Science" is one of the few articles to cite criticism of Kuhn's work in the history and philosophy of science.

Later in the nineties, Richard E. Miller refers to Kuhnian concept when writing about the "incommensurability of the locations, conditions, and goals in academic and popular reading practices" in his 1994 "Composing English Studies: Towards a Social History of the Discipline"; he cites borrowers such as Stephen North, Susan Miller, and Robert J. Connors (171). In their 1994 "Evolving Paradigms: WAC and the Rhetoric of Inquiry," Judy Kirscht, Rhonda Levine, and John Reiff write of the "shift" away from the current traditional model in writing practice and theory that focused greater attention on writing in other disciplines (370). Exploring discipline-based writing, social construction, and the rhetoric of inquiry, Kirscht, Levine, and Reiff paraphrase Hairston's 1982 "winds of change" idea to assert that new winds have sprung up, "carrying with them fresh ways of seeing, new sets of questions, and the possibilities of shaping the field and the classroom" (379); these new ways require a recontextualization of the debate between *"writing to learn* and *writing in the disciplines,* through which we collectively and consciously reshape our paradigms of writing" (380).

If Kuhn's use seems more widespread within CCC articles, the book reviewers in CCC seem less willing to use Kuhn than those writing in the early eighties. In her 1990 review of David Bleich's The Double Perspective: Language, Literacy, and Social Relations, Joyce I. Middleton notes that Bleich "shapes a conceptual framework for talking about varied discourse styles and arguing for a new paradigm," assuming her readership's understanding of the term to carry her point (231). David Bartholomae refers to Susan Miller's use of Kuhn in a review of her 1991 Textual Carnivals: The Politics of Composition. Bartholomae notes Miller's argument that "the great 'paradigm'

shift of 'process research and 'process' pedagogy changed little" other than "celebrating an independent, isolated subjectivity and placing the act of writing apart from any recognizable historical or social context" (511). In a 1992 review of Joseph M. Moxley's Creative Writing in America: Theory and Pedagogy, Chuck Guilford quotes Moxley's contention that the discipline of creative writing "is preparing to undergo a paradigm shift, a period of self-reflexiveness in which we question our theories and practices" (Qtd in Guilford 106). Other book reviewers throughout the remainder of the period follow the same pattern, using terms such as paradigm or shift as part of the language of composition or referring to the use of Kuhn or his terminology in the books reviewed. No one makes any serious effort as in the mid-eighties to use Kuhn's concepts as the *basis* for reviewing a book.

In books, Kuhn's presence has tended to become part of the greater ecosphere within which composition studies exists. George L. Dillon mentions Kuhn's normal science, paradigms, shifts, disciplinary matrix, exemplars, and incommensurability in his 1991 Contending Rhetorics: Writing in Academic Disciplines, his assessment of academic discourse aimed at clarifying "how that discourse constructs not only knowledge but also value" (ix). Patricia Harkin and John Schilb's 1991 anthology, Contending with Words: Composition and Rhetoric in a Postmodern Age, contains a number of essays reflecting the influence of Kuhn's theories on composition studies. Harkin and Schilb mention Kuhn or his ideas in separate essays within the volume. Victor J. Vitanza adds his voice to theirs in his essay "Three Countertheses: Or, A Critical In(ter)vention into Composition Theories and Pedagogies," his "counterresponse" to "the strong *will* of the field of composition" to systematize, control, and teach the language of composing (140). In her 1992 Academic Discourse and Critical Consciousness, Patricia Bizzell reflects upon the importance of her own

1979 article on Kuhn, setting it within the larger context of the development of her own critical consciousness.

The 1994 anthology, W. Ross Winterowd and Vincent Gillespie's Composition in Context: Essays in Honor of Donald C. Stewart, follows much the same pattern as Harkin and Schilb's. The collection contains numerous uses of Kuhnian terminology as well as Kuhn's ideas. For example, James A. Berlin, in "Postmodernism, the College Curriculum, and Composition," draws upon Kuhn's ideas about "disciplinary formation," "social construction" resulting from community agreement about "standards of truth," and the effects of the terministic screens that "form research paradigms that change over time" (56). In "Crisis and Panacea in Composition Studies: A History," Robert J. Connors mentions crises, revolutions, discipline building, the use of textbooks, conversion experiences, and paradigms, while warning that "pragmatic pedagogical issues can increasingly get lost as composition studies borrows more and more of its issues from philosophy and social science" (101). Although Kuhn is directed cited only once in the anthology, sufficient evidence of his ideas exists to show a broad knowledge of the concepts he promotes in Structure.

