Winter 2005

Boundary -work in United States psychology: A study of three interdisciplinary programs

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BOUNDARY-WORK IN UNITED STATES PSYCHOLOGY: A STUDY OF THREE INTERDISCIPLINARY PROGRAMS

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DISSERTATION

Submitted to the University of New Hampshire

in Partial Fulfillment of

the Requirements for the Degree of

Doctor of Philosophy

in

Psychology

December, 2005

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November 18, 2005

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DEDICATION

For my mother and father

ACKNOWLEDGMENTS

First, I would like to thank my dissertation committee for their patience and guidance as I wrote this manuscript. William Woodward provided me with timely feedback, gentle criticism, and persistent encouragement throughout this process. His patience, flexibility, and knowledge of the history of psychology have significantly improved my research. John Limber helped foster my interest in cognition, consciousness, and cognitive science through his stimulating class concerning Julian Jaynes' controversial ideas on the formation of human consciousness. Also, John was of immense help on my master's degree committee for a project regarding domain-specific reasoning in social exchanges. Jan Golinski provided me with an interesting course on historiography that allowed me to extend my reading outside the boundaries of the history of psychology. I also found his book, *Making Natural Knowledge*, an invaluable resource on the relationship between history of science and the sociology of scientific knowledge. Derek Paar helped me greatly throughout my years as a student. His initial encouragement and friendly advice gave me the confidence to pursue my interests in chaos theory. I would not have pursued my doctoral degree without his intervention early in my undergraduate career.

I would like to thank Peter Fernald who has done so much for me during my graduate career at UNH. He accepted me into the UNH psychology program and served as my master's degree committee chairperson. He provided me with vast amounts of encouragement throughout my years at UNH. I only hope I can be half as good a teacher to my students as he has been to me.

I would also like to thank many friends who have supported me throughout my undergraduate and graduate education. AT UNH, I would like to thank Ken Gardner, Mary Harmon, Kelly Perrachi, and Kathy Bailey for many "discussions" on various "psychological" topics. At Springfield College, I would like to thank Ann and John Moriarty for their kindness, humor, and generosity. Jim Merrill, Bob Clausi, and George Vafiades were also immensely influential in my intellectual pursuits as an undergraduate at Springfield. In Vermont, I would like to thank a number of people who have helped me in various ways throughout my educational pursuits. First, I need to thank Stephen Mulholland for keeping me relatively sane for 35 years and for our numerous conversations about psychological topics. I would like to thank Scott Noren for our many discussions on all things evolutionary and triarchic throughout the years. I want to thank Paul Fila, Todd Field, and Brent Benson for keeping me focused on finishing this project with their "gentle" persistence. In New Jersey, I would like to thank Tim and Stacey Lanni for being so supportive over the years. I hope there will always be 25 plays on the jukebox.

I need to thank a group of people who have kept me motivated in my pursuit of a doctoral degree: Rick Lyons, Chris Higgins, Gene and Katherine

Beebe, Fred-Ted-Ned or Red (also known as Bob Paquette), George Paquette, Ed Preseau, Freddy Marchaland, and finally Trevor Hughes (whose name I changed to protect the innocent).

I would like to extend my special thanks to Michael Graham and Patrick Loughan for being so close and supportive to my father and me for all these years.

Lastly, I need to acknowledge my parents, John and Jeanne Root, for their infinite patience and support during my educational pursuits. Without them, this endeavor would have been impossible. To my mother and father, I dedicate this dissertation.

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ABSTRACT

BOUNDARY-WORK IN UNITED STATES PSYCHOLOGY: A STUDY OF THREE INTERDISCIPLINARY PROGRAMS

by

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University of New Hampshire, December, 2005

Between 1970 and 2000 scientists from three interdisciplinary programs—evolutionary psychology, cognitive science, and chaos theory—contributed to changing U.S. psychology's disciplinary boundaries. These interdisciplinary scientists brought about this change through their conceptual, material, and social practices. Psychologists used "boundary-work" as a means to control the influx of these various practices. Boundary-work connotes activities that promote scientists' epistemic authority in society. Boundary-work also serves to demarcate a science's particular collection of knowledge from other collections. Through their boundary-work activities, various psychologists resisted some of the practices of these interdisciplinary scientists while making accommodations for other types of practices. These resistances and accommodations illustrate the ways in which psychologists conveyed their epistemic authority and demarcated their discipline's boundaries between these three decades.

The purpose of my dissertation is to describe psychologists' boundary-work in reaction to the introduction of these interdisciplinary programs' practices between 1970 and 2000. First, I present an overview of psychology's complex disciplinary boundaries. I then use the history of psychology and sociology of scientific knowledge literature to describe the nature of boundary-work activities. Next, I present the foundational components and a brief history of each interdisciplinary program. Fourth, I outline each program's conceptual, material, and social practices. Lastly, I discuss psychologists' resistances and accommodations to each interdisciplinary program's practices with reference to how they affected psychology's disciplinary boundaries.

My results indicate that certain psychologists most often resisted evolutionary psychologists', cognitive scientists', and chaos theorists' conceptual practices. Psychologists' resistances seemed ineffective in preventing these conceptual practices from entering the discipline and did not stop other psychologists from using them. Accommodations occurred for all types of practices for all three programs, indicating that psychology's disciplinary boundaries are relatively permeable. I argue that psychologists made accommodations for these practices to increase their epistemic authority within the scientific community and throughout society. Finally, I discuss the advantages of writing psychology's history through an examination of psychologists' boundary-work.

INTRODUCTION

Like every discipline in the humanities, natural sciences, and social sciences, the discipline of psychology within the United States (U.S.) is an expansive and complex field of knowledge production.[1] Psychology's particular collection of knowledge represents every dimension of human behavior, cognition, and emotion. It includes knowledge of our perceptual capacities to view complex external stimuli, our ability to make judgments and decisions given complex information, the role of memory in our ability to recall pertinent information, our interpersonal communication skills, and the motivations that allow us to carry out our daily activities. Essentially, psychological knowledge encompasses the "individual," or the "self," and prescribes how we relate to, act on, and make sense of our social world.

The Dynamic Nature of Psychological Knowledge

Psychological knowledge is neither stationary nor inflexible; it does not remain immutable after psychological communities generally agree that it possesses some pragmatic or truth value for the discipline and the public. Instead, this knowledge is dynamic, fluid, and locally contingent upon time and place. Such factors as changes in generations, economic adjustments, political

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[1] I use the terms "U.S. psychology" and "psychology" interchangeably throughout the text because I focus on psychological knowledge in the United States. It is important to note that psychology's theories, methods, and concepts vary based on one's culture (Danziger, 1997). For example, there are differences in how individualist and collectivist cultures interact in their social environments (Schoeneman, 1994; Trandis, 1994).

upheavals, and new research findings ultimately affect how psychological knowledge circumscribes and represents our behaviors, thoughts, and emotions (Chaiklin, 2001; Shweder & Levine, 1984). These factors, in turn, affect the ways in which we view and understand others and ourselves. In her discussion of contemporary psychological knowledge, Lisa Blackman (1994) describes this knowledge as a "form of discursive activity which is not only relative to the socio-cultural, historical and institutional setting of psychology, but also comes to define the very ways we understand ourselves as human beings" (p. 487). As psychological knowledge changes and is transmitted throughout our cultural milieu, our phenomenological experiences of what it is like to act, think, and feel also change.

Examples abound of how psychological knowledge changes in pace with societal and cultural changes. For instance, in his book, *American Cool*, the historian Peter Stearns (1994) discusses people's changing emotional behaviors between the nineteenth and twentieth centuries. According to Stearns, people changed their "emotional styles"—the management and presentation of emotions to others—between the two centuries. In the nineteenth century, people generally acknowledged their emotions but also tried to control and channel their emotions appropriately. If people were angry with others, it was common to acknowledge this anger while directing it towards others in an acceptable manner. In the early to middle twentieth century however, there was a departure from this emotional style. In general, people shifted away from channeling their emotions in appropriate ways and began to hide their emotions behind a "cool" exterior.

People were controlling their emotions as much as people did in the nineteenth century, but they were not as likely to channel them appropriately. Instead, the social context of the early to middle twentieth century, especially during and after the two world wars, played a role in discouraging the outward expression of emotions. It was not that emotionality disappeared altogether. Instead, people began to stifle their emotionality. Stearns argues that this change in emotional styles affected how people came to understand their emotional lives and how they interacted with others socially.

Ideas about the nature of human intelligence also have changed over the years (Fancher, 1985; Sokal, 1987). In the early 1900s the English psychologist Charles Spearman (1904) believed intelligence was a single intellectual ability called the "g" factor. More recently, psychologists such as L. L. Thurstone (1934), Robert Sternberg (1985), and Howard Gardner (1983) have challenged Spearman's findings and posited various ideas about multiple intelligences. Sternberg, for instance, believes there is evidence to suggest that people possess three broad categories of intelligence: analytic, creative, and practical. Gardner believes that we have seven different intelligences: verbal, mathematical, spatial, musical, interpersonal, intrapersonal, and kinesthetic. This change has ramifications for how we behave towards and perceive others. Imagine a situation in a high school where a student receives a low intelligence test score from the perspective of Spearman's conception of intelligence. It is likely that teachers will think of this student as inadequate in most if not all subjects due to their low intelligence score.[2] When using any of the multiple intelligence theories however, teachers may not be as likely to label that same student as inadequate in all subjects. Instead, the teacher may believe the student is poor in the subjects of mathematics and vocabulary, but believe the student is strong in musical or kinesthetic abilities.

Psychologists' views on how to characterize personality have changed as well. Psychoanalyst Sigmund Freud believed that adult personalities form through psychosexual stages of development (i.e., oral, anal, phallic, latent, genital stages) occurring in early childhood. According to Freud, when primary caregivers do not meet the needs of the child, the child may become fixated at a particular stage. As an adult, when they experience anxiety, they may revert back to that particular stage of fixation. On the other hand, contemporary experimental personality theorists view personality as comprised of traits, produced mainly through heredity, that change relatively little throughout the lifespan (e.g., Goldberg, 1992; McCrae & Costa, 1987). Each of these ideas about the nature of personality has ramifications for how we interact with other people. In the 1960s and 1970s, at the height of Freud's popularity in the U.S., people may have viewed a very talkative person as being orally fixated. From the modern trait perspective that person would likely be identified as an extrovert.

This profuse and provisional knowledge helps create the extensive nature of psychology's disciplinary structure. Producing knowledge about how we

[2] There is some evidence that this scenario would hold true (Rosenthal, 1994; Rosenthal & Jacobson, 1968). Robert Rosenthal's research suggests that people tend to treat others in a manner that coincides with their beliefs about that individual.

understand ourselves and make sense of the world is a vast undertaking. Many times psychological knowledge production leads to heterogeneous and contested knowledge product, such as the debate over the factors involved in intelligence. Simply expressed, psychological knowledge does not "sit still." Psychologists continually revise this knowledge based on numerous factors including changing social contexts, new research, and innovative perspectives by, for example, feminist and multicultural psychologists (e.g., Hall & Barongan, 2001; Morawski, 1994).

Making Psychological Knowledge Public

Psychologists, as the knowledge producers in this multiplicity of fields from sensory psychology and neuroscience to counseling and community psychology, use a variety of techniques and procedures to create, reproduce, represent, and disseminate these knowledge products to their colleagues in psychology, other scientific communities, funding agencies, and the public. Irrespective of who obtains and uses this knowledge, it represents a public commodity and the process of its circulation and distribution is a social activity. This social activity consists of a reciprocal relationship between knowledge producers and knowledge consumers.

To make their knowledge public, psychologists engage in a number of practices. Like other scientists, they present their findings at professional conferences around the world. They report their experimental research results by publishing in peer-reviewed journals. They write both technical and popular books that distill their sometimes abstruse research findings for professionals in other subdisciplines and the lay public alike. Other psychologists use their professional training to prescribe modes of conduct for their clients during psychotherapy sessions. These psychologists also perform assessments of psychological functioning and make diagnoses on the mentally ill while working in mental health clinics, private practices, and hospitals. Still others invent and reconfigure instruments (e.g., personality inventories, intelligence tests) that they then attempt to bring into and sometimes market to the psychology community and the public. Whether psychological knowledge takes the form of a professional journal article or advice on a call-in radio show, it exists "out there" in the world for anyone to acquire, consume, and evaluate.

The form a particular piece of knowledge takes and its presentation in some public domain varies considerably from psychologist to psychologist. Psychologists' specialty areas, their institutional training, the laboratories in which they conduct their research, the quality of their mentoring relationships, and various other factors all contribute to the presentation format of psychological knowledge. The complexity and expansive scope of psychologists' activities in their knowledge production play a pivotal role in creating the field's expansive and complex *disciplinary boundaries*. In the next section, I explore the concept of "discipline" and the role of disciplinary boundaries.

The Nature of Disciplines and Disciplinary Boundaries

What does the term "discipline" mean and what exactly constitutes a discipline like psychology? The word "discipline" is ubiquitous in academic culture so it would seem superfluous to define the obvious. Within academe, there are disciplines for medicine, economics, political science, art history, physics, and psychology along with a host of others. But the meaning of discipline goes beyond the superficial labeling common in academic institutions. The nature of disciplines contains tacit assumptions about the production of knowledge that call for explication in order to appreciate the modern discipline of psychology.

Disciplines

According to Ellen Messer-Davidow and her colleagues (1993), organizing knowledge into separate disciplines is of relatively recent origins—occurring approximately 200 years ago—and it has been less than 100 years since the production of this knowledge took place within the academic settings familiar today. David Shumway and Messer-Davidow (1991) assert that the organization of knowledge into disciplines began with the formation of scientific societies such as the Royal Society of London and the Académie des Sciences of France in the seventeenth and eighteenth centuries. These scientific societies provided learned people with the freedom to study in specialized areas of knowledge that went beyond the traditional subjects of grammar, rhetoric, and ethics. Between the late eighteenth and middle of the nineteenth centuries, this specialization led to the division of natural philosophy into independent sciences such as physics, chemistry, and biology. With the proliferation of scientific findings during this time, the idea and utility of distinct disciplines came into scientific and public consciousness. But there is more to the term "discipline" than the formation of the now common academic disciplines.

"Discipline" contains a double meaning that is important for my discussion of psychology's disciplinary boundaries and disciplinary boundaries in general. French philosopher Michel Foucault (1977) discusses the underlying meaning of the term in *Discipline and Punish*, his poignant exposition on the structure of prison systems and schools. First, Foucault tells us that discipline is a method for assisting people in mastering a particular subject. It is the regimentation, organization, and transmission of information necessary to help individuals understand particular forms of knowledge. Secondly, discipline connotes a way of controlling or exerting power over other people's behavior. According to Foucault, power over others and people's compliance with standardized modes of conduct dictates the ways in which we interact with each other in the world.

Foucault's two meanings of discipline have implications for current academic disciplines. For instance, the sociologist of science Robert Kohler (1981) posits that academic disciplines indicate specialized knowledge and skills that individuals pursuing a particular specialization need to master. Secondly, he suggests that disciplines are political establishments for obtaining resources and "demarcating areas of academic turf" (p. 104). Timothy Lenoir (1993) also uses Foucault's dual meaning of discipline to imply that disciplines are the "infrastructure of science...that are institutionalized formations for organizing schemes of perception, appreciation, and action, and for inculcating them as tools of cognition and communication" (p. 72). Both Kohler and Lenoir suggest that traditional academic disciplines connote a form of cognitive authority used for internal organization (i.e., keeping its practitioners in line) and for demarcating one discipline from another (i.e., acknowledging some forms of knowledge as germane to the discipline while excluding other forms).

Messer-Davidow and colleagues (1993) suggest four dimensions characteristic of disciplines based on Foucault's framework. First, disciplines assist in producing our world. They provide us with theories, concepts, and ideas of how the world works. These various knowledge products circumscribe the way we perceive the world. Secondly, disciplines develop knowledge producers. Disciplines provide training grounds for students interested in producing knowledge about some part of the world. The training these students receive varies based on their disciplinary specialization. Third, disciplines produce prestige and value. Ostensibly, the public views knowledge producers as possessing a valuable commodity for society. If the knowledge does prove valuable, the public bestows privilege and rewards, usually in the form of financial support, to the knowledge producers. Finally, disciplines serve as icons for society's advancement. While one can debate whether knowledge truly does progress in some linear fashion, disciplines and their practitioners assist in promoting the *idea* that knowledge progresses in this fashion.

If Messer-Davidow and colleagues (1993) four dimensions are viable characterizations of disciplines, then an area worthy of investigation is how knowledge producers construct and manage their disciplinary boundaries. But what exactly are disciplinary boundaries?

Disciplinary Boundaries

Disciplinary boundaries are the *physical*, *intellectual*, and *rhetorical* spaces within which knowledge producers engage in their research and professional practices.[3] Within these spaces, a discipline's practitioners make epistemic claims about a set of circumscribed phenomena germane to the overall subject matter of their discipline. They also serve as spaces within which students receive training in prescribed modes of conduct appropriate for their chosen field.

The physicality of disciplinary boundaries is apparent in the separate buildings assigned to different academic departments within colleges and universities. The physical spaces, whether they are for physics, chemistry, or psychology departments, provide demarcations for the types of knowledge produced. Physics departments produce knowledge on properties of the universe such as gravitational and electromagnetic forces. Similarly, chemistry departments are physical spaces arranged for producing knowledge about inorganic compounds such as phosphate ions and carbonic acid. Psychology departments, of course, are spaces arranged for the production of knowledge about human thought, behavior, and emotion.

These physical boundaries may be broken down further into separate laboratory spaces that signify the knowledge production of distinct subdisciplines. Cognitive psychologists, for instance, have laboratory spaces configured in ways

[3] While it is common to consider disciplines as strictly deriving from higher education systems, this is a limited conception of the term "discipline." There are many individuals conducting research or that have clinical practices that hold no *institutional* affiliations but are still affiliated with a particular discipline. For instance, industrial/organizational psychologists work predominately in businesses and corporations, but they still consider themselves affiliated with the discipline of psychology. The same thing is true of clinical and counseling psychologists who choose to work in private practice or hospitals.

that differentiate their spaces from those of physiological and social psychologists. But this notion of physical boundaries extends beyond university settings. Outside the academic realm, clinical and counseling psychologists practice their disciplinary training in physical spaces such as private practices and hospital mental wards.

The intellectual components of disciplinary boundaries consist of *how* knowledge producers investigate and think about the subject matter within their discipline. For instance, scientists' training in the classroom, laboratory, and in the field play a role in circumscribing a discipline's boundaries. Other components that form this intellectual component include their methodologies, concepts, theories, and instruments. For example, while biologists, psychologists, and sociologists all study human beings, their training and methods constrains them to study human beings in different ways. Biologists may study human mating behavior in a qualitatively different way than either psychologists or sociologists. Biologists may be interested in only comparing the allele frequency of human mate pairs. They may not be concerned with the level of commitment each partner shares with the other, or the strategies they used to become a mate pair. Psychologists, on the other hand, may not concern themselves with allele frequencies, but may instead investigate the motivational underpinning of mate choices, or the dominance hierarchies in relationships. Sociologists may ignore the approaches of biologists and psychologists and perhaps study the ramifications of teenage pregnancy on the welfare system.

Closely related to the intellectual component of disciplinary boundaries is how knowledge producers use rhetoric to delineate their discipline's boundaries. This is often a powerful way for them to describe the contours of their disciplinary boundaries. The purpose of rhetoric is to persuade an audience of the credibility of a particular claim. In their books, presentations, and journal articles, scientists use rhetorical devices to persuade audiences of their science's reliability and integrity. One way scientists establish a discipline's intellectual credibility is by writing their discipline's history. In the words of James Good and Richard Roberts (1993) scientists often employ a "rhetoric of legitimation" in these histories. Scientists legitimize their science by telling internalist histories of the "great" dates, ideas, and scientists of their discipline for the purposes of gaining respect. Psychologists, no less than other scientists, employ this strategy frequently in writing their disciplinary histories. For instance, the historian of psychology Edwin G. Boring (1929) virtually ignored the applied and clinical areas of psychology when writing his history of the field. Numerous historians of psychology including Mitchell Ash (1983), Laurel Furumoto (1989), William Woodward (1995), and Benjamin Harris (1997) have critiqued these types of disciplinary histories as serving a legitimizing purpose and distorting the past.

Other rhetorical devices scientists use are *inscriptions* to persuade audiences of the validity of their claims (Latour, 1987). Inscriptions are visual representations of knowledge such as graphs, figures, and tables within texts. These inscriptions augment or reinforce the author's textual statements. Scientists especially use inscription practices as a means to reinforce their knowledge claims. For instance, graphs allow a potentially doubtful reader to experience visually the changing nature of a particular phenomenon. Irrespective of the branch of science from which a scientist hails, they all engage in these types of rhetorical strategies to reassure others and themselves of their disciplinary boundaries. While chemists can speak more assuredly on the number of protons in each chemical element than psychologists can about the structures and functions of the cerebral cortex, both types of scientists employ rhetoric in an effort to demarcate their knowledge claims from others'.

In whatever manner these boundaries are fashioned, they are an important, though often unexamined, component of many sciences. They provide a sense of identity, status, stability, and community for each discipline's members. The sociologist of science Steven Fuller (1991) describes the role of disciplinary boundaries as "providing structure needed for a variety of functions, ranging from the allocation of cognitive authority and material resources to the establishment of reliable access to some extra-social reality" (p. 302). For scientists, disciplinary boundaries connote areas of expertise. High energy physicists, for example, reputedly possess expert knowledge of subatomic particles like electrons and photons, but not expert knowledge on the behavior of galaxies and solar systems. In psychology, most consider clinical psychologists experts at identifying and treating psychopathological conditions such as depression, anxiety, and schizophrenia. But clinical psychologists are not experts in human judgment and decision-making.

One of the benefits of this shared expertise among scientists is the accumulation of power. This power often manifests itself in scientists' decisions about incorporating knowledge into or excluding knowledge from their discipline's boundaries. This is the process of *boundary-work*.

Disciplinary Boundary-Work

Protecting, patrolling, and reconfiguring a science's boundaries is what the sociologist of science Thomas Gieryn (1983) identifies as boundary-work. Boundary-work usually takes the form of either scientists' *resistances* or *accommodations* to new concepts, theories, and technologies. Examining a science's boundaries can answer many useful questions about a science's history. For instance, one can trace how a discipline's boundaries have changed since its inception. Disciplinary boundaries indicate the kinds of knowledge deemed useful or inappropriate by scientists during a particular period. Similarly, boundaries suggest the types of practices scientists use and how these practices change as a result of scientists' resistances and accommodations to different forms of knowledge. Examining disciplinary boundaries can also specify how scientists promote, stabilize, and expand their discipline. Finally, these boundaries suggest the kinds of changes that occur within a discipline when scientists reconfigure their field's boundaries.

One of the reasons that scientists open their discipline's boundaries is to augment their existing cognitive authority by incorporating some theory, technology, or method. For instance, in the decades between 1920 and 1940 paleontologists and experimental biologists opened their disciplinary boundaries after they realized the importance of Gregor Mendel's work on heredity for understanding how species evolve (Bowler, 1989). Combining Mendel's work with Darwin's theory of natural selection resulted in the "modern synthesis" in evolutionary biology (e.g., Dobzhansky, 1951; Huxley, 1942). The modern synthesis helped promote evolutionary biologists' cognitive status in the sciences by providing a cohesive conceptual framework for the discipline.

Conversely, there are times when scientists attempt to close their boundaries and prohibit some theory, concept, opinion, practice, method, object, or instrument deemed undesirable from entering the field. Sometimes knowledge may threaten the existing authority of scientists and they may take action to remove the perceived threat. Before the 1920s, for instance, evolutionary biology relied on Lamarckism and orthogenesis to explain the process of species evolution.[4] At the time, both ideas were popular among the lay public and many scientists. This popularity was due, in part, to the implicit suggestion that some higher power controlled the evolutionary process. This idea resonated with the beliefs of many of the religious-minded public and scientists at the time. But relying on teleological explanations was unacceptable for an assumedly rigorous science like evolutionary biology. Evolutionary biologists could not substantiate either of these ideas in their research. It was threatening to the credibility of their science and they eventually eliminated Lamarckism and orthogenesis in favor of Mendel's and Darwin's ideas (Bowler, 1989).

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[4] The French naturalist Jean Baptiste de Lamarck developed Lamarckism in 1809. Lamarckism is the theory that species evolve through the inheritance of acquired characteristics. Orthogenesis, which was espoused by many naturalists in the nineteenth century, is the process of species evolving along a predetermined linear path (Mayr, 1991).

Statement of Purpose

I will explore the modification of U.S. psychology's disciplinary boundaries by systems of knowledge produced by scientists from three interdisciplinary programs—evolutionary psychology, cognitive science, chaos theory— between 1970 and 2000.[5] Scientists from each of these interdisciplinary programs introduced different forms of knowledge into psychology that somehow affected the field and the practices of some of its scientists and practitioners. Interdisciplinary programs arise when scientists from disparate fields come together because of some common interest in a particular problem or subject. To examine these forms of knowledge, I look at the types of practices these scientists introduced into the discipline and the types of boundary-work psychologists used in reaction to these practices.

Practices under Investigation

Following the emphasis on examining scientific practices from the sociology of scientific knowledge literature (e.g., Pickering, 1995; Shapin, 1982; Sulloway, 1991), I divided scientific practices into three interrelated categories: *conceptual*, *material*, and *social*. Conceptual practices consist of activities pertaining to what is commonly associated with the cognitive dimensions of science. Conceptual practices include activities such as the use of metaphor for introducing new concepts into the field, developing or refining research methods,

[5] The term "interdisciplinary" has numerous interpretations and the scientific literature is rife with terms that connote different aspect of interdisciplinarity. The humanities scholar Julie Thompson Klein (1990) discusses the many terms used to exemplify interdisciplinarity. These terms include "multidisciplinarity," "pluridisciplinarity," and "transdisciplinarity." I use the terms "interdisciplinary" and "interdisciplinarity" as they appear to be the most recognizable and appropriate for my research.

and constructing and modifying scientific theories. Examples from psychology include the refinement of models of human memory systems and introducing new statistical techniques for analyzing data.

Material practices are those that deal with the physical manifestations of a science. They include the construction of tests and assessment procedures, the manipulation of laboratory equipment and space, the creation of new instruments and apparatus, and the use of existing instruments and apparatus for new purposes. In psychology, material practices would include the development and implementation of intelligence tests such as the Stanford-Binet Intelligence Test and the Wechsler Adult Intelligence Scale. Material practices also would include the development and use of instruments like functional resonance magnetic imaging (fMRI) devices and Skinner boxes.

Social practices deal with the interpersonal dimensions of science. Social practices entail attending conferences, publishing research, interactions with funding agencies, establishing relationships between laboratories, and facilitating associations with other academic institutions and departments. In psychology, social practices take the form of endeavors such as applying for funding from the National Institute for Mental Health or publishing in the journal *American Psychologist*.

These types of practices are interrelated. Scientists who write empirical research articles usually publish their findings in scientific journals, book chapters, textbooks, or monographs (social practice). Scientists most often derive inspiration for their research from some kind of theory and their research findings contain various concepts, models, methods, and possibly metaphors that support their research findings (conceptual practice). It is also feasible to assume that scientists who publish their research have developed specific instruments or artifacts like particle accelerators or Skinner boxes for investigating a particular phenomenon (material practice).

I contend that the conceptual, material, and social practices of evolutionary psychologists, cognitive scientists, and chaos theorists spurred psychologists to engage in boundary-work. Psychologists either resisted or made accommodations for these practices that ultimately restructured the boundaries of psychology.

Psychologists resist certain practices because they have their own conceptual, material, and social practices that they use in their day-to-day professional activities. In his book *Academic Tribes and Territories*, Tony Becher (1989) emphasized that "the attitudes, activities and cognitive styles of groups of academics representing a particular discipline are closely bound up with the characteristics and structures of the knowledge domains with which such groups are professionally concerned" (p. 20). Psychologists train either explicitly or implicitly in these types of practices; therefore, they have a professional and sometimes emotional attachment to these practices. Conversely, psychologists can sometimes make accommodations for a new practice. This usually happens when they realize the need for or practicality of employing a new analytic technique for their data. Or they may adopt a concept from another discipline that more appropriately conveys the meaning of a particular subject. I will demonstrate instances of these resistances and accommodations in my case studies.

Rationale for my Investigation

I have two reasons for investigating psychologists' boundary-work and using these three programs as my case studies. First, my historical research interests intersect with these three interdisciplinary programs. Evolutionary psychologists investigate adaptations in human behavior, thought, and emotions. Cognitive scientists study the computational aspects of human cognition. Chaos theorists deal with orderly behavior in seemingly randomly behaving systems. Fortunately, these three programs infringed upon psychology's boundaries at approximately the same time. Secondly, the study of boundary-work in psychology is relatively uncharted territory. I believe that studying psychologists' boundary-work is essential for developing a deeper understanding and appreciation for the history of the discipline. Many authors of history and systems of psychology textbook still subscribe to the great man or great idea style of writing so they tend to present distorted disciplinary histories (e.g., Hergenhahn, 2001; Schultz & Schultz, 2000). Many of these textbooks are the work of psychologists who are amateur historians. Ever since Robert Young's (1966) criticism of the scholarship in the history of the behavioral sciences, professional historians of psychology have responded with revisionist histories of the discipline focused on narrower topics in specific contexts (e.g., Blumenthal, 1975; Furumoto & Scarborough, 1986; Guthrie, 1998; Harris, 1979). My approach complements revisionist histories of psychology in that it focuses on boundary-work in specific and narrow contexts, but it also brings to light external factors (i.e., other scientists' practices) that play a role in shaping psychology's boundaries. Employing boundary-work analysis also allows one to move between the "micro-histories" common in today's historiographic approach and the "big picture" approach advocated more recently by historian of science Roger Smith (1997; 1998).

Structure of my Investigation

In Chapter 1, I discuss the processes involved in scientists' boundary-work. I introduce three kinds of threats to scientists' authority and explain the types of boundary-work used in reaction to these threats. Next, I discuss how boundary-work encapsulates the different activities in which scientists engage for *establishing*, *maintaining*, and *extending* a discipline's boundaries. Finally, I discuss how scientists resist and accommodate different forms of knowledge in their boundary-work activities.

In my second chapter, I go deeper than I have in this introduction into the complexity and mercurial nature of U.S. psychology's boundaries. I try to capture what these boundaries looked like between 1970 and 2000 and provide historical examples of psychology's changing boundaries. To do this, I provide two illustrations of psychology's changing boundaries during its history as a discipline. Secondly, I identify three reasons for psychology's complex boundaries during the 1970s up to the new millennium: (a) the different levels of analysis in psychological studies, (b) psychology's reflexive nature, and (c) the complexity of its disciplinary structure.[6] Lastly, I review the state of psychology's boundaries during this period by discussing psychology's professional structure as an academic discipline, its training of students, and its putative fragmentation into specialty areas.

In the next three chapters, I introduce my case studies: evolutionary psychology, cognitive science, and chaos theory. I provide a brief history for each case study, the program's theoretical framework, and the conceptual, material, and social practices that affected psychology's boundaries. Finally, I discuss the resistances and accommodations psychologists displayed when carrying out their boundary-work in response to the practices of these interdisciplinary programs.

In my concluding chapter, I argue that despite psychologists' efforts in the thirty years between 1970 and 2000 to establish and maintain psychology's disciplinary boundaries, my investigation indicates that the boundaries of U.S. psychology remained fluid, permeable, and dynamic. Psychologists especially resisted the introduction of new conceptual practices. They were less likely to resist new material and social practices. Despite these resistances, accommodations occurred with all three kinds of practices for all three interdisciplinary programs. Conversely, psychologists more readily accepted the various practices of cognitive scientists and evolutionary psychologists than they did the practices of chaos theorists. Interestingly, today some psychologists are

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[6] Note that these three reasons for psychology's complex boundaries are just as applicable today as they were thirty years ago. See Stephen Yanchar's (1997) discussion on the fragmented structure of the discipline.

combining these three interdisciplinary programs in various ways in order to conduct their research.

Finally, I discuss the benefits of using the sociology of science and boundary-work for understanding and appreciating the historical processes that helped shape psychology. Psychology's boundaries change or shift as psychologists carry out their own conceptual, material, and social practices. But it is also important to recognize that psychologists' resistances and accommodations to the various practices permeating their field's boundaries produce additional changes and shifts. These boundary-work activities need to be explored more fully in order to obtain a more comprehensive account of the history of psychology and to appreciate the ways in which psychology dictates and is dictated by the social world.

