PATH in Psychology

THE *Life Cycle* OF PSYCHOLOGICAL IDEAS

Understanding Prominence and the Dynamics of Intellectual Change

Edited by

THOMAS C. DALTON

and

RAND B. EVANS

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PREFACE

The idea for this book first emerged from a symposium invited by Division 1 of the American Psychological Association (APA) that was conceived by Lewis Lipsitt, who was president of the Division in 2000. The symposium, "Reflections in the Mirror of Psychology's Past," chaired by co-editor, Thomas Dalton was organized to pay tribute to John Popplestone and Marion McPherson, who founded the Archives of the History of American Psychology at the University of Akron, Ohio in 1965. The panel included John Popplestone, my co-editor, Rand Evans and Robert Wozniak, who have contributed chapters to this book. John and Marion, who both served as past presidents of Division 26 of the History of Psychology, retired in 1999 and turned the leadership of the collection over to its new director, David Baker. They were honored at that time by the APA with a Presidential Citation for their achievements and were given a Festschrift in April 2000 hosted by the Akron archives attended by several distinguished psychologists that included Lewis Lipsitt, Ludy T. Benjamin and John Burnham. An honorary fund also was established in their names for individual donations. Sadly, Marion passed away shortly afterward, but her spirit and determination live on at the Akron archives.

John and Marion's tireless efforts to make this a truly great repository are indicated by the sheer size of the collection. The archive now possesses the papers of more than 700 psychologists and the records of more than 100 psychology journals. It has stored 700 kinds of psychological apparatus and testing instruments, 3000 rare photos and nearly 153 miles of child development films, examples of which are wonderfully displayed in their popular book, *An Illustrated History of American Psychology*. Ludy Benjamin, who spent countless productive hours researching the Akron archives,

perhaps best described John and Marion's pioneering contribution, when he wrote:

> So one can argue that the time was right for someone to have the historical consciousness to recognize the need for a central archive for psychology. I want to emphasize the word vision . . . Vision is a rare commodity. In this context it means to see things in ways that no one else does. It means to be able to see in long stretches, to look beyond your own time and see needs that no one else may anticipate. The Zeitgeist may aid in the focus of such vision, but it isn't sufficient as an explanation.

Pioneers possess the uncanny ability to distance themselves from current events and anticipate future interests and needs long before they become apparent as present day concerns. Psychologists can point with pride at the many insights of its greatest thinkers and the marvelously inventive experiments of its laboratory researchers who contributed to human well-being. Signs of scientific advancement and professional success abound on every front even though most psychologists find it daunting to show how competing psychological ideas and theories form a coherent body of thought. The field of psychology continues to splinter into a multitude of sub-disciplinary groups barely able to communicate with each other. Consensus remains elusive on what the field should strive to be or do in the future. Given these predicaments, we may question whether it is possible for scientists and practitoners in a field to take a self-critical look at what they are doing, when there is no larger perspective on which we can base these judgments. Nevertheless, we can attempt to make our biases and values more explicit by adopting a broader historical perspective. We can interrogate the past to better comprehend the forces that contribute to convergent beliefs and their dissipation and thus avoid becoming unwitting victims of our own illusions.

The contributors to this volume address this vexing problem of perspective by taking a closer look at the relationship between the processes by which intellectual recognition is attained and the forces that contribute to the endurance or erosion of support for a body of thought over time. A scientific discipline and its specializations have evolved from predecessor fields whose philosophical perspectives and assumptions have undergone revision. Historians try to render explicit the social contexts and scientific processes through which these beliefs and assumptions are adopted, tested, validated or repudiated. Psychologists have been guilty at different times of uncritically celebrating, misinterpreting or misrepresenting the ideas of its major thinkers and scientists, and have sometimes ignored or overlooked key episodes, documented in this book, which put individuals

and events in an entirely different light. The contributors rejoin these fragmentary elements of personal biography, professional circumstance, theoretical debate and social controversy into a more coherent understanding of the forces that contribute to prominence and new ideas and beliefs that sometimes shift the intellectual and moral center of gravity of culture in a democratic society.

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INTRODUCTION

PUTTING PROMINENCE IN HISTORICAL AND ANALYTICAL CONTEXT

The past is a mirror reflecting the images of ourselves created by thinkers whose ideas and contributions we hold in high esteem. The intellectual achievements of our eminent forebears seem to persist as indelible images that are inscribed in our minds and lodged in our hearts. Though separated by decades and even centuries, psychology's founders continue to enchant and provoke us because their ideas have withstood the test of time.

Perhaps a better analogy to describe how (and why) we remember, reflect on and interpret the intellectual contributions of prominent psychologists differently over time is that of looking into a *rear view mirror*. In this instance, everything is in motion and undergoing change. The past is gradually receding from view, as it disappears over the horizon. The future is moving towards us, first as dimly perceived problems whose significance is uncertain, until they come into sharper conceptual and theoretical focus. Our predecessors' discoveries and theories are milestones that enable us to map the territory already traversed. We rely on their insights, metaphors and methods to understand unfamiliar phenomena and try to anticipate what lies on the road ahead. As we move closer to the horizon, we can look back and sometimes better grasp in their entirety the intellectual, personal and institutional factors that supported the emergence of a unique discovery, idea or perspective, which continue to hold our attention.

The rapid pace of scientific discovery forces historians continually to reassess the contributions of their contemporaries and predecessors and to put their work in historical and contemporary perspective. Scientists are recognized and rewarded for their originality and their intellectual

authorship through a system of citations that records their influence and traces the dispersal of their ideas through time. This system for intellectual recognition provides an important source of data for quantitative analyses, which reveal how the pathways through which intellectual credit is distributed change over time as prominent thinkers gain influence in new fields, while having a declining impact in others. But those who are rewarded with recognition for their originality do not always pay their cognitive debts to those who contributed to their own intellectual development. Historians occasionally find new archives that provide significant new information about a person's background, the sources of their ideas or other influences, or new experiences that put their ideas in a new light. Reconstructing the circumstances surrounding these episodes enables historians to distribute credit more equitably. Examples are not difficult to find in which new evidence surfaces that instigates a wholesale reassessment of an individual's work, such as Frank Sulloway's (1979) biographical investigation of Freud's intellectual roots. The discovery of John Dewey's largely unknown collaboration in the 1930s with infant experimentalist Myrtle McGraw also has stimulated a re-examination of the scientific basis of his theory of mind and inquiry (Dalton & Bergenn, 1996 & Dalton, 2002).

It is important to put the contributions of great psychologists in a larger cultural context, because eminent psychologists, such as William James and John Dewey, were *public intellectuals*, whose ideas were disseminated widely to become the possession of American culture. Pragmatism rapidly became a movement whose origin was of less importance than its power as a practical technique that could be adapted to solve new problems. But when a body of ideas or theory moves beyond the control of intellectual communities into the larger culture, the social or policy implications are frequently contested. This is indicative of the public's inclination to reduce complex theories and evidence to simple either/or slogans, such as that engendered by the nature versus nurture debate. This tendency to dichotomize is reinforced by the intellectual propensity, documented by Simonton (2000), of psychologists who attain eminence, to do so by taking extreme positions on issues and debates that enable them to stand out from the crowd. But this strategy attracts competitors and rivals who adopt opposing positions at the other end of the continuum rather than encouraging moderation and balance.

The forces that shift the center of gravity of scholarship toward new interpretations of classic works remain perplexing and controversial. The study of the history of ideas advances through a dynamic process in which the worth of previous contributions is always measured according to new expectations and different values. This sometimes enables scholars to attain the psychological distance necessary to take a more balanced and nuanced

view of past accomplishments. But it also runs the risk of taking ideas out of context and ignoring the time-sensitive and culturally bound nature of thought of a specific era. Thus scholars who seek new meaning and significance in ideas that have endured must be sensitive to time and place in their assessments and attempts to adapt them to changing institutional needs and cultural demands.

FOCUS OF THIS BOOK

This book focuses on the familiar but little understood cycle whereby some "great" psychologists' ideas reach a pinnacle of influence that endure while others slide into oblivion and then are "rediscovered" and rehabilitated to become relevant again (see Watson and Evans, 1991). The contributors to this volume examine and assess several factors (i.e., personal professional, scientific, organizational, theoretical and ideological, etc.) that contribute to this cycle whereby some influential psychologists enjoy enduring prominence for their ideas, while others suffer periods of indifference, misinterpretation and sometimes, derision before being revived and seen in a new light. A closely related issue examined here is why prevailing ideas and assumptions about psychological phenomena undergo significant change that sometimes topple or even reverse received theory. Why some theories and theorists' reputations flourish, attract adherents and form schools of thought while others don't are interesting questions that require us to adopt a larger perspective, by comparing individuals and their intellectual contributions across time and through different domains.

The editors and contributors shed light on this putative cycle and examine why it seemingly contributes to the never-ending search for the origins and founders of a discipline and to attempts to retrace their theoretical heritage. Provocative questions are addressed that include the following: Through what interpersonal and professional processes do scientific leaders gain recognition as "founders" of a discipline? How and why do the histories of a field written by its practitioners differ from those written by those outside the field? How do individual cognitive orientations, personal styles, professional activities, theoretical perspectives and scholarly productivity affect and predict recognition and prominence? How do career decisions and strategies affect the prospects of recognition and posterity? What are the long-term career advantages and disadvantages of having a prominent mentor? What enables a body of work to withstand distortion by false attribution, labeling and stereotyping? Why do some theories attract renewed attention while others fail to gain a foothold needed to sustain long term intellectual development? How do rivalries affect the

processes of professional recognition for originality and impact? Through what processes is a theory interpreted and appropriated to become a school of thought or is culturally dispersed through a movement that attracts adherents? When do the signs become apparent that a movement (e.g., behaviorism) is on decline or will run out of steam? Through what intellectual and institutional processes do new theoretical perspectives emerge and how do they become dominant to influence prevailing views about what phenomena are considered important objects of investigation?

We argue that the processes through which psychologists and other scientists attain eminence and authority in their field of endeavor can be more clearly understood, when viewed within a broader historical and institutional framework. Our contention is that the processes through which professional identities are constructed, expertise is acquired, innovation is rewarded and theoretical disputes are resolved over time furnish information pertinent to understanding the role of prominence in demarcating a field of study. Moreover, we believe that the question of how leading psychologists capture the attention and inspire the analysis of or emulation by their contemporaries and successors cannot be neatly separated from underlying professional and institutional processes. These processes bestow merit and confer authority on those whose ideas are appropriated, repudiated and/or rejuvenated over time. Prominence is a reward for being recognized for making contributions that can involve the successful adoption of different roles. Some scientists gain an enormous reputation for seminal discoveries or persuasive theories while others attain recognition for professional leadership, mentoring students, developing innovative methods or applications or brilliantly synthesizing intellectual trends begun by others. Each of these modes of prominence is attained through different pathways of recognition.

Contributors to this volume take different approaches toward understanding the phenomena of prominence. Some contributors focus on historical figures whose ideas have undergone interpretive revision and whose popularity has gone up or down, or attracted different audiences or adherents over time. From these individual cases factors are identified that are relevant to questions and issues about prominence, which have been raised in this introduction. Others approach the phenomena from a different angle of vision to understand the functions of prominence in forming schools or instigating intellectual or social movements. Significant factors and recurring themes are singled out that enable us to generalize from individual cases and episodes. Analyses of how the career fortunes of single individuals intersect with contextual factors and institutional forces contributes to our understanding of the larger phenomena of intellectual change in general (see Collins, 1998 and Sulloway, 1996).

European and American psychologists attained academic and professional recognition sooner than their colleagues in the other social sciences, but this success did not depend solely on their methodological skills or commitment to science. Wilhelm Wundt, William James, John Dewey, Stanley Hall, Sigmund Freud and John Watson were visionaries, who recognized more clearly than their contemporaries that the fortunes of psychology were inextricably bound with their roles as professionals and reformers. They perceptively anticipated that the demand for psychology would rise, when its techniques and theories were seen as levers to bring about social reform and human betterment. That is why pragmatism and behaviorism alike were not simply distinctive philosophical and theoretical positions about knowledge and human behavior, but became *social and educational movements* that advocated the societal adoption of distinctive child-rearing and educational policies and strategies.

A more complete explanation of these related phenomena then should take into consideration the following questions posed by the contributors to this volume and discussed in a concluding chapter:

- Through what personal, professional, institutional and political processes do seminal thinkers attain prominence and how is this recognition sustained over time?;
- Why do some theorist's ideas gain widespread acceptance to form schools of thought or social movements that have wide cultural impact?
- Why do some scientific innovators fail to get recognition for their discoveries or their ideas are misrepresented, misinterpreted or associated erroneously with a school of thought?
- Why do schools of thought attract different audiences, adherents and critics over time, who see their ideas in a different light?;

OVERVIEW OF THE CHAPTERS

Reconstructing Psychology's Founding and Growth

Contributors to Part 1 focus on questions pertinent to the founding of psychology as a discipline, its growth and assessment as a profession, its processes of intellectual recognition, the changing influence among its dominant theoretical perspectives and their relationship to other fields. Rand Evans traces an important source of "origin myths" to textbooks and conferences that have become key mediums through which knowledge is disseminated and beliefs are reinforced about those who have been "first"

in the field. This reflects a sequential view of history that focuses on the contributions and intellectual leadership of a few pioneering individuals. But Evans shows why this conception of great men serves certain functional, institutional purposes that are not clearly understood.

Founders are largely transitional figures, according to Evans, who may have started but do not perfect the theories and methods carried forward by second and third generation scientists. Wilhem Wundt boldly undertook the institutional initiatives that helped establish psychology as an independent discipline. But even these accomplishments are not enough to guarantee founder status, as in the case of G. Stanley Hall. Hall, who, unlike William James, squandered much intellectual and political capital by his imperious and blunt leadership style and professional vendettas.

Political strategies are employed to advance professional causes that are sometimes not obvious but nonetheless significant. Evans describes how Hall and James competed for recognition as the founder of their discipline by employing strategies that ultimately had paradoxical consequences. Evans shows that there are symbolic uses of founding and founders that satisfy the psychological need for leadership, authority and continuity. He argues that founders not only must demonstrate superior intellectual capacities and institutional leadership skills, but also must possess human virtues that dignify them (and us) and justify our belief in their authority and legitimacy.

Robert Wozniak contends that the received "truths" of psychology's history are now viewed by some observers to be much more complicated, contextualized, and open to interpretation than was once realized. This has led to a critical reexamination of the traditional way to record the history of psychology involving a linear, incrementalist birth-revision-replacement-survival format. The processes supporting the creation and endurance of ideas do not conform always to a predictable linear trajectory but appear to reflect cyclical forces involving reputation, prominence and changing assessments of classic works.

Wozniak focuses on three important issues relating to the life cycle of ideas. First, he examines how history comes to regard a work of the past as a "classic" and its author as a "major contributor." This involves an analysis of the general criteria by which ideas come to be seen in retrospect as "significant." Second, he analyses why some contributors and ideas famous in their own day subsequently disappear from recorded history. And third, he assesses the conditions under which ideas seemingly lost to history are sometimes rediscovered and the reputations of their authors rehabilitated. Wozniak uses James Mark Baldwin's career and works as a case study to illustrate how several factors he describes help explain why

Baldwin's classic works experienced a renaissance despite his personal humiliation and professional derogation.

John Popplestone contends that scholars who hold fundamentally different perspectives about the field of psychology have documented the history and growth of psychology as a discipline and profession. Trained psychologists focus on traditional questions about how the past led to the present and how scientific progress has been achieved. They try to understand how contemporary psychology came into being, explain how different research traditions emerged and how they relate to each other. The focus is largely internal, emphasizing ideas, concepts and the people and places that are associated with them. Practitioners of this approach include experimentalist Edwin Boring and social psychologist Gardner Murphy, who conducted research and wrote textbooks for students in the field.

Another group comes from outside the field of psychology, Popplestone contends, who view academic disciplines as social and political organizations whose practitioners reflect intellectual and professional biases and whose knowledge disguises subtle forms of power. These practitioners of "science and society studies," inspired by Michel Foucault, contend that they are not bound by psychologists' interests and biases and thus are not forced to adopt an uncritical, progressive view of its history. Popplestone illustrates through several case studies the differences in cognitive orientation, research styles and political attitudes that lead these observers to take sharply contrasting perspectives toward the history of psychology as a science and profession. He takes the position that practitioners in the field of psychology are more likely to be sensitive to problems of bias, factual validity, chronology and contextual interpretation that may cloud historical assessments by those who view psychology from the outside.

As a psychologist, Dean Simonton has devoted more than a quarter century to the scientific study of creative genius in the arts and sciences. He argues that Boring was mistaken in claiming that the zeitgeist perspective on history (i. e., the general intellectual, cultural or moral climate of an era) is more naturalistic than is the great person or genius perspective. Instead, Simonton believes that prominence can be treated as a "natural phenomena" in which history is partially rooted in individual biography. He contends that there are good scientific reasons to focus on that portion of the personal biography that matters most—research and publications—because he found that the cross-sectional distribution of total lifetime output is highly skewed right. Accordingly, a small percentage of the individuals in any given domain of research contribute disproportionately to the total contributions. Significantly, this concentration of productivity at the highest echelons—a pattern that prevails in all of the sciences—is

strongly associated with the citations that a scientist receives in the research literature. Simonton summarizes the findings of a series of citation studies, supplemented by qualitative and contextual analyses that he and other scientists have conducted, which demonstrate that the relationship between productivity, intellectual quality, citation frequency, eminence and enduring prominence conforms to recurring statistically predictable pattern. Simonton explains why he believes that individual and situational factors can be combined into a single causal model for the scientific analysis of psychology's history.

Thomas Kuhn started a debate about the structure and practice of science that has challenged psychology and other disciplines to think critically about the institutional processes through which knowledge is produced and theoretically integrated. He believed that the conduct of science conformed to a cyclical pattern in which mid-range puzzle solving and theoretical competition gives way, under the accumulated weight of anomalies, to the adoption of a completely new paradigmatic framework of understanding. This paradigm furnishes the unifying assumptions and methods for the practice of "normal" science until the accumulated evidence of exceptions again forces the overthrow and replacement of the prevailing paradigm. Jessica Tracy, Richard Robins and Samuel Gosling present the results of several quantitative analyses of citation practices among competing schools of thought in psychology, which, they contend, suggest that the field of psychology is "multiparadigmatic," and thus there is no need to assume the eventual emergence of a unifying paradigm. Accordingly, several dominant theoretical perspectives may compete indefinitely for scientific leadership and experience the waxing and waning associated with the uneven rate of methodological innovation and scientific discovery. They document the changing fortunes of behaviorist, psychoanalytic, cognitive and neuroscience perspectives involving the ascendance of cognitive and neuroscientific schools. While the authors point out that these latter two schools have introduced a strong interdisciplinary orientation in psychology, they caution that it would be premature to interpret that this as a sign of paradigmatic unification.

Comparative Psychology

The larger intellectual and institutional forces shaping the field of psychology reflect underlying individual differences in style, strategy and substance. In part 2, Donald Dewsbury presents several interesting case studies that examine and assess why noteworthy comparative psychologists and ethologists have differed in their ability to attain prominence and retain recognition over time. He describes personal and professional

factors that are not easily quantified but immeasurably affect the recognition and respect accorded to comparative psychologists for their contributions to psychological science. Dewsbury illustrates why notoriety for a significant discovery or the development of a powerful theory does not translate easily and smoothly into a reputation that endures. Dewsbury shows how a variety of factors that include personality, writing style, academic pedigree, professional connections, intellectual adaptability and promotional skills, among others, contribute positively or negatively to prominence and historical posterity.

Michael Corballis and Stephen Lea contend that from its inception comparative psychology has been divided by a long-standing debate between those who believe in the uniqueness of the human mind and those who argue for the continuity of mind between humans and closely related animal species. The philosopher René Descartes believed that the possession of a mind, language and soul fundamentally set humans apart from beasts because the mind is a God-given, non-material entity. Charles Darwin, of course, proposed that humans evolved through natural selection that included a shared ancestry with our ape relatives. Wundt and Titchener were dualists who believed that only human minds were accessible to introspection. The behaviorist revolution eliminated the mind as a serious subject of study thus opening the door to comparative studies. But dualism was reintroduced, Corballis and Lea argue, by Noam Chomsky, an early leader in cognitive science, who contended that language and syntax are rooted in human genetics. But the discovery that humans and chimpanzees share 98.4% of their DNA instigated sophisticated empirical studies, the authors assess, involving the potential shared ape and human capacity to imitate gestures, read other minds and display handedness, which have contributed to a renewed belief in the continuity of species.

Cognitive Science and Consciousness

The rise of cognitive psychology constitutes and important episode in the history of psychology. But little is known among American psychologists about the pioneering role of Otto Selz in the German Würzburg school after the turn of the twentieth century. In Part 3, Pieter van Strien and Erik Faas draw on newly available archives that indicate that Selz never received proper credit for his seminal ideas for both personal and professional reasons, which included his persecution and untimely death during the holocaust. Selz advanced the theory that familiar, goal-directed actions are guided by "anticipatory schemas" that involve the activation of underlying motor programs. "Productive thought" requires devising new methods of creative problem solving that go beyond an existing

cognitive repertoire. Selz adopted a position that differed fundamentally from association psychology and proposed a structural holism that strongly influenced gestalt psychologists, such as Koffka, who failed to credit Selz's influence. Selz's ideas had a greater immediate impact on educational policy in both Germany and The Netherlands.

The authors contend however, that the philosopher Karl Popper could have contributed to a revival of Selz's psychological theory had he explicitly acknowledged that he had adapted Selz s ideas to fit his theory of inquiry that hypotheses must be testable and thus falsifiable by experience. Not until the early 1950s did Selz receive the recognition denied him during his lifetime. That is when the pioneers in cognitive psychology Herbert Simon and Alan Newell credited Selz with having inspired their information processing theories of human and machine intelligence. Van Strien and Fass provide fresh insights why Selz's theory was rediscovered despite undergoing a process of appropriation that nearly erased personal credit for his subsequent influence.

Thomas Dalton and Bernard Baars examine and assess why the scientific study of mind and consciousness, which have been central topics of philosophical analysis for centuries, was revived after nearly being extinguished by the behaviorist movement in America at the turn of the twentieth century. Philosophers and scientists have always been divided about whether consciousness is an objective phenomena that can be studied scientifically or a subjective one only accessible to the individual who experiences his own mental and bodily states. William James believed in the scientific importance of conscious volition and attention. However, the behaviorists successfully terminated, for several decades, scientific resolution of this debate by dismissing consciousness as unsuitable for scientific study.

Through his connections to the Josiah Macy Jr., Foundation, pragmatist co-founder John Dewey contributed to the revival of the interdisciplinary scientific analysis of mind and consciousness. In the early 1940s, Dewey and his foundation collaborators sponsored a series of conferences that included cybernetics, the brain and consciousness, which encouraged scientists to confront their uncertainties, reexamine their assumptions and find new methods to make the mind accessible through inquiry. Dalton and Baars trace the subsequent political and professional strategies undertaken by veterans of the Macy conferences from the 1950's to early 1980s to secure financial support, increase participation of key scientists in research and gain institutional recognition of mind and consciousness as valid and credible objects of scientific analysis. The authors also examine the theoretical and technical breakthroughs in the early 1990s that catapulted the study of consciousness into the cultural mainstream as an interdisciplinary science

capable of testing rival theories and contributing to human well being and betterment.

Pragmatism, Development and Social Psychology

Dewey believed that experience played a central role not only in the integration of brain and behavior but in the construction of social communities dedicated to intelligent action for the public good. In Part 4, Thomas Dalton and Sheldon White examine the challenges that Dewey, his colleagues and collaborators experienced in demonstrating how these two fundamental and complementary dimensions of pragmatism could be studied scientifically and theoretically unified.

Dalton describes infant experimentalist Myrtle McGraw's little known collaboration with Dewey in the 1930s to determine whether the pattern of inquiry that Dewey proposed in *Logic: The Theory of Inquiry* was foreshadowed in biological processes of growth and development. He predicted that her work would "revolutionize the field" of child development. Nevertheless, McGraw was never able to publicly benefit from her close association with her famous mentor because that relationship was confidential. Her innovative methods of special stimulation and studies of early motor development have inspired research by contemporary experimentalists. But only recently has neuroscientific research corroborated her contentions that early experience contributes to brain growth and the rapid expansion and acquisition of motor and cognitive skills. But McGraw has never been able to escape completely from the shadow of neural maturationism (i. e., that brain development precedes behavioral development) and the nature versus nurture debate that continues to stalk her work. These paradoxes are worth examining, because they enable us to retrace the intellectual and professional pathways through which McGraw's developmental theories have been interpreted and contested and to examine McGraw's novel strategy to counter misrepresentations and rectify her own blunders.

As noted before, modest but steady progress has been made in understanding how humans and apes perceive, process and communicate differently their awareness and knowledge of other minds. But Sheldon White argues that our uniquely human capacity to use language to interact in socially and morally significant ways is an important phenomena that enables us to understand the relationship between individual and social development. Even before the fall of behaviorism, White observes that there were several attempts by philosophers and psychologists that included Josiah Royce, John Dewey, Floyd Allport, James Mark Baldwin and George Mead to provide the outlines of a social psychology. But White contends that Dewey's efforts and that of his colleagues were disregarded

and subsequently "unremembered" by their successors in 1960s who desperately needed an experimental social psychology to make Head Start and other Great Society initiatives truly successful. White suggests how a new social psychology can overcome the problem of subjectivity, which besets attempts to connect individual and group processes, by establishing "social proof structures" that enable intersubjectively validated judgments and assessments.

Career Pathways and Professional Impact

The choices made early in a career about what to study and publish (see Simonton's chapter) and what strategies to adopt to fulfill professional ambitions can strongly influence ultimate attainments and eventual recognition. In Part 5, Kathy Milar (and see Robert Wozniak's chapter in Part 1) examine the distinguished but troubled careers of Helen Bradford Thompson Woolley and James Mark Baldwin. After showing great promise early in their careers, for different reasons they became mired in personal difficulties that adversely affected their professional reputations. This part will focus on Woolley's career.

The fields of child development, social and school psychology attracted several women who were pioneers and innovators. A few gained recognition for their contributions. Nancy Bailey, McGraw and Lois Murphy come to mind in child growth and social development. However, Milar examines why Helen Bradford Thompson Woolley does not evoke the same name recognition, even though she made several important contributions to psychology when the field was just emerging. Her accomplishments are impressive. Woolley was one of the first generation of American women to become an experimental psychologist. She conducted the first experimental investigation of sex differences in psychological characteristics and was one of the first psychologists in the United States to be employed by a public school. She was the first psychologist and the first woman to serve as president of the National Vocational Guidance Association and also was a leader in the nursery school movement of the early 1920s. It is perplexing nevertheless, that a former Dewey student at the University of Chicago and contributor to Dewey's classic book, *Studies in Logical Theory* would not enjoy enduring recognition for her subsequent accomplishments.

Woolley is best remembered among feminist historians for her sex differences research which formed only a very small part of her whole career. While there are a few good brief accounts of her life, Milar is critical of these and other accounts because the authors either have underestimated her accomplishments or distorted them. Milar provides a more complete

description of Woolley's life and work and discusses some of the reasons for her obscurity. While some of the factors influencing the absence of women scientists from histories of their disciplines are pertinent to understanding Woolley's relative obscurity, Milar contends that Woolley's little known mental illness played an important role. Woolley's difficulties coping in a professional manner with her illness and the silence about it among her closest colleagues cast a shadow over her accomplishments. Milar's chapter is an attempt to break that silence and recover the story of this complex, brilliant and tragic woman.

The contributors to this volume document the lives and works of pioneers in the field of psychology with whom undergraduates and graduate students should be acquainted in the course of their studies. Teachers of introductory and advanced courses in the theory and history of psychology will find this book attractive, because it describes and illustrates how the foundations of a discipline are constructed and remodeled over time through the intellectual innovations and strategic interactions of its most visible leaders. Scientists and scholars who want to know more about the roots of their discipline also will find this volume useful. Here they are exposed to different perspectives about how a field of knowledge and practices are formed and reshaped over time to address unanticipated problems and issues that require the adoption of new theories and methods of inquiry.

There are a number of texts that have examined the history of psychology from the point of view of its intellectual history, but rarely do the authors examine underlying patterns that involve the interplay among prominence, professionalization and organizational development. There is an emerging but modest scholarly literature that addresses themes and issues that are the focus of this book. These works include Evans et al. (1992) collection on the history of the APA and (Dewsbury's, 1996; 1997) volumes that examine the history of the divisions of the APA. Ever since Ben-David and Collins' (1966) sociological analysis of the intellectual and professional origins of American psychology, psychologists have been challenged to mount a critical response and to present an alternative perspective by those who have been trained within the discipline (see Ross, 1967 and Danziger, 1979). Book length studies by psychologists that put prominence in a theoretical and historical perspective similar to that contemplated by the editors and contributors include the classic work by Coan (1978) and a more recent book by Simonton (2002), who is also a contributor to this book. *The Life Cycle of Psychological Ideas* promises to stimulate a renewed interest among psychologists in their roots, the pathways to success and the processes through which the field has been transformed since the turn of the twentieth century.

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Part 1

Reconstructing Psychology's Founding and Growth

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1

NEW GROWTH FROM PHANTOM LIMBS

TENUOUS ATTRIBUTIONS TO OUR PREDECESSORS

Rand B. Evans

INTRODUCTION

The question posed in this chapter is "Have we need of founders." That turns out to be two questions. One is "Are founders essential for understanding the development of movements and ideas in science?" The other is "Have we a purpose for founders?" Part of the answer to the second question depends, in part, on who "we" are. These questions come at the intersection of two old but relevant notions of historical progress: naturalistic vs. personalistic views of history. These issues will be explored first with Wilhelm Wundt and the founding of experimental psychology and then with G. Stanley Hall and William James and the founding of American psychology. A third issue that will be addressed has to do with the complicating factor of historical figures themselves believing in personalism and in founders. In some cases one can see their behavior and the behavior of their followers influenced by these beliefs.

ORIGIN MYTHS

The phrase, "origin myth" describes much of the lore that pervades the historiography of science about the beginnings of movements or of ideas. Origin myths develop from a mixture of erroneous or imperfect oral tradition, misinterpretation, idealization, oversimplification, suppositions about what "must have happened" and even plain and simple fabrications about the past.

Oral Traditions

One source of such mythology comes from received oral tradition. Every student has stories about his or her professors. These may be from direct experience but just as likely from graduate student lore handed down from generations of other graduate students. The professors themselves are a source of this lore. The professors were once graduate students and have the lore of their professors and laboratories. Embellishment, misattribution, ellipsis and other transformations of memory are major sources of imperfect and often erroneous information about the beginning of things. The compounding of such stories through generations of interpreters often results in versions of events that bear little relationship to documentary evidence. Such lore often finds its way into print by way of autobiographies, obituaries and "personal communications" cited in articles, as well as in historical textbooks of the discipline.

Textbooks

Textbooks, particularly histories of disciplines, are often a source of such misinformation. The textbook writer's job is to take a bewildering collection of events and ideas and write some digestible summarization by which readers can make sense about the past of their discipline. It is easy for historians who write primarily for other historians to forget that most people read the history of a discipline to find out how it all began and how things got from there to here. That runs directly against the grain of much of "professional" historiography, but it is true, nevertheless, and will probably continue to be true. There really *is* more than one way to write history and more than one purpose in its writing. The heuristic element in history textbooks often leads the writer to add interesting bits of lore that make the figure more real to the reader. Since few scholars have the range of an entire discipline for their expertise, it is not unexpected that the textbook writer will depend on other sources for the accuracy of information, without being able to track down every tale for its documentary sources.

Textbook writing is a daunting task under the best of conditions, but it is particularly so when brevity seems more on the minds of editors than detail and precision in content. The demand for brevity and its resulting broad stroke renderings has become for historiography in all fields, the besetting sin of our age. It is not only in textbook writing that we find these faults. The same proclivities are found in scholarly works by highly revered historiographers. The cause of treating complex subjects accurately is not helped by 15 or even 25 minute paper sessions at conferences or 1200 word journal articles or 10 page book chapters. To render anything but the simplest subject into such bite size portions works against the communication of the subtleties of complicated stories. The historiographer's heuristic shorthand and can give only the most basic impressions of events and people and leads to oversimplification and to pat stories. Both are conducive to creation myths and other similar manifestations. A recent history of psychology text covers the entire subject from ancient Greece to the present in 358, rather small, printed pages. The discussion of Plato gets two pages but Aristotle fares somewhat better with 4 pages. The mind boggles.

Originators and Being First

Perhaps another reason origin myths develop so easily has to do with human nature, itself. Francis Bacon warned us that humans are prone to "presuppose the existence of more order and regularity in the world than it finds." (Bacon, 1620/1861). This seems to be a real human tendency and affects our views of foundings and founders in that we tend to assume that every current idea or movement must have a clear-cut beginning somewhere and some identifiable initiator to go with it.

It is not surprising that a major source of creation myths comes from considerations of "firsts" and "founders." A discussion of "firsts" is like a discussion of baseball statistics. If you add enough conditional clauses to a "first," many candidates can be made to fit the honor. Since founders are typically those who have done lots of firsts, the problem is just compounded.

HAVE WE NEED OF FOUNDERS?

Wilhelm Wundt

The question of whether we have need of founders is really two questions. One is "Are founders essential for the understanding of movements and ideas in science?" The other is "Have we a purpose for

founders." Over the past 20 years, historical revisionists, social constructionists, deconstructionists and other "ists" have made considerable criticism of the traditional selection of founders and sometimes of a need for founders at all. In the late 70s, for instance, Kurt Danziger wrote on the "Social Origins of Modern Psychology." In that article, Danziger argued against Wilhelm Wundt being credited with the founding of experimental psychology. Danziger said, in part, that Wundt

> . . . is a singularly inappropriate figure to choose as the originator of the modern psychologist's professional identity. He was in fact a strong opponent of the separation of psychology from philosophy, holding that the most important problems in psychology were so closely connected with philosophical problems that a separation of the two would reduce the psychologist to the level of an artisan imprisoned by a covert and naive metaphysics. . . . As for his supposed contribution in making systematic experimental work part of the psychologist's role definition, it should be remembered that, for Wundt, experimental psychology was but a small part of his life's work. In any case, the example of someone like Helmholtz shows that systematic experimental work on psychological problems has no necessary connection with the elaboration of the role of professional psychologist.
>
> It was the generation that succeeded Wundt, which first conceived a distinct professional identity for the psychologist. The first effective steps in that direction were not taken until the closing years of the nineteenth century and the early years of the twentieth. Those who took those steps, parvenues that they were, had reasons of their own for seeking to shine in the reflected prestige of an established figure like Wundt. In doing so they laid the foundations for what, yet another generation later, was to be come a full blown "origin myth." (Danziger, 1979, p. 31).

There is much to disagree with in Danziger's assessment of Wundt, but I would agree with him on one point, that Wundt's students used his name and reputation for their own purposes and that of the new experimental psychology. Some of them called themselves or allowed themselves to be called Wundtian, even though they may have shared few of the ideas of the master. E. B. Titchener, for instance, wrote in 1918 that:

> Wundt is, I take it, the very first large figure in the history of thought who is *temperamentally* psychological. I thought I saw this as long ago as 1888, and I have confirmed my opinion again and again. I believe that Wundt's generalizations are mostly wrong; I do not at the moment recall any one of the larger ones that I accept today—though I have in my time swallowed most of them; but I still affirm that Wundt's *instinct* is psychological, even where it leads him astray . . . ; and he still

> seems to me to be the one man among the "philosophers" of whom this can be said. That is his importance: not the special things he teaches. (Titchener, 1918, p. 215)

The revisionist historians of 20 years ago could not understand why Titchener failed to reject Wundt and his views openly. Had they but read the corpus of Titchener's work, they would have found plenty of examples of open disagreement with Wundt's positions. But, open disagreements or not, why should Titchener reject the caché of Wundt's reputation? He had transcended Wundt's teachings but still felt a part of the tradition Wundt represented. It is clear that Titchener and others used Wundt's reputation for their own purposes and saluted the uniform more than the currency of the ideas.

William James

We can see a similar phenomenon in late 19th century America concerning William James. The psychologists who were working to professionalize American psychology in the late 19th and early 20th century seized upon William James as founder and high priest of the new, experimental psychology, although James held virtually none of the values of that movement and was actually hostile to many of them. I do not dispute the importance of James, of the provocativeness of his speculations, or of the popularity of his writings. But, why is it James was given the status of prime mover of psychology in America? For that matter, why do we need a founder at all?

I suggested many years ago that rather than having a founder or founders, one can just as easily see the emergence of the new psychology in America as the result of the steady and gradual diffusion of psychological thought from the intellectual and mental philosophical thought of the early 19th century. Ideas coming from overseas gradually were integrated and formed over the years several strains of thought that would evolve into the various camps in evidence by the 1880s. (Evans, 1984) Decades earlier, intellectual and mental philosophy can be seen to have emerged from the matrix of moral philosophy in quite the same way, without the need for a founder. The processes of intellectual diffusion that brought the new psychology forth from this matrix were similar to those that brought political science, anthropology and sociology out of the same matrix.

I suggest that one of the processes leading to this evolution was the rise of the textbook in American colleges, particularly in philosophy. Psychological thought can be found in the American college curricula at least to the early 18th century. I have argued, along with others, that it is through

the shift from original sources to a dependence on textbooks, particularly in the 19th century that the differentiation in academic subject matter begins. The dependence on textbooks led to defining courses in terms of the textbook content. The course content led, at least in part, to the differentiation and crystallization of disciplines that we see by the 1880s. (Evans, 1984; Bryson, 1932; Snow, 1907). There were other forces as well, of course, but one need not single out a founder of the feast to explain the process.

Projecting this analysis into the 1880s, it can be argued that no one individual founded the "new psychology" in America. It can be seen as a progression of qualitative changes influenced by ideas from within and without. In terms of the "new psychology," William James contributed his share of influence through his *Principles of Psychology* and his articles. He helped demonstrate the possibilities of a naturalistic psychology, but not necessarily an experimental psychology. Is this enough to make him the founder of American psychology?

It has been more than 20 years since Danziger wrote his evaluation of Wundt as a founder. It has been almost as long since I wrote my argument about James and American psychology. Even so, Wundt is still considered the founder of experimental psychology by just about everyone who recognizes his name and William James stands unchallenged as the founder of American psychology. I wager the same will be the case 20 years from now. Inertia is great when it comes to enthroned founders. It is even greater when one tries to argue against the need for founders. The personalistic theory of history, the "great man" theory, is still very entrenched. There exists a need for gods, saints and founders. While this perhaps less true among professional historians, it still remains generally true among scientists themselves. So, if we must have founders, how do we come by them?

HOW WE IDENTIFY FOUNDERS

Neither Wundt nor James were representative of what their disciplines became nor of the disciplines from which they, themselves, emerged. They were both transitional figures. Founders, real or imagined, usually are. E. G. Boring once described a "great" as being similar to the horn of a rhinoceros. It is a feature that is not vital for the creature's existence or survival, but it is the feature by which we identify it. (Boring, 1950). If this is true, then it should not surprise us to find founders identified at times of accelerated changes in the progress of a discipline. If such is the case, how do we distinguish founders from all the other figures of the same time?

Danziger seems to have based his rejection of Wundt as a founder more on Wundt's values rather than his actions. So it appears that Danziger holds that founders epitomize the values of a particular movement or idea. If this were an accurate assessment, then Wundt would fail the test. Wundt was not willing to separate experimental psychology from its philosophical apron strings, although many of his students were eager to do so. Wundt did not believe the experimental method (in terms of introspection) was applicable across the breadth of the subject matter. Wundt had to have two psychologies, one for the lower mental processes which was experimental (largely introspective) and one for the higher mental processes which used other methods of investigation. Many of the next generation used Mach's positivism to unify experimental psychology within one method. Wundt was not positivistic, at least in the sense of Mach. Wundt required accessory concepts and doctrines that the next generation largely ignored. The list goes on.

Even if it were true to say, as Danziger does that experimental psychology was not the center of Wundt's world view, this would not disqualify him as a founder. It seems to me that we typically identify founders by their actions concerning the object of their founding, not by their global interests or values. So Wundt gets the credit for several actions that have to do with the structure of the discipline. He established what is generally considered the first research laboratory in the field. He produced the first doctoral student with a laboratory thesis in psychology and started a journal in which experimental studies were published from his laboratory. He designed several instruments and methodologies that would be come standard in the field, and published a vast number of significant articles and books promoting the experimental agenda. He also produced a large number of laboratory trained students who went on to found laboratories in their own countries. These are just to name the most obvious of his actions. That Wundt was not supportive of German professional societies, a negative example that Danziger uses, is hardly sufficient to negate all the other actions of Wundt as a founder.

As I said earlier, these "firsts" are like baseball statistics, so one should take that admonition in mind and have some sense of humor when dealing in these matters. Just about all of these "firsts" I have credited to Wundt have been the source of discussion and conflict among historians in the field and I am not looking to add any more to that already lengthy literature. The fact that Wundt did not hold to all the values or methods of second generation psychologists should not surprise us. After all, he was *not* a second-generation psychologist. As a genuinely transitional character, he could move only so far into the new discipline and no farther. At least in part it was because of Wundt's structural and academic/political activities that

generations of psychologists and historians of psychology credit him with the founding of experimental psychology and not Fechner or Helmholtz. Wherever things were happening, Wundt seemed to be there.

IS WILLIAM JAMES THE FOUNDER OF AMERICAN PSYCHOLOGY?

If we must have a founder of American psychology, why is it William James? The criterion of values that Danziger uses against Wundt may also be used against William James as the founder of American psychology. He had all the negative characteristics Danziger mentions about Wundt regarding shared values with the second and third generation experimentalists. James did not like the laboratory or laboratory work and, even in his *Principles of Psychology* demonstrated his preference for the traditional philosophical method. He openly criticized the laboratory and its findings. He did few experiments in his entire career and, after publishing the *Principles*, turned his appointment from Professor of Psychology back to Professor of Philosophy and turned away from psychology to philosophy. Hugo Münsterberg, writing to J. McKeen Cattell in 1909, said that he felt James had done nothing for psychology for the previous decade (Münsterberg, 1909).

James also seems not to have carried out many actions necessary for developing and promoting the new psychology. Of course, there was James' laboratory at Harvard. It was an informal demonstrational laboratory established around 1875 that James later touted in the 1890s as the first in America, though James could not remember just when it was established. (James, 1895) Even Cattell, (1898), who was firmly in James's camp, admitted that James' first lab at Harvard was for physiology demonstrations. The research laboratory in psychology at Harvard really did not get into operation until the early 1890s and then primarily under the direction of Hugo Münsterberg.

In terms of the actions one ordinarily expects of founders, James has little to show with the exception of his *Principles of Psychology*. Was that enough? J McKeen Cattell seems to have thought so. In 1898, Cattell, spoke before the American Association for the Advancement of Science, and said that:

> The history of psychology here prior to 1880 could be set forth as briefly as the alleged chapter on snakes in a natural history of Iceland—"There are no snakes in Iceland." (Cattell, 1898, p.12).

Cattell went on to say that

> . . . the land lay fallow and twenty years ago the seed was sown. James, at Harvard, began the publication of a series of striking articles, culminating in the issue, in 1890s, of the *Principles of Psychology*, a work of genius such as is rare in any science or in any country.

Cattell (1929) repeated his statement in slightly different form as late as 1929.

So, why James as founder? I suggest that, just as with the case of Wilhelm Wundt, the second and third generation of American psychologists needed a founder for academic and professional political reasons.

In the professionalization of a discipline, a "founder," treated as a standard or banner, is very useful in rallying together otherwise rather disparate individuals and groups. The professionalization of a discipline or group, in part, involves creating a self-identity, drawing a line between "us" and "them;" between our group and "theirs," between the receding past and the progressing present. To crown a founder is to recognize that a new epoch has begun. The distinction the new laboratory psychologists needed to make was between the psychology of the laboratory and that of the mental and intellectual philosophers who were still in evidence in 1890s American colleges.

In the 1890s psychology programs were still, by and large, housed in departments of philosophy. There was a growing stress between the philosophical and experimental psychologists. Laboratories were expensive, for one thing, and the proportion of departmental budgets going to experimental work became greater and greater. But psychology in America was growing faster than philosophy, which never had the strength in America that it had in European academies. A break was inevitable.

An important strategy in professionalization is to make a clear break with the past and to make small differences appear to be large ones. To do this a real distinction had to be made between the old "armchair" psychologies of mental philosophy and intellectual philosophy and the "new psychology" of the laboratory. In doing this, the psychologists were separating themselves from their colleagues within departments of philosophy and at the same time laying claim to the subject matter previously in the philosophers' domain. In their rhetoric, the psychologists interested in the laboratory in the 1890s were making the term psychology synonymous with experimental psychology and requiring the line of thought formerly called psychology in philosophy programs to take the adjectival modifier, philosophical psychology. Cattell was making just such a distinction in his 1898 address cited above.

Most of the earlier philosophical psychologies were tinged with the lingering concept of soul. James' distinction between mind and soul and

his banishment of soul from consideration as a psychological concept in his *Principles of Psychology*, provided a criterion to differentiate the new experimental psychology from the older philosophical psychology. James' "naturalistic psychology" though not necessarily an experimental psychology, was sufficient, if not ideal, to make him the symbol for the new experimental psychology in America.

But this is a pat answer, incomplete at best in understanding what was going on in that decade of the 1890s. If there were no alternatives to William James for founder, this explanation might suffice but such is not the case. A deeper analysis reveals the importance of partisanship in creating a founder for professionalization purposes. There was someone contemporaneous with William James who appears to have done everything one could ask to qualify as a founder. That person was G. Stanley Hall. Why isn't Hall the founder of American psychology?

PERSONALISM AND THE RIVALRY OVER AMERICAN PSYCHOLOGY'S FOUNDING

William James and G. Stanley Hall

The belief in "greats" and "founders" is deeply engrained in human thought. The story of the "founding" of American psychology involves the competition of two men who deeply believed in the concepts of "greats" and thus a personalistic theory of history and who were at the center of currents of academic and professional politics: William James and G. Stanley Hall. I suggest that James was installed in the position of founder by partisans, at least in part, to prevent it from going to G. Stanley Hall.

If some present-day historians do not believe in the concept of "greats" and founders, G. Stanley Hall and William James certainly did. Hall would later write a book titled *Founders of Modern Psychology*, dealing with major figures in Europe he knew in two forays as a student there. It included chapters on Zeller, Lotze, Fechner, Hartmann, Helmholtz and Wundt. The fact that even though the book was published in 1912 there is no mention of a founder of American psychology and no mention of William James, with whom Hall studied at Harvard, speaks volumes.

James was also a believer in personalism. The power of the will was basic to his psychological view. It can be argued that Hall was a great self-promoter while James was content to let his friends praise his achievements for him. Yet, as we will see, James could be stirred to indignant and public attacks if his precedence was questioned.

Hall was an ambitious man. In the 1880s and early 1890s Hall had reason to believe he was the founder of American psychology or was close to it. If we revisit some of the things that we traditionally credit Wundt with in his "founding" of the new psychology, we can see why Hall might have believed as he did.

Hall opened what is arguably the first research laboratory in psychology in America, at Johns Hopkins in 1883. He later called it the "first Wundtian laboratory in America." (Hall, 1912) Although not officially recognized by the university, Hall's laboratory was, in fact, an operating research laboratory devoted to psychological research. He founded the first journal in English devoted specifically to psychology, *The American Journal of Psychology*, in 1887. He graduated the first doctorate in psychology in America, Joseph Jastrow. Hall argued more vigorously for the laboratory than James, for the use of the experimental method, naturalistic explanation, and produced more doctorates in psychology than James. He founded a department of psychology at Clark University and one of the best laboratories in psychology in the world at that time. So why isn't Hall considered the founder of the new psychology in America? He certainly meets the both the values and activities criteria.

Quite apart from the brilliance of James' *Principles*, I think Hall lost the race because of some of the very acts that should have made him the founder of the new psychology in America. Through a series of unwise and infuriating acts he squandered the capital he had built up by his positive actions and values. These matters go back to some events involving Hall, James, J. McKeen Cattell, J. Mark Baldwin and a few others in the early 1890s. If Hall did not mean to be the founder of American psychology, his actions certainly belied the fact. It is as though Hall had a checklist about what it took to be a founder. He had most of the requisites already when he founded the *American Journal of Psychology* 1887. That gave him the highest exposure of any psychologist in America. *The American Journal of Psychology* became Hall's bully pulpit to attack or praise the books and articles appearing in psychology in the late 1880s and to promote psychology as he saw it by publishing experimental studies and the summaries of experimental studies from other journals. As the only mainstream psychological publication in America, the editorship also gave Hall power to select or reject articles. Hall's book and literature reviews brutally attacked the older philosophical psychologies still being produced Had he reserved his attacks on Borden Bowne (Hall, 1887b), James McCosh (Hall, 1887a) and others of that waning generation of philosophical psychologists, he might have escaped retribution, but he seems to have reserved particularly tough and even vicious attacks for the other two senior American "new" psychologists of his time, G. T. Ladd (Hall, 1887c) and William James (Hall, 1892).

These and other attacks on authors in the new literature as well as Hall's capricious selection of articles for the *American Journal of Psychology* caused a stir of resentment initiating a series of events that would eventually lead to the destruction of Hall's credibility. (Ross, 1972, 231–237)

In 1892, Hall founded the American Psychological Association. This was a major coup for Hall and largely completed the checklist for the things-one-does to become a founder of a new discipline. It was something even Wundt had failed to do. It was, at the same time, the beginning of the end for Hall's hopes to be the leader of the new psychology in America. Instead of giving him the institutional structure to shape the professionalization of the new psychology in America, it fell instead into the hands of his opponents to do the same thing. Hall was elected as first President of the APA and presided at its first formal meeting. That should have capped Hall's position as undisputed leader of the new psychology in America. However, Hall did not realize he would have to contend with two very ambitious second-generation psychologists, J. McKeen Cattell and James Mark Baldwin. That first formal meeting of the APA was anything but a coronation for Hall. The meeting was marked by controversy and challenges to Hall's editorship of the *American Journal of Psychology* by Cattell and Baldwin. More will be said on the controversy later. The flap at the meeting was so public and rancorous that Hall did not attend the second meeting of Association. That next year George T. Ladd was elected President and in 1894 William James was elected president with J. McKeen Cattell as secretary. For the remainder of the decade the presidency was filled with detractors of Hall and supporters of James, such as Cattell in 1895, Fullerton in 1896, Baldwin in 1897, Münsterberg in 1898 and John Dewey in 1899. Hall soon found himself shut out of his own organization and rarely attended the APA for the remainder of his life.

The uproar at the first meeting of the APA had to do with Hall's management of the *American Journal of Psychology*. Cattell and Baldwin were dissatisfied and perhaps a little jealous of the control Hall had on the publication of articles in the United States through the *American Journal of Psychology*. The *Journal* was Hall's property and not that of the APA and so it is rather incredible that Cattell and Baldwin would give Hall an ultimatum either to share the editorial power with them or they would start a new journal that would compete or supplant it. Hall, not surprisingly, refused to share the *Journal* with Cattell and Baldwin, though he tried to find compromises. Cattell and Baldwin refused to consider any of Hall's compromises, so, true to their word, they established the *Psychological Review* in 1894. The two journals effectively split the psychological community, with Cattell and Baldwin drawing William James and Hugo Münsterberg in on his side and Hall drawing in E. B. Titchener and his coterie in on his.

The animosities created by this division would echo for many years in the American psychological scene. (Evans and Scott, 1978).

Initially, James appears to have had little knowledge of what was going on and did not think highly of Cattell's behavior or influence. When he learned of it, he expressed horror at what was being done, often in his name. Still, James did nothing to stop it and finally threw his considerable reputation behind the *Psychological Review*. An added humiliation for Hall was that the proceedings of the APA were not even published in Hall's journal. They were published by order of the Board in the competing *Psychological Review*.

Hall, acting defensively to the controversy and perhaps reacting to the prominence James had attained, merely made things worse for himself. In 1895, Hall reorganized the *American Journal of Psychology*. Hall now shared the editorship with Titchener and several others who had been supportive during the fight with Cattell and Baldwin. In that first issue, Hall wrote a remarkably self-serving editorial promoting himself, without explicitly saying so, as the founder of the new psychology in America. He claimed among other things, that he founded first laboratory in America and that it was one of his students who founded the first psychological laboratory at Harvard. Both statements were direct challenges to James' precedence. What Hall was thinking of as the first laboratory was of his research laboratory at Johns Hopkins rather than James's demonstrational laboratory at Harvard. He was also thinking that his student, Herbert Nichols, was at Harvard during the hiatus between James' relinquishing laboratory instruction and Münsterberg's arrival to take it over. This laboratory that was opened then was the one Hall and most others considered the first real research laboratory at Harvard. Hall was not counting the earlier laboratories since they were demonstrational rather than research laboratories.

Hall's editorial resulted in an angry letter from James with a demand for a retraction and an apology. Cattell seized on this issue and letters followed from Cattell, Baldwin and many others making Hall seem like a megalomaniac. Cattell managed to have the letters published in the magazine, *Science*, as well as in the *Psychological Review*. It was a major humiliation for Hall and his credibility would never recover. (Ross, 1972, 242–250). Remarkably, only 3 years later, Cattell would admit to Hall's precedence in terms of a research laboratory and admitted that James' early laboratory was for physiology. (Cattell, 1898, p. 110). That came too late, of course, James had already won the the battle for priority.

It was this same year that this literary flap occurred, 1895, that Cattell made perhaps the earliest of his praises of William James' priority. Cattell claimed for James' *Principles of Psychology*, that it "breathed the breath of

life into the dust of psychology," (Cattell, 1896, p.61) and identified his work with the new experimental psychology.

Why Hall, who had done virtually all the things necessary to be a founder was rejected and James was accepted, who had done little if any of the organizational things that would credit a founder can be answered at least partially in terms of the sway James' camp held in the American psychological scene by 1898. With James' allies in control of the APA, the *Psychological Review* and *Science*, much of the opinion making of the small population of American psychologists was definitely slanted toward William James and away from Hall. From then on, Hall was considered by many American psychologists to be a bit of a charlatan, a self-promoter and a difficult person to work with. James was represented as a gentleman, an honorable and brilliant scholar, virtually a psychological saint. Such are the images that create mythology. While these are not necessarily incorrect assessments of the two men, they are certainly exaggerations in both cases. These are the images that have been recorded in the lore of psychology ever since. Even today one criticizes James with care lest some of his present day admirers take umbrance. By 1898, the victory of James's followers was complete. The war was won and, even though William James had really left the new psychology for philosophy by then, he continued and continues to be viewed as its founder in America.

SOME LESSONS ABOUT FOUNDERS AND HISTORIOGRAPHY

One lesson that might be gained from all this is we can probably write histories of science without relying on the shorthand device of founders and other pat stories. A second lesson is that the limitations of publication, conference papers and textbooks will continue to press for the kind of brevity that promotes the use of explanatory shorthand concepts, such as founders. If historiographers do not write of founders and foundings, the lore of oral transmission within scientific disciplines will continue to be filled with them. Former students pass on the lore heard from their professors to the next generation as gospel. That received oral tradition often finds its way into print and is rife with origin myths and other pat stories. The belief in "greats" and founders runs very deep. People have believed in them for centuries, long before the "great man" or personalistic theory got its name in the 19th century.