The combination of CCC articles, personal anthologies such as Bizzell's, and collections of essays about the nature of composition studies and allied inquiry suggests the extent to which Kuhn's theory and language have become a factor in the study of writing theory, practice, and pedagogy. The use of Kuhn in the nineties crosses so many lines that acknowledging all of those uses, even those just in CCC alone, would take up too much space here. Instead, I will concentrate on four as a reflection of Kuhn's influence: Carol Berkenkotter's "Paradigm Debates, Turf Wars, and the Conduct of Sociocognitive Inquiry in Composition" in the May 1991 issue of CCC; Thomas Kent and Edward Schiappa's 1991-1992 debate spread over two issues of CCC; Susan Miller's 1991

Textual Carnivals: The Politics of Composition; and Gary Tate and Sidney I. Dobrin's 1994 Composition Theory for the Postmodern Classroom.

Carol Berkenkotter, 1991

Carol Berkenkotter, in her 1991 "Paradigm Debates, Turf Wars, and the Conduct of Sociocognitive Inquiry in Composition," invokes Kuhn in an examination of "the roots of some disciplinary quarrels that have polarized our thinking in composition studies," acting "as obstacles to reading and evaluating research and to training graduate students to conduct multimodal inquiry" (151). Quoting from Phelps's Composition as a Human Science, Berkenkotter asserts that "Kuhn's emphasis on the 'unformalizable elements and nonempirical, nonlogical factors' that govern scientific change opened the door to a number of alternative world views" (153). She then turns to N. L. Gage's 1989 "The Paradigm Wars and Their Aftermath: A Historical Sketch of Research on Teaching since 1989" for additional evidence of "competition between fields using different models of inquiry" (Berkenkotter 158). Berkenkotter contends that "the social/human sciences are in the midst of a paradigm crisis" growing out of disciplinary scrutinization of "dependence on positivist assumptions historically associated with the hard sciences" (165). She argues for better articulation of "the models of knowing that inform our practice" in composition studies and suggests the "need . . . to examine the ways in which our rhetorical practices and discourse expectations and conventions reflect the assumptions that constitute those models" (Berkenkotter 166).

Berkenkotter makes no attempt to explain any of Kuhn's concepts or ideas, depending instead upon an assumption that her audience is already familiar with them. She uses "paradigm" in Kuhn's sense of disciplinary matrix and suggests the understood and often unarticulated quality of those elements shared by a community of specialists.

Simultaneously, she charts the disciplinary conflict between potentially opposing "models of knowing." Kuhn's ideas serve as the framework for characterizing and understanding the conflict over the construction of knowledge.

Thomas Kent and Edward Schiappa, 1991-1992

Thomas Kent attacks Kuhn in an examination of "the different elaborations of community--elaborations such as discourse community, interpretive community, speech community, and disciplinary community" ("Idea" 425). In his 1991 "On the Very Idea of a Discourse Community," Kent labels "rhetoricians like Kenneth Bruffee, Stanley Fish, Greg Myers, John Trimbur, David Bartholomae, and Karen Burke LeFevre" as "Kuhnian internalists," advocates of social construction or those leaning towards it. Such internalists posit, according to Kent, that "all we can ever know is a conceptual scheme--usually called social norms--shared by a specific discourse community" ("Idea" 427). He goes on to identify three major problems with "the social constructionist view of Kuhnism": the self-refuting assertion of "incommensurate conceptual schemes," "global skepticism," and "relativism" (Kent, "Idea" 428-29).

Kent argues that acceptance of incommensurable languages translates into denial of the existence of discourse communities, writing: "Discourse communities cannot be incommensurate, for if they were, we would not even recognize them as discourse communities." According to Kuhn, we cannot avoid the problem of skepticism if we accept some sort of internalist conceptual scheme that "constitutes a place where human subjectivity and reality merge." Finally, he asserts that "nothing exists to authorize one set of practices or beliefs over another set" under social construction because "meaning and knowledge are relative to a conceptual scheme, and . . . conceptual schemes in the

form of social norms obviously change from place to place and from time to time" (Kent, "Idea" 428-29).

Kent's criticism of Kuhnian social construction turns into an explication of a version of philosopher Donald Davidson's "externalism" that allows Kent to reconciliate an anti-foundationalist position with social constructionist theory. He explores Davidson's "coherence theory of truth and knowledge" and notes Davidson's contention that "no shared framework, epistemological system, grammar, or repertoire can account for successful communicative interaction" (Kent, "Idea" 432). Kent believes that Davidson's externalism solves the problems of social constructionist theory by positing that "internal mental states unfold from our communicative interactions with one another"; such communication as Davidson proposes enables "us to understand the languages of others and the world around us" (Idea" 433-34).