CHAPTER I

DISCIPLINARY BOUNDARY-WORK AND THE SOCIOLOGY OF SCIENTIFIC KNOWLEDGE

Boundary-work analysis is a procedure used to understand scientists' activities for protecting the cognitive authority of their field. Boundary-work is an idea that came to fruition within the field of sociology of scientific knowledge (SSK), which is an area of science studies. "Science studies" is used as an umbrella term to represent fields such as science and technology studies, sociology of science, sociology of scientific knowledge, philosophy of science, gender studies, literary studies, and the history of science (Biagioli, 1999; Pickering, 1995). The field of SSK came into prominence in science studies within the past thirty years mainly through the efforts of British sociologists and philosophers. Recently it has enjoyed growing success among U.S. sociologists, philosophers, and historians of science as well as various researchers in France, Germany, Israel, and Australia (Shapin, 1995). At present, sociologists of science have mainly analyzed the physical and biological sciences (e.g., Barnes & Shapin, 1979; Jasanoff, Markle, Petersen, & Pinch, 1995; Latour & Woolgar, 1986; Pickering, 1992; Shapin, 1982). Despite its growing popularity, there has been very little work done by historians of psychology from this perspective. This is unfortunate because I believe SSK can contribute a complementary dimension to previous historical studies in psychology, especially pertaining to psychologists' boundary-work activities. Before exploring the process of boundary-work itself, it is important to gain an understanding of SSK. Many dimensions of boundary-work analysis stem from the ideas put forth by sociologists of scientific knowledge.

Sociology of Scientific Knowledge

The sociology of scientific knowledge is a heterogeneous field of study because there are many different conceptions of the appropriate objects to study in science. These conceptions have changed throughout SSK's disciplinary history. I outline a few of them here. From the 1930s to the 1960s, Robert Merton's (1937; 1938; 1968) ideas on the sociology of science were taken as the definitive position in the field. Merton focused on the rewards in science (e.g., Nobel Prizes) and the effect these had on how scientists conducted their work. Merton (1968) observed that eminent scientists received a disproportionate number of rewards for their work, while other scientists, though perhaps just as deserving, did not. These rewards, or the lack of them, affected the motivational level of the scientists. One of the criticisms of Merton's approach is that he did not take his ideas much further into the social dimensions of science. To Merton, the veracity or "truth-value" of scientists' knowledge products were unquestionable and, therefore, untainted by social elements. It was not until the 1960s that SSK became more social in its focus.

In the late 1960s and into the 1970s a more social component came to the forefront in studies of science inspired by some radical ideas about scientists and the making of scientific knowledge. Thomas Kuhn's (1962/1996) widely read book *The Structure of Scientific Revolutions*, and the philosopher Paul Feyerabend's (1978) *Against Method* became popular frameworks for understanding scientific practice. Kuhn criticized scientists' beliefs in the "scientific method" and their belief that this method would lead to some form of absolute truth. Instead, he chose to focus on the social dimensions of science that allowed scientists to conduct their work. Kuhn suggested that the use of exemplars—problems scientists were trained to see during their indoctrination as students—were used as analogies to solve new problems in the course of "normal science." The main point that Kuhn contributed to SSK was not the popularly used term "paradigm shift," but the notion that scientists' training dictates their observations, thus *creating* scientific facts in particular experimental contexts. Like Kuhn, Feyerabend questioned the validity of a single scientific method and demonstrated that many scientists do not follow a single scientific method—the idea that "anything goes"—using examples from usually unquestioned models of progress in science like the Copernican revolution.

Since the 1970s, sociologists of scientific knowledge place even more emphasis on the social components of science. For example, they moved beyond merely looking at how social components *interfere* in scientific practice and replaced this perspective with a focus on how social components *contribute* to scientific practices. The historian of science Jan Golinski (1990) identified two ways sociologists of scientific knowledge are now looking at scientific practices. First, there are the ethnomethodologists that study scientists *in situ* such as Bruno Latour's (1986) study of biochemists, Karin Knorr-Cetina's (1981) work with groups of scientists studying plant proteins, Andrew Pickering's (1981; 1984) investigations of physicists studying quarks, and Harry Collins' (1974) study of laser research. Next, there are studies that look at how science is made public in books, journals, and presentations such as Timothy Lenoir's (1998) work on communication and inscription practices. In his book, *The Mangle of Practice*, Pickering (1993) advocates a different version of SSK through an investigation of how materials in science, such as machines, have an agency all their own that act upon humans. For instance, the weather controls our behavior to a great extent in that if it is raining we are apt to pack an umbrella for work. His argument is that humans are not the only things in science that possess agency. Materials, according to Pickering, also possess agency that can affect scientists' practices.

The main function of SSK is to develop a comprehensive account of how science works, that is, how scientists develop, use, and maintain scientific knowledge. But what constitutes knowledge, let alone scientific knowledge? Barry Barnes (1990) outlines four characteristics of knowledge. First, any body of knowledge exists as beliefs inherited from previous generations. In the case of scientists, they inherit these bodies of knowledge from their mentors, teachers, textbooks, and other members of their discipline. Secondly, one can distinguish between inherited knowledge and mere belief in that some kind of authority (e.g., credible scientists) creates this knowledge. This characteristic is what distinguishes authoritative knowledge from mere belief. Another characteristic of knowledge is that various procedures accompany it for the purpose of validating this knowledge in particular contexts. There are methods, instruments, and various technologies that demonstrate and authenticate this knowledge. Things like laboratory equipment, computer simulations, data analysis software, and videos validate this knowledge. Lastly, individuals can use and interpret this inherited knowledge in different ways; however, this is one of the problematic aspects of knowledge. According to Barnes, there are no assurances when it comes to the proper use of the inherited knowledge.

While there are currently numerous versions of SSK (e.g., the Edinburgh School and the Bath School), sociologists of scientific knowledge generally agree that in order to understand the process of knowledge production in science it is important not only to study the final product of scientific research, but also to consider the social forces that act on scientists and the practices they utilize to develop their scientific knowledge.[7] Summarizing the impact and importance of these types of studies, the historian of science Jan Golinski (1990) stated:

> In tackling the practices of scientists at work, these researchers could be said to have transcended the limits traditionally regarded as circumscribing sociological investigations; they were dealing with the 'content' of science and not just with its institutional 'context.' By concentrating on the observable features of scientific practice, they had exposed the interpretive and contingent nature of experiments, bringing to

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[7] Both the Edinburgh School and the Bath School are representatives of the "strong programme" in science studies. Essentially followers of the strong programme believe in the primacy of social factors in the creation of scientific knowledge. This contrasts with weaker versions of this position where rational thought and empirical observation supersede the social components of scientific knowledge. The Edinburgh School is associated with David Bloor and Barry Barnes. The Bath School is associated with Harry Collins.

light the rich variety of elements that enter into the making of scientific facts. (p. 493)

Sociologists of science contend that because scientists are human, they are prone to subjective experiences that affect their judgment and their interpretations of the knowledge they produce. A founder of the "strong programme" in SSK, David Bloor (1991), in addition to numerous other sociologists of science (e.g., Barnes, Bloor, & Henry, 1996; Knorr-Cetina, 1999; Pickering, 1981) argue that cultural influences, usually presumed to have no effect on science, actually affect the creation and development of scientists' theories and findings. Cultural phenomena such as the types of external funding programs available and pressures put on scientists by lobbyists and politicians play a role in the development of scientific knowledge. Encapsulating this idea, Andrew Pickering (1992) stated that "scientific knowledge has to be seen, not as the transparent representation of nature, but rather as knowledge relative to a particular culture" (p. 5). Undoubtedly, most scientists would acknowledge that outside forces do affect the production of their knowledge. They, however, would probably argue that this does not affect the "truth value" of the knowledge they produce. While not denying that external factors affect their practices, they probably would deny that these factors affect the factuality of their knowledge. Scientists commonly argue that other laboratories run by different experimenters can replicate their findings. Another common argument is that their data fit a particularly successful theory. Truth eventually separates from untruth even if there are occasional local disagreements. One of the responsibilities of sociologists of science is to question this confidence and describe instances where replication or theory fails to produce factuality.

Steven Shapin (1982), in his influential article on the benefits of the SSK perspective for the history of science, provides some examples. Shapin discusses how, in the 1860s, Thomas Henry Huxley, a British biologist and one of Charles Darwin's most passionate supporters, discovered a protoplasmic organism he named *Bathybius haeckelii* in sea-bed mud that had been preserved in alcohol for 10 years. A number of American, German, and British biologists and geologists replicated these findings. At the time, Huxley's findings were significant because they lent credence to two theories about the origins and nature of life on earth: the nebular hypothesis and abiogenesis.[8] As time passed, many biologists and geologists questioned the factuality of Huxley's findings. They argued that *Bathybius haeckelii* was not a living organism but, in fact, partly constructed by the observers' imagination and the effect of alcohol on calcium sulfate contained in the sea-bed mud. Those scientists who supported either the nebular or the abiogenesis hypotheses refused to admit their mistake because they were intellectually committed to these beliefs. *Bathybius haeckelii* supported their theories about the process and origins of life on earth. Shapin concluded the story with the fate of *Bathybius*, which "died a gradual death, assisted by the

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[8] Immanuel Kant and Pierre Simon de LaPlace developed the nebular hypothesis independently in the eighteenth century. LaPlace posited that the alignment of the planets around the Sun was caused by gas and dust collapsing and spinning due to gravitational forces. The planets, the Sun, and their planar alignment were a result of these forces. Evolutionists such as Robert Chambers used this hypothesis as a means to explain the configuration of the natural world through historical contingency (Bynum, Browne, & Porter, 1981). Abiogenesis, also known as spontaneous generation, is the idea that non-living things can produce living organisms (Thain & Hickman, 1994).

writings of scientists who opposed the theories which its existence had been used to support" (p. 160).

This is not to say that sociologists of scientific knowledge believe everything scientists discover are illusions. This represents an extreme form of relativism that inhibits meaningful discourse on any subject. Rather, sociologists of science believe it is important to realize that science is inherently a social activity between not only scientists, but also between scientists and the rest of the world. These social activities are worthy of investigation in order to obtain a more comprehensive picture of the nature and history of science.

Instances of external factors that influence scientists are relatively abundant. In his history of the life of Francis Galton, the geneticist Nicholas Wright Gillham (2001) discussed the influence of the social and political climate of Victorian England on Galton's belief in the hereditary nature of people's mental abilities. Many scientists agreed that social class and profession were the direct result of race and proper breeding. Rich white men were successful because, in the words of Herbert Spencer, they were more "fit" to survive in the world. Conversely, "savages" from Tierra del Fuego, for instance, were savage because they were lower on the evolutionary ladder than whites were. During his life, Galton observed that many people of eminence—judges, scientists, literary figures—also had offspring who were successful. He concluded, albeit erroneously, that those mental abilities were inherited from the parents by their offspring. It was not conceivable to Galton—and many others in the Victorian climate—that environmental factors such as lack of money, social resources, and education could play a role in whether or not one would become eminent. In a similar vein, the conservative political climate at that time affected research on eugenics—a term coined by Galton to connote social changes that could improve the human race. In the U.S. during the early nineteenth century, for example, many wealthy individuals were concerned about the growing numbers of the poor and convinced legislators to pass immigration acts and forced sterilization laws to prevent the poor from procreating. The popular opinion at the time was that preventing the poor, criminals, and the feebleminded from procreating would lead to improvements in society.

In the next section, I move from SSK to discussing boundary-work. I begin by discussing two philosophical perspectives on boundary-work, and then I move into a definition of boundary-work itself. I then discuss the types of threats that may prompt scientists to engage in boundary work. Next, I present three types of boundary-work common in the sciences. The last two sections cover the distinction between permeable and impermeable boundaries and the types of behaviors scientists use while engaging in boundary-work.

Philosophical Perspectives on Boundary-work

Gieryn (1995) outlined two philosophical perspectives for approaching the study of scientific boundaries: essentialism and constructivism. Essentialists believe that the boundaries of science are real, not locally contingent, and invariable. These boundaries constitute what separates science from other human activities. From the essentialists' standpoint, science and the scientific approach to knowledge produce valid and reliable findings. Science produces truths and facts. If problems ever arise from scientific findings, the fault lies not in science, but with the individual or the method used to produce the knowledge. Science is inviolate, while human interpretations of the data are faulty.

Constructivists' argue that there is no one method of science that produces valid and reliable knowledge. Instead, science is locally contingent, subject to contextual factors that affect the activities of science in a particular place or at a particular time. As Gieryn (1995) stated:

> 'Science' is no single thing: characteristics attributed to science vary widely depending upon the specific intellectual or professional activity designated as 'non-science,' and upon particular goals of the boundary-work. The boundaries of science are ambiguous, flexible, historically changing, contextually variable, internally inconsistent, and sometimes disputed. (p. 792)

I take the perspective of the constructivists in my investigation. Based on my research, there is enough evidence to suggest that scientific boundaries are subject to the individuals within a scientific discipline and to factors outside the discipline affecting its boundaries. The constructivist position I take does not entail denying that there is a real world out there that impinges on our senses. The issues of the fundamental reality of the world and the possibility that facts actually are what they supposedly represent are not at issue here. What is at issue here is the contingent nature between sciences' boundaries and human perception, bias, and interpretation in the production of these boundaries. This is where the constructivist perspective plays an essential role.

Outline of Boundary-work

Traditionally, sociologists of scientific knowledge use the term "boundary-work" to describe attempts by scientists to raise their field of knowledge production to authoritative and expert status.[9] Usually this occurs when scientists make the distinction between a legitimate science and those disciplines considered "non-sciences" or "pseudo-sciences." According to Gieryn (1999), the process of boundary-work is the "discursive attribution of selected qualities to scientists, scientific methods, and scientific claims for the purpose of drawing a rhetorical boundary between science and some less authoritative residual non-science" (p. 4-5). Based on Gieryn's articulation, boundary-work represents the action of demarcating one discipline's intellectual territory from another discipline's intellectual territory or from a group's collective knowledge generally considered outside the scientific arena (e.g., religious organizations). Metaphorically, scientists' boundary-work produces a cartographic illustration of disciplinary terrains that function as guideposts for the relationships between scientific disciplines or from groups outside of science. These work in much the same way as traditional maps portray the boundaries between counties, states, and countries. Unlike traditional maps that represent fairly permanent demarcations, maps of scientific boundaries change as scientists incorporate or expunge material when an individual or group contests this material. Gieryn (1995) proclaims:

[9] While I use scientists for my examples, other endeavors such as businesses and politicians use boundary-work too.

> Whatever ends up as inside science or out is a local and episodic accomplishment, a consequence of rhetorical games of inclusion and exclusion in which agonistic parties do their best to justify their cultural map for audiences whose support, power, or influence they seek to enroll (p. 406).

The historian of science and science writer Michael Shermer (1997; 2001) exemplifies one strategy that scientists use to expunge the knowledge claims of pseudo-scientists.[10] Shermer specializes in debunking claims of pseudo-scientists such as those who claim they have discovered extra-sensory perception (ESP) or those who believe in alien abductions. In the case of those believing in ESP, Shermer emphasizes the role of controlled experimentation that seems to refute any kind of claims of ESP. In the case of alien abductions, especially those who believe this happens at night, Shermer and others propose this is merely a case of sleep paralysis.

Scientists' boundary-work leads to a discipline's cognitive or "epistemic authority" whereby scientists gain the "power to define, describe, and explain bounded domains of reality" (Gieryn, 1999, p. 1). Boundary-work within and between the sciences delineates those social groups that have access to "factuality" and "truth" within given knowledge domains and who do not possess these truths. Those possessing this knowledge are generally considered experts who are granted the authority to guide those ignorant of these truths (i.e., other

[10] Many scientists, philosophers, and science writers engage in ousting pseudo-scientific claims from scientific orthodoxy to teach students and the public about critical thinking (e.g., Gardner, 1957; Plait, 2002; Stanovich, 2001).

scientists and the public). Given the power bequeathed to scientists in their role as experts, it is only reasonable to assume that scientists are motivated to keep their expert status. Barry Barnes and his colleagues (1996) contend that "scientific boundaries are defined and maintained by social groups concerned to protect and promote their cognitive authority, intellectual hegemony, professional integrity, and whatever political and economic power they might be able to command by attaining these things" (p. 16). Despite the epistemic authority granted to scientists through their boundary-work and their motivation for keeping this authority, there exist threats to the authority of these scientists.

Threats to Scientists' Authority

Threats to scientists' epistemic authority come mainly from three sources (Gieryn & Figert, 1986). First, institutions outside scientific circles, such as religious organizations, corporations, and engineers, may claim authority over specific forms of knowledge. A current exemplar of this kind of threat comes from the claims of creationists about "intelligent design." Intelligent design is the idea that living organisms are so complex, yet perfectly designed for their environment, that there has to be some form of intelligent designer (usually a deity) that at least started the process of evolution (e.g., Behe, 1996; Demski, 2002; Kenyon, 1989). In the guise of legitimate science, creationists attack evolutionary theory as consisting of unfounded speculation.

Another example of this kind of threat comes from Gieryn's (1983) study of the competition between scientists and engineers in the nineteenth century over claims of scientific knowledge. Each group tried to establish their profession as superior to the other. Gieryn focuses on the Victorian science writer John Tyndall and his disputes with engineers on who possessed expert knowledge. Tyndall used various rhetorical strategies to make scientists look more appealing and have more epistemic authority to the public.

Political factions, such as the military, constitute the second threat to scientists' cognitive authority. The military may dictate the types of projects on which scientists work because they contribute a great deal of funding to various scientists, including psychologists. Additionally, the military may consider certain knowledge as best kept within military domains because of potential threats to national security. Unlike the usual production process, scientists working for the military may never be able to make their knowledge public.

The final threat comes from within the scientific community. Cases of fraudulent practices (e.g., plagiarism, falsifying data, and unethical treatment of participants) within scientific disciplines threaten the credibility of scientists. A well documented case in the early twentieth century of fraudulent practice within psychology stems from the debate between hereditarians and environmentalists over the factor most responsible for intellectual ability. Sir Cyril Burt, the British educational psychologist, was accused of intentionally falsifying his twin studies data on intelligence in an attempt to establish the supremacy of heredity over environmental factors (Zenderland, 1990).[11]

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[11] While the evidence to suggest that Burt falsified his twin studies data is convincing, the historian of psychology, Franz Samelson (1992), questions if this is not a secondary issue to a more serious problem. Samelson contends that the bigger issue is the question of how so many other psychologists ignored the blatant methodological flaws and still published Burt's research.

Another instance of questioning the credibility of scientists is the controversy concerning the psychologist Henry H. Goddard and his presumably retouched photographs of the Kallikak family. In his book, *The Mismeasure of Man*, paleontologist Stephen Jay Gould (1981) contends that Goddard purposely retouched the images to make the Kallikak family look ignorant and sinister to validate his claim that one side of the Kallikak family produced feebleminded offspring. The historian of psychology Raymond Fancher (1987) rebuffs Gould's claims and provides evidence that photographs taken during Goddard's time were sometimes retouched to bring out the physical features of the people.

Scientists do their best to counter these threats to their disciplinary authority. Scientists use discursive and rhetorical strategies to promote their theories, methods, practices, technologies, and knowledge claims as effective ways of discerning the truth about particular phenomena. Despite these threats, there still exists the idea that scientists are authoritative and expert knowledge producers. As a result, a discipline gains power over various social dimensions of its knowledge. With this power comes the authority to decide who obtains and utilizes this knowledge, how to best use the knowledge in the laboratory and in society, and who will benefit most from its implementation.

Types of Boundary-Work

In the following section, I outline three interrelated processes of boundary-work: (a) the establishment of a science's boundaries, (b) the maintenance of a science's boundaries, and (c) the expansion of its boundaries.

Establishing Boundaries

The establishment of a discipline's boundaries occurs primarily during the initial stages of a science's development. It is the time when scientists market their discipline to other sciences and the rest of society. This marketing strategy establishes who "owns" the discipline, demonstrates how it differs from other areas of knowledge, and outlines the relationship it has with these areas (Shumway & Messer-Davidow, 1991). As with the other stages, the initial stage of boundary-work contains struggles for power, expertise, and authority. During the establishment process, scientists attempt to provide justification for the worthiness of their discipline. Scientists distinguish their discipline from other scientific disciplines in addition to other professions outside the scientific community that supposedly have similar goals. Scientists validate their knowledge claims as "truthful" through the implementation of their various technologies and methodologies. Moreover, scientists persuade the public sector of their potential contributions to the advancement of knowledge and applications to society.

Many boundary-work studies pertain to the establishment of a science's disciplinary boundaries (Klein, 1996). Investigations of how scientists establish their discipline are useful for the appreciation of the genesis of a discipline. They can outline the successful and unsuccessful strategies scientists use to establish their science as legitimate. This is especially beneficial when developing a revisionist history of any science because earlier histories may contain "Whiggish" interpretations of events that tend to glorify the genesis of a discipline.

But often overlooked is how scientists maintain and extend their disciplinary boundaries.

Maintaining Boundaries

As a discipline matures, scientists focus more on maintaining their discipline's boundaries in order to keep their acquired credibility and material resources. As Steven Fuller (1985) suggests, by the time a science reaches this stage "its practitioners must define and maintain the 'normal' state of objects in the domain. This involves experimental and textual techniques for foregrounding the problematic claims under study against a background of claims that are stipulated to be unproblematic" (p. 5). Through scientists' continuing efforts to legitimize their field, a science becomes professionalized and institutionalized. Professionalization and institutionalization are of the utmost importance for gaining respect among scientific colleagues and the public because these processes demonstrate that the scientific community and the university system perceives a science as credible. Professionalization also helps to establish the standards, training regimes, and methods that the scientists will use as their daily practices. Gieryn and his colleagues (1985) believe that:

> 'Professionalization' appears as a form of occupational control: organizational innovations restrict the supply of practitioners (through training and licensing programs) and standardize professional practice by threat of sanction (through codes of ethics enforced by professional associations). The rise of professions to positions of relatively high prestige, resources, authority and autonomy results from successful

> struggles to obliterate, absorb, or dominate competing providers of 'similar' services or commodities. (p. 393)

As Gieryn suggests, during this stage scientists form professional organizations, create content-specific journals, and develop academic departments. Increases in funding opportunities from within educational institutions, governmental agencies, and private organizations usually accompany these developments. The benefits scientists gain from this increased prestige do not just affect the scientists. In her discussion of boundary maintenance, Henrika Kuklick (1980) suggests that "while the professionalized group enjoys increased wealth and prestige, society as a whole presumably also gains, because the achievement of professional organizations supposedly depends on an improvement in the quality of the services the group provides for the public" (p. 201). It is apparent that scientists have a stake in maintaining their boundary structure.

Expanding Boundaries

After a science becomes professionalized and institutionalized, scientists sometimes concern themselves with expanding or extending their science's disciplinary boundaries. Scientists undertake expanding their discipline's boundaries in many ways. Scientists sometimes incorporate alternative theoretical perspectives from other disciplines. They seek funding from sources normally sought from other fields. Scientists borrow technologies from other fields and engage in collaborative research with scientists from other disciplines.

This is a somewhat simplified version of the boundary-work process for the sake of explanation. The boundary-work process is usually more complex.

Establishing, maintaining, and extending boundaries do not always occur in a stepwise, linear fashion. Scientists constantly engage in these three processes at different times and with different emphases throughout a discipline's history. For instance, scientists may focus more on reestablishing their discipline's boundaries when certain funding resources are no longer available or a fundamental theory upon which a discipline depends is no longer viable. Scientists may be more apt to maintain the structure of their boundaries when new technologies created in other disciplines infringe upon their territory. Irrespective of the scenario, the particular type of boundary-work in which practitioners engage depends on numerous factors such as a discipline's internal coherence, relationships with other disciplines, political and economic changes, and a discipline's relationship to the public.

Impermeable and Permeable Boundaries

Disciplines differ in terms of their permeability based on the uniformity and coherence of their scientists (Shumway & Messer-Davidow, 1991). Viewed on a continuum, one can arrange disciplines in terms of their relative permeability or impermeability. In his book, *Academic Tribes and Territories*, Tony Becher (1989) distinguishes between these two types of boundaries:

> Impermeable boundaries are in general a concomitant of tightly knit, convergent disciplinary communities and an indicator of the stability and coherence of the intellectual fields they inhabit. Permeable boundaries are associated with loosely knit, divergent academic groups and signal a more

fragmented, less stable and comparatively open-ended epistemological structure. (p. 37-38)

Because disciplinary boundaries vary in terms of their permeability, different disciplines employ different kinds of boundary-work. Each field has different priorities with respect to how their scientists demarcate their discipline's boundaries and the strategies they use to defend them. Some sciences' boundaries change more rapidly than other sciences' boundaries based on advancements in theory, collaborations with other disciplines, and the availability of technological innovations and funding opportunities. It appears that psychology is a discipline with relative permeable boundaries. Psychology's boundaries change frequently because it is a young discipline, its subject matter is immense and certain areas of psychology deal with phenomena that are difficult to study empirically and this leads to many disputes of knowledge. Similarly, chemistry, physics, and biology are instances of disciplines with relatively permeable boundaries. Recently, their boundaries have overlapped in such fields as macromolecular studies, biophysics, and biochemistry (Klein, 1996). Other sciences are on more solid footing and do not engage in as much boundary-work. According to Becher (1989), economics is an example of a field with relatively impermeable boundaries. He argues that the field's practitioners accept basic economic theory and that very little knowledge from outside the discipline affects this basic theory. This is the case even though economics interacts with the fields of mathematics and political science. Becher emphasizes this point stating, "Within economics, those who question the basic axioms of the subject are regarded as deranged if not possibly dangerous, and are liable to find themselves cast into a wilderness of their own" (p. 37).

Resistances and Accommodations to Knowledge

While I have provided a typical definition of boundary-work, I use the term boundary-work to represent the dynamic interplay between scientists' *resistances* and *accommodations.* Frequently, resistance comes from scientists trying to maintain or police their discipline's boundary structure. Barry Barnes and his colleagues (1996) propose that scientists "can be expected to police the existing boundaries of science, to avoid the intrusion of whatever may detract from its reputation and to seek to expel anything potentially disreputable which arises within it (p. 140). Resistances occur when scientists perceive threats to their practices and accompanying knowledge domains. Conversely, boundary accommodations occur when practitioners—individuals or groups—within a discipline realize the potential utility of a particular idea, instrument, or theory coming from another discipline. The practitioners incorporate this knowledge into their own practices and sometimes fundamentally change areas of the discipline.

In summary, boundary-work analysis is a relatively recent approach to studying scientific practices within the sociology of scientific knowledge. Sociologists of scientific knowledge use scientists' boundary-work to analyze how scientists make distinctions between their own knowledge production activities and the knowledge production activities of other groups. Essentially, boundary-work manifests in three forms: establishment, maintenance, and expansion. Various scientists may focus on any of these three forms throughout a discipline’s history. Irrespective of the form in which boundary-work occurs, scientists exhibit either resistances or accommodations to the various conceptual, material, and social practices of scientists in other disciplines when these practices infringe upon their cognitive authority. With this framework in mind, I can now transition into on how psychologists have tried to establish, maintain, and extend their field’s disciplinary boundaries.

CHAPTER II

THE COMPLEX DISCIPLINARY BOUNDARIES OF U.S. PSYCHOLOGY

In his book, *The Human Sciences*, the historian of science Roger Smith (1997) issued this caveat: "Psychology has no 'origin': its identity in the twentieth century is fragmented and its roots diverse. Psychology is a cluster of activities with a family resemblance but no common identity" (p. 493). This statement would lead one to believe that a comprehensive history of psychology would be problematic. As a discipline, psychology is simply too complex to make any definitive statements about it as a science. Taken in one way Smith's caveat is true. Ever since psychology's professionalization and institutionalization as an academic discipline in the late nineteenth century, the disciplinary boundaries of U.S. psychology have undergone numerous changes. In a sense, psychology has had myriad "identities" throughout its history that connote the changing conceptions of psychological knowledge. At times viewed as a pseudo-science and at other times a natural science, psychology's multiple identities prove difficult to describe. Nevertheless, this complexity proves advantageous for studying the boundary-work process. When psychologists establish, maintain, or extend their field, these activities provide insight into psychologists' boundary-work activities. To begin outlining the complexity of psychology's boundaries, I present two examples of when psychology's boundaries changed. My first example derives from a time when psychologists were attempting to establish the boundaries of the field. My second example comes from how psychologists extended their boundaries during World War I and World War II. Lastly, I discuss the reasons for psychology's complex boundaries between 1970 and 2000 in addition to how psychologists attempted to manage them.

Psychologists Establish Psychology's Boundaries

Before the Civil War, one could consider psychology as having no boundaries. The discipline of U.S. psychology did not yet exist. Unquestionably, at this time there were many philosophers such as Herbert Spencer, James McCosh, and Jonathan Edwards, whose interests were psychological in nature, but there was no consensus as to what to study, no common methodology, no professional organizations, no scientific journals, and no academic departments devoted to psychology. Instead, U.S. colleges and universities in the middle to late 1800s offered courses in moral and mental philosophy (Fuchs, 2000; Kosits, 2004; Maier, 2004). Moral and mental philosophers were mainly Protestant ordained ministers (Richards, 1995). In general, they believed that in order to comprehend God's purposes, people must examine their own minds (Fuchs, 2000). The primary purpose of these moral philosophy courses was to train students for the seminary or professorships. Once trained, these individuals would then educate a new generation of students on the importance of examining the mind in order to understand and appreciate the relationship between humans and God.

Laboratories, Quantification, and Experimentation

The transition from moral philosophy to academic psychology came at a time when U.S. colleges and universities were in the process of departmentalizing in the German academic tradition (Watson & Evans, 1991). Many pioneers in psychology such as G. Stanley Hall, James McKeen Cattell, Lightner Witmer, and Harry Kirke Wolfe traveled to Europe to train with psychologists like Wilhelm Wundt at the University of Leipzig in the 1880s. They returned to establish psychological laboratories and clinics in various colleges and universities such as Johns Hopkins University, Clark University, the University of Pennsylvania, and Columbia Collage (Benjamin, 1992; Camfield, 1973; Leahey, 1992). Edward B. Titchener, employed at Cornell University after his training with Wundt, exemplified this new trend. Titchener's structuralist perspective focused on uncovering the elementary units of the mind through experimentation (Tweney, 1987). This was a move away from the more subjective introspective inferences about the mind made by the mental philosophers. Another instance of boundary-making that assisted in establishing psychology's boundaries was William James' (1890/1950) textbook, *Principles of Psychology*. James' *Principles* helped lay the foundation for an empiricist tradition in U.S. psychology that distanced itself from the metaphysics of Scottish faculty psychologists and the philosophy of associationists such as David Hume, James and John Stuart Mill, and Alexander Bain that were popular at the time. In 1875, James established the first psychology laboratory at Harvard University (Bjork, 1988). James' version of psychology was a psychology combining the work of physiologists, experimental psychologists, and comparative psychologists.

The establishment of these laboratories and clinics rapidly placed psychology within the academic landscape and helped shape its boundaries as a legitimate science. The historian of psychology David Leary (1987) stated that by the mid-1890s psychology "had all the trappings of an established scientific discipline: laboratories, journals, professorships, graduate programs, a professional organization, and at least one clear case of application" (p. 316). By 1903, psychology produced more doctoral students than all other sciences except physics, chemistry, and zoology (O'Donnell, 1985). U.S. intellectuals and students alike were coming in droves to the new field of psychology.

The quantification of and experimentation on psychological phenomena between the late nineteenth and early twentieth centuries was a major factor in separating psychology from its philosophical and metaphysical roots (Hornstein, 1988). Psychologists began defining their field by the phenomena they could quantify, while eliminating phenomena not quantifiable. This process helped promote psychology as a legitimate and empirical science. Establishing psychology as a legitimate science was of the utmost importance for the success of psychologists at the time. The psychologist Knight Dunlap (1920) echoed the belief of many psychologists at the time when he proclaimed that "there is a great need for a scientific psychology, and unless the scientific study of psychology is promoted, in laboratories, and in the field: and unless the teaching of scientific psychology is fostered in the class room, and in print, culture will be impaired, and social progress impeded" (p. 516). Historians of psychology generally agree that this was a pivotal time for establishing psychology as a genuine science. For instance, in his treatise on the origins of behaviorism, John O'Donnell (1985) remarked: "In the last quarter of the nineteenth century...psychology secured recognition as a separate academic discipline by promoting its status as an experimental laboratory science" (p. 1). The emphasis on instruments and instrumentation during the "brass instruments" age in psychology's history fostered a standardization of psychology's subject matter in addition to standardizing their human subjects. Echoing O'Donnell, Deborah Coon (1993) suggested that the "laboratory and its apparatus...become pivotal to the process of psychology's professionalization, and its differentiation from philosophy within the university" (p. 763). Psychologists' emphasis on developing laboratories, quantifying psychological phenomena, and their subsequent experimentation legitimized their field, helped establish its disciplinary boundaries, and putatively placed psychology on fairly equal footing with sciences like physics, chemistry, and biology. Psychologists such as Cattell also published their work in general science journals of the time. Journals such as *Science* and *Popular Science Monthly* helped legitimize the field (Camfield, 1973).