A third lesson is that the concept of founders, like that of "greats," can be useful in the professionalization of a discipline or of schools of thought

within a discipline. Whether or not founders is a legitimate concept, it is certainly a useful one.

A fourth lesson is that, because personalism is such a deeply ingrained belief, the significance of being a founder has not always been lost on the individuals involved in the events themselves. For many of these individuals, their place in posterity was just as important to them as their contemporary fame.

Voltaire's famous axiom, "If God did not exist, it would be necessary to invent him ," works for founders as well. Perhaps a corollary should be added to that statement: If we did not invent founders, there would be volunteers for the honor.

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2

LOST CLASSICS AND FORGOTTEN CONTRIBUTORS

JAMES MARK BALDWIN AS A CASE STUDY IN THE DISAPPEARANCE AND REDISCOVERY OF IDEAS

Robert H. Wozniak

Textbook intellectual "history" of science has traditionally been written as a story of innovation, conflict, and relative survival. In stories of this sort, the vast majority of ideas (concepts, theories, methods) are portrayed as having been born, living for a period, and proceeding inevitably to eventual demise through revision or replacement by newer ideas deemed to be more sophisticated and/or more adequate. Amidst this flux, a very few, special ideas somehow manage to evade the common fate, surviving into the modern era, far beyond the years of their creation. The works in which these rare ideas first appear come to be known as "classics" and their authors dubbed "major contributors."

Within psychology the paradigm for this linear, incremental, survivalist approach to history, echoed more or less by E. G. Boring (1929) in his famous *History of Experimental Psychology* and the many textbook writers who have followed him, was established by Théodule Ribot (1879). In his *La Psychologie allemande contemporaine*, translated into English in 1886, Ribot traced the rise of the "new experimental psychology" from Immanuel Kant through Johann Friedrich Herbart and Gustav Theodor Fechner to Wilhelm Wundt.

Kant (1786), according to Ribot, "ventured to predict 'that psychology could never be raised to the rank of an exact natural science"' (Ribot, 1886, p. 43) because mathematics is inapplicable to internal phenomena varying in only a single dimension, time, and because internal phenomena are inaccessible to experiment. Herbart (1824/1825) then responded to the first of Kant's criticisms, arguing that ideas vary not only in time but in quality and intensity and applying mathematics to this analysis. And Fechner (1860) responded to the second by demonstrating that mental events can be systematically manipulated as a function of variation in physical events and measured via the just noticeable difference. Finally, Wundt brought the "new" psychology to fruition by providing it's first self-reflective discussion of scientific methodology in the *Beiträge zur Theorie der Sinneswahrnehmung* (1862), it's first experimental handbook, the *Grundzüge der physiologischen Psychologie* (1873/1874) and, in 1879, it's first experimental laboratory.

In later histories, building on Ribot but focusing largely on developments in the United States, the received story takes much the same birth, revision, replacement, survival form. Edward Bradford Titchener's (1896) "structuralist" analysis of consciousness via systematic experimental introspection is portrayed as representing the Wundtian approach in America. "Functionalism" from William James (1890) and John Dewey (1896) through James Rowland Angell (1907) is typically construed as prospering in opposition to structuralism and as culminating, when taken to its logical extreme, in the "behaviorist revolution" of John B. Watson (1913). Rejecting both consciousness and introspection, Watsonian behaviorism is said to have swept away the last remnants of the Titchnerian opposition, only to fall victim itself, in its oversimplifications, to the more sophisticated neobehaviorisms of Edward Tolman (1938), B. F. Skinner (1938), and Clark Hull (1943) and to a certain extent to the criticisms of gestalt psychology (e.g., Koffka, 1935). Finally, neobehaviorism is portrayed as having then itself been overthrown and gestalt psychology largely co-opted in the "cognitive revolution" of the late 1950s and early 1960s—a revolution that is presumed to have established the parameters of the field as it currently exists (Gardner, 1985).

This sort of rational reconstruction makes a good story. It is relatively easy to understand and remember; and it serves as an intellectual scaffold on which to hang the many names, concepts, and dates that threaten to overwhelm the beginning student. Unfortunately, it is also little more than a caricature that does considerable violence to the complexities of history. Although like all caricatures, it contains a kernel of "truth," some of the more interesting historical work in psychology of the past thirty years has

been devoted to unpacking, deconstructing, and overturning the origin myths perpetuated by traditional histories of this sort.

What does it mean to say that Wundt founded psychology's first laboratory in 1879 (Bringmann, Bringmann & Ungerer, 1980) or that he championed the experimental method (Danziger, 1980; Wundt & Diamond, 1980)? To what extent was Titchener really a Wundtian (Blumenthal, 1975, 1980; Tweney & Yachanin, 1980)? What are some of the unrecognized sources of functionalism (e.g., Shook, 1995)? Did Watson trigger a behaviorist revolution (Samelson, 1981)? Did the early behavorists reject consciousness and mentalisms or merely redefine them (Wozniak, 1997)? What role did Gestalt psychology play in psychological thought and how did it relate to the "cognitive revolution" (Ash, 1995; Murray, 1995)? Was there a "cognitive revolution," and if so in what sense (Greenwood, 1999)?

It is probably fair to say that many of the received "truths" of psychology's history are now seen, at least by historians, to be much more complicated, contextualized, and open to interpretation than was once realized. Interestingly, however, one aspect of the traditional approach to the history of psychology has received relatively little explicit critical attention, although this criticism is clearly implicit in recent historiography. This is the assumption that the growth of ideas exhibits a linear, incrementalist birth-revision-replacement-survival pattern.

The goal of this chapter is to examine a counterinstance to this pattern, the case in which ideas and contributors well-known in their day pass first into a period of relative oblivion, dropping entirely or almost entirely out of the citation pool. They are then rediscovered by a much later generation so that the relevant texts are belatedly raised to the status of "classics" and their authors come to be seen as "major contributors." To do this, the chapter will be divided into two general sections. In the first section, I will address three issues of relevance to any such analysis. First, how is it that history comes to regard a work of the past as a "classic" and its author as a "major contributor"? What are some of the general criteria by which ideas come to be seen in retrospect as "significant"? Second, what factors may influence the disappearance of ideas, i.e., why do some contributors and ideas famous in their own day seem to vanish into the mists of history? And third, under what conditions are ideas seemingly lost to history sometimes rediscovered and the reputations of their authors rehabilitated?

In the second section, I will briefly discuss the life, works, near total disappearance, and eventual rediscovery of the ideas of James Mark Baldwin as a case study in the cyclical life of ideas. In this analysis, I will focus on the way in which the life cycle of Baldwin's thought and intellectual reputation

illustrates the hypothesized factors of disappearance and rehabilitation discussed in section one.

SURVIVAL, DISAPPEARANCE, REDISCOVERY: THE LIFE CYCLE OF IDEAS

Classics and Major Contributors: Why Do Some Ideas Survive?

Why are certain ideas seen as having had a lasting influence? Why do certain works come to be consensually viewed as "classics" in the field? Why do certain authors go down in history as "major contributors"? What, in short, makes a work in psychology "significant"? Clearly, any attempt to answer these questions raises two thorny issues: what constitutes significance and why do we care? I will address the second of these issues first.

The Value of "Significance"

Why should "significance" matter? Why do we persist in thinking in terms of "classic contributions" and "major contributors"? Does "significance" make a difference or is this just an exercise in whig history (or both)? A proper answer to this question would require a treatise in itself. Why does every school child in England know the date of the Battle of Hastings and every child in America the date of Columbus's "discovery" of the new world? Why do psychologists care that Wundt is said to have founded the first laboratory in 1879 (a debatable fact) or refer to John B. Watson as the "father of behaviorism" (an even more debatable fact)?

One answer may be that there seems to be something psychologically compelling about the use of significant events to parse time in historical narrative. One cannot imagine writing a history of psychology focused entirely on the mundane: the ordinary study, the routine publication, the minor theoretical idea, the figures that no one remembers. Although most of what takes place in the development of a field is just this sort of normal science, and there is little doubt that normal science is more important in the growth of the discipline than is typically allowed, history is a story and it is difficult to make routine science into a gripping story.

A second answer is that history by its nature is about change and certain events appear to be more centrally involved in the production of change than others. These events are of just the sort that we label as "significant." If a goal of historical narrative is to increase our understanding of sources and directions of change, one would expect the analysis of significant contributions to be of central concern.

Although an emphasis in historical narrative on significant contributions may therefore be inescapable, it is important to realize that any such analysis, no matter how useful, serves not only to describe but to distort the flow of events. To paraphrase William James in his remarkable description of the way in which introspection distorts the flow of consciousness by isolating the significant moment, focusing on critical moments in the flow of history leaves us prone to overemphasize the "substantive" at the expense of the "transitive." It is like "seizing a spinning top to catch its motion" (James, 1890, Vol. 1, p. 244). This may be a necessary consequence of the use of linguistic narrative; but it is a distortion nonetheless.

Criteria of "Significance"

That said, and given our apparently natural proclivity to focus on the substantive at the expense of the transitive, it is of interest to ask why certain contributions seem to qualify in retrospect for "classic" status. What, in effect, comes to count as a "substantive" when we step back and reflect on the flow of history? In an attempt to address this issue, I have recently reviewed the contents, circumstances of production, and presumed influence of a large number of works from various periods and on various topics within modern psychology that would, I think, make almost anyone's list of "classics" (Wozniak, 1999). Although this analysis makes clear the fact that contributions have come to be seen as "significant" for widely varying reasons, these reasons fall naturally into two broad categories. These categories are respectively "social impact" and "the production of creative novelty."

SOCIAL IMPACT. With regard to social impact, a number of questions can be asked. Is a work presumed to have influenced others? If so, is the influence seen as having been exerted on the discipline as a whole, on an individual or small group of individuals, or on the broader society? And what constitutes this influence? Is the work believed to have generated controversy and reaction? Is it reputed to have provided an especially clear or persuasive articulation of perspective that converted others to its point of view? Are its concepts, terminology, or methods seen as having been taken up by others? Is it thought to have set a standard or defined a model that others chose to follow?

Examples of all of these presumed forms of social influence can be found among texts generally regarded as among the most significant in the history of the field. Those usually characterized as having exerted a major influence on the discipline as a whole, for example, include works of Alexander Bain (1855, 1859), Herbert Spencer (1855), Francis Galton (1869, 1874, 1883), Wundt (1873/1874), and, of course, James (1890). Bain is widely seen as having given psychology both its first balanced sensorimotor

associationism and a new physiological point of view. Spencer's evolutionary thinking is typically described as having transformed psychology's conception of the nature and function of consciousness, animal and child mind, and even the functional organization of the brain (e.g., Young, 1970). Galton is revered for having taught psychology the importance of individual variability and statistical thinking. Wundt is credited with establishing psychology's belief that laboratory experimentation is the *sine qua non* of scientific methodology (despite what historians now realize was the extent of Wundt's own misgivings with regard to the applicability of experiment). James, of course, is generally viewed as having revolutionized the field with the breadth and vigor of his overall conception.

Prominent among those whose influence on individuals or groups of individuals has been considered particularly important are Hippolyte Taine, Ernst Mach, and George John Romanes. Taine (1870) helped define a distinctively French approach to psychology whose influence was apparent in the work of virtually all later French psychologists. Mach's (1886) "phenomenalism" provided Oswald Külpe (1893) and Titchener (1896) with the key to classifying psychology within the natural sciences; and Romanes (1888) articulated a broad theory of mental evolution that influenced both C. Lloyd Morgan's (1894, 1896) conception of comparative and Baldwin's (1895, 1897) view of developmental psychology.

Although early psychological science is not, for the most part, thought to have exerted much of an influence on the broader society, there are important exceptions. Hippolyte Bernheim's (1886) suggestive therapeutics is famous for having spread techniques of scientific psychotherapy throughout Europe and North America. In writing about Christine Beauchamp, his celebrated case of multiple personality, Morton Prince (1906) is generally credited with having helped raise public awareness of the importance of understanding exceptional mental states. Walter Dill Scott (1908) and Hugo Münsterberg (1913) are well known for having brought the principles of psychology to the marketplace; and Edward L. Thorndike (1913/1914) for taking them into the classroom.

As anyone knows who has glanced through the published correspondence of William James (1992–2002), controversy was extremely common in the early years of scientific psychology. Three of the most famous controversies, especially for the light they shed on the way in which psychological thinking was shaped during the period, involved Galton, Romanes, and Külpe respectively. Galton's (1869) strong position on the heritability of intelligence and opposition from those, like de Candolle (1872), who found the source of human intellectual variability in nurture rather than nature has been well documented (e.g., Fancher, 1979). So too has opposition to Romanes's (1888) fervent belief in mental evolution and the continuity

of human and animal mind by those wishing to preserve some shred of human uniqueness in the face of evolutionary thinking (Richards, 1987). And Boring (1929) describes the way in which Külpe's (1893) reductionistic, sensationalistic account of psychology as a natural science provoked Wundt (1896) to write a countertext.

Notable among those famed for mounting especially persuasive arguments for particular viewpoints are Henry Maudsley (1870) on the value of a psychosomatic perspective, Hermann von Helmholtz (1863) on empiricism as against nativism, and Titchener (1896) for the structuralist approach. Concepts linked to their origins include William B. Carpenter's (1874) "unconscious cerebration," Galton's (1874) "nature/nurture," Rudolf Hermann Lotze's (1881) "local signs," Janet's (1889) "subconscious," and Thorndike's (1911) "effect." Among those widely reputed to have set a standard for later work, one might list Fechner (1860), Helmholtz (1863, 1867), David Ferrier (1876), and Hermann Ebbinghaus (1885). Each provided models of systematic, quantitative research.

CREATIVE NOVELTY. A second broad criterion of significance, overlapping with the first, is the generation of creative novelty. Is a contribution known for having introduced new concepts, terminology, points of view, methods, or applications? Is it reputed to have been the first to report empirical discoveries? Is it known for having synthesized available material in new ways, bringing disparate ideas or observations together for the first time in one place or defining or systematizing a new area or discipline? Is it thought to have anticipated later developments even if, in its day, these anticipations may have been ignored or borne implications not fully understood even by the authors themselves?

In addition to the concepts, theories, and methods already mentioned, there are many others recognized by later writers for their novelty. Among these are Taine's (1870) theory of hallucination, Galton's (1874) questionnaire method, and John Hughlings Jackson's 1881–1887 theory of the functional architecture of brain systems (Jackson, 1932). George Henry Lewes's (1877) emergentist argument against reductionism, Ebbinghaus's (1885) memory methods, Baldwin's (1897) theory of social adaptation, and James's (1890) theories of emotion, self and, most famously, "stream of thought" have also been lauded for their relative novelty. Among empirical discoveries widely thought to be of particular importance one would certainly have to list Fechner's (1860) psychophysical law, Ferrier's (1876) localization of olfactory and auditory cortex, Morgan's (1896) identification of instinctive and acquired behaviors in neonatal birds, Leonard T. Hobhouse's (1901) experimental observation of insightful problem solution in higher animals, and Oscar Pfungst's (1907) discovery of the "Clever Hans" effect.

Those besides James (1890) who are generally known for having brought previously disparate material together in new syntheses or systematized material for the first time include Harald Höffding (1882) and George Trumbull Ladd (1887), both of whom published extremely influential textbooks of the new scientific psychology. Thorndike (1904) compiled psychology's first statistical handbook. Edmund B. Huey's (1908) synthetic review of the reading literature is usually credited with establishing the psychology of reading as a subfield in its own right. And William McDougall's (1908) social psychology is routinely referenced as the first text written from a purely psychological rather than a sociological point of view.

Finally, as might be expected, historians have shown that the early scientific literature in psychology is replete with anticipations of later, more modern concepts. Before James (1884), for example, Höffding (1882) spoke of consciousness as a "stream." Before Thorndike (1911), Bain (1855) articulated a law of effect; before Jean Piaget (1936), Hobhouse (1901) articulated a four-stage theory of the development of intellectual adaptation (see Wozniak, 1999, for a discussion of these anticipations). Indeed, the search for anticipations of this sort is unending, so much so in fact that most historians would probably subscribe to the belief that there is nothing truly new under the sun.

Complex, Clear, Correct, but Still Forgotten: Why Do Some Ideas Disappear?

Amidst all of these various forms of historical significance, there is one that is of particular interest in the context of this chapter. It consists of those ideas, more or less influential in their day and later seen by moderns as anticipating or even transcending the modern state of knowledge, that nonetheless failed to survive continuously into the modern era. These are ideas that have only belatedly been seen as significant, works that have only belatedly been deemed classics, thoughts that had to be rediscovered, authors whose reputations had to be rehabilitated—all long after they had disappeared from the scene.

Traditional explanations for the disappearance of ideas are often couched in terms of doomed ideas being overly simple, confused (i.e., failing to make relevant distinctions where relevant distinctions were necessary), or just plain wrong (i.e., inadequate in relation to some presumed "reality"). Such explanations are obviously problematic on both logical and epistemological grounds; but they are also problematic in that they cannot account for the many examples in psychology's history of ideas that were not obviously overly simple, confused, or "wrong," but which somehow

still failed to survive beyond the general period of their introduction. One such example, that of the ideas of James Mark Baldwin, will be analyzed shortly. The question at hand here is whether it might be possible to suggest other sensible explanations for the demise of ideas, explanations that lie neither in the nature of conceptual progress nor in the epistemology of truth.

A long look across the decades of psychology's development leads inevitably to the conclusion that one source of effects of this sort might lie in the nature of psychology as a human, social discipline. Try as they may to maintain standards of objectivity, scientists are nonetheless subject to all of the interpersonal and group-level social effects characteristic of any human group; and this suggests a number of possible, albeit admittedly speculative, hypotheses about factors of this type.

For the purposes of this discussion, I have couched these hypotheses in terms of seven possible effects. The first four have to do with factors that seem to operate at the level of individual social relations, how and why individuals accept or reject ideas produced by others. The last three have to do with broader sociohistorical trends within the discipline, a specification, perhaps, of what Boring (1929) liked to call the "zeitgeist." These will now be briefly described for whatever heuristic value they may have; they will then be discussed again in relation to Baldwin.

1. The Infertility Effect

ALL THINGS BEING EQUAL, IDEAS THAT FAIL TO CAPTURE AND MOTIVATE STUDENTS/FOLLOWERS OR IDEAS ARTICULATED BY THOSE WHO HAVE NO STUDENTS/FOLLOWERS TEND TO DISAPPEAR. One of the most interesting examples of this effect may be found in the rapid disappearance from the literature of the ideas of Alfred Binet. With a single famous exception—the intelligence test (Binet & Simon, 1905; Binet, 1908; Binet, 1911) for which Binet had followers such as Ovide Décroly, Théodore Simon, William Stern, Henry H. Goddard, and Lewis Terman)—Binet's most interesting and important contributions were lost to posterity with his death. These include work on children's understanding of number (Binet, 1890a), the development of word meanings (Binet, 1890b), the psychology of chess players and mathematical prodigies (Binet & Henneguy, 1894), and the development of visual memory (Binet & Henri, 1894).

One reason for this (another may have been the time-warp effect, see below) may have been that Binet had no students. He was twice an unsuccessful applicant for a regular academic position in France, once at the Collège de France, once at the Sorbonne (Nicolas & Ferrand, 2002). As a result, no one carried forward his many exceptionally innovative ideas. Only many years after the fact was Binet recognized as a pioneer not only

in testing, of which everyone is aware, but in cognitive psychology and cognitive development more generally (Pollack & Brenner, 1969). But by then the tasks he introduced and the new questions he asked had been independently developed and discovered by others.

2. The Oedipal Effect

ALL THINGS BEING EQUAL, INFLUENTIAL IDEAS ACTIVELY REJECTED, SYSTEMATICALLY REWORKED, OR UNACKNOWLEDGED BY HEIRS WISHING TO STAKE A CLAIM FOR THEIR OWN ORIGINALITY TEND TO DISAPPEAR THROUGH ASSIMILATION OR TRANSFORMATION. Perhaps the most famous cases of Oedipal effect in the history of psychology involve the various important students of Freud—Jung, Adler, Rank, Stekel, Reich—who had to break with the master in order to establish their own independence as thinkers. Those fully conversant with Freud can undoubtedly distinguish ideas that are truly Freudian from those that are more properly Jungian, Adlerian or Rankian, etc. Nevertheless, the general trend in the minds of non-psychologists and even psychologists who are not themselves analytically inclined has been for psychoanalysis to be increasingly treated as a monolithic point of view.

3. The Caricaturization Effect

ALL THINGS BEING EQUAL, IDEAS THAT ARE REPLACED IN THE LITERATURE BY THEIR OWN CARICATURES, BY ACCOUNTS THAT DISTORT THROUGH OMISSION AND SIMPLIFICATION, TEND TO DISAPPEAR. Probably the most serious source of caricaturization is the textbook account, although scientific literature reviews are by no means exempt from this tendency. The problem can arise, of course, from the limitations of space and the consequent need for condensation and from the need to simplify for readers with little background in the field. It can also arise, however, from reviewers own inadequate understanding of the ideas reviewed or even from willful distortion. Unfortunately, readers unwilling to consult the original texts and prone to take these simplifications as accurate may reject a set of ideas as oversimplified when in fact it is the caricaturization that is oversimplified.

One thinks, in this regard, of the accounts of Piaget that can be found in virtually all basic developmental textbooks. Thus, for example, the extraordinarily sophisticated and detailed ideas in Piaget's (1936) *La naissance de l'intelligence chez l'enfant* are grotesquely caricatured in almost all contemporary accounts of Piaget's sensorimotor stages. This has led many to assume Piaget to be "wrong" on logical or empirical grounds when in fact they have little idea of what Piaget actually believed.

Another interesting example is Boring's (1929) infamous textbook characterization of Wundt as psychology's first self-conscious

methodologist. Wundt *was* a self-conscious methodologist and a thoroughgoing champion of experimentalism but hardly the first—Mill, Fechner, and Helmholtz, among others, preceded him in this regard. While Wundt did argue for experimental methods, he limited their applicability to the study of physiological psychology, believing them to be wholly inappropriate to the study of higher mental processes. In Boring's portrayal (and hence in the minds of generations of students who learned their history from Boring), Wundt's *Grundzüge der physiologischen Psychologie* (1873/1874), was "the most important book in the history of modern psychology . . . symbol of his metamorphosis from physiologist to psychologist, and . . . [an attempt] systematically to cover the range of psychological fact" (Boring, 1929, pp. 317). Most historians would now agree, however, that the *Grundzüge*'s usefulness was rather short-lived. The consensus is that there is a very strong thread of continuity running to the *Grundzüge* from Wundt's earlier *Beiträge* (1862) and *Vorlesungen über die Menschen- und Thierseele* (1863). Moreover, the *Grundzüge*'s coverage of psychological fact was narrowly restricted to *physiological* psychology, despite Wundt's strong interest in topics such as the psychology of language, custom, law, conscience, society, family, and religion. Yet Boring's Wundt became everyone's Wundt and it took modern scholarship to begin to uncover the Wundt that nobody knew.

4. The Loser Rejection Effect

ALL THINGS BEING EQUAL, IDEAS THAT COME TO BE SEEN AS ASSOCIATED WITH A LOSING CAUSE TEND TO DISAPPEAR. Social psychologists have long known that, given a choice, people will shun a losing cause or an individual characterized as a loser in preference to winning causes and winners. Ideas can become associated with losing causes for a variety of reasons, including severe logical criticism, failure to replicate, distortion, or loss of reputation of the individual with whom the ideas are associated. One possible example of this effect might be found in the relative demise of operant analyses of verbal behavior following the Chomsky-Skinner debate—a debate that Chomsky (1959) was widely perceived to have won and to have won largely through logical criticism of the Skinnerian (1957) position.

5. The Pendulum Effect

ALL THINGS BEING EQUAL, IDEAS THAT ARE OUT OF PHASE WITH THE PREVALENT TREND OF THE SCIENCE TEND TO DISAPPEAR. Although the notion of "revolutions" in psychology has clearly been badly overstated, it would appear that throughout its history, and depending to an extent on geographic location as well, the body of psychological thought has tended to oscillate between extremes. These include:

rationalism/empiricism, elementalism/holism, conscious/unconscious, nature/nurture, mind/behavior, individual/society, cognitive/affective, theory/fact. This tendency toward oscillation seems to lend itself to a natural pruning of ideas at one end of the spectrum when another is coming into play. Examples that come readily to mind might include Christian von Ehrenfels (1890) early formulation of the principle of "gestaltqualität" in an era of profound analytic elementalism. Münsterberg's (1909) idealistic view of the nature of the psychical was advanced in an era and a place (Harvard) where pragmatic realism was the order of the day. And, of course, Binet's contributions to the experimental analysis of higher mental processes occurred at a time when laboratory psychology was devoted primarily to lower-order sensory processes, reaction time, and psychophysics.

6. The Time Warp Effect

ALL THINGS BEING EQUAL, IDEAS THAT ARE BORN BEFORE THEIR TIME HAS COME, EITHER BECAUSE THE INTELLECTUAL GROUND HAS YET TO BE PREPARED OR, MORE FREQUENTLY, BECAUSE RELEVANT TECHNOLOGICAL INNOVATIONS HAVE YET TO EMERGE, TEND TO DISAPPEAR. The classic examples here, of course, are the various instantiations of the logic machine. These range from Charles Babbage's analytical engine of 1832, through Alfred Smee's relational and differential machines (1851) and William Stanley Jevons' logic piano (1869) to the electrical logic machine built at Roosevelt University by psychologist Benjamin Burack in 1936 (see Burack, 1949). The goal of all of these visionaries was to simulate human logical thought; but the technology even to approximate this goal was not yet available.

7. The Method Effect

ALL THINGS BEING EQUAL, IDEAS FOR WHICH THERE ARE NO EMPIRICAL METHODS TEND TO DISAPPEAR. Whatever else contemporary psychology may be, it is exceptionally method driven; and it has been method driven since its inception. The questions asked and the answers obtained have been shaped and limited by available methods and prevalent paradigms. These include the psychophysical, reaction-time, and sensory methods of the early laboratories, the jumping stands, mazes, and Skinner boxes of the animal labs, and the eye trackers, video coding software, and computerized presentations of the modern era. Although there have been exceptions to this rule in the era of scientific psychology (e.g., James, 1890), ideas incapable of being transformed into testable hypotheses, no matter how clever or how intuitively appealing, are not likely to prosper. One of the best examples of this effect can be found in the work of Baldwin, to whom we shall return momentarily.

Back to the Future: Why Are Ideas Sometimes Rediscovered?

Our ability even to discuss the existence of a category of forgotten but "classic" ideas obviously implies their later rediscovery. This in turn raises the question of factors that may be involved in the rediscovery of ideas and the concomitant rehabilitation of their author's reputation. A survey of obvious instances where this has occurred suggests at least three relevant factors whose effects might be interesting to explore in future work. They involve legitimation, the quest for identity, and the search for ideas of heuristic value.

1. The Legitimation Effect

Legitimation via the past ("look this idea isn't so crazy, so and so held it as long ago as 1860") is a time-worn approach not only in psychology but in virtually all domains of discourse. In psychology, the classic example is probably that of Edwin G. Boring (1929), who quite explicitly wrote his *History of Experimental Psychology* as an apology for what he perceived to be a tradition of experimentalism under threat (O'Donnell, 1979). By placing that tradition within the flow of events from Descartes to the modern era, Boring was, in effect, asserting its legitimacy as heir to the past.

2. The Identity Effect

Everyone needs a sense of identity; and one of the ways to achieve this sense is to conceive of oneself within a family history, to give oneself, in effect, roots. In psychology, the question comes down to finding one's intellectual father/mother, grandfather/grandmother, etc. This motivates a search of the past for those who may have held ideas similar to one's own; and it is probably how and why the vast majority of anticipations have been discovered. Specific examples are unnecessary in that the literature is littered with introductory sections of chapters and books grounding work about to be discussed in anticipations by those who have come before.

3. The Heuristic Value Effect

Although this is probably not as common as it should be, given the modern chronocentrism of most contemporary psychologists, it is possible in principle to mine the past for ideas to be used in the future. This is of particular relevance when methods or technological advances have become available that make it possible to study phenomena suggested by old, speculative, but intellectually generative ideas. Current connectionist use of Donald Hebb's (1949) concepts of neural circuitry and the many references in the literature to William James's (1890) ideas about attention,

emotion, and the self are obvious examples of the value of looking backward in order to look forward.

JAMES MARK BALDWIN: THE CASE OF A MISSING MAJOR CONTRIBUTOR

This brings us to the interesting case of James Mark Baldwin (1861–1934). Among those of his generation Baldwin is arguably American psychology's greatest contributor, both professionally and intellectually (Wozniak, 2001). By 1929, however, he had disappeared so thoroughly from the literature that even before he died, Boring could write that his "felicitous literary style, surpassed only by James', gave a transient vitality to his ideas; but his effect was not permanent," (1929, p. 518). And this was a view that Boring saw no reason to change when in 1950 he revised and expanded his history.

How could this happen? Was Baldwin really that important in his own day? What factors led to his disappearance from the literature? And why was he rediscovered? As we will see, all of the factors discussed in the section above appear to be relevant to Baldwin's case and I will briefly address each in turn. Before proceeding to that analysis, however, let me make the case for Baldwin's professional and intellectual importance in his own day.

Baldwin's Contributions

Professionally, Baldwin founded experimental laboratories at the University of Toronto (1890) and Princeton (1893) and refounded Hall's lapsed laboratory at Johns Hopkins (1904). With James McKeen Cattell, in 1894, he established the field's most influential journal, *The Psychological Review*, with its associated *Psychological Index* (forerunner to *Psychological Abstracts*) and *Psychological Monographs* (first appearing in 1895). After splitting with Cattell in December of 1903, he founded another important journal, *The Psychological Bulletin*.

Baldwin was a founding member, first President (1904–1908), and for several years the guiding force behind the Southern Society of Philosophy and Psychology. He held membership and was variably active in at least seventeen professional societies worldwide. He served as President of the American Psychological Association (APA) in 1897, and he was the instigating force behind the formation of two of the APA's earliest committees, the Committee on Physical and Mental Tests (1895) and the Standing Committee on Psychological and Philosophical Terminology (1898). In

1902, Baldwin served as an Advisory Committee (of one) on Psychology to the Carnegie Institution of Washington, by means of which he first placed psychology on the philanthropic funding agenda, albeit with limited success.

Intellectually, Baldwin was one of the field's most prolific authors and, with James and Dewey, one of its most sophisticated thinkers. Over the course of his career, he published 22 books and approximately 150 articles. Among his early publications were the field's first controlled experimental studies of infant behavior (Baldwin, 1890, 1891b, 1892a, 1892b, 1893) and a textbook, *Handbook of Psychology: Feelings and Will* (1891a), that was among the first to emphasize the fundamental importance of motor functions (dynamogenesis, kinaesthesis) in cognitive process. Indeed, Baldwin's priority in this area led Münsterberg to remark that he and Baldwin were "the 'motor men' on the psychological car."' (Baldwin, 1930, p. 3).

The work for which Baldwin has become most famous in the modern era of his rediscovery, however, was published in mid-career. It consists of two major books, *Mental Development in the Child and the Race* (1895) and *Social and Ethical Interpretations in Mental Development* (1897), and two important journal articles, one on evolutionary biology (1896) and one on evolutionary epistemology (1898).

In his two book length contributions, Baldwin elaborated a biosocial approach to intelligence that introduced a level of complexity in the conceptualizaton of the mind, its evolutionary origins, ontogenetic development, and sociocultural formation that went far beyond the prevailing thought of the period. He addressed topics as varied as the nature of developmental and evolutionary mechanisms, the relationship between reason and reality, the genesis of logic, and the value of aesthetic experience. In focusing on the nature of development in children, he discussed habit, imitation, creative invention, altruism, egoism, morality, social suggestibility, social self, self-awareness, theory of mind, and enculturation. He employed and in some cases introduced concepts such as multiplicity of self, ideal self, self-esteem, assimilation, accommodation, primary circular reaction, genetic logic, genetic epistemology, and social heredity.

In his 1896 paper on evolutionary mechanisms, "A new factor in evolution," he introduced a concept which has become known in evolutionary biology as the "Baldwin effect." As Baldwin himself later described it, this was the view that "an organism's modifications, acquired in adaptation to its living environment—let us say in learning to swim — may affect its descendants—who learn to swim more easily—*without inducing any inheritance of these modifications by the children from the parents*. The process in question is this: the acquisitions keep alive, from generation to generation, natural variations in the same direction as themselves, and so the later

come in time, by further variation, to supersede the former altogether." (Baldwin, 1926, pp. 69–70).

Largely dismissed as an evolutionary mechanism of any importance by those involved in formulating the "modern synthesis" in evolutionary theory (see Dunn, 1966), the Baldwin effect has undergone something of a recent renaissance. It first was briefly discussed by Stephen J. Gould (1980) and then picked up by those seeking to develop evolutionary computational algorithms (Hinton & Nowlan, 1987). Finally, Baldwin's 1898 paper, "On Selective Thinking," based on his 1897 APA Presidential address, has come to be seen by some (e.g., Campbell, 1982) as a "classic" in the development of instrumentalist epistemology.

Although Baldwin's later career work is not particularly well-known in the modern era, it was nonetheless widely discussed and reviewed in his own day. Between 1901 and 1905, he edited a three-volume *Dictionary of Philosophy and Psychology* that is still one of the best sources for turn-of-the-century thought in these disciplines. And between 1906 and 1911, he published *Thoughts and Things*, a three-volume genetic logic that developed and vastly extended the ideas first introduced in his 1896 paper on Selective Thinking.

Baldwin's intellectual esteem among his contemporaries is indicated by the fact that he was awarded one of Oxford University's first two honorary doctorates of science (1900) and honorary degrees from the University of Glasgow (1901), the College of South Carolina (1905), and the University of Geneva (1909). In 1904, he was appointed to serve as one of only two departmental speakers for Psychology (the other being Cattell) at the 1904 St. Louis International Congress of Arts and Science. In 1909, he was elected by delegates to the Sixth International Congress of Psychology in Geneva to serve as "président d'effectif" for the Seventh Congress, planned for 1913 in the United States, a congress that was never held (Evans & Scott, 1978). And finally, in 1910, he received the exceptional academic honor of being elected to succeed William James as Correspondent of the Academy of Moral and Political Sciences, Institut de France.

Baldwin's Disappearance from the Literature

Why then isn't Baldwin better known? How is it that so few have read his work? The simple answer and the answer that is often given is that Baldwin's eclipse followed upon and resulted from his disappearance from the American academic scene in 1909. Threatened with public scandal, he resigned his professorship at Johns Hopkins and fled into self-imposed, life-long exile from American academic and personal life, an exile spent largely in France and England and primarily without official academic appointment.

Careful analysis of Baldwin's career, however, suggests that the situation was a great deal more complex than at first appears. While his resignation and self-imposed exile were undoubtedly important factors in the demise of his influence, it would seem that all of the effects tending toward the demise of ideas described in the first section of this chapter were operative in Baldwin's case. Given his importance during his active career and his near total disappearance from the literature—a disappearance that began almost immediately after his resignation in 1909 and lasted well into the 1970s—this is perhaps not surprising.

Among Americans, Baldwin had very few students and none of any importance. W. J. Shaw followed Baldwin from a B.A. at Toronto to Princeton where he received the M.A. and collaborated with Baldwin on two papers (Baldwin & Shaw, 1895a, 1895b). After accepting a teaching position at Wesleyan, however, he seems to have published little. At Princeton in the 1890s, in addition to Shaw, Baldwin taught both Wilbur Marshall Urban (Ph.D., Leipzig) and Francis Kennedy (Ph.D. Leipzig). While Urban went on to a distinguished career as a philosopher, his work shows little of Baldwin's influence; and Kennedy died prematurely in 1901.

At Hopkins during Baldwin's tenure, as Knight Dunlap has pointed out, only "two doctorates were awarded in philosophy, and none in psychology" (Dunlap, 1932, Vol. 2, p. 43). Presumably Dunlap had in mind W. D. Furry (1907) and W. W. Costin (1908), neither of whom became known as a scholar. One student working in psychology at the time, Trigant Burrow (Ph.D., 1909), did go on to a productive and creative career, and his early work may owe something to Baldwin. Immediately after Baldwin departed Hopkins, however, Burrow left psychology for medical school and eventual psychoanalytic training and made his best known contributions to psychiatry (Burrow, 1927, 1949). It is fair, I think, to say that Baldwin suffered more than most from the infertility effect.

What about oedipal conflict? Clearly, since Baldwin had no immediate students of any consequence, any Oedipal effect in his case would have had to have taken a slightly different form; and in my view it did. There were two major American thinkers, both sociologists, both of whom were rough contemporaries of Baldwin's, both of whom profited intellectually in significant ways from Baldwin's ideas, especially those expressed in *Social and Ethical Interpretations*, and neither of whom gave Baldwin any real credit for this influence. They are Franklin Henry Giddings and George Herbert Mead; and it has been left to others (Gillin, 1927, for Giddings; Joas, 1985, for Mead) to unearth and emphasize Baldwin's importance for their thinking.

Baldwin spent much of his active career locked in controversy. Indeed, he published responses to critical accounts of his work by no fewer than

a dozen writers representing four disciplines. His critics included psychologists Titchener, William Romaine Newbold, Cattell, James Sully, Herbert Nichols, and Margerat Schallenberger; biologists Edward Drinker Cope and C. Wesley Mills; sociologist Franklin H. Giddings; and philosophers F. C. S. Schiller, Bernard Bosanquet, and Addison W. Moore.

Even a cursory reading of Baldwin's exchanges with these critics leaves the reader in something of a muddle as to what Baldwin and the others were actually claiming. This is compounded, in Baldwin's case, by an egregious tendency to neologism, frequent lack of clarity in expression, and a tendency to compose arguments by concatenating portions of earlier texts written for other purposes. As Yerkes (1903) put it in his review of Baldwin's 1902 *Development and Evolution*: "One can but feel that the author might well have taken the trouble to carefully rewrite it in a systematic and logical fashion instead of merely throwing together a lot of fragmentary discussions . . ." (p. 348). In short, although Baldwin's ideas may not have lasted long enough for textbooks to caricature and distort them, his contemporaries and even his own infelicities of expression served that purpose just as well or better.

The scandal that ended Baldwin's career resulted from his having been arrested in a Baltimore bordello. He compounded this offense in the minds of some by denying that he was there for the obvious purpose. In America in 1909, twin immoralities of this sort were extremely embarrassing at best and ostracizing at worst; and the use of Baldwin's name in print seems to have become virtually taboo. Prior to 1909, Baldwin is cited with reasonable frequency in both the American and European literatures. After 1909, it is only Europeans such Henri Bergson, Edouard Claparede, Pierre Janet, E. B. Poulton, and C. Lloyd Morgan who cite Baldwin with any regularity. If ever anyone was rejected as a loser, it was Baldwin.

From the late 1880s through the period just before World War I, American psychology can for the most part be roughly divided into "theorizers" (e.g., James, Dewey, Münsterberg), who thought great thoughts but did little research, and "fact gatherers" (e.g., G. Stanley Hall, E. C. Sanford, Cattell, E. W. Scripture), who published research with little or no theory. For the most part these factions co-existed in reasonable peace. Thus, for example, during most of its early years, theoretical papers were common in the general program of the APA meetings and the psychologists usually met in at least one joint annual session with the American Philosophical Association. Sometimes, however, conflict boiled over. This is reflected in regular debates at early APA meetings about splitting the program into two parts, one devoted to theory, another to research. This debate fueled the sometimes, acrimonious competition between Hall's *American Journal of*

Psychology (fact-gatherers) and Baldwin and Cattell's *Psychological Review* (much more tolerant of speculative theory).

Baldwin was, of course, very much in the camp of the "theorizers." In 1895, for example, he expressed this point of view forcefully, decrying:

> that most vicious and philistine attempt in some quarters to put science in the strait-jacket of barren observation, to draw the life-blood of all science—speculative advance into the secrets of all things—this ultra-positivistic cry has come here as everywhere else, and put a ban upon theory. On the contrary, give us theories, theories, always theories! Let every man who has a theory pronounce his theory! (Baldwin, 1895, pp. 37–38).

Unfortunately for Baldwin, however, by the time he resigned in 1909, American psychology was committing itself ever more deeply to an experimental empiricism that would soon culminate in the behaviorism of the 1920s. Theories, especially speculative theories, were rapidly falling out of favor; and Baldwin, perhaps more than most, suffered from the swing of the pendulum. When he died in 1934, Margaret Floy Washburn, detailed to write his obituary for the *American Journal of Psychology*, summed up the attitude of the day:

> Although he was one of the earliest psychologists of modern times to recognize the fundamental influence of motor processes, and perhaps the pioneer psychologist to make imitation a basic concept, his theories on child psychology suffered from a lack of factual basis . . . What made his observations so valuable in his opinion was the fact that he made them under the influence of his theories! . . . Professor T. L. Bolton, reviewing the book in 1895, commented on this statement: 'This has been Professor Baldwin's great mistake, and it has rendered his book in some parts a barren waste of speculation.' It accounts for the fact that the name of this brilliant thinker seldom if ever appears in present-day volumes on child psychology (Washburn, 1935, p. 169).

Finally, Baldwin was unquestionably ahead of his time intellectually, in both the "time warp" sense that the ground had not yet been prepared for his ideas and the "method effect" sense that there were no methods by which his ideas could be tested. With the possible exception of Josiah Royce (1895) and John Dewey (1903, 1916), there were no others during the period, psychologists or philosophers, addressing Baldwin's questions. In retrospect, it is somewhat surprising, given the state of the art in psychology, that Baldwin was cited or read at all; and when he was read, one suspects that he was often little understood. Furthermore, whatever his proclivities for laboratory research may have been, Baldwin had no choice,

given his intellectual interests, but to be a theorist. In 1909, neither he nor anybody else had scientific methods adequate to the sophistication of his ideas or the study of his questions. In America, those methods wouldn't become readily available and in use until the late 1960s.

Baldwin's Rediscovery

In Europe, however, the situation was different. While Baldwin had no important American students/followers, his influence on two European thinkers destined to make exceptional contributions to both developmental method and theory was formative and significant. These two were the Swiss genetic epistemologist, Jean Piaget, and the Russian genetic psychologist, Lev Semenovich Vygotsky. In Piaget's (1982) own view, Baldwin's influence came about primarily through the *Mental Development*, from which Piaget took (with major revisions and additions) the sensorimotor principle and concepts such as "circular reaction" and "assimilation/accommodation." One suspects, however, that Baldwin's later *Thought and Things* (1906–1911), which focused on genetic logic and genetic epistemology, and *History of Psychology* (1913), itself a genetic epistemology, may also have engaged Piaget's attention.

Vygotsky, on the other hand, appears to have been influenced primarily by ideas articulated in *Social and Ethical Interpretations* (Valsiner & Van der Veer, 1988). This influence, more indirect than direct, was apparently mediated by Pierre Janet, himself heavily influenced by personal contact with Baldwin in the 1920s (Van der Veer & Valsiner, 1988). These ideas include Baldwin's arguments for the fundamental sociality of mind, the importance of enculturation, and the role of what Baldwin described as "the mass of organized tradition, custom, usage, social habit . . . already embodied in the institutions and ways of acting, thinking . . . of a given social group, considered as the normal heritage of the individual social child" (1897, p. 301).

And therein hangs the tale of Baldwin's rediscovery. In the late 1960s, American developmental psychologists, filled with the enthusiasms of the new "cognitive revolution," learned to their surprise that two European psychologists, Piaget and Vygotsky, had begun to pursue questions of cognitive development as early as the 1920s and that Piaget was still doing so. (Vygotsky, of course, had died in 1934). First Piaget, and then eventually Vygotsky, came into vogue among American developmentalists, a vogue in Piaget's case that lasted perhaps twenty years and one that is still ongoing in the case of Vygotsky.

The beginning rediscovery of Baldwin by developmental psychologists is intimately linked to the ascendance of Piaget. It can be dated to the early 1980s, when Robert B. Cairns (1980, 1983) published two influential

accounts of Baldwin's ideas. John M. Broughton and D. John Freeman-Moir (1982) also edited a book that brought together those interested in and/or influenced by Baldwin to discuss the nature and impact of his theories. The first of Cairns's publications was a retrospective review entitled "Developmental theory before Piaget: the remarkable contributions of James Mark Baldwin." The second was a history chapter, "The emergence of developmental psychology," published in Mussen's *Handbook of Child Psychology*. In both papers, Cairns contextualized the discussion of Baldwin in terms of his intellectual relationship to Piaget, then at the peak of his popularity among Americans, and also to the Piaget-inspired moral development theories of Lawrence Kohlberg, also widely discussed at the time. Broughton and Freeman-Moir's book, *The Cognitive Developmental Psychology of James Mark Baldwin: Current Research and Theory in Genetic Epistemology*, was not only subtitled in terms of "genetic epistemology," but contained an interview with Piaget and a chapter by Kohlberg on moral development.

Does this initial rediscovery exhibit the legitimation, identity, and heuristic effects hypothesized above? It is possible that it does, at least in terms of legitimation and identity (the search for heuristic value seems to have come later); but serious evaluation of this issue will require much more evidence than is currently available. At this point, we can only speculate. By the early 1980s, the pendulum in developmental psychology had begun to swing from the extreme cognitive orientation of the 1970s toward what would become, under the impetus of attachment theory and the popularization of Vygotsky's sociohistorical view, an almost equally extreme social orientation in the 1990s. It is of interest that Baldwin was first rediscovered as a "cognitive" rather than a "social" developmentalist. Even though Cairns included *Social and Ethical Interpretations* in his retrospective review, for example, the broader implications of Baldwin's ideas for a bio*social* theory (and hence those aspects of Baldwin's thought that influenced Vygotsky) were neglected. Whether this was implicit legitimation of the cognitive approach in the face of rising interest in social process or merely reflective of the extreme cognitivism of the period is an open question.

What is also of interest is that in many of the rediscovery era accounts, the fact that Baldwin was an American figures rather prominently. In an era where developmental psychology was dominated by European thought and method, it is probably not coincidental that American psychologists would go in search of American roots for their work. Piaget and Vygotsky may have been the immediate source of inspiration for many in the 1970s and 1980s; but if Baldwin was the distal source, then those following Piaget or Vygotsky were still working in a tradition that was fundamentally American.

Finally, with respect to the heuristic value of Baldwin's work, I think that it is fair to say that this was not a prominent feature of the early period of rediscovery. In recent years, however, a number of developmental psychologists, including Susan Harter, Andrew Meltzoff, Gil Noam, Robbie Case, George Butterworth, Jan Valsiner, Esther Thelen, and Linda Smith have returned to Baldwin as a source of inspiration for their work. Whether this trend will continue in the future only time will tell. But it is important to note that there are whole areas of Baldwin's thought, most especially that contained in *Thought and Things* and in portions of *Social and Ethical Interpretations* that have yet to be mined for the many insights they contain. In Baldwin's case, a return to past ideas may truly be an instance of "back to the future."

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AUTHOR'S NOTE

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3

REINVENTING THE PAST THROUGH REINTERPRETATION

REFLECTIONS ON THE HISTORY OF PSYCHOLOGY—35 YEARS IN THE TRENCHES[1]

John A. Popplestone, Ph.D.

INTRODUCTION

I make the assumption that the readers of this collection feel generally benign toward the enterprise of history but there is also a body of opinion about history, in its entirety, that reveals ambiguity, or rejection.

From the political left we have "Let the Past Serve the Present," Chairman Mao—who may not have been listening to himself, or "The Revolution has no need of historians," Lenin on the occasion of the execution of a Romanoff Grand Duke who wrote histories. And "He who lives in the past walks into the future backwards," Confucius, which may have

[1] This essay was originally written as a contribution to a symposium "Reflections in the Mirror of Psychology's Past—Honoring J. Popplestone and M. McPherson" (APA 2000, Washington).

But then symposium chair, Thomas Dalton, asked that it be included in this projected volume. However, it seemed that the version intended for ephemeral oral presentation before a group of friends and colleagues would not be suitable for the finality of print and inclusions in a published volume. Therefore, I have solicited the advice of friends who were present in Washington, or whom I could ask for time to read and have also been able to ask colleagues quoted or cited in this version to give me a piece of their minds . . . or their good wishes as the case may be.

suffered in translation. "Those who do not know history are doomed to repeat it," by George Santayana, who may be centrist. While on the right we have "History is bunk", by Henry Ford the man who created a hymn to the past—Greenfield Village. Lastly an ironic statement, "There is nothing new, except what is forgotten," Rose Bertain (1744–1813). Ms. Bertain was dressmaker to the best-dressed woman in Europe, Marie Antoinette, Queen of France.

Attitudes toward the history of psychology, inside the field, vary as well:

The Negatives—First

There is a strongly held belief by some psychologists that the history of the field is irrelevant to the actual pursuit of psychology in our times. They feel that what they are doing, their career, their work has no relation to the past. Typically these people are young, enthusiastic, in a hurry, with no time for the leavening role of history. If they are pressed they will report that history is merely an account of old errors, now in the past, and therefore of no interest.

One Robert Oetjen (about 1971) was old enough not to be excused for this kind of error. As a former physicist and then currently dean of the College of Liberal Arts at The University of Akron he explained to me, "You know John, whenever I meet a scientist who is knowledgeable about the history of his field I know that I am dealing with an incompetent."

Once I was assaulted by a drunken clinical psychologist in the bar of the Washington Hilton who objected to my self-description as an historian of psychology. He told me he felt that in a world in which there was so much suffering and misfortune no one could in good conscience dwell in the past. He was in no shape at that time to hear history described as action research.

In fact, this view of the past as error may have as much potency as the charge that it is irrelevant. Error is seen as powerful and fearful while obsolescence is passive. The psychologist seems to feel the cold breath of

My thanks for their help to Ludy T. Benjamin, Jr., David B. Baker, Michael M. Sokal, John C. Burnham, John C. Carson, and Ellen Herman. I am also happy to thank Thomas C. Dalton and Rand Evans whose editorial opinions have greatly improved the manuscript that I sent to them. I also thank "a couple of anonymous reviewers" whose warnings and questions I have heeded. But, of course, I alone am responsible for what follows. This is one of my few times in more than 35 years that something has appeared in print without my wife's collaboration and, always, her firm red pencil. So here I stand alone. Perhaps only a grumpy old man, but one whose more than 35 years of involvement in the history of psychology have formed opinions and a viewpoint, which perhaps entitles me to speak on the nature of things as. . . . I see them.

the historian making judgments. People who were not there, who did not take part but who now are passing judgment on their betters, the working scientist. This attitude is, in part, created by the kind of historian who proceeds with righteous indignation (and easy self-enhancement) to denounce an elder parent's errors. This shallow thrill of condemning the actions of others should not be a characteristic of good history but it does illuminate and contaminate the work of some quasi-historians who can enhance themselves by finding something dumb said by a psychologist, in the context of the past, and using it to express their own moral, intellectual and up-to-date superiority. For example Steven Jay Gould, whose views about psychology are discussed later, along with two other case studies, John C. Carson and Ellen Herman. But at this point let me mention that ideology can triumph over *facts*, not just their interpretation.

At the time of the APA Centennial, in 1992, the Task Force organizing the observance was urged by many ardent feminists to ignore the role of the founder of the organization and its first president, G. Stanley Hall, because he had said some foolish 19th century things about the academic abilities of women. Apparently his role in the 1892 organization of the association was to be sent down the memory hole to oblivion, or at least, not earn any special attention.

Another view of this occurred, when I explained this 'the past is error syndrome' to Robert Sears (1908–1989). He corrected me and said, "No, John we are not so much afraid of the judgment of history as we are of being ignored by history."

There is also a type of historian, and the historian's critic, for whom history is antiquarianism. That is, a comfortable dwelling in some part of the exotic past where the concerns of the present can be ignored. The—perhaps—strange events of the past are used as a personal enhancement and the role of the event in the past is not seen as part of a pattern or context but exists in its own right as something comforting.

Even if a substantial portion of the psychological community does not believe that the history of psychology has any real value, there are also many others who believe that it is important and valuable.

The Positives—Second

A course in the history of the field is an APA mandated requirement in the curricula of graduate programs in any of the professional areas in psychology. (Probably a requirement more often honored in the breach than in the observation).

This positive vote by those who value history sees history as relevant to the ongoing enterprise of modern psychology, both as science and as

application. They view knowledge about the history of the field as utilitarian. Any clinical psychologist will recite the mantra that "the patient's past is the best predictor of the future." By analogy, they contend that knowledge of the history of the field will allow the student to place the discipline in perspective. Perhaps the errors of the past will not need to be repeated, and the successes of the past can be explored for current relevancy. There is also the hope that by seeing the divergence and breadth of psychology, both past and present, the contemporary psychologist may, hopefully, be instilled with some modesty by avoiding privileging any exclusive perspective with too much vigor.

According to this point of view, history is how it was and is how the present came to be, and thus serves as a warning to be cautious about over interpreting present and future events. Understanding the history of psychology can be an ego enhancing experience in ancestor worship, for the psychologist (and others), who are willing to suspend irony and allow a free flow of delight and surprise can take some reflected pride in the accomplishments of the founders of the first generation and their second and third generation successors.

It has been said that psychologists are prone to physics envy, that is, to low voiced expressions of modesty when they look at the present status of their field in contrast to the maturity of hard sciences like physics, chemistry, and biology. But in fact, we should be boasting of the wonderful things that psychologists have accomplished in a few short decades—since the critical events, which took place in the former dining hall for poor students at Leipzig in the winter of 1879–1880.

While psychologists have not always been 'nice' people, all warm and fuzzy, clear browed and glowing with mental health, they have always been very well educated, very smart, and usually very hard working. It has been said that Albert Einstein was standing on the shoulders of mathematicians and physicists of preceding millennia when he saw farther than they had seen. But pioneering psychologists had no one's shoulders to stand on when they began their explorations of the new continent, the landscape of the human mind. If we have not always been as clear eyed and as prescient as we might have been, nevertheless, psychology since 1879–80 has created a new subject matter, a new methodology and even new techniques of application. Psychologists have a history in which they may take great pride. We have not done badly at all. There is a Persian proverb that, "the dogs may bark but nevertheless the caravan moves on." Similarly, despite its' critics, psychology has continued to advance.

There are some advantages and disadvantages to the study of history as a generic discipline. The study of political behavior, or 'war', or

institutions *is* sometimes discouraging. It can easily lead to pessimism about the human animal. However, the study of mankind's intellectual efforts—the person as scientist for example—is very heartening. George Sarton (1884–1956) saw the history of science as 'an endless series of victories of the human mind, victories without counterbalancing failures, that is, without dishonorable and humiliating ones, and, without atrocities.' I regret that this quotation, from my lecture notes, is without citation. But it is accurate. When we study the history of psychology—or any other science—we can easily find ourselves accepting the idea that we are indefinitely perfectible. At the very least the human animal is certainly shown in a more attractive guise.

So let us grant that the history of psychology *may* have some value. Then what is the actual status of the field? When I began, 35 years ago, the field of the history of psychology could be described as a Mom and Pop operation. Many of the historians were autodidacts, psychologists with a hobby of the history of their field. But in 1965, the history of psychology took a great leap forward with three foundings, in all of which Robert I. Watson (1909–1980) played a seminal part:

1. *The Journal of the History of the Behavioral Sciences (JHBS)* began publication.
2. Division 26, The Division of the History of Psychology was organized, with 86 Fellows and 148 members listed in the APA *Directory* of 1966.
3. And the Archives of the History of American Psychology was founded at The University of Akron.