Further, Kent cites Davidson's own analysis of Kuhn as proof of the inadequacy of Kuhnian social construction. According to Kent, Davidson has pointed out that: "Kuhn is quite capable of understanding pre-revolutionary discourse by employing his own post-revolutionary idiom, so, clearly, Kuhn is caught in a contradictory claim: If paradigms are incommensurate, how can we move easily from one to another?" (Kent, "Idea" 434) Within Davidson's position, Kent finds ammunition to refute social constructionist claims that discourse depends upon some framework of social norms. He asserts that "interpretation determines our social norms" and admits only that "discourse occurs in some sort of social context" (Kent, "Idea" 434). Throughout the remainder of his essay, Kent maintains his assault on the Kuhnian version of social construction.

Kent's essay is one of the few in the nineties to at least partially review Kuhn's concepts as they apply to social construction rather than just mention Kuhn's name or assume a set interpretation of Kuhn's ideas. In so doing, Kent imports another philosopher in an attempt to undermine the Kuhnian version of social construction. In a

later contribution to the 1994 Composition Theory for the Postmodern Classroom, Kent will continue to focus on criticism of Kuhnian social construction.

Kent's essay provokes a 1992 response by Edward Schiappa and a further reply by Kent. Suggesting that Kuhn has retreated from the original claims of incommensurability as criticized by Kent, Schiappa complains that "Kent parodies SC [social construction] as saying different discourse communities cannot communicate *at all,*" thus missing Kuhn's point that "the communication is *partial* in the same way foreign language translation is always partial." Accordingly, Schiappa argues that Kent has misinterpreted Kuhn and ignored Kuhn's responses to criticism. He suggests that Kent "imports a false dichotomy" with his promulgation of global skepticism and relativism. Further, Schiappa goes on to declare that Kent's importation of Davidson is subject to the same charges "that Kent brings against" social construction. He finds little difference between the "Kent/Davidson" thesis and Kuhn's ideas. Indeed, Schiappa states "the only difference between Kuhn and Kent" that he observes "is that the former is willing to acknowledge the role of the social; in particular, the role of prior socialization in the process of interpretation" (522-23).

Finally, Schiappa dismisses Kent's claims by rejecting Kent's description of social construction as "unlike any I [Schiappa] know of" (522-23). In noting that "Davidson eschewed his 'coherence theory' and 'truth' as an epistemic concept," Schiappa further attacks the legitimacy of Kent's argument. He concludes:

> The primary difference between Kent and SC [social construction] is the insistence of SC that our account of language and knowledge transcend strictly ephemeral interpersonal communication in order to note the significance of shared interpretation, habituated discourse practices, and the power of socialization. (Schiappa 523).

In a one page reply, Kent responds to two issues raised by Schiappa. First, Kent writes that he "is less sanguine than . . . Schiappa concerning Kuhn's complete abandonment of the incommensurability thesis." He argues that Kuhn continues to defend incommensurability, insisting that any categorizations of other cultures "derive from our own conceptual scheme." Adhering to the notion of incommensurability thus "leads directly . . . to the kind of relativism and skepticism" Kent outlines in his original article. Kent proceeds to point out that Richard Rorty and others have complained "about Kuhn's conceptualism" ("Reply" 524).

Second, Kent addresses Schiappa's analysis of Kent's differences "with social constructionists." Kent argues that social constructionists seem intent on saying that:

> [P]eople in different discourse communities think differently about the world, that changing discourse communities means learning a different language, that communication is convention-bound, that people who speak different languages don't understand the world in the same way, that knowledge is relative to discourse, that situation and context give a text its meaning, and so on. (Kent, "Reply" 524)

Believing that these claims "produce more problems than they solve," Kent categorically denies the validity of such social constructionist aims.

Ultimately, Kent agrees with Schiappa about overlooking Davidson's own retreat from "'coherence theory' or at least the term *coherence.*" Therefore Kent inserts a passage from Davidson in his reply suggesting compromise. Kent "will be happy to give up what Schiappa calls [Kent's] 'ephemeral interpersonal' account of communication if

he [Schiappa] will give up the very idea of a discourse community" (524), something both are unlikely to do.