Psychologists and the World Wars

During and after World War I and II, U.S. psychologists expanded their areas of expertise and boundaries by including more applied areas, while continuing their quantitative studies of psychophysiological variables and mental testing. Public mental health, assessing national character, and social problems were some of the newer topics in which psychologists strove to become authorities. Attempting to extend psychologists' realms of expertise, psychologists such as Robert M. Yerkes of Yale University, Henry H. Goddard of the Vineland Training Institute in New Jersey, and Lewis M. Terman of Stanford University fervently petitioned the military during World War I to include psychologists in the war effort. The result of their efforts was the mental testing of military personnel. These mental tests served a dual purpose. First, mental tests identified those personnel supposedly mentally unfit for military service. Secondly, they served to identify soldiers with superior mental abilities for officer selection (Carson, 1993; Fancher, 1985; Reed, 1987). The mental testing of soldiers afforded psychologists the opportunity to create new instruments (mental tests) and employ them on a scale never before thought possible.

Women expanded the boundaries of psychology during this time as well. While women had been present in and contributed to psychology since its formation, they represented a minority in the field until World War I.[12] It was during World War I that large numbers of women began to fill the ranks. The influx of women was due in part to psychologists focusing on more applied and socially relevant problems (Furumoto, 1987). The general attitude of psychologists at the time was that the men would continue to invest their time in experimentation while the women, seen as being able to fill the "caretaker" role, were more skillful at applied problems. Women assisted in expanding

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[12] See Elizabeth Scarborough and Laurel Furumoto's (1987) *Untold Lives* for an examination of the first generation of women psychologists such as Margaret Floy Washburn and Mary Whiton Calkins and their contributions to and experiences in the field.

psychology's boundaries by taking on this role and immersing themselves in the care of the mentally ill, the poor, and the family.

World War II saw psychologists again collaborating with the military, however in a greater capacity than they did in the first war. This allowed psychologists to expand their boundaries even further. Military departments such as the Office of Strategic Services (the predecessor of the Central Intelligence Agency), the Office of War Information, and the Psychological Warfare Division of Supreme Headquarters sought out the expertise of psychologists, even giving them a name: sykewarriors. Psychologists, in their role as sykewarriors, interpreted and influenced enemy morale, and diagnosed "national character," which were personality profiles of various countries. Psychologists were moving beyond their role as mere mental testers and applying their various theories to worldwide problems. In her book, *The Romance of American Psychology*, Ellen Herman (1995) summarized the importance of these opportunities for psychologists stating, "The reputation of psychological experts had risen from one of lowly technician to one of wise consultants and managers whose wartime accomplishments...deserved a generous payoff in public appreciation and government funds" (p. 19). With the study of U.S. psychology becoming firmly embedded within academic institutions, the influx of women psychologists, psychologists' concerns with social issues, and the public coming to recognize psychologists as authorities on various aspects of social life, the boundaries of psychology expanded at an enormous rate.

After World War II, the disciplinary boundaries of psychology continued to expand in both the research and clinical areas. Research on cognition began in earnest, in part due to research during the war on feedback systems and information theory as well as the decline in popularity of behaviorist theories. In the clinical field, theorists such as Abraham Maslow and Carl Rogers developed humanistic theories of psychological functioning that clinicians subsequently used in therapeutic interventions. There is no question that psychology's boundaries changed throughout its history. The next question that needs addressing is what are the reasons for psychology's complex boundaries?

Reasons for Psychology's Complex Boundaries

There are three interrelated reasons for psychology's expansiveness and complexity. As I mentioned previously, psychologists are both reductive and expansive in exploring the "individual" or the "self." For instance at the microscopic level, physiological psychologists investigate subjects like the structure of various neurotransmitters (e.g., acetylcholine, serotonin, and dopamine), neural communication, and how extraneous substances like psychotropic medication, alcohol, and illicit drugs affect neural transmission. At another scale of representation, psychologists study various components of personality, cognitive development throughout the lifespan, gender differences, and a host of other phenomena. At the macroscopic level, psychologists investigate the social behavior of groups of people within cities, states, and countries.

Psychologists also study nonhuman species such as rats, pigeons, chimpanzees, and dolphins. There are several reasons for using nonhuman species in psychological research. Some species' reproduction rates are more rapid, such as the reproduction rates of laboratory rats. Faster reproduction rates allow psychologists to study successive generations more briefly. Some species' physiological systems are less complex than human physiological systems so experimental manipulations like cortical ablations are easier to perform. Laboratory animals are allegedly easier to control and standardize than humans during experimental research (Logan, 1999). It is easier to control extraneous and confounding variables with many laboratory animals because their environments can be controlled and modified based on the experimenter's discretion. Another reason why other species are used in experimental research is that ethical considerations regarding the treatment of human subjects prohibit carrying out certain investigations with humans but not with other species. While knowledge pertaining to other species' psychological and physiological makeup is useful, the primary goal of utilizing other species is for interspecies comparisons that produce usable knowledge about human psychological and physiological processes.

A second reason for psychology's disciplinary complexity is its reflexive nature. Psychologists interact with their subject matter *qua* participants. In discussing psychology's reflexivity, Roger Smith (1997) stated that there exists a "reciprocal relationship between psychology and the lives of ordinary people: people are ultimately both the subject matter of the human sciences and makers of psychological knowledge" (p. 57). Psychology differs from the natural sciences because of this reflexivity. Scientists such as chemists or physicists usually deal with inanimate substances. When chemists or physicists perform experiments on these substances, they usually do not have to worry about their substances interacting with them. Psychologists, on the other hand, interact with their subject matter through their experimentation and clinical interventions. Complications about the validity and reliability of their findings arise because participants in experiments or clients in therapeutic situations often act in unpredictable ways. Prejudice and bias can interfere with psychologists' establishment and interpretation of knowledge (Kirk, 1995; Rosenthal & Rosnow, 1991). Participants can be uncooperative and try to sabotage experiments. Participants may try to appease experimenters by reporting results they believe will please the experimenters.

Participants are not the only source of bias in experiments; experimenters can introduce bias into their own experiments. In their data, experimenters can often find the results they are expecting to find. Experimenters can exert pressure on participants to answer in ways that may be favorable to the desired outcome. These complications, and many others, lead to uncertainty about psychological knowledge. The uncertainty spurs psychologists to refine continually their hypotheses, methods of experimentation, and data interpretation. The ambiguity of interpreting data, the refinement of methods, and the myriad biases in psychologists' experimental research contribute to the complexity of the discipline.

A third reason for the complexity of psychology is the complexity of its disciplinary structure. Even though colleges and universities place psychology under one heading, it is comprised of numerous subdisciplines such as cognitive psychology, psychobiology, developmental psychology, personality psychology, and social psychology. Each of these subdisciplines has their own specialized training regimes for undergraduate and graduate students, esoteric terminology, funding resources, conferences, and content-specific journals. While these sub-disciplines reflect the various content areas of psychology, there is sometimes significant overlap between them. For instance, developmental psychologists may rely on a cognitive theory to interpret adolescent mental development or a personality theory to discern changes in personality through the lifespan. The vast array of subdisciplines and the interactions between them contribute to psychology's expansive and complex boundaries.

Changes in Psychology's Disciplinary Boundaries, 1970-2000

Throughout its history the internal sharing of conceptual, methodological, and technological resources between psychologists, and external factors such as collaborations with other scientists, the public sector, and governmental and political organizations all have contributed to psychology's changing boundaries. For instance, internal sharing occurs when one subdiscipline borrows psychological concepts from another. Some cognitive psychologists, for example, are reexamining Freud's ideas on the unconscious mind because of their interests in unconscious perception, memory, and emotion (e.g., Allen & Reber, 1998; Kihlstrom, 1999). Subdisciplines also borrow methodologies from other subdisciplines. The methodology of psychophysical reaction time used by Hermann von Helmholtz in the nineteenth century is now used to measure reaction times in cognition experiments (e.g., Pashler, 1993). Collaborations between psychologists in different specialty areas sometimes produce new directions for research. Collaborations between cognitive psychologists and cognitive neuroscientists produce mappings of the intersection between brain structures and information processing using functional magnetic resonance imaging (e.g., Hayes, Ryan, & Schnyer, 2004; Ragland, Gur, & Valdez, 2004). Newly developed theories foster investigations into subjects traditionally viewed as beyond the purview of psychologists. Theories of emotional intelligence are a set of newly developed ideas now employed in some school systems to monitor students' behaviors and emotional stability (e.g., Elias, Arnold, & Hussey, 2003; Tiwari & Srivastava, 2004).

Externally, governmental funding agencies such as the National Science Foundation and the National Institute of Mental Health, and private funding organizations like the Sloan and Templeton Foundations provide financial support for psychological research. These funding resources sometimes control the types of investigations that psychologists conduct. Psychologists often respond to the needs of various social institutions such as the need for improved assessment procedures in secondary education. Political and world events such as the September 11th World Trade Center attacks prompt psychologists to provide therapeutic assistance for the bombing victims and their families (Goodman, Morgan, & Juriga, 2004; Miller, 2002; Stewart, 2004). In a similar vein, psychologists have developed research programs for understanding terrorism (Ursano, Fullerton, & Norwood, 2003) while others have gotten involved in peace psychology and anti-war efforts (Christie, Wagner, & Winter, 2001).

Psychology's Professional Structure

An indicator of psychology's complex disciplinary boundaries is how psychologists partition the field. One of the ways psychologists partition the field is through the organizational structure of its professional organizations. The American Psychological Association (APA) is the largest professional organization of psychologists in the world. Since the APA's founding in 1892, the organization has burgeoned into fifty-five separate divisions.[13] The divisions span many diverse categories like military psychology, history of psychology, gay, lesbian, and bisexual issues, community psychology, theoretical and philosophical psychology, educational psychology, and personality and social psychology. These fifty-five divisions each have their own specialized research areas, separate informal communication networks (e.g., internet list servers), and many of them publish specialty journals. The myriad divisions of the APA indicate that the use of psychological knowledge spans many levels of society.

Academic Departments and the Training of Students

Within the institutional structure of higher education, psychology departments are often times divided into subdisciplines based on specialized

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[13] There are no divisions 4 and 11. Between 1945 and 1948, when the American Association of Applied Psychology and American Psychological Association formally organized, the Psychometric Society (Division 4) chose not to become a division. In 1946 Division 11, Abnormal Psychology and Psychotherapy, merged with Division 12, Clinical Psychology. Both divisions remain vacant today.

research areas. It is common to find psychology departments partitioned into specialty areas such as cognition, behavioral analysis, clinical or counseling psychology, physiological psychology, and developmental psychology. Depending on the particular department, there are different modes of indoctrinating students into the discipline.

The scientist-practitioner model of training students, also known as the Boulder model, was developed at the Conference of Graduate Education in Clinical Psychology in Boulder, Colorado between August and September of 1949 (Benjamin & Baker, 2000). The purpose of creating the Boulder model was to establish a standardized method of training graduate students. Psychology departments utilizing this model train graduate students in both the research and clinical dimensions of the discipline, with more emphasis placed on the research dimension.

Another model of training students is the practitioner-scholar model. The practitioner-scholar model emphasizes the counseling and clinical dimensions of training more than the research aspect. This model originated from dissatisfaction with the Boulder model's lack of emphasis on clinical training (Peterson, 2003). Several schools including Adelphi University, the University of Illinois at Urbana-Champaign, and the California School of Professional Psychology established programs for doctorates in psychology (Psy.D.).

<u>Psychology as a Fragmented Discipline</u>

The diversity and seemingly fragmented structure of psychology's subject matter makes psychology's disciplinary boundaries more complex. Voicing his concern about this fragmentation, psychologist Amedo Giorgi (1992) observed, "It is clear to most astute observers of the field that psychology's disciplinary status is ambiguous at best and chaotic at worst…its place among the other sciences are still to be determined in a manner acceptable to the majority of psychologists" (p. 46). One of the main reasons the discipline appears fragmented is due to the increasing number of specialty areas and because of the lack of theoretical unity in the field (Bevan, 1991; Gilgen, 1987; Yanchar, 1997; Yanchar & Slife, 1997). Another difficulty is the apparent disorganization of psychology's experimental literature (Katzko, 2002). For instance, the areas of biopsychology, physiological psychology, cognitive neuroscience, and behavioral neuroscience are separate specialty areas despite their interrelatedness. Each of these specialty areas produces knowledge with their own instruments, methods of analysis, and theories with little or no overlap between them. Psychologists William Bevan and Frank Kessel (1994) lamented, "Fragmentation fosters a mind-set characterized by separateness, but competitiveness that is excessive, and by alienation from intellectual and human concerns" (p. 505). With the incorporation of new subject matter, psychology's disciplinary boundaries become increasingly fragmented. As a result, it becomes more difficult to discern how psychology is a distinct and separate discipline from other disciplines such as sociology, biology, economics, and anthropology.

Accompanying concerns about psychology's fragmentation are concerns about the lack of productive communication between subdisciplines or "discourse communities" (Slife, 2000). Psychology's specialized discourse communities have their own esoteric terminology for various psychological phenomena. These specialized languages make it difficult for subdisciplines to communicate with one another.[14] For instance, without prior training in personality psychology, it is difficult to differentiate between the concepts of "self-concept" and "self-image." Another difficulty with specialized languages occurs when a subdiscipline uses a completely different term to connote the same psychological variable. This excess of concepts can occur even between opposing camps within the same subdiscipline. Murray White's (1985) study of the status of cognitive psychology indicated that there were confusions about the term "icon." White found icon in various guises such as iconic memory, iconic storage, preattentive memory, visual sensory memory, and visual sensory store among others, sometimes within the same book.

There are several other concerns about psychology's fragmentation. One concern is that psychology lacks a common unit of measurement, unlike the measurement units found in disciplines like physics (Tryon, 1996). As the clinical psychologist Warren Tryon stated, "Exact replication is only possible with equivalent units of measurements because an investigator using a larger unit will find fewer of them than will an investigator using a smaller unit when exactly the same quantity has been replicated" (p. 214). The lack of measurement coherence may cause problems with the replication of research results. A

[14] This has been a long-standing problem of the sciences in general, not just in psychology. See Harding (1938) for a discussion on the esoteric and often times needless terminology in the sciences.

potential ramification of this is that psychologists may use different measurement units as proof against replication.

In response to the concerns of psychology's lack of cohesion, there have been numerous proposals for unifying the discipline of psychology under a common theoretical framework (Kimble, 1994; Staats, 1983; Sternberg & Grigorenko, 2001; Yanchar & Slife, 2000). For instance, the psychologist Arthur Staats (1991) explained that psychology's fragmentation is a result of numerous, unrelated studies and that only by making a conscious effort to tie together these unrelated studies will psychology begin to mature as a science similar to that of physics. The sociobiologist Edward O. Wilson (1998) suggested subsuming psychology under biology to make psychology more coherent with the physical and natural sciences. Rather than separate, unrelated disciplines, the discipline could be placed in a hierarchy with sciences like physics and chemistry at the most basic level and social sciences such as psychology placed under biology.

Robert Sternberg (2001; 2001) proposed that psychology's unity may come from the using a multiparadigmatic, multidisciplinary, and integrative approach that facilitates converging operations. Psychologists would accept different theoretical, subdisciplinary, and methodological views that would come to examine certain psychological phenomena. For instance, psychologists studying personality, development, and behavioral genetics could use the methods of factor analysis and longitudinal studies for research on intelligence. Howard Gardner (1992) asserted that, in the future, many of psychology's subdisciplines could merge with other fields such as cognitive science and neuroscience, leaving a “core” of phenomena for the remaining psychologists with which to contend. Essentially, Gardner believes that psychology’s boundaries will dissipate as other scientific communities subsume psychology’s various subdisciplines.

Others disagree that fragmentation in psychology constitutes a problem. Irwin Altman (1987) proposed that the dynamic interplay between unifying psychology and pursuits in specialized subfields promoted healthy growth within the discipline. The more specialized psychology became the more problems psychologists could potentially solve. Wayne Viney (1996) believed that psychology does not need to unify because the discipline is in no worse shape than any other science. Most other sciences display some kind of disunity, fragmentation, or specialization; therefore, psychologists are chasing a mythical idea of unification.

The complexity of psychology’s boundaries and the disputes psychologists have over them indicate that psychology’s boundary-work usually does not affect *all* of psychology. It is rare when something affects the entire field’s boundaries.[15] Instead, these disputes usually occur at a local level between individual psychologists, or between competing laboratories, or even between subdisciplines. In the next three chapters, I discuss instances of psychologists’ local boundary-work when dealing with the introduction of different kinds of

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[15] I should note that one present change taking place in the field of clinical psychology is the turf battle between clinical psychologists and psychiatrists over prescription privileges. If clinical psychologists are granted prescription privileges—and in some states they now have this privilege—this would represent an example of a large scale expansion of boundaries that would affect approximately 44% of psychologists (American Psychological Association, 1993).

material, conceptual, and social practices by evolutionary psychologists, cognitive scientists, and chaos theorists. Psychologists reacted to the introduction of their practices in different ways. Some psychologists recognized the potential value of these scientists' practices and "opened the door" to their knowledge products. Other psychologists vigorously fought against these scientists' practices, believing that they were either irrelevant or potentially damaging to the reputation of the field.

CHAPTER III

ADAPTATIONS OF THE MIND: EVOLUTIONARY PSYCHOLOGY

Scientists, philosophers, social theorists, political scientists, psychologists, and economists throughout the nineteenth and twentieth centuries have possessed a deep fascination with using evolutionary theories as a means of comprehending and explaining human thought, behavior, and emotions. In the preface to his book, *What Evolution Is*, the evolutionary biologist Ernst Mayr (2001) encapsulated this fascination: "The thinking of modern humans, whether we realize it or not, is profoundly affected—one is almost tempted to say determined—by evolutionary thinking" (p. xii). Several individuals throughout the early nineteenth century and continuing to the present exemplified this fascination by employing various forms of evolutionary theory to generate their theories and inspire their research. A number of these thinkers derived their ideas from the French naturalist Jean Baptiste de Lamarck and the English naturalist Charles Darwin.

In the next section, I briefly outline some of the ways scientists, philosophers used Lamarck's, and Darwin's ideas as means of explaining human thought, behavior, and emotions. Then, I describe the science of evolutionary psychology as practiced today. Third, I provide a history of the rise of evolutionary psychology. Fourth, I explain the conceptual, material, and social practices of evolutionary psychologists. Finally, I elucidate psychologists' boundary-work strategies in reaction to these practices.

Lamarck and Spencer on Behavior

Lamarck's theory of inheritance of acquired characteristics, also called Lamarckism, was one of the first evolutionary theories in the nineteenth century to suggest how one generation of organisms transmits various traits to the next generation. According to Lamarck, the frequent use of a particular trait strengthens that trait. Subsequently, the next generation inherits that particular trait. Lamarck believed the opposite was also true. Traits not used will atrophy as a result of this disuse and will not appear in the next generation. A classic example used in many books about evolutionary thought is the giraffe's neck (e.g., Bowler, 1989; Mayr, 2001). According to Lamarck's theory, a side effect of giraffes stretching their necks to obtain succulent leaves at the tops of trees was the slight elongation of their necks. The trait of elongated necks passed from one generation to the next, as giraffes' neck proceeded to get longer in each new generation. To Lamarck, a species' *behavior* and the free will to carry out that behavior were the important components for transmitting these traits to subsequent generations. It was not a giant leap to focus the idea of inherited characteristics to human evolution. The English philosopher Herbert Spencer was largely responsible for this move during the nineteenth century.

Spencer used this teleological perspective as a model for how to improve civilization (Richards, 1987). Spencer believed that, similar to giraffes passing the trait of elongated necks to their young, humans could pass desirable behaviors onto their progeny by engaging in desirable behaviors. He was particularly interested in the idea of humans passing on mental faculties (e.g., intelligence, moral standards) that would ensure justice and fair treatment in a society that would become more and more complex as time passed. Those individuals who exercised these mental faculties would strengthen them; those faculties not exercised would atrophy. As the philosopher and historian Robert J. Richards (1998) suggests of Spencer's belief in this process, "It would create both organism and societies of organisms that displayed greater specialization of parts within an overall integration of functions—more complex organisms and societies, that is, more progressive and perfect organisms and societies" (p. 596). If each generation utilized their mental faculties to their fullest potential, this would improve each successive generation's society. It is important to note that Spencer's idea of society's progress was a "laissez-faire" perspective. The historian of biology Peter Bowler (1989) points out that Spencer believed society's progress "could not be reached more rapidly through human interference—the process of social development was so complex that it was better to let nature take its own slow course" (p. 227).

August Weismann's (1882) germ plasm theory of heredity and experiments with rats effectively disproved Lamarckism as a viable explanatory process for evolving organisms. Weismann severed rats' tails and bred them to see if this acquired characteristic would appear in future generations. There was no such characteristic in the following generations. Subsequently, this led to the dismissal of Spencer's ideas on social progress due to their inherent Lamarckian tone. [16]

Darwin and His Followers in Psychology

Despite these ideas concerning the nature of humans and their societal progress, it really was not until Charles Darwin addressed the issues of human psychology that *psychologists* began using evolution as a viable means of understanding the individual and their relationship to the social world. Darwin spent little time discussing humans in the *Origin of Species* (1859/1964), but in the final chapter Darwin argues for a new interpretation of human psychology stating, "In the distant future I see open fields for far more important researches. Psychology will be based on a new foundation, that of the necessary acquirement of each mental power and capacity by gradation. Light will be thrown on the origin of man and his history" (p. 488). It was not until *The Descent of Man* (1871/1981) and *The Expression of the Emotions in Man and Animals* (1872/1998) that Darwin began to explore fully the implications of evolution by natural selection on human psychology. In *The Descent*, Darwin proposes the idea that humans descended from other animals. In addition, Darwin argues that animals possess mental faculties similar to those found in humans. Dogs, for instance, possess a form of intelligence that is not qualitatively, but quantitatively different from human intelligence. By doing this, Darwin created a bridge

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[16] Lamarckism occasionally reappears in one form or another. For instance, the late paleontologist Stephen Jay Gould (1998) argues that Lamarckism is an appropriate metaphor for cultural change from one generation to the next. Each generation transmits its cultural heritage through things such as books, buildings, and tools. On occasion, ideas or lifestyles amalgamate and these amalgamations are transmitted to subsequent generations.

between humans and other species so that studying "lower" forms of life would provide some insight into "higher" forms of life. In *The Expression*, Darwin extends the application of natural selection to human emotions by studying the facial expressions of humans and other species. He argues that emotional qualities were continuous between species and that human emotions were inherited adaptations from a common ancestor that had ensured our species' survival. The study of human facial expressions, emotions, and their relationship to our evolutionary history continues to this day (e.g., Ekman, 1992; Keltner & Buswell, 1996; Neese, 1990). Paul Ekman (2002), for instance, carries on Darwin's work by examining people's "microexpressions" and how these may indicate whether a person is being truthful in a conversation.

Galton's Hereditarian Theories

During and after Darwin's life several other figures contributed to the role of evolutionary theory in psychology. The English polymath and Darwin's cousin, Francis Galton, used Darwin's theory as inspiration for his ideas on eminence and genius, in addition to his development of eugenics (Gillham, 2001; Sweeney, 2001). Galton conducted research on eminent relatives, twins, and adopted children in an attempt to verify his belief in the inheritance of mental characteristics (e.g., intellect, swiftness of thought). Furthermore, Galton contended that these mental characteristics could be selectively bred within a society. Similar to Spencer, Galton believed that the better the mental characteristics of the individuals, the more society could improve. This led to his advocacy of "positive" eugenics for improving society.[17]

The Comparative Psychologists

Many comparative psychologists in the late nineteenth century attempted to use natural selection, Lamarckism, or a combination of the two, to contend that our species' intelligence could direct the process of human evolution. For instance, the British naturalist and close friend to Darwin, George Romanes, continued to extend Darwin's ideas on the continuity between human and animal reasoning abilities (Watson & Evans, 1991). The British psychologist Conway Lloyd Morgan believed that humans could use their intelligence—something that he believed was a distinctly human quality—to choose adaptations that would benefit our survival (Richards, 1987). Those who chose their behaviors correctly would survive more often than those who chose their behaviors incorrectly.

James, Baldwin, and Hall

Psychologists William James, James Mark Baldwin, and G. Stanley Hall used various evolutionary ideas to support their own views. Darwin's materialist philosophy inspired James because he believed that by adopting it psychologists could move away from metaphysical supposition (Burkhardt & Bowers, 1981). Much of the framework for James' (1890) *Principles of Psychology* and the functionalist perspective James played a part in developing, was inspired by

[17] Positive eugenics is the idea of selecting appropriate mates for reproduction for the purpose of improving society. This contrasts with "negative" eugenics, which was the idea that unfit individuals (e.g., poor, mentally retarded) should be prevented from procreating often through such means as forced sterilization.

Darwin's work. James argued that consciousness, emotions, and instincts were undoubtedly the product of evolution by natural selection and used Darwin's studies as support for this hypothesis.

Baldwin (1901), like Spencer and Morgan before him, believed that the transmission of cultural practices—via learning—from generation to generation was an important factor in the evolution of humans. Baldwin favored the idea of "organic selection," also called the Baldwin effect. Organic selection indicated that the learning process might help protect some of an individual's physical characteristics long enough for them to vary and eventually be selected (Richards, 1987). Even though the *idea* of transmission via cultural practices was similar to the ideas of Morgan and Spencer, Baldwin insisted that organic selection was random rather than a teleological process.

Hall's "genetic" psychology was inspired by Ernst Haeckel's recapitulationist theory—ontogeny recapitulates phylogeny (Cravens & Burnham, 1971; Gould, 1977). Haeckel was one of Darwin's most ardent supporters in Germany and Hall used his recapitulation theory of species development as a means to explain the mental development of children. Hall believed that, much like a child's physical development from early evolutionary forms, a child's mental development progresses through stages that mimic human evolutionary history (Benjamin & Baker, 2004).

The Intelligence Testers

In the spirit of Galton, Robert Yerkes, Lewis Terman, and Henry Goddard used natural selection throughout the early twentieth century to support their beliefs in eugenics practices. Social Darwinism, the use of evolutionary theory to support political and economic practices, was common throughout the early 1900s (Minton, 1987). These psychologists developed intelligence tests that coincided with immigration restrictions, involuntary sterilization, and incarcerations of those deemed "unfit" or too dangerous to improve society (Kamin, 1974; Sweeney, 2001). As I stated in Chapter 2, the military used these intelligence tests during the war to separate candidates based on their scores. The intelligence testing movement also fostered racial discrimination practices within this country.

<u>Ethology</u>

After the "Modern Synthesis" in evolutionary biology, the biologists Konrad Lorenz and Nicholis Tinbergen developed ethology, which is the study of the origins and functions of animal behavior. Lorenz (1952) and Tinbergen (1951) studied the evolutionary function of fixed action patterns and imprinting of such species as ducks, geese, and stickleback fish. In 1973, Lorenz and Tinbergen, along with Karl von Frisch shared the Nobel Prize in Physiology for their research on animal behavior. The study of ethology fell into disuse as it became apparent that few ethologists could make their research relevant for understanding humans (Plotkin, 2004). It was not until sociobiology came onto the scene that the Modern Synthesis shed light on humans. I will return to sociobiology later in the chapter.

Recent Uses of Darwin's Theory

There are many other examples of the uses of evolutionary theory for understanding some of what psychologists study. For instance, the philosopher Susan Blackmore (1999; 2000) recently proposed that the transmission of information from one generation to the next can be modeled after Darwin's theory of selection. She calls these pieces of information "memes," which represent ideas, behaviors, instructions, or any other information that is passed on to others. The psychologist Donald Campbell (1975) proposed a similar idea with his "evolutionary epistemology" for understanding our moral traditions from a selectionist perspective. Campbell believed that our current moral structure derived from a process of random variation in moral activity and our subsequent selection of appropriate moral conduct among these varieties.

The Decline of Evolutionary Thought in Psychology

In a certain respect then, the idea of an "evolutionary" psychology was not shocking to psychologists when the interdisciplinary evolutionary psychologists began to publish their ideas in the middle 1980s. There are some who consider Darwin the first "evolutionary psychologist," while others, like the behavioral geneticist Henry Plotkin (2004) believe James Mark Baldwin the first. These uses of evolutionary theory to explain humans and their interactions in the world represent relatively independent and isolated attempts at bringing evolutionary theory to bear on psychological phenomena. There was no attempt to organize these ideas into a common theoretical framework and language. Similarly, there were no professional organizations or content-specific journals for evolutionary interpretations of psychological functioning. Evolutionary psychology had not yet become “professionalized” or “institutionalized” within the discipline. Plotkin typifies this problem by his remarks on Baldwin: “He had no disciples, no followers or converts who spread the word and formed a school of thought. Eighty years were to pass before that was to happen, and then it owed nothing at all to Baldwin’s work” (p. 123).

Another factor involved in the general decline of evolutionary perspectives in psychology was the rise in popularity of cultural explanations of behavior, originating in the work of the cultural anthropologist Franz Boas in the early 1900s. Boas’s work continued through students such as Ruth Benedict and later by Margaret Mead. Studies by these individuals brought into question the dominance of evolutionary explanations of human capacities. Instead, Boas and others argued that one’s culture plays a significant role in development. Accompanying this cultural relativism, the 1950s through the 1970s were a time of civil unrest, protests, and the questioning of authority, typified by the Civil Rights movement, gay liberation, and feminist movements (Zinn, 1999). All of these factors brought into question the validity of genetic determinism.

It was the middle 1970s to the middle 1980s that brought about a revival in evolutionary thinking in psychology, first through sociobiology and subsequently through evolutionary psychology. But this revival brought with it controversies about race, equality, and freedom that had been bubbling under the surface for the past thirty years on two fronts: scientific and political. Encapsulating this tension, Pascal Boyer and Jutta Heckhausen (2000), suggest that evolutionary psychology is "arguably one of the most important new developments in the behavioral sciences over the past 20 years. It is also most controversial" (p. 917).

Outline of Evolutionary Psychology

Evolutionary psychology is an interdisciplinary program fostered on understanding the mind from an *adaptationist* perspective. The adaptationist perspective attempts to explain the functional role of human information and decision making processes (Daly & Wilson, 1995). The evolutionary psychologist David Buss and his colleagues (1998) contend that adaptations are "inherited and reliably developing characteristics that came into existence as a feature of a species through natural selection because it helped to directly or indirectly facilitate reproduction during the period of its evolution" (p. 535). Examples of adaptations are ubiquitous in nature. For instance, the hollow bone structure of bird wings is an adaptation for thermoregulation. The particular configuration of colors on moth's wings is an adaptation that provides protective camouflage from potential predators. The human eye's response to light and dark—the opening and closing of the pupil and the firing of rods and cones—are adaptations for perceiving visual stimuli in varying conditions of light.

The adaptationist perspective forms the boundaries of the research evolutionary psychologists conduct. This perspective was largely influenced by David Marr's (1982) functional approach to the visual system. Basically, Marr asked the question: What problem must be solved for each structure and process in the visual system? From there he reasoned that the human eyes' adaptations solved particular spatial and perceptual problems for humans interacting in their environment. Evolutionary psychologists believe that all human adaptations occurred in what they call the *environment of evolutionary adaptedness* (EEA). They identify the EEA as the Pleistocene era in which humans flourished for two million years as hunter-gatherers (e.g., Barkow, 1992; Tooby & Cosmides, 1990). Most contend that, due to the great lengths of time needed for adaptations to occur, there have been no further human adaptations since this time. Leda Cosmides and her colleagues (1992) argue that "our ancestors spent the last two million years as Pleistocene hunter-gatherers...and...the few thousand years since the scattered appearance of agriculture is only a small stretch in evolutionary terms...For that reason, it is unlikely that new complex designs could evolve in so few generations" (p. 5). They reason that the problems humans had to have solved during this time were those concerned with basic needs such as food, shelter, mating, and bartering for goods and services.