Today there are two journals, *History of Psychology*, published by the APA, and *JHBS* and there are other publication outlets as well, such as the *American Psychologist*, which did not publish much history in 1965. Two other learned societies—Cheiron and the Forum for the History of Human Science (affiliated with the History of Science Society)—were formed after the organization of APA Division 26, which are also dedicated to historical research.

Accordingly, the structures needed to carry out the historical enterprise are firmly in place. More will be built, of course, old ones will be refined and improved, but in the years since 1965 the basic tools have been created. Future historians of psychology now receive systematic training. The cohort of self taught, autodidacts are being joined by sophisticated historiographers who know how to exploit the resources and methods that now exist. Consequently, one would expect our current historiography to

be richer, more nuanced, more accurate, more provocative, and more useful and valuable than the history of 35 or so years ago. Is it? Well, yes and no. The field is roughly directed into two major kinds of historiography.

Internal, Linear, Narrative Traditional History

Today it would appear that we have at least two kinds of historians of psychology, and of course they produce more than two kinds of history. The first history can be called internal, narrative, linear history. It deals with how the past led up to the present. This kind of history addresses several related questions. It should help students see how their different courses relate to one another. It explains how it is possible for one discipline such as psychology to include both the nervous system and the "aspirations of the human spirit?" It also traces how contemporary psychology came into being and what roads were taken to get here today. These are questions that delve into the who, what, where, when and why of the discipline as a whole. The answers are found in a history that is largely internal, emphasizing ideas, concepts, and the people and places that are associated with them. Texts of this genre are the standard setting books by Boring (1929, 1950) and Murphy (1949, 1972).

One of the devices for assessing the status of any field in psychology is to describe the way it is presented in standard textbooks. These texts usually represent a consensus that is not considered controversial. They also try to be comprehensive to enhance market appeal and thus be widely adopted by mainstream psychologists. There are currently almost a dozen pretty good undergraduate textbooks in the history of psychology, all of which are rather similar to one another and clearly exhibit their debt to Boring and Murphy's classic texts.

Let me describe two current examples as anecdotes to indicate the status of the field as presented by psychologists, acting as historians:

Recently (2000) a publisher asked me to review 14 chapters of an outline and manuscript of an undergraduate history text that they were considering for publication that I refer to as A. I chose to compare it to another textbook B (1999), currently on the market, and thus a convenient comparison and contrast.

These two books present almost exactly the same chronologies and are about the same length. A is 14 chapters with 441 pages, and B has 17 chapters for 580 pages. They are standard USA-centered history textbooks, which tend to downplay the philosophical, European, proto-psychology, which preceded the empirical, laboratory psychology and—as usual—involve the ancestors of Wundt and James. (It should be pointed out that the roots of psychology in the Western tradition beginning with the ancient

Greeks are considered too remote and thus get short shrift from most modern textbooks). Both texts arrive in Leipzig by Chapter 4 and end with the recent rise of cognitive psychology. Both texts present the "received history of psychology" without any significant departures from the canon. These volumes appear to reflect a consensus about the history of psychology.

Then I had the opportunity to read another recent contribution to the history of psychology intended for students (as presented in the outlines of 26 chapters) published in the winter of 2003. Donald K. Freedheim (2003) is editor of "Volume I, History of Psychology" which is the introduction to a 12 volume work, *Handbook of Psychology* by Irving B. Weiner (2003), who is editor in chief.

In *Volume 1, The History of Psychology* there are 26 chapters written by 59 authors (and co-authors) all but four of whom are members of APA or APS, or both. In my opinion, 25 of the chapters fall into the category of 'received' or 'traditional' historiography, only one seems to be within the domain of "new" or "science studies" history.

I think that the two texts and these chapter outlines may serve as a useful illustration, for our purposes, of the status of the history of psychology as written by psychologists. Without a detailed examination, the content of these two examples seem remarkably similar to that of any standard undergraduate history of psychology textbook. The major deviation in the edited volume comes with several chapters given to professional, applied psychology and it devotes more space to newer content areas like health psychology, ethnic psychology, and women/gender topics than the more traditional textbooks do.

This traditional, familiar, "classical" history assumes progress in which the present is seen as an improvement over the past. Therefore, we can understand the present in terms of the past from which the present emerges. So, for example, the contemporary intelligence test has multiple origins: later 19th century society and education, the beginnings of developmental psychology, the kinds of people Binet, Terman and Merrill, and Goddard were, the available statistics, and we can even go back to ancient ideas about the nature of the mind and the origins of intelligence, and its lack. Further on we learn that multiple sources were at work and which determined that intelligence (the mind) could indeed be measured and the way in which it could be done. And we are told that this method of determining intelligence has value in advancing human welfare, for example, by placing children in a school system in ways that would be most beneficial to them. This 'history' of intelligence testing contrasts sharply with a 'science study' account of the same topic which we will now examine.

Science Studies: External Histories of Psychology

There is, at least, one other kind of history of psychology being practiced. We find a history of psychology written by non-psychologists under the influence of various French "theoreticians," popularly Michel Foucault (1926–1964). The generic title of their field is 'science studies' in which the science is not really of importance. But some of these have selected psychology as the science of focus.

Practitioners of science studies typically have no training in psychology—one practitioner was offered a membership in the American Psychological Association (APA) in recognition of his real contribution to the history of psychology—refused, in order to remain a "member of the loyal opposition." Many people in the field of science studies are not well informed about psychology either as a discipline and/or profession. They are more outspoken about what they do not like about psychology than certain about their actual knowledge of the field. In fact, they sometimes insist or strongly imply that their lack of information about and involvement as practitioners is a virtue that insures a fresh point of view about the field. They tell us that they are able to provide an objective appraisal of psychology and its history, which cannot be done by the psychologist-as-historian. They frequently make the analogy that, they say, the history of medicine was just an exercise in self-congratulation when it was written by physicians as an adjunct of their actual practice of medicine. It was only, they say, when the *historians* came to study medicine that a "critical" history could come into existence that would bring enlightenment to the field. (Words like "hermeneutics," "liminal," and "critical" are an important part of this incantation). I think that it is safe to say that many of the "science studies" people do not like psychology, or psychologists, at all and some are even, in fact, quite hostile. We might call them "psychophobes." They are conspicuous in the discussions at the meetings of the Cheiron Society.

CARSON'S PANOPTICON

My richest experience with the 'science studies' version of psychology took place in June 1988 when the Cheiron Society met at Princeton and one of the papers read was entitled "Is the Mental Test a Foucaultian Panopticon?" The author was John Carson, identified as in the "Program in the History of Science": at Princeton. His affiliation and the acceptance of his paper by the program committee is sufficient endorsement that it is worthy to serve as an example of their "new history of psychology." Carson is

currently on the faculty of the University of Michigan, and is therefore a major player and this is not a straw man.

A little background is in order here for those who are unfamiliar with "science studies" and Michel Foucault (1926–1984). He is a French essayist; I cannot call him an historian or even a philosopher, who is much admired by some "science study" people. In fact, to an outsider he seems to be central to the whole field. Apparently Foucault suggested (in *Discipline and Punish*, 1979) the panopticon as a model of the means and method for controlling human behavior that characterizes modern society. Carson takes this suggestion and carries it to a rather special focus.

The panopticon was originally conceived by the 18th century philosopher Jeremy Bentham (1798–1832) as a model prison. It consists of a central tower or observation post from which guards have full observation of the prisoners, isolated in solitary cells on the periphery. The guard can observe many prisoners, who cannot see their observer. [Some were actually constructed. One in Philadelphia still exists and is now open as a tourist attraction]. Carson then asks the question, a naïve rhetorical device, is a "mental test," really a panopticon, a perfect prison? By mental tests he means a group intelligence test to be used with children. As I sat in the audience and heard this read, for the first time, my reaction was that it was a clever parody of a "science study." I did have some qualms that this young man, probably a graduate student, would write something that might well offend a power figure. But as time went on I realized that he meant it, this was serious stuff, and I was the only person in the audience who said this 'emperor' (and Foucault) have no clothes on. Any psychologist should be able to tell you that intelligence tests are not 18th century prisons, not even as a farfetched analogy or as John C. Burnham (personal communication, Feb. 2001) describes it "the Carson paper is using postmodern metaphor, rather than constructionism." Carson's paper seems to me to mean what it says. I think we should respect his opinion. But things are not always so direct. Since I did not want to misrepresent Carson (and his point of view) I sent a copy of my APA 2000 speech, designed for oral presentation, to him and to two nonpsychologist historians of psychology for rebuttal and correction.

The title of the Carson (1988) paper, to remind you, is a question, "Is the Mental Test a Foucaultian Panopticon?" and by page 4 (of the eight-page manuscript) he answers ". . . it would seem that the system of mental testing has much of the Panopticon about it." And as a standard debating device I can now restate the premise and say, "yes a group intelligence test for use with children is, indeed, a panopticon, that is an 18th century prison." We should note, in all fairness, that Carson characterizes this as a "caricature" of his argument. (Carson, 2002). He suggests that a substitute

for my conclusions about his Foucaultista (my preferred way to characterize a Foucaultian point of view)[2] panopticon is "Any psychologist should be able to tell you that intelligence tests are not shaped by modern understandings of power and normalization." It is too late to hear what, for example Lewis Terman (1887–1956) and Maud Merrill (1888–1978) would say about all of this. Would they be surprised to learn that the Stanford-Binet (1937) is all about "power and normalization?" They thought they were measuring human ability.

My original, oral presentation of this chapter in 2000 offered only the Carson-panopticon as an example of the history of psychology as seen by a non-psychologist, historian, psychophobe. However, one of the later readers of that version suggested that it would be "a mistake to focus so much on just one paper. I think that adding several parallel examples of different varieties would be more profitable" (J. C. Burnham, personal communication, Feb. 2001).

The two non-psychologist historians of psychology seem to feel that the manuscript they read was too blunt, too personal, and perhaps—not as sophisticated as it should be to comprehend Carson's position. With this in mind it seems only fair to look at another history of psychology from a Foucaultista point of view.

HERMAN'S *THE ROMANCE OF AMERICAN PSYCHOLOGY*

This second example, an anecdote, was a discovery prompted by a statement (boast? confession?) by "Ellen Herman (1996) on the Genesis of Her Book," *The Romance of American Psychology: Political Culture in the Age of Experts]* (*Cheiron Newsletter*, 1996). She said, "I had never taken even an introductory course in psychology. (I still haven't)." (Herman, 1996, p. 4).

When I was writing this essay it occurred that I should contact Herman for further elaboration of her statement. I asked if her lack of instruction in the subject matter was advantageous to her role as a historian of psychology? Herman denied that she concluded that this was an advantage. She replied that her "location in history (rather than psychology) had a great deal to do with the sorts of questions I asked (and did not ask) about psychology's history." (Herman, 2002).

[2] This word with the ending "ista" is not entirely a neologism. It is, I think from the Spanish, as in "Sandanista" or—more recently—the *NY Times* and *The Village Voice* speak of a "fashionista", meaning a fanatical adherent of a particular point of view. A true believer. A guerilla. Bearing a banner with a strange device FOUCAULT.

Herman's *The Romance of American Psychology: Political Culture in the Age of Experts* is a very large target. (And) It would be a healthy exercise to go a few rounds dealing with its meretricious content but, alas, time and space restrict. Instead let us note that from the misuse of the technical term 'romance' in her title to the woefully inadequate index at the end there are many problems.

Herman and *The Romance* are much more formidable than Carson and the panopticon. It is longer (at 406 pages), has more footnotes, is less focused and therefore deserves a little more attention than the Carson panopticon. A more descriptive title would be "Selected Social-Political Activities By Various Behavioral Scientists and Others Between 1940 (approx.) and 1970 (approx)—with a PS about Feminism."

This is not going to be a book review but two specific factual errors do demand notice because they leap off the page. APA did not make training in psychotherapy a requirement of graduate education in psychology in 1947 or any other time, as Herman states on p. 259. The members of Division 3—(APA experimental psychology)—APA officers and others clearly would consider such a proposal preposterous and untenable. Just what text or discourse or dream Herman has misquoted or misinterpreted is unclear but only someone with no knowledge of the nature of the psychological world would ever imagine such an event. Herman, also, for some reason, repeatedly and consistently identifies the estimable Dael Wolfle as Dael Wolf.

Herman does not use the words "psychology" or "psychologist" with any specificity. Almost any definition might well be subject to some criticism but this non-psychologist uses both terms as a layperson might in casual conversation, in a context outside academic and professional limits. Neither 'psychology' nor 'psychologist' is in her index. Although both terms have legal definitions and are recognizably restricted in academic and professional circles, Herman does not feel the need to abide by these definitions, when she says:

> "... what does the term "psychology" mean? My use of the term "psychological" does not stop at the margins of an academic discipline or the boundaries of a professional job category. [!] Rather it indicates an emphasis on analyzing *mental processes, interpersonal relationships, introspection*, and *behavior* as a way of explaining both individual and social realities (p. 45)." (emphasis added)

None of these indicated terms are included in her index. Her use of the term "introspection" is particularly egregious since it does have a quite technical meaning to a real psychologist. (see Popplestone and McPherson, 1988).

The American Psychological Association has quite specific, defining requirements for membership but Herman seems not to recognize that these restrictions exist. She is willing to include anthropologists, sociologists, pediatricians, social workers, psychiatrists, psychoanalysts, linguists, politicians and—above all—journalists. As Herman sees it, they are all psychologists and they all deserve whatever she hands out as 'prizes.'

It seems that psychology and psychologist are terms that have no extra-Herman meaning but rather are whatever Herman says they are. The amorphous quality of her selected subject matter is compounded by the pervasive imposition of the point-of-view of Foucault. We, briefly, encountered Foucault in the Carson-panopticon example of history-of-psychology by a non-psychologist and here he comes again, although in *The Romance* Herman does not acknowledge or cite his utility. Foucault is omitted from her Index. However, she does include him her autobiography in the 1996 *Cheiron Newsletter* in a list of 21 "scholars," she says, "without whom my own work would have been unthinkable . . . these scholars helped to further my education and shape my thinking about psychology and history" (Herman, 1996, p. 5).

The Index also omits the conceptual abstractions "Power" and "Knowledge," which must be mentioned in the same breath as the name of Foucault. Herman's dependence on Foucault for her "theory" of history—in general and for the specifics of her narrative about psychology in the post-war era are crucial to understanding what this utterly-naive-about-psychology historian writes about the recent history of psychology.

THE FOUCAULTIAN OR FOUCAULTISTA SUBTEXT

As I indicated before, Foucault seems to be central to an understanding of one school of current historical writing about psychology between ca. 1940 and the present, ca. 1995 (the publication date of Herman's *The Romance*). We will not try to summarize or present a neat account of the Foucaultista position here but simply demonstrate its operation in the writing of the history of psychology by people who are not psychologists, such as Carson and Ellen Herman. She is unfamiliar with the field of psychology (other than with the error prone certainty of the autodidact), and relies on Foucault's unusual notion of 'theory.' It is necessary to make clear to the psychologist reader for whom "theory" has particular references in the philosophy of science that this is not what the Foucaultistas mean by "theory." For them "theory" is a series of more or less articulated beliefs, about the interpretation of science, literature and the arts based on assumptions about the hidden meanings of the evidence being considered from

the point of view of feminist theory, gay theory, postmodern theory, etc. These currents are determined by the magic of "deconstruction." For example, as we have seen in the Carson example, a contemporary group test of intelligence is redefined, *presto chango*, and *mirabile dictu*, as actually an 18th century prison (Carson, 1988).

Foucault offers the world system of beliefs in which poorly defined political forces have seized loose control of information and knowledge and are using them to oppress the wretched of the earth.

Standard references to Foucault vis-à-vis the writing of history seem to agree on his focus:

> "French theory and Anglophone facts . . . power/knowledge." (Smith, 1997, p. 859)
> "[Foucault] prefers the compound term "power/knowledge." (Windschuttle, 1996, p. 129)
> "[Scott] . . . accepts Foucault's notion that knowledge constitutes power and domination." (Iggers, 1997, p. 131)

This knowledge/power formula is not exactly revealed wisdom, Francis Bacon observed "Nam et ipsa scientia potes tas est" in *Meditations Socrae*, 1597, but for the Foucaultistas it is a revelation that underlies everything else that they do. Herman (1995) begins her opus with the extraordinary declaration that:

"The subject of this book is based on an extraordinary quest for power" (p. 5). She then repeats this mantra in more or less identical terms in her descriptions of how psychology developed as a field of knowledge and a profession: Despite Herman's omission of citing Foucault, it is apparent that his influence on her thinking is pervasive and salient, as indicated by the following passages from *The Romance*:

> "the story of psychology's rise to public power . . . " (p. 6)
> "imagine themselves more powerful than they actually were" (p. 6)
> "psychological experts' rise to power" (p. 9)
> "gave them their first taste of power" (p. 9)
> "psychological knowledge and power" (p. 9)
> "assumption about knowledge and power" (p. 10)
> "obstacles littered their path to power" (p. 12)
> "the exercise of power in recent history" (p. 15)
> "Psychological wisdom had not yet been put to the repressive purposes that would appear such defining features of its postwar public career" (p. 81)
> "knowledge and power" (p. 121)
> "intellectuals new to power" (p. 121)
> "gaining power themselves" (p. 173)
> "to enlarge psychology's jurisdiction" (p. 123)

> "achieving the authority they wanted in the post war decades" (p. 126)
> "psychology as a weapon" (p. 130)
> "unwitting servants of power" (p. 189)
> "psychology's rise to power during the postwar decades changed ordinary Americans' expectations of their lives" (p. 305)
> "experts never hesitated . . . with an eye toward enhancing their scientific standing and improving the effectiveness and marketability of their technological talents . . . " (p. 305)

A review of *The Romance* by the greatly respected senior psychologist, M. Brewster Smith (1996), bears a title with a question mark, "Quest for Power?" It is a healthy counter to the historical revisionism of Herman. It is important to hear from Smith who points out that he was there during many of the episodes that Herman can only know from her selective reading of secondary documents. Even though Herman is a professional historian, she clearly distains to the use of primary documents, has not examined archival holdings, and no where does she confirm or expand her understanding through interviews and oral history. Apparently her reliance on theory (i.e. Foucault) and secondary sources was enough. We need also to note that Brewster Smith is nowhere mentioned in the index or the text.

The whole of Smith's review-memoir needs to be read as an antidote to the virulence of the Herman account. He says, "As an active participant in precisely the period of American cultural history that Ellen Herman covers . . . I find her historical perspective revisionist and troubling." (p. 4). He lists psychological activities in World War II and summarizes them as activities that have often been pointed to with pride; as such they are sitting ducks for revisionist analysis.

Smith recalls that "The real professionals whom I knew. . . were devoted to the jobs they were doing, with no sign of second thoughts about advancement for themselves or the professional disciplines they represented . . . many examples might be cited of professionals using their training and imagination to do well what needed to be done . . . " (p. 5). The principals, he says, " . . . devoted hard work and imaginative improvisation rather than scheming for power and influence . . . " He concludes that

> I cannot help reading her book as a whole as congruent with the neoconservative stance that expansion of governmental activity is always bad; that aspiration to advise and influence government is always suspect; and that our faulty attempts to apply psychological perspectives in the interest of human betterment do more harm than good . . . she drops gratuitous disparaging remarks about a whole array of actors and events that I continue to hold in historical respect (p. 6).

There is ample evidence that Herman is more interested in burying psychology then appraising it objectively or even bracing it with cold reproach (which any psychologist can do with no difficulty at all.) The following passages from *The Romance* are indicative of this bias:

> "While psychologists were the first to admit, to one another at least, that they actually knew appallingly little about their supposed competence . . . (p. 133) . . .
> ". . . thus garnering financial support under false pretences" (p. 134)
> [which] ". . . leaves little doubt that psychological expertise had tremendous repressive potential . . . " (p. 236)
> [and reinforces the] "suspicion that psychological expertise might have oppressive consequences" (p. 255)
> [For example] "community mental health was so much rhetoric splattered over an unattractive reality of domination by elites" (p. 256)

CONTRASTING INTERPRETATIONS OF THE BOULDER CONFERENCE, 1949

Practitioners of science studies characterize the history of psychology with a skeptical and wary attitude toward the activities of psychologists and sometimes express their direct hostility and disparagement or psychophobia. Herman runs the full range of critical attitudes but she rarely takes a point of view that allows her to be tolerant or understand how psychologists perceive their profession or the problems of psychology. Perhaps this is because she knows so little about it. Perhaps the difference in approach and conclusions can be illustrated by contrasting the *The Romance* account of the psychologists' attempts to adopt professional standards and licensing procedures at the so-called Boulder Conference, with the narratives of it by psychologists. The Boulder Conference is best understood if placed within the context of the issues and events that led up to it in 1949.

Psychologists offered their services to the public from the beginning involving school systems and other institutions. But the lack of definition of who was qualified to offer service and what training they should receive and what psychological materials (tests) should be available was all quite ambiguous. But from the beginnings of psychology in this country the psychologists have been responsible and have tried to 'police' themselves by specifying educational requirements, and establishing professional organizations with membership requirements. In the 1940's and 1950's much effort was directed to state level licensure and certification, which would provide legal definitions of who was (and therefore who was not) a psychologist and the tasks in which they would be considered to have competency.

By 1970 this task was pretty well complete nationally, despite the vigorous opposition of organized psychiatry and medicine and other professions that felt threatened by the intrusion of psychologists into their domains.

The Boulder Conference of 1949, was a pivotal event in the culmination of psychology's efforts at self-regulation. It specified the education and training of aspiring clinical psychologists to be defined as scientist-practitioners. This became the norm, and then the target of those who wished to reform the standard. There are at least three good accounts of the Boulder Conference written by knowledgeable psychologists (Misiak and Sexton, 1966, pp. 216–217, Routh, 1994 and Albee et al., 2000). The first two were available to Herman but were not used. The second, of course came too late for her to be able to benefit from it if she had so wished.

Herman gives little acknowledgement to the long time, well documented efforts to professionalize clinical psychology (Misiak and Sexton, 1966, pp. 194–221). Instead she challenges psychologists' motives to help people, instead viewing it as an attempt to gain institutional power, to preserve the distinction between basic research and practice rather than dissolving them into one uniform mechanism of control. The difference between her outside-of-psychology treatment (Herman, 1995) and that of a psychologist-acting-as-historian (Routh, 1994) is apparent from their contrasting introductions to a discussion of "the Boulder Conference."

> The first effort to take stock of feverish [!] post war efforts to establish new training programs in clinical psychology came in August 1949 in Boulder Colorado. Thanks to an NIMH grant seventy-one psychologists from around the United States met to consider the future of clinical training on the graduate level. (Herman, 1995, p. 259)
>
> The Conference of Graduate Education in Clinical Psychology was held at the University of Colorado, Boulder, in August 1949. It was sponsored by the National Institute of Mental Health and was attended by approximately 70 participants (Raimy, 1950). Perhaps a third of these were at one time or another among the leadership of Division 12 (of the American Psychological Association, Division of Clinical and Abnormal (sic) Psychology (1999 membership 1,148) and many were leaders in psychology-at-large. (Routh, 1994, p. 123)
>
> Problems were nevertheless immediately apparent, although no one present at the conference seemed to know exactly what a clinical psychologist was or what a clinical psychologist did, they quickly agreed that a doctoral degree was necessary to do it [!]. The Ph.D. was necessary "to protect the public and create some order out of the present confusion . . . " (Herman,1995, p. 259).
>
> The model of clinical psychology training agreed upon . . . aimed at attaining scientist-practitioners. The didactic and scientific part of

> the training was already understood by graduate schools . . . what was less familiar was the attempt to include "experimental training as well - practica and internships and to allow students to do research on clinical as well as traditional topics . . . a historic synthesis between the goals of academicians and clinicians." (Routh, 1994, p. 124)

Neither Herman nor Routh explain that the requirement of the Ph.D. was—and is—controversial, as the role of the MA psychologist vis-à-vis the public and the terminal degree psychologist has a long and contentious history, but one which is peripheral to this discussion. But any psychologist knows about this problem, which is especially severe in the applied technical fields. Recommending that clinical psychologists obtain the Ph.D. before being licensed was a bold and not always popular point, particularly when there was the possibility of it becoming a legal requirement. Boulder participants were very concerned with how the training of clinical psychologists was defined and they (and the rest of psychology) were determined to act in an ethical and conscientious way to protect the public. Herman finds these efforts amusing and revealing.

Herman's pervasive distain of psychology and psychologists also is evident in her criticism of psychotherapy. She describes the 1952 publication of a study on the efficacy of psychotherapy by Hans Eysenck as the "first really damaging critique" (Herman, 1995, pp. 260–261). She notes that Eysenck concluded that psychotherapy provided no benefit and, in fact, was damaging. She cites his work approvingly, saying that "Eysenck's heresy provided psychotherapy's defenders with years of work: . . . they assiduously devised ever more creative ways to design and measure psychotherapeutic outcomes . . . [this] evolved into a small industry." (Herman, 1995, pp. 260–261). This is the end of her paragraph and her discussion. She does not go on to describe the results of what is now more than fifty years of research on this topic but only refers the reader in a footnote to a chapter in a book for which there is no citation beyond the page numbers. A short, objective, layman's presentation of the status of psychotherapy as revealed by fifty years of research can be found in Morton Hunt (1993) especially pp. 595–598. This could have been cited, or even quoted by Herman but she did not.

A further comparison of Herman and Routh's contrasting accounts of Boulder would require too much space here. But it would emphasize that Herman does not understand the previous history of trying to regulate clinical psychology education and practice, she does not understand the personalities involved, and does not have her facts straight, as we have seen, (APA never made psychotherapy mandatory for all graduate education in psychology). She implies that the psychologists (or the APA?)

in some way ripped off the government, by implying that they exploited "government generosity," "thanks to an NIMH grant." In fact the VA and NIMH, which were seeking qualified clinical psychologists for positions in VA facilities asked the APA to assist in identifying qualified programs. These programs, Herman seems to feel, were designed as the products of impaired consciousness, "feverish."

The Boulder conference provided structure during a period of uncertainty about educational content and the field in general. But Herman dismisses these efforts from her position as a non-psychologist revisionist. Remember her attack, "no one at the conference seemed to know exactly what a clinical psychologist was or what a clinical psychologist did (p. 289) . . . the practice of psychotherapy in particular was equally baffling (p. 266) (Herman, 1995, p. 260). The whole point of Boulder, of course, was to begin to provide those specifications. But Herman prefers to ridicule the serious efforts of truly responsible people.

The Boulder conference set the model for the training of clinical psychologists for that era and is still in effect, with some variations. Changes in training requirements have emerged in response to the large number of clinical psychologists who define themselves as practitioners in independent, or quasi independent practice and whose vocational goals are application and practice with little or no interest in or expectation to do research.

The importance of Boulder in the whole history of psychology is that it was an attempt by psychologists themselves to specify what constitutes adequate training and thus to define explicitly the professional responsibilities of clinical psychologists offering professional services to the public. It was an ethically responsible attempt at self-regulation by the psychological community in the absence of any extra-community policing and control.

STEPHEN JAY GOULD'S *THE MISMEASURE OF MAN*

Carson and Herman are just two examples but a folklorist will point out that in a narrative "two" is not enough, and that "four" is too many. So we have three little pigs, and three blind mice, three fates, but so far only two psychophobes. Let us add a third non-psychologist, acting as an historian of psychology but adding some variety since he seems not to be a part of the Foucault Francophone parade. The third example of a wayward external historical commentator is Stephen Jay Gould, the late professor of paleobiology at Harvard.

A follower of his, Andrew Hamilton (2002), in a eulogy published in the *History of Science Newsletter* salutes his master as "unrelenting in his criticism of creationism, human intelligence testing, and other pseudo sciences" (p. 6)—a sentence that invites a red pencil treatment. This sweeping generalized assault on the credibility of psychology follows the hoary line that Gould articulated in a volume *The Mismeasure of Man* (1981, 1996) in which he criticizes the hereditarian arguments put forward by some nineteenth and early twentieth century psychologists who sought to measure human ability. But Gould curiously ignores current writings by psychologists on psychometrics. In fact, like Herman, he boasts of his ignorance "I know little about the technicalities of item selection in mental testing or the social use of results in contemporary America" (Gould, 1996, p. 40). But that does not prevent him from consuming 352 pages (Gould, 1981) and 444 pages (Gould, 1996) about topics of which he admits he knows little. In particular, Gould and Hamilton enjoy the old, tired, unkind, boring game of kicking Henry Goddard (1866–1957) around one more time. (One wonders where the psychophobes would be if Goddard had never existed, perhaps they would have had to create him.)

The attacks by Gould and Hamilton are all very familiar but with one piquant addition, that Gould accuses Goddard of retouching some photographs included in the publication of the Kallikak study in order to make them look more "diabolical." (This word is not used at any point by Goddard but is wholly a judgment of Gould.) This accusation of fakery prompted full scale defensive responses by a series of psychologists, none of whom had any real need or motivation to defend poor dead Henry Goddard other than a simple revulsion at Gould's attack on him. He may well have been wrong about many things but his integrity had never been attacked until the biologist Gould reached beyond the grave to accuse him of falsification of data. The whole unsavory episode is well told by Zenderland (1998), the author of the definitive biography of Goddard and a non-psychologist, historian who nevertheless is led more by a respect for the empirical record than by a need to advance a point of view, a "theory," or a "prejudice."

Gould's villains are all from the earliest periods of human assessment even those who may be called psychologists (as opposed to those from various kinds of outdated disciplines like phrenology), tend to be pioneers long dead. His most conspicuous targets, with death dates, are: Binet, 1911; Burt, 1971; Goddard, 1957; Spearman, 1945; Terman, 1956; Thurstone, 1955. All were dead within 10 to 70 years preceding the date of Gould's own publication. One of his examples is Jensen born in 1923 and still alive in 1981 but he omits David Wechsler (d. 1981) whose extremely popular tests

of intelligence ranging from infancy to senescence and dating from 1939 to 1989 are—for better or worse—much better indications of the field of clinical application in the year of the publication of *The Mismeasure of Man*. In fact there is no reference to Wechsler in the index of either of Gould's (1981;1996) two editions of *Mismeasure of Man!*

Gould's selection from the dim past allows him the easy triumph of indicating just how wrong some psychologists have been but he never admits that they might be doing better today or even in 1981. It is as if I were to describe the current state of physics and of physiology citing only the work of René Descartes (1596–1650), by stressing the role of the pineal in the interaction of the soul and the body, and then ignoring everything in physics and biology which took place after the death of Descartes.

CONCLUDING THOUGHTS

Is the Carson paper typical? Is Herman on the 'cutting edge'? Did Gould, alone, find fraud in Goddard? Have I picked extreme examples for the purpose of this discussion? Not really. History can be written from various viewpoints, as we have seen, feminist theory, Marxist theory, post-colonial theory, postmodern theory, gay theory, whatever. This may be all very well for the "science studies" person working his or her merry way toward tenure. But if the psychologist who wants to understand how the enterprise, for example, of measuring intellectual ability or intellectual achievement came about and why it takes the form that it now has, this is no help at all. I put it to you.

When a non-psychologist staff member of *Contemporary Psychology* reviewed a history book in 1994, she took the psychologist authors to task for not utilizing "the most recent developments in the writing of history" (Gullickson, 1994). If she means by that that the authors failed to see that intelligence tests are really 18th century prisons then I think they did so because they honestly did not believe that this would increase understanding in any way.

However, this second group of historians, the "science studies" people, may find it exhilarating to "show" that an intelligence test is an 18th century prison, sort of a cadenza, remarkable for its style and verve, a demonstration of facility, like a mannerist painting that is intended to astonish us by showing that a bowl of fruit can be made to look like a human face (Archibaldo, 1527?–1593). Here the point is the *technique* that is demonstrated, the "wow" factor, it is not the accuracy of the content or its ability to illuminate. But, we ask, is that enough? Is that the function of history?

However, it would appear that the historians of psychology, the textbook writers, publishers, adopters, and so forth are paying little or no attention to the "science studies" people. The psychologist-as-historian continues to produce received, traditional, linear history of psychology (Only one chapter out of 25 in the Freedheim-Weiner volume is "a science study"). An informal survey of current undergraduate history of psychology texts was conducted (informal means what I had in the office, N = 8 published between 1976–1999) and a survey of the name indices did not turn up any references to Foucault.

I do not want to dismiss these science study people but I also feel that some are frequently so far from the content of psychology and so opposed to an internal history that the psychologist might well say that they miss the point. But perhaps they are also correct in saying that the psychologist, socialized into the particular culture that is psychology in our times may be unable to see psychology, warts and all. Perhaps we are unable to 'decode' the message or 'deconstruct' the text?

The psychologist-acting-as-historian is likely to believe that there are real, actual facts that reflect reality. All of our training, and probably our preference, are for the existence of actual quantifiable, sensorial, verifiable things. Events can be replicated. Consensual validation is possible.

Psychologists-acting-as-historians of psychology were then, and are now, tied to the world of facts. Most psychologists have been educated to deal in tangibles, observed and quantified, and at the best this orientation has come into their history writing as well. Franz Samelson (1975, 1979, 1992) is an excellent example of the psychologist-as-historian who is interested in the utility of the empirical approach to history, who wants to find out what really was said, or done, as opposed to what might have been done. Samelson is also a worthy example of the psychologist who is not merely reciting Whiggish tales of improvement and accomplishment as a self-congratulatory exercise. He is a righteous judge when he feels that the psychologists have erred—more so than this observer necessarily would support. But I do think it is important to acknowledge psychology's faults in order to increase it's opportunities for improvement. Let's get those sinners and backsliders!

Whether psychologists can be excused for what now appear to be errors as 'simply part of the prevailing culture' or condemned as, for example, racists is not our present concern. Samelson rises to the accusation that psychologists bear responsibility for the immigration restriction laws of 1924 (Gould, 1981, pp. 187, 261–262, 323) and demonstrates that whatever their beliefs then, and no matter how repugnant they are to contemporary psychologists, in fact psychologists bear little or no responsibility for the legislation being protested. Individual psychologists *may* have approved

or *even supported* the rationale for restrictive immigration but they had no organized political power and should not be either praised or blamed for the social policies of the past.

For the psychologist, history involves memorial behavior, perception and other psychological processes. Psychologists know a lot about memory, how fragile a thing it is—influenced by the circumstances under which the material was acquired, by the nature of the material, the method of learning, the method of measuring, the values and expectations of the rememberer and so forth.

Psychologists do not expect memory or history, to be absolute, perfect, exact (Do the science studies people?). Memory is embedded in the situational context and reflects the many complex variables that went into this creation. Similarly, the psychologist-as-historian knows that history will be temporal and contextual, that the main focus will be what the current times call for, not a perfect, absolute, Platonic ideal. Only people in academic departments of English and history think that it can be something more.

I think that I have made it clear that my sympathies lie with a history of psychology that is traditional, linear, and largely internal. But I must also acknowledge that the second kind of history, the Foucaultista deconstructive history, may well have its merits. We are all the better for being alerted to the possibility of bias (gender, ethnic, social class, etc.), that may creep into our meditations. But I hope that we always remain alert to the danger of over personalizing the events and straying too far from the facts. However, if the deconstructionists are right, then we have a serious problem since the mainstream of the history of psychology—certainly as represented in our textbooks and the Division 26 program—continues on a traditional (conservative?) path first shown by Boring and Murphy.

It may well be that the history of psychology, as written by psychologists, and passed onto the next generation of psychologists is missing the point. Are we teaching the past, as it was taught in the past, to the exclusion of the new? The now?

I have described the recent past of the history of psychology and have tried to indicate the current status of the field as I see it. It would seem the more modest and appropriate role for me is not to try to predict, direct or prescribe the future. The future belongs, of course, to younger scholars who may just be beginning their careers. But I (a grumpy old man) must be allowed to point out, as others have, that if they see farther than our generation has seen, it will be because they are using the scholarly culture we created. They are standing on our shoulders.

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HIGH-IMPACT RESEARCH PROGRAMS IN PSYCHOLOGY
QUANTITATIVE AND QUALITATIVE ASPECTS

Dean Keith Simonton

The English historian Thomas Carlyle (1841) once said that "Universal History, the history of what man has accomplished in this world, is at bottom the History of the Great Men who have worked here" (p. 1). This great-person theory of history had a direct impact on psychology's conception of genius. This is apparent in Francis Galton's (1869) classic work on *Hereditary Genius*. Here Galton defined genius in terms of enduring reputation, as gauged by "the opinion of contemporaries, revised by posterity . . . the reputation of a leader of opinion, of an originator, of a man to whom the world deliberately acknowledges itself largely indebted" (Galton 1892/1972, p. 77). This individualistic perspective appears most compatible with psychology's own disciplinary disposition. After all, psychological research is explicitly dedicated to the scientific study of *individual* cognition, emotion, and behavior. Not surprisingly, this viewpoint also permeates many textbooks in the history of psychology, such as Fancher's (1979) *Pioneers of Psychology*.

Yet not everyone concurs with this individualistic point of view—especially when applied to the history of science. Indeed, a staunchly anti-individualistic stance was advocated by E. G. Boring (1963), the influential American historian of psychology. Boring rejected the genius theory,

arguing that it is embarrassingly "personalistic." As a consequence, he argued for a "naturalistic" perspective that explicates the major events of history in terms of the zeitgeist (i.e., the "Spirit of the Times"). The psychologists called "great" are merely the agents of larger, impersonal forces operating in the intellectual climate of the discipline. Indeed, Boring believed that it in the future history could be written without the supposed big names because their causal status would be reduced to mere epiphenomena. "When that day comes," concluded Boring (1963), "... we shall look back—surely we shall or rather our posterity will—on the personalized history of science of the twentieth century with an indulgent smile and think: How egocentric and immature they all were in those days!" (p. 25). Boring's (1963) view has definite echoes in the ideas of contemporary historians of psychology. A good example is the so-called "new history" which, according to Furumoto (1989), "tends to be critical rather than ceremonial, contextual rather than simply the history of ideas, and more inclusive, going beyond the study of 'great men"' (p. 16).

I believe that Boring committed a grave error in affirming that the zeitgeist theory perspective on history is more naturalistic than is the great person or genius perspective. As a psychologist who has devoted more than a quarter century to the scientific study of creative genius in the arts and sciences, I am convinced that the individuals who populate the history texts can be considered to constitute "natural phenomena" (Simonton, 1984a, 1990). Such phenomena are certainly worthy of investigation using the full arsenal of theoretical analyses and methodological techniques (Simonton, 1999a, 1999b). Moreover, a substantial literature has accumulated on the psychology of science, a subdiscipline specifically focused on the cognitive, developmental, dispositional, and social factors underlying scientific behavior (Feist & Gorman, 1998; Simonton, 1988b). Just as significant is the fact that a sizable proportion of this literature has concentrated on the characteristics of eminent psychologists (Simonton, 2002). Accordingly, considerable headway has been made toward understanding what it takes for an individual to have a major impact on the intellectual history of psychological science. Although not endorsing Carlyle's (1941) extreme individualism, the scientific evidence now suggests that history is partially rooted in individual biography. In the case of the history of any scientific discipline, the portion of the personal biography that matters most is the individual's research bibliography—the very portion that invariably consumes the largest number of pages in the curriculum vitae of almost any notable scientist.

I will substantiate this generalization by reviewing the key empirical findings on this question.

NOMOTHETIC FINDINGS

I begin by discussing the quantitative characteristics of scientific research programs that are most strongly associated with high-impact careers in psychological science. I end by reviewing the qualitative attributes of research programs that also influence a psychologist's impact on the discipline's history.

Quantitative Characteristics

It is rare for a history of psychology text not to mention the prolific output of the discipline's founder, Wilhelm Wundt. Frequently the textbook's author will quote E. G. Boring's (1950) observations on the subject:

> his daughter's bibliography cites 491 items, where an "item" is taken as any writing, from one of less than a single page up to the entire 2,353 pages of the last edition of the *Physiologishen Psychologie*. If we exclude mere reprinted editions, but include all the pages of every revised edition, and adding-machine shows that Wundt in these 491 items wrote about 53,735 pages in the sixty-eight years between 1853 and 1920. In spite of all the many one-page items, Wundt's average adventure into print was about 110 pages long, with over seven such adventures in the average year. If there are 24,836 days in sixty-eight years, then Wundt wrote or revised at the average rate of 2.2 pages a day from 1853 to 1920, which comes to about one word every two minutes, day and night, for the entire sixty-eight years. (p. 345)

It is equally clear from this passage that Wundt not only was highly productive, but also that his prolific output extended over an extremely long career, from 1853 when he was 21 to 1920 when he was 88. It is also worth noting that the first edition of Wundt's *Physiologishen Psychologie*, which is often regarded as his single greatest contribution, appeared in 1873–1874, when he was in his early 40s. Amazingly, "The period from 1870–1879, during which Wundt published his *magnum opus* . . . was [also] the most productive period of Wundt's life in terms of individual publications" (Bringman & Balk, 1983, pp. 72–73). Yet Wundt's productive career can be considered pretty typical of high-impact psychologists. To make this case, I need to look at two key sets of empirical findings. The first concerns individual differences in creative productivity and recognition, the second longitudinal changes in output and impact across the career course.

Individual Differences

A large and consistent literature leads to the following 5 conclusions about cross-sectional variation in productivity and recognition:

1. Contradicting the common assumption that human abilities are distributed according to the normal or "bell-shaped" curve, the cross-sectional distribution of total lifetime output is highly positively skewed (Simonton, 1997, 1999c). As a necessary consequence, a small percentage of the individuals in any given domain of creative achievement account for the lion's share of the total contributions. This productive elitism holds for all of the sciences (Lotka, 1926; Price, 1963; Simonton, 1988b), including psychology (Simon, 1954; Simonton, 2002). For instance, Dennis (1954c) scrutinized four distinct samples of psychologists to determine "whether the aggregate publications of any generation of scientists are made up primarily of the work of the highly productive minority or are composed chiefly of the contributions of the less productive majority" (p. 191). The overall pattern holds across all four samples. Those psychologists who are in the top 10% in terms of output account for between 37 and 47% of all publications, with a mean of 41%. The bottom 10%, in contrast, ranges from 0 to 3%, with an average of less than 1%. In fact, the top half of the most productive psychologists are responsible for 90% of the total output, leaving the bottom half with the remaining 10%. Hence, the bulk of the psychological research can be credited to a highly prolific elite. Indeed, among these elite producers were such notables as Mary Calkins, June Downey, Knight Dunlap, Clarence E. Ferree, Shepard I. Franz, Charles H. Judd, James H. Leuba, Max F. Meyer, Lewis M. Terman, Edward L. Thorndike, Howard C. Warren, Margaret F. Washburn, John B. Watson, and Robert M. Yerkes. These names were all included among the "important psychologists" identified by Annin, Boring, and R. I. Watson in 1968.
2. The foregoing list of names implies that total productivity might be positively correlated with eminence, both contemporary and posthumous. This inference is amply justified in the empirical literature (Simonton, 1984a, 2002). In the first place, total output is strongly associated with the citations that a scientist receives in the research literature (Simonton, 1988b), a result that holds specifically for psychologists as well (Simonton, 2002). For instance, when Myers (1970) identified the most frequently cited authors according to references to their work in over a dozen prestigious journals, the 99th percentile in citation counts included some obvious big names in the field, such as William K. Estes, Leon Festinger, Harry F. Harlow, Carl I. Hovland, Clark L. Hull, Jean Piaget, B. F. Skinner, and Edward L. Thorndike (see also Endler, Rushton, & Roediger, 1978). Total productivity is also highly correlated with receiving

various professional awards and honors (Albert, 1975; Ashton & Oppenheim, 1978; S. Cole & J. R. Cole, 1973; Feist, 1993; Simonton, 1992a). Finally, the most prolific scientists, including psychologists, are most likely to receive considerable posthumous recognition, whether in the form of continued research citations or prominent treatment in biographical dictionaries, encyclopedias, histories, and other reference works, whether general or disciplinary specific (Simonton, 1984b, 1992a, 2002). For instance, Dennis (1954a) studied the long-term distinction attained by 19th-century scientists whose total output ranged from 1 to 458 publications, and found that the count predicted whether or not that scientist was later honored with an entry in a 20th-century edition of the *Encyclopaedia Britannica* (see also Simonton, 1984a). The fact that both contemporary and posthumous acclaim are grounded in productive output helps explain why eminence measures display considerable temporal continuity over historical time (Simonton, 1991a). For instance, Over (1982c) showed that the eminence of psychologists at the beginning of the 20th century correlated .72 with the eminence of those same psychologists more than a half-century later. Thus, at the top of the list were notables like William James, James McKeen Cattell, Hugo Münsterberg, G. Stanley Hall, Jame M. Baldwin, Edward B. Titchener, Josiah Royce, George T. Ladd, John Dewey, and Joseph Jastrow, whereas at the bottom were virtual nonentities like E. F. Buchner, A. C. Armstrong, and T. L. Bolton.

3. The primary reason why productivity and recognition are so strongly associated is that total output is highly correlated with the actual quality of that output (Crandall, 1978). For example, the best single predictor of a scientist's citation rate is the total number of publications listed on his or her curriculum vitae (Feist, 1993; Helmreich et al., 1980). In psychology, the typical intercorrelations range between .50 and .70, meaning that between a quarter and half of the variance is shared between the two variables (Rodgers & Maranto, 1989; Simonton, 1992a). The number of citations then provides the proximate predictor of the scientist's eminence by other criteria. In the case of psychology, citation counts predict peer ratings of eminence (Clark, 1957; Simonton, 1992a), election as President of the American Psychological Association (Myers, 1970; Simonton, 1992a), having a biographical entry in *American Men of Science* (Myers, 1970), and receiving honors like APA's Distinguished Scientific Contribution Award and the US National Medal of Science (Myers, 1970). Even among those who were already among psychology's elite, eminence was tied to output.

APA Presidents who published more frequently are more prone to receive citations in 37 widely used introductory psychology textbooks (Suedfeld, 1985).

4. The positive association between quantity and quality implies that the most prolific scientists produce not only the most high-impact contributions, but also the most ignored or non-influential offerings. This conclusion is endorsed by empirical research (Simonton, 1997, 2002). Most strikingly, the quantity-quality relation is governed by the *equal-odds rule* (Simonton, 1997). The ratio of citations to total publications (or the ratio of total cited publications to all publications) does not systematically vary according to a researcher's output (Platz, 1965; Simonton, 1985; White & White, 1978). For example, the number of citations per publication is not larger for those who are the most prolific (Davis, 1987; White & White, 1978). Hence, the most productive psychologists have not figured out a way to increase their success rate. These findings are mathematically most consistent with a straightforward model that specifies the number of citations to be a positive linear function of the number of publications plus random error term that has roughly the same variance as total publications (cf. Feist, 1997; Platz & Blakelock, 1960).
5. Although publications can take many forms, two genre enjoy primary importance: journal articles and books. Of these two main types of publication vehicles, it is the book that is most likely to have the highest impact on contemporary and subsequent psychologists. The superiority of books was indicated in two separate investigations. In the first, Heyduk and Fenigstein (1984) asked eminent psychologists to identify those "texts or articles . . . which have significantly influenced your work and though, both past and present, in your major area of psychology" (p. 556). Not only were extremely few articles mentioned, but also when a scientific paper was deemed influential a book by the same author usually proved even more so. For instance, Clark Hull's articles on "A Functional Interpretation of the Conditioned Reflex" and "The Goal Gradient Hypothesis and Maze Learning" were each mentioned twice, but his book on the *Principles of Behavior* was mentioned 7 times. Only one author out of the 39 most influential psychologists had more impact through an article rather than a book: Lee Cronbach, in his classic 1957 paper on "The Two Disciplines of Scientific Psychology." All told, fewer than 3% of these eminent contributors staked their fame on an article rather than a book. Furthermore, 92% of the works that influenced eminent psychologists were books or monographs, leaving only

8% to be credited to articles. The second inquiry corroborated these results by applying historiometric techniques to the careers of 69 eminent American psychologists (Simonton, 1992a). The number of cited publications, the total number of citations, and the number of citations to his or her single most cited work were all positively correlated with number the proportion of his or her output that was represented by books rather than journal articles. The superior impact of the more ambitious publications was also shown by looking at the psychologist's most frequently cited work. Although books only accounted for 17% of all the publications credited to these 69 psychologists, books represented 45% of those works that received the most citations. Examples of such high-impact book-length contributions include Gordon Allport's *The Nature of Prejudice* (with 198 citations) and Carl Hovland's *Communication and Persuasion* (with 135 citations).

Based on the foregoing findings, it would be expected that Wundt would be highly prolific and that his single most significant contribution would be a book. It is this body of work that helps secure his posthumous reputation as psychology's progenitor. Yet the research also suggests that not everything Wundt produced would be influential. In fact, much of his work was largely ignored in his own time, and continues unnoticed today.

Longitudinal Changes

Interestingly, the oldest quantitative analysis in the behavioral sciences concerns the relation between age and creative productivity. The first such investigation was conducted by Adolphe Quételet (1835/1968), a pioneer statistician better known for introducing the normal curve as the basis for individual differences. Although his data came from the careers of eminent English and French dramatists, much subsequent research has focused on other forms of creativity, including creative output in the sciences, psychological and otherwise (Simonton, 1988, 2002). Taken together, this large literature leads to the following 5 conclusions:

1. The relation between age and creative achievement is best described according to a single-peaked function, sometimes called an "inverted backward's J" curve (Simonton, 1988a, 1997). The specific location of the career peak tends to vary according to the domain of creative achievement (Dennis, 1966; Lehman, 1953). For psychology, this maximum point tends to fall in the late 30s or early 40s (Simonton, 2002). For instance, Lehman (1953) studied the careers of

50 individuals who made 85 major contributions to psychology, the raw data coming from a chronological table published in Flugel's (1933) *A Hundred Years of Psychology*. The peak age fell in the 35–39 period, with a median of 42.6 and a mean of 44.5. In a later inquiry, Lehman (1966) used a greater variety of sources, but arrived at the same general conclusion. Besides using a newer edition of Flugel's (1951) work, Lehman examined tabulations derived from the contributions listed other classic history volumes, including E. G. Boring's (1950) and Gardiner Murphy's (1949) *Historical Introduction to Modern Psychology*. In addition, Lehman scrutinized 1,530 important contributions by 1,002 still-living psychologists as listed in the classic introductory text *Experimental Psychology* by Robert S. Woodworth and Harold Schlosberg (1954). The peak for making a great contribution to psychology landed once more in the 35–39 age period, with a median and mean falling in the early to middle 40s. Furthermore, this career peak holds for more contemporary samples of psychologists. For example, a study of more than 1,000 academic psychologists concluded that "productivity typically began at a low rate in the 20s, increased to a peak around age 40, then decreased in the later years" (Horner, Rushton, & Vernon, 1986, p. 319).

2. The equal-odds rule that describes the relation between quantity and quality of total lifetime output also describes the relation between quantity and quality of output during any given period of a career (Oromaner, 1977; Over, 1989; Simonton, 1988a, 1997). Those periods in which psychologists produce the most total publications will tend to be the same periods in which they publish their high-impact work (S. Cole, 1972; Over, 1988, 1989). For instance, one investigation looked at the careers of 10 distinguished psychologists who had received APA's Distinguished Scientific Contribution Award: Wolfgang Köhler, Carl Hovland, Gordon Allport, Kenneth Spence, Edward Tolman, Carl Rogers, B. F. Skinner, J. P. Guilford, Donald T. Campbell, and Albert Bandura (Simonton, 1985). Their total output was first divided into high- and low-impact works according to the citations they received in the professional literature, and then tabulated into two longitudinal time series (using 5-year intervals). Statistical analysis showed that (a) the output of high-impact publications correlates highly with the output of low-impact publications and (b) the ratio of high-impact publications to total output stays roughly constant throughout the career, neither increasing nor decreasing systematically. The hit rate appeared age invariant.

3. Notwithstanding the single-peaked age function, individual differences in output accounts for far more variance in productivity and impact than do longitudinal changes (Levin & Stephan, 1989, 1992; Over, 1982a, 1982b). Hence, a highly prolific psychologist who is an octogenarian can boast more output toward the career's end than can a far less productive colleague who is 40 years younger. In fact, individual differences in output per age period are highly consistent across the career course (S. Cole, 1979; Dennis, 1954b; Helmreich, Spence, & Thorbecke, 1981; Simonton, 1997). Those who are highly prolific in their 20s and 30s will continue to be so in their 40s, 50s, 60s, and beyond. It is significant that the age curves for total output assume the same form for both high and low producers (Christensen & Jacomb, 1962; Horner, Rushton, & Vernon, 1986; Simonton, 1991a, 1992a, 1997; Zuckerman, 1977). In particular, the career peak is attained at the same age no matter what the average level of productivity. It is just that the peak is higher for the more prolific contributors. In other words, the age at maximum output is more or less invariant across scientists working within the same field, but the maximum output rate at that peak varies dramatically across those same scientists.
4. The previous point has important implications for the longitudinal location of the three career landmarks (Simonton, 1997). These are taken to be the age at the first high-impact publication, the age at the publication with the highest impact, and the age at the last high-impact publication (Raskin, 1936; Simonton, 1991a). On the one hand, because the age of maximum output is uncorrelated with total output, the middle career landmark will tend to appear at the same age no matter how prolific is the scientist (Simonton, 1991a, 1992a). On the other hand, individual differences in output will have major consequences for the placement of the first and last career landmark. According to the equal-odds rule, quality of output is directly proportional to quantity of output, a probabilistic relation that holds both across and within careers. Hence, greater the total output at the beginning of the career, the sooner will appear the first high-impact contribution. By the same token, the greater the output toward the end of the career, the later will appear the last high-impact contribution. These predictions have been confirmed in empirical research. For example, a study of 69 eminent American psychologists found that the total output correlated $-.25$ with the age at first cited work and .30 with the age at the last cited work, but almost exactly zero with the age at the most cited work (Simonton, 1992a).

5. More technically speaking, the expected trajectory of creative productivity across the life span is not a function of chronological age but rather career or professional age (Simonton, 1991a, 1997). Most frequently, career age is defined as the number of years that have transpired since earning the PhD (Bayer & Dutton, 1977; Lyons, 1968). This distinction is especially important in determining the location of the career peak (Simonton, 1997). Those who begin their careers later than the norm will have the acme shifted correspondingly later (Simonton, 1991a). This consequence was nicely illustrated by Zusne's (1976) study of 213 eminent figures in the history of psychology (drawn from Annin, Boring, & Watson, 1968). The age at best or most influential work was shown to be located at the harmonic mean of the ages of the first and last contribution (as determined from Watson, 1974). Specifically, if P is the chronological age at first work and L is the chronological age at last work, then the work with the highest impact will appear at $2(P^{-1} + L^{-1})^{-1}$ (Simonton, 2002). In fact, the correlation between the predicted location of the middle career landmark and the actual location was .52.

For the most part, these nomothetic findings are applicable to the career of Wilhelm Wundt. For instance, his impressive total lifetime productivity was attained by maintaining consistently high productivity throughout his career. Furthermore, the age period in which he generated the most productions—namely the late 30s and early 40s—was also the period in which his single most influential work was published, the *Physiologishen Psychologie*. Even the prediction of Zusne's (1976) harmonic mean model comes out quite accurate. According to Watson's (1974) bibliography, Wundt's first publication in psychology (as distinguished from his early work in physiology) appeared when he was 26, and the last came out when he was 88. Hence, given $P = 26$ and $L = 88$, his work with the highest impact should have emerged at age 40, only one year short of the true figure.