The exchange between Kent and Schiappa reflects one of the few additional borrowings of Kuhn that occur during the nineties. Both men debate interpretations of Kuhn as one of the architects of social construction. In their debates, they add to the knowledge of Kuhn in the composition community instead of just accepting particular appropriations of Kuhn already articulated by others. Both go back to primary sources, citing Kuhn's Structure and Copernican Revolution for example, as well as referring to other works from the history and philosophy of science. Their debate does not close simply with their exchanges; Kent continues to articulate his position in later work and others respond to him, especially about his denigration of the concept of discourse community.

Susan Miller, 1991

Susan Miller labels her 1991 Textual Carnivals: The Politics of Composition as "a narration of what it is like to be a scholar devoted to written texts that no established discourse endows with cultural or literary privilege" (ix). In that narration, she asserts that compositionists "are uniquely placed across the border separating textual and 'real' worlds," a position that may allow us to "become more powerful critics of the intellectual politics" that enclose those worlds (Carnivals x). In essence, Miller tells a story that characterizes composition in terms of the Bakhtinian concept of "carnival," which she defines as "a licensed suspension and inversion of the dominant culture" as seen "from the peculiar situation of a transitional site within an institution" (Carnivals 1). Miller's story focuses on the political tensions within English studies, especially the "uneasy relationship between 'composition' and 'literature'" (Carnivals 2). She focuses on the historical and contemporary relations between composition and the academy.

Miller argues that composition has become "an institutionalized academic site," one that is "permanently established but marginalized" (Carnivals 81). In her exploration of those political relationships, Miller analyzes the influence of Kuhn on composition studies, attempting to chart the extent to which Kuhn's concept of paradigms has affected the growth of composition studies as a discipline.

Miller devotes a section of chapter three of Textual Carnivals to her analysis of Kuhn. She begins by touching upon the "well-motivated sources" for "the models and 'paradigms' that legitimate composition theory and practice." Miller points to Hairston's "The Winds of Change: Thomas Kuhn and the Revolution in the Teaching of Writing" as a seminal work and notes that Hairston argued that "the primary value of establishing a paradigm of 'process' in composition was to provide a space for systematically discovering and describing" the actual writing process. The process model encouraged teacher intervention during the process of writing, leading away from the analysis of product. Miller suggests that various programs such as workshops, peer review, and emphasis on revision "appear to verify this particular shift" from product to process (Carnivals 105).

Miller further suggests the importance of creating a "symbol system" for composition "by claiming its possession of a 'paradigm' like those that centralize the activities of other professional groups and well-established disciplines" (Carnivals 105). She writes:

> Hairston's lengthy review of Kuhn's argument and Robert Connors's perceptive analysis of it in "Composition Studies and Science" both highlight how important it has been for composition to take this way toward legitimating itself, not in this case by extending the ancient discipline of rhetoric as a credential, but by

creating a new area of inquiry that can share the substantive and social status of contemporary "science." (Miller, Carnivals 105-6)

Later, Miller continues: "This legitimacy, if not a scientific identity, appears to be the most clearly achieved result from paradigm theory in composition." As a result of such claims, compositionists can, like North in The Making of Knowledge in Composition, "define" the various "'schools' and professional groups on which a discipline's status depends." According to this position, composition takes on legitimate value as a "subject" (Miller, Carnivals 106).

But Miller questions the validity of Kuhn's paradigm theory to adequately account for the legitimization of composition. She wonders "whether a paradigm shift has actually occurred, what its quality is, and how it has, or has not, created intellectual subjectivity/subject of composition that has specific consequences for the field's intellectual politics." While agreeing that the model has produced results, she questions "the quality of the differences resulting from process theory." Miller argues that "Research in composition is not generally undertaken, as Kuhn's model of paradigm shift would say it would now be, to verify a new normalizing theory based on a revolutionary idea that sets a new set of problems for the field" (Miller, Carnivals 106-7). She concludes instead: "In a political analysis of the subject of composition, it is clear that process theory, by focusing on 'what actual writers do,' celebrates new tools used in the classroom and other situations for writing, tools that were not available when composition courses began" (Miller, Carnivals 107).

For Miller, the changes in the technology of writing "since the late nineteenth-century" account more for any shift in classroom practices than the movement towards a process paradigm. Improved technology translates into "new possibilities for sustaining the actual writing processes over a lengthy time," but does "not necessarily form new

theory." Miller notes simply that "the attempt to describe these actual processes as a comprehensive theory of textual production raises some additional intellectual problems" (Carnivals 107). Such reasoning leads Miller to declare that a paradigm shift has "not quite" occurred yet. She argues that "A realistic history of writing suggests that 'process' is serviceable mainly as an affective improvement in the classroom and as a way of granting composition a qualified academic legitimacy." Even granting process theory "partial paradigmatic status" does not support a view of process theory as "an accurate or even a very historically different theory of contemporary writing" when viewed from Miller's historical and theoretical perspective (Miller, Carnivals 108).