<u>Definitions of Evolutionary Psychology</u>

Various evolutionary psychologists have attempted to define their field's program of research and there is considerable agreement about the purpose of evolutionary psychology and the importance of the adaptationist perspective between these definitions. Before going into these definitions, there are a few issues with terminology that need addressing. First, evolutionary psychologists frequently use the term "mind" to mean anything that the brain does, this is, the functions of the brain. Secondly, they often use the term "psychological mechanism," which they also refer to as "information-processing mechanism" and "Darwinian algorithms." Psychological mechanisms are units in the brain that process specific pieces of information coming from the environment that helped humans solve a constrained range of problems that we faced in our ancestral environment.[18] Most evolutionary psychologists believe humans possess a large number of these mechanisms that today assist us, and sometimes impede us, in solving specific problems in our modern world. For instance, Cosmides (1989) proposed that humans possess a "cheater detection" mechanism that allows us to analyze social exchanges of goods and services and to detect those who may receive benefits from this exchange without having to pay a cost (i.e., someone who is potentially cheating in the social exchange). The idea of psychological mechanisms is inspired, in part, by "modular" theories of mind espoused by individuals such as the philosopher Jerry Fodor (1983) and the linguist Noam Chomsky (1984).[19]

In terms of the general agreement of evolutionary psychologists at defining their field, I present three definitions that I believe are typical of the ones found in the evolutionary psychology literature. Louise Barrett, Robin Dunbar, and John Lycett (2002) define evolutionary psychology as a science that "identifies the selection pressures that have shaped the human psyche over the course of evolutionary time, and then tests whether our psychological mechanisms actually show the features one would expect if they were designed

[18] Evolutionary psychologists are not specific about where these psychological mechanisms exist in the brain. Psychological mechanisms are, to a certain extent, spatially ambiguous.

[19] Neither Fodor nor Chomsky seem particularly open to the ideas of evolutionary psychologists. Fodor (2000; 2000) has brought into question some of the findings of evolutionary psychologists who support modularity of mind and Chomsky seems reluctant to attribute evolutionary processes to his universal grammar module (Pinker, 1994).

to solve these particular adaptive problems" (p. 10). Leda Cosmides, John Tooby, and Jerome Barkow (1992) define evolutionary psychology as:

> Psychology that is informed by the additional knowledge that evolutionary biology has to offer, in the expectation that understanding the process that designed the human mind will advance the discovery of its architecture...and by focusing on the evolved information-processing mechanisms that comprise the human mind, supplies the necessary connection between evolutionary biology and the complex, irreducible social and cultural phenomena studied by anthropologists, sociologists, economists, and historians. (p. 3)

Catrin Rode and Xiao Wang (2000) describe evolutionary psychology as a "new approach to psychology [that] uses knowledge and principles from modern Darwinian theories in research of the human mind. In this view, the mind consists of a rich array of information-processing mechanisms that were designed by natural selection to solve adaptive problems that were recurrent in hominid evolution" (p. 926).

In summary then, evolutionary psychology is an interdisciplinary field that searches for a mesh between current function and past adaptations to common problems found in our ancestral history. By positing specific adaptive problems faced by our ancestors, evolutionary psychologists can test for domain-specific functions in our information processing capacities.

In line with these definitions, David Buss (1999) outlines four essential questions that evolutionary psychologists attempt to ask as they carry out their research. The first question they attempt to answer pertains to identifying the causal processes that designed the human mind. What were the types of environments humans had to face that fundamentally shaped our brains? The second question on which they focus is how the human mind was designed the way that it was. How is it that the human mind possesses certain faculties and abilities but not others? Another question pertains to the function of the mind. What are the major functions of the parts of the brain and how do they work together? Finally, they ask questions of a perceptual nature. How does the mind interpret stimuli from the environment and use this information to produce behavior?

These definitions and the questions evolutionary psychologists attempt to answer demonstrate the departure from past research on the relationship between evolutionary theory and psychology. The main difference between the newer and older versions is that the newer versions of evolutionary psychology organize around specific objectives. First, the new definitions point to modern Darwinian theories, implying that evolutionary psychologists consider findings from modern genetics and evolutionary biology in their own research. Secondly, there are explicit statements regarding testing hypotheses about our evolutionary history using data from the present. For instance, evolutionary psychologists may posit hypotheses about dominance hierarchies in human mating systems that occurred in our ancestral environment and use data from modern human subjects to test these hypotheses. Finally, these definitions make the human *mind* the focal point for evolutionary psychologists' investigations.

History of Evolutionary Psychology

"Evolutionary psychology" is a somewhat misleading title because the term "psychology" gives one the impression that it is merely a branch or subdiscipline of psychology. Evolutionary psychology is more than a subdiscipline. Those scientists considered evolutionary psychologists come from an array of disciplines including evolutionary biology, cultural anthropology, psychology, sociology, sociobiology, and ethology. Because of evolutionary psychology's recent origins, the usual archival sources for a comprehensive history are ill-formed or absent. Nevertheless, I attempt to put the available pieces together to form a tentative sketch of how evolutionary psychology came to be in order to make my arguments about psychologists' boundary-work in response to this program.

Evolutionary psychology developed as an interdisciplinary program in the late 1980s when many scientists broke away from the discipline of sociobiology. According to the sociologist Ullica Segerstråle (2000), the break away from sociobiology arose mainly for political reasons. The political controversy began with the publication of Harvard University entomologist Edward O. Wilson's (1975) *Sociobiology*. Wilson defined sociobiology as:

> ...the systematic study of the biological basis of all social behavior. For the present, it focuses on animal societies, their population structure, castes, and communication, together with all of the physiology underlying the social adaptations. But the discipline is also concerned with the social

> behavior of early man and the adaptive features of organization in the more primitive contemporary human societies. (p. 4)

In the book, Wilson argues that genes are essential for the production of social behaviors. The main controversy centers on the final chapter where Wilson discusses the implications of genes in determining *human* social behavior. Wilson's "genocentric" perspective of human social behavior in *Sociobiology* caused considerable controversy. By Wilson's (1994) own account, numerous factions found its fundamental framework to be too deterministic. The controversy stemmed from the belief that sociobiologists were ostensibly justifying acts such as rape, spousal abuse, and numerous other forms of aggression through their belief in genetic determinism. A group of students and professors calling themselves the Sociobiology Study Group (SSG) formed just before the publication of Wilson's book to respond to his claims (Segerstråle, 2000). Members included Stephen Jay Gould, Ruth Benedict, and Richard Lewontin. For some of these critics, sociobiology heralded a new form of social Darwinism. For instance, in a *New York Times Review of Books* in 1975 the SSG (1978) issued this statement:

> These theories provided an important basis for the enactment of sterilization laws and restrictive immigration laws by the United States between 1910 and 1930 and also for the eugenics policies which led to the establishment of gas chambers in Nazi Germany. The latest attempts to reinvigorate these tired theories comes with the alleged creation of a new discipline, sociobiology. (p. 20)

Perhaps the most well-known, and public, response to sociobiology came on February 15th, 1978 in Washington when Wilson was to deliver a talk on sociobiology for the American Association for the Advancement of Science (AAAS). Protesters from the International Committee Against Racism showed up to protest and one of them ran to the front and poured water on Wilson's head exclaiming that he was "all wet" (Wilson, 1994).[20]

Scientists interested in the implications of evolutionary thought on human mind and behavior moved away from the politically charged sociobiologists and created the title "evolutionary psychology" in the early to middle 1980s. Changing names seemed to divert some, but not all, of the controversy. Philosopher Val Dusek (2000), a major critic of sociobiology, observed that evolutionary psychology "arose two decades later as a more muted and explicitly non-racist replacement for sociobiology" (p. 556). In the span between the publication of Wilson's book and the beginnings of evolutionary psychology, the political tensions seemed to abate, and, in fact, there appeared to be a general shift in consciousness towards the acceptance of biologically determined behavior. Dusek (1999) outlined five factors that may have contributed to this general shift towards biological thinking. First, people were attracted to the potential medical and economic benefits genetic research had to offer. Second, Derek Freeman's (1983) critique of Margaret Mead's analysis of Samoan culture brought cultural relativism into question. Another factor was the Minnesota twin studies, which

[20] Val Dusek (1999) was present at the 1978 AAAS meeting and retold the story from a different perspective. He noted that most of the protesters were African-Americans and after the black woman poured water over Wilson's head, he exclaimed that it felt as if an aborigine had speared him. Most accounts of the event have glaringly omitted Wilson's statement.

reincarnated the idea that genetics had a much ignored role in people's intelligence and personality. Fourth, brain imaging technology began to demonstrate sex differences in the brain. Lastly, the ongoing "discovery" of genes for particular characteristics such as bipolar disorder and homosexuality was an attractive explanation to many. These factors worked in favor of evolutionary psychologists. Society was ready to accept a more deterministic and biological view of humans during the 1980s than it was in the 1970s. This shift in attitude played a role in the acceptance of evolutionary psychologists' practices.

Conceptual Practices

Evolutionary psychologists brought many "new" concepts and theories to bear on existing psychological phenomena such as the environment of evolutionary adaptedness, long and short term mating strategies, cheater-detection algorithms, parental investment theory, the paternity uncertainty hypothesis, and reciprocal altruism.[21] To do this, they co-opted ideas from influential evolutionary biologists such as William Hamilton, George Williams, and Robert Trivers to foster their credibility. Hamilton's (1964) kin selection or inclusive fitness theory, Williams' (1966) requirements for adaptations, and Trivers' (1971) reciprocal altruism theory all were brought into psychology as tools for evolutionary psychologists' investigations. Leda Cosmides, a psychologist, and John Tooby, an anthropologist, both from the University of California at Santa Barbara, were two of evolutionary psychology's most ardent

[21] While many of these concepts and theories were new to psychologists, they were, to a certain extent, old to sciences like evolutionary biology.

spokespeople. They, along with anthropologist Jerome Barkow, called for a "conceptual integration" in psychology whereby the "behavioral and social sciences should make themselves mutually consistent and consistent with what is known in the natural sciences" (1992, p. 4).[22] In doing this they critiqued, what they called, the "standard social science model" in psychology, believing it "suffers from a series of major defects that make it a profoundly misleading framework" (Tooby & Cosmides, 1992, p. 2). At the heart of the standard social science model, at least as evolutionary psychologists conceive it, is the belief held in the social sciences that the human mind is initially a *tabula rasa* that, based on experiences within the environment, forms our cognitive abilities and behavioral repertoire. According to Cosmides and other evolutionary psychologists, they were fighting to supplant the naïve model of human psychological functioning that had been popular for at least a century.

Evolutionary psychologists follow closely the standard methods of statistics and data analysis traditionally used by psychologists. Evolutionary psychologists commonly employed analytic techniques such as analysis of variance, bivariate and multiple regression, and t-tests. These and other methods provided evolutionary psychologists with the analytic tools they needed to conduct their experimental and correlational studies. Similarly, their research methods included questionnaires, self-reports, experimental manipulations, and naturalistic observations. While some advocates of evolutionary psychology used

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[22] E. O. Wilson (1998) in his book *Consilience* also advocated this kind of conceptual integration.

archeological data, field studies, and fossil records, the *psychologists* of evolutionary psychology did not employ these methods.

Material Practices

One relatively innovative material practice of some evolutionary psychologists was the way they conducted experimental procedures on the computer. For instance, in a study about physical attractiveness, Victor Johnston and Melissa Franklin (1993) developed a computer program that could morph facial features in pictures so their participants could "evolve" the faces on the computer screen.[23] Similar programs were used to measure body type preferences by manipulating a body's waist-to-hip ratio (Singh, 1993). The utilization of these types of programs obviates the need of random samples of humans to generate the various faces and body types. Instead, computer software creates an almost infinite number of samples with various physical characteristics. Nevertheless, this procedure does not eliminate the need for participants in psychological studies.

Because evolutionary psychologists follow closely the methodology of psychologists, they did not introduce many other material practices into the discipline. For instance, Cosmides (1989; 1992) used the Wason selection task—a task traditionally used for reasoning research—to test her theories about social exchange and the cheater detection mechanism. Jeffery Simpson and Stephen Gangstead (1990; 1991) developed the Sociosexual Orientation Inventory, which

[23] I should note, however, that Francis Galton used composite photography, a similar albeit less modern procedure, in order to identify features such as criminal types and people suffering from various diseases.

measures the willingness of individuals to engage in sexual behavior in the absence of exclusive relationships. But this inventory was used for a relatively small number of studies, mostly by the two creators, and was not frequently used elsewhere due to its poor reliability. Similarly, other evolutionary psychologists created various questionnaires for their research but nothing that would constitute a significant material practice that was new to the discipline.

Social Practices

The social practices of evolutionary psychologists are similar in form to the social practices of my other two case studies. The social practices of evolutionary psychologists take the form of professionalizing their interdisciplinary program and the rhetorical strategies they employed to persuade psychologists and the public of the viability of evolutionary psychology. In terms of professionalization, evolutionary psychologists established journals, created professional societies, held conferences, and developed funding resources. In terms of their rhetorical strategies, many evolutionary psychologists touted their field as a new paradigm for psychology and continued their efforts to disengage from the shadow of sociobiology.

Evolutionary psychologists founded The Human Behavior and Evolution Society (HBES) on October 29th, 1988 at the University of Michigan. Participants at a national conference on Evolution, Psychology, and Psychiatry voted to create the organization, decided on a name for the society, elected the first officers, and created the society's constitution. The University of Michigan Evolution and Human Behavior Program sponsored the conference. In 1989, HBES held its first independent conference in Evanston, Illinois at Northwestern University. The evolutionary biologist William D. Hamilton was the society's first president. Interestingly, the keynote speaker for the first conference was E. O. Wilson who, as Segerstråle (2000) reported, took them to task for not calling HBES the Society for Human Sociobiology. According to Constance Holden (1996), who covered the story in *Science*, Wilson "chastised the group for...displaying a 'failure of nerve'—caving in to critics who still label sociobiology as 'racist and determinist'" (p. 15). Holden reported that HBES members thought the new name of the society was more appropriate for their goals. Nicholas Blurton-Jones, an anthropologist attending the conference stated, "Sociobiology raised too many hackles and got us into too much trouble" (p. 15).[24] Establishing HBES helped this interdisciplinary program create distance from sociobiology. Even though "human behavior" and "evolution" in the HBES name encapsulated what sociobiology was all about, these seemed to be less threatening terms.

In 1994, HBES took over the reigns of publishing responsibilities of the journal *Ethology and Sociobiology*, but by 1996 had changed the name of the journal to *Evolution and Human Behavior*. Dusek (2000) describes this change of names as strategic. Academics tended to associate "ethology" in the title of the journal with Konrad Lorenz and his eugenics beliefs and Nazi sympathies. Wilson's reputed racism made the term "sociobiology" problematic in the title. Not only did the new title foster continuity between the society and the journal, but it

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[24] After the publication of Holden's brief announcement of the journal change, twelve scientists aligned with sociobiology, including notables like Sarah Blaffer Hrdy and Marc Hauser, penned their names to a brief report (1996) about the contributions sociobiologists made to such fields as women's studies, art, philosophy, botany, and psychology.

also assisted in further distancing evolutionary psychology from sociobiology. Calling the society HBES and changing the name of their journal seemed to help the society's membership as academics joined the Society from a diverse array of scientific and humanities professions including biology, medicine, economics, literature, law, and art.

HBES provided opportunities for psychologists to interact with scientists from numerous other disciplines outside its own boundaries. Through the society's annual conferences, other scientists exposed psychologists to the recent findings in evolution research. These conferences served not only as a physical space to hear about new research, but also to extend the communication networks of psychologists for potential research collaborations and funding opportunities. Additionally, *Evolution and Human Behavior* provided a venue for psychologists to publish research specifically pertaining to adaptations in human mind and behavior.

Evolutionary psychologists also engaged in rhetorical strategies beyond the ones just discussed. The rhetoric of evolutionary psychologists was a powerful and persuasive component in their arsenal. With their discourse came the backing of "harder" sciences like biology and a powerful scientific icon: Charles Darwin. Evolutionary psychologists and some popular science writers were claiming that evolutionary psychology was a new way of thinking about psychological phenomena that would usurp the standard social science model discussed earlier. There were many claims that evolutionary psychology would become *the* paradigm in psychology. In *The Moral Animal*, one of the popularized accounts of evolutionary psychology, the science writer Robert Wright (1994) dramatically expressed this idea:

> The new Darwinian social scientists [evolutionary psychologists] are fighting a doctrine that has dominated their fields for much of this century: the idea that biology doesn't matter much—that the uniquely malleable human mind, together with the unique force of culture, has severed our behavior from its evolutionary roots; that there is no inherent human nature driving human events, but that, rather, our essential nature is to be driven. (p. 5)

Wright continued his discourse by calling evolutionary psychology "revolutionary" and set the stage for his readers to believe that evolutionary psychologists have struggled, paid their dues, and are now ready to create a paradigm shift in the sciences: "In many ways, what is now happening fits Thomas Kuhn's description of a 'paradigm shift'...A group of mainly young scholars have challenged the settled worldview of their elders, met with bitter resistance, persevered, and begun to flourish" (p. 6).

Psychologists, not just science writers, adopted this strategy in their work. For instance, Jeffery Simpson (1995) proclaimed evolutionary psychology as a "new paradigm whose time has come." David Buss (1995) called evolutionary psychology a "new paradigm" and a "metatheory" in addition to proposing that the field could "dissolve" or "cross" disciplinary boundaries in psychology as long as psychologists embraced the adaptationist perspective. Peggy Cerra and Robert Kurzban (1995) contended that evolutionary psychology would provide bridges between disciplines such as anthropology, economics, literature, and neuroscience. By making these claims, advocates of evolutionary psychology attempted to place their field on the same footing as other putative paradigm shifts such as Copernicus' heliocentric theory and Einstein's theory of general relativity.

Evolutionary psychologists brought their varied practices into the discipline of psychology. How did traditional psychologists react to these practices? What types of boundary-work did psychologists use in response to these practices? Were evolutionary psychologists and their ideas readily accepted, or did they meet resistance from psychologists? The next two sections deal with psychologists' resistances and accommodations to evolutionary psychologists' practices.

Resistances to Evolutionary Psychology

Certain psychologists as well as various philosophers, feminist scholars, biologists, and other scientists questioned the validity of the practices of evolutionary psychologists during the 1980s and 1990s. In fact, psychologists criticizing evolutionary psychology often relied on arguments set forth by philosophers and other scientists as ammunition for their boundary-work. But evolutionary psychology, due to its relative newness to psychology and its interdisciplinary nature, posed problems for those trying to prevent evolutionary psychologists from using their theories to explain psychological phenomena.

The philosopher Paul Davies (1996b) articulated the difficulty in resisting the tenets of evolutionary psychology when he proposed that "criticizing evolutionary psychology is no simple matter. The difficulty is not that evolutionary psychology enjoys especial power or plausibility, but rather that it is not yet a well-articulated form of inquiry" (p. 559). For instance, there are numerous debates within the evolutionary sciences pertaining to the appropriate mechanism(s) of evolution (e.g., Gould, 2002; Mayr, 2001; Williams, 1966). Does evolution occur strictly by Darwinian natural selection? Or are processes like mutations and genetic drift or a combination of all three responsible? Furthermore, there are questions about the appropriate *level* at which evolution occurs. Does evolution occur at the level of genes, which many evolutionary psychologists argue is the case? Or does evolution occur either at the level of individual organisms or at the species level? Accompanying the ambiguities of evolutionary theory, as Davies suggested, evolutionary psychology is still a new field and as a new field, there are still too few studies demonstrating the reliability and validity of its theories.

Dusek (2000) suggested another criticism along these lines. He argues that evolutionary psychologists are not experts in molecular biology—especially the psychologists—but despite this, they still use the "harder" sciences as justification for their ideas. It is difficult to acquire a sense of trust and expertise in evolutionary psychologists when their lack of training is apparent. Often, when others attack their ideas, evolutionary psychologists will resort to calling the critic(s) cultural relativists. For instance, in David Buss' (2003) account of his early research into sexual strategies, he described the dominant theories of psychology as being comprised of "social learning, socialization, and arbitrary social roles—all precursors of *social constructionism* [italics added]" (p. 219). In *How the Mind Works*, Steven Pinker (1997) frequently attacked the humanities and their poststructuralist, deconstructionist, and postmodernist perspectives as being extreme views.

One of the main resistances that came from psychologists was that evolutionary psychologists' explanations of psychological mechanism were nothing more than "just so stories" (Gannon, 2002; Rose & Rose, 2000; Schlinger, 1996). The phrase derives from the Rudyard Kipling book of the same name that contained outrageous stories to explain the functionality of things such as how leopards got their spots. In an evolutionary context, "just so stories" represent post hoc rationales for supposed adaptations. Psychologists used arguments developed by Steven Jay Gould and Richard Lewontin (1979) in support of their contention. The thrust of Gould and Lewontin's position is that it is difficult to disentangle true adaptations from mere byproducts of the evolutionary process. Some behavior or structure may appear at first to be an adaptation, but in fact, may be nothing more than a chance mutation. For instance, birds' hollow bone structure at first appears to be an adaptation for flight, however biologists have demonstrated that these hollow bones were employed first for thermoregulation and the ability to fly is a mere byproduct of that evolutionary process.

From this standpoint, evolutionary psychologists' belief in humans possessing multiple psychological mechanisms that arose as adaptations from our time in the Pleistocene age proves problematic. As the psychologist Linda

Gannon (2002) expressed in her critique of evolutionary psychology: "Labeling certain traits and behaviors adaptations and others by-products is arbitrary since a methodology has not been developed with which to determine if a trait or behavior is a direct adaptation of a by-product of other evolved traits" (p. 150). As there is no definitive method for identifying adaptations, especially about psychological phenomena, it is difficult to believe evolutionary psychologists who propose adaptations for psychological phenomena such as social exchange (Cosmides & Tooby, 1992), mate selection (Buss, 1994), language (Pinker & Bloom, 1992), dominance hierarchies (Cummins, 1999), and rape (Thornhill & Thornhill, 1990). Unlike bones, psychological phenomena leave no fossil records, making it difficult to infer present functions from past events.

Accompanying the critique of adaptations, psychologists resisted evolutionary psychologists' adaptationist perspective on another front. Social psychologist Shawn Meghan Burn (1996) pointed out that the adaptationist perspective fails to consider processes other than evolution (e.g., cultural norms) as viable means of assessing experimental outcomes. Burn analyzed evolutionary social psychologist Douglas Kenrick and colleagues' (1990) research on earning capacity. Kenrick and his colleagues found that women rated men's earning capacity as higher than men did in searching for potential mates. They concluded that their findings provided evidence for an adapted difference between men and women. Men seek women who are young and attractive because this is an indication of a woman's fertility. Women seek men with large quantities of material resources (i.e., money, power) because this ensures support for both the woman and their children. Burn criticized their findings because they failed to consider cultural factors. Burn suggested these findings "could be due to the fact that both women and men know that women's earning potentials are lower and so they both look to men to be the dominant wage earners. It could be due to social norms that communicate to men and women that a man's value is determined in part by his earning power" (p. 27).

Psychologists Alice Eagly and Wendy Wood (1999) proffered a similar criticism about evolutionary psychologists' research results. One of the "landmark" studies in evolutionary psychology during the late 1980s was David Buss' (1989b) study of differences in mate preferences. Buss studied mate preferences in 37 cultures and questioned more than 10,000 individuals. Similar to Kenrick's results, his research indicated that men on average would look for mates with good reproductive potential and women would look for mates with good resource potential. He believed that these differences were a product of the adapted problems males and females had to solve during the Pleistocene era. This is exactly the kind of study evolutionary psychologists' found attractive. It contained a large sample size—for psychological research—and it crossed cultural boundaries. This appeared to be strong evidence for a universal human nature, at least for mate selection preferences. Eagly and Wood took Buss' data and tested hypotheses from the perspective that gender differences in mate selection would be made on the basis of female and male social roles within a political and economic structure. Buss' data confirmed their hypotheses. Eagly and Wood suggested that the social role theory could be empirically tested because the roles males and females adopt today can be examined, unlike Buss' suppositions about our ancestral environment, which cannot be examined. The developmental psychologist Annette Karmiloff-Smith (2000) also questioned the validity of evolutionary psychologists' findings on psychological mechanisms, calling it the "Swiss Army knife" approach. Karmiloff-Smith suggested that there simply is not enough evidence to state definitively that mechanisms for higher cognitive processes exist. Instead, she argued that what evolution did do for our brains was allow it to modify continually its structure in response to environmental demands, thus making it appear that humans possess psychological mechanisms.

Another way that psychologists resisted the introduction of evolutionary psychologists' practices into the discipline pertains to the political ramifications of evolutionary psychologists' research on gender differences. Eagly (1995), for instance, claimed that gender differences research conducted by evolutionary psychologists may be used "in far less beneficial ways by misogynist forces in society" (p. 155). Gender differences research could possibly lead to different standards of education for males and females. Another possible ramification of believing too much in evolutionary psychologists' research is that their findings seemed to justify the "glass ceiling"—the invisible barrier in businesses and organizations that prevent women from attaining high status and high paying jobs.

Resistances to evolutionary psychology came mostly from feminist psychologists who believed that culture plays a more important role than evolution in explaining psychological phenomena. To a certain extent, evolutionary psychologists met minimal resistance when introducing their various practices into the discipline. Psychologists directed their resistances towards evolutionary psychologists' research conclusions. But questioning research conclusions is a common practice in all fields of psychology. In fact, most resistance to evolutionary psychology came from outside the discipline of psychology as is seen in critiques by philosophers (e.g., Davies, 1996a; Jerry Fodor, 2000), various scientists in the biological sciences (Fausto-Sterling, 1997; Gould, 1991), and science journalists (e.g., Tierney, 2002). One possible reason for this minimal resistance may be that evolutionary psychologists' own boundary-work was effective enough to distance their field from the controversy caused by sociobiology. Evolutionary psychologists did not have to contend with the excess baggage of sociobiology as they postured for intellectual space in psychology. Another factor may be that evolutionary psychology's interdisciplinary nature helped legitimize the discipline to psychologists. The psychologists of evolutionary psychology were proposing theories supposedly backed by disciplines in which most psychologists were unfamiliar or in which they lacked expertise. Psychologists may not have felt qualified to provide sufficient resistance to the program. But this lack of resistance indicates something else that is important in the context of boundary-work. It appears that, largely, psychologists did not view evolutionary psychologists as pseudo-scientists like parapsychologists or phrenologists, but rather as legitimate scientists whose theories were debatable and contested within the boundaries of the discipline. The next questions I try to answer are how and to what extent did psychologists accommodate evolutionary psychology?

Accommodations to Evolutionary Psychology

Evolutionary psychology seemed quite popular within society in the 1990s. There were numerous articles in newspapers and magazines (e.g., Ferguson, 2001, March 19; Pinker, 1997, October; Wright, 1995, August) and a number of books written for scientists and the public (e.g., Buss, 1994; Pinker, 1994, 1997; Ridley, 1996; Wright, 1994), many of them bestsellers. It seems that, in general, the public accepted the claims of evolutionary psychologists. So how did psychologists accommodate evolutionary psychology? Despite the resistances I just described, psychologists made accommodations in numerous subdisciplines in psychology. One of the ways to examine these accommodations is to look at the research literature. Did journal editors and review committees allow evolutionary psychologists to publish in their journals? What kinds of journals published evolutionary accounts of human behavior? Were these accounts published in top tier journals with wide readerships or were they relegated to fringe journals with small readerships and their own content-specific journal, *Evolution and Human Behavior*?

Evolutionary psychologists' published in a wide array of top tiered journals in the late 1980s and throughout the 1990s. Journal like *American Psychologist*, *Journal for Personality and Social Psychology*, *Cognition*, *Behavioral and Brain Science*, and *Human Development* are but a few of the top tiered journals to publish evolutionary psychologists' research. The topics of this research covered almost every subdiscipline in psychology including cognition (Cosmides & Tooby, 1996; Gigerenzer & Hug, 1992), biological psychology (Deacon, 1997), human development (Bjorklund & Pellegrini, 2000; Geary & Bjorklund, 2000), personality (Buss, 1989a; Shackelford & Larsen, 1997), social psychology (Kenrick, Groth, Trost, & Sadalla, 1993), clinical and abnormal psychology (Cosmides & Tooby, 1999), and the psychology of women (Campbell, 1999). In a similar vein, the *Annual Review of Psychology*, which covers timely issues in more depth than traditional journals, featured evolutionary psychology (Buss, 1991),.

Another indicator of psychologists' accommodations for evolutionary psychologists is through pedagogy. Were there instances of psychologists incorporating evolutionary psychology into psychology departments, laboratories, and the instruction of students? It seems that this was the case. There were a number of places to study evolutionary psychology around the country. The University of California, Santa Barbara developed a Center for Evolutionary Psychology that trained graduate students.[25] Florida Atlantic University's psychology department created a graduate program specializing in evolutionary psychology. Psychologist Robert Haskell co-founded the New England Institute of Cognitive Science and Evolutionary Psychology in Maine. While David Buss was at Harvard University and the University of Michigan, he trained graduate students in evolutionary psychology.[26] The psychology department of the State

[25] I should note that due to the interdisciplinary nature of evolutionary psychology, these training centers were sometimes independent of psychology departments.

[26] David Buss now has an evolutionary psychology laboratory at the University of Texas at Austin, as does the evolutionary social psychologist Norman Li.

University of New York, Albany formed the Evolution and Human Behavior Laboratory. Additionally, there were an increasing number of textbooks written for undergraduate and graduate students pertaining to evolutionary psychology (e.g., Badcock, 2000; Buss, 1999; Evans, 2000; Mynatt & Doherty, 1999). Accompanying this increase in textbooks, psychologists began teaching an increasing number of classes pertaining to evolutionary psychology. Finally, some of the top selling introductory textbook authors started covering evolutionary psychology (e.g., Gleitman, Fridlund, & Reisberg, 1999; Myers, 2001; Tavris & Wade, 2001) and some introductory textbooks have evolutionary themes running throughout the textbook (e.g., Gray, 1999). In his advocacy of incorporating evolutionary perspectives into teaching psychology classes Peter Gray (1996) contended, "We should devote far more attention than we do to evolutionary theory in our psychology courses. Evolutionary theory is the only truly integrative theory that psychology can ever have...Evolutionary theory should be a central component of the introductory course and an integral part of almost every other course" (p. 207). It appears that many psychologists responded in the affirmative to Gray's urgings.

Perhaps the best indicator of psychologists' accommodations to evolutionary psychologists' practices is contained in the book *Sex, Power, Conflict* edited by David Buss and psychologist Neil Malamuth (1996). In the book, Buss and Malamuth solicited evolutionary psychologists and feminist psychologists to discuss gender differences research. The book contains three sections divided in the Hegelian tradition of thesis, antithesis, and synthesis. The first section contains evolutionary perspectives on gender differences. Feminist perspectives on gender differences occupy the second section. In the final section, authors Felicia Pratto, Barbara Smuts, Malamuth, and Buss respectively attempt to combine evolutionary and feminist perspectives to reach some kind of common ground on the controversial issue of gender differences. Since feminist psychologists were particularly critical of evolutionary psychology, this book served as a bridge between the two opposing camps.

In summary, psychologists engaged in a great deal of boundary-work as evolutionary psychologists began appearing in the discipline in the 1980s. Psychologists questioned the validity of evolutionary approaches to mind and behavior by attacking their theoretical models and criticizing their interpretations of data. In addition, they enlisted the aid of philosophers and other scientists to support their criticisms of evolutionary psychologists. One of the things they did not resist was the formation of HBES and the development of the *Evolution and Human Behavior* journal. In terms of accommodations, evolutionary psychology seemed to gain in popularity and support from the 1980s to the new millennium. Journal editors published their research, book companies' marketed evolutionary psychology textbooks for undergraduate and graduate students, introductory textbooks began covering evolutionary psychology as a viable program, psychology professors began offering classes in the field, and some feminists and evolutionary psychologists reached a delicate compromise. But psychologists did not totally accommodate evolutionary psychologists. For instance, most psychologists did not make attempts to unify psychology under the banner of evolutionary psychology. Instead, the status of evolutionary psychology seemed equivalent to the status of other theories like behaviorism and cognitive psychology. Including sections on evolutionary psychological research in various textbooks did not radically change the books' overall framework. Textbook authors still placed more coverage on traditional explanations of topics such as decision-making, relationships, personality traits, and psychotherapy. There were no attempts to have evolutionary psychology become a division in the American Psychological Association, though this may have something to do with its interdisciplinary nature. Finally, I found no accounts in the literature where a psychologist trained in one theoretical framework, read about evolutionary psychology and subsequently decided to give up their research interests and pursue a program of evolutionarily inspired research. Undoubtedly, this occurred for a few individuals, but it certainly was not a trend. Nevertheless, I cannot say the same thing about students. Many students became attracted to evolutionary psychology, whether it was from taking a class, reading the literature, or working with a professor, during their undergraduate careers.