Qualitative Attributes

If Wundt can be considered the founder of experimental psychology, then Francis Galton can be viewed as the founder of correlational psychology. Although Galton's publications touch upon a diversity of topics (see Galton, 1883), three themes permeate virtually all of his research: (a) a fascination with individual differences, and especially variation in human abilities; (b) a preoccupation with the genetic inheritance of those individual

differences, including the potential for eugenic interventions; and (c) a commitment to the quantification and statistical analysis of that variation. Another remarkable feature of Galton's lifetime output is his willingness to advocate extreme positions. For instance, his 1869 book on *Hereditary Genius* argued for an unqualified genetic determinism, a forthright stance that almost immediately provoked debate (e.g., Candolle, 1873). Indeed, Galton can be considered the founder of the nature-nurture controversy in the behavioral sciences (Galton, 1874).

Significantly, empirical research indicates that these two qualitative features of Galton's research program—thematic consistency and conceptual audacity—may represent general attributes of high-impact psychologists.

Thematic Consistency

The importance of this qualitative attribute was first demonstrated on a sample of 69 eminent psychologists (Simonton, 1992a). Because this study was designed to commemorate the 100th anniversary of the founding of the American Psychological Association, it concentrated on a sample of American psychologists, including the founder and first APA president, G. Stanley Hall. The subjects were born between 1842 (George T. Ladd) and 1912 (Carl I. Hovland), and were publishing between 1879 and 1967. For each psychologist was compiled a bibliography of English-language titles using Watson's (1974) *Eminent Contributors to Psychology*. Once the titles were placed in machine-readable form, they could be subjected to computerized content analyses. Among the several content-analytical measures thus quantified was a standard index known as the *type-token ratio* (TTR). The TTR consists of the ratio of types (distinct words) to tokens (total words). A high ratio means that a text is riddled with lots of different words, whereas a low ratio means that a text has many repeated words. Normally this classic measure is used to assess linguistic complexity. In this application, however, a high ratio indicates that a psychologist's life work addresses a considerable range of research topics, whereas a low ratio implies that the psychologist has concentrated his or her research program on a restricted number of scientific questions.

After calculating TTR scores for all 69 bibliographies, the next step was to assess the differential impact of the psychologists whose research programs they represent. This was accomplished using *Social Sciences Citation Index Five-Year Cumulation 1981–1985* (1987). In particular, from this source were obtained log-transformed counts of the number of total citations, the number of total cited publications, and the number of citations to the most-cited work (the middle career landmark). The type-token ratio correlated negatively with all three measures of disciplinary impact ($rs = -.38, -.31,$

and −.39, respectively). Indeed, this content-analytical measure even correlated negatively with long-term eminence, as assessed by posthumous reputation ($r = -.30$, after partialling out year of birth). Hence, the greatest psychologists among the 69 are those who have the same title descriptors cropping up again and again throughout their publication lists. These scientists are not dilettantes who fritter around from topic to topic.

Hence, diversity of subject matter is not highly valued as a research strategy. Rather, these results appear to be most consistent with "a commonly accepted view of the productive scientist . . . [as] someone who continually chips away at a specific subject-matter area, becomes wedded to it, and is identified as a specialist in it by other scientists" (Garvey & Tomita, 1972, p. 379). Professional success may require a high degree of continuity in a psychologist's research program (Crane, 1965). Moreover, there is evidence that the benefits of a highly focused research program are not confined to these 69 deceased psychologists. Another investigation looked at 99 contemporary physicists, chemists, and biologists at top-rated research universities, almost a third of whom had been elected to the National Academy of Sciences (Feist, 1997). Again the type-token ratio was negatively related to the quantity of research, the total number of citations received, NAS membership, and an indicator of global eminence (consisting of peer ratings of creativity and historical significance, professional visibility, and the prestige of the highest honor received). Although the TTR did not have an independent effect on global eminence once productivity was statistically controlled, the same held for the 69 psychologists as well.

The latter outcome implies that the depth of a research program determines a scientist's reputation largely through its positive influence on his or her total productivity. As Walter Cannon (1945), the distinguished physiologist once noted,

> it seems probable that co-ordinate progress in research, process characterized by a natural development from one group of ideas to another, instead of a flitting from interest to interest in a quite inconsequential manner, is conducive to persistent effectiveness in productive scholarship. In this type of research, as studies advance and new facts are discovered, fruitful ideas accumulate and earlier ideas take on new meanings. As a result, fresh opportunities for exploration are frequently disclosed. (p. 218)

When one contemplates the careers of the truly great psychologists, the truth of Cannon's remark becomes most apparent. Besides such figures as Freud, Pavlov, Piaget, and Skinner, we must certainly include Francis Galton.

Conceptual Audacity

The impact of this second qualitative attribute was examined in the careers of 54 eminent psychologists (Simonton, 2000). These psychologists were those who were active from the 1880s to the 1950s and "who emerged among the top 50 in overall ratings or among the top 10 in the ratings for any decade" (Coan & Zagona, 1962, p. 716). The mean year of birth was around 1872, with a range from 1801 for Gustav Fechner to 1919 for William Estes. The 54 represented every major subdiscipline (physiological, comparative, cognitive, personality, developmental, educational, social, clinical, etc.) as well all the major schools (Structuralism, Functionalism, Behaviorism, Gestalt, Psychoanalytic, Humanistic, etc.). Besides the fact that all of these psychologists exerted considerable influence on the discipline, these 54 have the distinctive asset that they had already been evaluated according to their theoretical and methodological dispositions. Specifically, after surveying 232 experts Coan (1968, 1979) was able to assess these 54 psychologists along the following six dimensions: (a) *Objectivistic versus Subjectivistic*—emphasis on observable behavior versus emphasis on subjective experience (e.g., Watson, Pavlov, Skinner, and Hull versus Jung, Brentano, Adler, Piaget, Fechner, and Janet); (b) *Elementaristic versus Holistic*—emphasis on molecular or atomistic analysis versus emphasis on molar analysis (e.g., Spence, Titchener, Estes, Hull, Wundt, Pavlov, and Skinner versus Goldstein, Koffka, G. Allport, Lewin, and Rogers); (c) *Impersonal versus Personal*—emphasis on the nomothetic, deterministic, abstract, and tightly controlled versus emphasis on the idiographic, emotional, and the unconscious (e.g., Hull, Skinner, Titchener, and G. E. Müller versus Rorschach, Adler, Jung, Janet, G. Allport, and Charcot); (d) *Quantitative versus Qualitative*—emphasis on mathematics, statistics, and precision versus emphasis on qualitative attributes and processes (e.g., Estes, Thurstone, Spearman, Binet, and Ebbinghaus versus Freud, Charcot, Wertheimer, Sullivan, and Köhler); (e) *Static versus Dynamic*—emphasis on the normative and stable versus emphasis on motivation, emotion, and the self (e.g., Wundt, Mach, Fechner, Spearman, and Külpe versus McDougall, Mowrer, Freud, and James); and (f) *Exogenist versus Endogenist*—emphasis on environmental determinants and social influences versus emphasis on biological determinants and heredity (e.g., Skinner, Angell, Hull, Rogers, and Watson versus Galton, Freud, Hall, McDougall, and Cannon).

Because these 6 factors were themselves correlated (Coan, 1968, 1979), the measures were subjected to a factor analysis (Simonton, 2000). The result was a single general factor that contrasts elementaristic, objectivistic, quantitative, exogenist, impersonal, and static psychologists against their holistic, subjectivistic, qualitative, personal, endogenist, and dynamic colleagues. When the standardized scores on the 6 factors are summed to

produce a composite measure, the resulting internal-consistency reliability coefficient (alpha) was .85 (Simonton, 2000). The composite measure can be interpreted as assessing a bipolar dimension that contrasts a natural science orientation against a human science orientation.

Finally, the differential long-term impact of these 54 psychologists was then assessed according to the total number of citations received in the two most recent 5-year accumulations of the *Social Sciences Citation Index* (1983, 1992). The "test-retest" reliability was so substantial ($r = .84$) that the two measures could be summed to produce a composite index of disciplinary influence. Because the resulting measure was also skewed left with an unusually extended upper tail, the citation indicator underwent a logarithmic transformation that made it more closely approximate the normal distribution.

This citation measure has a negative correlation with the psychologist's theoretical and methodological orientation as represented by the general factor (ß = −0.23, controlling for birth year). That is, those great psychologists who adopt a human-science orientation are more likely to boast long-term impact. This outcome essentially replicates Roeckelein's (1972) demonstration that tender-minded psychologists (like Freud and Piaget) tended to be more eminent—as judged by eponymic status in introductory psychology textbooks—relative to tough-minded psychologists (like Skinner, Hull, and Tolman). Nevertheless, residual analysis revealed substantial departures from linearity, indicating that the relation has a curvilinear component. When a quadratic function was added to the linear function, the citation measure was found to be a curvilinear, backward-J function of a psychologist's position on the natural- versus human-science dimension (ß = 0.26 for the quadratic term and ß = −0.22 for the linear term, again controlling for birth year). Together these terms account for 11% of the total variance in long-term impact, with the curvilinear function accounting for slightly more of the curve than the linear function (6% versus 5%).

Three features of this curve deserve emphasis. First, the highest total citations tend to be received by those eminent psychologists who score lowest on the general factor. These are psychologists inclined toward the subjectivistic, qualitative, holistic, personal, dynamic, and endogenist side of psychology. Eminent figures in this group include Sigmund Freud, Jung, Adler, James, Gordon Allport, and Carl Rogers. Second, the next highest total citations tend to be received by those eminent psychologists who score highest on this same factor. These are those who lean toward the objectivistic, quantitative, elementaristic, impersonal, static, and exogenist. Skinner, Harlow, Thurstone, and Estes are among the illustrious psychologists in this category. Third, psychologists situated at the bottom of the backward-J

curve are those who have taken more moderate positions, their long-term influence evidently declining as a consequence. Actually, the low point is off-center, shifted toward those who score half of a standard deviation above average on the general factor. The distinguished psychologists in this group are J. R. Angell, G. E. Müller, and J. M. Cattell.

Significantly, this curvilinear relation holds not just for the general factor, but also for many of the separate components that make up that factor. This curvilinear function specifically holds for the Exogenist versus Endogenist. Because this contrasts Nurture against Nature as determinants of human behavior, it represents a debate on which Galton took an especially strong stand. In precise terms, Galton scored 2.5 standard deviations below the mean (viz. toward the biological determinants and heredity). On the other hand, because his views on the other five dimensions were more moderate—none exceeding one standard deviation in either direction—Galton ended up scoring only about a third of a standard deviation below the mean on the general factor. As a result, he is only predicted to fall in the middle of the pack with respect to long-term citations. That is almost exactly the case. The Studentized residual error of prediction is less than a standard deviation (viz. 0.73), showing that his career falls right into place with respect to the nomothetic relationship.

I should point that the above curvilinear function is corroborated by an earlier investigation of the differential eminence of the 2,012 thinkers who define the Western intellectual tradition from the ancient Greeks to the 20th century (Simonton, 1976). Those philosophers who attained the greatest long-term distinction were those who tended to take strong positions on such issues as empiricism versus rationalism, materialism versus idealism, determinism versus free will, and individualism versus collectivism. Indeed, a comparable pattern was observed in another study of 15,618 compositions that make up the classical repertoire in Western music (Simonton, 1980). The works with the most long-term impact (in terms of performance and recording frequencies) tend to be those that depart significantly from the stylistic norms. Hence, long-term influence is more secure when one somehow manages to "defy the crowd" (Sternberg, 2002).

CONCLUSION

I have shown that no contradiction exists between the focus on an individual's impact on psychology's history and a naturalistic or scientific analysis. Using the empirical, quantitative, and nomothetic findings in the psychology of science, it is possible to describe the quantitative and qualitative aspects of those research programs that are most likely to

have short- and long-term impact on the field. The quantitative attributes included both individual differences in total lifetime output and the longitudinal form of the career trajectory, whereas the qualitative characteristics involved both thematic consistency and conceptual audacity. Moreover, these research results were used to shed light on the influence of two high-impact psychologists: Wilhelm Wundt and Francis Galton. For the most part, each was shown to fit a general profile of expectations implied by the empirical literature. In a sense, the research literature provides the basis for scientific Hempelian "covering laws" for the explaining why certain psychologists have played a particularly prominent part in the discipline's annals (Hempel, 1965; see Simonton, 1995). Better yet, these nomothetic results can sometimes generate fairly precise predictions, as was demonstrated when Zusne's (1976) harmonic mean model was used to predict the age at which Wundt would be expected to write his most influential work—a prediction that was only one year off.

Admittedly, these explanatory principles are statistical rather than deterministic; there will exist numerous "exceptions to the rule." None of the effect sizes for any of the reported findings are so big that 100% of the phenomenon can be successfully explicated. Nevertheless, a sizable proportion of the unexplained variance or errors of prediction can be attributed to the participation of other nomothetic factors. Research has already teased out many more useful explanatory principles besides those discussed in this chapter (see Simonton, 2002, for an inventory). At the same time, investigators have already unearthed some of the antecedent variables that impinge upon the factors already mentioned. A case in point is the research on the factors that underlie the long and productive career that is so crucial to long-term influence (Simonton, 2002). To retain creative vitality throughout the life span, older scientists must avoid the tendency to become less receptive to new ideas, a developmental trend known as "Planck's Principle" (Diamond, 1980; Hull, Tessner, & Diamond, 1978; Sulloway, 1996; Whaples, 1991). The requisite receptiveness can be preserved by continuing to read widely and by maintaining active professional activities, including extensive collegial contacts (Dennis & Girden, 1954; Simonton, 1992b). Often this program of continued intellectual resuscitation requires that the investigator avoid the temptation of assuming the increased administrative responsibility that are usually associated with senior status (Horner, Murray, & Rushton, 1994; Roe, 1965; Zuckerman & Merton, 1972).

Future investigations will no doubt unearth additional variables that enhance or hinder a psychologist's short- and long-term impact on the discipline. These additions to the explanatory arsenal will reduce the amount of the phenomenon that remains inexplicable. Furthermore, even modest effect sizes can have major consequences (Rosenthal, 1990; Rosenthal

& Rubin, 1979). To illustrate, a correlation of .40 between two dichotomous variables (with 50–50 splits) means that "only" 16% of the variance is shared. In more concrete terms, fully 30% of the cases will contradict the nomothetic association (i.e., fall in the wrong quadrant of the 2 × 2 table). Yet not only do 70% of the cases still comply with statistical expectation, but also the practical consequences can be quite substantial. If this were a drug treatment for a fatal disease, a correlation of .40 still implies that the chances of survival would be more than doubled by taking the medication. This enhancement is far from negligible. Just as significant is the fact that small effects, with accumulated across time or aggregated across individuals, can end up having potent repercussions (Abelson, 1985).

Nonetheless, it must be said that the naturalistic individualism illustrated in this chapter is not inconsistent with the belief that contextual factors, such as the zeitgeist, have a major part to play. On the contrary, one of the genuine assets of a scientific analysis of psychology's history is that individual and situational factors can be combined into a single causal model (Simonton, 2002). By doing so, it becomes possible to assess the relative contributions of individual and situational factors to the emergence of high-impact psychologists. Studies adopting this integrative approach have in fact shown that both personal and contextual variables have major roles to play (e.g., Simonton, 1976, 1992a). For instance, in one study I assessed 69 eminent American on a variable called *Ortgeist fit* (Simonton, 1992a). That is, I conducted a computerized content analysis of publication titles that assessed the extent to which a psychologist's body of work dealt with what could be considered the most popular topics, methods, and concepts in American psychology. Among the 69 psychologists, Edward L. Thorndike, Edward B. Titchener, Edward C. Tolman, Kenneth W. Spence, and Clark L. Hull exhibited the best fit to the American Ortgeist, whereas Douglas McGregor, Elton Mayo, Clarence E. Ferree, William Henry Burnham, and Edward K. Strong displayed the worst fit. As these names would suggest, Ortgeist fit was positively correlated with a psychologist's impact, whether contemporary or posthumous. Nevertheless, individual differences in creative productivity still had a substantial impact on these criterion variables even after controlling for Ortgeist fit. Indeed, the explanatory power of the individual variable exceeded that of the situational variable.

The omission of either set of factors would therefore represent a measurable loss in our ability to understand and explain the phenomenon. A complete naturalistic analysis of high-impact research must necessarily combine the genius with the zeitgeist. Disciplinary impact is demonstrably a matter of being the right person at the right place and at the right time.

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5

TRACKING TRENDS IN PSYCHOLOGICAL SCIENCE

AN EMPIRICAL ANALYSIS OF THE HISTORY OF PSYCHOLOGY

Jessica L. Tracy, Richard W. Robins, Samuel D. Gosling

INTRODUCTION

Debates about scientific trends pervade all fields of scientific research. Scientists often monitor and discuss the amount of attention given to the various schools of thought within their field, and they typically hold strong views about which schools are currently "hot." Competition among opposing schools can produce a continuing tug-of-war between prevailing and competing ideas, providing fertile ground for speculation. Although such debates may stimulate lively conversation, they are rarely informative and often misguided. Unfortunately, scientists often make consequential decisions—about allocating funding, hiring faculty, and so on—based on their personal views. Such decisions profoundly shape the direction taken by a science, and thus should be guided by empirical research rather than mere speculation.

Since the birth of scientific psychology more than a century ago, many schools have risen and fallen from prominence. Much has changed since Heidbreder (1933) described the early days of the field in terms of "seven

psychologies." Over the past several decades, four major schools have competed to become the predominant model for understanding human behavior: psychoanalytic, behaviorist, cognitive, and neuroscientific.

Lively polemics surround the debate concerning which of these four schools are becoming more influential and which are waning in prominence. If you ask psychologists which is the prevailing school of thought, you are likely to get different answers depending on the affiliation of the person you ask. The psychoanalytic perspective, first, has come under fire, with *Time* and other popular magazines asking "Is Freud dead?" Within scientific journals, however, debate continues over the prominence and relevance of Freudian ideas to contemporary psychological research. For example, Crews (1996) wrote, "[T]here is literally nothing to be said, scientifically or therapeutically, to the advantage of the entire Freudian system or any of its component dogmas" (p. 63). In response to such arguments, Westen (1998) argued, "although commentators periodically declare that Freud is dead, his repeated burials lie on shaky grounds" (p. 333); "Freud has left an important—and I believe indelible—mark on human self-understanding" (p. 362).

Another popular contention is that the cognitive perspective now dominates psychological science, having prevailed over psychoanalysis and the subsequent Skinnerian behaviorist tradition (e.g., Baars, 1986; Gardner, 1985; Hunt, 1993; Sperry, 1988); Nobel laureate Roger Sperry has claimed that this "cognitive revolution" is "widely recognized and well-documented . . . and appears to constitute a true shift of paradigm" (Sperry, 1988, p. 608). In contrast, others have argued that no such cognitive revolution has occurred: "the repeated declaration of a revolution may be more a reflection of the enthusiasm many cognitive psychologists have for their subdiscipline than of actual events" (Friman, Allen, Kerwin, & Larzelere, 1993, p. 662). Similarly, Leahey (1991) suggested that "Cognitive scientists believe in a revolution because it provides them with an origin myth, an account of their beginnings that helps legitimize their practice of science" but, he bluntly states, "there was no revolution" (p. 362).

Others have come to the defense of the behaviorist school: "although I have conducted no study, prepared no in-depth interview, nor even any shallow survey of opinions, I do contend that behavioral analysis is alive and kicking and that I for one knew it all along" (Salzinger, 1994, p. 816). Pierce (1996) agrees, "Behaviorism is not dead . . . the field of behavior analysis continues to thrive and progress, even in the absence of its mentor B. F. Skinner" (p. 461). Similarly, Friman et al. (1993) have concluded emphatically, "reports on the death of behavioral psychology . . . appear greatly exaggerated" (p. 664).

Finally, and most recently, some have argued that the neuroscientific perspective is flourishing (e.g., Churchland, 1998)—indeed, the 1990s were designated the "Decade of the Brain" by congressional resolution. There are even concerns (and for some, hopes) that the field of psychology will ultimately be reduced to a subfield of neuroscience (Bechtel, 1988; Miller & Keller, 2000).

In line with our belief that conclusions about trends in the field should be guided by empirical data, several years ago we conducted a series of analyses to chart changes in the prominence of various schools from 1950 to 1997 (Robins, Gosling, & Craik, 1999). In this chapter, we review those findings and augment our earlier analyses by charting these trends through 2002. Based on assertions made in scientific journals, we expected to find evidence for: the decline of the psychoanalytic and behavioral schools, the transformation of the field by a cognitive revolution, and a recent rise in the prominence of neuroscience.

CONCEPTUALIZING PROMINENCE AND SCIENTIFIC PROGRESS

Before discussing the findings, we first consider several ways of conceptualizing prominence. In our earlier paper (Robins et al., 1999), we defined prominence as the degree to which the mainstream of the field pays attention to a school's scientific products (e.g., research findings and theories). Thus, our intent was not to trace the history of ideas in modern psychology (e.g., Kendler, 1987), but rather to document specific empirical trends in the amount of attention devoted to each school by the field as a whole. From a Kuhnian (1962, 1970) perspective, the school receiving the most attention could be seen as having attained the status of a dominant paradigm. Kuhn portrayed the development of a science as moving from a *preparadigmatic* stage, characterized by multiple, competing schools, to a *paradigmatic* (or *normal science*) stage, characterized by a single dominant paradigm of shared assumptions and methodologies. Once a field has attained paradigmatic status, further scientific development involves a succession of revolutions in which the dominant paradigm within the field is displaced by a new one. However, a number of scholars have debated whether psychology has even reached the paradigmatic stage of science (e.g., Briskman, 1972; Leahey, 1991; Schultz, 1981; Warren, 1971; Watson, 1977). For example, Masterman (1970) argued that psychology and other social sciences may be characterized by a *multiparadigmatic* stage, which precedes the eventual emergence of a single dominant paradigm.

In contrast to Kuhn and Masterman, Lakatos' (1970) theory of scientific development allows for multiple, competing schools at any stage of a science's historical development. These schools (research programmes in Lakatos' terminology) may coexist indefinitely, and there is no assumption that one school will eventually emerge as the dominant paradigm. Lakatos views scientific progress as akin to a horse race in which competing schools progress, degenerate, get revived, and so on, depending on the ability of the school to generate new hypotheses that lead to empirical discoveries. Lakatos (1970) argued that a school becomes "progressive" when it yields new predictions that lead to empirical successes; Lakatos believed that "empirical evidence is the final arbiter among competing research programs" (Gholson & Barker, 1985, p. 757). Thus, from Lakatos' perspective, a school's prominence could be conceptualized as a reflection of its scientific merit; that is, prominence is attained when the empirical evidence supports the validity and viability of the school's basic assumptions and methods.

In sharp contrast, Latour's (1987) social constructionist view of scientific development suggests that a school's prominence is determined by sociological, not scientific, factors. Specifically, schools rise in prominence when they successfully disseminate their scientific products to the rest of the field through communication networks that determine what becomes attended to and widely known. Conversely, schools decline in prominence when they are no longer able to effectively communicate their findings through journals, conferences, and other outlets for scientific information.

A final perspective is that scientific prominence can be seen as simply mirroring fad and fashion in the field (Christensen-Szalanski & Beach, 1984; Teo & Febbraro, 2002). Teo and Febbraro (2002) argue, "Psychology's history can be studied as a history of fads" (p. 458). For example, according to Christensen-Szalanski and Beach (1984), researchers in the 1970s and 1980s focused on biases in judgment and decision-making because they were jumping on a bandwagon to question the rationality of human judgment and identify flaws in the way people reason (cf. Robins & Craik, 1993). From this perspective, a school's prominence is determined by what's currently in vogue in the broader scientific and intellectual community, not by the ability of a school to document scientific truths or even its ability to effectively publicize its scientific products.

Measuring Prominence

In Robins et al. (1999), we measured trends in prominence in four of the most influential and widely recognized schools within mainstream

scientific psychology: psychoanalysis, behaviorism, cognitive psychology, and neuroscience. Prominence is a difficult construct to measure, so we searched for convergence across three indices. These three indices assess prominence specifically within psychological science, and not within the sciences and humanities as a whole.

Assessing the content of flagship journals in psychology

Our first index was based on an analysis of the subject matter of articles published in four 'flagship' psychology publications: the *American Psychologist*, *Annual Review of Psychology*, *Psychological Bulletin*, and *Psychological Review.* These publications were selected because they aim to publish articles representing the entire field of psychology and because they were the most frequently cited of all psychology journals [*Social Sciences Citation Index (SSCI): Journal Citation Reports (JCR)*, 2001]. The four flagship publications cut across subdisciplines and are read by a broad range of psychologists. The flagship publications serve dual roles in the field: They reflect current trends and they define an agenda for the future. Thus, a school's prominence in the flagship publications can serve as an indicator of its prominence in mainstream scientific psychology. For example, if neuroscience has been increasing in scientific prominence, we would expect to find an increase in articles on neuroscience topics appearing in the flagship publications.

We used the *PsycINFO* database to measure the proportion of articles in the flagship publications relevant to each school. *PsycINFO* permits keyword searches that retrieve all articles containing a specified word stem in one of four locations: the article title, abstract, subject index, and keyword phrases listed by the authors. Keyword stems can be specified with the "#" sign as "wildcard" terms. These stems allow one to search simultaneously for a broad range of terms related to a single topic. Thus, for example, the keyword "psychoanal#" will capture not only the keyword "psychoanalysis", but also the keywords "psychoanalytic", "psychoanalyze", "psychoanalysts", etc. We used the following keywords to represent each school: 'psychoanal#' was used to represent psychoanalysis; 'cognit#' was selected to represent cognitive psychology; 'neuropsy#' and 'neurosci#' were selected to represent neuroscience; and 'reinforc#' and 'conditioning' were selected to represent the behaviorist school (the stems "behav#" or "behavior#" are too generic to be representative of the behaviorist school because assessment of behaviors is an important research tool in all four schools). It is worth noting that these keywords may not be equivalent in terms of representing their respective schools. For example, the keyword 'cognit#' may capture more articles from the cognitive school than the keyword 'psychoanal#' captures from the psychoanalytic school. Therefore,

mean differences among the schools may reflect, in part, differences in the keywords chosen to represent each school, and should be interpreted cautiously. Thus, our keyword analyses focus primarily on *trends* over time.

For each school, we calculated the percentage of articles published in the four flagship publications that included one or more of the keywords selected to represent that school. This index was computed annually from 1950 (the earliest date when all four flagship publications existed) through 2002. Trends in scientific prominence were obtained by plotting these values over time and then smoothing the data.

Assessing the content of psychology dissertations

Our second index of scientific prominence was based on an analysis of the subject matter of dissertations. Specifically, we examined the subject matter of doctoral dissertations listed in the *PsycINFO* database from 1967 to 2002. We began this search in 1967 because dissertations in the *PsycINFO* historical database prior to 1967 used a different indexing policy, which did not include subject terms, making searches before and after 1967 incomparable. We searched for the subject matter of dissertations by searching for subject and title words rather than keywords, because the indexing policy changed again in 1995 such that keyword searches before and after that date became incomparable. We computed the index by calculating the percentage of dissertations that included at least one of the subject or title words previously selected to represent each school. Compared to the flagship publications, dissertations may provide a better gauge of cutting-edge research that is attracting the attention of young scientists in the field. Thus, an analysis of the subject matter of dissertations may detect emerging trends earlier than the flagship publications, which may reflect more established scientific perspectives.

Assessing the citation rates for subdisciplinary journals by the flagship publications

Our third index of prominence focused more directly on the scientific products of each of the four schools. Specifically, we examined how frequently articles published in subdisciplinary journals associated with each school were cited by the four flagship publications. By determining how many times an article is cited and who cites it, citation analyses provide a valuable empirical gauge of the level of attention an article has received (Garfield, 1979; Robins & Craik, 1993). Because many journals are associated with particular schools (e.g., cognitive psychologists tend to publish in cognitive journals and not psychoanalytic journals), we can examine the scientific prominence of a school by determining how frequently journals

within that school are cited in the flagship publications. If a school has been increasing in prominence, we would expect to find an increase in citations to articles published in its subdisciplinary journals. Thus, we measured scientific prominence by the number of times the flagship publications cited articles published in the top journals in psychoanalysis, behaviorism, cognitive psychology, and neuroscience.

How can one identify the top journals in a school? Friman et al. (1993) developed a set of criteria for selecting the top journals based on citation impact rankings (the number of times a journal has been cited divided by the number of articles it has published over a two year period) and evaluations of influence and importance by experts (journal editors). Using these criteria, Friman et al. (1993) identified the top four journals for the behaviorist, cognitive, and psychoanalytic schools. We used a similar procedure to identify the top four neuroscientific journals in psychology. However, because the field of neuroscience consists of a broad range of disciplines outside psychology (e.g., neurochemistry), many top neuroscientific journals (e.g., *Neuron*) publish little of relevance to psychologists and high citation rates do not necessarily reflect prominence in psychology. Therefore, we used ratings of influence and importance by neuroscientists working within psychology departments (Robins et al., 1999).

The top four journals from each school are shown in Table 1 on the next page. The scientific prominence of each journal was gauged by the degree to which the articles it published were cited in the flagship publications. Specifically, we computed the total number of times per year the flagship publications cited articles published in each subdisciplinary journal. We then summed these citation rates across the four journals associated with each school to obtain an index of the prominence of the psychoanalytic, behavioral, cognitive, and neuroscientific schools. This index was computed annually from 1977 through 2001 (the years *SSCI: JCR* was available).

In summary, we used three different approaches to measure prominence: We looked at the subject matter of articles published in the most influential ("flagship") psychology publications, the subject matter of dissertations, and the degree to which the flagship publications cited articles from each school's core journals. Together, these three indices of scientific prominence allowed us to determine which of the four schools are currently prominent and what specific trends can be identified over the past several decades. By including three measures and searching for convergence across measures, we hope to ensure the generalizability of the findings. To the extent that all three indices point to the same trends, we can be confident that the findings do not depend on biases associated with any single method.

TABLE 1. Leading Journals From Each School

Journal (2001 citation impact)	Year first published
Psychoanalytic school	
International Journal of Psychoanalysis (1.0)	1920
Psychoanalytic Quarterly (0.8)	1932
Journal of the American Psychoanalytic Association (0.8)	1953
Contemporary Psychoanalysis (0.6)	1964
Behavioral school	
Journal of Experimental Analysis of Behavior (1.4)	1958
Behaviour Research and Therapy (2.0)	1963
Journal of Applied Behavior Analysis (0.8)	1968
Behavior Therapy (1.4)	1970
Cognitive school	
Cognitive Psychology (3.7)	1970
Cognition (2.7)	1972
Memory and Cognition (1.6)	1973
Journal of Experimental Psychology: Learning, Memory, & Cognition (2.0)	1975
Neuroscientific school	
Journal of Neurophysiology (3.5)	1938
Annual Review of Neuroscience (27.2)	1978
Trends in Neurosciences (16.5)	1978
Journal of Neuroscience (8.2)	1981

NOTE: The citation impact of a journal is a measure of the frequency with which the average article in that journal has been cited in a particular year. Citation impact is computed by dividing the number of all current citations to a journal over the previous two years by the total number of articles published in that journal over those two years.

Findings From an Empirical Study of Trends in Psychology

Our three indices of prominence paint strikingly similar portraits of the recent history of psychology (see Figures 1–3), providing converging evidence for the following conclusions.

The death of the psychoanalytic perspective

According to all three indices, the psychoanalytic school has not fared well over the past few decades. Scientific psychologists have paid little attention to research published in the preeminent psychoanalytic journals and a psychoanalytic focus has been virtually nonexistent in either flagship publications or dissertations. Thus, contemporary psychoanalytic research is not being assimilated directly into mainstream scientific psychology. This does not mean that "Freud is dead," but rather that his presence may be felt indirectly. Indeed, many of Freud's basic ideas—for example, that unconscious processes influence behavior and that early childhood experiences

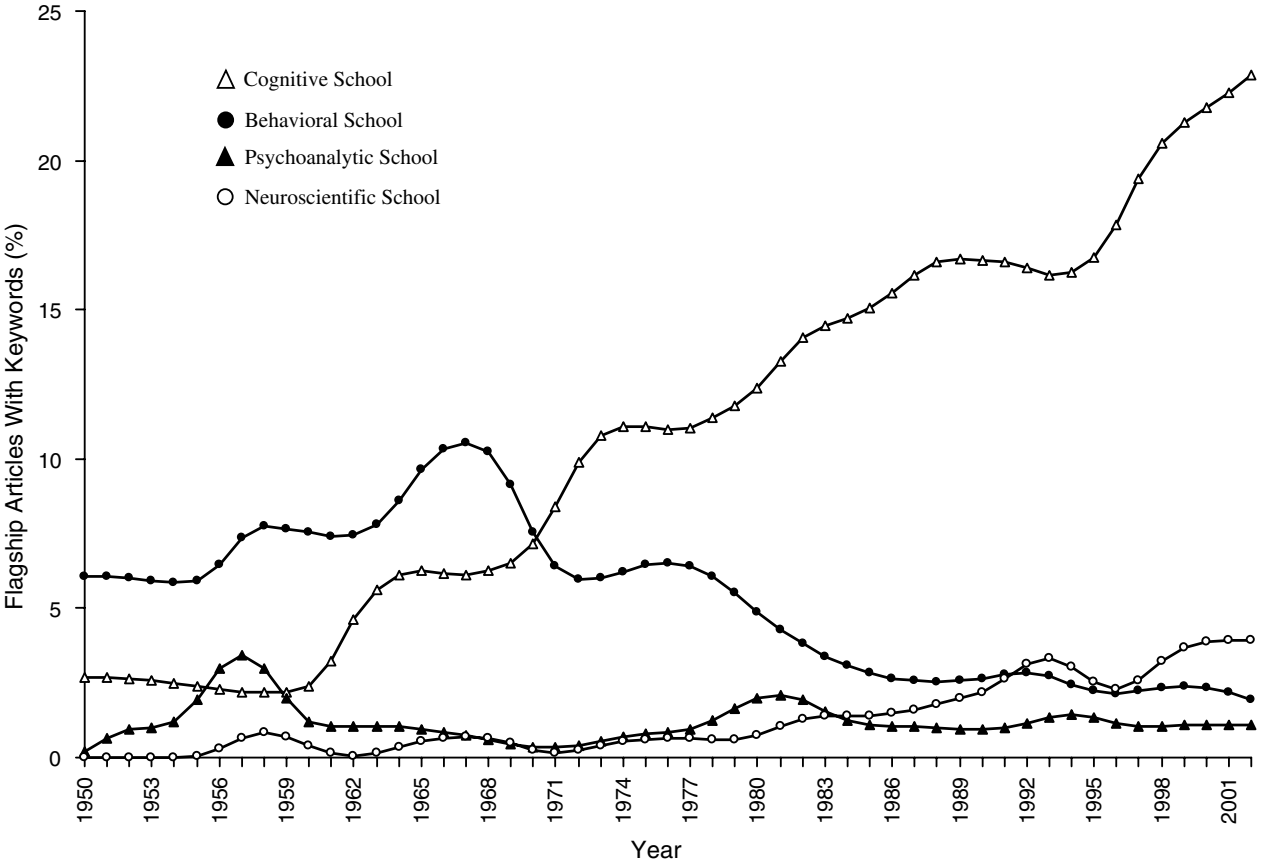

Figure 1. Percentage of articles published in the flagship publications that include keywords relevant to the cognitive, behavioral, psychoanalytic, and neuroscientific schools. A smoothing function was used to transform the raw data.

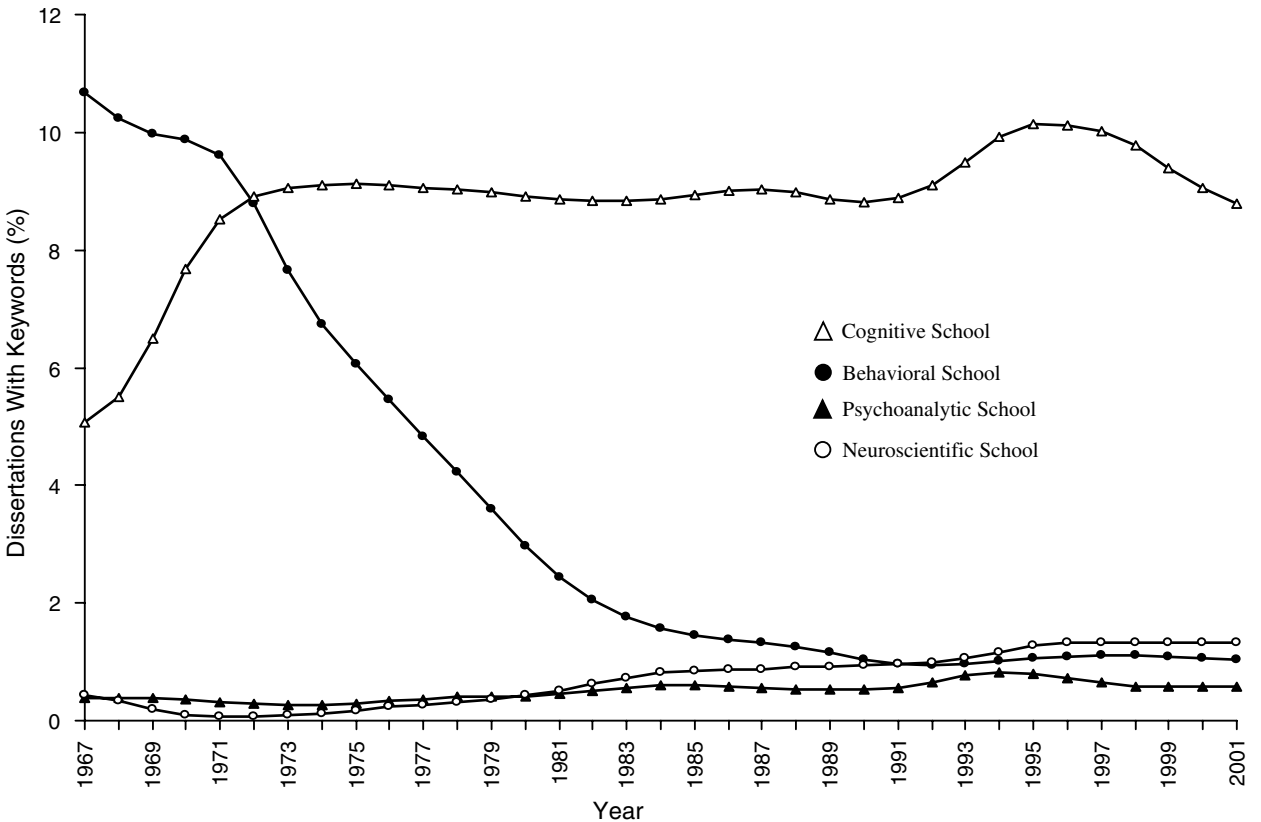

Figure 2. Percentage of dissertations that include keywords relevant to the cognitive, behavioral, psychoanalytic, and neuroscientific schools. A smoothing function was used to transform the raw data.

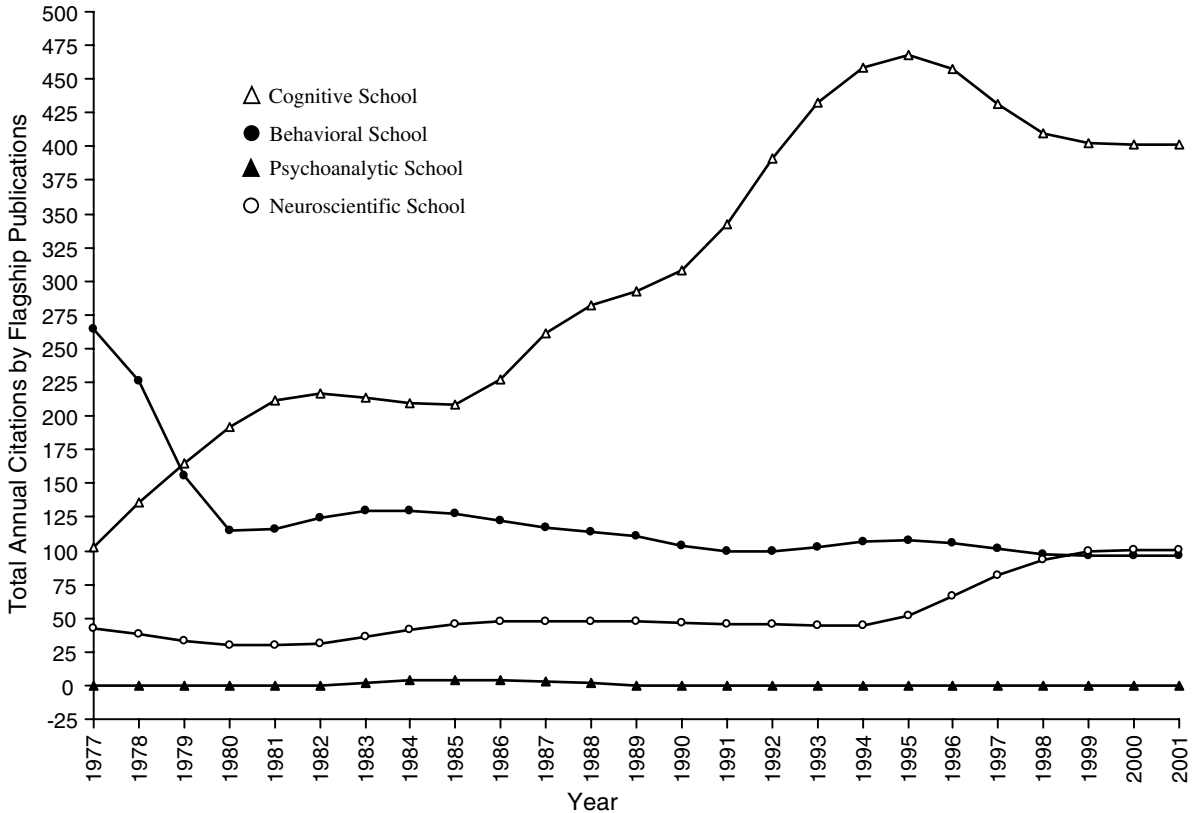

Figure 3. Total number of citations per year by the four flagship publications to articles published in the cognitive, behavioral, psychoanalytic, and neuroscientific journals. A smoothing function was used to transform the raw data.

influence adult development—have become incorporated into the foundation of psychology as a science (Kihlstrom, 1994; Westen, 1998). More generally, psychoanalytic thought continues to be influential in the broader intellectual community, shaping scholarship in the humanities (dissertations such as "Kafka's Hunger artist and the psychoanalytic approach to literature" are not uncommon) and other social sciences (Friman et al., 1993).

Behavioral psychology: Reports of its demise are NOT exaggerated

Despite claims to the contrary, behavioral psychology, and its associated concepts of conditioning and reinforcement, has been on the decline in psychological science. The one apparent exception to this decline was a flurry of attention to the behavioral school in 1992. Closer inspection revealed that this peak was due to a set of *American Psychologist* articles commemorating B.F. Skinner upon his death, which can hardly be taken as an indication of the health of the school. Thus, when graphing the keyword trends, we replaced this extreme outlier (10 standard deviations above the mean) with the mean value for the other 11 months of the 1992 volume of the *American Psychologist*. Overall, our findings furnish hard evidence to substantiate what has become academic lore: Sometime during the 1970s, the prominence of the behavioral school gave way to the ascension of the cognitive school. Furthermore, in the past several years, the neuroscience school has also overtaken behaviorism, as can be seen from all three of our analyses. This picture of contemporary psychology contrasts sharply with the view that behavioral psychology has maintained a high profile throughout the 1980s (e.g., Friman et al., 1993). However, as was the case for psychoanalysis, behavioral concepts and methods continue to be used by psychologists to describe and study human behavior.

Empirical evidence for a cognitive revolution

Our analyses showed that the cognitive school has overtaken the behavioral school as the most prominent of the four schools, supporting the claim that there has been a "cognitive revolution." Interestingly, according to the keyword analysis of the flagship publications, the cognitive school first began to rise in prominence around 1960, soon after the 1956 symposium on information theory which some view as the birth of the cognitive revolution (Gardner, 1985). When did cognitive psychology surpass the behavioral school in prominence? All three indices place the ascension of cognitive psychology over behavioral psychology in the 1970s, although they differ in the precise date. What might account for the cognitive school's rise to prominence? Although there are certainly many factors, the central driving force is probably the computer revolution. Computers

provided scientists with a new metaphor for conceptualizing how the mind works, one based on information processing and associated concepts of storage, retrieval, computational operations, and so on. Perhaps equally important, computers paved the way for the development of new methods for the scientific measurement of mental processes (e.g., highly controlled presentation of stimuli, reaction times, dichotic listening, simulations of cognitive processes).

However, the cognitive school's dramatic rise in prominence shows some sign of abating—the dissertation subject matter analysis and the flagship citation analysis both show its trajectory leveling off in the 1990s. The flagship subject matter analysis seems to indicate otherwise, but the findings from the dissertations analysis may foreshadow a similar outcome in the flagships over the next several years. It remains to be seen whether this plateau represents a temporary pause in growth or a zenith in the cognitive school's rise to prominence.

The emergence of a neuroscience revolution?

In our original study, we failed to find evidence that mainstream psychology was paying increasing attention to neuroscientific research. However, our follow-up analyses demonstrate that over the past five years neuroscience has become increasingly prominent in psychological science, although the magnitude of the increase is relatively modest. All three analyses showed that neuroscience had, by the mid to late 1990s, reached higher levels of prominence than behaviorism or psychoanalysis.

In addition, it is important to note that neuroscience has, for some time, been achieving high levels of prominence *outside* the field of psychology. The four neuroscientific journals examined in the present study are among the most frequently cited of all scientific journals when citations by both psychology and non-psychology sources are considered (see the citation impact values in Table 1). In fact, the *Annual Review of Neuroscience* and *Trends in Neurosciences* have citation rates comparable to the journal *Science*. Moreover, when we compared citations by *Science* to the four core neuroscientific journals we found further evidence for the prominence of neuroscience. As shown in Figure 4, *Science* articles have dramatically increased their citations to the four neuroscientific journals, with the upsurge beginning in the late 1980s (the recent plateau may be due to the dramatic proliferation of neuroscientific journals, which reduces the number of citations to any single journal). In contrast, *Science* articles rarely, if ever, cite journals from the cognitive, behavioral, and psychoanalytic schools. Also supporting the idea that neuroscience is growing, but not necessarily in psychology, is the fact that membership in the Society for Neuroscience has skyrocketed since it was founded in 1970 (see Figure 5).

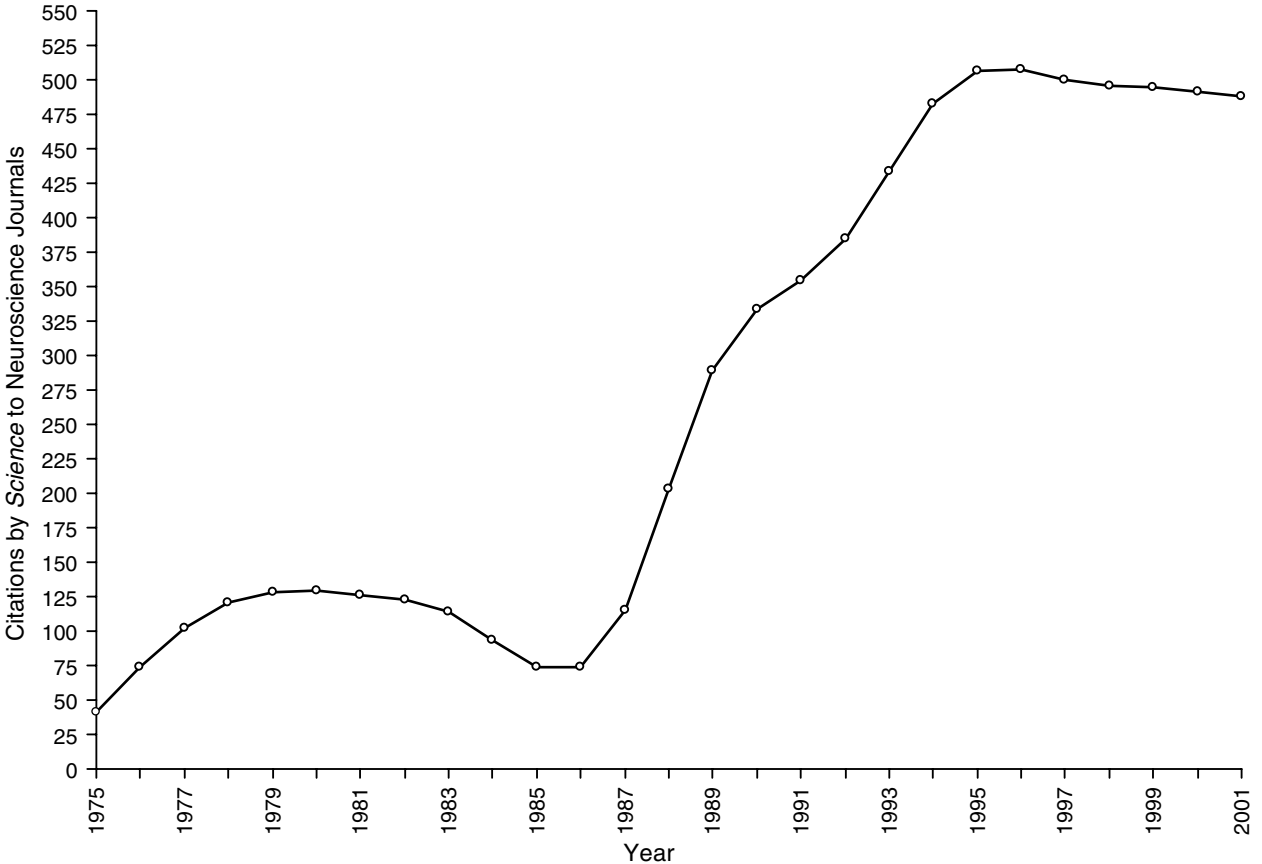

Figure 4. Annual membership in the Society for Neuroscience. A smoothing function was used to transform the raw data.

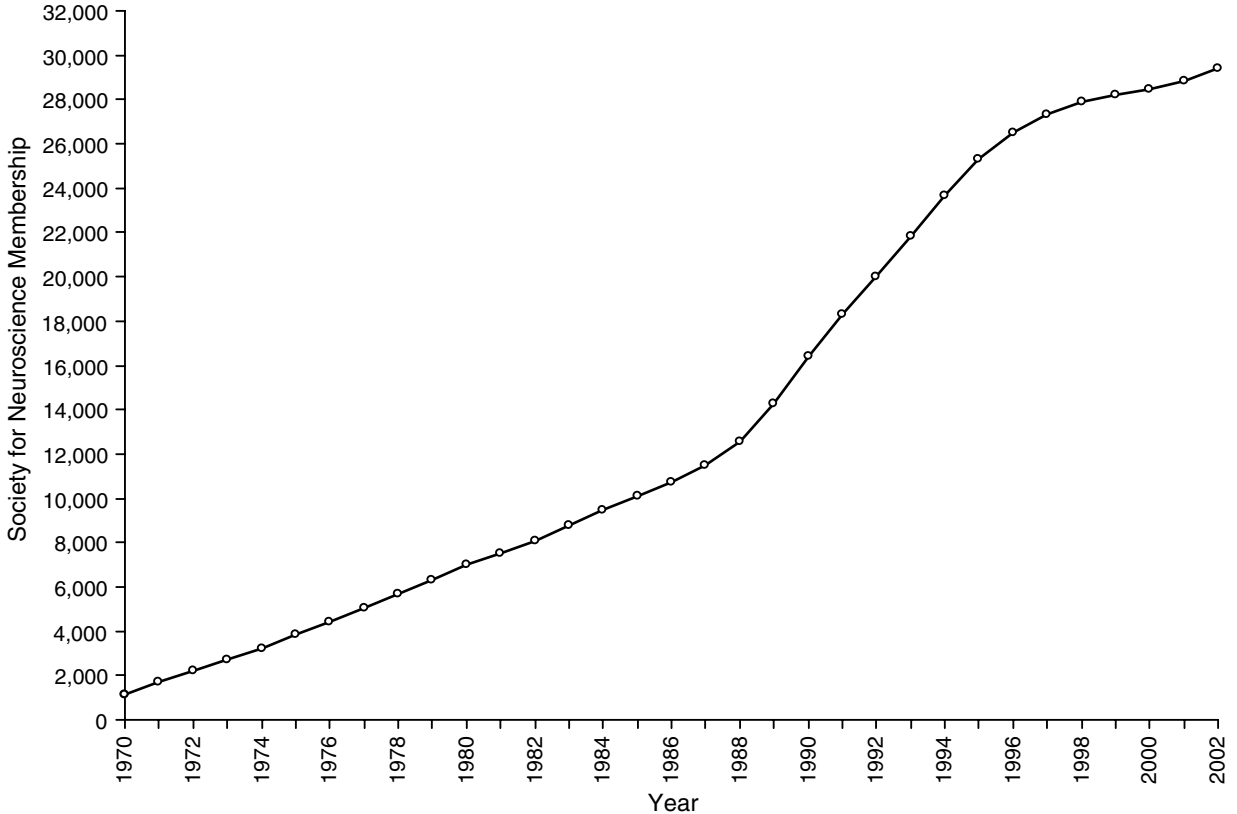

Figure 5. Total number of citations per year by the journal *Science* to the leading neuroscientific journals. A smoothing function was used to transform the raw data.

Thus, much of the growth in neuroscience seems to be taking place outside of psychology. Neuroscience is typically considered both a behavioral and a biological science, but at this point, neuroscience seems to be located more centrally in biology than in psychology. In some sense, neuroscience may even constitute its own independent scientific discipline.

DISCUSSION OF FINDINGS

Together, these four trends tell a story about the recent history of psychological science. Overall, the findings reflect the decline and virtual death of psychoanalysis within scientific psychology; the gradual decline of behavioral psychology; the sharp ascent and, perhaps, leveling-off, of cognitive psychology; and the beginning of neuroscientific psychology's rise. These findings are further supported by the founding dates of the four journals from each school (see Table 1). The psychoanalytic journals had the earliest founding dates (median = 1943), followed by the behavioral journals (median = 1965), the cognitive journals (median = 1973) and, most recently, the neuroscientific journals (median = 1978). Furthermore, although there are few if any new journals which are devoted to the study of psychoanalysis or behaviorism, there has been an explosion of new journals in cognitive psychology, cognitive-neuroscience, and neuroscience.

In fact, the emergence of the inter-disciplinary area of cognitive neuroscience suggests another indicator of neuroscience's rise to prominence. Cognitive neuroscience has become an important part of psychology, as indicated by the publication of the *Handbook of Cognitive Neuroscience* in 1984, the creation of the *Journal of Cognitive Neuroscience* in 1989, and the establishment of the *Cognitive Neuroscience Society* in 1994. Several other areas of psychology also have recently expanded to incorporate a neuroscientific perspective. There has been a proliferation of conferences, books, journals, and societies centered around "affective neuroscience," "social neuroscience," "behavioral neuroscience," "psychoneuroimmunology," and "developmental neuroscience." This trend of interdisciplinary overlap may be repeating history: During the 1970s and 1980s, when cognitive psychology reached what may have been its peak in prominence, "social cognition," "cognitive development," and other cognition-focused approaches provided a guiding framework for much of the research in psychological science. In some sense, the rise and fall of interdisciplinary topics may be a harbinger of the dominant, or at least the newly emergent, paradigm in the field. However, we must also be cautious in our interpretations of such non-empirically based data; as Miller and Keller (2000) have noted,

psychologists may use terminology from neuroscience, the currently "hot" area, to "repackage their phenomena to sound biological [due to] political pressures" (p. 213), rather than to find useful or meaningful interconnections or areas of overlap between neuroscience and other psychological schools.