Miller, like so many others, does not go back to the primary sources of Kuhn's ideas. She depends upon the interpretation of Hairston and Connors for much of her information about Kuhn. By looking at Hairston's essay, Miller chooses one of the principal appropriations of Kuhn with which most compositionists can perhaps claim some familiarity. By engaging Connors's essay, she demonstrates some limited knowledge of the response of the history and philosophy of science to Kuhn's ideas, although she does not really exploit those disciplinary responses in her argument. Ultimately, her rejection of the process paradigm or model comes down to a conclusion that the move from product to process does "not quite" meet the tenets of Kuhn's concept of paradigm shift. Without exactly accepting or rejecting Kuhn's theory, Miller manages to ambush one of the exemplars for which that theory is often used to explain in composition studies--the shift from writing product to process.

Composition Theory for the Postmodern Classroom, 1994

In their 1994 Composition Theory for the Postmodern Classroom, Gary Tate and Sidney I. Dobrin return to the postmodern themes developed by Phelps in her 1988 text.

Many of the contributors to Tate and Dobrin's collection of essays previously published in the Journal of Advanced Composition make strong efforts to tie postmodern theory to the actual classroom teaching while drawing upon Kuhnian ideas. Jacqueline J. Royster, in her Foreword to the collection, speaks of "revolution and evolution," the needs of the "disciplinary community," "shifts" in our scholarly "thinking and patterns of operation," and the creation of "new paradigms" ([xi]). In "Some Difficulties with Collaborative Learning," David W. Smit lists Kuhn's Structure as one of the "semi canonical texts" for collaborative learning and writing (69). When writing about competition between theories and the development of science, David Bleich mentions Kuhn and his forerunner, Ludwik Fleck, in "Sexism in Academic Styles of Learning" (169). In "Articulation Theory and the Problem of Determination: A Reading of Lives on the Boundary," John Trimbur notes the propensity of authors of "literature reviews and histories of writing instruction" to use "taxonomic" strategies to classify "various pedagogies, rhetorics, and research programs . . . with perhaps the discontinuity of a Kuhnian paradigm shift to demarcate one approach from another" (239).

Thomas Kent credits Kuhn with influencing the "claim that subjectivity is socially constructed," noting a suspicion that Kuhn no longer accepts "most of the beliefs attributed to him [Kuhn] regarding the social construction of knowledge." In "Externalism and the Production of Discourse," Kent goes on to argue that Stanley Fish and Kenneth Bruffee "have done the most to promote Kuhnism in the areas of discourse production and discourse reception." Further, Kent concludes that: "For Kuhnians . . . different communities construct different and largely incommensurate languages, and these languages represent the world in entirely different ways" ("Externalism" 298-99). In an endnote, Kent confesses: "I stress the Kuhnian variety of social-constructionist theory because I believe it to be the most pervasive and influential version" ("Externalism" 309).

In Postmodern Classroom, Joseph Petraglia also mentions Kuhn's brand of social constructionist theory, remarking that Thomas Kuhn and Richard Rorty "appear to be most often cited in composition scholarship" on social construction (314). Petraglia later examines Kuhn's notion of paradigm shift when questioning the central premise of social construction that "reality changes as knowledge changes" (325). In casting doubt on the premise, Petraglia likens "composition's constructionists" to Kuhn and accuses them of being "often content to confirm that inter-communal consensus is subject to change" and doing "little either to show *how* competing communities arrive at all-important consensus or to acknowledge that consensus-building may not always be a progressive, 'liberatory' process, that it could involve coercion instead" (326).

The breadth of the essays employing Kuhn within the Tate and Dobrin anthology speaks volumes about the extent to which composition studies has become acquainted with Kuhnian concepts. Various compositionists suggest the continued play of Kuhn's ideas of incommensurability, paradigm, shift, and crisis. They point to Kuhn's influence on theories of social construction. While sometimes questioning the efficacy of the views Kuhn expressed in Structure, many still find that Kuhn's theory provides a convenient frame for arguing a variety of positions within composition studies. Their uses of him suggest a widespread knowledge of Kuhn exists within composition studies.