Can one consider the story of evolutionary psychology a successful tale of boundary maneuvering within psychology? From an historical perspective it is simply too soon to tell. Given that evolutionary psychology is such a young field, there needs to be more time and distance from the subject in order to obtain an appropriate perspective.

CHAPTER IV

COMPUTATIONAL THEORIES OF THE MIND: COGNITIVE SCIENCE

Cognitive science is an amalgamation of scientific disciplines including neuroscience, cognitive psychology, computer science, artificial intelligence, linguistics, anthropology, and philosophy of mind. Cognitive scientists collaborate in this interdisciplinary endeavor in an attempt to comprehend the phenomenon of information processing. Beginning in the 1970s, when cognitive science developed into a truly interdisciplinary program (see, Leahey, 1987; Watson & Evans, 1991), cognitive scientists came to infringe upon the disciplinary boundaries of psychology and affected psychologists' knowledge production activities through their conceptual, material, and social practices.

Throughout psychology's history, various psychologists have used cognitive approaches as a way to understand human mind, behavior, and emotions. In the late 1800s, German physiologist Hermann Ebbinghaus (1885/1913) carried out experiments on his own memory capacity by testing his recall of nonsense syllables. Ebbinghaus' recall results formed the well-known forgetting curve that professors today still introduce to students in various psychology classes. In *The Principles of Psychology*, William James (1890) proclaimed psychology "the study of mental life" and characterized its phenomena as “feelings, desires, cognitions, reasonings, decisions, and the like” (p. 1). In his experimental research, the structuralist E. B. Titchener (1910) attempted to discern the underlying components of consciousness. Titchener trained participants in his experiments to introspect on what he believed to be the foundations of consciousness: sensations, feelings, and images. In the early 1930s the British psychologist Frederick Bartlett (1932) found that memories constitute reconstructions of events rather than real events.

There exists a pervasive myth found in many textbooks of psychology that behaviorism “took over” psychology throughout the middle twentieth century, thus wiping out any mention of things cognitive (e.g., Galotti, 2004; Robinson-Riegler & Robinson-Riegler, 2004). Philosopher Gary Hatfield (2002) summarizes this myth stating, “There is a general impression that during the period from 1920 to the mid 1950s, behaviorism succeeded in driving cognitive topics and cognitive theoretical notions out of experimental psychology in America” (p. 221). Hatfield continued his narrative with a more appropriate interpretation: “Although behaviorism became strong or even dominant in the period 1920-1960, it by no means was able to stamp out the study of cognition…in American psychology” (p. 221).

In fact, revisionist historians of psychology are now pointing out that there were psychologists still employing cognitive approaches in their theoretical and experimental practices during this period (e.g., Greenwood, 1999; e.g., Mandler, 2002). For instance, the neo-behaviorists Edward C. Tolman and Clark L. Hull were amenable to postulating “internal states” as long as one expressed these states operationally as intervening between external stimuli and behavioral action (Greenwood, 1999). In his research on laboratory rats' ability to maneuver around a maze to acquire food and water, Tolman (1948) proposed that, through a series of trials, rats develop a "cognitive map" of the maze that allows them to more quickly acquire food and water. The ideas on perception of Gestalt psychologists like Wolfgang Köhler, Kurt Koffka, and Max Wertheimer, immigrating to the U.S. during World War II, were decidedly cognitive in nature (see, Sokal, 1984).

Given these example and many others (e.g., Piaget, 1929), it is apparent that cognitive perspectives were present from the beginning of psychology's disciplinary history and continued through to the present. Also, these examples demonstrate the vacuity of the myth that behaviorism completely wiped out cognition as a topic of study in psychology. Psychologists' familiarity with these various cognitive perspectives assisted in the acceptance of the practices of cognitive scientists in the early 1970s. Whether or not psychologists espoused a cognitive perspective, they were at least acquainted with the study of cognitive processes through James, Titchener, Piaget, and others.

It was also during the late 1960s that the development and introduction of the subdiscipline of cognitive psychology paved the way for the rest of cognitive science in psychology in the 1970s. While cognitive scientists now consider cognitive psychologists part of their interdisciplinary program, in the late 1960s the many disciplines comprising cognitive science had yet to professionalize and institutionalize into a formal interdisciplinary program. Cognitive psychologists—

who began incorporating ideas from linguistics, computer science, neuroscience, and artificial intelligence—helped foster the introduction of cognitive science to the discipline of psychology. In the next section, I define cognitive science and describe some of the fundamental assumptions associated with this interdisciplinary program.

Outline of Cognitive Science

Defining an interdisciplinary program like cognitive science can prove problematic for several reasons. One problem is that different disciplines may use their own esoteric terminology to define the interdisciplinary program, thus leading to confusion among the disciplines. Cognitive psychologists may prefer to use terms such as memory and intelligence when defining cognitive science, whereas computer scientists may prefer expressions such as data structures and computation in their definitions. Another potential problem in coming to some agreement about the nature of cognitive science is that the various disciplines comprising cognitive science may define it only in terms of their own role in the interdisciplinary program. For instance, artificial intelligence researchers may define cognitive science as an interdisciplinary field that attempts to develop machines or software programs that mimic human information processing capabilities. Philosophers of mind may ignore this definition and instead propose that cognitive scientists explore the possibility of understanding consciousness or of instantiating consciousness in a machine. Finally, it is important to note that not all cognitive psychologists, artificial intelligence researchers, computer scientists, and linguists consider themselves cognitive scientists. Some carry out research without affiliating themselves with this interdisciplinary program. Despite these potential difficulties, many cognitive scientists converge on similar definitions of their field.

Defining Cognitive Science

Cognitive psychologist Howard Gardner (1985) enthusiastically describes cognitive science as "the mind's new science." Gardner goes on to define cognitive science as an "empirically based effort to answer long standing epistemological questions—particularly those concerned with the nature of [human] knowledge, its components, its sources, its development, and its deployment" (p. 6). In their interviews with twenty eminent cognitive scientists, Peter Baumgartner and Sabine Payr (1995) summarize cognitive science as a "joint effort of specific disciplines to answer long-standing questions about the working of the mind—particularly knowledge, its acquisition, storage, and use in intellectual activity" (p. 11). Herbert Simon and Craig Kaplan (1989) characterize it as the "study of intelligence and intelligent systems, with particular reference to intelligent behavior as computation" (p. 1). In his overview of cognitive science, philosopher Owen Flanagan (1991) describes cognitive science as a "well-organized committee of disciplines and subdisciplines, all of which claim to have something to contribute to our understanding of mentality" (p. 177).[27] The common element shared by these various definitions is that cognitive scientists study how humans and machines process information. In other words, cognitive

[27] There are others who believe cognitive science is more of a perspective various disciplines use rather than an interdisciplinary program in and of itself (e.g., Hunt, 1989; Scheerer, 1988).

science is the study of how humans and machines receive, store, retrieve, transform, and transmit information (Stillings et al., 1987). This fundamental assumption allows the various subdisciplines of cognitive science to communicate with one another. A cognitive psychologist, for instance, can use this information processing perspective to discuss how humans store information in memory in the same way that a computer scientist can talk of storage in a computer's memory.

Cognitive scientist Michael Dawson (1998) proposes viewing the study of information processing from three interrelated levels of analysis. The first level of study is the computational level. At this level, cognitive scientists ask, "What information processing problem is the system solving?"[28] After identifying the information processing problem, these scientists can then formally describe the problem using some form of logical analysis or mathematical modeling. The second level of analysis is the algorithmic level. At this level, cognitive scientists ask, "What method is the system using to solve this information processing problem?" At this level, cognitive scientists attempt to discover the procedure a system uses to accomplish some kind of goal. At the heart of this level is the idea that information processing involves some kind of algorithm for manipulating internal symbols that represent something in the external world. When one views a tree outside, there is no one-to-one mapping of the image of the tree in our

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[28] Note that the term "system" can represent either human or machine information processing.

brains.[29] Instead, there is a manipulation of some kind of symbol system that acts as a representation of the tree during information processing. The third level of analysis is the implementation level. For this level of analysis cognitive scientists ask, "What physical structures are used to implement the algorithm in order to solve the problem?" For instance, cognitive scientists may search for the specific area(s) or structure(s) in the brain that carry out a particular algorithm for solving a problem.

The ways in which cognitive scientists study information processing vary due to their interdisciplinary perspective. Some cognitive scientists develop theories about cognitive processes divorced from human or animal experimentation. Others study animals and humans in order to create theories about mental life. Finally, there are those who study the organization and computational abilities of intelligent machines (Simon & Kaplan, 1989).

Despite their differing approaches, cognitive scientists rely on several methods in order to obtain converging evidence on information processing phenomena. They sometimes utilize psychophysical data to identify certain abilities and deficits in our ability to recognize and differentiate various stimuli in the environment. Reaction time experiments are popular among cognitive psychologists for comparing, say, lexical decisions under varying conditions. Artificial intelligence researchers and computer scientists develop software programs and mathematical models that they believe effectively reproduce components of human information processing. One of the more recent and

[29] While there is no one-to-one mapping in the brain, external stimuli are projected topographically on the visual cortex (see, Kosslyn & Koenig, 1992).

popular avenues for cognitive science research is brought about by brain imaging technology. Neuroscientists use techniques such as computed tomography scans (CT scans), positron emission tomography (PET) and functional magnetic resonance imaging (fMRI) to identify the brain structures involved in various information processing tasks. While each cognitive scientist uses a few of these methods, they rely on discoveries from cognitive scientists using other methods to develop a complete picture of information processing.

In the following section, I discuss the origins of the cognitive science approach to human information processing. Given the vast number of disciplines associated with cognitive science, I delimit this history by focusing on the parts of its history most germane to psychology.

History of Cognitive Science

Cognitive science has its roots in the work of a number of different fields that came to bear on the topic of information processing. Even though cognitive science became a professionalized interdisciplinary program in the middle 1970s, the foundation of cognitive science came from earlier research. One of the fields in the formation of cognitive science was computer science. Of particular importance are the connections computer scientists developed between how humans and machines process information. In the 1930s, logician Alan Turing (1936) developed a thought experiment pertaining to a simple machine—what is now known as the Turing Machine—that could perform various kinds of computations.[30] According to Turing, this machine could carry out any kind of well-defined computation as long as it possessed the correct instructions, enough time, and enough resources to carry out the computations. The idea of a Turing Machine prompted others such as John von Neumann to develop its physical manifestation—the digital computer.

In a later paper on computing machines and intelligence, Turing (1950) proposed a test, now called the Turing test, to discern if a computing machine could display some form of intelligence.[31] In the test, a human would converse with other humans and a computing machine via some communication device. Turing argued that if the human could not tell the difference between the computing machine and the other humans' dialog, this would provide a kind of evidence for machine intelligence. Largely inspired by Turing and von Neumann, the workings of the digital computer came to be an important metaphor for conceptualizing how humans process information throughout the formative years of cognitive science.

Contemporaneously, research on neural networks, cybernetics, and information theory played a role in the development of cognitive science. Neurophysiologist Warren McCulloch and logician Walter Pitts (1943) developed the idea of neural networks. They created a mathematical model in which individualized "neurons" operate as binary devices—on/off switches—interacting

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[30] Some point to Charles Babbage as the progenitor of the idea of thinking machines, but cognitive psychologist and historian of psychology Christopher Green (2001) argues against this interpretation.

[31] Note that Turing uses the phrase "computing machine" as there were no computers in the modern sense at the time.

with each other to complete some prescribed function. In the same decade, the Canadian psychologist Donald Hebb (1949) developed a hypothesis about how neural connections in the brain are strengthened via repeated interactions between assemblies of neurons. Neural networks began to serve as viable mathematical models for the workings of human neural connections (Bechtel, Abrahamsen, & Graham, 1999).

From cybernetics, cognitive scientists borrowed the idea of feedback processes. During World War II, scientists used the idea of feedback as a method for improving anti-aircraft missiles. Mathematician Norbert Wiener (1948) and his colleagues began developing the idea of feedback as a way of understanding goal-directed behavior in humans and machines. Contemporaneously, the mathematician Claude Shannon (1948, July and October) published work on a way to transmit information effectively and developed statistical procedures for measuring information flow. As these ideas became public, scientists began describing human and machine information processing using the notions of feedback and descriptions of the flow of information.

With these ideas in place, it fell upon the next generation of researchers to build machines that emulated human thought processes. The 1950s proved to be a pivotal time for the development of cognitive science as researchers from numerous disciplines came to focus on the phenomenon of information processing. Scientists also developed some of the first artificial intelligence programs. In 1956, Herbert Simon, Allan Newell, and J. Clifford Shaw developed the General Logic Theorist and then, in 1959, the General Problem Solver (e.g., 1958). Basically, both programs could solve simple logic problems and prove mathematical theorems by breaking down a problem into sub-goals in an attempt to minimize the distance between its present state and some future goal state. Newell and Simon compared the program's performance on problem solving tasks with people's introspective reports of the same tasks and found that the program operated in a similar manner to people's introspective reports. The similarity between how the programs and humans solved problems provided some evidence for the appropriateness of the analogy between computer and human information processes.

It was also in 1956 that mathematicians Marvin Minsky and John McCarthy organized a two-month workshop at Dartmouth College that brought together a number of researchers, including Newell and Simon, interested in artificial intelligence (Gardner, 1985). The two-month workshop allowed researchers opportunities to present their research, trade technologies, make inroads into the connection between humans and machines, and develop the rudiments of the discipline of artificial intelligence.

A conference later that year fostered further inroads into the development of cognitive science. Between September 10 and 12, 1956, a number of researchers attended the Symposium on Information Theory at Massachusetts Institute of Technology (MIT) (Bechtel et al., 1999). Psychologist George Miller presented his ideas on short-term memory capacity, Newell and Simon demonstrated a proof of a theorem by their General Logic Theorist, Nathan

Rochester and his colleagues presented a computer model of Hebb's neuronal assemblies, and Noam Chomsky delivered a talk on his transformational grammar. Miller (2003) retrospectively described the moment he believes cognitive science was created stating, "I date the moment of conception of cognitive science as 11 September 1956, the second day of a symposium organized by the 'Special Interest Group in Information Theory' (p. 142). Miller's declaration is merely a personal belief rather than a statement of fact. There is no evidence that during this time scientists tried to develop the ideas presented at this conference into some meaningful interdisciplinary program. The MIT conference and the one at Dartmouth did however served as a turning point in the development of cognitive science because it became apparent to the presenters from various disciplines at these events that they shared a common view that the mind was an information processor.

Linguistics also came to the forefront in the 1950s. It was not that linguistics was a new field. Structural linguists such as Ferdinand de Saussure and Leonard Bloomfield studied language, albeit in different ways, throughout the nineteenth and early twentieth centuries. Rather, linguistics came to the attention of many social scientists due to a change in perspective in how to approach the study of language. Noam Chomsky (1957) published his theory of transformational grammar in *Syntactic Structures*. In the monograph, Chomsky pointed out the inadequacies of relying solely on phrase-structures—breaking sentences in components such as noun and verb phrases—for a complete understanding of language. In addition to phrase structure, Chomsky advocated the need for a transformational grammar—a procedure that changes one sentence into another or establishes a relationship between two sentences. Chomsky revised these ideas continually throughout the 1960s and 1970s (see e.g., Chomsky, 1965, 1972). For instance, he added the ideas of deep and surface grammatical structures to his theory. But throughout these revisions, he maintained the importance of syntax in understanding any natural language. Also important to cognitive science were Chomsky's ideas on universal grammar. Chomsky believed that infants possess an innate propensity for understanding any natural language. This propensity implied a modular view of the mind, at least for language capabilities. This modular view helped develop another connection between humans and the digital computer. Digital computers operated in a modular fashion. They could only do certain functions with proficiency, such as solving logic puzzles. Implicating modular language capacities in humans helped solidify the relationship between humans and computers.

During the late 1950s, a number of psychologists began moving away from the theoretical position of behaviorism. This was evident in the rise in publications pertaining to cognition that came out during the late 1950s and 1960s (Scheerer, 1988). George Miller (1956) published his well-known paper on short-term memory capacity, which he had earlier presented at the symposium at MIT. Harvard psychologist Jerome Bruner and colleagues (1956) published *A Study in Thinking*, which departed from behaviorists' stimulus-response models of learning and focused on how humans play an active role in forming concepts.

Their theories of the strategies people employ to solve problems mimicked those from computer programs like the General Logic Theorist (Johnson-Laird, 1988). It was also at this time that Leon Festinger (1957) came out with his theory of cognitive dissonance. Finally, Chomsky (1959) provided a critical and, some say, devastating analysis of behaviorist B. F. Skinner's (1957) *Verbal Behavior.* In his critique, Chomsky argued that language acquisition could not occur via simple associations, reinforcements, or stimulus-response sequences. These theories simply do not account for either the inherent creativity in our language production or young children's use of language. Chomsky argued that children's language ability is at odds with Skinner's behaviorist assumptions. For instance, young children use phrases like "I goed to the park." But adults do not make these kinds of grammatical mistakes. Chomsky proposed that it would be extremely unlikely for children to speak ungrammatically if simple stimulus-response learning was occurring. Chomsky's critique pointed out one of the limitations of behaviorist theory and, similar to the other ideas described above, had a decidedly cognitive tone that threatened behaviorism's popularity in psychology.

The interest in cognition continued into the 1960s. Jean Piaget's work on children's cognitive development, though developed in the 1920s, caught U. S. psychologists' attention in the 1960s. Miller (1962) published a textbook with a decidedly cognitive flavor entitled *Psychology, the Science of Mental Life*. Finally, psychologist Ulric Neisser (1967) introduced a new generation of students to a cognitive approach to the study of psychology in his textbook *Cognitive Psychology*.

The cognitive turn in psychological studies spurred the development of the Center for Cognitive Studies at Harvard University in 1960. Psychologists Jerome Bruner and George Miller developed the Center under the auspices of ten years of funding from the Carnegie Corporation (Bechtel et al., 1999).[32] The Center provided opportunities for young scientists to pursue their interests in linguistics, attention, memory, language processing, and a host of other topics germane to cognitive science. Other schools developed similar institutes in the 1970s such as the Institute for Cognitive Science at the University of California, San Diego.

Despite scientists converging towards solving problems about cognition and information processing, there was little interdisciplinary flavor to much of their work. The theoretical chemist and artificial intelligence researcher Christopher Longuet-Higgins (1987) coined the phrase "cognitive science" during an address on artificial intelligence in 1973. The phrase caught on among this diverse group of scientists and they adopted it as the name of their program. It was also during the 1970s that psychologists, computer scientists, linguists, and others professionalized and institutionalized cognitive science as an interdisciplinary program.

By 1983, a host of colleges and universities around the country had established centers for cognitive science including Stanford University, Cornell Medical School, the University of Chicago, and Princeton University (Bechtel et al., 1999). At Princeton, for instance, Miller and Gil Harman, with funding from the James S. McDonnell Foundation, developed the Program for Cognitive Science

[32] The Center closed in 1970 after the Carnegie funding ran out.

and the Laboratory of Cognitive Science (Harman, 1988; Hirst, 1988). From the 1980s into the 1990s, cognitive science expanded its scope to include neuroscientists and philosophers of mind.

The next sections outline the types of conceptual, material, and social practices cognitive scientists brought into psychology.

Conceptual Practices

Cognitive scientists affected the boundaries of psychology through the introduction of numerous theoretical and mathematical models of how humans process information. During the 1970s and 1980s, the digital computer served as the metaphor for conceptualizing how humans process information. Using complex objects in the environment as metaphors for our minds is common practice in the sciences (Jaynes, 1990). The historian of psychology David Leary (1990) argues that metaphors serve a directive function in that different metaphors can direct researchers to see different things in the same phenomenon. For instance, both René Descartes and Sigmund Freud used hydraulic metaphors to portray various functions of the mind. In the post-World War II era, the digital computer served this metaphorical function. David Rumelhart (1998), co-founder of the Institute of Cognitive Science as UCSD, exclaimed that the computer "has been a valuable source of metaphors through which we have come to understand and appreciate how mental activities might arise out of the operations of simple-component processing elements" (p. 207). Accompanying this metaphor, terms related to computers and information processing such as parallel distributed processing, neural networks, expert systems, channel capacity, coding, and connectionism became part of the vocabulary of cognitive psychologists and psychologists in general.

With the computer-as-mind metaphor, cognitive scientists created two types of models for understanding human information processing (Dawson, 1998). The first, which served as the dominant model during the 1970s, was the classical or symbolic model of human information processing. From the classical perspective, all of our cognitive abilities occur via a systematic process that is similar to the processing carried out by digital computers. The steps in cognitive processing occur in serial order, with each step occurring logically before the next step in the same way a digital computer carries out tasks. The most well known example of the classical model is the Turing Machine described above.

The second model of human information processing is the connectionist network, also known as parallel distributed processing networks (e.g., McClelland & Rumelhart, 1981). Connectionist models came into prominence during the 1980s and 1990s. Cognitive scientists who espouse this model of information processing believe that the correct way to model our brains is through an interconnected network of nodes. There are four defining features of connectionist networks (Schneider, 1987). First, all information processing occurs as a result of these simple nodes and their interconnections. These nodes can be in one of two states at any given time: on (excited) or off (inhibited).[33] Second, there is an assignment of weighted values for the connections between nodes. Knowledge is stored in the weighted connections between nodes. Third, the

[33] Some newer models break away from this dichotomy and indicate gradients of excitation for these nodes.

behavior of any particular node in the network is contingent upon the inputs to that node and the subsequent transformation of these inputs into new information. Information input causes certain nodes to activate and these nodes in turn either activate or inhibit other nodes. Lastly, learning involves changing the connections between the nodes in the network via feed forward and back propagation processes. Frank Rosenblatt's (1958) one-layer perceptron model is an early version of a connectionist network and, more recently, other cognitive scientists (e.g., Rumelhart, Hinton, & Williams, 1986) have produced multi-layer perceptron models (see Figure 1).

Cognitive psychologists were already using many of the methods used by cognitive scientists such as mathematical modeling, measuring reaction times and analyzing psychophysical data. It is safe to say that most psychologists were familiar with these types of practices as they were appearing in widely read psychological journals. One of the difficulties and criticisms leveled at cognitive scientists using these methods was that they could not actually examine the thinking processes of individuals (Ericsson, 1998). Instead, cognitive scientists had to rely on behavioral data to infer what was going on in the unobservable brain.

A group of methods that psychologists did have to contend with in the 1980s and 1990s were brain imaging techniques brought into the discipline from neuroscientists. Brain imaging techniques such as fMRI scans, allowed cognitive scientists to "look inside the mind" of their participants. These scientists could duplicate three dimensional images of the brain and map out the various

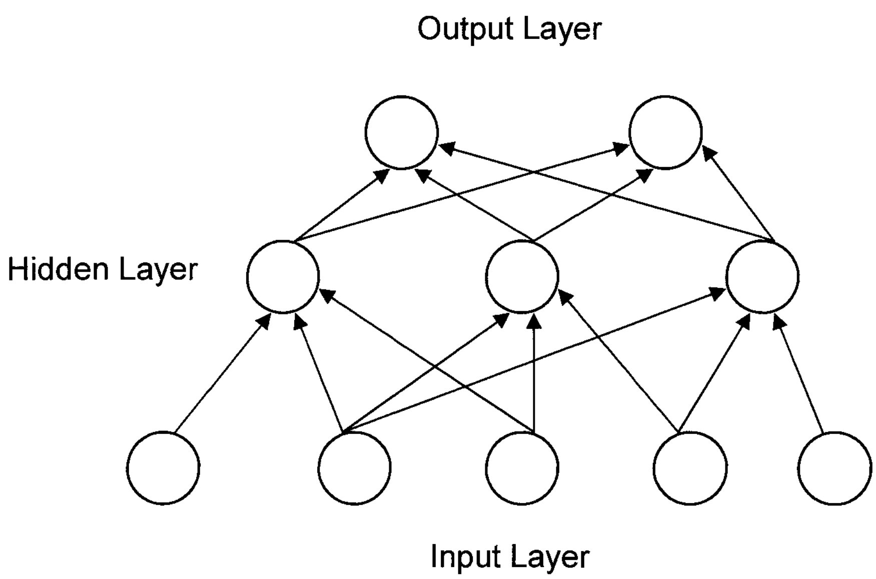

Figure 1. Reconstruction of a multi-layer perceptron with feed forward, partial connectivity. The circles represent nodes in the perceptron. The directional arrows indicate the direction of information flow and nodal connectivity. The perceptron contains three layers with connectivity between successive layers. The directional arrows represent weighted connections between nodes. The input layer receives information from the environment and transforms this information into a representation of this information. Depending on a node's level of excitation or inhibition, it will transmit its information to connected nodes in the hidden layer. The hidden layer carries out further processing and then sends this information to the output layer. The information in the output layer represents a response to the initial information. In a massively parallel model, all nodes in every layer connect via bidirectional arrows indicating feed forward and back propagation processes.

structures involved in tasks such as language comprehension, problem-solving, and object identification. Another advantage of these techniques was that neuroscientists could compare "healthy" brains with those from individuals suffering from a particular brain abnormality such as Broca's aphasia and prosopagnosia. These techniques were not only a boon for cognitive scientists but they served as a powerful inscription device that promoted their ideas. I discuss brain imaging as an inscription device below.

Material Practices

The primary use of computers as a technological tool in psychology is for collecting and analyzing data derived from humans and other species. Software programs such as SPSS, SAS, and Minitab are mainstays in modern psychological data analysis. These programs allow psychologists to analyze quickly and efficiently large amounts of data. Psychologists also use human-computer interfaces to conduct research and manipulate variables, as was the case with evolutionary psychologists using various software programs to manipulate body type and facial feature images.

Cognitive scientists introduced psychologists to a new technological use for computers—developing computers and computer programs to emulate human thought processes. This is a radical shift from traditional psychological research. With the use of computers and computer programs, cognitive scientists extended the realm of investigating human mind, behavior, and emotion from organic, sentient life to inorganic machines.

Earlier I mentioned attempts by artificial intelligence researchers to develop programs for cognitive processes such as Newell and Simon's General Logic Theorist. Another example is computer scientist Edward Feigenbaum and colleagues' (1971) DENDRAL program, which was used to identify organic compounds. As a test of the program's efficacy, Feigenbaum presented DENDRAL with mass spectrograph data on particular organic compounds. DENDRAL was to gather data about the molecular structure of these organic compounds and then generate a list of all potential organic compounds for the given data. DENDRAL was as successful at correctly identifying the organic compounds as expert chemists (Gardner, 1985). In 1970, Terry Winograd developed a program called SHRDLU. Part of the program was a virtual world in which SHRDLU could act. In the "world" were basic, colored geometric shapes in various configurations. One of the remarkable aspects of this program was that one could type in instructions for SHRDLU to carry out in this virtual world. For instance, one could ask SHRDLU something like, "Place the blue triangle on top of the red square." SHRDLU would interpret these written commands and carry out the given tasks in this virtual world. The advance from Newel and Simon's work to Winograd's indicates the notable achievements of artificial intelligence researchers in a relatively short time and is an indication that many cognitive scientists believed they were on the right track towards understanding human information processing.

These programs represent a few of the initial attempts at artificial intelligence. But these programs could only perform a few specified tasks quickly and efficiently. While these *expert systems*—programs that can only execute a few tasks such as playing chess or proving theorems—performed their tasks well and provided insight into a few human problem-solving strategies, they were severely limited as representations of human cognition. Humans take on a variety of cognitive tasks in parallel and do them with relative ease. Additionally, cognitive scientists largely ignored attempts at simulating motivational and emotional capacities or the relations between these two and other cognitive processes (Simon, 1979).

The challenge then to artificial intelligence researchers was to design a program that emulated the versatility and variety of human cognitive abilities in addition to motivational and emotional capabilities. One of the more recent and advanced forms of artificial intelligence is COG, a form of artificial intelligence created at the artificial intelligence laboratories at MIT. COG is being equipped with visual, proprioceptive, and other sensory capacities for the purposes of understanding our thought, feelings, and behaviors. Rodney Brooks, the developer of COG, believes that in order to develop a truly artificial intelligence; one must build it with the same physical and mental characteristics of humans. COG represents a form of material practice whose use is not to speed up the process of data analysis, or make complex calculations easier, or record psychological data, but to be something like us.

Social Practices

One of the social practices that helped the relationship between cognitive science and psychology was cognitive scientists' ability to obtain large amounts of funding for their projects. In the mid-1970s on through to the mid-1980s, many cognitive scientists convinced the Alfred P. Sloan Foundation to contribute approximately twenty million dollars to all areas of cognitive science research (Gardner, 1985). Cognitive scientists used much of this money to create cognitive science research centers at the numerous colleges and universities mentioned above. As the Sloan initiatives came to an end, the System Development Foundation (SDF) became involved in funding cognitive science research, investing twenty-six million dollars from 1982 to 1984 (Bechtel et al., 1999). The biggest single contribution of the SDF went to the Center for the Study of Language and Information at Stanford University. This Center fostered research relationships in the areas of linguistics, computer science, and reasoning between faculty at Stanford and research organizations such as the Palo Alto Research Center (formerly Xerox-Parc). Additional funding during the 1980s came from various sources such as the Office of Naval Research, the Air Force Office of Scientific Research, and the National Science Foundation (NSF) (Schneider, 1987). The National Science Foundation, in particular, was important to cognitive science research. When the Sloan initiatives and SDF funds stopped, the NSF continued to fund these large research centers.

In the 1970s, the following new journals appeared: *Cognitive Psychology* (1970), *Cognition* (1972), *Memory & Cognition* (1973), and the *Journal of Mental Imagery* (1977). Cognitive scientists began issuing *Cognitive Science: A Multidisciplinary Journal of Artificial Intelligence, Psychology, and Language* in 1977. Other journals followed such as *Behavioral and Brain Sciences* in 1978 and *Cognition and Brain Theory* in 1981. Two years later, cognitive scientists formed the Cognitive Science Society (CSS). The Society held its first conference in 1979 at the University of California, San Diego sponsored by the university's Program in Cognitive Science and funded by the Sloan Foundation. In 1980, *Cognitive Science* became the official journal of the CSS and the journal broadened its scope to include contributions from developmental psychology, philosophy, and cognitive anthropology (Collins, 1980). In 1985, *Cognitive Science* merged with *Cognition and Brain Theory*, which resulted in more contributions by philosophers and neuroscientists (Schunn, Crowley, & Okada, 1998). By 1997, the subtitle of *Cognitive Science* changed to "A Multidisciplinary Journal of Anthropology, Artificial Intelligence, Education, Linguistics, Neuroscience, Philosophy, Psychology."

Undergraduate and graduate classes in cognitive science became common in colleges and universities. In 1985, Vassar College held a conference with approximately fifty colleges and universities concerned with implementing cognitive science into the undergraduate curriculum (Gardner, 1985). Michael Dawson (1998) estimates that by 1995 colleges and universities developed approximately two hundred cognitive science programs around the world. Many of these centers and programs acted independently of psychology departments. In some cases, cognitive psychologists kept their original disciplinary affiliation but began working in cognitive science centers. In other cases, psychology departments dissolved to make way for the development of cognitive science centers (Hirst, 1988).

Like my other case studies, cognitive scientists claimed their field as a revolution or paradigm shift for psychological science. These claims served the rhetorical function of persuading others of their expert and authoritative status in the field of human mind and behavior. The historian of psychology Thomas Leahey (1992) contented that, “Belief in a cognitive revolution is an entrenched part of modern psychology’s form of life” (p. 313). Their claims of a revolutionary field rode the coattails of the general cognitive and mentalist perspectives that had supposedly ousted the behaviorist perspective prevalent in psychology at the time. For instance, a statement by a number of cognitive scientists to the Research Briefing Panel on Cognitive Science and Artificial Intelligence claimed that cognitive science was “advancing our understanding of the nature of mind and the nature of intelligence on a scale that is proving revolutionary” (Estes et al., 1983, p. 21). Roger Sperry (1993) proclaimed that “by 1971 it was already clear that many psychologists had come to recognize that their discipline was in the process of a major paradigm shift, in which behaviorism was being replaced by an opposing new mentalism or cognitivism” (p. 881).