Nonetheless, it is clear that the brain has become a common currency for discussing psychological ideas, and this holds for virtually all areas of the field. Psychologists are now asking questions such as, "Are the only acceptable concepts those with biological reality?" (Kimble, 2000, p. 208); and "Can there be any doubt of the increasing biologization of psychology?" (Slife & Burchfield, 2002, p. 250). Such questions may ring a familiar chord to many psychologists: Similar questions were asked several decades ago, but with reference to the behavioral school and concepts.

In fact, if we examine any single psychological phenomenon, we can see that whichever school is prominent determines how it is studied. For example, the emotional phenomenon of fear is currently viewed according to models that specify its neuroanatomy and neurochemistry, and which make use of magnetic resonance imaging (MRI), brain lesioned patients and animals, and single-cell recording in rats (e.g., Amaral, 2002; Damasio, 1999; Davidson, 2001; LeDoux, 1996; Panksepp, 1999). However, in the early part of the century, when psychoanalysis was the dominant school of thought, fear was studied as displaced anxiety, typically due to repressed sexuality, as in the case of Freud's famous interpretation of the phobic "Little Hans" (Freud, 1909/1955). Later in the century, fear was reduced to a behavioral response to conditioned stimuli, and Little Hans' phobia was reinterpreted in this light (Wolpe & Rachman, 1960). By the 1970s and '80s, fear became seen as a cognitive appraisal of threat and, in fact, all emotions were viewed as cognitions paired with undifferentiated arousal (Schachter & Singer, 1962). Thus, the study of fear provides an example of how trends in the field influence the study of important psychological phenomena and shape the way scientific research is conducted.

However, despite the fact that neuroscience shows some signs of influencing the study of certain psychological phenomena, it is clearly more prominent outside the domain of psychology. In some sense, the field of neuroscience may be at a crossroads: Will it remain within the psychological sciences, will it align itself even more strongly with biological science, or will it break its allegiance to either discipline and form its own field? In our opinion, psychology should not let neuroscience slip away, perhaps taking with it a host of topics formerly studied by psychologists. Thus far, even the psychologists who are increasingly oriented towards understanding the relevant neural mechanisms of the topics they study (e.g., emotion, attention, memory) have remained centrally located within psychology.

Yet, the integration of the neuroscientific perspective within psychology cannot be taken for granted. Indeed, at times it would seem that many psychologists must be reminded that "Just because biological factors are *necessary* to a person's behavior does not mean these factors are a complete and *sufficient* cause or explanation of this behavior; other conditions may be necessary for the particular behavior to occur" (Miller & Keller, 2000, p. 252). Just as Plomin (1997) has urged psychologists not to lose DNA to the molecular geneticists, Squire (1997) has recently called on psychologists to take active steps to strengthen links between psychology and neuroscience. Kimble (2000) noted that "Behavioral concepts tell biological psychology what to look for in experiments [and] the details of [biological] data often suggest refinements of the behavioral concepts, thus initiating a new cycle of behavioral and biological investigation" (p. 209). Kosslyn et al. (2002) recently argued that, "linking psychology to biology in a way that respects individual differences may have profound implications for psychotherapy" (p. 349). Even those who advocate reducing psychology to neuroscience have argued that "neuroscience needs psychology because it needs to know what the system does" (Churchland, 1986, p. 373).

On the other hand, some researchers have questioned whether higher-level sciences like psychology and lower-level sciences like neuroscience can "offer any useful guidance to each other" and have suggested that psychology and neuroscience "must simply pursue their own problems in their own way" (Bechtel, 1988, p. 78). As Laird Cermak, former editor of *Neuropsychology*, pointed out, "Neuropsychology is a discipline for which the boundaries are still being defined—it is still inventing itself" (*APA Monitor*, 1996). We anticipate that the next few years will see many "border disputes" between psychology and other disciplines to determine the extent to which neuroscientists view psychology as their primary academic home. E. O. Wilson (1999) has even suggested that psychology (as well as other social sciences) may be at a critical juncture, which will determine whether it degenerates into a post-modernist free-for-all or evolves into a fully-fledged natural science. Although we would not cast psychology's prospects in terms of such extremes, we do believe that psychologists must work harder to integrate neuroscience within the field.

Writing to the general psychological community, Squire (1997) argued, "As the next century approaches, one should celebrate and encourage the increasing partnership between psychology and neuroscience." We agree, and our finding of a recent rise in attention to the neuroscientific school may be the first empirical evidence for the emergence of such a partnership. In our opinion, the brain and behavior need to be studied in tandem, and psychology should not let neuroscience slip away. However, neuroscience should not become the single prevailing perspective in the field.

Miller and Keller (2000) made a related point about fear: "Researchers are learning a great deal about the biology of fear—and the psychology of fear—from studies of the amygdala (e.g., Lang, Davis, & Öhman, 2000), but this does not mean that fear *is* activity in the amygdala. That is simply not the meaning of the term. 'Fear' is not reducible to biology" (p. 212). We strongly agree with this sentiment: Important psychological phenomena cannot be completely explained from the perspective of any single school. As Zuckerman (1999) noted, "One thing that both behavioral and post-Freudian psychoanalytic theories had in common was the conviction that learning and life experiences alone could account for all disorders" (p. 413). Fear cannot be reduced to amygdala activity, nor to repressed castration anxiety, nor to a conditioned response to a stimulus, nor to a cognitive appraisal of threat. Fear may be all of these things, and it can best be understood when various ideas of the different schools of thought are viewed as complementary, rather than antagonistic.

Implications for the Progress of Psychology as a Science

Although our analyses are neutral with regard to different views of scientific progress, several perspectives within the history and philosophy of science may aid in interpreting our empirical findings. First, from a Kuhnian perspective, our findings point to the cognitive school as the most recent in a succession of dominant paradigms. But other forms of evidence are required before a Kuhnian revolution can be declared; Kuhn's emphasis on the socialization process within science would call for evidence that, for example, the textbooks of scientific psychology have become primarily cognitive in orientation.

However, given debates about whether psychology has even reached the paradigmatic stage of science (e.g., Briskman, 1972; Leahey, 1991; Schultz, 1981; Warren, 1971; Watson, 1977), the proper question is not whether cognitive psychology is a newly dominant paradigm arising from the revolutionary ashes of a prior dominant paradigm, but which of the various schools is most likely to gain a transcendent status and introduce the longawaited paradigmatic stage of scientific psychology's development. Viewed within a multiparadigmatic framework, cognitive psychology may be the most likely of the existing schools to move psychology to the Kuhnian stage of normal science. However, the apparent emergence of the neuroscientific school allows us to question how long the cognitive school will remain dominant, and it is possible that within the next decade the two schools will directly compete for transcendence.

Viewed from the Lakatosian perspective, our findings imply that the cognitive school has attained prominence because it has been a progressive

research program; that is, it has yielded new hypotheses and findings. The Lakatosian perspective also implies that the current surge in cognitive psychology need not be interpreted as a scientific revolution, but could represent a revitalization of a perspective that was progressive in the early days of psychology (e.g., Wundt and James both emphasized the role of mental activity), degenerative during the middle of the 20th century, and now revived to prominence. Similarly, the currently lower prominence of the behavioral school may be seen as only a temporary slump, leaving open the possibility that the behavioral research program may once again rise to prominence in the field. Finally, the Lakatosian view implies that the four schools can inform each other; that is, they are not strictly "incommensurable" as Kuhn would have argued. Consistent with this view, there does seem to be productive exchange among the various schools of psychology. As noted earlier, cognitive neuroscience represents a fusion of ideas from the cognitive and neuroscientific schools, suggesting that these two perspectives may be able to coexist fruitfully. More generally, psychology appears to be coagulating around broader scientific orientations that cut across traditional fields of study (e.g., cognitive science combines psychology, linguistics, computer science, philosophy, etc.). At the same time, psychology also seems to be dividing into increasingly specialized camps (e.g., visual attention; stereotypes; implicit memory), which may eventually render broad classifications such as "cognitive psychology" obsolete. This last possibility raises a troubling question: Is psychology a splintered field, or one moving toward an overarching orientation that will tie together its diverse threads?

From Latour's perspective, cognitive psychology may have become prominent because its scientific products were effectively "publicized" to the rest of the field. This account may also be used to explain the newfound rise of neuroscience, a field that has been widely discussed by scientists, extensively covered in the popular media, and further promoted by an act of Congress (the congressional resolution labeling the 1990s as the "Decade of the Brain").

From the perspective that prominence is based on passing fads and constantly shifting trends (Christensen-Szalanski & Beach, 1984), the rise of cognitive psychology may constitute an ephemeral blip in the history of psychology, rather than a long-term shift in the orientation of the field. Clearly, though, the fact that the cognitive perspective has sustained its prominence for more than two decades argues against this idea. Thus, although we recognize that there are paths other than scientific merit to achieving prominence, we believe that the robust scientific trends documented in the present study reflect more than just fad or fashion. It remains to be seen whether the emerging neuroscientific perspective will have the

same sustained influence as the cognitive perspective, and work its way into the conceptual and methodological infrastructure of the field.

Limitations

After we published our findings on trends in the field (Robins et al., 1999), a series of published commentaries on our article raised a number of concerns, both methodological and conceptual. Not surprisingly, the question of what's hot and what's not sparks contentious debate. In fact, the wide range of responses elicited by our findings supported our initial contention that speculating about trends is a popular pastime among academics.

Methodological Issues

Are our empirical analyses a reasonable and feasible approach to informing the widespread speculation within the field about scientific trends? We believe they are, and we had hoped that our original article would serve as a clarion call for a more empirically informed approach to the history of psychology. Several commentators on our original article, however, expressed objections to the specific methods we used in conducting these analyses. It was suggested, for example, that we expand our keyword searches (Friman, Allen, Kerwin, & Larzelere, 2000). We agree that these analyses could not detect all of the articles related to each school. For example, the keyword "cognit#" will not detect all cognitive articles because even articles that are central to the cognitive perspective may not have "cognit#" in their title, list of keywords, or abstract. This issue is even more pertinent for our dissertation analyses, which relied on searches of subject or title words only, omitting the abstract. Moreover, as we have noted previously (Robins & Craik, 1994), even the four broad schools of thought do not exhaust the perspectives found in psychology. Thus, articles from an evolutionary, behavioral genetic, or social constructionist perspective are unlikely to include any of our keywords. Finally, our keyword indices may not identify psychological research that is centered on specific content domains (e.g., self-esteem, emotion regulation) and/or applied issues (e.g., industrial psychology, sports psychology). Given these points we would not expect our keywords to capture the full range of articles published in the flagship publications.

Of course, there is no end to the additional analyses that could be performed or the additional data that could be collected. Thus, we aimed to circumvent several potential limitations through the use of a multi-method approach. Specifically, we replicated our findings across three measures of prominence, and we explored several sets of keywords and journals (see

Robins et al., 1999). The three measures paint strikingly similar portraits of the recent history of psychology, indicating that the trends are not dependent on any particular measure, journal, or keyword. Thus, to question our findings, one would need to (a) identify flaws in all three of our measures *and* (b) explain why these three flawed measures all point to the same trends.

Furthermore, two of those who commented on our original articles conducted additional analyses that converged with our results. Tagler's (2000) analysis of prominent psychologists supports the dominance of the cognitive school over the behaviorist school in recent years. He found, for example, that B.F. Skinner was rated the most influential psychologist in 1966 and 1981, but by 1998 had dropped to 7th most-influential and been replaced by cognitively-oriented psychologists. Gibson (2000) reached a similar conclusion based on his analysis of APA presidential speeches. He also replicated our finding that the 1970s was the specific decade in which the cognitive school overtook behaviorism in prominence. Both of these studies thus reveal the same trends as our three sets of analyses, and of course neither relied on our choice of keywords, methods, or journals.

One thing that we found striking about the commentaries to our articles, as well as the feedback we received from reviewers and other readers, was that most of these scientists had numerous suggestions for additional analyses but none took their own advice. We find it surprising that scientists seem to leave their "science hats" at the door when it comes to evaluating and understanding trends of prominence in science. One point that we hope to make with this chapter is that the methods we used are available to any researcher who wishes to replicate or dispute our findings, or to uncover trends in other areas. Most of our analyses can be conducted quickly and easily, using publicly accessible databases. We encourage other researchers to test contrary claims and to use alternative methods. Behaviorist-oriented researchers seemed the most reluctant to accept our findings, so we would be interested to learn what set of specific keywords would successfully reverse the empirical trends we report for the behavioral and cognitive schools. At this point, however, the accumulated empirical evidence supports our findings (including the additional analyses reported by Gibson and Tagler).

Prominence can be assessed through a wide variety of indices, so, in some ways, our analyses may serve as an example of how to empirically quantify prominence. We urge other researchers to use these methods to assess trends in other fields of science, trends within particular areas of psychology, or to improve upon our analyses by expanding or changing the domains of journals and subject-matter indicators that we used. One interesting undertaking, of relevance to the present volume, would be to chart

empirically the pattern of recurring ideas within the field of psychology. The key point is that beliefs about how the field is changing are not always supported empirically, and claims about trends need to be substantiated with data.

Conceptual issues

Leigland (2000) critiqued our undifferentiated view of the behavioral school, and called for a historical analysis that takes into account the complexities of behaviorism and its intellectual offspring. This is an important point; each school of thought examined in our analyses may well have contributed to other areas, which have now become more prominent than the progenitor. For example, it could be argued that behavioral neuroscience is an offshoot of behaviorism. Our own interest was in charting trends at a macro level, but we hope that others will conduct more fine-grained empirical analyses and use the complementary methods of scholarly historical analysis to tease apart the trends among interrelated areas. Important issues regarding the multiple voices within behaviorism, for example, may be more properly treated within the intellectual history of psychology (Kendler, 1987).

Another limitation is our focus on four, and only four, schools of psychology. The substantive domains covered by these four schools do not comprise most of our field's scientific activity. As we have noted elsewhere (Robins, & Craik, 1994), much else is underway within contemporary psychology. We have simply focused upon an analysis of four traditions that have received speculative comparative discussion in the research literature, in textbooks on the history of psychology, and in informal 'shop talk' at research centers.

Finally, one commentator suggested that our empirical research was biased by our particular theoretical orientation. However, we are rather eclectic psychologists with broad interests in affective (Tracy), developmental (Robins), ecological (Gosling), and social-personality (all) psychology. We set out unaware of having any particular 'axe to grind', and, in short, our bias is not for or against a particular school, but against speculating about trends without empirically documenting them.

WHAT LIES DOWN THE ROAD FOR PSYCHOLOGICAL SCIENCE?

It is worth concluding on a simple point: Scientific understanding accrues from the cumulative findings of a program of studies. No single study can address all research questions and methodological limitations. We are

arguing for treating the history of psychology as an empirical research area that can yield cumulative, quantitative, and increasingly sophisticated and differentiated insights. That is, we do not envision single study, yes-or-no answers about trends in the field, but rather we see incremental progress toward understanding the development of the field. We view our study as part of a broader scientific enterprise, the goal of which is to document replicable trends using empirical indicators and thus move beyond polemics. If psychology is indeed at a crossroads that will determine its future as a science, as E. O. Wilson recently argued (Wilson, 1999), then it is incumbent on the field to understand its history.

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Part 2

Comparative Psychology

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6

BECOMING BIG FROGS IN A SMALL POND

ON GAINING EMINENCE IN COMPARATIVE PSYCHOLOGY

Donald A. Dewsbury

My goal in this chapter is to explore some of the organizing principles of this volume within the field of comparative psychology. I consider some of the reasons that individual comparative psychologists and their ideas have gained influence and, where I can, why they have lost it at a later time. Comparative psychology is a relatively small part of the overall field of psychology but its small size may make analysis somewhat more straightforward than in larger areas.

Although I prefer well-defined methodology, my approach here must be somewhat informal and subjective. I have no pretensions of a detailed quantitative study (cf. Simonton, 2002); rather this is a preliminary search to uncover some factors that appear relevant to the spread of research and ideas.. I have tried to use my knowledge of the field to discern patterns that have characterized the ebb and flow of comparative psychologists and their ideas. Although this may make it difficult to defend specific cases I believe that a reasonable pattern emerges.

My core group will consists of comparative psychologists—those interested in studying the genesis, control, and consequences of behavior in a wide variety of species. However, I will stray outside of arbitrary boundaries as well because some illustrative examples can be found in the

work of ethologists and other zoologists working on the same problems as comparative psychologists.

First, I discuss some case studies with the goal of revealing determinants of influence. I then consider some factors not emerging from these case studies. Finally, I try to bring these together in a table listing the factors that have emerged.

QUALITY

We must not forget the obvious. Writers on rhetoric and the psychology of science sometimes fail to mention that some ideas or experiments are simply more effective within the context of their time and their discourse community than are others. The psychologist may provide an elegant solution to a problem or conduct an experiment that appears to solve a dispute that is under debate within the field at the time. Such work may be influential regardless of the factors to be considered below. It is because this need not always be the case that the remainder of this chapter is necessary.

CASE STUDIES

Seizures in Rats

Psychologist Norman R. F. Maier received the Thousand Dollar Prize of the American Association for the Advancement of Science for his studies of conflict-induced seizures in rats (Dewsbury, 1993). In the Lashley Jumping Stand, rats were required to jump across a gap toward one of two windows, one of which that was designated as correct and led to food where the other was deemed incorrect and caused the rat to bump its nose and fall into a safety net. If the rat hesitated, a brief blast of air was used to cause it to perform. When a variety of stimuli were used, rats learned these discriminations readily. However, when the problem was arranged so as to be insoluble, they tended either to fixate on one choice or to display convulsive seizures. Maier treated this as "neurotic" behavior and the phenomenon received considerable media attention and the prize. The lead critic of this research was Clifford T. Morgan, who reported that similar convulsions could be elicited by an air blast alone, without the conflict situation used by Maier. According to Morgan, Maier's effect was an artifact of the procedures used. A rash of publications from these two authors, and others, followed. Most observers concluded that Morgan's interpretation had been correct and that Maier's observations had little to

do with either conflict or neurosis. Morgan continued with his career as a powerful physiological psychologist; Maier was disillusioned and for this, and other reasons, left the field but was successful in industrial psychology. Archival information suggests that, in fact, Maier had successfully defended his position. Morgan admitted his error in a letter to Maier, though not in public (Dewsbury, 1993). The retraction never appeared in any journal.

Why did Morgan's view prevail over Maier's? Morgan was an establishment scientist from the then dominant Northeastern schools who worked within the prevailing context and interacted well with his fellow scientists. Maier was a less sociable Mid-Westerner who was regarded as something of a maverick scientist; he had been influenced by Gestalt psychologists and suggested the importance of processes more cognitive than was generally accepted at the time. Although he was an internationally recognized scientist who produced over 200 articles and a dozen books, upon his death he received few of the accolades to be expected from such productivity. After the conflict, Maier proposed the satirical "Maier's Law," according to which most psychologists tended to dismiss facts that were incompatible with their theories rather than the reverse (Maier, 1960). This case suggests that such factors as academic pedigree, sociability, effective networking, and conformity with prevailing theory can influence acceptance of ideas.

Cognitive Ethology

American ethologist Donald R. Griffin built a reputation as a leading animal behaviorist based on research into such important topics as orientation in migrating birds and the sonar systems of bats. Later in his career he turned to a different set of issues.

Issues of animal consciousness and related mental experience had been prominent in 19th-century comparative psychology; however, they had been virtually eliminated from the field as the result of critiques from a number of perspectives not the least of which was the behaviorist approach. Griffin came to believe that the science of the late 20th century had advanced to the point where windows to the minds of nonhuman animals could be found and he authored a series of books and other works championing his new "cognitive ethology" (e.g., Griffin, 1976). Others had made similar proposals that lacked both authority and effect. Griffin's views have been highly criticized by many psychologists, including those studying animal cognition (e.g., Yoerg, 1992) but have led the way toward a renewed interest in such topics that would have been unlikely without Griffin's authority. According to a *New York Times* article, "were it not for

the fact that he had a tenured position at Rockefeller University as well as an international reputation, Dr. Griffin might have found himself out of a job . . . but the field Dr. Griffin christened cognitive ethology ultimately took off" (Eakin, 2001, p. xx).

Surely, Griffin's ideas were important in gaining recognition. In addition, however, I suggest that his position as one of great authority, his persistence, and a human proclivity of anthropomorphic interpretation also were important.

Dancing Bees

In 1973, in recognition of his research on honeybees, Karl von Frisch shared the Nobel Prize for Physiology or Medicine. Von Frisch demonstrated that, upon returning to the hive from a successful foraging trip, honeybees engage in a display he termed a "dance." In a series of experiments, bees were trained to go to one food source and then hive mates, who had observed the dance, were observed to determine whether they flew to the same or a different location. When the food source was more than a certain distance from the hive, about 100 m in some strains, the distance to the food was indicated by the pace of the waggle dance performed. Direction, relative to the sun, was indicated by the angle of the dance in the hive (von Frisch, 1950).

When American zoologist Adrian Wenner repeated these experiments he found the dance to be of little significance in transmission of information concerning the location of food. Rather, Wenner believed that von Frisch's experiments were flawed; bees, he thought, relied on odor cues to find food. This discrepancy led one of the most high-profile controversies in the history of animal behavior studies (Veldink, 1989). Wenner, James Gould (a student of Griffin), and others conducted research and wrote critiques back and forth. If one discusses the issue with most animal behaviorists or consults a textbook in the field today, one will find that Gould's experiments were critical and supported von Frisch's position. As revealed in relatively recent personal correspondence, however, Wenner still maintains his position.

Regardless of one's view of the resolution, many would agree that von Frisch's original experiments were suggestive but inadequate with respect to today's standards regarding several matters of design. Veldink (1989), working in the context of the work of Bruno Latour, explored the reasons for the acceptance of von Frisch's position at the time when supporting data were inadequate. Veldink suggested that the notion of a bee dance gripped the imagination as the olfactory hypothesis did not. Further, through sheer repetition an inertia can be built up making it difficult

to discredit an "established" idea. Veldink suggested that there was an appeal of the animal itself, what she called "the delightful and capricious honey-bee" (p. 170). She noted differences in the perceived personal qualities of von Frisch, the polite gentleman, versus Wenner, the young upstart. Authorities in related areas, such as E. O. Wilson and Griffin, weighed in on the side of the von Frisch proposals, thus tilting the balance. Wenner, like Maier, got discouraged changed areas, shifting to marine biology (Veldink, 1989). Another factor was the apparent weight, as opposed to quality, of the evidence presented. Following Latour, Veldink noted the way in which theory can be converted into fact. That is, that findings are decontextualized in secondary references and presented as if they had been definitively established (see also, Dewsbury, 1998). Finally, Veldink suggested that teleological arguments were influential; the view was that such complex behavior as the waggle dance must have a function. To repeat, what is at issue herein is not which position was "correct," but which non-data-related factors appeared to favor von Frisch.

Ethology and Comparative Psychology

Comparative psychology and ethology represent two approaches to the study of animal behavior that developed around the middle of the 20th century. There was always much overlap between the two. Nevertheless, ethology was developed in Europe by zoologists who tended to emphasize the study of birds, fish and insects. Ethologists often made observations and conducted experiments in the field. They viewed much animal behavior as being innate and were interested in how the behavior evolved. Comparative psychology was developed in the United States by psychologists. Whereas some psychologists did exactly what was just described for ethologists, many others tended to study mammals in the laboratory and were especially interested in the study of learning and the development of behavior.

Although comparative psychology has a long, continuous history, it can be argued that ethology enjoyed greater success (Dewsbury, 1997). It was ethologists who often wrote positively about their field; comparative psychologists usually belittled theirs. Ethologists, rather than comparative psychologists, were often invited to contribute prestigious chapters and addresses in psychology. Seeking advanced training, many psychologists went to ethological laboratories; few ethologists went to psychological laboratories. Three ethologists shared the 1973 Nobel Prize for Physiology or Medicine; comparative psychologists received no such honor.

It would be difficult to assess the quality and importance of the two approaches in any objective manner so as to demonstrate that one was more

deserving of success that the other. Even if it were true that ethology was somehow better than comparative psychology, however, there may have been other factors contributing to its success (Dewsbury, 1997). I studied this by comparing two sets of articles appearing in popular magazines during the 1940s and 1950s: one from ethologist Niko Tinbergen and one from psychologist Frank Beach. The ethological articles often contained statements concerning the joys of research, extolling the beautiful scenery and weather and the pleasures of observing animals in nature. I found no such rhetoric in Beach's articles. Whereas both authors sought a scientific approach to animal behavior, Beach's was colder more slanted to objectivity; Tinbergen's was softer and placed the observer more prominently in the research process. Whereas Tinbergen might describe his animals as "happy," "confused," or "delightful," Beach would write of the "beasts" or of a "lowly" creature. Tinbergen's articles rarely included historical references; Beach's sometimes did—perhaps in an effort to legitimate the research. Both sets of articles were richly illustrated. Almost all of Tinbergen's photographs were taken outdoors; some showed beautiful panoramas. Most of Beach's were of animals performing learned acts. Overall, Tinbergen's articles presented a more humane image with the human close to nature; Beach's were in the tradition of dispassionate scientific inquiry. Similar differences in rhetoric can be found in other writings from the two disciplines. It is likely that many of the students of the 1940s and 1950s encountering these two approaches were more swept away by the romantic rhetoric of the ethologists than with the hard science of the psychologists and that this was one factor in the ethologists' success.

Robert Yerkes and the Rockefeller Foundation

Robert Yerkes was perhaps the most influential comparative psychologist of the first half of the 20th century. His dream from his graduate school days at Harvard was to found a primate station in a warm-weather climate. Funding would have to come from private foundations as large Federal programs were not yet available. For many years Yerkes enlisted the support of his colleagues, wrote to various potential sources, visited with many officials, made plans, and tried to cajole support for his facility. Finally, in 1925 Yerkes secured funding for a primate facility on the Yale University campus. In January of 1929 the Rockefeller Foundation approved $25,000 for a feasibility study of a remote primate station. Yerkes selected a site in Orange Park, Florida and full funding of $500,000 was approved later in the year; the facility that would become the Yerkes Laboratories of Primate Biology was opened in 1930.

Yerkes was a progressivist who believed that knowledge of chimpanzee behavior would be critical in a program of social engineering to improve human behavior. These goals were in line with the progressivist leanings of the Rockefeller Foundation. The Foundation had been established with a rather general mandate to promote human well-being through the acquisition of knowledge, relief of suffering, and the advancement of human progress. In the late 1920s, the foundation was redirected to emphasize knowledge rather than application; science and medicine were viewed as the best routes to the improvement of the human condition. It was probably the affinity between their goals that lead to the Rockefeller funding even though the Rockefeller officials were somewhat skeptical of Yerkes' abilities.

Yerkes and the Rockefeller Foundation officials would soon have sharp disagreements concerning the conduct of the facility. During the 1930s Warren Weaver at the foundation began directing more funds toward molecular biology. In that milieu, Yerkes' version of a more integrative psychobiology appeared old-fashioned. Among the concerns, the most serious centered about Yerkes himself, his handling of his staff members, and his tendency to control his Station. Yerkes was viewed as too much of an old-fashioned naturalist in an era when the "modern" approach was becoming more reductionistic. The tide was turning against Yerkes and new questions were raised. During 1936–37 the Rockefeller officials sought the opinions of some experts in the field. The Rockefeller representatives, and others, wanted the Station opened up for wider use by visiting scientists; Yerkes objected. The appropriateness of chimpanzees as ideal animals for research was questioned. There was concern with the progress made in Orange Park; Edward Tolman indicated that Yerkes' output was not consistent with the support granted. Perfectly timed at the outset, Yerkes' approach fell in disfavor when Rockefeller embraced research in molecular biology.

Eventually, the only way to continue the station was for Yerkes to resign as Director. For a while he remained as the nominal director, though residing in New Haven; he formally retired as director in 1941. Yerkes' contribution in founding the Station stands as a major accomplishment. He was unable, however, to take it to the next level and develop the station into a productive research facility. He had been the right man in the right place at the right time to found the Station but now appeared as a figure from a Greek tragedy.

Where Yerkes he had been in synchrony with the ebbs and flows in the surrounding milieu during the late 1920s, he was out of synchrony by the mid-1930s. The lesson here is that success is correlated with the prevailing

emphases of the time. A proposal that would be regarded favorably at one time might be treated unfavorably a few years later as the emphasis was shifted. Some scientists find a way to adapt to changing priorities; others are insensitive to them or unwilling to shift priorities.

Data Simplification

One important step in promoting one's results so that they become widely known and their originator gains status and eminence is with what I have termed data simplification (Dewsbury, 1998). The results of any experiment are obtained at a particular time and place and using a particular methodology. Several authors have pointed out that the way in which such specific results become accepted phenomena is through a process of decontexualization. A result that was obtained once under specific conditions is stated as a general principle. There is a shift from "these rats did so and so" to "rats do so and so."

Data simplification is a part of this. Most data are flawed in one way or another. The investigator is obliged to mention these difficulties in the published primary report of the experiment. However, mention of these difficulties is usually confined to the Results and Discussion sections. The abstract and conclusions are written as it there were no difficulties. This is the process of primary simplification. The next step taken by the investigator is to discuss the results in grant proposals, review articles, talks, theoretical papers, and popularizations. The limitations and difficulties recede even further into the background. This is secondary simplification. Finally, the experiment is discussed by other authors in review articles, textbooks, and the media—limitations of the results are nowhere to be found. Thus, what might have been a problematical and limited result gets transformed into an accepted fact.

I illustrated this with some of Robert Yerkes' research (Dewsbury, 1998). Yerkes was interested in the dominance relations between male and female chimpanzees and how they changed through the menstrual cycle. He studied this by introducing a single piece of food in the presence of two animals and determining which one secured the morsel. He started with 450 tests during 45 menstrual cycles but reasoned that there were only nine cycles with 1,680 observations (less than 20%) that were valid. The criteria for eliminating data were most questionable. The resulting data appeared to show a trend that females obtained the food when in estrus but the males obtained it at other times. Yerkes concluded that males granted privilege to the females. However, the graph of even the reduced data was irregular so Yerkes produced two idealized plots of his data and published them. It was those fictional plots that became identified with the phenomenon.

As might be guessed, Yerkes' conclusions were questioned by feminists among others. Few, however, went back to the original data to see how flimsy were the foundations of Yerkes' conclusions.

Data are often messy. The effective scientific promoter smooths out the difficulties with the data each time the story is retold and thus transforms messy data into clean, established phenomenon. The ineffective promoter allows the imperfect data to sit or is guarded in writing about them.

Harry Harlow

Harry F. Harlow was perhaps the most successful of the comparative psychologists; he was well-funded, garnered many honors, and became highly respected and quoted throughout the field of psychology. Among his best known work was his finding of the importance for young rhesus monkeys of the ability to cling to the mother of some other object that provided "contact comfort." Even more famous are his findings of deficits in social behavior in rhesus monkeys that followed various kinds of isolation in infancy (e.g., Harlow, Harlow, & Suomi, 1971). Also well known are his findings that monkeys can learn to learn. He found that in "learning set" experiments, performance improved on successive problems (Harlow, 1949); the phenomenon is commonly reported in textbooks.

It is interesting that these phenomena were also studied at the Yerkes Laboratories of Primate Biology. Indeed, Yerkes had earlier used the phrase "learning to learn" that became associated with Harlow (Yerkes, 1943, p. 130). McCulloch's (1939) study of young chimpanzees deprived of their mothers includes clear descriptions of a role for contact comfort as well as an anticipation of Harlow's later error-factor theory. Henry Nissen at the Yerkes Laboratories conducted a long series of studies of infant chimpanzees reared in isolation.

In all of these cases, it was Harlow's work that became famous and often cited; much of the Orange Park work rests quietly on library shelves. Surely, Harlow was a skilled experimenter. He had an advantage working with rhesus monkeys in that they were easier to obtain in larger numbers and developed mor quickly so that data could be generated more quickly. An important factor, however, was salesmanship. Harlow was a brilliant lecturer and master writer. He presented his material in such a way as to embed it in the minds of his readers and listeners. Rumbaugh (1997) wrote of Harlow's "genius, expressed crisply even in his speech and recorded for all times by his industrious pen and office" (p. 207). Yerkes, by contrast, "lacked the salesmanship, the charisma that ran richly throughout Harlow's personality" (p. 207). Further, those who conducted the infant studies program in Orange Park, Henry Nissen and his colleagues, were

low-key, self effacing scientists with a somewhat casual attitude about publishing results. This contrasted with Harlow's "industrious pen."

Yet another aspect of Harlow's success was his use of humor. Harlow's lectures and writings were packed with clever and humorous statements that made his work a pleasure to hear or read while that of others is often burdensome (e.g., Harlow, 1958). He could also coin a phrase that both captured a phenomenon and implanted it in the reader's memory.

As with Yerkes, Harlow's work can be viewed in the context of broader changes in our culture. His research was done at a time of great interest in the effects of early experience as the result of influences such as the work of Sigmund Freud and Head Start programs in the United States. This worked to Harlow's advantage. However, a cultural shift related to a different set of issues has worked to Harlow's disadvantage. Many around the world have become more sensitive to issues of animal welfare than in the past. Harlow has been singled out above most others with sharp criticism. His work has been called "inhumane," and "ethically unacceptable" (e.g., Liss, 2002, p. 8). Thus, although his research has retained its place in the literature, Harlow himself has been reviled in many venues.

Worms that Learn

Rivaling Harlow in flamboyance was Michigan psychologist James V. McConnell. During the 1950s and 1960s there was great skepticism regarding the possibility of learning in invertebrates. McConnell conducted a substantial research program on learning in flatworms, or planarians—the evolutionarily oldest species with a bilaterally symmetrical nervous system (e.g., McConnell, 1965). Although the early studies lacked certain controls, McConnell and his associates later provided definitive evidence of classical conditioning in planarians. However, he went well beyond this. In the belief that learning was encoded in "memory molecules" of RNA, McConnell and his associates published studies purporting to show, among other things, that memories could be transferred from one animal to another via cannibalism or by extracting RNA from trained animals and injecting it into untrained animals. These results have been generally rejected by most scientists. In the process, however, many observers have rejected the data on learning in flatworms. It is as it the over-extension negated the positive results that were obtained.

McConnell, who had a background in broadcasting, was a master publicist and went well beyond the usual means of disseminating his results. He became a media personality, written up in major magazines and appearing on such high-profile television programs as the Steve Allen "Tonight Show." In popularizing his work he introduced such notions as a memory

pill and a professor-burger. With his affinity to science fiction, he wrote a fictionalized version of his own experiences in beginning his research with flatworms. He started his own journal, The *Worm Runner's Digest* (later joined by the *Journal of Biological Psychology*) to publish both scientific results and humor about science. His studies became well known and were the topic of hundreds of replications by high school students for science fair projects. Later, McConnell abandoned animal research, wrote an idiosyncratic, but successful, textbook in introductory psychology, and became such a high-profile advocate of a popularized psychology as to become a target of the "Unabomber."

Today, in what Rilling (1996) calls the "mystery of the vanished citations," McConnell has almost disappeared from the literature. According to Rilling, those who did this early work on the biochemistry of memory "have become nonpersons—eclipsed, put down, or written out of the contemporary search for the engram" (p. 590). Rilling discussed the difficulties that McConnell experienced in publishing his research in conventional journals. He noted, however, that "the profession does not have a mechanism for providing peer review for the attempts of psychologists to popularize their discipline" (p. 594)(i.e., tertiary simplification).

There is an interesting twist to the issue of authority and the acceptance of results. During the 1960s, McConnell adopted a counter-culture stance with which to poke fun at the scientific establishment (see Rilling, 1996). In the short run, this was a successful strategy for gaining recognition. In the long run, it might have accelerated his disappearance from the literature.

Surely one reason for the disappearance of McConnell's work is his over-extension of his results; the phenomenon was over-sold. Thus, one can over-sell as well as under-sell. Once it appeared that the work on the memory molecule could not withstand criticism, McConnell's whole oeuvre was called into question and forgotten. Another reason may have been a reaction to his high-profile personality. Many scientists resent the kind of grandstanding displayed by McConnell. It may seem paradoxical to many scientists, but some were bothered that he was too open about his experiments. He was casual in relaying all of the little problems and idiosyncrasies of his research. These are details that are suppressed in most experimental reports. His "allowing backstage contingencies to show through" was a violation of the accepted code for science reporting (Travis, 1980, p. 178).

McConnell's use of humor surely offended some scientists. it was one thing for Harlow to introduce humor in presenting his results but quite another thing for McConnell to use humor to poke fun at the scientific establishment. McConnell cited his use of humor in the *Worm Runner's Digest* as a factor in his losing grants and having submitted articles rejected

for publication. He noted "it would seem that a little humor goes a very long way—towards excommunication!" (1969, p. 242).

SOME ADDITIONAL CASES, CHARACTERISTICS, AND EXTENSIONS

There are many other cases that reveal some aspects of the determinants of the spread of ideas in comparative psychology. Because space is limited, I deal with some additional factors and examples in brief.

Characteristics of the Scientist

A number of factors related to individual scientists have been associated with creativity by a number of students of the creative process. Simonton (2002) for example, found such factors as independence and resistance to pressures for conformity to be important.

A major factor in the acceptance of ideas is the perceived authority of their proponents. A proposal that might be rejected out of hand were it to come from a fledgling scientist might be granted great attention and influence were it to come from one who is perceived as an authority. Age and accomplishment are surely major factors in the development of such authority. A major award, such as a Nobel Prize, can have great impact. With this authority, scientists sometimes can have influence outside of the field within which the award was given and when they defend unpopular ideas. Roger Sperry, a psychologist who studied neural plasticity in a variety of species of vertebrates, received the 1981 Nobel Prize for Physiology and Medicine. Late in his career, Sperry turned to more philosophical issues, proposing that consciousness both emerged from brain activity and could affect it. He wrote various articles spelling out the implications of his thinking (e.g., Sperry, 1988) that were not popular with his reductionistic colleagues but garnered much attention. In another example, McConnell was not helped in his efforts to establish a literature on learning in flatworms by the fact that Melvin Calvin, a 1961 Nobel Prize winner in chemistry, was among his critics.

Other scientists may not be perceived as quite as authoritative as the very top echelon but gain influence through other attributes that leave them portrayed with sympathetic images. Who has not seen the television programs of Jane Goodall with her wild chimpanzees? These have given her a position of influence on a broad range of issues well beyond those of chimpanzee behavior. The European ethologists, especially Nobel Prize winners Niko Tinbergen and Konrad Lorenz, were especially effective in

this regard. The image of Tinbergen as the "curious naturalist" was a factor allowing him to speak out on such issues as world peace even before winning the prize. Later, he reflected on "a not wholly pleasant awareness that too many people suddenly began to look up to me as if I were a kind of superior know-all, understand-all, and in general a *wise* man" (Tinbergen, 1985, p. 456). Lorenz's kindly and genial image, particularly with imprinted birds, was reproduced frequently in the media and gave him similar credibility. An interesting perspective on the waning of such credibility can be seen in that many are re-evaluating his scientific work in the light of revelations of Lorenz's activities with the Nazi party during the time of World War II.

One critical aspect of success in spreading one's ideas is the ability to network. This is a function of a variety of factors. We know Charles Darwin, and not Alfred Russel Wallace, as the prime mover of the theory of evolution by natural selection in part because of his network of social interactions. Darwin was of the British upper class and in England during the critical 1850s; Wallace was of the lower class and in the Pacific at the time. Comparative psychologist Margaret Floy Washburn, along with other women, were excluded from E. B. Titchener's Experimentalists meetings and thus she was unable to exchange ideas and build relationships that were open to men.

One factor in networking and gaining access within networks is sociability. The Psychological Round Table (PRT) was formed by a group of young experimental psychologists in reaction to what was perceived as the stodgy Society of Experimental Psychologists—the direct descendent of Titchener's Experimentalists group. To be invited to the PRT meetings one had to be an outstanding scientist. The ability to socialize was also important, however. "Sociability was important. In addition to the intellectual stimulation at those meetings, individuals were supposed to have a good time. If they did not seem to enjoy themselves, then they might not be invited to return" (Benjamin, 1977, p. 544). A number of comparative psychologists, including Beach and Harlow, participated; Maier did not. Benjamin concluded that "as a communication network, it has played a major role in what could be called the middle period of American psychology" (p. 549).

Two other related factors in such networking are the intellectual pedigree and place of employment. During the first part of the 20th century, the Ivy League schools were the most prestigious. For many years, the prestigious northeastern schools dominated psychology. The PRT, for example, was a northeastern organization composed primarily of faculty from universities located there. It is possible that the top schools both attract the top students and provide the best education (see Simonton, 2002). In addition,

however, individuals tended to both be more familiar with and to promote others from their same graduate institution or place of employment. Beach, for example, was quite generous in aiding students in securing jobs and writing assignments. Coming from Harlow's laboratory at Wisconsin was a badge of prestige. In addition, Harlow would not only help a graduate find a job, but donate monkeys and equipment, and invite them back for an informal summer stipend program to aid their transition to independence (Gluck, 1984); the supervisors of many other PhD graduates lacked the resources to provide such aid.

There is an interesting twist to the networking phenomenon. For some, it can be advantageous to be a newcomer—out of the established loop. Outsiders may be able to see problems from a new perspective, unfettered by established dogma, and may be able to promote innovations more effectively than insiders. This was a prime rationale for Louis Leakey in selecting women such as Jane Goodall with no formal training to do field work on the behavior of nonhuman primates.

A first step in successful research is to secure funding. I studied grants from the National Institute of Mental Health (NIMH) to comparative psychologists during 1948–1963 (Dewsbury, 2002b). A total of 117 grants were spread over 78 individual comparative psychologists. The top fund getters were Harlow, Daniel S. Lehrman, J. P. Scott, and C. P. Richter. There appears to be a reciprocal, positive-feedback relationship between success in science and funding for research. Successful scientists get more funding and well-funded scientists have more success. Comparing the top 10 fund getters to the second 10, one finds that the top 10 received approximately 2,500 citations in the *Social Science Citation Index* for 1956–1965; the next 10 received just 1,700. The difference would have been greater had not the eminent Canadian D. O. Hebb, who received less money than he would have had he been an American, been in second tier of fund getters. Five of the top 10 fund getters were elected to membership in the National Academy of Sciences of the USA (Beach, Harlow, Lehrman, Nissen, and Richter); only one from the second 10 (Hebb) was elected.

Characteristics of the Research or Idea

As noted above, perceived quality within a scientific context is an important determinant of the reception of the research or idea. Historians often neglect this in searching for the kinds of factors that I emphasize here.

One factor in gaining recognition for one's research is the species under study. The vast majority of undergraduate students who come to my office with an interest in careers in animal behavior studies want to work with either nonhuman primates or dolphins. Few, if any, want to work

with snakes or tarantulas. Humans interact differently with different animals and the species studied is an important determinant of the extent to which a study will be cited and spread into textbooks and the media. There are many important factors, as can be seen by reading the chapters in Davis and Balfour's (1992) *The Inevitable Bond: Examining Scientist-Animal Interactions*. Snakes are among the most disliked of species; there have been numerous suggestions that fear of snakes may be innate in humans. People are surprised to find that octopuses have an excellent ability to learn. We tend to favor species that are evolutionarily close to us, that have similar sensory systems to us, those with which we have much contact (e.g., dogs and cats), and young animals. We read human expression into species like dolphins. Research on the charming honeybee and Alex the talking parrot has received much publicity. If one wants to have research reach the media, including nature programs on television, one should work with primates, not rats, mice, or snakes.

A way to ensure that one's ideas rest in obscurity is to adopt a middle-of-the-road position (see also Simonton, 2002). It is extreme positions that have influence and are frequently cited. The centrist position may appear more reasonable but is easily forgotten. Macphail (1985) proposed the null hypothesis that "there are, in fact, neither quantitative nor qualitative differences among the intellects of non-human vertebrates" (p. 37). Although most comparative psychologists appear to disagree with Macphail, they cite him, in part, because he provides the logical end-point to a range of positions. Most animal behaviorists see but a limited role for group selection, the notion that individuals may sometimes forsake fitness-enhancing acts for the good of the group or species. V. C. Wynne-Edwards (1962) published an encyclopedic defense of group selection. Again, although most would disagree with his conclusions, the work has become a standard. Criticisms of comparative psychology such as Robert B. Lockard's (1971) "Reflections of the Fall of Comparative Psychology: Is There a Message for Us All?" provides another example of the influence of extreme proposals.

Characteristics of the Presentation and Promotion

The primary route to successful transmission of ideas and research is to publish them. There have been some comparative psychologists, such as University of Michigan learning psychologist John F. Shepard, who published so little of his substantial quantity of research that it has been easily forgotten.

One way in which to gain recognition and some degree of proprietary control is with a definitive synthesis that defines a new sub-field

or approach. Typically, individuals other than the synthesizer conducted much of the work. The field becomes associated, however, with the name of the individual who puts it all between two covers and provides a name and an identity. The most obvious examples are John B. Watson (1914), who took ideas that were already circulating and blended them into behaviorism, and Edward O. Wilson (1975), who did the same for sociobiology. On a smaller scale, Frank Beach had a similar skill in writing a book that defined behavioral endocrinology (Beach, 1948) and in creating seminal papers such as one that defined the study of sexual behavior in female mammals (Beach, 1976).

Another factor in establishing priority involves controlling terminology. The principle of organic selection, which provides a possible means through which traits that appear to have evolved by inheritance of acquired traits (Lamarckian inheritance) might have evolved according to Darwinian principles, was proposed by James Mark Baldwin, C. Lloyd Morgan, and H. F. Osborn at about the same time near the end of the 19th century. Today, we know it as the "Baldwin effect." Initially, the concept went under several different names. It was Baldwin, however, who wrote of the proposal in several different venues and was able to control the terminology used in discussing it. By doing this, he gained what appeared to be priority and the concept became identified with him. "He knew that controlling the terms under which an idea traveled and making others use your language identified the ideas with you" (Richards, 1987, p. 490).

Yet another factor in gaining recognition comes from the use of a catchy title. Some authors choose titles that are descriptive, detailed, and somewhat tedious. Others catch they eye with a catchy phrase. Consider, for example, "The Snark Was a Boojum" (Beach, 1950) and "Locks and Beagles" (Beach, 1969). Contrast "How an instinct is Learned" (Hailman, 1969) with "Normally Occurring Environmental and Behavioral Influences on Gene Activity: From Central Dogma to Probabilistic Epigenesis" (Gottlieb, 1998).

An effective title may not be very informative but can attract readers. The name that catches interest need not always be in the title; it is sometimes in the name of the phenomenon itself. Such phenomena as latent learning and the Coolidge effect (Dewsbury, 1981) have surely both benefitted from catchy names.

Writing style is an important determinant of impact. Authors with fluid writing styles, such as Harlow, are likely to have greater impact than those authors whose ideas may be just as important but that are expressed in more ponderous prose. The writings of comparative psychologist T. C. Schneirla provide an example of that latter. Although Schneirla's prose was

generally precise, he was "justly criticized for an obscure writing style" (Lazar, 1974, p. 178).

In general, articles with a single theme have greater impact than those in which an author tries to draw several sets of conclusions. Each may get lost in a complex article.

The ability to attract students, including postdoctoral fellows, is important in getting one's ideas and research a position of lasting prominence. Harlow supervised the doctoral dissertations of 35 graduate students many of whom went on to help the spread of his approach to research. Beach supervised the work of 41 pre- and postdoctoral students. Schneirla attracted a strong core of students who have remained aggressively loyal to his theoretical approach over the years. By contrast, Karl Lashley, one of Schneirla's mentors supervised a group of highly talented students who went in many different directions but who still bore the mark of Lashley's approach (Dewsbury, 2002a).

In order for ideas to spread there must be a receptive environment. This entails both time and place. Psychologists who have immigrated have not always found a receptive environment. Among those working in comparative psychology, examples are provided in Wolfgang Köhler, William McDougall, and Paul Schiller.

The spread of ideas is also affected by timing. The environment may be receptive to an idea at one time but not at another. In the case of Maier, discussed above, his fate probably would have ben different had he written at a later date when the field was more open to cognitive approaches. A classical case is that of Edwin B. Twitmyer, who is credited with discovering the conditioned reflex at about the same time as the better-known Ivan P. Pavlov. Coon (1982) provided several reasons for the lack of interest in Twitmyer's research. Among them, she noted that the psychologists of his time probably were unprepared to understand the significance of a mechanism so "primitive," as they were seeking knowledge of the contents of the mind. Coon also noted the inaccessible location of reports of Twitmyer's work and Twitmyer's marginal status that made him unable to risk disapproval of his work. Research on the effects of early experience on later behavior mushroomed during the 1950s and 1960s. There were many studies of effects on growth, emotionality, exploration, and learning of manipulations such as handling, shocking, and cooling of rodents and of raising them in enriched and impoverished environments. This was a time of considerable influence of Freudian thought and of intervention programs such as Head Start. Although interest in the process of behavioral development remains strong, there have been relatively few of these early experience studies in recent years.

LATER DEVELOPMENTS

My focus in this chapter has been upon the factors that affect the initial spread of results and ideas. As noted throughout this volume, ideas often go out of favor; some then return in a cyclical pattern. It would appear that an important reason that research and ideas are forgotten is that the framework that provided the rationale for the research changes. Thus, for example, in the 1940s and 1950s much research effort was devoted to testing the learning theory of Clark L. Hull. When that theory proved unworkable, many of the studies conceived for its testing lost their relevance.

Although this process occurs in all fields, it may be somewhat less significant in comparative psychology than in other parts of psychology. If so, this would be because less work in comparative psychology is theory-driven. If one is asking questions about nature, rather than testing a specific theory, the results are less likely to become irrelevant. One who is trying to describe the nest defense pattern of a gull or the copulatory behavior of a grasshopper mouse may face less risk of obscurity than one testing the implications of a complex theory. It is true that the methods used even in such studies change over time so that many classical studies need to be readdressed. However, an optimistic conclusion might be that comparative psychology has a more cumulative foundation than do more theory-driven fields.

That said, there are some examples of research that becomes more or less favored as the prevailing theoretical framework changes. An example in comparative psychology might be the disappearance from the literature of McConnell's memory transfer research when the RNA theory of memory collapsed. Work on the biochemical bases of memory has reappeared, though in very different ways from those envisaged by McConnell. Yerkes' integrative psychobiology went out of favor as more molecular approaches began to come into favor during the 1930s. Such approaches came back into favor with the influx of influence from European ethologists during the 1950s.

Studies of the evolution and adaptive significance of behavior have received at least two major boosts since World War II. The first was from the importation of the work of the European ethologists. An example can be found in the work of American biologists Charles Otis Whitman and Wallace Craig. Their work on the evolution and control of behavior was rather obscure before Konrad Lorenz found in it the precursors of his own approach and the work became widely known within the field. A second period when such studies mushroomed in prominence followed the development of ideas of kin selection and selection working at the level of the individual by W. D. Hamilton, Robert Trivers, and others. Such studies have become the most prominent in the area of animal behavior studies. To

the extent that they test the ideas of these authors, they may risk obscurity should the scientific winds later change direction. Evolutionary interpretations of human behavior have flowed in and out of favor and are currently in the ascendancy with the development of evolutionary psychology—an outgrowth of this approach.

TABLE 1. Factors (Positive or Negative) Emerging from a Study of Comparative Psychology as Important for Scientific Influence

Factor	Cases
Characteristics of the Scientist	
Academic pedigree	Maier, Beach, Harlow
Position of authority	Griffin
Perceived Authority	Sperry, Calvin, Tinbergen
Appealing persona	von Frisch, Goodall, Lorenz
Sociability	Maier, Beach, Harlow
Effective networking,	Maier, Goodall, Beach
Aggressive publication habits	Harlow
Characteristics of the Research or Idea	
Conformity with prevailing theory	Maier
Captivating appeal of idea	von Frisch
Anthropomorphic bias	Griffin
Appeal of the species	von Frisch
Extreme positions	Macphail, Wynne-Edwards, Lockard
Inertia through repetition	von Frisch
Characteristics of the Presentation and Promotion	
Sheer weight of evidence	von Frisch
Data simplification	Yerkes
Converting theory into fact	von Frisch
Ignoring evidence contrary to theory	Maier
Definitive synthesis	Watson, Wilson, Beach
Catchy title	Beach, Hailman
Catchy phrases	Harlow
Control terminology	Baldwin
Single theme	Tinbergen
Romantic rhetoric	Tinbergen
Readable writing style	Harlow, Schneirla
Introduce moderate humor	Harlow
Avoidance of over-extension	McConnell
Avoid excessive popularization	McConnell
Salesmanship	Harlow
Support from authorities	von Frisch
Attract students	Harlow, Beach, Schneirla, Lashley
Synchrony with prevailing emphases	Yerkes, Harlow, Maier, Twitmyer
Receptive location	Köhler, McDougall, Schiller
Persistence	Griffin, Maier, Wenner

Surely, the most obvious example of changing fashions in psychology during the twentieth century concerned cognitive psychology. With the so-called behaviorist revolution and the anti-instinct revolt during the first half of the century, cognitive interpretations went out of favor. With the re-emergence of cognition after the 1960s, some research that had been out of favor returned. The work of Sperry and Griffin discussed above are prime examples.

The concept of the cell assembly developed by Donald O. Hebb dwelt in relative obscurity until this time. It is often forgotten that during the supposed height of behaviorist influence during the 1930s there was in fact a very active field of animal cognition (Dewsbury, 2000). What it lacked was a coherent focus and the synthesizer to tie it together into a coherent package. A citation analysis of the work of European comparative psychologist Paul H. Schiller shows that his work was not frequently cited during the 1940s, the time of Schiller's death, but became more frequently cited during the 1970s and 1980s as the dominance of behaviorism became lessened (Dewsbury, 1994).

CONCLUSIONS

We must never forget that the successful spread of a piece of research or theory depends in part on its perceived quality within the context of the science in which it is done. That said, many other factors can affect success. These are summarized in Table 1 and include characteristics of the scientist, the research or idea, and of its presentation and promotion. Scientific acumen is important but so is good salesmanship.

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7

THE PSYCHOLOGY OF *HOMO SAPIENS*: CHANGING COMPARATIVE PERSPECTIVES

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Are we fallen angels, or are we animals? That question, in various guises, has dominated a good deal of philosophical and religious thought, and influences our everyday lives in the way we treat animals, and even the way we treat our fellow humans—when we disapprove of the way people behave, we readily resort to name-calling, with words like "brute", "bitch", "pig" or "maggot", that clearly imply our own sense of superiority. On the other hand, our physical similarity to other species, especially the other primates, is obvious, even to the point that bodily tissue can be successfully transplanted from animals to humans. Such conflicting perceptions not only influence our daily lives, they have also had a profound on the discipline of psychology itself. Academically, they derive from impeccable sources.