The CCC articles, counterstatements, and book reviews along with the various anthologies show a profession of composition studies widely cognizant of Kuhn's ideas as expressed principally in the second edition of Structure. Simultaneously, these sources, with few exceptions, reflect extremely little of Kuhn's reception in his own field. Though some compositionists such as Kent and Schiappa consult the history and philosophy of science for clues to interpreting Kuhn, mostly to refute him, none of those compositionists delve very deeply into Kuhn's disciplines. Compositionists have rarely

consulted other works of Kuhn's or the disciplinary responses of the history and philosophy of science to Kuhn's ideas. Others turn like Berkenkotter to the responses of disciplines such as education and the social sciences. With the exception of Kent's analysis of Kuhn's version of social construction and the response it provokes, little new of Kuhn appears. Most compositionists who invoke Kuhn's name or ideas during the period generally continue to depend upon earlier appropriations by Young, Bizzell, and Hairston.

Implications for a Discipline of Composition Studies

> We must and do believe that what we teach, investigate, and theorize about will result in a better world. [32]
>
> --Robert J. Connors, 1994

What does comparing a single borrowing by composition studies to the original version and its evolution within a discipline outside of our profession reveal about the nature and future of composition studies? For composition studies, the comparison suggests that the value of any borrowing rests in the political advantage its use entails, its contribution to our understanding of our discipline, and its utility to our pedagogical mission. During the seventies and eighties, Kuhn's work, essentially in its 1970 version, offered compositionists a way to access the prestige of science and claim a share of that prestige for our own profession. His ideas provided political ammunition in the struggle to establish composition studies as valid academic inquiry. As an aid to understanding the discipline, Kuhn's conception of paradigm as "disciplinary matrix" and his theory of revolutionary advance provided an operant methodology or analogy for

organizing major conceptual themes in composition research and theory. At the same time, Kuhn's ideas provided a convenient framework for assessing the content of composition courses, facilitating a restructuring of content from a product to a process orientation. Kuhnian theory quickly gained the status of tacit knowledge within composition studies because of the debate it engendered over its political use and convenience.

But does borrowing only occur as a matter of political advantage or convenience? The case of Kuhn's theory would seem to confirm this to a certain extent. Borrowing Kuhnian theory combines the political advantage of established criteria for claiming disciplinary status with the convenient organizational concept of the paradigm as disciplinary matrix. It offers terminology useful for describing various movements within composition theory, praxis, and pedagogy. Still, assessing a borrowing by the criteria of political advantage or convenience does not mean that a particular borrowing such as Kuhnian theory in any of its interpretations necessarily translates into being better than any idea to which we already adhere as a profession. If anything, I suspect another criteria influences our propensity to look outside of composition studies--novelty.

In a professional world dominated by the ideal of "publish or perish," a world where critical decisions such as jobs and tenure often depend upon the frequency in which one's name appears in print, novelty may promote interdisciplinary borrowing. Being the first to import ideas from outside of what we consider composition studies may bring a certain prestige in and of itself. Using the archival record as I have done here, it is difficult to assess the potential influence novelty plays as a criterion for borrowing.

Setting aside issues of political advantage, convenience, and novelty, assessing the influence of Kuhn's use by compositionists reveals at least one weakness associated with borrowing across disciplinary boundaries. Composition's appropriation of Kuhnian ideas rarely has included any acknowledgment of the reception those ideas met in their

original disciplines. His ideas continued to evolve in response to criticism in the history and philosophy of science, transitioning from emphasis on the paradigm and normal science as organizing principles to greater emphasis on disciplinary communities and notions associated with incommensurability. From early on, Kuhn's ideas met stiff resistance in his own disciplines. Little of that resistance or the debate over his ideas ever transferred to composition studies; instead, we seem to have judiciously avoided using criticism from Kuhn's own fields to counter or bolster his use in our own. Accepting any borrowed theory *carte blanche*, without first examining its reception in its original discipline, entails the risk of promoting discredited ideas and misinvesting effort and resources in vain pursuit of validating those ideas. Imports often take on a life of their own in a new discipline once they cross disciplinary boundaries. Disregarding disciplinary receptions forces us to invent or discover many of the same arguments for ourselves, an often time-consuming process.