Bernard Baars (1986) is perhaps the most articulate individual regarding the putative revolutionary nature of these ideas. Baars calls it a “quiet revolution in thought” in which “the dominant meta-theory of the previous 50 years [behaviorism] was discarded or changed fundamentally, and a new point of view began to take shape” (p. 4). Baars breaks the cognitive revolution into three stages. A group of scientists characterizes each stage. Those scientists initially trained as behaviorists who subsequently changed to cognitivism, Baars characterizes as “adapters.” The adapters were able to break away from the traditional behaviorist perspective after realizing the anomalies caused by this perspective. Examples of Baars’ adapters are the aforementioned George Miller, James Jenkins, who studied language acquisition, and George Mandler, whose research focused on memory and emotions. The “persuaders” constitute the second stage in Baars’ model. The persuaders’ role was to change the psychological community’s perspective from behaviorism to cognitivism. Baars claims Ulric Neisser as an exemplar of a persuader due to his influential textbook on cognitive psychology. The “nucleators” comprise the final stage. The nucleators came from outside psychology but provided assistance in bringing about the revolution via new techniques and ideas from other disciplines. Baars considers individuals like Noam Chomsky, Herbert Simon, and Jerry Fodor, the philosopher of mind, as nucleators.

The inclusion of neuroscientists and their brain imaging studies in cognitive science provided another social practice to which psychologists had to attend. In the Social Science Citation Index from 1980 to 1996 publications of brain imaging studies ran into the thousands, increased to almost 350 in 1996, and the rate was doubling every 18-24 months (Fox, 1997). Brain imaging “powerfully redefines concepts like behavior, nurture, culture and environment” (Beaulieu, 2003, p. 563). I mentioned previously that one of the major criticisms of the cognitive approach was the need to *infer* mental events. The introduction of brain imaging helped alleviate this critique and created a powerful inscription device for cognitive scientists. These images or mappings of the brain redefined concepts like “behavior” and “culture” and recast them in biological terms, specifically through the ability to view spatial patterns in the brain. Cognitive scientists displayed these spatial patterns in their publications, bringing credibility to their research. They were able to propose theories about cognitive processes such as memory and intelligence in addition to providing a visual display of where in the brain these processes took place. Similarly, cognitive scientists could create pictorial images of the structural differences between “healthy” and “pathological” brains. Brain images formed powerful “visual arguments” about the nature of cognitive processes and helped to promote cognitive scientists as authorities in the field of cognition (Dumit, 2004). The 1990s reflected the popularity and perceived importance of these kinds of brain studies. The 1990s were proclaimed the “Decade of the Brain” by President George H. Bush. The Library of Congress and the National Institute of Mental Health collaborated to bring this message to the public through conferences and other forms of media.

Given these numerous conceptual, material, and social practices, it is important to examine psychologists’ reactions to these practices. The next section outlines the resistances and accommodations psychologists made to the claims of cognitive scientists.

Resistances to Cognitive Science

Resistances to the practices of cognitive scientists came mainly from behaviorists. Their primary contention pertained to cognitive scientists’ focus on mental events. As I described earlier, with the exception of radical behaviorists (e.g., John B. Watson, B. F. Skinner), most neobehaviorists (e.g., Tolman, Hull) did not deny the existence of mental events. Instead, they tended to relegate these events as either impossible to study empirically or as intermediary between the more important environmental and human action variables. Behaviorists found this to be the major weakness of cognitive approaches. The behaviorists argued that even if mental events such as concept formation and representations of stimuli exist, these events were unobservable. There was no place for the study of mental events in a truly scientific psychology.

The most public resistance to cognitive science came from psychologist B. F. Skinner, whose behaviorist theory of operant conditioning stood in direct opposition to the tenets of cognitive science. Skinner's resistances make sense in light of the boundary-work framework. He created a theory that was recognizably antithetical to cognitive science, yet Skinner was arguably the most well-known psychologist of the twentieth century (Rutherford, 2000). Skinner derived much attention with his operant conditioning research and with this attention came epistemic authority and expert status. Skinner developed enough expert status that he felt confident to write about issues such as improving society and the elimination of free will through operant conditioning and behavior modification (e.g., Skinner, 1971, 1976). He had a personal stake in defending his intellectual territory, as did those professionals trained in behaviorist doctrine for years.

Skinner (1986), for instance, attacked those who espoused a cognitive perspective because they "invent internal surrogates, which become the subject of their science" (p. 79). Elsewhere, Skinner (1985) outlined five major reasons why psychologists should not adopt a cognitive science perspective. First, he believed cognitive scientists' use of the metaphor of storage was inappropriate because he believed people did not store representation of experiences in their minds. Second, as the above quote illustrates, he felt that speculation of internal processes was a mistake, as no one could directly observe these processes. Third, Skinner accused cognitive scientists of "emasculating the experimental analysis of behavior" with their focus on descriptions of environments instead of the actual environment and reports of intentions rather than behaviors. Fourth, he believed it a mistake to rely on states of mind as a cause of behavior rather than as side effects of the true causes of behavior. Finally, Skinner objected to the "relaxing standards of definition and logical thinking," resulting in a "flood of speculation" that was "inimical to science" (p. 300).

Behaviorists had another reason to resist the practices of cognitive scientists. Most behaviorists used laboratory animals for their experimentation. Essentially, the data they obtained to verify their theories pertaining to human behavior were products of animal models. According to historian of science Hunter Crowther-Heyck (1999), "The study of rats in T mazes or pigeons in Skinner boxes was the paradigmatic experiment. The study of human behavior was not wholly neglected by behaviorists...but even when human behavior was the subject of study it was generally the 'animal' aspects of humanity, rather than the 'higher qualities,' such as language, that were examined" (p. 47). Cognitive scientists, on the other hand, used humans *and* computers as their data sources, but most found little use for nonhuman species in their research.[34]

Some psychologists also resisted cognitive scientists' reliance on artificial intelligence and the computer metaphor as models for human information processing.[35] Skinner resisted the use of artificial intelligence, believing it offered no new insight into human behavior. In an article explaining why psychology had yet to become a science of behavior, Skinner (1987) questioned the need for cognitive scientists' reliance on artificial intelligence stating, "If an artificial organism can be designed to do what logicians and mathematicians do, or even more than they have ever done, it will be the best evidence we have that intuitive mathematical and logical thinking is only following rules, no matter how obscure. And following rules is behaving" (p. 784). The cognitive psychologist and historian Christopher Green (2001) brought into question the viability of using connectionist networks as models for neural processes. He argued that individuals applying these networks are vague in terms of what these networks supposedly model because "Despite some superficial similarities, the way in which connectionist networks are constructed appears to violate some basic facts on neural function" (p 104). One possibility for psychologists' resistances to artificial intelligence and computer modeling was the threat posed by these

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[34] I should note that cognitive ethologists use nonhuman species in their research on animal minds (e.g., Bekoff, 1998; Premack & Woodruff, 1978).

[35] Many philosophers connected to cognitive science have engaged in the debate on the computer metaphor and artificial intelligence (e.g., Dennett, 1998; Hardcastle, 1995; Haugeland, 1981; Horst, 1996; Searle, 1980). Also see mathematician Roger Penrose's (1994) *Shadows of the Mind* for an exposition of the different levels of belief (i.e., strong artificial intelligence, weak artificial intelligence) in artificial intelligence.

endeavors. This perceived threat may not be a consequence of psychologists giving up some kind of expert status, but rather a more general threat to human nature. Humans have always perceived themselves as the pinnacle of intelligent life. The resistance to artificial intelligence research may be a reaction to the threat that, in principle, something could be as intelligent as humans could.

Another resistance came from those psychologists who felt that neuroscience and brain imaging would somehow damage the field. Some felt that the increased use of brain imaging technology might lead to erasing the "individual" from psychological research (Beaulieu, 2003). Images from PET scans, for instance, require averaging and renormalizing data over many subjects. These brain images remove the unique structural features of each individual's brain. Given the inherent complexity of the brain, these images may provide the wrong impression about brain structure and the localization of function. Additionally, demonstrating the location of a particular structure and correlating it with a particular function answers the questions of *where* and *what* but does not get at the question of *how* people carry out processes such as memory and problem solving. Some (e.g., Goertzel, 1995; Paller, 1995) criticized the materialist assumptions behind the growing popularity of brain imaging. Finally, others were concerned with the increasing reliance on neuroscience for explanations of human mind and behavior. For instance, Skinner (1987) believed that cognitive scientists may put themselves out of a job due to their reliance on neuroscience. He stated, "Once you tell the world that another science will explain what your key terms really mean, you must forgive the world if it decides that the other science is doing the important work" (p. 784).

Accommodations to Cognitive Science

There are several instances of psychologists making accommodations for cognitive science. One measure of psychologists' accommodations for cognitive science is the number of citations attributed to cognitive science in psychology. Were psychologists using the literature of cognitive science more than the literature of, say, behaviorism? A citation analysis comparing cognitive psychology and behaviorism literature indicates the increasing number of citations pertaining to cognitive topics in flagship journals (*American Psychologist*, *Annual Review of Psychology*, *Psychological Bulletin*, *Psychological Review*) from 1977 to 1996 as compared with citations of behaviorist topics (Robins, Gosling, & Craik, 1999). During this twenty year span, there appeared to be a ubiquity of cognitive approaches to understanding a variety of phenomena in psychology's various subdisciplines including social psychology (e.g., Fiske & Haslam, 1996), personality (e.g., Matthews, Derryberry, & Siegle, 2000), educational psychology (e.g., Glaser, 2000), developmental psychology (e.g., Carey, 1990), and clinical psychology (e.g., Stein & Young, 1992). Previously, I mentioned the number of journals pertaining to cognition that came out during the 1970s such as *Cognition*, *Cognitive Psychology*, and *Memory & Cognition*.

In terms of pedagogical accommodations, cognitive psychology courses became part of the curriculum at most colleges and universities. Some institutions offered courses, and undergraduate and graduate specializations in cognitive science through their psychology departments (e.g., Carnegie Mellon University, University of Pittsburgh) instead of in separate centers or institutes. Some introductory textbooks began covering cognitive science, albeit sparingly. But there was usually ample coverage of cognitive psychology (e.g., Gleitman et al., 1999; Gray, 1999). Cognitive psychology textbook authors, of course, devoted space to the history of cognitive science, in addition to topics such as artificial intelligence, linguistics, and connectionist networks (e.g., Anderson, 2005; Galotti, 2004; Martindale, 1991). Publication began on many textbooks for undergraduate and graduates students in cognitive science (e.g., Collins & Smith, 1988; Johnson-Laird, 1988; Posner, 1993; e.g., Thagard, 1996).[36] Along with the inclusion of the topic of cognitive science in some psychology textbooks, PET images, CT scans, and fMRI brain images served as picture covers for many of psychology's textbooks (Beaulieu, 2002).

Similar to evolutionary psychology there is no division of cognitive science within the American Psychological Association, but this may pertain to cognitive science's interdisciplinary nature. Most people aligned with cognitive science can be members of the APA's experimental psychology division (Division 3), the Psychonomic Society, the American Psychological Society, and the Association for the Scientific Study of Consciousness. So there are a number of professional organizations for those who consider themselves cognitive scientists within the discipline.

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[36] While interdisciplinary in nature, most of these cognitive science textbooks primarily focus on information processing from a psychological perspective rather than perspectives from linguistics, artificial intelligence, computer science, or philosophy of mind.

Did psychologists accommodate cognitive science enough for one to consider cognitive science a scientific revolution or paradigm shift in psychology? Certainly, one of the social practices of many cognitive scientists was to pronounce the field as such and they introduced a number of conceptual and material practices into the field. Examining the status of cognitive science as a paradigm, historian of psychology Thomas Leahey (1987) answers in the negative stating, "Cognitive scientists believe in a revolution because it provides them with an origin myth, an account of their beginnings that helps legitimize their practice or science...But there was no revolution...The cognitive revolution was an illusion" (p. 456). Murray White's (1985) analysis of cognitive psychology textbooks mentioned in Chapter 2 concurs with Leahey's assessment. White's analysis indicated little agreement among textbook authors about important ideas and concepts within cognitive psychology. The computer-as-mind metaphor served as a useful heuristic for developing models of information processing, but few psychologists outside the study of cognition use it with any regularity. There was also the question of the extent of cognitive scientists' actual knowledge of human mind and behavior. For instance, cognitive scientist Donald Norman (1980) admits that he is "struck by how little is known about so much of cognition...I have studied memory for years, yet am unable to answer even simple questions about the use of memory" (p. 8). In order to remedy this deficiency, Norman urged cognitive scientists to incorporate social, cultural, and emotional perspectives into their work. Finally, behaviorism, the alleged usurped paradigm, never entirely disappeared with the advent of cognitive science.

Behaviorists still conducted and published stimulus-response and reinforcement theory research throughout cognitive scientists' involvement in psychology. While it is safe to say that cognitive science achieved some degree of respect within the discipline of psychology between the 1970s and 2000, it did not provide a new paradigm for psychology.

In summary, the relationship between the interdisciplinary program of cognitive science and the discipline of psychology is still evolving. As computing technology advances at an exponential rate, advanced forms of artificial intelligence are possible. This leads to the possibility of new, sophisticated models of human information processing that may affect the boundaries of psychology. As Howard Gardner (1992) suggested, sometime in the future cognitive science may absorb most of the topics and personnel in psychology, leaving the remaining psychologists just a few issues to deal with such as therapy and motivation. Until then, psychologists will need to continue their boundary-work efforts of resistance and accommodation with the conceptual, material, and social practices of cognitive science.

CHAPTER V

THE SELF-ORGANIZING MIND: CHAOS THEORY

Of my three case studies, chaos theory is perhaps the most unorthodox interdisciplinary program to have prompted psychologists to engage in boundary-work in the period between 1970 and 2000. Its connection and relevance to psychology is not as apparent as that of evolutionary psychology and cognitive science. Evolutionary psychologists conduct research on the adaptive mechanisms that helped humans survive and reproduce during the Pleistocene age and on how these mechanisms play a role in our cognitive and behavioral skills today. Cognitive scientists investigate human and machine information processing systems and attempt to develop models of these processes in order to gain an understanding of cognitive skills such as memory, language, decision making, and problem solving. But "chaos" is an unusual and evocative term that conjures up images of disorder and randomness. The *Oxford English Dictionary* (OED) defines chaos in a number of different ways such as a "gaping void, "utter confusion and disorder," and "a confused mass or mixture." These definitions of chaos seem paradoxical to the goals and general interests of psychologists. Psychologists are usually concerned with finding regularities in or prescribing order to psychological phenomena. For instance, personality psychologists attempt to create order in personality typologies by grouping people according to categories such as “extroversion,” “conscientiousness,” and “neuroticism.” Clinical psychologists rely on the *Diagnostic and Statistical Manual of Mental Disorders* (1994) as a taxonomy for the various psychological disorders. Physiological psychologists attempt to find order between brain structure and brain function. So a theory pertaining to chaotic phenomena appears to be at odds with the general philosophical perspective within the discipline of psychology. But in 1997, the *OED* added another definition to its list on chaos, one that provides a different viewpoint on this common word: “The behaviour of a system which is governed by deterministic laws but is so unpredictable as to appear random.” Even though this definition derives from the fields of mathematics, physics, chemistry, and biology, the idea that deterministic behavior is often unpredictable resonates with the complexity and often unpredictable nature of human mind and behavior. It is this idea that a small number of psychologists found favorable and attempted to use to reinterpret numerous psychological phenomena beginning in the middle 1980s.

Outline of Chaos Theory

Chaos theory is a group of experimental techniques, mathematical tools, and theoretical models pertaining to how dissipative, nonlinear dynamic systems produce this complex order as they change over time. Many scientists from fields such as physics, mathematics, chemistry, biology, ecology, and meteorology began developing chaos theory in the middle 1970s to understand chaotic behavior in the dynamic systems apparent in their respective fields. In order to begin to understand chaos theory's relationship to psychology it is first necessary to provide a brief outline of the theory. While there are numerous mathematical accounts of chaos theory (e.g., Arrowsmith & Place, 1990; Hasselblatt & Katok, 2003; Peitgen, Jürgens, & Saupe, 1992; Schuster, 1984), most of these accounts are directed towards scientists and mathematicians possessing knowledge of advanced mathematics. I present a conceptual account that I believe does not sacrifice the underlying principles of chaos theory.

Dynamic Systems

Simply, chaos or chaotic behavior is a property of *dynamic systems*. A dynamic system is a set of interrelated and interacting components that change over time. Dynamic systems can be composed of molecules, billiard balls, chemical compounds, electrical circuitry, mechanical parts, living tissue, and a host of other substances. Dynamic systems are ubiquitous in the laboratory and in nature; therefore, it is relatively easy to generate some examples. For instance, something as simple as a cup of hot coffee is a dynamic system. The molecules within the beverage interact with each other in complex ways while at the same time the coffee's "behavior" is constrained by the parameters of the coffee mug. As the coffee in the mug cools, the molecules on the top of the coffee slow and proceed to fall to the bottom of the mug. Conversely, the rapidly moving molecules at the bottom of the mug push their way to the top. This convection cycle continues until the temperature of the coffee reaches an equilibrium state equivalent to the temperature of the environment. A more complex example of a dynamic system is the process of human ontogenetic development. The process of human development from a fertilized egg, through puberty, and into adulthood constitutes a complex interaction of innumerable endogenous and exogenous variables. Genetic factors, socioeconomic status, life stressors, nutrition, exercise, and quality of education change throughout the ontogenetic process often in unexpected and surprising ways.

Conservative and Dissipative Dynamic Systems

There are two kinds of dynamic systems: *conservative* and *dissipative* (Schuster, 1984). Conservative dynamic systems do not lose energy over time, whereas dissipative dynamic systems do lose energy over time (Williams, 1997). A model of a "forced" pendulum is an example of a conservative dynamic system. Some kind of mechanical device pushes the bob of the pendulum, causing it to oscillate from one point to the other and, barring the influence of friction, the bob does not lose energy. The forced pendulum model is, of course, an idealized dynamic system because it ignores the effects of friction. In fact, there are very few truly conservative systems in the real world. A "dampened" pendulum—one that loses energy as a result of friction—is an example of a dissipative dynamic system. As the bob oscillates, it slowly loses or dissipates energy until it comes to a complete stop (see Figure 2). Some conservative and all dissipative dynamic systems are *nonlinear* as opposed to linear dynamic systems.

Linear and Nonlinear Systems

Linear dynamic systems are any systems that map onto a straight line using the formula $y = mx + b$. For instance, a plot of the relationship between two

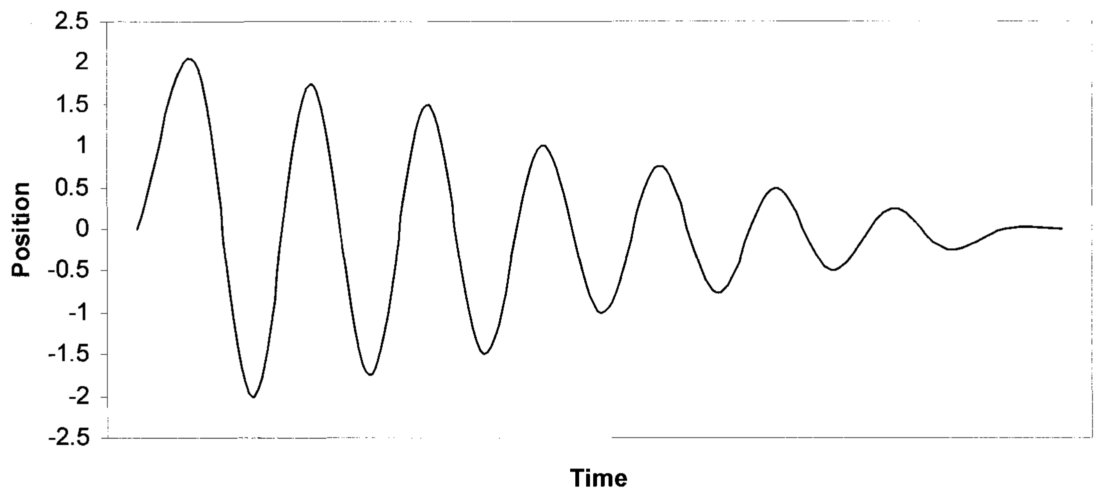

Figure 2. Hypothetical time series for a “dampened” pendulum.

idealized time series variables such as years in a company and earnings is a linear dynamic system. This system is dynamic because employee earnings increase as a function of the number of years of employment. Linear dynamic systems, consisting of two or more variables, are often modeled using solvable difference or differential equations that describe the behavior of the entire system.[37] Difference and differential equations may describe the behavior of nonlinear dynamic systems as well, but these systems do not plot as a straight line and sometimes are not solvable. A simple equation—though not a difference or differential equation—for demonstrating a nonlinear dynamic system is Equation 1:

$$x_{t+1} = 1.9 - x_t^2 \tag{1}$$

In this equation, x_t is an arbitrary value at time 1 and x_{t+1} reiterates into the equation as a new value for x at time 2. The "forced" pendulum example is a relatively simple example of a conservative, nonlinear dynamic system. Plotting the position of the forced pendulum as a function of time produces a line of consistent peaks and valleys (see Figure 3). Barring any external perturbations, one can perfectly predict the position or the state of the pendulum at any given time. Other dynamic systems prevent perfect prediction due to their inherent

[37] Difference equations are used to model variables that are discrete (i.e., restricted to integer values such as time in years), while differential equations are used to model variables that are continuous (i.e., values can take on any number such as the case of the swinging pendulum).

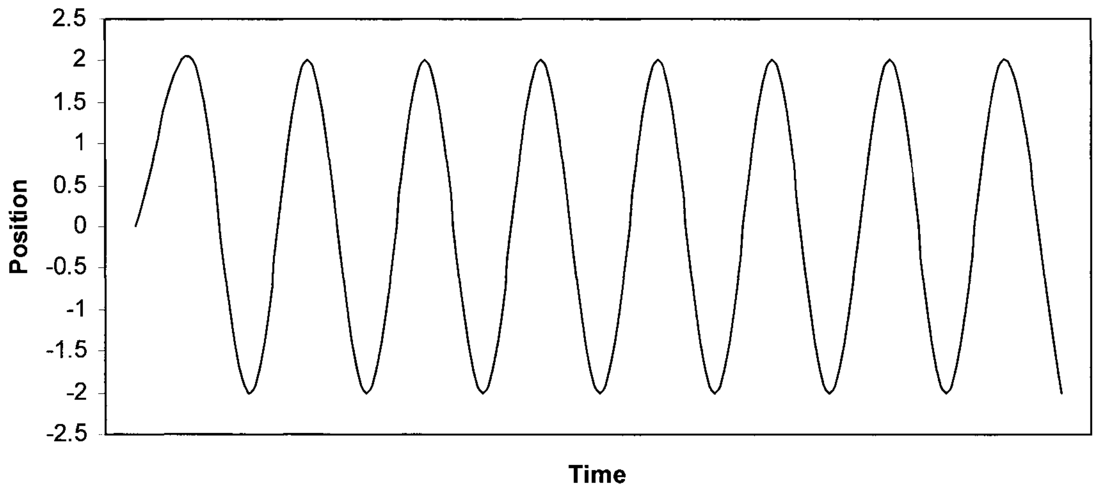

Figure 3. Hypothetical time series of a “forced” pendulum.

complexity and these are usually dissipative, nonlinear dynamic systems. A common cause of this complexity is the number of variables in the system. This is the case with human ontogenesis. The complexity arises from the number of variables and their interactions, making predictions about the behavior of the system difficult if not impossible. For instance, given an infant's birth weight and an almost infinite supply of data about the infant's current state, it would be exceedingly difficult to predict perfectly the height or weight of that infant at age 1 or 2 and even more unlikely when the infant was 21 years old.

Some, but not all, nonlinear systems exhibit chaotic behavior, that is, seemingly random behavior that, depending on the mode of analysis, reveals a hidden complex order. The phenomenon of chaotic behavior is only evident in dissipative, nonlinear dynamic systems, so the remainder of my discussion focuses exclusively on these types of systems.

Chaotic Nonlinear Systems

Dissipative, nonlinear dynamic systems are common in the real world. Outside of scientists' laboratories, it is the case that most systems interact with their environments in complex and often unpredictable ways, which leads to chaotic behavior. These systems exist at all levels of representation, from the molecular level to the planetary level. Chaotic systems exhibit a behavior called *sensitive dependence on initial conditions*, also known as the "butterfly effect" (Lorenz, 1963).[38] This behavior indicates that a system is sensitive to tiny

[38] A common example to illustrate the butterfly effect is the idea that a butterfly flapping its wings in Texas, in principle, could cause a tornado in Japan.

perturbations that could, but do not always, significantly affect the behavior of that system. Because it is difficult to measure the initial states of a system with absolute certainty, sensitive dependence makes perfect prediction exceedingly difficult. Take, for instance, two drops of water that are extremely close together at the top of a waterfall. Even though physicists may know the precise equations of motion for fluids, and the effects of gravity and friction for these droplets, predicting where they will land at the bottom of a waterfall with any degree of precision is virtually impossible. One seemingly irrelevant change in the position of one of the droplets results in changing the measurement in position of the droplet. This change radically affects the outcome of where that particular water droplet will land. In fact, even if the droplets began infinitely close to each other, the effects of innumerable variables would cause a radical divergence in their positions at the bottom of the waterfall. Physicists and engineers have to deal with sensitive dependence on initial conditions as they attempt to explain phenomena such as turbulence in fluids and aerodynamics.

As a chaotic system changes over time, it reaches *bifurcation* points that indicate potential qualitative changes in the system's behavior (see Figure 4). Through a series of bifurcations, a chaotic system settles into a particular configuration of behavior known as an *attractor*. Attractors come in three categories: fixed point, limit cycle, and strange (Abraham & Shaw, 1983). Fixed point attractors characterize behaviors that have reached equilibrium. For instance, when dropping a tiny iron ball down a funnel, the ball will roll around the

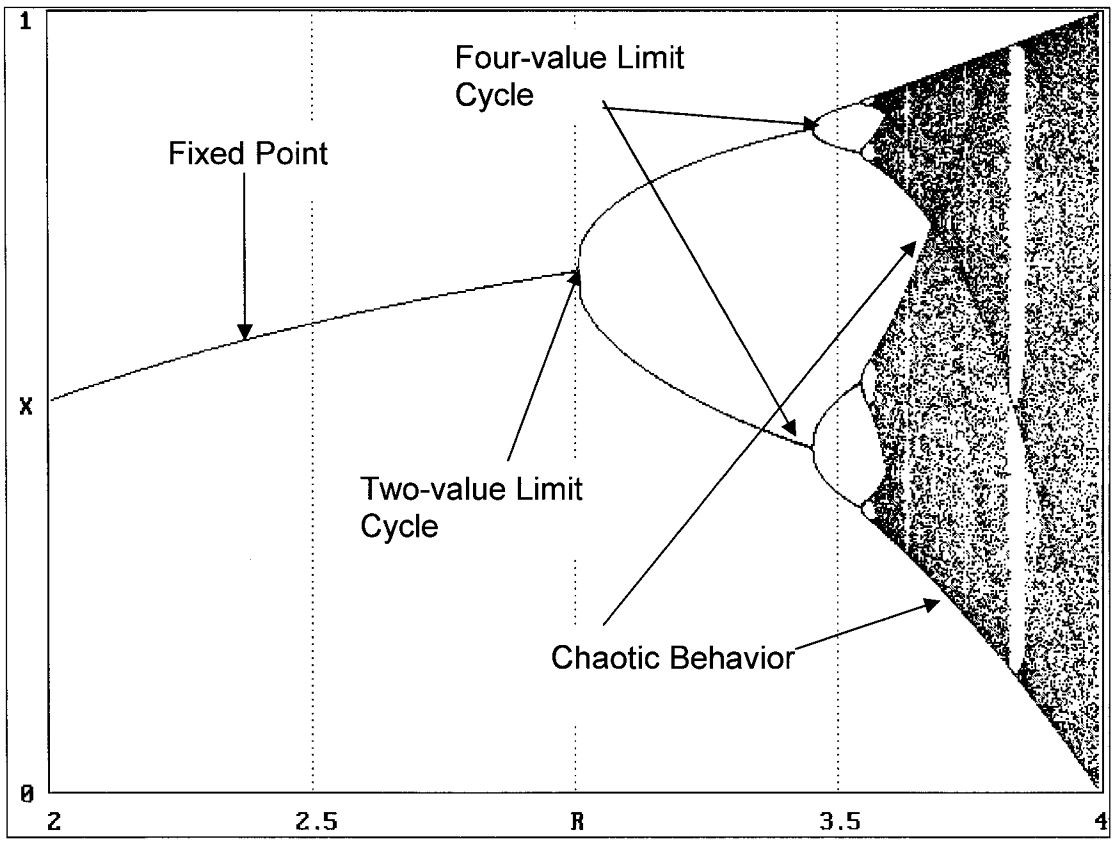

Figure 4. Bifurcation diagram illustrating the potential qualitative shifts in a given system using every parameter for a reiterative equation. For parameter values R = 2 through 3, the system exhibits a series of fixed point attractors. At R = 3, the system oscillates between two potential states. Before R = 3.5 the system bifurcates again and oscillates between 4 potential states. After R = 3.5 the system becomes chaotic and never repeats the same value twice. (Adapted from a bifurcation diagram courtesy of Clint Sprott, University of Wisconsin-Madison).

funnel until it reaches its fixed point attractor—the resting state at the bottom of the funnel. Limit cycle attractors occur when a system oscillates between two or more states. This behavior is evident in the previous example of a forced pendulum where the bob of the pendulum comes to rest briefly in one state, then, after the pendulum is forced, moves to its second resting state. Strange attractors describe a system's behavior when it settles into a particular behavioral configuration that does not change beyond certain parameters. Within these parameters, however, the system never repeats the same behavior twice. While the system's behavior may appear random, the system is actually demonstrating a complex order. This is one of the behaviors most characteristic of chaotic systems and can be found in systems such as weather patterns (Lorenz, 1963), solid mechanics (Moon, 1991), circuit boards (Lesurf, 1991), epilepsy (Babloyantz & Destexhe, 1986), and heart beat arrhythmias (Glass, Goldberger, & Courtemanche, 1987).

One way of identifying the type of attractor a system exhibits is through an analysis of its *phase space* portrait. Phase space is a multidimensional graphical representation of a system's behavior over time. The dimensions most often illustrated in phase space portraits are the X and Y coordinates and time. Phase space portraits work similarly to a traditional Cartesian grid in that a particular location in the portrait defines the state of a system. Instead of points in Cartesian space, phase space portraits outline the trajectories of systems. For a system exhibiting a fixed point attractor like a "dampened" pendulum or an iron ball rolling around in a funnel (see Figure 5), the phase space

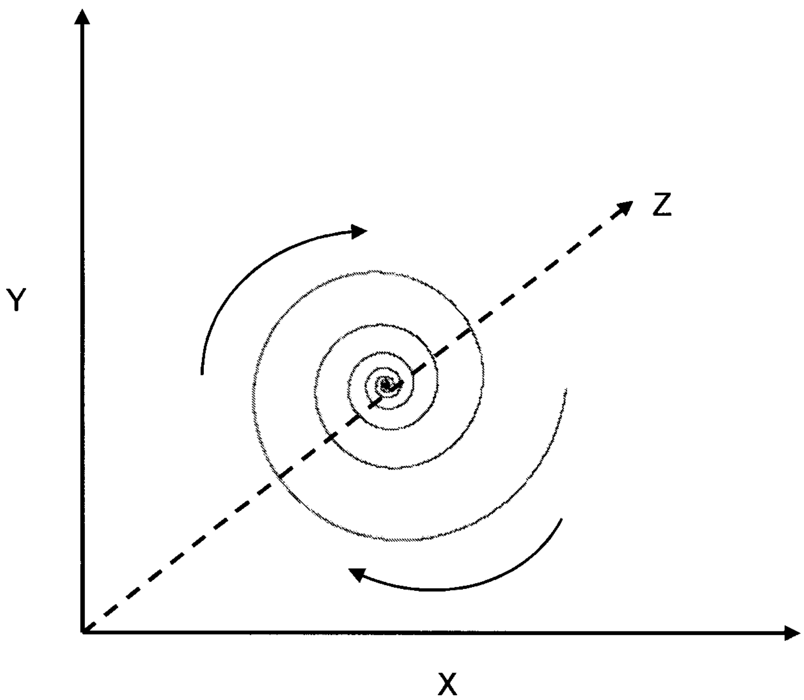

Figure 5. Phase space portrait of a fixed point attractor. The curved arrows indicate trajectory direction. This image corresponds to the time series of the "dampened" pendulum in Figure 1.

trajectory would look like an ever decreasing spiral that would signify the loss of energy of the pendulum over time. A phase space portrait of a limit cycle attractor such as a forced pendulum would map out to a circle or a series of circles to indicate the perpetual and repetitive behavior of the pendulum (see Figure 6). One half of the circle signifies the pendulum's trajectory from left to right; the other half of the circle signifies the pendulum's trajectory from right to left. Strange attractors can take on many different forms in phase space. As I mentioned earlier, strange attractors show a complex order. One can tell the difference between random behavior and this kind of attractor in that random behavior fills the entire phase space portrait whereas strange attractors fill only specific portions of phase space. Perhaps the most common strange attractor shape is the Lorenz attractor (see Figure 7).