At one extreme lies the philosophical tradition established by Descartes (1647/1985), which holds that humans are fundamentally different from other animals. Descartes was intrigued by mechanical toys, which were popular at the time, and concluded that it should be possible, at least in principle, to make a mechanical replica of an animal, even an ape, that would be essentially indistinguishable from the living creature. Humans, on the other hand, enjoyed a freedom of action and thought, endowed by God, that could not be reduced to mechanical principles. He

was especially impressed by the fact that human language was unbounded, a quality even enjoyed by human "imbeciles" but apparently unattainable by any nonhuman animal. This led Descartes to *mind-body dualism*, the belief that the human mind possessed a nonmaterial quality that was distinct from any bodily function, and that operated through the pineal gland. That organ has failed to live up to this role, but was later replaced by the hippocampus minor, thought to be present in humans but not in the other great apes.

At the other extreme lies the tradition of continuity between humans and other animals implied by Darwin's (1859) theory of the origin of species through natural selection. Darwin did not at first spell out the implications of natural selection for human evolution, but later made the explicit suggestion that humans are descended from African apes (Darwin, 1872). Any differences between humans and apes, he thought, were differences of degree rather than of kind, and at the end of *Origin of Species* Darwin predicted that "Psychology will be based on a new foundation."

THE CARTESIAN BEGINNINGS

Scientific psychology began, however, in the Cartesian tradition of dualism. The foundation of experimental psychology is generally attributed to Wilhelm Wundt, who established the first laboratory in Leipzig, Germany in 1879. Although Darwin's theory was being widely debated at the time, Wundt was a dualist, and sought to establish a science of psychology based on *introspection*. Where the physical sciences involved looking outward to the natural world, psychological science would look inward to the subjective world. This approach seemed to preclude the study of the psychology of other species, although late in his career Wundt (1894) appeared to accept the possibility of animal introspection.

As the 19th century drew to a close there were growing doubts about the use of introspection as a scientific method, although psychology remained largely dualistic. Alfred Binet (1886), before he became interested in the measurement of intelligence, wrote a book on reasoning on the assumption that reasoning simply involved a play of images, accessible through introspection. Later, however, he came to believe that many thoughts were not comprised of images and therefore inaccessible to introspection (Binet, 1902). In 1894 Oswald Kulpe, one of Wundt's former students, established what came to be known as the Würzburg school, which also challenged the notions that thoughts could be studied through introspection. The Würzburg school established the notion of *unanschauliche Bewusstheit*,

which was later translated into English as "imageless thought" (Boring, 1929).

The British psychologist E. B. Titchener (1998) transported the method of introspection from Leipzig to Cornell University in the United States, where it came to be known as *Titchenerism* (Boring, 1969). Despite the growing influence of the Würzburg school in Germany, Titchener (1909) continued for a while to maintain that imageless thought could be reduced to fleeting sensations, but by 1910 even he had accepted that at least some forms of habitual thought were unconscious, and therefore inaccessible to introspection. Nevertheless he continued to argue for a mental discontinuity between humans and other animals. In the 1913 edition of his book *A Primer of Psychology* he wrote:

> . . . we must not lose sight of the advantage that even a little thinking gives man over the animals. There is evidence that the higher animals are, at times, actively imaginative. But it is highly significant that, although many of them have the physical means of speech, man alone has developed an articulate language, the vehicle of symbolic imagination or thought. The very fact that he can accept judgments ready made, that he can be passively attentive to groups of word ideas, is a clear indication of his mental superiority [p. 218].

THE BEHAVIORAL REVOLUTION

Nevertheless, 1913 was a fateful year for Titchener and the introspectionists, because it marked the beginning of a revolution that was to completely alter the face of psychological science. In that year, J. B. Watson published his famous article "Psychology as a behaviorist views it" in the *Psychological Review*. With behaviorism, Cartesian dualism effectively gave way to the Darwinian idea of continuity between humans and other animals. Watson put it bluntly: "The behaviorist, in his efforts to get a unitary scheme of animal response, recognizes no dividing line between man and brutes"—although one might argue that the reference to "brutes" tends to give the game away. Gone was the sense of an internal world accessible only to introspection, and in its place came the idea that psychology was an objective science, reducible to overt behavior. Moreover, psychological principles applied equally to humans and other animals, and rats and later pigeons moved into the laboratories of experimental psychology.

By the 1950s behaviorism was dominant, at least in the US and most of the English-speaking world. The dominant theorist was Clark L. Hull,

whose 1952 book *A Behavior System* seemed at the time the ultimate in theoretical sophistication. Hull saw behavior in terms of the dependences between stimuli and responses (S-R theory), with intervening hypothetical constructs that effectively represented mental events. But Hull's theories, like those of Wundt, are now largely forgotten, except perhaps in the area of motivation. The more influential theorist in the longer term was B. F. Skinner, who sought to banish all intervening constructs and reduce psychology completely to observable events (e.g., Skinner, 1938). Animals, and especially pigeons, continued to dominate Skinnerian psychology, revealing the principles of behavior that could be applied equally to human behavior. Skinner did not hesitate to pronounce on matters of importance to human psychology in books such as *Beyond Freedom and Dignity* (1973), but his most ambitious undertaking was his 1957 book, *Verbal Behavior*, in which he challenged one of the pillars of Cartesian dualism, human language.

In an appendix to *Verbal Behavior*, Skinner told how he had dined in 1934 with the British philosopher Alfred North Whitehead, and had tried to explain to Whitehead the power and elegance of behaviorism. Whitehead, feeling obliged to offer a Cartesian challenge, uttered the sentence "No black scorpion is falling upon this table," and asked Skinner to explain why he might have said that. The reply in the appendix was over 20 years in the making and has Freudian overtones, which is ironic because Skinner was known for his hostility to psychoanalysis. Skinner suggested, in effect, that the black scorpion was symbolic of behaviorism itself, and Whitehead was expressing the fear that it might take over. Skinner was not to know that another black scorpion was ready to attack behaviorism itself.

THE COGNITIVE REVOLUTION

Language

As with the fall of introspectionism, however, when the crash came it came quickly, and language was perhaps the critical issue, as it had been for Descartes. 1957 was the fateful year for behaviorism, for not only was Skinner's *Verbal Behavior* published in that year, it was also the year in which Noam Chomsky's *Syntactic Structures* appeared. This slim volume, based on Chomsky's PhD thesis, was to have much greater impact than Skinner's *Verbal Behavior*. In 1959, to drive the point home, Chomsky published his famous review of *Verbal Behavior*, and psychological theory has never been quite the same since.

Like Descartes, Chomsky stressed the open-endedness of language, and argued that it could not be reduced simply to associations. Furthermore, language was uniquely human, a clear case of discontinuity between humans and other animals. He wrote:

> The unboundedness of human speech, as an expression of limitless thought, is an entirely different matter [from animal communication], because of the freedom from stimulus control and the appropriateness to new situations . . . Modern studies of animal communication so far offer no counter-evidence to the Cartesian assumption that human language is based on an entirely different principle. Each known animal communication system either consists of a fixed number of signals, each associated with a specific range of eliciting systems or internal states, or a fixed number of 'linguistic dimensions', each associated with a nonlinguistic dimension (Chomsky, 1980, pp. 77–78).

Although a self-confessed Cartesian (e.g., Chomsky, 1966), Chomsky did not resort to non-material influences, or to God, to explain the open-endedness of language. Instead he argued that language could be explained in terms of symbols and recursive rules. And where Descartes had argued that human language could not be reduced to mechanical principles, there is one machine that might well embody the principles of human language. This is the digital computer, although it must be said that no computer has yet come close to producing or understanding language with anything like the competence of a four-year-old human child.

Another argument against the behavioral view of language was the child's seemingly miraculous ability to acquire syntax. Chomsky argued that it was impossible in principle for any device to extract the knowledge required for syntax purely from the information provided by the linguistic environment, because of what he called "the poverty of the stimulus." Syntax must therefore be an innate endowment. Chomsky (1980) went to far as to claim that the very idea of "learning" might be obsolete: "It is possible," he wrote, " that the notion 'learning' might go the way of the rising and setting of the sun" [p. 245]. Provocative remarks like this effectively challenged the efforts of over 50 years of behaviorism.

The Chomskyan notion that only humans have true language did not go completely unchallenged, and a number of psychologists have tried to show that our great-ape cousins can be taught something at least approximating human language. Early attempts to teach chimpanzees to *talk* proved fruitless (e.g., Kellogg, 1968), but at least some progress has been made in teaching great apes to communicate visually, either through a simplified version of American Sign Language (Gardner & Gardner, 1969; Miles, 1990; Patterson, 1978) or through use of a keyboard

containing symbols representing objects and actions to which the animal points (Savage-Rumbaugh, Murphy, Sevcik, Brakke, Williams & Rumbaugh, 1993). Although these studies show that great apes can construct simple requests using sequences of a few symbols, there is no evidence for anything approaching the sophistication of human language.

Pinker (1991) probably spoke for most linguists and psycholinguists when he wrote that the apes in these studies "just don't 'get it'" [p. 340]. The level of communication that they achieve has been termed "protolanguage" by Bickerton (1995), roughly equivalent to the level of language demonstrated by a two-year-old child or a Broca's aphasic, and essentially devoid of syntax. Bickerton noted that a similar level has been demonstrated in dolphins, sea lions, and an African gray parrot. He argued for what has been termed the "big bang" theory of the evolution of language: "... true language, via the emergence of syntax, was a catastrophic event, occurring within the first few generations of *Homo sapiens sapiens*" [p. 69].

An Even Bigger Bang

An extreme view of the "big bang" theory has been proposed by Crow (1993, 1998), who has suggested that there was a 'speciation event' around 170,000 years ago that gave rise to *Homo sapiens* with unique attributes of language, cerebral asymmetry, theory of mind, Machiavellian intelligence, and a predisposition to schizophrenia. He argues that the critical gene is located in homologous regions of the *X* and *Y* chromosomes, and more specifically in the *X*q21.3/*Y*p11.2 region of homology (Crow, 2002), and there is indeed evidence for changes to the human Y chromosome occurring after the human-ape split some 5 million years ago, but before the common ancestor of modern human *Y*s at around 170,000 years ago (Tyler-Smith, 2002). Yet there is reason to doubt this scenario. For example, if the gene has to do with handedness and cerebral asymmetry, then it is likely to be polymorphic (e.g., Annett, 2002; McManus, 1999), and polymorphisms are unstable on the *Y* chromosome under any selection regime (Corballis, 1997).

One might also doubt whether a single gene could be responsible for the claimed extent of discontinuity between humans and other species, including the great apes. Further, as we shall see below, there is growing evidence to suggest that cerebral and manual asymmetry is not unique to humans. In short, the argument for discontinuity may well still depend largely on a perspective derived from Cartesian dualism.

Information-Processing Theories

Although it was the nature of language that gave the main impetus to the cognitive revolution, there was more to it than that. The American author Howard Gardner (1985) has proposed that the cognitive revolution actually began in the year preceding the publication of *Syntactic Structure*. In 1956, on 11 September—a date that was much later to prove fateful for quite another reason—there was a conference at MIT that included Chomsky on syntactic structures, Herbert Simon and Allan Newell on the first complete proof of a theorem carried out by a computing machine, and George A. Miller on the magical number seven. But a quieter revolution may have been in progress well before that. British psychologists, in particular, were less caught in the tide of behaviorism, and might see the seeds of the cognitive revolution as having been sown in the work of such authors as Bartlett (1932), Craik (1943), Cherry (1953), and Broadbent (1958). These authors saw the human mind as an information-processing system that could be viewed objectively, and that later led to artificial intelligence and computer-based models of the mind. This broader view of psychological process, incorporating attention, perception, memory, motor skill, as well as language and symbolic processing, was captured for US psychologists by Ulric Neisser's influential book, *Cognitive Psychology*, published in 1967.

Neuropsychology

Along with the cognitive revolution, there was also a revival of interest in the brain, spurred in part by the publication of D. O. Hebb's book *Organization of Behavior* in 1949. This allowed some of the rats to remain in departments of psychology, and led eventually to the emergence of behavioral neuroscience. It also led to the emergence of neuropsychology, which focused on the human brain, and added a new dimension to cognitive psychology—the amalgam was later to be called cognitive neuroscience. A dominant theme of the neuropsychology of the 1960s through the 1980s was cerebral asymmetry, which seemed to lend further support to the discontinuity between humans and other animals. Unlike other animals, the great majority of humans are right-handed, and it seemed natural to link this with the use and manufacture of tools, and one of us even argued that manufacture has the open-ended, generative character of language itself (Corballis, 1991). But more critically, the left-cerebral dominance for language seemed to add further support for the uniqueness of language. Chomsky also seized on this point:

> . . . a chimpanzee is very smart and has all kinds of sensorimotor constructions (causality, representational functions, and so forth), but one thing is missing: that little part of the left hemisphere that is responsible for the very specific functions of human language (quoted in Piattelli-Palmarini, 1980, p. 182).

Evolutionary Psychology

The emergence of evolutionary psychology in the 1990s might seem to signal the arrival, at last, of the new foundation for psychology that Darwin had promised. Curiously, however, evolutionary psychology owes at least as much to the cognitive revolution as to evolutionary biology, and has if anything reinforced the idea of a discontinuity between humans and other animals. The seminal publication in the field was the 1992 volume *The Adapted Mind: Evolutionary Psychology and the Generation of Culture* (Barkow, Cosmides & Tooby, 1992), although evolutionary psychology had already been announced in the work of Tooby and Cosmides (e.g., 1989).

Evolutionary psychology was heavily influenced by Fodor's (1983) view that the human mind is composed of separate, encapsulated components that he called "modules." These modules are thought to have emerged as adaptations in the course of hominid evolution, especially during the Pleistocene when our forebears had developed a hunter-gatherer form of existence on the African savannah. According to Pinker (1994) in his book *How the Mind Works*, adaptations to the hunter-gatherer phase make up 99 percent of our evolutionary heritage. This implies that we owe very little to our primate heritage, and also very little to the first 3 or 4 million years of hominid evolution, since the Pleistocene dates from only about 1.8 million years ago.

The agenda of the evolutionary psychologists was to "reverse-engineer" the modules underlying contemporary human cognition and social behavior to the selective pressures operating during the Pleistocene. The paradigm case was that of Cosmides (1989), who showed that performance on a logical reasoning task depended on the way the task was phrased. If the task is phrased in terms of "cheater-detection" in a social setting, people performed well, but if it is framed in abstract terms the task proved much more difficult. This suggested a "cheater-detection" module that had evolved among hunter-gatherer peoples, but that was specific to certain social settings. Although language is also regarded as having evolved in the Pleistocene, and to be modular in form (e.g., Pinker, 1991), evolutionary psychologists have tended to address questions more related to interpersonal psychology, such as mate selection (Buss, 1994) or facial attractiveness (e.g., Fink & Penton-Voak, 2002).

Not all psychologists interested in evolution have adhered to the strictures of evolutionary psychology in the narrow sense defined by Tooby and Cosmides (1989). Lea (1999), for example, summarizes extensive evidence from comparative psychology that "hominid-like intelligence is not confined to hominids" [p. 34]. Byrne (2000) argues similarly in relation to other human traits commonly studied by the evolutionary psychologists:

> In reality, some of the most convincing accounts given of the likely evolutionary origin of modern human traits—patterns of infanticide and homicide, partner choice and matrimonial systems—are based on design principles that apply to apes, monkeys, and even nonprimates, not specifically to Pleistocene hunter/gatherers. Clearly, important human traits have a much greater antiquity than the Pleistocene [p. 545].

BEYOND THE COGNITIVE REVOLUTION

The cognitive revolution and ensuing developments did not mean that the battle against continuity was completely won—there are always black scorpions lurking. *The Journal of the Experimental Analysis of Behavior* survives as the vehicle for hard-line behaviorists, and some animal behaviorists regrouped under the rubric of *animal cognition*, turning the tools and concepts of cognitive psychology to the study of human learning, in some cases with the goal of showing that animals have conscious minds just as humans do. Where behaviorism seemed to dehumanize humans, the cognitive revolution may have been instrumental in humanizing animals, providing fuel for animal rights movements.

More specific impetus to the continuity view came from the molecular studies showing that humans are much closer to chimpanzees than previously suspected. As recently as the 1960s, it was commonly supposed that the common ancestor of humans and chimpanzees dated from some 20 or 30 million years ago, allowing plenty of time for physical and mental divergence. The analysis of molecular data, beginning with the work of Sarich and Wilson (1967) comparing the albumens of living primate species, forced a radical reappraisal of our similarities to other apes, and of the dates of divergence between the different species. Analysis of DNA sequences has confirmed the major conclusions of this earlier work, showing that the hominid clade diverged from that leading to modern chimpanzees and bonobos only about 5 or 6 million years ago (Cann, Stoneking, & Wilson, 1987; Waddell & Penny, 1996). By one estimate, the chimpanzee has about 99.6 percent of its amino acid sequences and 98.4 percent of its DNA nucleotide sequences in common with our own species (Goodman, 1992).

These developments may have spurred greater efforts toward demonstrating a psychological continuity between ourselves and our nearest primate relatives. Attempts to demonstrate that great apes possess the capacity for human-like language appear largely to have stalled, while other aspects of behavior and cognition have come to the fore. Some of these are reviewed below.

Theory of Mind

A quarter of a century ago, Premack and Woodruff (1978) deflected some of the attention away from language by asking the intriguing question: "Does the chimpanzee have a theory of mind?" Individuals are said to have a theory of mind if they can attribute mental states to others. A number of tests of theory of mind have been proposed, including tactical deception, mental perspective-taking, and imitation.

Tactical Deception

Deception is widespread in nature, whether in the camouflage of a butterfly wing or the uncanny ability of the lyre bird to imitate the calls of other species—including, it is said, the sound of a beer can being opened. Tactical deception, however, is that in which the deception is based on an appreciation of what the deceived animal is actually thinking, or what it can see. Whiten and Byrne (1988) collected a data base of anecdotal evidence suggesting tactical deception from primate researchers working in field settings. They screened the reports to eliminate cases in which the animals might have learned to deceive through trial and error, and concluded that only the four species of ape occasionally showed evidence of having deceived on the basis of an understanding of what the deceived animal could see or know. Even so, there were relatively few instance—only 12 from common chimpanzees and three each from bonobos, gorillas, and orangutans—so there remains some doubt as to whether tactical deception truly shows that great apes can "read the minds" of others.

Taking the Perspective of Others

A simpler question is whether great apes can understand what another individual can see. Chimpanzees naturally and easily follow the gaze of others, and human children also show this ability as early as the second year of life (Moore & Corkum, 1998; Tomasello, Hare & Agnetta, 1999). There is evidence, however, that chimpanzees do not interpret or understand pointing or eye gaze in quite the same way that even three-year-old human children do. This was illustrated by experiments carried out by Povinelli and his colleagues (see Povinelli, Bering & Giambrone, 2000, for

a summary). Chimpanzees can easily be taught to approach people they know and beg for food. If a person sits in front of a chimpanzee and points to one of two boxes to left or right, the chimpanzee understands readily enough that if it wants food, it should go to the box that the person is pointing to. But the choice breaks down if the person points from some distance away, and is systematically reversed if the person sits closer to the box that does not contain the food and points to the other one. It seems that chimpanzees respond on the basis of how close the pointing hand is to the box containing the food, and not on the basis of where the hand is actually pointing. Again, young children have little difficulty with these choices.

There is some evidence, though, that the work of Povinelli and his colleagues underestimates the social intelligence of the chimpanzee. Chimpanzees are by nature competitive creatures, and one may wonder why they should trust what humans are trying to indicate. Dogs, in contrast, have been bred to cooperate with humans, and Hare and Tomasello (1999) have shown that dogs do seem to be able to choose food sources according to where either a person or another dog is looking or pointing. Hare, Call, Agnetta, and Tomasello (2000) have also shown that chimpanzees are aware of what other *chimpanzees* can see, and modify their behavior accordingly. For example, a chimpanzee will approach food when a more dominant chimpanzee cannot see the food, but will be reluctant to do so when they can see that the dominant chimpanzee is watching.

Imitation

Another activity thought to reveal theory of mind is imitation, which implies the ability to see the world from the perspective of the individual imitated. Byrne and Russon (1998) note that the evidence as to whether animals can imitate is "disappointingly inconclusive" [p. 667]. It has been suggested that great apes are capable of "emulation", which in which the observer learns to achieve the same outcome as the observed, but without reproducing the actual behavior. Unlike human children, though, they are not capable of true "imitation", in which the observer achieves the outcome by copying the specific actions of the observed (e.g., Call & Tomasello, 1995). Byrne and Russon (1998) have argued that great apes can imitate, but only at what they call the "program level", in which subroutine structure and hierarchical layout of the imitated sequence of actions are preserved. Unlike humans, though, they seldom achieve the "action level" of imitation, in which the actions themselves are duplicated.

Mental Time Travel

There has also been some discussion as to whether nonhuman species are capable of mental time travel into the future. For example, chimpanzees

have been shown to make pointed tools from sticks at one location for use later in termite fishing at another (Goodall, 1986). Again, though, it has been argued that such behaviour is governed by present drive state rather than anticipation of a future one (Suddendorf & Corballis, 1997). Tactical deception in great apes, discussed earlier, has also been taken as evidence of action with some future benefit in mind, but others have argued that such behaviours can be explained as a response to immediate behavioural and contextual cues whose significance is learned from past experience (Tomasello & Call, 1997). Although more complex, such anticipatory behaviours may be no more indicative of an awareness of the future than the conditioned salivation of a dog to a bell signalling the arrival of food. As Atance and O'Neill (2002, p. 537) state, "At present, the consensus appears to be that primates are solely present-oriented."

Laterality

The idea that population-level handedness and cerebral asymmetry are unique to humans has also been challenged. Finch (1941) claimed that there was no systematic population-level right handedness in chimpanzees, but Hopkins and his colleagues have recently shown a right-hand preference among captive chimpanzees for some activities, including bimanual feeding, as in extracting peanut butter with one hand from a tube held in the other (Hopkins, 1996). In both cases, the ratio of right- to left-handers appears to be only about 2:1, whereas in humans the ratio is about 9:1. In an extensive review of evidence, McGrew and Marchant (1997) are nevertheless skeptical of most claims of species-level biases in handedness in nonhuman primates, and conclude by stating that "only chimpanzees show signs of a population bias . . . to the right, but only in captivity and only incompletely [p. 201]." In a more recent study of handedness in the chimpanzees of the Mahale Mountains in Tanzania, McGrew and Marchant (2001) again report the absence of any population bias, and suggest that findings of weak right handedness in captive chimpanzees "may be inadvertently shaped by the routine acts of the humans" [p. 355]. To complicate matters further, Corp and Byrne (in press) have recorded handedness in chimpanzees in the wild at Mahale, Tanzania while they process fruit, and report that most males are left-handed and most females are right handed. This suggests the present-day sex difference in human handedness, with a slightly higher frequency of left-handedness in men than in women, may derive from this asymmetry, but that a strong right shift (Annett, 2002) has been superimposed upon it.

There is also evidence that cerebral asymmetry for species-specific vocal calls is present in other species, including the frogs (Bauer, 1993), passerine birds (Nottebohm, 1977), mice (Ehert, 1987), rats (Fitch, Brown,

O'Connor & Tallal, 1993), and marmosets (Rogers, 2000). Rhesus monkeys (Hauser & Anderson, 1994) and Japanese macaques (Heffner & Heffner, 1984) show a right-sided advantage in the perception of species-specific vocalizations, suggesting a left-cerebral specialization that may be associated with left-cerebral dominance for the production of these sounds. In the majority of humans, the temporal planum, which is associated with language comprehension in humans, is larger on the left than on the right (Foundas, Leonard & Heilman, 1995; Geschwind & Levitsky, 1968; Jäncke & Steinmetz, 1993). This asymmetry does not appear to be present in rhesus monkeys or baboons (Wada, Clarke & Hamm, 1975), but is clearly evident in chimpanzees (Gannon, Holloway, Broadfield & Braun, 1998; Hopkins, Marino, Rilling & McGregor, 1998).

One might argue from this welter of evidence that cerebral asymmetry is not unique to humans after all. Yet these asymmetries may derive from a left-hemispheric dominance of vocal control, going back perhaps 170 million years to the origins of the vocal cords themselves (Bauer, 1993). Cortical asymmetries may have been driven by this asymmetry. For example, human language may have evolved initially as a system of manual gestures, but may have then become lateralized to the left hemisphere as vocal elements were introduced, so that language itself became predominantly left-hemispheric. This might explain why right-handedness evolved, and why there is an association between right-handedness and the left-cerebral control of speech (Corballis, in press). If this scenario is correct, then this pattern of asymmetry may be considered unique to humans after all.

Summary

The evidence reviewed in this section reveals that the question of continuity vs. discontinuity is alive on several fronts, but the evidence remains inconclusive. Perhaps the majority view is that, in terms of cognitive abilities, our nearest relatives the chimpanzee and bonobo remain roughly on a par with a two-year-old human child. If that is so, then some of the distinctive characteristics of human cognition, such as language, theory of mind, and reasoning, remain out of reach of the great apes.

CONCLUSION

For much of its history, scientific psychology was dominated by one or other view of the relation of humans to other animals. In the beginning, a Cartesian discontinuity prevailed. This was followed by the Darwinian continuity of behaviorism, which was in turn overthrown by the neo-Cartesian emphases of the cognitive revolution. Throughout these events

the issue was seldom explicitly addressed, and the fluctuations in underlying philosophy were driven as much by questions of method as by the question of whether humans are unique. For example, the collapse of dualistic introspectionism and the emergence of behaviorism arose because of dissatisfaction with introspection as a method, and a desire to align psychology with the other sciences that are based on objective reality. The subjective mind, in effect, was banished. The cognitive revolution effectively restored the mind, but preserved the view that the mind could be studied only through objective methods.

In more recent times, the issue of continuity vs. discontinuity has become more explicit, perhaps because of the molecular evidence showing our close relation to the great apes. In molecular terms, humans are actually closer to chimpanzees than chimpanzees are to gorillas. Such facts seem to pose an explicit challenge to the notion, still dominant in cognitive science, that there is a profound discontinuity of mind between ourselves and our nearest relatives. And even without the intellectual backing of such figures as Descartes and Darwin, intuitive arguments for continuity and discontinuity remain compelling. On the one hand, we feel as though we are different from other species, even apes, and the way we have dominated the earth with our distinctive artefacts leads to a powerful sense that we are vastly superior to any other living creature—closer indeed to angels than to apes. On the other hand, the physical evidence points overwhelmingly to our primate heritage, and looking into the eyes of a chimpanzee inevitably evokes a sense of fellow feeling. The problem is that the two views are starkly opposed, leading to schisms in the interpretation of the behavior and capacities of animals, and in particular of great apes, that go beyond the data given. This schism is, as it were, a monkey on the back of comparative psychologists trying to come to terms with our primate heritage.

In his Introduction to a recent issue of *Cognitive Science* devoted to primate cognition, Tomasello (2000) wrote:

> . . . human cognition is a specific (in the literal meaning of the word) instance of primate cognition, and evolution by means of natural selection is a mostly conservative process that preserves adaptations for as long as they work. Human cognition is thus not just similar to nonhuman primate cognition, it is identical in many of its structures. The study of nonhuman primate cognition should therefore play a more important role in cognitive science than is currently the case [p. 351].

That says it for Darwinian continuity, but it did not take long for the opposite view to emerge from other researchers in primate cognition. Povinelli and Bering (2002) decry the overzealous attempts "to dismantle arguments

of human uniqueness". They nevertheless reaffirm the importance of comparative psychology: "A true comparative science of animal minds . . . will recognize the complex diversity of the animal kingdom, and will thus view Homo sapiens as one more species with a unique set of adaptive skills crying out to be identified and understood" [p. 115].

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Part 3

Cognitive Science and Consciousness

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8

HOW OTTO SELZ BECAME A FORERUNNER OF THE COGNITIVE REVOLUTION

Pieter J. van Strien, Erik Faas[1]

> Probably the major turning point in the history of thinking came with the work of Otto Selz.
>
> (Mandler & Mandler, 1964, p. 223)

> And then we discovered that we needed something else that was anticipated by psychologists—we needed directed associations. [...] It takes a little bit of hindsight, but you can go back now and read Selz [...], who was one of the Würzburg group, [...] and translate him into our modern information processing formulas very directly.
>
> (Herbert A. Simon in Baars, 1986, p. 365–66)

Otto Selz is not remembered as one of the *great men* in the history of psychology. Though his name can be found in Boring and Watson's list of 538 important psychologists since 1600 (Watson, 1977), neither Boring's own *History of experimental psychology* (1950) nor Watson's (1978) *Great Psychologists* spend even a footnote on him. The same applies for most

[1] The authors wish to thank Theo Herrmann, professor emeritus of Mannheim University, Dr. Alexandre Métraux, head of the Selz Archives at Mannheim, and Prof. Michel ter Hark of Groningen University for their comments and helpful information, respectively about Otto Selz and the intellectual debt of Karl Popper to Selz, and, finally, Thomas C. Dalton for his encouragement and painstaking editorial work.

other histories of psychology, not only those written by Americans, but also those of German origin. Yet, some prominent representatives of modern psychology, particularly in the cognitive area, assign an epoch-making importance to his work. And to the philosopher Karl Popper Selz was one of the few psychologists whose work he deemed worthwhile. There are few other psychologists whose name has been subjected to the fickleness of fame to the same degree.

Who was Otto Selz, and what was his principal contribution to psychology? In the first section of this chapter we shall present a brief overview of his career, that untimely was disrupted by his exile and tragic end as a victim of the holocaust. The second section will be devoted to a synopsis of his work, and to the intriguing question why his ideas found only a moderate response in his own time. The meager reception of his work will appear to be due both to an unfavorable scientific climate and to personal factors. During his life-time, as is shown in section three, his influence (apart from one study in musical creativity) was practically confined to the educational domain. It was only after his death that the seed he had sowed began to germinate, not only in the field of psychology, but in an unnoticed way also in philosophy. The fourth section deals with the latter: Selz's hidden influence on Karl Poppers critical rationalism. The fifth section recounts the rediscovery of his work by the "cognitivists", and its mediation by Adriaan de Groot's study of *Thought and Choice in Chess*. In the final section the fate of Selz is used to discuss the vicissitudes of influence and fame. It will be shown that "greatness" in science (as in other domains of culture) is only partly a matter of the personal genius of an individual, but principally a matter of the reception of his or her work, and the degree to which it becomes instrumental in the development of a new paradigm—in some cases only after several decades. In this transmission personal contacts play a much larger role than is often assumed. Apart from this, the function of "great men" in providing scientists with a respectable pedigree should not be underestimated.

OTTO SELZ, A BRIEF BIOGRAPHY[2]

Otto Selz was born in Munich on February 14, 1881 as the son of a small Jewish banker. Though philosophy was his primary interest, he complied with his fathers wish and enrolled as a law student at the University of Munich, and completed this study with high honors in 1907. In the

[2] Based on the following sources: Seebohm (1970), de Groot (1973), Groffmann (1981), Koob (1981), Reinert (1981), Métraux & Herrmann (1991), Métraux (1993), and Beckmann (2001).

meantime he widely read in philosophy and psychology, which enabled him only two years later to take his degree in philosophy under Theodor Lipps *maxima cum laude*, with psychology as the main subject. He subsequently went to Bonn, where he started experimental research on thought processes at the institute of Oswald Külpe, who just had exchanged his chair at Würzburg for a professorate at Bonn University. He found there also *Privatdozent* Karl Bühler, one of Külpe's most promising pupils, who had followed his master to Bonn. Selz himself was admitted as *Privatdozent* in Philosophy at Bonn in 1912. He never practiced law, and in fact bitterly regretted the lost years all his life. He was gauche in his social contacts and remained single all his life.

The first results of Selz's experimental work were published in 1913 in a book *Über die Gesetze des geordneten Denkverlaufs* (On the laws of ordered thinking), an investigation in which the introspective method developed in the Würzburg School was applied to thought processes. Following the "Leipzig" experimental model that was current in his days (viz. Danziger, 1990, van Strien, 2004), he worked with a limited number of highly trained subjects. Among them were Külpe and Bühler, to name only the most prominent. But just as the other Külpe-disciples, and unlike Wundt, Selz asked not for simple reactions, but for a detailed retrospective report of the way the problem posed to the subject was tackled. The protocols sometimes ran to almost a full page.

Selz's further study of the laws of thinking was interrupted by the First World War. He served at the West-front, where he moved up to the rank of a sergeant-major, and after having been wounded, was decorated with the Iron Cross in 1917. When he had become *Privatdozent* it was suggested to Selz that he undergo baptism, because that would it make easier for him as a Jew to be offered a university chair, but he considered such an hypocritical act to be demeaning and incongruous with his self respect.

After the war—an irony of fate—he obtained a lectureship in the philosophy of law at Bonn, and in 1921 his appointment as *professor extraordinary* followed. However, his heart remained with psychology, and in 1922, delayed by the war, the second half of his magnum opus on the laws of thinking appeared: *Über die Gesetze des geordneten Denkverlaufs II: Zur Psychology des produktiven Denkens und des Irrtums* (On the laws of orderly thinking II: the psychology of productive thinking and of error). Nearly half of the book was again taken up by experimental reports.

In the next year, 1923, his labor was rewarded, and he was appointed full professor of Philosophy, Psychology and Pedagogy at the *Handelshochschule* at Mannheim. Being a commercial academy, Mannheim had a practical orientation. Selz took advantage of this orientation by engaging a number of teachers in a research project in which his ideas on productive

thinking were applied to school children to raise their level of intellectual achievement. The results were published partly by his students, partly by Selz himself. Selz also engaged himself several times in the practical role of a court-appointed forensic expert in assessing the reliability of juvenile testimony in cases of sexual abuse (Selz, 1928a).

In 1929 Mannheim was upgraded to the level of a university with the right to award doctoral diplomas, and in the same year Selz was unanimously elected as its first rector. The pedagogue Heinrich Kindler (1929) was the first to be awarded the doctoral degree under Selz. For Selz's most favorite disciple, Julius Bahle, who had applied his master's ideas on productive thinking to musical composition,[3] the new regulation came too late: he had to resort to Selz's colleague Karl Marbe at Würzburg, another pupil of Külpe, because his dissertation (though published only in 1930) was already completed in 1928. Selz's personal interest also widened in this period to include phenomena of scientific and artistic creativity.

The ten years between 1923 and 1933 must have been relatively the most happy decade in Selz's life. His investigations on fostering intellectual achievement earned him some fame in educational circles. The Dutch pedagogue Philip Kohnstamm visited him at Mannheim, and stimulated his pupils at Amsterdam to do research in the same line, which led to a number of doctoral dissertations. From Amsterdam Selz's influence spread to educationalists elsewhere in the Dutch speaking language area, to the effect that his theories were also applied in studies of Belgian and South-African authors. The French *Journal de Psychologie* published a lecture given by Selz for the Kant society (Selz, 1929), and there was interest for his work even in China. He had gained sufficient renown to be eligible for a call from a more famous university, the dream of every German *Ordinarius*. On the other hand, his ideas on productive thinking did not find the acclaim Selz had hoped for. He got some positive reviews, but the ideas of the *Gestalt* School, that was in its heyday at that time, got a much wider recognition, and at various occasions Selz felt that his ideas were used by them without giving him due credit.

In 1933 his career came to a sudden end with the Nazi's rise to power. On the basis of the new racial laws he was sent on indefinite leave of absence and denied access to his institute and its laboratory. In the seclusion of his private library he started writing a book on "the structural principles of the phenomenal world"—a second *opus magnum*, meant to offer

[3] In America Bahle's approach was applied to poets and painters by Catherine Patrick (1937, 1935), but instead of Selz's ideas on productive thinking she took Wallas's four-stage theory of creativity as her theoretical framework.

a definite answer to both the Berlin gestalt psychology and the wholistic *Ganzheitspsychologie* of Felix Krüger and his new Leipzig school. But lacking the feedback of colleagues the project remained unfinished.[4] German scholars came under suspicion when they cited the work of Jews, and only some preliminary work was published abroad and partly posthumously (Selz, 1936, 1941a, 1941b, 1949). When Bahle published in 1936 a new book on musical creativity in the line of his dissertation, he could mention Selz's name only briefly in the preface. As he intimates in the foreword to the second edition (Bahle, 1947), even this brought him in trouble.

In 1938, in the wake of the *Kristallnacht*, Selz was arrested and detained for five weeks in concentration camp Dachau. He was released under the condition that he would leave the country. He sought refuge in the Netherlands, where Géza Révész and Philip Kohnstamm involved him in the work of the psychological laboratory and of the Amsterdam Teachers Seminar (the *Nutsseminarium*), and mediated in the provision of a modest financial allowance. After the German invasion in May 1940 Selz wrote letters to his colleague Kurt Koffka, who in time had found a position in America, but though Koffka made various efforts on Selz's behalf nothing came of them (Beckmann, 2001). His friends at Amsterdam offered to find him a hiding place several times, but Selz said that he had done nothing wrong, and trusted that the iron cross with which he had been distinguished during the World War I would protect him. So he continued his theoretical work at his boarding address.

In spite of letters of protection written by Révész and other prominent persons, Selz was taken to the transition camp Westerbork in July 1943, and deported from there a month later to Auschwitz. His death is recorded to have taken place "near Auschwitz" on August 27. Most probably he already died from exhaustion or suffocation during transport. The last sign of life his friends received was a postcard from Westerbork telling them that he planned a series of lectures there on behalf of the other inmates.

SELZ'S PSYCHOLOGY OF THINKING AND ITS RECEPTION AMONG HIS CONTEMPORARIES

What was so special about Selz's work that it was seen as an anticipation of cognitive psychology later on? And why did it receive only a moderate acclaim among his contemporaries rather than being acknowledged by them as a major breakthrough? Doing justice to the intricacies of

[4] In the Selz Archives at Mannheim several hundreds of hardly readable manuscript pages are preserved.

his rather elaborate theories would require a chapter, if not a book, of its own, and so we must confine ourselves to the most essential.

When at the turn of the century psychologists started investigating higher mental processes, such as thinking, they were confronted with the question how to explain the *directedness* in the search for a solution. Classical associationism could offer no answer because it held that thought was controlled solely by previous associative bonds. Georg Elias Müller, head of the Göttingen psychological laboratory, the second in rank after Leipzig, did acknowledge the role of the task itself in activating the "constellation" of associations that were relevant for the solution, but only as an auxiliary hypothesis that did not change the premises of associationism. The first real threat to associationism came from experiments of Ach, Messer and Watt, pupils of Külpe at Würzburg, in which subjects were asked to retrospectively report their thought processes while fulfilling a mental task. It appeared from these reports that thinking has a fundamentally goal-directed character.

The idea that mental life is not governed by diffuse reproductions, based on the strength of previous associations, but by a *determinierender Tendenz,* was taken up and further elaborated by Otto Selz in a way that took him beyond the Würzburgers (Herrmann & Katz, 2000). On the basis of meticulous experiments, in which subjects were asked to report on the train of thoughts that was set into motion by the task (e.g. find a co-ordinate to hunt, find a superordinate to newspaper, or give a definition of mortgage),[5] Selz developed his concept of an *anticipatory schema* that guides the problem solving process. According to his theory all goal-directed cognitive operations are regulated by such schematic anticipations of the complete solution. In the case of *reproductive* thinking we automatically actualize known methods to reach the anticipated goal. The problem solving process is monitored here by a reflexoidal control process. Accordingly, when an error occurs we try anew, just as in the case of a motor task such as bowling. In the case of *productive* thinking we must abstract from our cognitive repertoire the means that lead to the goal. In other words: we must find a yet unknown method for attaining a known goal. In some cases we stumble upon the viable means by coincidence, as has been the case in many famous scientific discoveries (Selz gives here the example of Benjamin Franklin, whose reawakened memory of having flown a kite led him to his proof of the electric charge of thunderclouds). Newly discovered means are added to the problem-solving stock of our culture and help us in the biological adaptation to our environment. According to Selz this is also the key to a true understanding of creative innovations. In the

[5] Other examples of tasks set by Selz were: Whole? Part? Subordinate? Description?

creativity-studies in which he engaged himself during the last twelve years of his life, Selz (1931, Selz, 1940) made advances in the direction of a *psychology of science* that takes account of the personality behind creative contributions (viz. van Strien, 1997).

It is not very surprising that the associationists would not agree with Selz's theory of thinking. But it is confounding why his ideas failed to receive support from Gestalt psychologists, who were equally vehement in combating associationism in this very period. And that the other Külpe-pupils, Karl Bühler aside, adopted an attitude of aloofness from the disputes in which Selz engaged himself. In accounting for the meager reception of his work, we have to take various factors into consideration: factors of "scientific climate" (A) but also personal factors (B).

(A) In Western-Europe the period after World War I was characterized by a shift from elementary thinking to wholistic, organismic thinking. The Gestalt psychologists owed a great deal of their popularity to their principle that the whole is more than the sum of the parts. Selz, however, contrary to the *Zeitgeist*, kept speaking of the reflexoidal, mechanistic operations of the mind. This led Gestalt psychologist Wilhelm Benary (1923) in a review of Selz's second book to call Selz's theory a machine-theory. And when both Bühler (1926) and Selz (1926) reproached Gestalt psychologist Kurt Koffka (1925) for not having given due credit to Selz's work in a handbook chapter on the state of the art in psychology, and even to have plagiarized Selz's ideas, Koffka (1927), dismissing Selz's theory as mechanistic, replied "how could I, when his theory is so fundamentally different!"

In fact there was a deep gulf between Selz and the gestaltists, deeper than Selz, perhaps, was aware. To the latter *gestalt* was an almost metaphysical principle, pervading the entire psychical and physical world. Selz's theory was also a gestalt-theory, as he kept emphasizing, but "structure-completion" was explained by him solely with the help of empirically corroborated nomothetic principles. And instead of the physicalism of some gestalt psychologists—Wolfgang Köhler most of all—Selz took a functionalistic-biological stance, albeit a "biology of the inner man."

Still deeper was the gulf between Selz's natural science approach and the *Geisteswissenschaftliche* psychology that surfaced after World War I, particularly in the German-speaking world. The humanities assumed here the role of model-sciences that the natural sciences had fulfilled previously. And though from our present-day stance there is a resemblance between Selz's reflexoidal functionalism and the S-R psychology of his time (e.g. Aebli, 1980, p. 43), American functional behaviorists were far from recognizing this resemblance. For them the disputes about the arbitrariness of introspection that had arisen between the functionalists and Titchener had called into question the validity of the Würzburg retrospective approach

as well. The theories of Selz—biological or not—were put on one heap with the "structuralists." Only some *middle of the road* authors, such as R.S. Woodworth (1938), took notice of his work. Selz's only notable international successes during his lifetime were in the Netherlands and, surprisingly, in China, where an overview of his psychology of thinking was requested by the *Chinese Lecture Association* in Peking. In fact, this German-language "condensed version" (Selz, 1924) became his most cited work later on.

(B) Even if Selz would have had better opportunities for promoting his work, it is doubtful whether he would have used them. He was the prototype of the old-fashioned scholar, who was committed to the principle that the author should not speak for his work, but that the work should speak for itself. He lived like a monk, fully devoted to his studies. He had few friends and missed the charisma to warm up others for his ideas. The Gestalt psychologists' use of rhetorical techniques to promulgate their work was completely foreign to him.

Though modest in personal matters, Selz could be very vehement when it came to fighting over a scientific controversy. Hardly established in Bonn as a young lecturer, he published a fierce attack on the conception of volition advanced by Narziss Ach, a senior member of the Würzburg School, whom he would have been better advised to win as an ally (Selz , 1910). A few years later he wrote (as an appendix to Selz, 1913) a devastating critique of G.E. Müller's constellation theory, which led to a polemic that, interrupted by the war, dragged on until 1920. Selz was convinced that he had achieved a major breakthrough in psychology, and when Kurt Koffka (1925), in the handbook chapter we cited already, claimed that the "new psychology" that had come about was the merit of Gestalt psychology, and that Selz had only done some preparatory groundwork, Selz hastened to put the record straight, and to vindicate his priority (Selz, 1926). Karl Bühler once remarked to him: "where you have hacked about, grass will never grow again" (see Seebohm, 1970, Appendix, p. 25). Though Selz never had made use of his license as a lawyer, his style of argumentation was that of a prosecutor in front of the bar. No doubt his harsh and pedantic polemics has put off potential allies. Only after his dismissal, he grew out of sheer necessity more resigned to the fact that he would not be given credit for his ideas. For example, when Selz found that Gestalt psychologist Karl Duncker's (1935) book on problem solving contained hardly more than a watered-down version of his own ideas, he wrote to his confidant Julius Bahle that he could not afford another polemic (Seebohm 1970, Appendix p. 18). And when Bahle apologized to Selz for not citing him in his book on musical creativity, Selz reassured him by saying "[. . .] it is quite immaterial whether my work remains linked to my name; all

that matters is that my lifework itself should survive" (Seebohm, 1970, Appendix p. 24).

Not only Selz's fierce style of debating but also his written usage put off his readers. He wrote, as Frijda & de Groot (1981, p. vii) state, a "painstakingly precise but tortuous and slow-moving German that, even by the standards of the time, was forbidding." His biographer Hans-Bernhard Seebohm (1981) calls his style "nothing short of torture for his readers." The result was that only those who were convinced of the value of his ideas beforehand were prepared to go through this torture.

In his biography of Selz Seebohm (1970, see also Seebohm, 1981) ventures a psychological explanation of Selz's personal style, in which his relationship to his father plays a crucial role. In fact, however, the historical data are too scarce for a well-founded *psychobiographical* analysis of his work. Yet we are pretty safe in supposing that, even if Jewish scholars would not have been cut off from German publication channels and from being cited after 1933, and even if Selz would have received a call from a more prestigious university, he would not easily have gained international renown, or at least have attained a prominent place in the history of German psychology. For this he would have needed a greater personal charisma and the enthusiasm of one or more influential disciples. Now he won this fame only posthumously.

PARADIGM LOST: THE "MANNHEIM SCHOOL"

Yet Selz did have several enthusiastic disciples at home and various followers abroad. But it was not his basic research in the line of the Würzburg paradigm that kindled the flame, but the practical application of his theory on educational problems. Though our biographical sketch easily could leave the impression that Selz was a somewhat other-worldly man, we must not under-estimate his ability to successfully apply his theories. A first opportunity to prove this ability came in the last year of the war. After having been wounded at the front, he was assigned to investigate the causes of aircraft accidents. Instead of assessing fitness as an airman on the basis of a test battery, as others did, he set out to identify the personal and intellectual conditions of safe flying on the basis of accident protocols. As in his problem solving experiments, he found that the proper dealing with emergency situations could be enhanced by building a semi-automatic reaction repertoire (Selz, 1919).

At Mannheim the presence of a number of elementary school teachers among his doctoral students gave Selz the opportunity to start a new line of applied research based on the same principles. In the course of a series of

experiments in school-classes (Andrae, 1929, Bauer, 1928, Herrmann, 1928, Kindler, 1929, Körber, 1928, Sand, 1930, and Baumann, published by Selz, 1928b) a program was created that consisted of the following steps: (A) analyzing the intellectual operations applied by the pupils in executing a specific task (such as defining words, subtraction or addition, comprehensive reading), (B) alerting the pupils to their problem solving habits and (C) getting the more advanced pupils to transfer their problem-solving methods to their less advanced classmates. To verify whether this transfer actually had the intended effect, a control class was formed in some experiments in which the third step was omitted. The aim of this "paradigm of practice" was to develop a method for raising the intellectual achievements of pupils.

Selz saw to it that the dissertations that resulted from this program were published (see Selz, 1932), and wrote a summarizing article, that (with dire consequences for one of the editors) was published after his dismissal (Selz, 1935; Frijda & de Groot, 1981, p. 261). From the introductory section it appears that his ambition was much more far-reaching than contributing to educational reform, but involved nothing less than bridging the gap between the differential and the nomothetic (*gesetzwissenschaftliche*) approach to intellectual functioning. His conception of intelligence as "a structure or system of cognitive operative modes," as explicated above, served as this bridge (see for a further discussion, de Groot, 1981a, 1981b, 1982).

In Germany the dissemination of the new approach was blocked by the ban on Jewish authors but in The Netherlands his ideas found fertile soil. As a small country, The Netherlands has always been open to influences from abroad, and reading French, English and German was part of general education. Though American ideas on school reform found some following in educational circles too, German pedagogical thinking generally was felt to be more congenial to The Netherlands' educational principles and experience. And so the leading Dutch pedagogues took notice of the experiments of Selz and his "Mannheim School" immediately on publication, and discussed them in the Dutch journal *Paedagogische Studiën.* Particularly Philip Kohnstamm, professor of pedagogy at the University of Amsterdam, became a convinced adherent and personal acquaintance of Selz. This gave Selz the courage to apply to him for help when he was expelled from Germany.

In the late 1920's a heated debate between pedagogues and psychologists had developed in the Netherlands around the value of IQ-scores for the admission to secondary education (see Deen, 1969). In this debate Selz's ideas about fostering intellectual achievements were more than welcome.

They showed that the test reflected only the *actual* level of intellectual functioning and not the *potential* level. At his institute, the *Nutsseminarium forPaedagogics*, Kohnstamm initiated a research program aimed on the one hand at developing a more appropriate selection instrument, and on the other hand at fostering intellectual achievement. In this context answering questions about texts on the basis of "silent reading" figured as an important didactical aid. The ideas of Selz, and the related ideas of Lindworsky and his pupils at Cologne, served as the theoretical basis of the program (see Van Strien, 2003a; 2003b). Among Kohnstamm's pupils were not only prominent Dutch pedagogues (M. J. Langeveld, B. G. Palland, F. W. Prins, H. W. F. Stellwag, and H. Nieuwenhuis, to name the most important), but also the South-African pedagogue B. F. Nel, who in his turn initiated several didactical experiments in his own country (see Prins, 1951).[6] In his exile Selz continued lecturing about the importance of fostering the intellectual development of children to a group of Jewish teachers (Selz, 1942/1991).

Inspired as they might have been by the ideas of Selz, none of the followers listed in this section had a sufficient international visibility to hand on the torch of his theories in wider circles. Selz's Swiss colleague Jean Piaget, who in his *Psychologie de l'intelligence* (1947/1950) paid ample attention to Selz's thought psychology, could have integrated the Mannheim enhancement-paradigm into his thinking, but instead criticized Selz for not paying due attention to the *genetic formation* of problem solving. In both the Mannheim paradigm and in Lev Vygotsky's conception of the *zone of proximal development* (see his 1934/1987, p. 209–214) the child is helped to attain a more effective level of problem solving. Given the fact that Vygotsky was aware of Selz's psychology of thinking (op. cit. p. 246–47), there would have been the chance that he had recognized the affinity, but he died of tuberculosis in 1934, the year before Selz's major article on the enhancement of intelligence appeared. Now he only criticized Selz for analyzing thinking estranged from speech.

In the Dutch-speaking world the Selz-Kohnstamm approach stood firm up to the early 1960's, but then gradually lost its paradigmatic power. Of course, the idea of fostering intellectual achievement lived on among pedagogues and psychologists, but only in the watered-down form of didactic guidelines. Selz's approach lost its paradigmatic power within ten years after his death. And the integration of intelligence theory into a

[6] A list of the most important Dutch, Belgian and S.-African publications citing Selz or elaborating upon his work can be found in van Strien (2003a), or can be obtained directly from the first author (P.J.van.Strien@PPSW.RUG.NL).

wider cognitive theory envisioned by him is still as far away as during his lifetime.

PARADIGM IN DISGUISE: POPPER'S CONJECTURES AND REFUTATIONS

It is an irony of fate, that Selz's most prominent follower was not a psychologist but a philosopher. And it was a bitter irony indeed that this philosopher: Karl Popper (1902–1994), though he held Selz in high esteem, was insufficiently aware of the debt he owed to him. The conventional wisdom holds that Popper was a younger member of the famous neo-positivistic "Vienna Circle" around Moritz Schlick, and then went his own way by putting falsification in the place of inductive verification, thus initiating a new direction in the philosophy of science. That the Vienna Circle influenced Popper's thinking is true, but in fact the roots of his philosophy lay elsewhere, and it is questionable whether Popper would have found his way without the guidance of the writings of Otto Selz.

Popper's intellectual development is well documented in the recent biography of Hacohen (2000). His formative years were marked by the turmoil after the collapse of the Habsburg monarchy. He broke off his formal education and left his father's home, to become a member of the rebellious socialist youth movement. He even was engaged in communist action for some time—soon recoiling, however, from the violent consequences of this ideology. His choices of a vocation, first as a cabinet maker, then as a teacher and youth welfare worker, were guided by his social ideals. He engaged in a lively exchange with famous intellectuals, but most of all he drew his inspiration from the various nuclei of artistic and intellectual renewal in "red Vienna." With the aim of qualifying as a teacher he became a student at the newly established progressive Teachers College. Yet he remained an outsider, until he entered the orbit of Karl Bühler, Selz's former colleague at Bonn, who was called to the newly established chair for psychology in Vienna in 1922.

As a student at the Teachers College Popper had the opportunity to take part in Bühler's lectures and seminars and to do research in the psychological institute. This led to a Ph.D. dissertation, supervised by both Bühler and Schlick, on the psychology of thought and discovery ("*Zur Methodenfrage der Denkpsychologie*", 1928). In his autobiography "*Unended Quest*," Popper (1976, p. 78) calls this (unpublished) dissertation a "hasty, last minute affair," originally intended only as a methodological introduction to his psychological work, but as matters turned out indicative of his turn to methodology. He says to have felt badly about this thesis, and to

have "never again even glanced at it". He states that one of the reasons for moving away from psychology was that he found that some of his results "had been anticipated, especially by Otto Selz." (op. cit., p. 76). In a letter to A. D. de Groot he is still more specific:

> It must have been in 1929 after I had taken my Ph.D. in 1928 that I realized that all thinking processes are problem-orientated, and that this has been discovered by Otto Selz years before. And that all attempts to solve a problem are trial-and-error-elimination processes, as has also been seen by Selz. Thus I felt that my own problem in psychology had been solved, essentially, by Selz. (Letter Karl Popper to A. D. de Groot, 1-7-1990, Archives of Dutch Psychology, ADNP, Groningen.)

Had Popper taken the trouble of glancing again at his dissertation before explaining his departure from psychology, then he would have found that Selz already featured prominently in the views expounded there (for a detailed analysis, see ter Hark, 1993 and ter Hark, 2003). It is true, that Popper's thesis suffered from an imbalance in its composition. More than half of it was devoted to the rejection of the *physicalistic* reduction of psychic phenomena, as argued by Schlick (his second mentor); the psychology of thought is presented only in the third and last section, although rather forcefully. He sides with Bühler and the "Würzburgers" in emphasizing the autonomous and active nature of thinking as a vital biological function. He even cites some crucial ideas of Selz, that anticipate his own later methodological principles:

> The Selzian concept of trying-out behavior seems to me to have striking parallels in the objective enterprise of science. In science too, theories, "models" as Bühler says), are tried out, and even in such a way that corresponds completely to the Selzian scheme. As is well-known, the actual ways of inquiry in no way correspond to the logical principles of representation; no more than the "operations" described by Selz correspond to the objective logical "operations." In spite of this the enterprise of science is in the long run evidently "task-directed" ['Aufgabengesteuert'], the determining tendencies come clearly to the fore. (Popper, 1928, p. 69, translation ter Hark, 1993, p. 587–88, slightly modified by the present authors).