The notion that writing is an extremely complex human process requires an openness to any methodology--political, convenient, or novel--that can potentially shed light on our practice and teaching. Richard Beach and Lillian S. Bridwell write in the introduction to their 1984 collection of essays, New directions in composition [sic], that "composition research is not a unique discipline, but a hybrid of disciplines, each having something to offer to those who are attempting to understand the extremely complex process of writing" (1). They note: "Underlying all the trends in composition research is one predominant theme: both proponents and critics of composition research are calling for a more valid and comprehensive theoretical base that would define, explain, and perhaps predict writing behavior" (Beach and Bridwell 2). Further, they assert that part of this validity must come from researchers "familiar with a variety of disciplines" cross-checking "their assumptions with those from other angles of inquiry" (Beach and Bridwell 6). They stress the need for a comprehensive theory, one that

places the complexity of the writing process within its many contexts--social, political, and psychological. This search for theory and validity leads to borrowing and importing from disciplines outside of composition.

Similarly, Lindemann and Tate argue in the preface to their 1991 An Introduction to Composition Studies that:

> The major concern of most composition specialists is teaching writing well. To do this work effectively, they also attempt to answer broader questions about literacy by studying composing from historical, social, psychological, political, and academic perspectives, *often borrowing useful concepts and methods from other disciplines* [my emphasis]. (Lindemann and Tate [v])

For Lindemann and Tate, composition studies involves "interdisciplinary, collaborative efforts to understand how people learn to write" (vi). In that understanding lies the seeds of better teaching and the future of composition studies.

Where might composition studies expand in the future? The range of topics listed in the most recent five years of CCCC programs (1991-1995) suggests the continued viability of interdisciplinary borrowing as a source of potential knowledge for the teaching, practice, and research of writing. As a discipline, we must continue to mine allied fields such as literary criticism, linguistics, and teacher education for the raw ore to smelt in the furnace of practice. Ideas from other disciplines such as cognitive psychology, sociology, philosophy, and science will continue to influence composition inquiry as well. And we must pay close attention to advances in technology, too. Technological advance, especially in computers, has already led to revolutions in the ways many of us write, forcing us to consider other avenues of developing practice and suggesting needed research. Importations of research methodologies such as ethnography

from the social sciences are assisting us in more completely describing the practice and teaching of writing. Interdisciplinary borrowing can, has, and will continue to inform the discipline of composition studies.

Our reliance upon other disciplines for ideas does not necessarily suggest or mean a fragmentation of a loosely unified discipline such as North foresees in his 1987 The Making of Knowledge in Composition. Instead, it suggests that composition studies fits more closely into the multiple paradigm category identified by Margaret Masterman in her 1966 response to Kuhn's original Structure and admitted by Kuhn in his later work. I assert that we possess multiple paradigms in composition studies and that these paradigms seem to align closely with the various subfields that compose the discipline. If we accept North's 1987 breakdown of composition studies into a series of communities based on methodological commitments and modes of inquiry, we see the adherence of the various communities to particular ways of knowledge-making. The pattern closely echoes Masterman's own definition of multiple paradigm science.

Such a position is supported by educational theorists such as N. L. Gage, one of the first to introduce Kuhn's notion of paradigms to the embryonic field of composition studies in 1963. Gage, in his October 1989 article, "The Paradigm Wars and Their Aftermath: A 'Historical' Sketch of Research on Teaching Since 1989," recommends forging a methodological peace or rapprochement between different paradigms by engaging "in an earnest dialogue, lifting the discussion to a new level of insight, making progress toward workable educational problems, and generating theory that fit together" ("Wars" 10). Inherent in his analysis is the acknowledgment of disciplinary commitments and competition manifested in "turf wars in the attempt to gain for one's own discipline a greater share of the research funds, the academic positions, and the other kinds of wherewithal needed for a discipline to flourish" (Gage, "Wars" 8). Gage's position depends upon declaring the oppositional component of Kuhn's paradigm invalid;

"this component had stated that any paradigm inherently applied an opposition to alternative paradigms" ("Wars" 8).

In addition to North's delineation of various communities within The Making of Knowledge in Composition, we can see support for the multiple paradigm position in other places throughout composition studies as well. The Research Network pre-CCCC workshops represent a recognition that we are "part of a common enterprise that both makes knowledge and uses that knowledge to help people write better" (Bazerman 223). The workshops have provided an additional venue for previewing research and encouraging dialogue across our different approaches to theory, practice, and pedagogy. Those different approaches have often erupted in "not-always-productive competition and mutual misunderstandings of each other's work" (Bazerman 223). Forums such as the CCCC Research Network workshops offer opportunities for greater discussion within composition studies, encouraging communication between proponents of various approaches to theory and practice. Simultaneously, the workshops help meet a growing need to share our multiple paradigm approaches and synthesize the knowledge being created.