One of the characteristics of strange attractors is that they exhibit *fractal* properties (Mandelbrot, 1967). Fractals are geometric shapes whose spatial dimensions are fractions (see Figure 8). Instead of being one-dimensional or two-dimensional, fractals have dimensions of, say, 2.3 or 1.5 (Goerner, 1994). Fractals display self-similarity, meaning a fractal's geometrical structure at all scales of measurement will be similar. The Koch snowflake in Figure 8 demonstrates this self-similar scaling property. Irrespective of the scale, the same shape will be evident: a triangle.

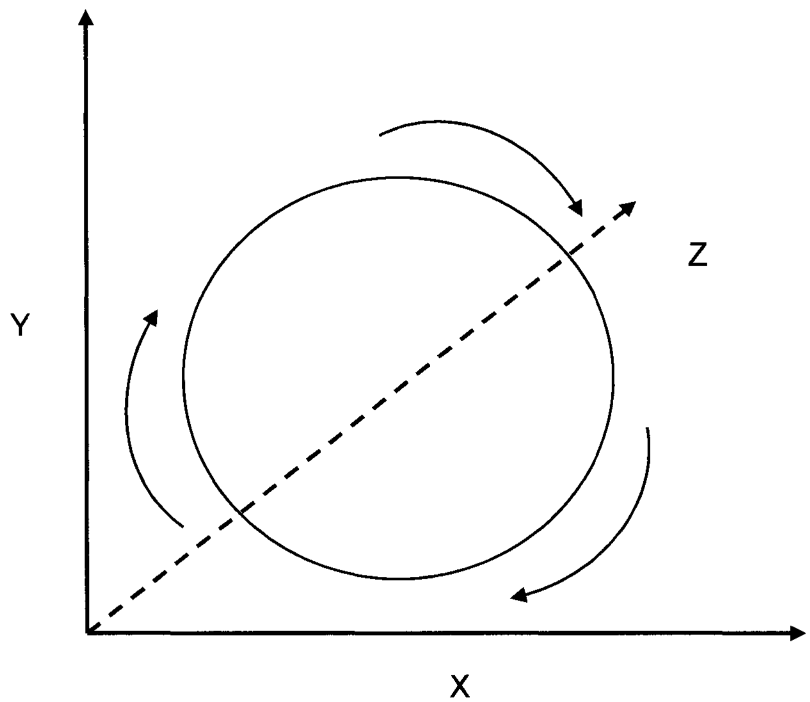

Figure 6. Phase space portrait of a limit cycle attractor. The curved arrows indicate trajectory direction. This image corresponds to the time series of the “forced” pendulum in Figure 2.

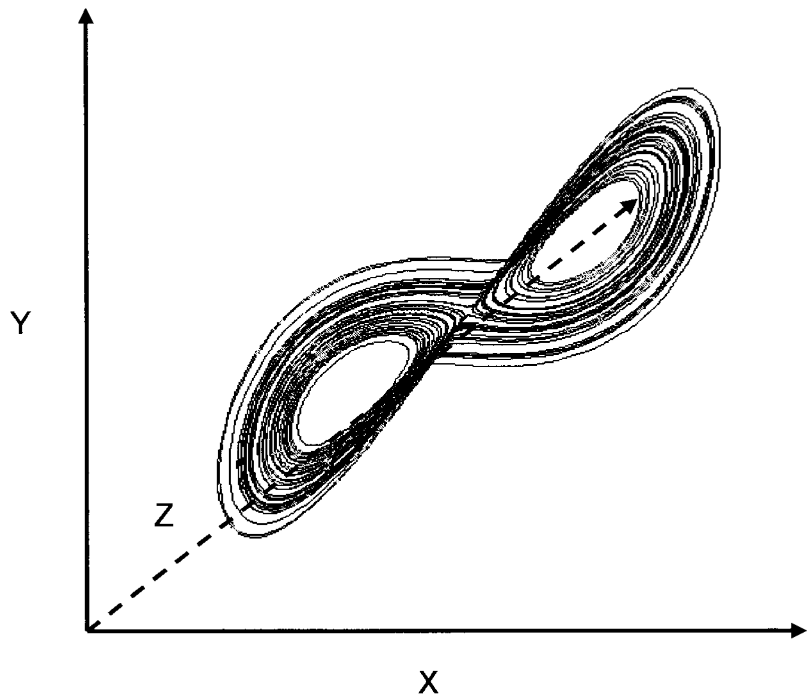

Figure 7. Phase space portrait of a strange attractor. This figure represents the Lorenz attractor. Note that even though its trajectory is unusual compared with the trajectories of the fixed point and limit cycle attractors in Figures 4 and 5, it only fills up a certain region of phase space. (Image of Lorenz attractor courtesy of Clint Sprott, University of Wisconsin-Madison).

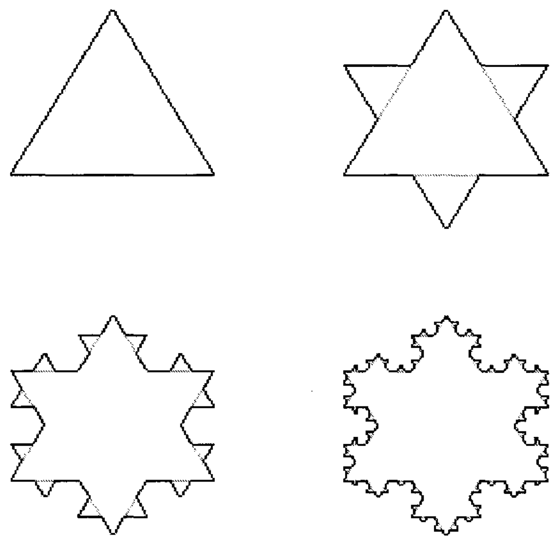

Figure 8. The Koch snowflake. One creates the Koch snowflake by taking the original triangle, reducing its size by one-third, and then placing this reduced copy in the middle third of each side of the original triangle, a process called affine transformations. In the image above, this iterative process occurs three times. (Image courtesy of Clint Sprott, University of Wisconsin-Madison).

The Koch snowflake is an idealized fractal, but there are abundant examples of objects that have fractal properties in nature such as leaves, flowers, broccoli, earthquake features, cracks in surfaces like sidewalks, the respiratory system, and the nervous system (Peitgen et al., 1992; Williams, 1997). If a strange attractor demonstrates fractal properties, its self-similarity suggests an ordered pattern and is evidence of chaotic behavior.

Defining Chaos

There are a number of problems with encapsulating chaos theory into an easily understood form. As I discussed at the beginning of the chapter, the term "chaos" is somewhat misleading because it implies random or stochastic behavior even though chaos theorists search for order in the behavior of systems within their respective disciplines. Another problem with chaos theory is that scientists use it to describe chaotic behavior *and* to describe nonlinear dynamic behavior in general. This leads to ambiguity because not all nonlinear systems display chaotic behavior. The term is also problematic because chaos theory is resistant to any definitive definition. Confusion arises over chaos theory's definition due to its interdisciplinary scope. Mathematicians want to define it one way and physicists another way. The mathematician Ian Stewart (1989) discussed the difficulty experienced by the Royal Society of London to define chaos theory during a meeting in 1987: "Although everybody present knew what they thought 'chaos' meant—it was their research field, so they really ought to have known—few were willing to offer a precise definition" (p. 17).[39] Chaos theory is also an umbrella term for a number of interrelated theories and concepts that do not sufficiently capture its precise meaning. Other names commonly, and sometimes inappropriately, associated with chaos theory include complexity theory, self-organization, self-organized criticality, fractal geometry, ergodic theory, the edge-of-chaos, and catastrophe theory. Catastrophe theory, for instance, is a method for modeling abrupt qualitative shifts—bifurcations—in a system's behavior as a result of small changes in a system's variables (Brown, 1995). While dissipative, nonlinear systems exhibit this kind of behavior, it is only one of many characteristics of chaos and therefore it is inappropriate to use catastrophe theory and chaos theory as equivalent terms.

Despite these difficulties, there are some scientists, mathematicians, and philosophers who have proffered definitions of chaos theory that inspired the *OED* definition I provided earlier. In his critical treatise on chaos theory, *In The Wake of Chaos*, the philosopher of science Stephen Kellert (1993) defined chaos theory as "the qualitative study on unstable aperiodic behavior in deterministic nonlinear dynamical systems" (p.2). The geologists Garnett Williams (1997) offered two related definitions: a) the study of "sustained and random-like long-term evolution that satisfies certain special mathematical criteria and that happens in deterministic, nonlinear, dynamical systems" and b) the study of "largely unpredictable long-term evolution occurring in a deterministic, nonlinear

[39] Stewart dates the meeting as 1986 but according to the *Proceedings of the Royal Society of London*, the meeting took place between February 4 and 5, 1987 with discussion organized by M. V. Berry, I. C. Percival, and N. O. Weiss.

dynamical system because of sensitivity to initial conditions" (p. 449). According to Stewart (1989) the Royal Society succinctly defined it as "stochastic behavior in a deterministic system" (p. 17). Considering the similarity of these definitions, it appears there is some general agreement on definitions. Nevertheless, the ambiguities I outlined above pertaining to the exact nature of this interdisciplinary theory were problematic throughout the 1970s, 1980s, and 1990s. In the next section, I provide a brief historical sketch of chaos theory and its uses.

History of Chaos Theory

The nineteenth century French mathematician Henri Poincaré was perhaps the first individual to identify and attempt to fully describe chaotic behavior mathematically (Barrow-Green, 1997; Diacu & Holmes, 1996). Poincaré was responding to a nationwide request of scientists made by King Oscar II of Sweden to discern if the solar system was stable. To do this, Poincaré studied the three-body problem of planetary motion, which deals hypothetically with two large stationary bodies and one smaller body revolving around the two larger bodies. While Poincaré was unsuccessful in his attempts to establish if the solar system was stable, he identified the complexity inherent in trying to describe the behavior of a system with more than two variables (bodies). Poincaré discovered that miniscule errors in measurement of the smaller bodies' initial states made prediction of its future state problematic—that is, sensitive dependence on initial conditions. As the system evolves over time, the trajectory of the smaller body becomes complex and difficult to map, a trajectory he appropriately called a homoclinic tangle.

Poincaré's work, however, went largely unnoticed by the scientific and mathematical communities. The trajectory of homoclinic tangles was simply too complicated to work out by hand for any reasonable length of time. It was not until the late 1960s and early 1970s that many scientists from a diverse array of fields came to work on similar problems in mathematics and the physical sciences (e.g., Lorenz, 1963; Mandelbrot, 1967; May, 1974, 1976). The availability of computers assisted greatly in resurrecting Poincaré's work on homoclinic tangles. Computers could perform the needed calculations quickly and provided visual displays of a system's complex dynamics, which helped scientists and mathematicians visualize how dynamic systems like the three-body problem change over time. The MIT meteorologist Edward Lorenz (1963), for example, modeled weather patterns using differential equations on his computer and inadvertently rediscovered the aforementioned sensitive dependence on initial conditions. He found that seemingly minute rounding errors in his computer programs of simulated weather produced radically different patterns of weather in the same manner as Poincaré's three-body problem.

Most chaos theorists generally agree that the word "chaos" only became a popular term associated with this kind of phenomena after two papers named this kind of behavior in systems "chaos." Robert May (1974), a population biologist from Princeton University, published a paper on simple predator-prey models of wildlife population growth. May's work indicated that, depending on parameter and initial state values, simple equations that model predatory-prey interactions can exhibit stable population values (fixed points), populations that cycle between certain values (limit cycles), and chaotic fluctuations of population values. Mathematician Tien-Yien Li and physicist James Yorke's (1975) paper "Period Three Implies Chaos" in the *American Mathematical Monthly* discussed similar chaotic behavior using simple equations in a more mathematical style.[40] The take home message of both papers was that simple equations that represent processes in nature simulate chaotic behavior. If these equations accurately represent processes in nature, then it should not be difficult to find real world examples of chaotic phenomena in nature. These papers enjoyed a wide readership among scientists from a number of disciplines and the term "chaos" caught on as a buzzword for this type of phenomena.

Accompanying the new research developments in chaos during the 1960s and 1970s was the emergence of professional meetings on the subject. For instance, in 1977 the New York Academy of Sciences recognized chaos theory's impact on the scientific community and sponsored a conference on chaotic behavior hosted by mathematician Okan Gurel and physiologist Otto Rössler (1979). There were only a few dozen participants at the conference, but they included the mathematician Benoit Mandelbrot (see, 1967), who developed the idea of fractal geometry, and the aforementioned Robert May and James Yorke. The New York Academy of Sciences held a second conference on chaos theory two years later. Unlike the first conference, which was relatively small, there were hundreds of professionals and graduate students flocking to this one (Gleick,

[40] An often overlooked citation from that same year was F. Henin and Ilya Prigogine(1974) paper entitled "Entropy, dynamics, and molecular chaos" in the *Proceedings of the National Academy of Sciences*.

1987). Chaos theory had caught on as a new way of thinking about various phenomena in the physical world and scientists were beginning to clamor for a piece of this new research area. Conferences on chaos theory continued throughout the 1980s and the topics of presentation became more diverse. For instance, the Royal Society of London's conference in 1987 included papers presented by Robert May and colleagues (1987) on chaos in biological populations, physiologist Leon Glass and colleagues (1987) on chaotic behavior in heartbeats, and physicist Jack Wisdom (1987) on chaos in the solar system. Chaos theory was gaining recognition as a legitimate topic of research even if the formal definitions about what it was and how to study it were at best ambiguous. Chaos theory was moving from the fringe areas of science to the mainstream of numerous disciplines.

The late 1970s and early 1980s presented graduate students with opportunities to explore phenomena now grouped together under the name "chaos." For example, Doyne Farmer, Robert Shaw, and James Crutchfield, graduate students from the University of California, Santa Cruz began investigating chaotic behavior in dripping faucets (Crutchfield, Farmer, Packard, & Shaw, 1986; Shaw, 1984). Graduate student interest prompted universities to begin offering classes in nonlinear dynamics, fractal geometry, and chaos theory. Similarly, scientists started creating institutes for the study of nonlinear and chaotic behavior. The University of Maryland founded the interdisciplinary Chaos Group in the middle 1970s. Cornell University started its Chaotic Dynamics Laboratory in the late 1970s. In 1980, Los Alamos opened the Center for

Nonlinear Studies. Others followed suit such as the Santa Fe Institute in 1984 and the Center for Nonlinear Studies at the University of North Texas in 1994.

With the advent of conferences and places to study, there emerged content-specific journals and textbooks pertaining to chaos theory. The journal *Chaos*, from the American Institute of Physics, began publication in 1991. David Campbell, a physicist and editor in chief of *Chaos,* was also the director of the Center for Nonlinear Studies at the Los Alamos National Laboratory. Technical manuals such as Ralph Abraham and Chris Shaw's (1983) *Dynamics: The Geometry of Behavior*, Robert Devaney's (1986) *An Introduction to Chaotic Dynamical Systems*, and Benoit Mandelbrot's (1982) *The Fractal Geometry of Nature* became popular among scientists and students trying to understand the ideas and methods of chaos theory.

It was during the middle 1980s that psychologists began seeing the possibility of using chaos theory as a way of understanding psychological phenomena. Chaos theory found its way into psychology via physiological and neuronal investigations in addition to popularized accounts of chaos. Physiologists began investigating phenomena such as heart rate variability (Goldberger, Bhargava, & West, 1985), physiological control systems (Glass & Mackay, 1979; Mackey & Glass, 1977), neurophysiology (Mandell, 1984), and brain activity (Babloyantz, Nicolis, & Salazar, 1985). The physiologists analyzed physiological time series data obtained from electrocardiogram and electroencephalogram output. Researchers in neural networks developed nonlinear mathematical models of neural processes (Carpenter & Grossberg, 1987). It was only a small leap from studying internal processes such as brain activity and neural networks to other psychological phenomena.

The 1980s and 1990s also saw the introduction of chaos theory to the lay public in the form of popularized books (e.g., Briggs & Peat, 1989; Carpenter & Grossberg, 1987; Coveney & Highfield, 1995; Gleick, 1987; Jantsch, 1980; Prigogine & Stengers, 1984; Stewart, 1989). These popularized accounts attempted to bring this esoteric subject to a more general audience. The focus of many of these accounts was on the scientists studying chaos rather than on the mathematics of chaos theory. The authors of these accounts dramatically characterized the individuals studying chaos theory as misunderstood geniuses, loners, and rebels that were fighting against the theoretical and philosophical dogma inherent in traditional disciplines. For instance in James Gleick's (1987) bestseller, *Chaos: Making a New Science*, he describes Mitchell Feigenbaum, a scientist working in the theoretical division of Los Alamos, this way:

> His hair was a ragged mane, sweeping back from his wide brow in the style of busts of German composers. His eyes were sudden and passionate. When he spoke, always rapidly, he tended to drop articles and pronouns in a vaguely European way, even though he was a native of Brooklyn. When he worked, he worked obsessively. When he could not work, he walked and thought, day or night, and night was best of all. The twenty-four hour day seemed too constraining. (p. 2)

These simplified versions were appealing to the general public as well as to psychologists. Many psychologists simply lacked the mathematical training needed to understand the underpinnings of chaos theory and these accounts gave them the illusion of understanding.

Accompanying the glamorized portrayals, these authors hailed chaos theory as a new Kuhnian paradigm shift for the sciences that would overthrow the putative dominant, reductionistic Newtonian worldview and replace it with a theory more conducive to studying holism, change, and complexity. In one of these accounts, Fritjof Capra (1996), theoretical physicist, stated, "We are now at the beginning of...a fundamental change of worldviews in science and society, a change of paradigms as radical as the Copernican revolution" (p. 4). Ralph Abraham, a mathematician from the University of California, Santa Cruz, stated that chaos theory was "*the* paradigm shift of paradigm shifts" (from Gleick, 1987). Elsewhere, Abraham (1994) called the fervor for this new perspective the "chaos revolution" because it had influenced virtually all the physical sciences. In 1989, the journal *Science* devoted a six part series to exploring chaos theory's uses in physiology, epidemiology, meteorology, quantum physics, and population biology. The sixth installment in the series discussed the possibility of chaos theory being a new paradigm in the sciences. In the discussion, the physicist Joseph Ford argued that science was "in the beginning of a major revolution...The whole way we see nature will be changed" (in Pool, 1989, p. 26).[41] In other media, the television show NOVA featured an episode entitled "The Strange New Science of Chaos" that was geared towards a general audience and pronounced chaos

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[41] Others were more skeptical of the merits of chaos theory as a new paradigm. The philosopher of science Steven Toulmin contended that while chaos theory brings into question the predictability of the world, it has not opened up a new level of behavior for investigation as had quantum mechanics and relativity (Pool, 1989).

theory as a revolution in the sciences (Taylor, 1989). Exemplifying the general spread of interest in chaos theory is the best selling novel and subsequent movie *Jurassic Park* in which one of the protagonists was a chaos theorist (Crichton, 1990). The take-home message of these books appealed to the public and psychologists' "folk knowledge" of the way the world worked and the philosophical assumptions scientists used in their research. Essentially, the take-home message was this: (1) nature is messy, complex, and often unpredictable, (2) nature is unpredictable because it exhibits nonlinear or chaotic behavior, (3) systems that display chaotic behavior are constantly evolving and are healthy, whereas systems that reach a steady state cease to evolve and perish, (4) the reductionism of the Newtonian perspective is inadequate to explain many of these "real world" processes, (5) what scientists in a laboratory can demonstrate after controlling all extraneous variables is nothing like the complex behavior one sees in nature.

With scientists from closely related disciplines such as physiology beginning to investigate chaos theory, the glut of popular books on the subject, and the rhetoric of a new paradigm coming from individuals in the "hard" sciences, some psychologists began to conduct research and write theoretical papers on chaos theory's possible uses for understanding human mind and behavior. The reference sections from many works pertaining to chaos theory in psychology support my claim that the popular books influenced these psychologists (e.g., Bütz, 1992; e.g., Sally Goerner, 1995; Krippner, 1994; Mosca, 1995; Parry, 1996). In the next section, I describe the kinds of practices psychologists incorporated into the discipline that were part of chaos theory.

Conceptual Practices

Chaos theory provided a wealth of concepts, theories, and methods on which psychologists needed to conduct boundary-work between the 1980s and 2000. Terms such as "fractals," "bifurcations," "the butterfly effect," "phase space," "self-organization," "auto-catalysis," and "strange attractors" began to appear in psychology's literature in the late 1980s and early 1990s. Research oriented and applied psychologists often used these terms as metaphors for describing psychological phenomena. The biopsychologist Frederick Abraham (1989), for instance, used the concept of attractors to develop a hypothetical model of Jung's concept of the psyche. Following Jung's idea that the human psyche oscillates, Abraham's model outlined the various states of the psyche as different kinds of attractors. According to Abraham's model, as the psyche focuses on various internal processes (e.g., two conflicting thoughts) and external stimuli (e.g., daily stressors), these forces produce bifurcations in the psyche that propel it between various attractor states.

Some clinical psychologists and family therapists began viewing humans in general and the therapeutic relationship between client and therapist as nonlinear dynamic systems (e.g., Chubb, 1990; Gottman, 1991; Lonie, 1991; Stevens, 1991). Clinical psychologist Michael Bütz (1993), for instance, claimed that psychotherapeutic interventions perturb the client *qua* a nonlinear system and it is the therapist's responsibility to understand that this destabilization is a necessary process for client change. Of this process Bütz stated

> Based on a client's history the therapist might select a particular time to make an interpretive intervention to bring the client's chaotic state towards greater complexity, and yet, at the same time to create new order. On the other hand, some interpretations may be used to destabilize previously held rigid beliefs that are no longer adaptive to move a client towards a more chaotic state. (p. 549)

Bütz argued that either of these options promotes "adaptive growth" in the client, which is the goal of effective treatment in therapy. Family therapist Margaret Ward (1995) contended that the family be seen as a nonlinear system that exhibits chaotic behavior. She argued that the tenets of chaos theory are a more effective way of viewing the family and its interactions than previous models of the family. She argued further that the possibility exists that therapists could begin to mathematically model family interactions and develop computer simulations of family dynamics in much the same way scientists from other disciplines were modeling their respective phenomena.

In line with these metaphorical uses, it also became fashionable to propose various psychological phenomena as self-organizing systems—an inherent property of any chaotic system in which it continually becomes more complex via interactions with the environment. Michael Schwalbe (1991) proposed the concept of the "self" as one such self-organizing system within humans. He proposed, "By considering the human organism as a...dynamical system with inherent capacities for self-organization, we can expand our vision and reconnect the body and the natural world to the emergence of self and mind" (p. 278). In a related vein, developmental psychologist Marc Lewis (1995) argued for a self-organizing process in the relationship between cognition and emotion feedback processes. Lewis believed that a central process in personality and behavioral development is the complex interaction between stability and chaos in the cognition and emotion system. Clinical psychologist Frank Masterpasqua (1997) argued that human development is a process of self-organization. This process keeps humans changing and growing as they develop. It is through this self-organizational process that people stay healthy. People who reach a state of equilibrium begin to experience problems such as psychopathology during their development because they stop self-organizing. According to Masterpasqua, a therapist's duty is to recognize this process towards psychopathology, distress, and disease and help clients move toward "far-from equilibrium" states that will foster the self-organizing process.

Other conceptual practices that psychologists borrowed from chaos theorists were their methods of analysis. These included methods such as attractor reconstruction, nonlinear time series analysis, and catastrophe modeling, in addition to measures of chaos such as Lyapunov exponents and Kolmogorov-Sinai entropy. Psychologists found two primary uses of chaos theory's methods. First, psychologists' used differential equations to build mathematical models of psychological phenomena. For instance, Stephen Guastello (1981) created a catastrophe model for equity in the work place in an attempt to outline various types of employee behavior—innovation, motivated performance, absenteeism, turnover, strike, riot—and at which point between employee costs and rewards would there be qualitative shifts from one behavior to the next. Frederick Abraham and colleagues (1990) developed a dynamical model of John Dollard and Neal Miller's approach-avoidance conflict scheme, which indicated that at certain parameters approach-avoidance behavior will exhibit fixed, limit cycle, and chaotic attractors. Scott Clair (1998) demonstrated that his catastrophe theory model of situational pressures was a better predictor of adolescent alcohol use than three traditional linear models.

The second use, and by far the more common one, was that psychologists used the methods of chaos theory to examine actual data to see if there was any evidence of chaotic behavior. Most data psychologists generated that lent itself to this kind of analysis were time series data. According to psychologist Keith Clayton (1997, July) these new approaches from chaos theory served three purposes in the analysis of time series data: (a) distinguishing chaotic from random data, (b) discerning if the data are from a deterministic system, and (c) assessing the dimensionality of the data. I present a few of the many studies pertaining to the search for chaotic behavior in psychological phenomena. For instance, Allen Neuringer and Cheryl Voss (1993) analyzed time series data of participants attempting to anticipate the next number in the logistics equation—an equation that produces chaotic output. The time series data indicated that subject responses closely resembled chaotic behavior. Therapist Gary Burlingame and colleagues (1995) analyzed times series data of client-therapist interactions using the Hill Interaction Matrix (1973) during time-limited psychotherapy. They discovered that the time series data contained a fractal dimension indicating a complex, nonlinear order.

Material Practices

The material practices of chaos theorists that prompted psychologists to engage in boundary-work were brought about largely by the increasing availability and pedagogical utility of computing technology during the 1980s. Computers were essential for carrying out the complex calculations necessary for detecting and analyzing a system's chaotic behavior. Not only were computers essential for carrying out the calculations, but advances in software development were making computer graphics more visually appealing. There were a number of software packages offering scientists the opportunity to view chaotic behavior visually including Mathematica, Dynamical Software (Schaffer, Truty, & Fuimer, 1988), and Chaos Data Analyzer (Sprott & Rowlands, 1992). These software packages afforded psychologists who lacked the mathematical background a way to comprehend chaos theory. The understanding came through graphically displayed output, simulations of chaotic behavior, and through the analysis components of the software. These programs allowed psychologists a way to begin exploring possible chaotic behavior in psychological phenomena.

Social Practices

In the late 1980s, individuals interested in the relationship between chaos theory and the social sciences began publishing newsletters such as *The Dynamics Newsletter* and *The Social Dynamicist*. At the time, most scientists interested in chaos theory worked in relative isolation, as was the case with the graduate students from Santa Cruz and Mitchell Feigenbaum at Los Alamos. It was a new field, especially to social sciences like psychology, and the published material on chaos could only be found in journals with which most social scientists were unfamiliar. These newsletters assisted in alleviating this problem. The newsletters helped to facilitate shared communication networks by providing psychologists with conference announcements, brief articles, software reviews, and contact information from a number of scientific disciplines including physics, chemistry, mathematics, and biology.

It was also during this time that conferences related to chaos theory and psychology began to appear. In 1987 Walter Freeman, Alan Garfinkel, and Otto Roessler presented a workshop on the implications of chaos theory for the study of cognitive processes at the International Conference on the Dynamics of Sensory and Cognitive Processing by the Brain in West Berlin (Freeman, 1987). In 1988, the Beth Israel Medical Center hosted a conference on mathematical models, many of which were models derived from chaos theory, for psychoanalysis and psychotherapy (Abraham, 1988). Other conferences also being held during this time included Fractals and Chaos in the Natural and Social Sciences held in Miami, Ohio in June of 1989 (Goerner, 1989b) and Chaos and its Implications for Psychology held in the same year at the Saybrook Institute in San Francisco (Goerner, 1989a).

Places to study chaos theory in psychology began to appear as well. For instance, in 1985, Scott Kelso, a psychologist at Florida Atlantic University, and

Herman Kaken, a theoretical physicist at Stuttgart, established the Center for Complex Systems at Kelso's university (Kelso, 1995). The Center offered training in complex systems brain research for undergraduates, graduate students, and postdoctoral fellows coming from the natural and behavioral sciences (Kelso, 1989). Funding for the Center came from the National Institute of Health, the Office of Naval Research, and the National Science Foundation (Kelso, 1995).

In 1991, psychologists and other scientists formed the Society for Chaos Theory in Psychology and the Life Science (SCTPLS) in San Francisco (Guastello, 1997). The Society aimed to bring together "researchers, theoreticians, and practitioners interested in applying dynamical systems theory, far-from equilibrium thermodynamics, self-organization, neural nets, fractals, cellular automata, and related forms of chaos, catastrophes, and bifurcations, nonlinear dynamics, and complexity theories to psychology and the life sciences" (p. 301). Due, in part, to the multiple areas of SCTPLS, members were an interdisciplinary group comprised largely of psychologists, neuroscientists, computer scientists, mathematicians, and educators. In 1997, the Society developed a quarterly journal, *Nonlinear Dynamics, Psychology, and Life Sciences*. As the title indicates, the journal sought to publish peer-reviewed papers pertaining to psychology *and* other sciences.

The rhetoric of a new paradigm was a common strategy for legitimizing the use of chaos theory in psychology. Bolstering these claims was the rhetoric about chaos as a new paradigm in the physical sciences. In his inaugural address of the first SCTPLS conference Fredrick Abraham (1995), the Society's second president, boldly pronounced: "Within five years, the dynamical vision [chaos theory] will be the hegemonical view in psychology" (p. xxvii). Psychotherapist Sally Goerner (1995) described chaos theory as a "revolution" that would have profound implications for comprehending psychological phenomena. Therapist and consciousness researcher Allan Combs (1995) proclaimed that "Chaos theory is not simply a set of novel procedures that can be imported into the existing establishment of scientific psychology; it in fact represents a fundamental *revolution* [italics added] in viewpoint" (p. 129). The psychiatrist Isla Lonie (1995) argued, "Chaos theory will be able to provide a paradigm that can permit a greater understanding of [psychodynamic processes] especially in that it provides a basis for demanding recognition that Newtonian science is unequal to the task" (p. 293). Similar to my other case studies, psychologists using chaos theory felt it would be the next "revolution" in both the applied and scientific realms of psychology. Given such strong claims, it is important to describe psychologists' reactions to such claims.

Resistances to Chaos Theory

Surprisingly, psychologists did not engage in much boundary-work with the introduction of chaos theory into the discipline. There were only a small number of psychologists interested in chaos theory and the work they did seemed to be of little importance and relevance to most psychologists' research. Despite the unorthodox nature of the theory, psychologists using it neither caused as much political controversy as the evolutionary psychologists nor threatened an established theory such as behaviorism like the cognitive scientists. Psychologists using chaos theory's models and methods were on the fringes of the discipline and apparently merited little attention, despite their claims that chaos theory constituted a paradigm shift for the discipline. Nevertheless, some psychologists, even those interested in using chaos theory in psychology, resisted some of its uses.

Because chaos theory's main uses were in the physical sciences, some psychologists questioned its relevance for psychology. They argued that while chaos theory may provide insight for processes such as transitions to turbulence in fluids, chemical reactions, planetary orbits, stock market fluctuations, and a host of other physical phenomena, its ability to explain human mind and behavior may be beyond its explanatory power. The psychologist Wayne Powell (1995) collaborated with Eric Kincanon, a physicist, to warn psychologists of their explorations into chaos theory. By collaborating with a physicist, Powell brought with him the authority and prestige of a "hard" science to help him patrol psychology's boundaries. While the overall tone of the article was enthusiastic about chaos theory's potential for explaining psychological phenomena, Powell and Kincanon made sure to point out that psychologists' misuse of chaos theory was still very apparent. In one of their major points of contention they stated, "Although many areas will benefit from the application of chaos theory, psychologists must still beware of the misapplication, misunderstanding, and poor metaphorical uses of chaos" (p. 504). For instance, they pointed out that in psychologist H. A. Skinner's (1989) article on the potential of chaos theory for explaining alcohol addiction, he misuses the idea of sensitive dependence on initial conditions. First, Powell and Kincanon argue, Skinner failed to explain how the chaos leading to sensitive dependence could be measured with any precision. Secondly, Skinner believed that small changes in interventions by therapists would have a “dramatic effect” on whether an individual developed an addiction. According to Powell and Kincanon, the problem with this supposition is that Skinner confused small changes with small measurements. They pointed out that it is small *measurement errors* and not small changes to the addiction system that lead to sensitive dependence on initial conditions.