In his later publications Popper categorically rejects a psychological approach to logical reasoning as a form of *psychologism*. Discovery proceeds by induction and creative leaps. What only counts now, according to Popper,—to use Reichenbach's (1938) famous distinction—is the context of justification: the formulation of falsifyable predictions. In his autobiography Popper (1976) traces the roots of his falsificationism back to his experiences as a youth with dogmatic communism, his experience of

Alfred Adler making an unfalsifyable diagnosis of one of his pupils that was unfalsifyable, and to the extraordinary power of Einstein's famous, precisely testable predictions as a good counter-example of falsifyable theorizing. But, significant as these experiences may have been, his "logic of research," in our opinion, is nothing but a stylized version of the problem solving schema that Selz had proposed years earlier (Berkson and Wettersten, 1984; ter Hark, 2003). Poppers famous conception of how science advances through a cycle of *conjectures and refutations* (Popper, 1963), too, is nothing but an epistemological translation of Selz's trying-out behavior ("probierenden Verhaltens"). As ter Hark shows, even the falsificationist imperative to search for negative instances can be found already in Selz's book of 1922 (p. 281). And Popper's conception of an objective and autonomous World 3, inhabited by scientific problems, concepts and arguments, is prefigured in the Selzian idea of a common stock of cultural achievements. Laboriously evolved by our ancestors, the scientific and artistic endeavors of later generations serve as routinely actualizable components of their problem solving and creative processes (Selz, 1924, p. 47).

Another clearly Selzian influence in Popper's work is his model of the mind as a *searchlight*, as opposed to the model of the mind as a bucket (Popper, 1948/1972). The bucket model, which Popper rejects, corresponds to the inductive approach of passive associationism. According to the searchlight model all observation is led by expectations; they form the *frame of reference* that confers meaning to our experiences, actions and observations. A clash between our hypotheses and our factual observations may force us to reconstruct our frame of reference. Popper speaks here clearly the language of a psychologist. While repudiating one form of psychologism he embraces a new form of it (ter Hark speaks of psychologism$_2$). Would he have been less frenetic in trying to steer clear of psychology, then his elaboration of the Selzian approach to scientific production, as Berkson and Wettersten (1984) show, could have become a major contribution to the cognitive psychology of science (see also van Strien, 1997).

Why, then, was Popper so keen on distancing himself from psychology and emphasizing that he was dealing with the *logic* of discovery? What happened in 1929? In looking for an explanation we must in the first place consider that young Popper's motives were primarily *pedagogical*. He wanted to reform dogmatic thinking habits and to call for an open, critical approach to the world. He saw psychology as the unruly domain of the irrational (later on he spoke of *World 2*), and logic as part of the orderly sphere of supra-individual rational thinking (*World 3)*. Popper became to view logic as the *education* of thinking, and this led him to hypostasize Selz's problem solving schema as the logical norm of scientific practice. In fact, Popper has remained an educator all his life.

A second factor is that Popper entered into a critical dialogue with members of the Vienna Circle in 1929 at a crucial time in his intellectual development. In his thesis on axiomatics in geometry, written in that year to attain the qualification for teaching mathematics and physics in lower secondary schools, Popper took the fist step toward a discussion of the methodology of science, and this gave him the courage to approach members of the circle (i. e., Victor Kraft, Herbert Feigl) on a personal basis. Feigl encouraged him to lay down his views in a book, and eventually this led to his magnum opus *Logik der Forschung* (1934), later translated into English under the title *The logic of scientific discovery* (1959). In his philosophy of science (hypothetico-decuctive) *falsification* became for Popper the device that distinguished his approach from the (inductive) verificationism of Logical Positivism. From this angle he engaged with increasing self-confidence in a debate with the Circle. It is conceivable that, to get credit in the world of philosophy, he had to play down his background in psychology, and the more so, because since the turn of the century *psychologism* was considered as a cardinal sin among philosophers (viz. Kusch, 1995). As Kusch shows, the "danger of psychologism" became in the first quarter of the 20th Century the battle cry of German-language philosophers in their struggle to regain the territory lost to the expanding experimental psychologists. And so the names of Bühler and Selz were blotted out from Popper's pedigree, and gradually also from his memory, a memory that, as both ter Hark (1993, 2002, 2003) and Popper's biographer Hacohen (2000) have shown, did not accurately portray the genesis of his methodological views. Popper remembered Selz (and Bühler) as prominent psychologists, but left them behind at the other side of the border that he had crossed.

PARADIGM REGAINED: FROM MANNHEIM TO CARNEGIE MELLON

During the 1950's an interesting change occurred in the reception of Selz's work. As we saw, The Netherlands was almost the only country in which Selz's theories were received in a positive way. As important as the educational applications reviewed above may be in understanding Selz's influence, Adriaan de Groot's experimental research, published in *Thought and Choice in Chess* (1946/1965), was more relevant in the long term, because Selz's self-reporting method was applied here for the first time in real-life problem-solving situations. Further studies on chess-playing followed (Jongman, 1968, de Groot & Gobet, 1996). Inspired by de Groot, Selz's method was also convincingly applied to the problem solving approaches of experts in specific fields (neurologists, Snoek, 1989, and

architects, Hamel, 1990). However, these studies had only a limited impact, because they were written in the Dutch language. The same applies for the few *proto-cognitivist* German-language studies that paid attention to the work of Selz (notably Graumann, 1965 and Herrmann, 1965).

Nevertheless, the cognitivist "Selz-revival" gradually expanded also into the Anglo-American world. The first introduction of the work of Selz to the English speaking scientific community came with the publication of a review of the psychology of thinking by the Oxford psychologist George Humphrey (1951).[7] In his book *Thinking*, Humphrey devotes a whole chapter to Selz and the importance of his work. In America his example was followed by Mandler & Mandler (1964).

In his autobiography, Herbert Simon—one of the founding fathers of the information processing approach—recalls that it was Humphrey's chapter, together with Adriaan de Groot's study (which he read in Dutch), that introduced Alan Newell and himself to the work of Selz. In their first article on *The Logic Theorist* Newell, Shaw and Simon (1963) mention Selz as an erroneously neglected successor to the Würzburg School. About fifteen years later they assign Selz in their *Human Problem Solving* (1972) even a much more influential role: "Our own work [...] owe[s] large debts to Selz" (p. 875).

When we consider the changing scientific and intellectual climate in The United States in the 1950's and 1960's, it is not surprising that the reception of Selz's work changed for the better. After years of domination by behaviorism, psychology witnessed in these years a revival of the interest in thinking processes and mental operations. Coinciding with the development and introduction of the first computers, two new directions emerged, involving artificial intelligence and the information processing approach that both underscored the value of productive thought. The introduction of the computer in psychology stimulated innovative minds like Newell and Simon not only to try to understand and to explain human thinking, but also to develop programs that are actually able to perform and simulate human tasks. Especially for this purpose, Selz's theory and concepts proved to be highly relevant and applicable. Once negatively considered as mechanistic and machinelike, his concepts proved now to be extremely useful. His concept of stringently unfolding thought processes was easily translatable into programs involving artificial simulations. Newell and Simon recognized that their own use of the concept "program" was rooted in Selz's concept of "operation". The relevance of Selz's ideas, once

[7] Humphrey had started his study already in the 1930's at the suggestion of his mentor, Frederic C. Bartlett, but due to the war the book appeared with a delay of more than ten years.

rediscovered, was also acknowledged in other fields, such as memory research (e.g. Kintsch (1974)).

Not only were Selz's theory and concepts influential, but his method of thinking aloud while fulfilling a particular task also proved useful. Thinking-aloud protocols were increasingly used as a basis for the construction of mathematical models of problem solving and chess-playing. Newell, Shaw & Simon (1958/1963, 1963) implemented these models in computer programs. Simon's pupil Ericsson has further extended the method of protocol analysis (Ericsson & Simon, 1984). In the last decades Artificial *Intelligence* (AI) experienced a veritable boom in the construction of expert systems. These systems were based on the problem solving procedures of top experts, that were derived by means of the thinking-aloud procedure (e. g. Kidd, 1978; McGraw & Harbinson-Briggs, 1989; Ericsson & Smith, 1991). In these recent advancements the goal is no longer the development of psychological theory, but the generation of *technology*, referred to in the specialist literature as *knowledge engineering* (e.g. Feigenbaum, 1984).

An interesting aspect of these new developments is that psychology is now used to generate *logical* inferences in a way that radically is at variance with Popper' anti-psychologism. Consequently, psychologism is no longer considered anathema, but intrinsic to the study of logical thought. Herbert Simon brought down logic from the Platonic heaven of World 3, and reinstated in its function of well-regulated thinking (see his *Models of discovery*, 1977). Instead of being considered purely a matter of creative illumination, scientific discovery now became again the subject of a *cognitive psychology of science*. The *naturalistic turn* in epistemology that took place in the past 25 years (viz. Kornblith, 1985) gives expression to the rehabilitation of psychology as the key to rational problem solving. In fact, this means a return to the route first chartered by Otto Selz.

In the 1970's and 1980's the value of Selz's theories for the study of cognition began also to dawn in the German-speaking world. Theo Herrmann (1972) draws on Selz in his psycholinguistics. Dietrich Dörner (1974) conceives the modern cybernetic information processing theory as a *renaissance* of Selz's and Duncker's psychology of thinking. Piaget-disciple Hans Aebli (1980, p. 43/44) sees in Selz's idea of control processes a precursor to the famous TOTE-schema of Miller, Galanter and Pribram and of modern performance theory. And he speaks of a "Selz-Renaissance" coming about. R. E. Schaefer even dedicated his book on *thinking, information processing, mathematical models and computer simulation* (1985) to Otto Selz, and Michael Hanke published three articles on Selz in the late 1980's and early 1990's (Hanke, 1989, 1992, 1993). In fact, however, these few swallows didn't make a new summer.

It is questionable, moreover, whether Selz, had he lived on, would have recognized himself in the approach that prevails in the information processing studies sketched in this section. The *functional-biological* stance that guided his approach to problem solving is replaced here by a much more mechanistic stance—the very "machine theory" from which he so fiercely distanced himself in his debate with the gestalt psychologists of his time! In this respect Popper held on much closer to Selz's premises.

THE VICISSITUDES OF FAME

Was Selz a great psychologist? When we take *influence* as a criterion of eminence, the threefold impact of his ideas—on education, on scientific methodology, and on cognitive psychology—Selz can be called one of the most influential psychologists of his generation. On the other hand, it must be admitted that only in the last of these three domains—cognitive psychology—Selz is recognized as a forerunner, and even there this recognition is limited to only a few but distinguished representatives. Although in the quote with which we opened our chapter Mandler and Mandler (1964), cited the work of Otto Selz as "probably the major turning point in the history of thinking," Ulric Neisser did not mention Selz at all in his famous book *Cognitive psychology* (1967). Selz's name is also conspicuously absent from Gardner's history of the cognitive revolution (Gardner, 1985). The same is the case, as we already noted, in most comprehensive histories of psychology. In Germany his theory is discussed in textbook chapters on the psychology of thinking, but mostly in a perfunctory way, and his ideas have not generated new research here. Even his interesting analyses of the creative personality and of creative thinking pointing in the direction of a psychology of science found only a moderate response. Does it nonetheless make sense to call Selz a great contributor?

From our present vantage point we can see it as Selz's merit was that he did not follow the organismic ideology of his time, as the Gestalt psychologists did, but remained analytic in his reconstruction of human problem solving processes, and thus he paved the way for Artificial Intelligence. Quoting a full-page translation into modern English of a Selz-text from 1935, de Groot (1983, p. 116) show how surprisingly *up-to-date* his ideas sound in our present-day ears. And in tracing the roots of cognitive psychology Wettersten comes to the conclusion that "it now appears that the decision to favour the Gestalt psychologists over Selz was a dubious one indeed." (Wettersten, 1992, p. 128). Should we downgrade, then, the rank of the Gestaltists in the Pantheon of psychology's history to make place for a statue of Otto Selz?

In calling Selz a forerunner of the cognitive psychology we forget that he could not foresee the developments in computer science. Selz's groundbreaking role became clear only in retrospect because Newell and Simon applied and transformed his work to fit new developments. Because these developments turned out to be very significant, Selz name got a (modest) place in the history of psychology. But even so this place never became generally recognized. This peculiar fate tempted Newell (1985, p. 396) to call Selz the "repeatedly rediscovered skeleton in cognitive psychology's closet"—a much humbler distinction than a statue!

The only conclusion we can draw in this situation is that greatness is not a characteristic or trait that is inherent to a person, as is the case with height, weight, and (to a certain degree) intelligence, but a *relational* concept. In his book on creativity in science the second author (Faas, 1998) has used Selz's career to show that creativity, rather than being a personal mental ability, is primarily a *label* attached to a scientist by his contemporaries or future generations for various reasons. How gifted a scientist may be, his or her greatness and creativity have to be acknowledged, and this requires that there be an audience who recognize the proposed ideas as valuable contributions to the field, and perhaps to the problems of the wider society.

In our second section we discussed in some detail the factors that impeded the reception of Selz's ideas during his lifetime that included: the changing *Zeitgeist*; his position at a second-rank university; the personal handicap of his hermetic style of writing and his harsh and pedantic polemics that put off potential allies; and finally, after 1933, the ban on Jewish scholars, that cut him off from German publication channels and even from being cited.

That a Jewish background as such was no obstacle for getting international recognition appears when we compare Selz's fate with that of the Jewish *Gestalt* psychologists who were affected by the same ban. Most of them had built international contacts and published in English already before the Nazi's took over, and had emigrated in time, to the effect that their school victoriously survived the purge, and eventually, in Boring's phrasing (1950, p. 600), "was dying of its success." Selz's work, to the contrary, was only occasionally cited in the international literature (Claparède, 1933, Guillaume, 1937, Vygotsky, op. cit., Woodworth, op. cit., Stevens & Bartley, 1951), and never published in English during his lifetime.

The importance of winning an audience and acquiring adherents and allies is illustrated by the way Herbert Simon staged his success (see Faas, 1998, ch. 5). He succeeded early on in securing several highly gifted and enthusiastic coworkers, presented the findings of his team at the earliest possible occasion, and was not shy in broadcasting their successes among

a wider audience. In brief: he acted like a fully-fledged P. R.-agent. Simon avowed that he took great care as an initial outsider not to provoke the established psychological community. "Rather than emphasizing the novelty of our theory and proclaiming a new school in psychology, Simon acknowledged that "we tried to show the continuity of our approach with the work of both our associationist and Gestalt predecessors" (Simon, 1991, p. 221). To strengthen his institutional position he played bridge with members of the faculty nearly every afternoon (Simon, 1980, p. 453). Looking back upon his career as a renown contributor, Simon was willing to acknowledge the inspiration he got from his German precursors, but emphasized on the other hand his own originality:

> My hunch (shared by Allen Newell) is that our concrete ideas for the design of the Logic Theorist were not much influenced by Selz or Duncker, but that we obtained considerable encouragement from knowing that there existed psychologists, outside the domain of American behaviorism, who would not be scandalized by the direction we were taking (Simon, 1981, p. 149).

We can safely say that, in combination with the preceding efforts of Humphrey and the Mandlers, it was principally Nobel Prize winner Herbert Simon's inclination to place his own work in a historical perspective, and to procure himself of a scientific pedigree, that rescued Selz from oblivion and assured him a place in the history of cognitive psychology. Selz would have lived on through the impact of his work, but only as an anonymous intellectual force, as was the case with the use Popper made of his ideas.

Acclamation as an eminent contributor is not necessarily based on the role of a forerunner, but also can occur for other historical reasons, as illustrated by Selz's fate in Germany. After a long period of "repression of the past" during the post-World War II era, efforts set in at the end of the 1960's to come to terms with the atrocities of the NAZI-regime. Otto Selz was remembered as the only psychology professor who was willfully "terminated" in a concentration camp. As a deed of reparation the German Association of Psychology awarded Selz posthumously the *Wilhelm Wundt Plakette* for his eminent achievements in psychology in 1971(viz. de Groot, 1973, Reinert, 1981).[8] And in the following year the *Institut für Psychologie und Erziehungswissenschaft* at Mannheim was renamed into *Otto Selz Institut*. It was from this institute that the studies on Selz and the reader of his work, cited above, originated, and that a beginning was made with

[8] The plaquette was placed in the Amsterdam psychological laboratory that had offered him a refuge.

the study of the Selz archives that were transferred from Amsterdam to Mannheim.

It is evident from the vicissitudes of Selz's career and belated fame that the "greatness" of a scientist depends only partly on his or her ideas, but to a large degree is determined by how others cite these ideas, amend them and develop them further and, in doing so, give due credit to their originator. And this, in turn, depends more than we usually realize on the degree to which the author succeeds in building alliances and finding an audience for his or her "message". Selz had a message, he was even possessed by it, but he only succeeded in interesting a few pedagogues with little international renown, and a few PhD students. It was only a happy coincidence that one of the latter, Adriaan de Groot, understood how to win an audience, not only for himself but also for his mentor. The *Gestalt* psychologists of Selz's own generation, to the contrary, formed a school in an early stage, founded a journal, and remained highly visible even in their exile. And Selz's "heir", Herbert Simon, also knew very well not only how to "sell" his own contribution, but also to place it in a historical tradition. Selz's other heir Karl Popper, conversely, in his effort to win the esteem of the philosophers of his day, chose to ignore the legacy of the *psychologist* Selz, and thus contributed to his (temporary) eclipse.

Not only the fate of individual psychologists, but also the fortune of new directions in the discipline depend on the degree to which their protagonists succeed in forging alliances and finding acclaim with an audience. Or, to use another metaphor, their success is measured by whether they find buyers in the market of ideas and practical services. The fate of *Geisteswissenschaftliche* psychology and characterology in Germany is a good example. As we saw already, it experienced a veritable boom in the period after World War I. The old "psychology of conscious" was labeled elementaristic and mechanistic, and thus considered unable to meet the spiritual needs of the new generation that had lived through the hell of the trenches. Selz's experimental psychology of thinking shared this fate. Only an approach that was sensitive to the irrational depths of the human person could find favor in the eyes of the champions of the new perspective. *Geisteswissenschaftliche* psychologists and characterologists succeeded in taking over the chairs of psychology in the majority of the universities, to penetrate into the editorial boards of the major journals and, after the Nazi's took over control, to get a leading role in the rapidly expanding psychological services of the German army (see Geuter, 1992). In other words: their message perfectly met the needs of a wide public, both within the discipline and within the surrounding society. Thirty years later a new generation relegated the same approach to the refuse dump of history, and the names of most protagonists of the *Geisteswissenschaftliche* approach sank

into oblivion. And even their approach as such finds no mention in most modern histories of psychology.

Historical investigations have shown that transformations in investigative style (Danziger, 1990), methodological rules (Dehue, 1991, 1995), and in basic approach in applied psychology (van Strien, 1998) can in a similar way be explained by the changing demands and preferences of audiences and market parties. To explicate these contextual forces, the first author has proposed a *relational model* of the development of science, in which the investigative activities of scientists are placed in the dynamic field of forces within the academic community and the surrounding society (van Strien, 1984, 1991). Using the Netherlands as a case, he shows how changes in the predominant approach within psychology were due to changing audiences and alliances: "... scientists who are unable to provide themselves with an audience or to hold its attention land in a vacuum, and risk eventually going 'out of business'." (van Strien, 1991, p. 363). To return to our subject: this was exactly what happened to Otto Selz. It was solely thanks to the legitimating activities of some of his "heirs," that he reappeared in the limelight of history.

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9

CONSCIOUSNESS REGAINED

THE SCIENTIFIC RESTORATION OF MIND AND BRAIN

Thomas C. Dalton, Bernard J. Baars

INTRODUCTION

This chapter critically examines the pathways through which the study of mind and consciousness entered the stream of twentieth century psychology with the disappointing consequence that the phenomenon of consciousness was concluded to be scientifically insoluble. Several philosophical, disciplinary, professional, and cultural factors converged which drove the study of consciousness out of psychology only to be embraced later by pioneering neuroscientists and psychologists who were unencumbered by behaviorist assumptions and sought new methods to explore the relation between the brain and behavior. Philanthropic and government support, professional organization and strategic leadership were critical to the revival of mind and consciousness in the 1950s as compelling if not uncontroversial objects of scientific attention.

Cybernetics and cognitive science contributed to the nascent revival of consciousness studies, but computing metaphors and mechanistic assumptions about mind, thought and language were challenged by American pragmatists and scientists who favored embodied, experiential conceptions of mind. Neurobiologists focused on how the brain works, how it develops, how it can be repaired when it is damaged and how it can be stimulated to enlarge the realm of human judgment and cognition. With

the advent of sophisticated imaging technologies, seemingly imponderable rival philosophical theories about human volition, judgment, intentionality and emotional expression can now be tested scientifically. Ironically, the field of psychology that once abandoned the study of consciousness is now engaging in a growing interdisciplinary and cultural movement to understand the mind and use the burgeoning knowledge about the brain to enhance human well-being.

COMPETING CONCEPTIONS OF MIND

The Continental Rationalists

Throughout history there has been a periodic swing between subjective and objective perspectives about mind and conscious experience that has impeded scientific advancement. Descartes separated the mind from the brain and the body from the soul in a dualism that has had a lasting legacy. Descartes asserted that while it would be possible to understand how the brain works, he believed that the mind and soul were subjective elements that could never be explained scientifically. Kant ultimately proposed a transcendental alternative in which the a priori principles of thought could be objectively understood even though this placed mind beyond the realm of human experience. Hegel rejected this dualism. He wanted science to rejoin knowledge and existence, sundered by Descartes and Kant, into a series of dialectical encounters between consciousness and nature in which mind is realized in Absolute Spirit. Unfortunately, Hegel's idealist conception of thought betrayed his willingness to impose spirit *on* nature rather than to understand how mind emerges from experience *in* nature.

Wundt and James

Even as scientific psychology was being founded in the 19th century, psychologists were caught up in these philosophical difficulties. Wilhelm Wundt, a physiologist by training, created a new field in order to study mental phenomena through introspection that were not considered accessible to scientific inquiry (Ben-David & Collins, 1966). William James' (1890) extraordinarily provocative metaphor for consciousness as a continuous stream of mental experiences, stimulated thinking about consciousness whose scientific implications remained unexamined for several decades until brain research resumed in the 1950s. James first proposed that consciousness was not a thing but a process involving volition and attention—a conception that strongly influenced his fellow pragmatist,

John Dewey. He also argued that events, which occur at the fringe or periphery of consciousness, influence perception just as pervasively as those occupying the center of vision.

But James was divided on how to construe the relationship between consciousness, mysticism and free will. James held that religious beliefs are based on ineffable experiences that can be explained through the study of subconscious processes. While James believed in the possibility of free will, he doubted that science would ever prove this to be so. Moreover, James and Lange proposed a controversial theory of emotion that seemingly reduced feeling to underlying physiological processes, thus creating tensions in James' theory of mind that undercut his premises about the efficacy of consciousness and the rational and voluntary nature of belief (See Taylor, 2002 & Barnard, 2002). Importantly, when the study of consciousness reemerged, James' ideas received renewed scientific attention and interest.

Freud

Toward the end of the 19th century other scientific thinkers—notably Pierre Janet and Sigmund Freud—began to infer unconscious processes quite freely, based on observable events such as post-hypnotic suggestion, conversion hysteria, multiple personality, slips of the tongue, motivated forgetting, and the like. Freud's insights have achieved extraordinary cultural influence (Ellenberger, 1970; Erdelyi, 1985). But Freud had curiously little impact on scientific psychology, in part because unconscious influences did not lend themselves to laboratory studies. Nevertheless, Freud inspired influential neurologists, psychologists and biologists in the mid-1950s (discussed later) to see if his concepts and theories could be used to support a scientifically based analysis of mind and consciousness.

THE REJECTION OF CONSCIOUS EXPERIENCE: BEHAVIORISM AND POSITIVISM

Behaviorism's Controversial Claims

A complete explanation for the widespread adoption and uncritical acceptance of the behaviorist paradigm in the social sciences is beyond scope of this paper. The conventional view is that behaviorists rejected 19th century psychology because it was unreliable and subjective, because it was mired in fruitless controversy, and because it was unscientific. "Consciousness", wrote John Watson in 1925, "is nothing but the soul of

theology" (Baars, 1986 p. 3). However, modern historical research has cast doubt on this view in all respects (Blumenthal, 1979; Danziger, 1979; Baars, 1986). It now appears that psychologists like Wundt used objective measures most of the time, and employed introspection only rarely. Even a cursory reading of James' great text (1890) indicates that he anticipated many "modern" empirical phenomena including the problematic relationship between thought, emotion and behavior (see Damasio (1999). Numerous important and reliable psychophysical effects were discovered in the 19th century. Many of these have been rediscovered since the passing of behaviorism, which include basic phenomena like selective attention, the capacity limits of short-term memory, mental imagery, context effects in comprehension, and the like.

It is worth pointing out that the behaviorist movement was opposed at the onset by late nineteenth century neuroanatomists and neurologists. Their important discoveries about the functional differentiation of the brain contributed to the subsequent distinction between motor, perceptual and cognitive processes that sustain modern theories of mind. But this knowledge about localized brain functions was exploited by phrenologists to contend that the neural basis of human cognitive functions and emotions eventually could be pinpointed and used to predict human behavior without reference to consciousness (Finger, 1994; 2000). Thus at the beginning of the 20th century consciousness was not viewed as a viable topic for psychology, even by those who wanted to better understand brain structures and functions, because no specific site had been discovered that lodged consciousness.

Why Behaviorism Attained Dominance

Many psychologists succumbed to the alluring prospect that behaviorism would bring about scientific unification. Behaviorism provided a common unit of analysis with the conditioned reflex and offered relatively straightforward experiments that produced replicable outcomes. Parents and educators were convinced that these methods could be applied in child rearing and schools to obtain more reliable and predictable developmental and educational results than through more traditional, "prescientific" approaches. Watson's confident predictions that behaviorism was an educational and vocational panacea seduced parents, educators and psychologists into accepting and adopting it as a cultural *fait accompli.* Behaviorism squared with the American creed of fairness and equal opportunity, even though the scientific status of its claim to enhance learning could not be fully scrutinized.

Philosophers of science and language also tried to dictate what was to be genuine psychology, which often sidelined mind. Ludwig Wittgenstein, in his various phases of development, inveighed against "mentalistic language"—the language of psychological common sense—as "a general disease of thinking" (Malcolm, 1967). In his later work he argued against the possibility of a "private language"—i.e., that people can really know themselves in any way. His fellow philosopher Gilbert Ryle presented very influential arguments against inferred mental entities, which he ridiculed as "ghosts in the machine" and "homunculi." Ryle (1949) believed that all mental inferences involved a mixing of incompatible categories, and that their use led to an infinite regress. From a modern psychological point of view, these twentieth century philosophers made strong but empirically problematic claims that are more properly left to science.

JOHN DEWEY'S ALTERNATIVE TO BEHAVIORISM: INTERACTING MINDS

The "American School" of Neurology

John Dewey, a cofounder of American pragmatism, believed that the behaviorist attempt to expunge mind and consciousness was mistaken. Dewey sought to avoid the methodological dilemmas of mind-body dualism and reductionism that caused many philosophers and psychologists to vacillate between the mental and physical—between reducing all psychological processes to conscious experience and reducing consciousness to brain functions. Dewey also stubbornly resisted the modern trends toward materialism and reductionism in science and logical formalism and epistemological realism in philosophy, which threatened to erase the naturalistic origins of mind. He adopted a psychobiological conception of mind proposed by the"American school" of neurologists led by Clarence L. Herrick, who viewed consciousness as an instrument for motor, cognitive and emotional integration (Windle, 1979). Dewey took the Darwinian position that the brain evolved in animals to mount more effective functional responses to environmental pressures. The evolutionary advantage of consciousness is that it enables the organism to discover new values by rendering explicit and in commensurate terms the physical and mental attitudes and desires that influenced past behavior and that will affect the outcome of future events. (Dalton, 2002).

Consciousness, Judgment and Value

Dewey contended, like James, that consciousness is not a thing but a process involving uncertainty and the transformation of indeterminate events into ones subject to human control. Beliefs and intentions are not about things that possess intrinsic worth or represent knowledge or truth but refer to actions performed on things that change their sequence or relationship to one another and that affect their efficacy. The capacity to shift attention between foreground and background is essential to balanced perception and judgment. This feature of consciousness makes mind contextual and dependent on the meaning and significance attributed to a situation in its entirety. Judgment grounded in sensori-motor functions is employed to detect and discriminate among qualitative and quantitative features of situations involving force, movement, duration, contrast and balance, among other elements, that affect sentient and energetic states and behavioral capabilities. Consciousness and judgment work in tandem with attitudes and emotions to enable the determination of whether changes in feelings, beliefs, behavior, intentions or meanings make a difference that have value in situations which satisfy a need or desire.

Dewey's Scientific Legacy

Only recently have psychologists and historians learned that Dewey collaborated in the 1930s with infant experimentalist Myrtle McGraw in her pioneering studies of the development of early motor processes (see Dalton and Bergenn, 1996). Dewey was testing his theory of mind in which the growth of the mind, brain and behavior is integrated through conscious experience (Dalton, 1999; Dalton & Bergenn, 1996). While McGraw's pioneering studies in early infant locomotion continue to be cited by contemporary researchers, her role in testing Dewey's conceptions of mind and consciousness have largely gone unrecognized (Dalton, 1998). In a subsequent chapter in this book, Dalton discusses in more detail their collaboration. He describes how Dewey and McGraw proposed to study brain states supporting consciousness, examines why McGraw failed to receive proper acknowledgement for her contributions to their endeavor and discusses how her case bears on the issue of prominence.

Dewey strongly believed that the phenomenon of consciousness could be better understood by focusing on experiences involving uncertainty about values and consequences whose significance and control involved cooperation among people with different minds and beliefs. These circumstances favored the suspension of belief, shared perceptions and mutual problem solving—crucial elements of his theory of inquiry. But many of

the pioneers in modern neuroscience seem to have better understood and appreciated Dewey's conception of inquiry and his interdisciplinary and collaborative conception of science than his ideas about mind and consciousness.

THE MACY CONFERENCES, 1942–1954

Dewey not only was McGraw's advisor and collaborator in her research at Babies Hospital, Columbia University throughout the 1930s, but he was also a founding trustee of the Josiah Macy, Jr. Foundation in New York from 1930–1944. The Macy Foundation funded McGraw's studies among numerous other psychobiological and medical investigations and continues to be involved in medical education today (Kast, 1937). From the mid 1930s to mid 1950s, the Macy Foundation also sponsored a series of 19 seminal interdisciplinary conferences (Fremont-Smith, 1951), several of which were pertinent to consciousness (discussed below), that included: cybernetics (1947–1953); nerve impulses (1950–1954), and problems of consciousness (1950–1954) (Rappleye, 1955).

Dewey student and protégé Lawrence K. Frank, Vice President of the Macy Foundation from 1936–1941 originated the conference idea while working for the Rockefeller Foundation where he formed an international and interdisciplinary network of child study institutes in the 1920s and 1930s (Frank, 1962). The Macy conferences cultivated multidisciplinary scientific approaches to problems involving significant public consequences—a Deweyan ideal enunciated in his famous 1927 book, *The Public and It's Problems* (Dalton, 2002). (Dewey participated in the first and longest running Macy conference on Aging from 1936–1952) The participants at these meetings challenged conventional ideas about mind and intelligence. Freud's ideas also were debated and the formative influence of Dewey's interactionist conceptions of mind and communication were apparent. Some of the participants carried forward these Freudian and Deweyan intellectual agendas about mind and consciousness into new scientific venues in subsequent decades (Heims, 1991, pp. 169–170).

Cybernetics: A Counterrevolution?

An extraordinary group of individuals from many disciplines participated in the conferences on cybernetics that included mathematician Norbert Weiner, physicist Warren McCulloch, anthropoligists Margaret Mead and Gregory Bateson, Lawrence K. Frank and Lawrence Kubie, a psychoanalyst (von Forester, 1949). Arturo Rosenblueth, a physiologist,

introduced the concept of "feedback mechanisms" to capture the circular causality involved in goal-directed behavior, whose attainment requires feedback and error correction. This recurring theme in the cybernetics meetings marked a significant departure from the tenets of classical behaviorism in which human behavior was controlled by underlying reflex mechanisms (see Heims, 1991). Although outspoken critics of behaviorism and psychoanalysis, McCulloch and Walter Pitts, an MIT mathematician advanced a mechanistic conception of mind that relegated consciousness to a secondary status and reduced thought to computational principles that could be mimicked by machines.

McCulloch's (1948) belief that neurobiological conceptions of mind would be replaced by ones based on physics discouraged the possibility of creative collaboration which doomed the future of cybernetics (see Dupuy, 1994). Moreover, at the Hixon symposium neurophysiologist Karl Lashley (1948) challenged McCulloch's assumption that programmed switching mechanisms can simulate the exchange of information in the brain, because the cortex sustains a complex level of interactions even in the absence of stimulation or functional response. While unable to build a machine that could think, the cybernetics engineers and mathematicians that included John von Neumann and Norbert Wiener (1948) formulated communication and information network theories that ultimately provided the foundation for the computing revolution. In fact, J. C. R. Licklider, a Harvard psychologist, an expert in psychoacoutics and language and a participant in the Macy meetings on the problems of consciousness (see Licklider, 1950), eventually headed the Information Processing Techniques Directory of the Defense Advanced Research Project Agency. Through his leadership the first electronically based communication system was created that led to the development of the Internet! (Norberg, 1988).

The conference series on cybernetics was the only Macy sponsored event to receive press coverage in *Time* and *Life Magazine*. Oliver Sacks, a famous neurologist (1994, p. 101) recalled his excitement as a boy "reading about [McCulloch and Weiner's] pioneering explorations of logical automata and nerve nets." Sacks noted that he "thought, as many of us did, that we were on the verge of a computer translation, perception, cognition; a brave new world in which ever more powerful computers would be able to mimic, and even take over, the chief functions of brain and mind" (p. 101). Pioneers in the fledgling field of cognitive science that included Herbert Simon, Alan Newell, Marvin Minsky and George Miller believed that human cognition could be better understood if strongly rooted in cybernetics inspired theories of information processing. Their earliest attempts in the 1950s to develop cognitive science were counterrevoutionary, according to Miller (2003), because they framed their theories within a behavioral

discourse that they eventually repudiated. Herbert Simon acknowledged the strategic importance of Otto Selz, who avanced a new theory of thinking that reconciled rather than repudiated competing perspectives. As van Strien and Fass note in their chapter in this volume, Simon recalled that "we obtained considerable encouragement from knowing that there existed psychologists, outside the domain of American behaviorism, who would not be scandalized by the direction we were taking" (Simon, 1981, p. 149).

Nerve Impulses

The Macy Foundation also sponsored five annual conferences on "nerve impulse" from 1950 to 1954 (Fremont-Smith, 1950). The conferences dealt with issues pertinent to understanding the molecular and biochemical basis, energetic dynamics and functional nature of synaptic and neurochemical processes of the nervous system and brain. New electrophysiological techniques were emerging pioneered by neurophysiologists Hans Berger, Herbert Jaspers, Wilder Penfield, which enabled scientists to measure more precisely than before nerve conduction involving excitation and inhibition. This accumulating knowledge enabled participants to propose well-grounded theories about the role of motorneurons in the brain stem and their relationship to reflex behavior, the corticospinal system and higher centers. These meetings reflected a new sense of purpose among scientists whose interest in mind and consciousness had been long discouraged. Nevertheless, their discussions did not reach the threshold of cognitive functions considered fundamental to mind and consciousness, such as memory, perception and judgment.

Problems of Consciousness

Perhaps the most provocative but ultimately, disappointing conference series sponsored by Macy was on "problems of consciousness" from 1950–1954. Chaired by David Wright, a Rhode Island psychiatrist, psychoanalysts dominated the group with such dignitaries as Gregory Zilboorg, Lewis Wolberg, David Rapaport, Roy Grinker and Frieda Fromm-Reichmann, among several others. However, there were also several physiologists that included Hudson Hoagland, Abramson, Nathaniel Kleitman, and Paul Weiss as well as experimental psychologists Donald Lindsley and Harold Schlosberg. In addition, Margaret Mead and Ashley Montagu were two distinguished anthropologists in attendance along with Talcott Parsons, an eclectic sociologist from Harvard University.

The first conference dealt creatively with sleep, wakefulness, hypnosis and time, phenomena that involve the continuum of states of consciousness

and its absence. Margaret Brenman's presentation offered a particularly cogent analysis of how hypnotic suggestion changes conscious awareness by controlling the focus of attention. There was also discussion of levels of consciousness including self-awareness. These were promising developments but participants gravitated toward Freudian theories to explain things that disrupt consciousness rather than trying to understand the neurobehavioral factors that make possible perception or memory.

Brown University psychologist Harold Schlosberg best expressed at the second conference the recurring difficulty the conference participants, such as David Rapaport, had in clearly distinguishing levels of consciousness:

> I wonder if one of the troubles that we have had throughout this discussion of levels, strata, and dimensions is not the fact that we are really dealing with two or three or more concepts lumped together under one general, one generic term, "consciousness," whereas they should not be.... But the topic that Dr. Rapaport was talking about deals with the "content of consciousness." It is related to things like learning and selective attention. The problem of integration keeps popping up here, the mutual antagonism between two related patterns of behavior (Schlosberg, 1951, p. 42).

Rapaport wanted to distinguish between pathological cases involving individuals who lose their personal identity *without* awareness and individuals who suffer a loss of personal identity *with* the awareness of doing so. He noted that the Korsakoff syndrome is often singled out as an example of the former condition, because of the assumption that the individual has sustained brain damage from alcoholism. But Rapaport contended that the Korsakoff patients' tendency to make up stories to fill in gaps of memory was not limited to these individuals alone. This was related to a more widespread tendency that predisposes people not only to contrive their experiences and memories but also to believe that these stories involve real events. In this instance, consciousness becomes a tool of self-deception (see Rapaport, 1951, pp. 30–31).

These assertions about the self-deceptive and error prone nature of human perception continue to surface today regarding the crucial distinction between the process and phenomenological contents of consciousness. Neurologists have now traced the Korsakoff syndrome to a damaged hippocampas that confuses in the person's mind the distinction between recent and past events. Persons with anosognosia, a paralysis caused by stroke, are unable to detect the paralysis and strangely, they deny it even though they see that they are unable to move the afflicted appendage. While the element of denial remains perplexing and could be explained by Freudian

theory (see Ramachandran, 1998), the inability to detect paralysis can be explained by the fact that the effects of a lesion in one hemisphere are generally not communicated to the side of the body that is unaffected. Moreover, cognitive neuroscientists today employ neuroimaging technologies that have isolated neural mechanisms that account for "inattentional" or "change blindness" that do not require a conception of the unconscious.

Macy's International Political Agenda

It is important to mention that Fremont-Smith, Lawrence Frank and the Macy Foundation pursued an international political agenda through their sponsored conference series. The conferences promoted international policies to secure peaceful technological change, economic growth, human development and well-being in the post-WWII era. Fremont-Smith, Margaret Mead and Frank became involved in the International Preparatory Commission that led to the establishment of the World Federation for Mental Health (WFMH) in 1948. The federation adopted the motto borrowed from the UNESCO constitution that: "Since wars begin in the minds of men, it is in the minds of men that the defense of peace must be constructed."

Frank and Mead co-authored the Preparatory Commission report urging that political leaders employ a "dynamic theory of personality" to deal with social and health problems in nations undergoing economic development (Heims, 1991, p. 171). Mead and Frank co-authored the UNESCO sponsored publication, *Cultural Patterns and Technical Change*, which recommended that scientific knowledge about infant and human development be provided to developing countries. Unfortunately they did not foresee the politically polarizing consequences of the Cold War and the tremendous cultural barriers that prevented successful transfer and adaptation of this knowledge.

HERBERT JASPER AND THE LAURENTIAN CONFERENCES ON BRAIN AND CONSCIOUSNESS, 1954–1997

A Model for International Scientific Cooperation

Several organizations emerged after WWII whose members were dedicated to world peace and international scientific cooperation. In 1949, The Council for International Organizations of Medical Sciences (CIOMS) was established jointly by the World Health Organization (WHO)

and UNESCO. CIOMS's promoted international activities in the field of biomedical sciences.

Herbert Jasper, a Canadian neurophysiologist sought closer collaboration between European colleagues involved in CIOMS and their North American counterparts. Jasper (1974, p. 405) acknowledged his debt to pragmatists John Dewey and Charles Peirce who inspired him to understand the brain by seeing how it responds to experience, because he concluded that "one would never know without trying." Jasper also shared Dewey's belief in the importance of interdisciplinary communication as an instrument for strengthening international ties among scientists (Jasper, 1996). Toward these ends in 1947 Jasper became the founding president of the International EEG Organization and editor of a new journal on the subject. With UNESCO's support, and building on increased interest, Jasper and his colleagues formed next the International Brain Research Organization (IBRO) in 1961. The IBRO is dedicated to increasing communication between brain researchers around the world. IBRO members also were instrumental in forming in the early 1980s the Society for Neuroscience, an interdisciplinary professional association, has grown rapidly since then with over 35,000 members in 2002 (Worden, Swazey and Adelman, 1974, p. xxi). During this period of professional development, Jasper and his colleagues (1998) organized a series of conferences from 1952 through 1970, which became known as the Laurentian Conferences on Brain Mechanisms and Consciousness described below. Jasper's et al. (1998) final conference on consciousness occurred in Montreal at McGill University in 1997 when he was 91 years old! He was a co-organizer and also a co-editor of the proceedings *Consciousness: At the Frontiers of Neuroscience*.

Brain Mechanisms and Consciousness

The first international conference on Brain Mechanisms and Consciousness was hosted by CIOMS in Paris in 1952. This meeting, co-chaired by neuroscientists Herbert Jasper (Canada), Edgar Adrian (UK) and Frederick Bremer (Belgium), attracted the attendance of 19 neurophysiologists that included Donald Hebb, Karl Lashley, Walter Penfield, and Harold Mcgoun among others. Lawrence Kubie was the only neurologist. Importantly, four of participants were already veterans of the Macy conferences.

Noteworthy presentations included Lashley's (1953) broadside attack on behaviorism, who contended that dismissing consciousness as subjective sidestepped the crucial question of explaining the existence of consciousness. Hebb (1953, p. 411) made an eloquent plea for inclusion of introspective reports in neurophysiological research citing experimental evidence that thought precedes language. But perhaps A. E. Fessard's

(1954a) talk was the most farsighted. In it he anticipated the contemporary notion that "in the hierarchy of conditions, that integration requires interaction between all parts of the system comes first, not centralization as is often supposed" (p. 207).

Jasper and Penfield invited theoretical speculation about the underlying neural processes that support different states of awareness involving conscious perception and deliberation. The role of the reticular activating system and thalamus were discussed which make possible interaction between cortex and subcortex, enabling the brain to reach the threshold of neuronal interaction and integration needed to support consciousness. But the scientists were divided on whether the reticular formation was limited to the function of arousal or, whether it indeed contributes to changes in specific patterns of activity associated with conscious thought. Moreover, many participants acknowledged that most acts are also performed without consciousness, suggesting that attention can be dissociated from underlying affective processes (Fessard, 1954b).

The Pontifical Academy of Science and Consciousness

These attempts by scientists to fathom the relation between brain and mind eventually attracted the interest and support of the Roman Catholic hierarchy. The Pontifical Academy of Science hosted a conference in 1964 that was organized by John Eccles. Before then, Charles Sherrington was the only neuroscientist that had been recognized by the Vatican for his research on the brain and was elected to the Pontifical Academy in 1936. The 1964 meeting was indeed a watershed event for religion and science in which Pope Paul VI addressed the group in the spirit of the 1960s ecumenical council to reexamine church doctrine. Pope Paul made a remarkable statement that clearly ceded to science the study of higher brain mechanisms and mind, while welcoming the light that this knowledge would shed on humans' moral and spiritual life and values.

> "Brain and conscious experience:" seeing these words associated suffices to make clear that there you touch on that which is most specifically human in man, on that which approaches most nearly the mechanisms of his psychology, the problems of his soul. To be sure, when you speak of 'consciousness,' you do not refer to the moral conscience: the very rigor of your methods ensures that you do not leave that strictly scientific domain which belongs to you. What you have in mind exclusively is the faculty of perceiving and of reacting to perception, that is to say, the psychophysiological concept, which constitutes one of the accepted meanings of the word 'conscience.'
>
> But who does not see the close connection between the cerebral mechanisms, as they appear from the results of experimentation, and

> the higher processes which concern the strictly spiritual activity of the soul?. . . By widening our field of view, We would like to profit by the occasion thus presented to Us to reaffirm before you the Church's attitude of esteem and confidence with regard to scientific thought in general (Pope Paul VI, 1966).

This papal statement significantly modified church policy on the relationship between brain and mind. For example, Gross (1998) reported, in his fascinating history of brain science, that for over 1500 years (from Roman times to Descartes) scientists held that the cerebral ventricles or the empty sinus cavities within the brain were the seat of intelligence. Theologians sanctioned this view because they believed that the mind and soul could not be corrupted by the physical elements of the brain that lay outside these voids.

Modern neuroscience has rendered these beliefs anachronistic. Recent brain imaging studies provide evidence that the prefrontal and other brain regions play a crucial role in moral choice, conscience and judgment (see Greene et al., 2001). There is a growing body of neuroscientific (LeDoux, 2002) and neurological research (Damasio, 1999; Damasio, 2003) which indicates that the capacity to make decisions requiring moral reflection is seriously compromised by prefrontal brain damage. While the catholic hierarchy may not be completely at ease with these findings, Pope Paul and his successors, including Pope John Paul II, continued to support the scientific study of mind while preserving Catholicism's traditional spiritual sphere of influence (John Paul, II, 1990).

Several other neurobiologists participated in the Pontifical Academy of Science in 1964 whose work was just gaining attention that included Roger W. Sperry, Benjamin Libet, and Vernon Mountcastle, communications professor, Donald M. McKay, and psychologist H. L. Teuber. These and other conference attendees speculated about the relationship between cerebral organization and conscious activity, discussed evidence for brain plasticity, examined the effects of stimulation and injury, looked at the role of attention and examined the issue of conscious control or free will. These are important phenomena in contemporary neuroscience because they pertain to the neural mechanisms that support consciousness, determine whether conscious experience can change brain structure or function and help assess whether our belief in free will is justified.

Key Issues for Contemporary Studies

Binding and Integration

The participants at the 1964 Vatican conference debated first whether consciousness can be localized or should rather be considered an emergent

phenomenon whose complexity encompasses the whole brain. MacKay 1966), a philosopher, drawing on cybernetics, contended that consciousness is sustained by a "metaorganizational" feature of brain processes that cannot be localized in the cerebral cortex. Mountcastle (1966) disagreed with MacKay, contending that consciousness is a cortically based function. He stressed that investigations of time-dependent dynamic aspects of cortical function would reveal complex patterns of neural response involving emergent properties of large populations of neurons. Jasper (1966) thoughtfully interjected that while consciousness involves an *interaction* between cortical and subcortical domains, there is something peculiarly unique about this interaction when consciousness is involved that enables an extraordinary selectivity of focus or awareness. Mountcastle concurred with Jasper that more details about this interaction may be revealed by study of the reticular formation—and the possible relationship between the frequency response of signals emerging from the reticular formation and the threshold of conscious awareness (Jasper, 1966).

Research about what binds or sustains conscious attention continues today. Crick and Koch (2003) contended, until recently, that synchrony of neuron firing is sufficient for conscious thought, which stresses integration while failing to explain selectivity. Edelman and Tononi (2000) argue instead that the selectivity and integrity of consciousness are sustained by reentrant connections, which favor stronger interactions between widely distributed neuron groups involving short-term, temporal correlation and synchrony. The power of local synchronization by itself is not sufficient to sustain consciousness. The level of coherence between widely distributed groups provides evidence for this view (Srinivasan et al., 1999). Resolving these theoretical differences about binding requires a more detailed understanding of the neural events that take place below the threshold of consciousness (i. e., neuromodulation of bodily states that affect perceived value). More information is also needed about the events that contribute to the lapse, extinction, or replacement of conscious perception or behavior, such as binocular rivalry, selective attention and sleep or brain dysfunctions, which affect the relationship and synchrony among neuron groups.

Neural Plasticity

Neural plasticity and the efficacy of conscious experience to change brain growth patterns or alter function were also discussed at the Vatican conference on brain mechanisms and consciousness. Lashley (and Donald Hebb, a psychologist) championed the idea that the human motor cortex is highly plastic and susceptible to variation. Lashley's experiments indicated that the motor cortexts of rats exposed to stimulation do not exhibit

a uniform response suggesting the movement maps differed to reflect the uniqueness of each rat's experience. The neuroscientific community did not accept nor fully grasp the implications of this discovery until 60 years later. That is when Michael Merzenich and Jon Kass demonstrated more convincingly that the receptive fields of the somatosensory cortex exhibited a different mapping within the same monkey when efferent inputs were surgically altered and reshaped by subsequent experience. The brain compensates for substantial loss of neurons that retrieve sensory or motor signals by enabling the activity or receptive fields of nearby neurons to invade the cortical space vacated and take over functions previously supported by lost neurons. Similarly, cross-modal neural plasticity accounts for the recovery of motor and speech functions incurred from strokes. For example, the loss of speech incurred by damage to the Broca's area in the left frontal cortex enables the right frontal areas, which are normally suppressed to support limited speech functions.

Attention

Vatican conference participants also examined the possible role of attention in facilitating changes in brain function. H. L. Teuber (1966) argued that when rats are exposed to enriched and complex environments they show greater reliance on vision and thus exhibit more alertness in executing specific tasks. A few years later Greenough et. al. (1993) discovered that rats which receive enriched experience learn and perform tasks more attentively. They also grow more synapses in the dentate nucleus of the hippocampus than those that receive non-enriched experiences. Perhaps the most convincing evidence that attention is instrumental to neural reorganization was presented recently by Merzenich and deCharms (1996). They showed that change in topographic maps occurred only in the auditory cortex of monkeys who *attended* to changes in sound frequencies; no change occurred in monkey's brains that received these same stimuli *passively*. While the question remains open as to how consciousness contributes to these neural effects, the capacity to attend to novel stimuli appears to be an important factor in attaining and integrating experience-dependent neural reorganization (see Stigler, 2001; Schwartz & Begley, 2002).

Free Will

Finally, neurophysiologist Benjamin Libet reported at the Vatican conference his pioneering attempts to employ EEG techniques to determine when subjects first become aware of sensory stimuli and how this affects our understanding of free will. Libet (1966) discovered that there is a half-second delay between the receipt of a stimulus and conscious awareness. These results led scientists prematurely to conclude that the neural

processes underlying self-consciousness may have causal force but consciousness itself is an after-affect. But Libet persisted in devising another series of studies in the 1980s to determine when subjects become consciously aware of the desire to act. Although he found that an unconscious motor activation precedes response (Libet et al., 1983a), subjects are still able to override and thus consciously and willfully control their decision to act (Libet et al., 1983b).

FRANCIS SCHMITT AND THE NEUROSCIENCES RESEARCH PROGRAM, 1962–1982

The momentum was building for several decades in the twentieth century to undertake a large scale and long-term program dedicated to the study of the brain. The proponents of a new brain science faced two crucial problems: (1) how to enlarge the number of scientists involved in the research and discussion and; (2) how to create an organizational structure that would sustain the research and increase support over time. Schmitt (1990, p. 189), who participated in the Macy conferences on nerve impulses, believed that a new "hybrid" field of biophysics was emerging in the 1950s that would reveal the chemical and electrical properties of energy that make possible the intercellular transfer of information in the brain. Through his connections with the National Institutes of Health, he obtained support for a study program at the University of Colorado at Boulder in 1958 to examine fundamental aspects of biophysical science. In attendance were veterans of the Macy conferences on nerve impulses Paul Weiss, W. A. Rosenblith and Donald Lindsley, who helped promote his agenda.

Building Support

Schmitt devised a research program called "the biophysics of the mind" (Adelman & Smith, 1995). He passionately pursued the goal of "improved intercommunication between minds," a Deweyan ideal that inspired the Macy Foundation conferences (Swazey, 1974, p.529). He strongly supported the idealistic aims of the UNESCO and CIOMS conference programs, which he believed able "to survive this present world crisis and advance a new quantum leap . . . in human evolution" (Swazey, 1974, p. 331). He found a receptive audience at MIT, which provided institutional sponsorship and at the National Institutes of Health (NIH), which awarded a multiyear grant in 1962 to develop the Neurosciences Research Program (NRP). (MIT resumed its support of brain science decades later with the creation of the McGovern Institute for Brain Research in 2000). An

Forty-third (and final) Stated Meeting of NRP Associates, March 14–17, 1982. Front row, left to right: K. Cusick, G. Adelman, R.D. Keynes, C.F. Stevens, E.R. Kandel, T.N. Wiesel, W.R. Adey, D.H. Hubel, P. Mueller, R.R. Llinas, F.E. Bloom, D. Ploog, T. Poggio. Second row, left to right: R.M. Held, P. Greengard, W.M. Cowan, D.A. Glaser, F.O. Schmitt, R.Y. Moore, W.E. Reichardt, H. Fernandez-Moran, P. Rakič, T. Melnechuk, M. Konishi, D. Bodian, T.H. Bullock. Third row, left to right: N. Bernick, A. Hodgkin, G.D. Fischbach, F.H.C. Crick, A.M. Graybiel, R.B. Livingston, L.C.M. DeMaeyer, G.M. Edelman, S.S. Kety, N.E. Miller, A.M. Liberman, R. Galambos, Y. Homsy, S.J. Bird, L.L. Iversen, S.H. Snyder, J.E. Dowling, W.E. Gall, W.H. Sweet, F. Plum, M. Calvin, F.G. Worden, H.V. Hyden. Back row, left to right: V.B. Mountcastle, D.M. MacKay, J. Szentágothai, S.L. Palay, F. Morrell, R.J. Wurtman, J.B. Martin, P. Marler, R.W. Nichols, J.-P. Changeux, H. Thoenen, J.J. Hopfield.

Forty-third (and final) stated meeting of NRP Associates, March 14–17, 1982. (Courtesy of NRP)

international "core group" of scientists were formed, many of whom became members of the board of trustees of the Neurosciences Research Foundation (NRF), an independent non-profit corporation. Hudson Hoagland, who attended Macy conferences and who was President of the American Academy of Science at the time, provided space in the Academy's building in Brookline to house the NRP staff (Mcgoun, 1974).

Promoting Discussion and Synthesis

Schmitt designed a remarkable participatory framework for the conduct of "work sessions," and "intensive study programs" that would attract natural scientists from several countries and fields who were interested in neuroscience. (Gross and Cori, 1974). The work sessions did not focus explicitly on mind or consciousness, but did examine neural processes and mechanisms that potentially contributed to understanding functions

involving consciousness, such as memory, language use and cognition (Schmitt, 1992, pp. 231–239). The NRP elected 75 associates, 13 of whom were awarded Nobel Prizes. The *NRP Bulletin* that was distributed worldwide included synthetic overviews of the field and discussion summaries that defined future directions and goals and identified the most promising research in the field (Schmitt, 1974, pp. 7–9). Schmitt constructed, through these instruments of discussion, recognition, coordination and dissemination, scientific networks equivalent in structure, if not stature, to the "invisible colleges" of the Royal Society of 17th century England (Swazey, 1974, p. 542).