What is at stake in claiming the status of a multiple paradigm perspective for composition studies? First, we can acknowledge both the discipline's humble beginnings as a subfield of English and our even deeper ties with rhetoric while claiming status within the academy on the basis of our contributions to knowledge about the practice and teaching of composition. Interdisciplinary borrowing has informed our inquiry and will continue to do so. Further, as a collection of communities united by our mutual interest in writing practice, we can bring multiple perspectives to bear on such practice. Thus, forums such as the CCCC have a vital role to play; they serve to introduce the ideas of the various communities or subfields to others, crossing the fuzzy internal methodological boundaries sometimes used to create allegiances to particular groups. Composition

studies continues to mature within a framework of pluralism or multiple perspectives unified by interest in writing theory, practice, and pedagogy. To abandon that tradition for a single paradigm in Kuhn's sense of a set of habits or exemplary achievements would mean silencing the multiplicity of voices that have served to establish composition studies as valid inquiry. We have already surpassed Masterman's vision of the non-paradigm science as well as Kuhn's original preparadigmatic stage to move towards legitimate inquiry into writing from multiple perspectives. Engaging in conversation with others within the overall umbrella of our interest in writing can lead to greater synthesis of knowledge about how we write, how others write, and how we can best teach writing.

I argue that such a multiple paradigm perspective should remain a foundational idea within composition studies, and, indeed, scholarship in general; members of other disciplines using other modes of inquiry from outside of the traditional, obfuscated disciplinary boundaries can provide theory and support for what we do in the classroom. They can assist inquiry into our own understanding of theory and practice. Such a notion is inherently egalitarian, working against even Kuhn's concept of a mature discipline practicing normal science driven by a single paradigm. Perhaps it is really a hallmark of composition studies as, to use Phelps's term, a human science that we can embrace a multiplicity of avenues to reach similar goals centered on better theory, better practice, and better teaching of writing. To be truly interdisciplinary is to welcome the multiplicity of ideas and methodologies from other disciplines that have so clearly informed our profession since its emergence from the dungeon in the sixties.

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APPENDIX ONE

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VITA

Born into a military family in 1952 and raised on a variety of U. S. military installations as well as on a farm in East Tennessee, Carl Joseph Arseneault seemed destined for a service career. In 1970, he graduated from F. T. Wills High School, Smyrna, GA, and went on to attend the University of Georgia, Athens where he earned a Bachelor of Arts in Political Science. At Georgia, he concentrated on international relations and public administration while completing his military studies.

Commissioned as a second lieutenant in June 1974, Carl entered the U. S. Army in September of the same year. He attended the U. S. Army Transportation Officer Basic Course at Fort Eustis, VA, and reported to his first assignment at Fort Stewart, GA, in January 1975. At Fort Stewart, he commanded the 24th Ordnance Company and served in a variety of platoon leader and staff positions. Carl was chiefly responsible for the organization of the 24th Infantry Division's Arrival/Departure Airfield Control Group in 1976 and participated in a number of major aerial deployments. From late 1978 to late 1979, he attended the Transportation Officer Advanced Course at Fort Eustis and the U. S. Army Organizational Effectiveness School at Fort Ord, CA, earning a promotion to Captain. Following successful completion of his courses, Carl was reassigned to Zweibruecken, Germany where he served as a management consultant and commanded the installation headquarters company. Upon his return to the United States in 1982, he was reassigned as Operations Officer, Columbus Army Recruiting Battalion (CARB), Columbus, OH. Carl married in 1983 and served at CARB until medically retired for service related disabilities.

After an Army career cut short by medical retirement, Carl returned to the family farm in Knoxville, TN, in 1985 and entered the undergraduate program in English at the University of Tennessee, Knoxville. He obtained his second Bachelor of Arts degree at

Tennessee in 1986, concentrating in technical writing. Carl then accompanied his wife to Germany on her tour of duty with the Department of Defense Dependent Schools System. In Germany, he substitute taught grades K-6 for two years and performed volunteer services in the local military community. Upon his return to the University of Tennessee, he entered the Master of Arts program in English, earning his degree in 1990. He continued at Tennessee, focusing on rhetoric and composition, linguistics, American literature, and curriculum development in his doctoral studies and receiving his Doctor of Philosophy degree in English in 1996.

When not teaching, Carl works as a freelance writer, copy editor, and simulation designer. His editing and publishing credits include a number of commercial conflict simulation designs as well as articles on military history. In addition, he serves as Vice President of the Southeastern Writing Center Association.