In another attempt at boundary-work with chaos theory, psychologist Susan Ayers (1997) questioned whether psychologists' metaphorical uses of chaos theory offered a viable explanatory framework in psychology. Using chaos as a metaphorical device was quite common in the published literature, especially among psychotherapists (e.g., Bütz, 1992; Mosca, 1995), family therapists (e.g., Chamberlain, 1995), and the closely related fields of psychiatry (e.g., Ehlers, 1995; Globus & Arpaia, 1994) and psychoanalysis (e.g., Levenson, 1994; Spruiell, 1993). While using metaphor to develop theories and to create new perspectives is common in science, there comes a time when the theory needs testing. Many psychologists seemed content to use merely the ideas from chaos theory as a metaphor. She continued her critique, stating “there are many obstacles to the application of chaos to psychology: chaos can be difficult to understand, difficult to apply and sometimes even difficult to believe” (p. 392). These difficulties were at the heart of psychologists' resistances to chaos theory. Many psychologists simply did not understand chaos theory and this is evident in the seemingly cavalier way some of them used concepts like fractals, nonlinearity, and bifurcations. But resistances also arose in reaction to psychologists using the methods of chaos theory to analyze their data.

Resistances came from those who questioned whether the methods of chaos theory could ever adequately explain psychological phenomena. Measuring static psychological variables with the precision that, say, physicists' measure physical variables is problematic. Describing and defining the parameters of *dynamic* psychological systems further exacerbates precise measurement. Exemplifying this type of resistance, mathematical psychologist R. Duncan Luce (1995) argued, "The difficulty with this approach is the crudity with which the dynamic processes are known. Until they are pinned down in much more detail, one cannot view this approach [chaos theory] as more than an interesting speculation" (p. 23). Essentially, his argument is that psychologists working with nonlinear models cannot work from the bottom-up and derive their dynamic variables from basic laws like those in physics. Instead, they have to infer dynamic processes from unobservable mechanisms through trial and error. Luce pointed to the work of Robert Gregson (e.g., 1988), who develops nonlinear models of psychophysical processes, as an example of this trial and error process. He ends his critique of nonlinear modeling on a less than enthusiastic note: "it is hard to be optimistic about our ability to test these nonlinear models effectively" (p. 20).

Paul Rapp (1995), a physiologist studying brain functions at the Medical College of Pennsylvania, issued this caveat to psychologists after many years of trying to demonstrate chaotic behavior in the brain: "Is there any evidence for chaos in the human central nervous system? The body of evidence in support of this conclusion continues to decrease as analytic methods improve" (p. 99). Similarly, Radford University brain researcher Karl Pribram (1996) discussed the difficulty of distinguishing between stochastic and chaotic behavior. Pribram and others found that nerve impulses function more characteristically as stochastic processes rather than as chaotic processes (Pribram, 1994).

Accommodations to Chaos Theory

Psychologists did not accommodate chaos theory as much as they did evolutionary psychology and cognitive science. Still, psychologists made some accommodations for chaos theory. For instance, the SCTPLS triggered interest from the American Psychological Association. In 1992, Karl Pribram persuaded psychologist Albert Gilgen from the University of Northern Iowa to contact the SCTPLS. The purpose of contacting the Society was to see if its members could promote chaos theory as a unifying theme for the APA's theoretical and philosophical psychology division (Division 24) annual program (Gilgen, 1995). The program helped to promote chaos theory's use in psychology by exposing a large number of psychologists to its tenets. The APA's interest continued when, in 1994, the journal *American Psychologist* published its first article on chaos theory entitled "Chaos, Self-Organization, and Psychology" (Barton, 1994). The issue of the journal even included a picture of a fractal on the cover. The article was critical of the uses of chaos in psychology, pointing out a number of methodological difficulties that needed addressing when using its methods to study human psychological processes. Nevertheless, Barton hailed chaos theory as a "new paradigm for understanding systems...gaining the attention of psychologists from a wide variety of specialty areas" (p. 5). The APA demonstrated its continuing interest in chaos theory by publishing an edited volume on the relationship of chaos theory to the areas of development, psychopathology, and psychotherapy entitled *The Psychological Meaning of Chaos* (Masterpasqua & Perna, 1997).

Another indicator of the acceptance of chaos theory in psychology is evident from a citation analysis of the *Social Science Citation Index* (SSCI) conducted by Peter Weingart and Sabine Maasen (1997) between the years 1974 and 1996. Their analysis indicates that the psychological literature accounted for the most citations pertaining to chaos theory of all the social sciences covered in the SSCI—including economics, business, sociology, and education between these years.[42] Additional analysis indicates that psychologists used chaos theory not only as a metaphorical device, but also as a methodological tool and theoretical framework in virtually every subdiscipline within psychology (Root, 2002, August). Examples from the psychological literature include applications to thought and memory (e.g., Clayton & Frey, 1996; e.g., Garson, 1996), intelligence (e.g., Goertzel, 1998), psychophysics (e.g., Gregson, 1992), behavioral analysis (e.g., Hoyert, 1992), development (e.g., Howe & Rabinowitz, 1994; Metzger, 1997), sensation and perception (e.g., Gilden, Schmuckler, & Clayton, 1993), psychotherapy (e.g., Fuhriman &

[42] Weingart and Maasen's original study separated citations in psychology from citations in experimental psychology. I combined the two here.

Burlingame, 1994; Hager, 1992; Paar, 1989, 1992), and industrial organizational psychology (e.g., Gregersen & Sailer, 1993; Guastello, 1995).

Despite these accommodations, psychologists did not accept the tenets of chaos theory as they did the ones of evolutionary psychology and cognitive science. The relative excitement about its potential uses and as a revolutionary paradigm quickly waned. As Stephen Guastello (2001) indicated, "The days of 'Here's Chaos!' have been gone for quite some time" (p. 27). There were only a relatively small number of courses taught on chaos theory in psychology and those who did offer these courses were usually members of SCTPLS (e.g., Keith Clayton, Fred Abraham, and Stephen Guastello). A cursory survey of undergraduate and graduate statistics and research textbooks (Aron & Aron, 1999; Gravetter & Wallnau, 2000; Grimm & Yarnold, 1998, 2000; Howell, 2004; Kirk, 1995; Myers & Well, 1995; Tabachnick & Fidell, 1996; Winer, Brown, & Michels, 1991) indicate that the mathematical modeling strategies and data analysis methods of chaos theory did not permeate the pedagogical research literature. Unlike evolutionary psychology, no introductory psychology textbooks contained sections on chaos theory's uses in psychology.

Of my three case studies, it appears that by 2000 chaos theory was still on the fringe of the discipline and on the verge of fading into disuse altogether. I postulate several reasons for chaos theory's distant relationship to the discipline of psychology. Most psychologists never received appropriate training in the methods of chaos theory so they never incorporated it into their research. Psychological data is often more "noisy" than data from physics, chemistry, and biology due to the difficulty psychologists experience when trying to control extraneous variables. It is often difficult to separate this noise from truly chaotic behavior even with the methods of chaos theory. Generating the appropriate and correct amounts of psychological data is difficult, especially considering the requirements to search for chaotic processes. Most chaos theorists agree that in order to find chaotic behavior in any type of system, one need acquire thousands, if not tens of thousands, of data points (Williams, 1997). This proves problematic for most psychologists, though physiological data may meet this requirement. Finally, as Rapp (1995) suggested, as the methods to measure chaotic behavior become more refined, there are questions as to whether psychological phenomena really do exhibit chaotic behavior.

Psychologists did not do much boundary-work pertaining to chaos theory. Psychologists realized the difficulties of using chaos theory to explain human mind and behavior. Some psychologists however, began using dynamic systems frameworks and the idea of self-organization to study psychological phenomena in fields such as development (e.g., Thelen, 1992; Thelen, Kelso, & Fogel, 1987), social psychology (e.g., Vallacher & Nowak, 1994), personality (e.g., Pervin, 2001) and cognition (e.g., van Geert, 2000; van Gelder, 1998). As identifying chaotic behavior in psychological data proved problematic, these psychologists decided to develop models of psychological phenomena based on the broader category of dynamic systems. While they do not ignore the possibility of chaotic behavior, they realize that chaotic behavior is only a small subset of possible behaviors in dynamic systems. One of the advantages of adopting this approach is that one can use both nonlinear and linear models and statistics in their investigations. So even if chaos theory dies out completely in psychology, the use of dynamic systems theory will keep the idea of complexity and unpredictability within the repertoire of psychologists' practices.

CHAPTER VI

CONCLUSIONS

The discipline of U.S. psychology is a complex web of interacting subdisciplines. Psychologists from each of these subdisciplines carve out niches of power, control, and expertise in particular psychological domains. Social psychologists carve out niches in the realm of social interactions, personality psychologists control information pertaining to the basic components of the “self,” and so on. Psychologists have a stake in keeping their epistemic authority in these domains. Society in general perceives psychologists as experts of human mind, behavior, and emotion and society elicits their help in many ways. The justice system, for instance, relies on psychologists’ expertise in evaluating competence and sanity in court cases. Businesses hire psychologists to improve worker productivity and evaluate new employees. It is difficult to find an area of society in which psychologists or their various conceptual, material, and social practices do not come into consideration.

Because of this perceived expertise, society relies on psychologists to prescribe appropriate ways of “being” in the world. Psychologists receive accolades, rewards, and prestige from their expert status. As I previously outlined, psychologists receive funding from various governmental and philanthropic sources such as the National Institute of Mental Health and the Sloan Foundation. These funds allow psychologists to pursue their areas of specialization. The more funding a psychologist receives typically results in more rewards (e.g., promotion and tenure) and prestige for the psychologist.

In order to keep this epistemic authority, psychologists engage in boundary-work. They patrol their boundaries, constantly scanning the disciplinary landscape for possible "cracks in the wall" where undesirables may enter. When psychologists find these undesirables, they use their authoritative position in society to demonstrate, through intellectual and political means, how they are distinct from these undesirables. Psychologists will often attack the scientific credibility of these undesirables, calling them pseudo-scientists, as they did and still continue to do with parapsychologists (e.g., Benjamin & Baker, 2004; Coon, 1992). In other situations, they will distance themselves from recognized fraudulent practices as some psychologists did after discovering discrepancies in Cyril Burt's intelligence data (e.g., Gieryn & Figert, 1986; Zenderland, 1990). Boundary-work is not only a tool for resisting practices; it is also a useful activity for bringing in new practices. Psychologists engage in boundary-work to incorporate new practices into the discipline, especially if they deem these practices as beneficial to their epistemic authority. At first, many psychologists resisted Freud's psychoanalytic theory, but after the growing public interest in Freud's theory, they began associating psychoanalytic theory with their own discipline (Hornstein, 1992). Psychologists' boundary-work is a continual process of resistances and accommodations towards objects, technology, ideas, metaphors, and theories impinging on their intellectual territory.

Between 1970 and 2000, the interdisciplinary programs of evolutionary psychology, cognitive science, and chaos theory impinged upon the intellectual territory of psychology. Scientists from each of these disciplines brought with them an array of conceptual, material, and social practices that forced psychologists to evaluate the boundaries of their discipline. Psychologists reacted to the practices of these programs by resisting particular dimensions of these scientists' practices or making accommodations for them. Within this thirty year span, psychologists dictated the allocation of these practices by what they believed to be useful for furthering their epistemic authority and general understanding of psychological phenomena.

Evolutionary Psychology

While psychologists have used the evolutionary theories of Lamarck and Darwin throughout the discipline's history, it was not until the 1980s that an organized group of scientists calling themselves evolutionary psychologists began to bring their conceptual, material, and social practices into the discipline. Evolutionary psychologists built their interdisciplinary program based on the controversial and politically-charged theory of sociobiology developed a decade earlier by the entomologist E. O. Wilson. Conducting boundary-work in their own program, evolutionary psychologists distanced themselves from sociobiology by focusing solely on the adaptive function of the human mind rather than the adaptive social behavior of all living organisms. Evolutionary psychologists proposed that cognitive abilities such as mate selection and retention, and reasoning in social exchanges were adaptations produced during our time in the Pleistocene era as hunter-gatherers.

Conceptually, evolutionary psychologists attacked psychologists' standard social science model, considering it an inadequate theoretical framework for psychology. Evolutionary psychologists' argued that humans were not born as "blank slates" but instead were born with innate, psychological mechanisms designed to solve adaptive problems such as those for detecting cheaters in social exchanges. In terms of material practices, evolutionary psychologists used software programs that changed the physical appearance of human faces and body types for their experiments on physical attractiveness. The social practices of evolutionary psychologists typify the ways in which organizations become professionalized and institutionalized. They developed a professional organization—The Human Behavior and Evolution Society—and a professional journal—*Evolution and Human Behavior*. These social practices enhanced evolutionary psychologists' credibility with the public and psychologists. Another social practice of evolutionary psychologists (e.g., Buss, 1995; Simpson, 1995) was their rhetoric about their theory being a revolution or paradigm shift in psychology.

Psychologists reacted in different ways to evolutionary psychology. Some resisted the practices of evolutionary psychologists, some made accommodations for these practices, and still others seemed to ignore their ideas altogether. I found that some psychologists resisted evolutionary psychologists' advocacy of the adaptationist perspective. Evolutionary psychologists relied on evolutionary biologists, anthropologists, and other scientists (e.g., George Williams, Robert Trivers) to outline the adaptationist perspective. Some psychologists responded by obtaining arguments from other scientists (e.g., Gould and Lewontin) to refute the adaptationist perspective (Rose & Rose, 2000). Others objected to evolutionary psychologists' reliance on adaptations and not enough on cultural factors in the understanding of human cognitive processes (Burn, 1996). Feminist psychologists such as Alice Eagly (1995) were concerned with the political ramifications of the gender difference findings of many evolutionary psychologists.

There is ample evidence that psychologists made accommodations for some of evolutionary psychologists' practices. Many top tier journals such as *Psychological Review* and *American Psychologist* published evolutionary accounts of various psychological phenomena. Evolutionary psychologists' research permeated many subdisciplines in psychology including cognition, development, and social psychology. Similarly, a number of introduction to psychology textbooks provided sections on evolutionary psychology. Also, textbooks for introductory and advanced psychology classes specifically written by evolutionary psychologists (e.g., Buss, 1999) came on the market. Accompanying the published accounts, a number of schools offered undergraduate and graduate courses in evolutionary psychology in addition to a few schools offering doctoral students specialization in evolutionary psychology. Finally, some evolutionary psychologists and feminist psychologists attempted rapprochement in the published literature (Buss & Malamuth, 1996).

Cognitive Science

The interdisciplinary program of cognitive science began infringing upon psychology's territory in the 1970s. The formation of cognitive science occurred as disciplines such as computer science, artificial intelligence, cognitive psychology, and neuroscience converged on the idea of using the digital computer as a metaphor for how humans process information. Cognitive scientists try to understand human cognitive processes such as memory, language, and problem solving, in addition to developing computer programs that mimic aspects of these processes.

Conceptually, cognitive scientists brought the computer-as-mind metaphor to many psychologists. For some, this metaphor served as a new way to view human cognitive activities. This helped shape the language some psychologists used to describe human thought, behavior, and emotions. The language became oriented towards computer programming and terms such as storage, parallel processing, and channel capacity began serving as descriptive terms for cognitive processes. With this language came two types of models—symbolism and connectionism—that psychologists employed in their attempts to understand information processing.

Cognitive scientists' material practices took the shape of attempting to create forms of artificial intelligence. Many psychologists still continue to use animal models to explain aspects of human psychological phenomena, but developing intelligence out of inorganic material was new and psychologists had to be contended with this new practice. While cognitive scientists made technological strides from Newall and Simon's simple General Logic Theorist to Brooks' more sophisticated COG, attempts at recognizable intelligence in machines still remain in their infancy.

Cognitive scientists brought many social practices to psychology. First, psychologists had to deal with the large amount of funding from governmental and private organizations cognitive scientists received for studying cognitive processes. Secondly, psychologists' realizations of the importance and possibility of studying mental events for their discipline spawned a number of journals specifically tailored to cognitive scientists such as *Cognition* and *Cognitive Science*. Accompanying the journals, cognitive scientists developed the Cognitive Science Society, which allowed scientists from various fields to interact and exchange ideas. Third, the undergraduate and graduate curriculum changed in many institutions as they began offering introductory and advanced classes in cognitive science. Some held classes in cognitive science institutions while others held them in psychology departments. Fourth, many cognitive scientists believed their approach to cognition was a revolution or paradigm shift for psychology (e.g., Sperry, 1988). Finally, cognitive scientists, specifically neuroscientists, brought brain imaging technology to the discipline. These images served to represent the internal structures and functions of the brain, while serving as a persuasive rhetorical device. Psychologists now had the technology to peer inside the mind of humans and watch the dynamical processes in action.

Resistances to cognitive scientists came mainly from behaviorists. Cognitive science threatened the behaviorist perspective that was prominent in psychology throughout the early to middle 1900s. Behaviorists found cognitive activity superfluous for hypothesizing or impossible to study. A science of psychology, so the behaviorists' argued, comes about only through a systematic study of stimuli and responses, and not through postulating mental events that act as intermediaries between stimuli and behaviors. I outlined a number of Skinner's (e.g., 1985) objections to cognitive science as a putative science. Other resistances came from those psychologists using animal models to explain human behavior. They found it difficult to accept the notion of using computers and computer programs to study humans. Others resisted cognitive scientists' reliance on brain imaging. Brian imaging, they argued, only tells part of the story of cognition. It answers the "where" questions but the not the "how" and "why" questions of cognitive processes.

Despite these resistances, some psychologists made accommodations for the ideas, theories, and technology of cognitive scientists. According to Robins and colleagues (1999), cognitive topics published in flagship journals rose from 1977 to 1996. This increase in citations coincides with the wide range of applications of cognitive science in psychology's subdisciplines including education, personality, developmental, and clinical psychology. Many psychology departments began offering cognitive science courses and several textbooks within the discipline provided it with ample coverage. Similarly, many cognitive psychology textbooks increased their coverage of the neuroscience aspects of cognitive science. With this increased coverage came the use of fMRI and PET images as evidence for cognitive processes.

Chaos Theory

Finally, in the mid-1980s some psychologists began considering the viability of chaos theory as a way of understanding and analyzing human mind, behavior, and emotions. I argued that psychologists became interested in chaotic behavior in psychological phenomena as physiologists (e.g., Skarda & Freeman, 1987) began exploring its features and as the popular science literature flooded with books on chaos theory (e.g., Gleick, 1987). In particular, the popularized accounts of chaos theory, with their promises of a scientific revolution, fascinated many psychologists. Many early works by psychologists referenced either the physiologists or the popular portrayals of chaos theory.

In terms of conceptual practices, some psychologists began adopting the language of chaos theory to explain various dimensions of human psychology. It was common to find terms such as "fractals," "self-organization," and "bifurcations" used metaphorically to explain diverse phenomena such as personality development and the psychotherapeutic process. Some psychologists also began employing many of the methods used by chaos theorists in other disciplines such as catastrophe modeling and nonlinear time series analysis.

The computer offered the most salient material practice for chaos theory. Some psychologists began using software programs such as Chaos Data Analyzer (Sprott & Rowlands, 1992) specifically designed to analyze large amounts of data quickly and efficiently. These types of programs helped identify chaotic behavior in psychological data. Additionally, they used these programs for visualizing the results of their experiments. The phenomena these psychologists studied were dynamic and these programs allowed them to view the changing nature of the systems under investigation in a much more effective way than offered by traditional data analysis.

Socially, psychologists interested in chaos theory began communicating through newsletters and conferences established largely for other scientists. These communication networks allowed psychologists to interact with scientists beyond their discipline and with other psychologists interested in chaos theory they might not have otherwise met. Centers to study chaotic behavior in psychological systems began to emerge in the 1990s such as the Center for Complex Systems at Florida Atlantic University. In 1991, those interested in the ramification of chaos theory for psychological phenomena formed the Society for Chaos Theory in Psychology and the Life Sciences. Six years after the Society, they began publication of their interdisciplinary journal, *Nonlinear Dynamics, Psychology, and Life Sciences*. As with the other interdisciplinary programs, some psychologists began referring to chaos theory as a new paradigm for psychological science (e.g., Combs, 1995).

Because employing chaos theory in psychology was a relatively small movement, many psychologists ignored its uses. Those psychologists who tried to resist its uses in the discipline attacked it on two fronts. First, they objected to using chaos theory as a metaphor for psychological phenomena. Sensitive dependence on initial conditions, for instance, offered an inappropriate metaphor for comments made by therapists to their clients because it pertains to measurement rather than changes in conversation. The second resistance involved psychologists trying to use the analytic techniques of chaos in their research. Chaos theory's methods were relatively new and they were being refined constantly throughout the 1990s. Some psychologists believed it to be premature to use such techniques. Others criticized the difficulty in measurement of dynamic processes. It was much easier to measure phenomenon in physics than it was psychological phenomena. Finally, there were those who questioned if any psychological processes truly exhibited chaotic behavior.

Despite chaos theory's fringe status in the discipline, psychologists did accept some of the practices inherent in this interdisciplinary program. For instance, psychologists elicited assistance from members of the Society to develop a program on unifying psychology for the APA in 1992. The APA also published a number of journal articles and a book on chaos theory. A number of subdisciplines including developmental, cognitive, clinical, and social psychology published accounts of chaos theory in their content-specific journals. In fact, according to the Social Science Citation Index, psychologists' publications account for the greatest number of citations on chaos theory of all the social sciences.

General Themes in Psychologists' Boundary-Work

Are there any general themes of psychologists' boundary-work in relation to these three interdisciplinary programs? Even though evolutionary psychologists, cognitive scientists, and chaos theorists introduced different types of practices into the discipline, they all contributed to changing psychology's boundaries to a certain degree. According to my analysis, resistances most often came when psychologists faced new conceptual practices. Psychologists resisted evolutionary psychologists' use of the adaptationist perspective, cognitive scientists' use of the computer-as-mind metaphor, and chaos theorists' chaotic behavior metaphor. Psychologists showed different resistances to the analytic methods of each of these programs. Evolutionary psychologists use mainly the same technologies for analyzing their data; therefore, psychologists did not resist these kinds of material practice. Some psychologists did resist cognitive scientists' reliance on computer program modeling as a viable means for understanding cognitive processes. Similarly, some psychologists objected to the fact that chaos theory's methods were inadequate for analyzing psychological phenomena.

Based on my analysis, it appears that psychologists' first line of defense in their boundary-work activities is to attack their opponents' conceptual practices. Conceptual practices lie at the heart of psychology's disciplinary structure. Psychologists rely on their theories and methods of analysis largely to define their discipline. Conceptual practices are the locus of psychologists' expert knowledge. It is not surprising then that psychologists resisted the introduction of new concepts, theories, and analytic methods. The conceptual practices of these three programs threatened many psychologists' roles as experts. Feminist psychologists felt threatened by the political ramifications of evolutionary psychologists' adaptationist perspective. Behaviorists felt their goal of a science of behavior challenged by cognitive scientists' contention of a science of information processing. Other psychologists faced questions about reductionism and predictability in their research programs from chaos theorists who believed in holistic approaches to research and an emphasis on understanding over prediction.

Even though the computer served as the primary material practice for each of these interdisciplinary programs, psychologists' resistances to the uses of computers were different for each program. Evolutionary psychologists' use of software programs to morph facial features and body types did not cause psychologists to engage in much boundary-work. Similarly, there was not much resistance to using computers to search for chaotic behavior in psychological processes, even though some psychologists objected to the validity of chaos theorists' findings. Cognitive scientists' use of computers in the development of artificial intelligence did cause resistance from some psychologists.

Psychologists, of course, were familiar with the many ways of using computers to facilitate experiments and analyze data. Since evolutionary psychologists and chaos theorists used the computer in these ways, it is not surprising that psychologists put little effort into resisting this type of material practice. But cognitive scientists' efforts to develop intelligent machines are a qualitatively different use of computer technology. Creating artificial intelligence brought up many issues for psychologists, not all of them explicitly stated. There are many ethical and philosophical dilemmas to this kind of endeavor usually only debated by philosophers. Do we rely solely on the Turing test to decide what constitutes an artificial intelligence? If not, what other kinds of tests can be used? Should scientists even attempt this feat without fully understanding human intelligence? Is it even possible to develop some kind of intelligence from inorganic material? While most psychologists did not concern themselves with these issues, many who resisted the possibility of artificial intelligence felt it necessary to defend their own material practices. Animals served as the primary source of data for many of these psychologists. The advent of something else taking over as a primary source of data in psychology was a threatening possibility.

Psychologists seemed not to resist many of the social practices of these interdisciplinary programs. This may have more to do with practicality than anything else. It is one thing to write a scathing book review or devastating critique of an opponent's theory. It is another thing altogether to try to stop people with a common interest from organizing. I found no instances of psychologists trying to prevent the development of any of the interdisciplinary programs' professional organizations, though this may be due to the lack of archival sources on the matter. Similarly, I found no resistances to the publication of their flagship journals. I should mention, however, that I found no evidence for the large professional organizations in psychology like the APA making attempts to develop distinct divisions for these scientists' areas of expertise.

One surprising feature of my analysis is that few psychologists attacked these interdisciplinary scientists' rhetoric of scientific revolutions and paradigm shifts. It seems likely that the incursion into psychologists' intellectual territory would prompt them to defend their disciplinary boundaries. Each program had individuals claiming revolutionary or paradigm status for their science, yet psychologists did not spend time refuting these claims. One possible explanation for psychologists' inactivity is that they found these claims preposterous and not worthy of a response. But this solution is problematic. Surely, if psychologists spent time resisting the possible gender inequality inherent in the adaptationist perspective, they would also spend time refuting the claims of a supposed paradigm shift. A second and more probable reason for this inactivity is that, since Kuhn's (1962/1996) *The Structure of Scientific Revolutions*, terms such as "revolution" and "paradigm" have become fairly ubiquitous in science. Scientists tend to use these terms seemingly without understanding Kuhn's message. Scientists often use phrases like "method X is a paradigm" or "theory Y is a paradigm" as a way to describe some common scientific activity or idea. It is probable that psychologists paid no attention to these claims because they were accustomed to seeing these terms in their own scientific literature. A third possibility is that psychologists' had minimal exposure to these claims in their resistance attempts. Psychologists may not have read as widely in the literature as I have and may not have found a significant number of scientists claiming their program as a new paradigm. These possibilities do not do this topic justice and I suggest future research on the lack of psychologists' boundary-work on these claims.

To a certain extent, psychologists' resistances were largely ineffective because accommodations occurred at every level of practice for all three interdisciplinary programs. By 2000, evolutionary psychology was a common theoretical framework found in psychology books, journals, conference presentation, and the classroom. The same is true of cognitive science, though it was just as likely that psychologists migrated out of their departments and into cognitive science institutes. While some psychologists accepted the ideas of chaos theory, many pulled back from trying to discover chaotic behavior and preferred to examine psychological phenomena from the larger perspective of dynamic systems.

These accommodations indicate that, overall, psychologists did not view these three interdisciplinary programs as pseudosciences or fraudulent practices. In this respect, psychologists' boundary-work for evolutionary psychology, cognitive science, and chaos theory was fundamentally different from the boundary-work they conducted in the late 1800s with the practices of spiritualists and psychic mediums (Coon, 1992). At that time, psychologists had to demonstrate to the public how psychology was different from the study of psychic phenomena, while at the same time ensuring that the spiritualists understood they were not part of the discipline. Instead, the boundary-work psychologists conducted in response to these interdisciplinary programs looks similar to boundary-work psychologists used with psychoanalysis throughout the 1900s (Hornstein, 1992). Initially, psychologists attacked psychoanalysts as being unscientific. By the beginning of the 1960s, psychologists began adopting ideas from psychoanalysis. Hornstein argues that this adoption helped make psychology a "more flexible and broad-based discipline." I believe this to be the end result of psychologists' accommodations to my three interdisciplinary programs. Psychologists were able to open new areas of expertise through their associations with these interdisciplinary programs. The interdisciplinary programs brought with them expertise from fields like evolutionary biology, computer science, physics, and mathematics. Conceptual practices such as the use of new metaphors and models, material practices like new computer programs and data analysis techniques, and social practices including increased funding and new professional journals helped psychologists develop into a more flexible and broad-based discipline between 1970 and 2000.

Interestingly, during this thirty year span the interdisciplinary programs began borrowing ideas from each other. I expected to find some overlap between cognitive scientists and evolutionary psychologists because of their closely related goal of studying the human mind. Indeed, Cosmides and Tooby (1995) argued for evolutionary psychology as a critical link between psychology and cognitive neuroscience. I also expected a small degree of overlap between cognitive scientists and chaos theorists in psychology. Earlier, I referenced scientists working on the connection between cognition and chaos theory (e.g., Goertzel, 1994). I also mentioned those who pulled back from searching for chaotic behavior and shifted their perspective towards viewing cognition as a dynamical system (e.g., Thelen & Smith, 1994; van Gelder, 1998). I did not expect to find a relationship between evolutionary psychologists and those studying chaotic or dynamic systems behavior in psychology. Interestingly, the evolutionary psychologist Douglas Kenrick and his colleagues (2001; 2003; 2002) are now exploring the relationship between evolutionary adaptations and dynamic systems theory.

The History of Psychology and the Sociology of Scientific Knowledge

My research attempted to bring the history of psychology and the sociology of scientific knowledge together using boundary-work as a framework. The choice of psychologists' boundary-work on the practices of scientists from three interdisciplinary programs was both productive and problematic. It was productive in the sense that I was able to examine the lines of intellectual demarcation that psychologists laid out for three different programs. This provided a broad survey with rich sources of information on their boundary-work activities in reaction to multiple infringements. I was also fortunate because these interdisciplinary programs developed and interacted with psychology over a relatively recent period in psychology's history. This made much of the information readily available in published form.

From a historical perspective, it was problematic because these programs were relatively new and are still influencing psychologists' boundary-work. In this respect, my story is incomplete. Another difficulty came with my brief histories of the interdisciplinary programs. Given their recent past, it was difficult to establish proper historical perspective on these programs. Many of their practitioners are still active in the field today. I had to dispense with archival material because, in this recent period, the documents have not yet been deposited.

I believe psychology's history to be unexplored territory for sociologists of scientific knowledge. Most accounts by sociologists of scientific knowledge focus on the physical sciences (e.g., Barnes & Shapin, 1979; Pickering, 1984). Their focus is strategic because one of their goals is to demonstrate the contingent nature of scientific practices. If they can demonstrate the contingent nature of scientific practices in the "hard" sciences, it should follow that scientific knowledge is constructed rather than discovered in the "softer" social sciences. But by ignoring social sciences like psychology, sociologists of science miss a number of opportunities to compare the fields of physics, chemistry, and biology with those such as sociology, anthropology, and psychology. For instance, future research may provide answers to questions like, "Do physicists' conceptual, material, and social practices differ fundamentally from psychologists' practices?" and "What are the similarities and differences between the intellectual boundaries of chemists and those of psychologists?".

Finally, I believe psychologists' boundary-work to be a viable and potentially fruitful area of study for historians of psychology. Few historians of psychology explicitly use boundary-work in their research. In the middle 1960s, Robert Young (1966) encouraged historians of psychology to move away from Whiggish history and focus on the fine details in psychology's history such as the institute and the laboratory. In the 1990s, Roger Smith (1998) argued that the history of psychology was mature enough to start looking again at the big picture. I believe it is time to start revising these histories through an examination of psychologists' boundary-work. Boundary-work analysis strikes a balance between the big picture and the small details. It does this by focusing on the intellectual territory of the discipline as a whole, while examining the pockets of controversy that play a role in shaping this intellectual territory. At the very least, I believe textbook histories of psychology should move away from the "great" men, ideas, and dates approach and focus more on how psychologists established, maintained, and extended their disciplinary boundaries. All the fascinating stories, the rich detail, and the archival research remain the same, but there would be a unifying purpose to the historical scholarship. The purpose would be to understand how psychologists achieved, kept, and furthered their expert status in society throughout the discipline's history.

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