GERALD EDELMAN AND THE NEUROSCIENCES INSTITUTE, 1982–

From Immunology to Neuroscience

Gerald Edelman, an immunologist, who was Associate Dean of Graduate Studies at Rockefeller University at the time, was first invited to participate in the NRP in 1964. Edelman (1974, p. 65) was impressed with Schmitt's attempts to "integrate, to probe, and to define things without pretense." Edelman soon attracted his colleagues' attention with his pioneering co-discovery that eventually led to the Nobel Prize in 1972 that revealed the chain structure of antibodies, and which accounted for their tremendous variability and versatility. Edelman presented a novel theory that antibodies in the immunologic system exhibit the capacity to remember deep structures of antigens that is strikingly similar but not equivalent to psychic memory (Eigen and De Maeyer, 1966). The evidence favored a "selective" (i.e., canvassing an existing repertoire of shapes to find the correct structural match) rather than an "instructive" (i.e., antibodies are instructed to fit correctly through the transfer of information from the antigen) mechanism (Edelman, 1999). Edelman's provocative thesis puts pressure on the conventional neuroscientific wisdom by contending that memory does not require a specific site for information storage and retrieval but may involve the dynamic and transient combinations of different recognition units or neuronal structures.

Collaboration and Legitimation

Edelman assumed leadership of NRP in 1982 and created the Neurosciences Institute (NSI) at the Rockefeller University. In 1991 the NSI staff

transferred operations to the Scripps Institute in La Jolla, California, before moving into their permanent quarters in Torrey Pines in 1993. In 1988 the NSI established a program in theoretical neurobiology and later, an experimental program, involving specially appointed resident fellows. A visiting fellow program also was established at the NSI, which has hosted small conferences for scientists involving over 1000 individuals from 300 institutions and 25 countries. The NSI also continues to host the NRP annual conference. Participants have included for example, neuroscientists Michael Merzenich and Eric Kandel, a Nobel Laureate, Harvard psychologist Daniel Schacter, neurologist Antonio Damasio, neurobiologists Jean-Pierre Changeux and Rudolfo Llinás and philosopher John Searle. These distinguished scientists have each contributed significantly to the science of mind and consciousness research. Their recognition increased the perceived legitimacy of a scientific investigation of the brain and the mind that has languished since the turn of the twentieth century.

Selectionism and the Brain

Gerald Edelman has contributed fundamentally to NSI's success by advancing an unrivaled and brilliant theory of mind. Edelman first presented his theory of neuronal group selection in *Neural Darwinism* (1987) and *Topobiology* (1988). In his glowing review of *Neural Darwinism* in the *New York Review of Books* (1986), Israel Rosenfield (1986) even included several technical articles that Edelman wrote or co-authored in scientific journals, which described his novel theory of memory and perception. Edelman presented a more detailed argument for primary and higher level consciousness in subsequent books that included *The Remembered Present*, (1989), *Bright Air, Brilliant Fire* (1992) and *A Universe of Consciousness (*2000), co-authored with Giulio Tononi). In these books, Edelman defined the key concepts of "selection," "degeneracy," and "reentry," which form the "dynamic core" of conscious experience. These terms are worth describing because they involve phenomena that have attracted the attention of and analysis by numerous contemporary neuroscientists, psychologists and social theorists.

Edelman contends that *selection* (i. e., systems for recognition of variation) takes place during development and through experience. Neural growth processes that result in billions of synapses and millions of connections between axons and dendrites are not preset but respond to contingencies of order, competition and probability that produce a unique individual brain. *Degeneracy* is the capacity of elements that are structurally different to perform the same function or yield the same output (Edelman

& Gally, 2001). Degeneracy contributes to flexibility by enabling neuronal groups to form connections that respond differently to the contingencies of experience. Early experiences are crucial in providing the organism sufficient stimulation from the environment to adopt behaviors and to make choices that have value. The interconnected neuronal groups that form in response to experience create perceptual maps and categories that enable the construction of reality. These maps communicate with one another through reentrant signaling processes yielding a basis for the conversion of perceptual categories of information into more complex and even metaphorical pictures and understandings of the world, as brilliantly argued by Johnson and Lakoff, 1999).

Importantly, the concept of reentry should not be confused with "feedback" as proposed by Norbert Weiner, to mean the detection and correction of errors. *Reentry* involves the ongoing parallel signaling between separate neuronal groups along ordered anatomical connections that occur in both directions simultaneously and recursively. Reentry makes possible the simultaneous comparisons of bodily (i. e., kinesthetic) mental and emotional states involved in making choices based on value. Those neuron groups that are interacting more strongly with one another and which sustain integration and a high degree of complexity during a given experience constitute the *dynamic core* of consciousness (according to Tononi and Edelman, 1998).

Implications for Development

Edelman's emergent conception of mind has found support among psychologists. Infant experimentalists Esther Thelen and Linda Smith contended that infants integrate and consolidate their motor and perceptual achievements through reentrant processes (Thelen and Smith, 1998). Infant experimentalists Philip R. Zelazo and Philip David Zelazo contend that the neurobiological structures supporting consciousness and the mental capabilities that derive from them emerge during infancy (Zelazo & Zelazo, 1998). Philip David Zelazo (2000) also argues that the recursive process whereby infants and young children acquire powers of self-reflection by subsuming lower level by higher level rule-governed reasoning processes also conforms to reentrant principles. Finally, Dalton (2000), a developmental theorist, contends that emotions help temporarily bind together specific patterns of reentrant connections that render feelings explicit and which sustain different behavioral states and postures. Through these neurobehavioral processes infants learn how to express their emotions by experiencing the different demands that these emotions place on

their energy and their capacity to communicate them accurately to care givers.

The Brain, Mind and Consciousness

Edelman's theory has precipitated an avalanche of commentary that includes the prestigious philosopher Charles Taylor (see Taylor 1994, pp. 233–235), who praised Edelman for his non-reductionist conception of mind. Edelman is critical of modular and genetic theories of the brain and mind proposed by some cognitive scientists. Edelman rejects the notion that the brain is hard-wired and that there are particular neurons exclusively dedicated to conscious processes. He also dismisses as naive the belief that consciousness can be isolated in specific areas of the brain. He believes that perception constructs reality rather than represents it. He also holds that the primary norms of human experience are variability and transformation rather predictability and redundancy and that with its fullest expression, higher level consciousness involving meaning and significance requires language. Finally, Edelman contends that no two patterns of neural connections supporting consciousness are the same. Not since the great debates instigated by Cartesian dualism, Kantian rationalism and Hegelian idealism have scientists and philosophers been presented with such a theoretically profound, empirically testable and morally significant theory of mind.

THE REVOLUTION IN NEUROIMAGING

The contemporary movement away from purely abstract computational and philosophical conceptualizations of mind to theories that are scientifically grounded in brain science marks an important milestone in the revival of consciousness studies. For several decades, unanswerable questions have been posed about the mind and consciousness that now can be framed empirically. By employing brain-imaging techniques, neuroscientists have recast philosophical debates about free will and determinism into biologically grounded (i.e., embodied) rival hypotheses about the genetic and experientially variable constituents of human thought and behavior. This once diffuse issue has been broken down into a series of functionally specific questions, for example, about prefrontal processes that underpin planning and expectation (Cabeza and Nyberg, 2000).

Mapping the Interactive Brain

These advances occurred because cognitive psychologists and neuroscientists agreed to map the brain according to a conventional grid system and accept as valid the assumption that changes in metabolic activity are indicative of shifts in cognitive activity. Thus subjects' performance of tasks vary according to the difference in amount of metabolic activity that occurs in relevant regions of the brain. The development and widespread acceptance of these brain mapping standards has contributed to an enormous output of research that has identified brain regions believed to play a crucial role in human perception, emotion, thought and behavior. The first generation neuroimaging studies led many scientists to believe (as do those conducting single cell electrical studies) that receptors and neurons are functionally specialized to perform genetically determined roles. Nevertheless, the theoretical significance of these studies of cognition is controversial largely because brain functions overlap and the same structures have been found to perform different functions (Edelman and Gally, 2001). Consequently, neuroscientists have proposed new methods to better capture the interactive nature and contextual basis of conscious brain processes first proposed by John Dewey that have thus far eluded cognitive neuroscientists and psychologists (McIntosh, 2000).

Diagnosing Brain Disorders

Brain imaging also may pinpoint the neurobiological sources of learning disorders, such as dyslexia, attention deficit disorders and many other brain-related dysfunctions that appear to adversely affect perception and attention. For example, until recently, infants were assumed to possess a genetic predisposition to learn language and that this capability was isolated in the left temporal region of Broca and Wernieke's areas. Research by psychologists Helen Neville (1993) and Elizabeth Bates (1999), however, suggests that, contrary to Noam Chomsky, children do not automatically understand syntax, but must be capable first of linking sounds with syllables before understanding more complex grammatical constructions. Auditory and visual cortex play an important role in the construction of speech and language use that engage both left and right hemispheres—a finding that has enabled dyslexics to adopt compensatory strategies. Brain imaging studies forced neuroscientists to drop the notion that structural and functional anatomy are equivalent and to recognize that cognition involves system-wide relationships and interactions within the brain (Beaulieu, 2002).

EXPANDING THE BOUNDARIES OF MIND: FROM SCIENCE TO CULTURE IN THE 1990s?

Thrust into the National Spotlight

The intellectual, scientific and organizational initiatives contributing to the reemergence of the study of consciousness converged with political and cultural forces in the mid-1990s. President Bush's proclamation to dedicate the 1990s to study of the brain stimulated scientific interest and government funding and aroused the news media to cover the brain and mind in more depth than in previous decades. Significantly, after reluctantly acquiescing to this slogan, the American Psychological Association quickly adopted in 1999 a new slogan for the first decade of the 21st century calling it the "decade of behavior." A major conference on infancy and the brain sponsored by the White House and organized by Hilary Clinton in 1997 capitalized on the Bush initiatives and the increased interest among parents in the developing brain. Several professional societies and groups in the United States and Great Britain also became active during this time organizing conferences and promulgating manifestos that proposed new methods for studying the mind and consciousness, which were touted as crucial to understanding the human condition in modernity.

A New Foundation for Conscious Experience

For example, in 1992 an interdisciplinary group of professors from physics, neuroscience, psychology and anthropology recommended a new epistemological approach to consciousness that its spokesman said, "takes the personal characteristics of the observer into account" (Harman, 1994, p. 143). The group urged that this approach be "radically empirical," "objective," acknowledge the "partial nature of scientific concepts of causality," emphasize the "unity of experience," and embrace a "participatory" approach to problem solving (pp. 147–148). This emphasis on epistemology and method was indicative of need to resurrect the term consciousness from its premature burial by behaviorists and logical positivists that denied that scientists could say anything meaningful about phenomena, which lacked empirical evidence. Several other conferences were convened through the mid-1990s, which also asserted the need to restore the scientific respectability of consciousness. (see Sutherland, 1994).

The reemergence of an interest in consciousness also signaled an increasing dissatisfaction with the intellectual hegemony of postmodernism and deconstruction. Michel Foucault, Jacques Derrida and Richard Rorty, among other notable leaders of this genre of thought, contended that mind,

self and consciousness are outmoded terms bequeathed by the Enlightenment. They argued that the belief in free will underpinning these terms no longer reflects the realities of a world of technologically subservient, decentered selves who exhibit the endless capacity for new forms of expression but who are also caught in self-spun webs of desire and deceit. Their belief that science is not impartial but implicated in culture of control and thus incapable of understanding or defending the freedom and integrity of human thought has failed to attract many converts. It also misjudges the motives and goals of pioneering neuroscientists who sought through their studies to enlarge individual access to the resources of mind and consciousness for intelligent communication and peaceful human interaction.

The Tucson Center and the Journal of Consciousness Studies

Perhaps the biggest boost to the popularization of consciousness studies occurred when an international conference "Toward a Scientific Basis of Consciousness" was held in Tucson in 1994. This became a minor media event when physicist Roger Penrose and anesthesiologist Stuart Hammeroff presented their quantum theory of consciousness that drew coverage by the *New York Times* science writer Sandra Blakeslee (Clark, 1994; Freeman, 1994). The proceedings of this and subsequent biennial conferences were published by MIT Press with the title, *Toward a Science of Consciousness*. The Center for Consciousness Studies was created in 1998 at the University of Arizona to provide small research grants and web-based courses. Conference and research topics have included evolutionary and developmental perspectives, phenomenal knowledge, neural correlates of consciousness, computational and cognitive approaches, first-person methodologies, aesthetics, sleep and dreaming among others.

The *Journal of Consciousness Studies* (*JCS*), founded by Keith Sutherland (1994) in 1994, provided an early forum for conference presenters and rapidly increased its readership among researchers in several fields who are interested in the problem of consciousness. Sutherland (1996) passionately supports the notion that a multidisciplinary program must be open to all points of view, including transpersonal and paranormal, and that this is the best antidote to the premature adoption of a paradigm that turns out to be profoundly mistaken. Moreover, Sutherland cited approvingly philosopher John Searle's admonition that "At the present state of our investigation of consciousness, we don't now how it works and we need to try all kinds of different ideas" (Sutherland, 1997, p. 386).

Sutherland's colleagues cautioned however, that by pursuing this broad-based approach, *JCS* and would forfeit its influence among

scientists. Bernard Baars, a psychologist cited the increased interest among mainstream scientific journals, such as *Science* and *Nature,* which were devoting more attention to brain-based studies of consciousness. Thomas Metzinger argued that the study of consciousness was in a chaotic, preparadigmatic state and that further progress necessitated the concentration of scientific intelligence and resources (see Sutherland, 1997, p. 385). Sutherland appropriately expressed concern about the possible Balkanization of consciousness studies, which he believed would create separate camps of scholars who pursued their own agendas in isolation. But this threat of specialization did not thwart the effort to create another society for the study of consciousness.

DEFINING A SCIENTIFIC AGENDA: THE ASSC 1997–

In 1997 a small group of philosophers and scientists that included: philosophers Thomas Metzinger and David Chalmers; psychologists Bernard Baars and William Banks and neuroscientists Christoph Koch, Patrick Wilken, and Jackie Andrade formed the Association for the Scientific Study of Consciousness (ASSC). The society is dedicated to the promotion of research within cognitive science, neuroscience, philosophy, and other relevant disciplines in the sciences and humanities to better understand the nature, function, and underlying mechanisms of consciousness. The ASSC sponsors an annual international conference, hosts Internet seminars, and has two official journals: *Consciousness and Cognition,* edited by William Banks, Bernard Baars and Anti Revonsuo and *Psyche*, an online journal edited by Patrick Wilken.

The ASSC hosts conferences with tightly focused themes involving experimentalists who utilize brain imaging and other technologies. This approach, reminiscent of the Macy conferences, has succeeded in attracting distinguished scientists who have illuminated several phenomena that constitute important core issues for consciousness and mind. The inaugural conference in 1997 at Claremont College examined implicit cognition and at Bremen in 1998, the neural correlates of consciousness were discussed. The third conference at Western Ontario looked at consciousness and self, while the fourth, in Brussels, focused on phenomena associated with the unity of consciousness, such as binding, integration and the dissociation of consciousness. The latest three conferences dealt with the perceptual contents of consciousness (Duke University, 2001), language (Barcelona, 2002) and models and mechanisms of consciousness (University of Memphis, 2003).

The Crucible of Culture

Thomas Metzinger (2000) edited a volume of essays on the neural correlates of consciousness contributed by participants at the conference in Bremen in 1998 (Mutalik 1998). Not since Frank Schmitt's efforts to document and synthesize the NRP conferences, has there been a comparable attempt to pull together contemporary research into such a coherent and thematic exploration of issues central to the function of the brain in relation to the mind and consciousness. The success of this endeavor, as Metzinger cogently notes, depends crucially on the recognition that scientific models are produced by socially interacting groups whose theories do not depend naively on some objective reality, but on *intersubjective* understanding and agreement. The contributors to Metzinger's book demonstrate historical continuity and also indicate substantial progress in understanding the neural dynamics underpinning conscious experience. For example, Damasio (2000) contends that emotions *inform* consciousness rather than override it, as neurophysiologists had previously believed, by being rooted in bodily feelings that furnish "second order neural maps" of events that have perturbed the individual and led him to the perceive and act differently than before.

Metzinger thoughtfully recognizes, as did his predecessors, that perhaps the most critical challenge researchers face is the need to increase public support by creating a "consciousness culture" that seeks new applications from knowledge about the mind:

> Our current lack of genuine consciousness culture can be interpreted as an expression of the fact that the project of the Enlightenment got stuck. What we need is not faith, but knowledge; what we are lacking is not a new metaphysics, but a new variant of practical rationality . . .
>
> We have to move away from a purely defensive position (as is currently widespread in the humanities), away from any cheap, counterproductive resentment. Laying the foundations for a consciousness culture means taking a more active attitude, a—nevertheless critical—point of view that allows us to ask positive questions like How would a future culture look that uses the results of consciousness research in a fruitful way? How to protect the individual from new potentials for manipulation and the dangerous side effects of commercially exploited, newly emerging consciousness technologies . . . .

Historical Continuity and Scientific Support

The ASSC has succeeded in attracting as leaders some of the most noteworthy and innovative scientists and theorists in psychology and the

neurosciences. As founding president, Bernard Baars (1986; 1988), a cognitive psychologist, is widely recognized for his historical analysis of the cognitive revolution and his cognitive theory of consciousness. His historical study included interviews with pioneers whose careers spanned the behaviorist and post-behaviorist eras, such as B. F. Skinner, George Mandler (1975), Howard Kendler and George Miller. Baars (1997; 2002) penetrating metaphor of mind in the theater of consciousness has helped researchers understand how the limited states or stages of conscious awareness are enlarged by access, through the recticular activating system, to an enormous array of subconscious mechanisms that enlarge our powers of thought and behavior. Baars' theory has unquestionably contributed to a revival of James' theory of the volitional mind by suggesting how conscious thoughts recruit physical processes to execute intended goals with consummate efficiency by exploiting the highly distributed but interactive nature of functional brain processes.

Another recent president is Christopher Frith, an experimental neurologist at University College of London. Through his studies of individuals with brain disorders, such as schizophrenia and autism, Frith has proposed a remarkable theory of consciousness, whose neural correlates link the capacity to form intentions with the ability to read other minds. Significantly, Frith's research was given added weight and credibility by being published in the journal *Science* (see Frith and Frith, 1999). Through the leadership of these and other ASSC presidents, the ASSC has succeeded in attracting distinguished neuroscientists around the world to participate in their conferences.

TOWARD A COMPARATIVE AND INTERDISCIPLINARY SCIENCE OF MIND AND CONSCIOUSNESS

The resurgent interest in mind and consciousness in the 1990s could not have been foreseen a few decades ago. The evocative counterculture of the 1960s introduced "consciousness raising" into the lexicon of serious scholarship. But this term had more to do with an increased social awareness of racial prejudice and inequality and with drug-based altered states of consciousness than with the relationship between mind and brain. The dramatic growth of cognitive psychology in the late 1970s eclipsed the prominence of the behavioral school within the field of psychology although it retained behavior as an important empirical indicator of mental events. In the 1980s, psychologists began to incorporate the computing based "information processing" conceptualization of mind in their studies of human cognition. This mechanistic mode of understanding mental

operations in terms of storage, retrieval and computation has given way to new methods of modeling brain processes that show greater sensitivity than before to emergent properties of brain function (Elman et al., 1998).

Modeling the Minds of Animals and Children

Pioneering studies in the 1970s also traced the ancestry of the human mind in chimpanzees. Psychologist Gordon Gallup (1970) developed a clever technique to determine if chimps are capable of self-recognition by seeing if they notice a change in their appearance in a mirror. Through this technique, not only have chimps demonstrated self-recognition but two year-old infants have also demonstrated the same capacity Lewis & Brooks-Gunn, 1979. Premack and Woodruff (1978) discovered that chimpanzees are able to attribute mental states to other con-specifics and thus possess a theory of mind. This has stimulated related lines of inquiry in animals and young children, described in this volume by Corballis and Lea, that include tactical deception, mental perspective taking and imitation. These important comparative studies underscore the need to better understand the evolution, developmental origins and cultural dynamics of mind.

Neurobiological conceptions of mind can be traced back to Dewey and his scientific colleagues Clarence L. Herrick, Charles. J. Herrick and C. M. Child (see Dalton and Bergenn, 1996) that focus on the emergent, self-organizing, dynamic and interactive nature of neurobehavioral networks and that stress the role of experience, context, emotion and effective (versus functional) connectivity. Significantly, these elements of mind preserve an evolutionary and functional role for consciousness that is not apparent in computational models that minimize awareness and construe mental operations in terms of automatic processes of informational exchange. Moreover, philosophers and neuroscientists find the cultural and ethical implications of an "embodied" conception of mind compelling. This conception opens new avenues for understanding how language, emotion, belief, and intention constitute our self-images and contribute to our awareness of and capacity to interact and communicate with other minds (Johnson and Lakoff, 2001; Dalton, 1999; Damasio, 1999; Frith and Frith, 1999).

Citation Trends

This increased interest in mind and consciousness among scientists is reflected in articles published in professional journals. A steady growth of the word "consciousness" in scientific publications is shown in Figure 1. It goes from almost zero citations in 1950 to more than 1400 in year 2000. The

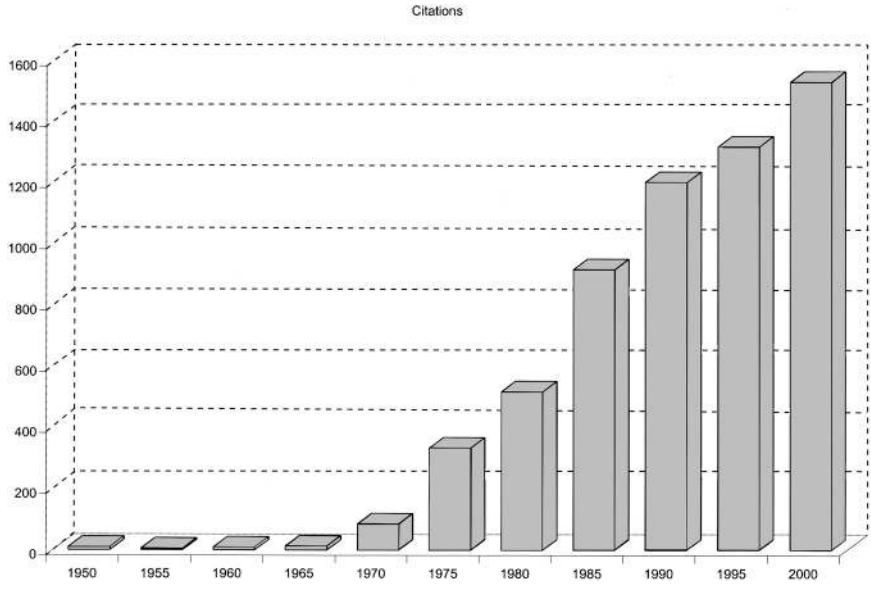

Figure 1. Citations per year of "consciousness."

numbers were collected from the biomedical literature, using PubMed—a biomedical database that contains 9 million titles and abstracts. They seem to confirm that consciousness is back in science. But it doesn't appear yet that the brain and the phenomena of consciousness are central concerns of psychology.

In their fascinating comparative study of citation patterns among flagship journals in behavioral, psychoanalytic, cognitive and neuroscientific fields over the last half-century that appears in this volume, Tracy, Robins and Gosling contend that neuroscience is achieving high levels of prominence *outside* the field of psychology. They found that the rate at which psychologists publishing in the top journals in these fields cited neuroscientific journals fell well below the citation rates of those who published their work in top scientific journals, such as *Science* and *Nature*. That is to say that mainstream psychologists did not pay increased attention to developments in neuroscience until only within the last five years, and then only modestly so. The authors found this surprising because the cognitive school has enjoyed the biggest surge in prominence in psychology and the brain has become the common currency for discussing psychological ideas. They urge that the field of psychology strengthen its disciplinary relationship with neuroscience rather than let it "slip away" and perhaps form stronger bonds with the biological sciences.

American Psychological Association Initiatives

Leaders in the field of psychology face daunting dilemmas in their attempts to support the growth of knowledge that strengthens rather than weakens ties with neuroscience. Psychologists face pressures, as do most professionals and academics, to not only produce new knowledge but to use that knowledge to enhance human well being. The centrifugal forces of specialization, and the demand for applied knowledge and therapeutic interventions are weakening the capacity of the American Psychological Association (APA) to sustain the participation of scientists by finding common intellectual ground (Fowler, 1996; Dewsbury, 1996). The APA has undertaken two recent initiatives that show promise of fostering interdisciplinary collaboration that is necessary to bring about reunification.

Sponsoring Neuroscientists

In 1998 and 1999, APA Divisions 3 (Experimental Psychology) and 6 (Behavioral Neuroscience and Comparative Psychology) jointly sponsored an invited symposium series, Mind, Brain and Behavior supported by a grant from the National Science Foundation. This innovative and well attended program attracted some of best known researchers in the cognitive sciences and neurosciences that addressed phenomena that included vision, perception, attention, memory, language and decision-making and choice. Invited speakers included, among others, Michael Posner, Michael Merzenich, Jeremy Wolfe, Larry Squire, Barbara Tversky and Marlene Behrman. The 1998 meeting also featured a Plenary address on consciousness by the late Harvard philosopher Robert Nozick. And Division 24 (Theoretical) sponsored an invited symposium on the "Revival of Consciousness in Psychology" that included Thomas Dalton, Bernard Baars, anthropologist Kathleen Gibson and infant experimentalist Philip David Zelazo.

Conference Reorganization

In 2001, the APA also restructured its annual meeting to reduce the size of the program and to promote interdivisional collaboration. The divisions were divided into program clusters of 3–6 units each and then asked to propose common program themes. Through this process, the APA sought more cooperation and consensus among the divisions regarding issues and themes likely to attract the most interest for organized panels. Perhaps not surprisingly, the program cluster that included Divisions 7 (Developmental), 3, 6 and several others selected two of three themes pertinent to mind and consciousness that included early experience and the

brain and consciousness and unconscious processes! This new program structure may furnish avenues for the reemergence of common themes like mind and brain that may contribute to reunification.

Undergraduate Education and the Brain

Beyond these initiatives however, undergraduates in psychology and in the arts and humanities want to become better informed about the revolutionary advances in scientific knowledge about the brain and mind. This educational effort is urgently needed because professors in the arts and humanities claim to be experts on the mind yet they know the least about the brain. Responding to this concern, Neil Rudenstein, then president of Harvard University approved in 1993 the first undergraduate certificate program in the nation in Mind, Brain and Behavior (MBB). MBB brings together over 60 scholars and nearly 600 students from the full range of academic disciplines and from the professional schools. The fellows in the MBB academic program critically probe the implications of the neuroscientific revolution and develop multi-level frames of reference that put "the brain in context" and that emphasize the interplay of biology, culture and ethics in human experience.

Elizabeth Coleman (2003), president of Bennington College and a recent NSI visiting fellow, has also undertaken an unprecedented initiative to break down artificial barriers to knowledge by eliminating departments and introducing interdisciplinary courses in brain and mind that are intended to stimulate institutional transformation. These initiatives at Harvard and Bennington may contribute to the development of a culture of consciousness that is needed to sustain long-term societal and scientific support. They may also level the playing field for faculty in the arts and humanities by providing access to brain imaging technologies that will enable them to ground their research in the latest knowledge about learning involving human perceptual, cognitive, emotional and aesthetic capabilities.

CONCLUSION: UNDERSTANDING THE SCIENTIFIC REVIVAL OF CONSCIOUSNESS

Our review suggests that there are numerous conceptual, interpersonal, organizational, professional, political and cultural factors, which have contributed to the events leading to the scientific revival of mind and consciousness. These developments do not yet appear to signal a major

paradigmatic shift away from reductionist scientific perspectives involving the dominance of physics and a molecular biology grounded in the genome. The prevailing paradigm that pursues the elusive knowledge of the universal physical forces that bind all natural things and that explains the biological origins of all living things, continues to relegate issues involving the relationships among mind, brain and behavior to a subordinate status. However, the study of mind and consciousness portend a much stronger neuroscientific influence in psychology and philosophy and the emergence of new methods and styles of inquiry in biology and neurology. These developments may revolutionize how biologists and practitioners in the social sciences and humanities understand the role of mind and experience in the conduct of inquiry and in the advancement and reconciliation of culture and science.

Religious and Scientific Domains

The study of the human mind and consciousness has deep historical roots, but knowledge of the brain lagged until nineteenth century scientists contributed modest but critical breakthroughs in our understanding of neuroanatomy and functional brain processes. For many centuries theologians and philosophers dominated the discourse on mind. Although significant, Descartes' attempt to model brain functions, which traced consciousness to the pineal gland (and the ventricles), did not advance brain science because he equated mind with soul that stood wholly apart from the brain. Papal acquiescence in the 1960s to scientific expertise in the realms of brain and mind, which preserved church authority in matters of conscience and soul, constituted an explicit break with the political worldview of Cartesian dualism. The separation of church authority and scientific method contributed, in part, to the rapid advancement of the scientific study of consciousness in last half of the twentieth century.

Prominence, Progress and Synthesis

While ideas sometimes seem to take on a life of their own, this perception underestimates the considerable role of prominence in sustaining interest in and attention on concepts that endure. In fact, mind and consciousness have attracted through the centuries an enormous number of distinguished proponents of competing theories. By the middle of the nineteenth century, the major philosophical theories of mind today were well established. This monopolization of the discourse on mind by philosophers has sometimes hindered progress because of the tendency among

Descartes, Kant and Hegel's successors, for example, to reaffirm their alternative perspectives in competing schools of thought rather than to develop novel approaches. Contemporary theorists and researchers are breaking the bonds of their philosophical heritage first undertaken by James and Dewey. Striking progress has occurred because leaders in brain science realized that the essence of inquiry involves the suspension of belief, the discovery of new methods and that originality has more to do with the synthesis and integration of knowledge than its origination. Nevertheless, dramatic progress in research on consciousness would not have occurred without scientists like Gerald Edelman, who creatively exploited the analogous processes of selection in immune and neural systems to advance a scientifically testable theory.

Essential Insights and Relevance to the Human Condition

The great philosophers of mind have contributed important insights about the human condition and the crucial role that beliefs and ideas about autonomy, freedom, and responsibility play in sustaining human dignity and integrity. But much of the progress in the science of mind since the late twentieth century has depended less on addressing bold questions regarding human ethics and creativity than on neurological and neurobiological disorders that diminish or alter human cognition, choice and behavior. By taking this tact of studying brain disorders, neuroscientists have been more successful in identifying the processes that make consciousness possible, by isolating those factors whose absence seriously compromises awareness, attention, the capacity to plan and anticipate, which ultimately interfere with judgment and ethical behavior. Victims of strokes now obtain more accurate diagnoses through neuroimaging than before and the successful treatment of children with dyslexia, attention deficit syndromes and other disorders has vastly improved. Federal laws now regulate the use and transplantation of neural stem cells, establish research guidelines regarding interventions and therapies for persons afflicted with Parkinson's and Altzeimer's disease and that control other related practices (Blank, 1999). The scientific study of consciousness will undeniably flourish if it can be demonstrated that advances in brain repair and the mitigation of cognitive dysfunction depend crucially on knowledge of human perception, attention, and the relationship between motor, cognitive and emotional states of mind. Importantly, this approach reflects Dewey's seminal ideas, who strongly emphasized that understanding the integrative nature mind and brain was the key to changing habits and understanding inquiry.

Conceptual Clarity and Communication

Advances in science are sometimes impeded by disputes over concepts whose terms defy precise definition. For a long time, philosophers disputed the nature of motion, energy, matter, space and time because they were unable to render these concepts into commensurate physical terms. Metaphors and analogies are literary devices that have often been employed effectively to best express similarities between known and unknown events. Synonyms are frequently employed to do so, sometimes with unfortunate results. Behaviorists substituted the term "conditioned reflex" for learning thus considerably reducing the realm of behavior indicative of higher cognitive function. Similarly brain-imaging researchers have been particularly vulnerable to the criticism that regional brain activation cannot be equated with the presence or absence of consciousness, but entail complex continually changing interrelationships among structures. The challenge today is to conceive of mind in terms that recognize its embodiment in multiple brain functions but that also reflects the *interdependence* between persons who must use their brains to communicate thoughts and experiences whose meaning and understanding require more than one mind. The future of neuroscientific studies of the brain will depend not only on their success in overcoming brain disorders that limit individual potential but on their capacity to enhance communication and interpersonal understanding.

Judgment, Sovereignty and Human Rights

Controversy has doggedly followed attempts to understand the human capacity for judgment, thought and reason. Throughout the centuries, theologians, philosophers and scientists were contented to accept the widespread belief that powers of the human mind were the product of divine creation, and that human judgment was imperfect and morally flawed. This explanation not only satisfied scientists who believed that mind was insoluble, but it also furnished a convenient justification for monarchical authority. Nineteenth century democratic movements challenged the sovereignty and legitimacy of monarchies and triggered a crisis in authority in modernity that led to democratic movements grounded in the belief in the inviolable rights of human beings. The belief in the capacity for self-reflection and self-governance naturally elevated the stature and dignity of men and women whose cognitive powers became the subject of increased interest, study and debate (Taylor, 1989). These same ethical and political considerations dictate that the uniqueness and integrity of each human mind must be respected and that the capacity for

conscious choice and emotional expression must be protected (see Damasio, 2003).

Scientific Legitimacy and Public Support

A controversial phenomena is unlikely to attract the serious interest of scientists or produce a growing body of research unless it is considered a legitimate object of inquiry supported by private and public funding. Private or public organizations or professional societies have never universally and unconditionally supported the study of mind and consciousness. Ever since ecclesiastic views of the soul and mind were considered sacrosanct, scientists have been reluctant to pursue investigations that would not attain peer approval and organizational support. Only when scientists were given the opportunity and financial support to candidly discuss the relation between mind and brain did the science move forward. Through strategic leadership, professional and organizational contacts were forged throughout the world that set the stage for cultural expansion and popularization. Through these mechanisms of expanded communication, scientists and educators are fulfilling the promise of Dewey's pragmatism whereby communities organized for intelligent action form publics dedicated to addressing issues with significant policy consequences. But the current popularity of mind and consciousness studies may not last unless it becomes institutionalized. Historically this has required that intellectual and professional ownership be asserted that generates a distinctive body of research. It is too soon to tell whether any single field will claim consciousness as its primary concern and it is also possible that an interdisciplinary amalgam of researchers may be formed from several fields.

Scientifically Testable Phenomena

Science does not advance when scientists spend more time proving and supporting what they already know rather than challenging assumptions and gaining new insights that produce new knowledge. The demand for reliable evidence and corroboration are important scientific principles that can be taken to extremes, as evidenced in the medieval era and during the Inquisition, when Galileo's ideas and novel methods were subjected to intellectually tortuous and treacherous logical challenges by those who opposed his new ideas. The emergence of the scientific study of mind and consciousness has been challenged by the unusual physical attributes of being supported by observable brain processes but whose phenomenal,

experiential properties are not directly accessible to third person analysis. The so-called "hard problem" of demonstrating what it is like to experience one's own and another's consciousness is asserted to lie at the heart of the scientific paradox of mind (Chalmers, 1995). Construing phenomenal experience as primarily an epistemological problem of self-knowledge needlessly reintroduces Cartesian dualism. Moreover, this seriously understates the interpersonal and intersubjective nature of consciousness, as Dewey understood its role in human experience.

Ultimately, it may be impossible to duplicate a first-person experiential perspective, because that would require that all internal and peripheral phenomena that make an experience uniquely personal be isolated and then correlated with brain states. Perhaps the conundrum of whether perception takes place inside or outside the brain, Hurley (1998, 420) asserts, can be avoided by allowing perception and action to be "constitutively as well as instrumentally interdependent." Brain states and perception are chronically underdetermined primarily because attention and action are needed to actualize any one intentional state of mind and there are always alternative strategies and structures available to do so that make each experience uniquely different (see Edelman and Tononi, 2000). Nevertheless, humans would be unable to communicate unless they were capable of sharing and mutually understanding common experiences.

Understanding the experiential nature of brain states ultimately requires comparative and inter-species analyses, as argued by Michael Corballis and Stephen Lea in this volume. This study may reveal unexpected similarities among neural processes and functional capabilities across species (Corballis, 2002). Technical advances in brain imaging methods may eventually enable the interpersonal and longitudinal studies of experiential states of mind needed to understand the quantitative and qualitative basis for differences in conscious perceptual processes (Montague et al., 2002). This line of inquiry will also demonstrate the tremendous flexibility and freedom humans possess to continually develop and expand their powers of mind and consciousness (see Adolphs, 2003).

Theoretical Pluralism and Interdisciplinary Collaboration

The publication of Thomas Kuhn's *The Structure of Scientific Revolutions* in 1962 precipitated a provocative debate whose outcome remains inconclusive. Kuhn was heralded for his analysis of the paradigmatic practice and revolutionary transformation of the sciences that he illustrated from the sixteenth through twentieth centuries. But scholarly

reaction quickly focused on conceptual distinctions between "normal" and "revolutionary" science and whether or not scientific fields qualified as paradigmatic or pre-paradigmatic in their theoretical and methodological development (see Fuller, 2000). Neuroscientists, psychologists and scholars from other disciplines engaged at different times in these debates. The intention was to become more theoretically self-conscious about disciplinary roots and practices and to examine whether fields of knowledge can and should be demarcated from one another according to some unique paradigmatic features (see Connolly, 1973; Overton, 1998 & Fuller, 2000).

Swazey and Worden (1974) wanted to see whether the field of neuroscience fits the Kuhnian pattern of science in which mid-range puzzle solving and theory testing gives way, under the accumulated weight of anomalies, to the adoption of a completely new framework of understanding. They concluded that neuroscience was pre-paradigmatic with research largely concentrated in exploratory and experimental studies. They believe that this is illustrated by the swing back and forth between plasticity and connectionist theories and between local and global theories of brain structure and function (see Sperry, 1974). Contrary to their analysis however, I believe that exploratory and experimental methods, complemented by continuous technological innovations and accompanied by vigorous competition between well-defined and testable alternative theories is the hallmark of *dynamic* science rather than a mature science that is theoretically moribund. The great strength of neuroscience thus far has been its capacity to attract many disciplines, to innovate and to continually spur new discoveries and theories that have revolutionized our knowledge of the relation between brain and behavior. This suggests that the rules of intellectual and scientific engagement characteristic of a "mature" science are being redefined.

Contrary to Swazey and Worden's (1974) assertion, the pervasive multidisciplinary character of contemporary neuroscience, including consciousness studies, is not indicative of the "immaturity" of this endeavor in a Kuhnian sense. Rather it signifies the hallmark of a new science of human experience in which brain, mind and consciousness play a fundamental role. In this scenario, breakthroughs in our knowledge of the human mind will increasingly depend on collaboration, the combination of resources, replicable experiences and shared insights. No one discipline or profession possesses an intellectual monopoly of ideas, concepts and theories about mind or their application. The future state of the art of scientific discovery and advancement in psychology and other fields may be foreshadowed in the emerging and growing interdisciplinary study of mind and consciousness documented in this chapter.

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Part 4

Pragmatism, Individual and Social Development

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MYRTLE McGRAW, THE MATURATION DEBATE AND AFTERMATH

Thomas C. Dalton

INTRODUCTION

Myrtle McGraw is an acknowledged pioneer in infant development (Bergenn, Dalton and Lipsitt, 1991). Indeed her innovative methods of special stimulation have been adopted successfully by contemporary experimentalists. They have demonstrated that early experience contributes to the rapid expansion and acquisition of motor and cognitive skills. Her mentor and little known collaborator founding American pragmatist, John Dewey (1935) predicted that her work would "revolutionize the field" of child development. Nevertheless, McGraw's collaboration with Dewey was never revealed nor acknowledged thus depriving her of the prestige of having a famous mentor. Nor has McGraw ever been able to escape completely from the shadow of maturationism and the nature versus nurture debate that continues to stalk her work. These paradoxes are worth examining, because they provide an opportunity to retrace the intellectual and professional pathways through which McGraw's developmental theories have been appropriated, interpreted and contested. The argument advanced in this essay is that McGraw actually established, with Dewey's help, a bold new framework for understanding the reciprocal relationship between brain and experience that will put this debate behind us and

perhaps, contribute to a better appreciation of McGraw's perspective about growth and development.

There are several intellectual, interpersonal and professional factors, discussed in this essay, that appear to have contributed to McGraw's difficulties in overcoming popular misconceptions of her work. An analysis of these factors will illuminate the phenomena whereby eminent psychologists are sometimes destined to be denied recognition for their most significant contributions, because their ideas do not square with the conventional wisdom of their era. There is also evidence that the weight of scientific opinion may be shifting in favor of a more balanced view of McGraw's theoretical perspective. Developmental scientists are showing signs of discarding the outmoded nature/nurture dichotomy and embracing a new consensus on the reciprocal relationship between brain and behavior in individual development that McGraw first advocated.

PUTTING McGRAW'S RESEARCH IN CONTEXT

None has better captured the timelessness of Myrtle Byram McGraw's life and work than her daughter, Mitzi Wertheim, who said that her "mother was born at the end of the nineteenth century, she lived in the twentieth century, and she thought in the twenty-first century" (Lipsitt 1990, p. 977). McGraw' s research took place when American parents were preoccupied with genetic influences on childhood, seeking unequivocal guidance as to whether nature or nurture was more important in shaping temperament, learning, and behavior. McGraw attempted to transcend the nature versus nurture dichotomy by demonstrating the reciprocal relationship between brain and behavior–between development and learning. But her findings demonstrating the significance of early experience in development were not clearly understood and were subject to misinterpretation. Moreover, McGraw's little known collaboration with founding American pragmatist John Dewey remained shrouded in mystery until new information surfaced, which indicates that Dewey considered her research crucial evidence in support of his experiential conception of mind. And evidence is mounting today in the 21st century that corroborates her pioneering discoveries by showing that neurobiological processes can be transformed through experience.

An Alabama School Girl's Dream Come True

McGraw's rather inconspicuous life as an Alabama schoolgirl took a significant, if not fateful turn when she wrote John Dewey as a teenager

from 1914 to 1918. The letters they exchanged subsequently disappeared but Dewey's correspondence with McGraw during the 1930s and early 1940s documents their close personal ties and remarkable collaboration in her infant studies. Dewey, who (McGraw 1990, p. 934) called her "intellectual godfather," remained in contact with McGraw while she attended Ohio Wesleyan, spoke at the school, and influenced her decision to study psychology at Columbia University, where Dewey was professor of philosophy. McGraw took several of Dewey's courses, while supplementing her study in psychology with classes in neuroanatomy before obtaining her Ph.D. in 1931. Dewey was one of the members of her dissertation committee that was chaired first by Helen Woolley and then Lois Meek (see the chapter on Woolley by Milar in this volume). McGraw (1967, p. 6) characterized her association with Dewey as "a very devoted sort of father-daughter relationship" in which she was considered a part of the family. McGraw recalled that Dewey suggested many of the ideas she tested in her research and said that "I discussed everything I did with him" (McGraw 1972a, p. 32). Indeed, McGraw recalled years later that: "Every now and then I

John Dewey and Myrtle McGraw, 1936. (Courtesy of Mitzi Wertheim)

wake up to the fact that some idea that I think was my own, if I happen to pick up something he wrote long ago, he was saying it. My connection with him was just learning by living and by talking" (McGraw 1972a, p. 32).

McGraw's studies provided Dewey the opportunity to ground his paleobiological and neuropsychological premises about mind and inquiry in the latest scientific evidence about early development. Dewey (1976; 1981) largely failed to convince his philosophical colleagues that learning and inquiry are not predetermined by formal rules of logic but are shaped by the strategies humans have devised to respond to uncertainty (Russell 1977). Consequently, Dewey (1986) wanted to identify the circumstances that contribute to the need for judgment, to isolate the specific traits involved, and to show how they helped form the pattern of inquiry. This knowledge would enable Dewey to explain how propositions enter existentially into inquiry through methods that bring events under control, while increasing the ability to meet new contingencies (see Dalton and Bergenn, 1996).

A Collaboration at the Neurological Institute and Babies Hospital

Dr. Frederick Tilney, a neurologist and Director of the Neurological Institute of New York, appointed McGraw associate director of the Normal Child Development Study, which was conducted throughout the 1930s at Babies Hospital, a pediatric division of the College of Physicians and Surgeons, Columbia University Presbyterian Hospital. Tilney was the first professor to teach courses in neuroembryology when the field was in its infancy and directed research at the institute before becoming its head in 1935 (Elsberg 1944; Pool 1975). Tilney (1923) had an absorbing interest in the evolution of the brain and its relationship to behavior. He believed that the phases involved in the progressive enlargement of mind and intelligence could be reconstructed by identifying the functional relationship between cortex and cerebellum in early infant development (Tilney and Kubie, 1931). Tilney created an advisory committee that included Dewey, his Columbia colleagues, psychologists Robert Woodworth and Edward L. Thorndike and several other physicians and scientists from Columbia University. George Coghill, a neuroanatomist, also served as an ad hoc member, while employed at the Wistar Institute in Philadelphia, as did noted behaviorist, John B. Watson, whose involvement was limited to occasional attendance at advisory meetings.

Lawrence K. Frank and the Macy Foundation

Dewey was a founding board member (1930–1944) of the Josiah Macy Jr. Foundation that supports medical research a fact unbeknownst to

Dewey's closest colleagues and one that has eluded researchers. Dewey, and protégé Lawrence K. Frank, who served as vice president from 1936–1941 under Ludwig Kast (1930–1941), had an unusual opportunity to influence the scope of McGraw's studies as well as to participate in the selection of many other collateral research projects funded during the same period (Kast 1936, pp 34–37; Rappleye 1955, p. ix). Frank created an unprecedented international and interdisciplinary network of academically based researchers and he created significant fellowship opportunities for women to enter the field of psychology (Dalton 1994). Frank (1935; 1935) considered human growth to constitute an essential focus of developmental studies. He defended the need for McGraw's studies, even though the Rockefeller Foundation was already funding Gesell's studies at Yale. Frank argued that McGraw was not establishing age norms, but trying to "delineate the sequence through which the child passes and to discover how far that sequence is modifiable by training" (Frank 1933). With Frank and Dewey's assistance, McGraw put together an interdisciplinary group of 10 researchers and 8 technicians that included a neurophysiologist, a physiologist, a biochemist, 2 pediatricians, 3 psychologists, and 2 nurses. The complete scope and sophisticated nature of this research documented in over 50 journal articles has yet to be fully appreciated! (See Dalton and Bergenn, 1998a for McGraw's own account of her research and that of her collaborators.)

Adapting George Coghill's Methods

McGraw (1975, p. 10) readily adapted Tilney and Coghill's methods in her infant studies, contending that "it is the experimental embryologists, not psychologists who deserve credit for formulating the most adequate theory of behavior development." McGraw (1979) acknowledged Coghill's extensive influence by saying that: "Coghill visited my laboratory many, many, many times—sometimes with Tilney, sometimes not. We talked and exchanged ideas. It was he, John Dewey, and the babies that got me thinking of process, not end result, or achievement." McGraw (1979) also stressed that collaboration among them was essential to the success of the project by saying: "Had he [Tilney] lived longer, Tilney, Coghill, and Dewey and I (let me say the babies) might have arrived at a synthesis of the meaning of structure and function"

Coghill contributed some crucial theoretical and methodological insights that strongly influenced McGraw's studies. Coghill (1930) found evidence that neural growth anticipates the acquisition of function and that movements associated with walking, such as stepping, could be performed before the onset of locomotion. This phenomena, which Coghill called "forward reference" constituted learning, because these precocious

movements contributed to the eventual proper coordination and integration of brain and behavior. Indeed, McGraw found evidence that infant behavior develops through processes of neurobehavioral differentiation and integration similar to those Coghill (1933a; 1936) observed in the fetal development of salamanders. She demonstrated how to tease a specific response from a diffuse one by changing the circumstances in which transient reflexes, such as the startle reaction, are employed to respond to a more complex situation, such as swimming. This enabled infants to master the challenges of coordination posed by swimming by learning how to engage these movements in a new situation demanding novel behavior.

FORESHADOWING THE MIND IN DEVELOPMENT

McGraw's research strongly supported Dewey's (1981, p. 30) previously untested assertion that "biological functions and structures prepare the way for deliberate inquiry" and "foreshadow its pattern." Dewey challenged the beliefs that humans possessed unalterable, permanent traits and that human development unfolded in a predictable and invariable sequence. Instead, Dewey (1988) believed that the form and function of human behavior depends entirely on contingencies of order, and that the mental attitudes we adopt decisively affect our behavior, modes of learning and achievement. Dewey (1975, p. viii) praised McGraw's work in his Introduction to her book *Growth*, declaring that she had tentatively established general principles of child development. Dewey also considered her contribution comparable to that of Michael Faraday, a physicist and nineteenth century pioneer in field theory (New York Times 1935, p. 19) because she demonstrated that growth processes advance like interpenetrating electromagnetic waves, redistributing energy through developmental experiences that enable behavior to assume new forms.

The Effects of Early Stimulation

McGraw's (1975) experimental studies furnished evidence that supported Dewey's experiential conception of mind. McGraw demonstrated the fruitfulness of Coghill's embryological perspective by grafting together different behaviors, just like embryologists transplant cells, to see what form they would exhibit within a new situation. She reasoned that the diffuse writhing and wiggling motions involved in the Moro reflex to surprise within the first few months after birth were neither dysfunctional nor functionally specific behaviors. Rather, they were indicative of uncertainty and the need for more information and behavioral context. This required that

she devise a situation that would enable her to tease a specific response out of a diffuse one. McGraw (1939) accomplished this feat by challenging infants to swim. She discovered that transient reflexive reactions, such as Moro and Babinski reflexes, considered dysfunctional at birth, proved instrumental, when transferred, in hastening the emergence and enhancing the quality of its ontogenetic counterpart. When infants first make contact with water their flailing movements barely keep them afloat. But when infants are placed in a supine position they are able to swim smoothly by moving arms and legs from side to side, as occurs in the Moro reflex to surprise (McGraw, 1975, pp. 122–130; 1939a).

By employing these methods, McGraw (1975, pp. 237–244) also discovered that Johnny could learn how to skate before he could walk smoothly, by providing early opportunities to practice stepping, and by exposing him to situations which challenged his sense of balance, such as walking on narrow tables. In addition, McGraw (1975, pp. 136–147; 151–160) showed that the introduction of a new technique, such as grasping, into an existing behavior pattern results in the elimination of more laborious methods. This was demonstrated by Johnny, who was able to ascend and descend slides by grasping the sides and by using his hands to dismount a stool, eliminating more laborious steps taken by Jimmy. These experimental situations, involving the substitution of behavior, suggested that the challenge of inquiry consists not in perfecting a task through repetition of motor actions, but by maintaining focus and balance to face unforeseen situations without losing one's footing or breaking stride.

Behavioral Patterns of Growth and Inquiry

McGraw's experiments with special stimulation demonstrated that awareness flickers at the earliest stages of infancy, sometimes exhibited by an urge for propulsion, or by a suggestive attitude of curiosity or arousal. Importantly, McGraw (1941) discovered that infants exhibit attentiveness and deliberation when they first begin to crawl, to reach and explore their immediate environment. McGraw (1975:306–308) also observed that growth processes are not straightforward. Developing behavior patterns alternate and overlap, according to McGraw, pulsating or oscillating forward and backward in rhythmic waves. Development proceeds through growth phases, involving exaggerated and inhibited movements, the elimination of excess motion, and the consolidation and integration of complex behaviors. This alternating sequence, involving frequent reversions to more rudimentary behavior seems paradoxical because individual variations in the timing, composition, and direction of movements occur without altering the general pattern of progressive development. Nevertheless,

with each backward swing, older traits and emergent capabilities are actually recombined and reintegrated in slightly different ways, according to McGraw, to produce novel ontogenetic behavior. Consequently, excessive or idiosyncratic elements of behavior patterns are eliminated in the course of development at the same time that new traits and functions are substituted that contribute to greater flexibility and adaptability (McGraw 1939).

According to McGraw, the development, consolidation and integration of complex behaviors proceeds through growth phases involving exaggerated and inhibited movements, the elimination of excess motion. This pattern is illustrated in the development of erect locomotion by the alternation between a wobbling and rigid gait, the adoption of wide and narrow stances, the raising and lowering of arms for balance and so forth. Consequently, infants and toddlers contribute to their own development by controlling how much conscious effort they put into practicing or rehearsing movements and acquiring experiences that contribute to the integration of their behavior.

The Bi-directionality of Brain Growth and Behavior

Perhaps McGraw's most important but least understood contribution to knowledge about infant growth was her conception of the reciprocal relationship between brain and behavior that occurs in early development. Gilbert Gottlieb (1998) contended that McGraw can take credit for tentatively formulating a *bi-directional* theory of brain structure and function that now has become the hallmark of his influential research (see Gottlieb, 1992; 1997). She proposed that neural structures and behavioral functions interact throughout early development. She likened this process to the interweaving of separate threads to form an integrated tapestry or repertoire of motor and cognitive skills. It is noteworthy that she first employed this metaphor in *Growth,* which was published four years *before* Gesell (1939) advanced his own theory of the "spiral organization of reciprocal interweaving" to explain infant development.

McGraw undertook her studies of locomotion when Le Roy Conel, a Harvard neuroanatomist, was just getting underway his over two decade long, eight volume research project to trace the development of the newborn brain to the age of six. Although they were unable to personally collaborate, Conel (1939) found evidence that supported McGraw's theories involving the reciprocal relationship between brain growth and behavioral development but his studies were overlooked until recently. By using more sophisticated techniques than available to Conel. Shankle et al. (1998) and his co-authors recently corroborated Conel's findings from birth to six years, thus lending additional support to McGraw's bidirectional theory

of development. This is an important milestone in the science of development that now makes it possible to chart more precisely the correlative changes in brain and behavior that occur from infancy through adolescence and beyond and to understand why neurobehavioral development varies considerably among children and adults.

McGraw (1943) was the first scientist in her era to identify key brain-behavior relationships to support her contention that experience is required to form and activate neuronal connections that link separate areas of the brain. She determined that at birth the rhythmic flexion and occasional spinal extension commences the process of prone locomotion. The most active reflexes are found in the hips and lower extremities. Conel (1939) determined that at this point that Betz cells were more numerous and showed extensive myelination (a sheath or lining around axons that enables transmission of signals) in the middle one third of the posterior wall of the anterior central gyrus. By about three months, McGraw observed that movement in the upper trunk, shoulder, and arms is more pronounced, as rhythmic activity in the lower body diminishes and becomes quiescent. Conel's (1947, p. 147) studies indicated a correlative development in the brain, as the posterior central gyrus supporting these movements was in the most advanced stage of development. However, by 6 months, Conel (1951, pp. 175–176) found that the premotor cortex assumed a dominant role in which the lower trunk, hands and feet were more active, while movements in the upper body were inhibited. This was consistent with McGraw's (1941, p. 93) findings that infants tend to push with their feet and pull with their hands at this stage of prone progression. Conel (1955) subsequently determined that by fifteen months these separate movements become well integrated memories in the hippocampus and cerebellum.

DEWEY'S STAKE IN McGRAW'S RESEARCH

The Emergence of Consciousness

Dewey was particularly interested in what McGraw's research would reveal about the emergence of mind and consciousness and role of judgment in inquiry. McGraw's evidence appeared to indicate that consciousness emerges through behaviorally and biologically mediated neuroanatomical and neuromuscular processes of reorganization that support the transition from minimal consciousness to self-consciousness. According to this scheme (see Figure 1), minimal consciousness is attained when an infant is able to sit up. Object consciousness or the awareness of the difference between self and other occurs with the act of reaching and pointing.

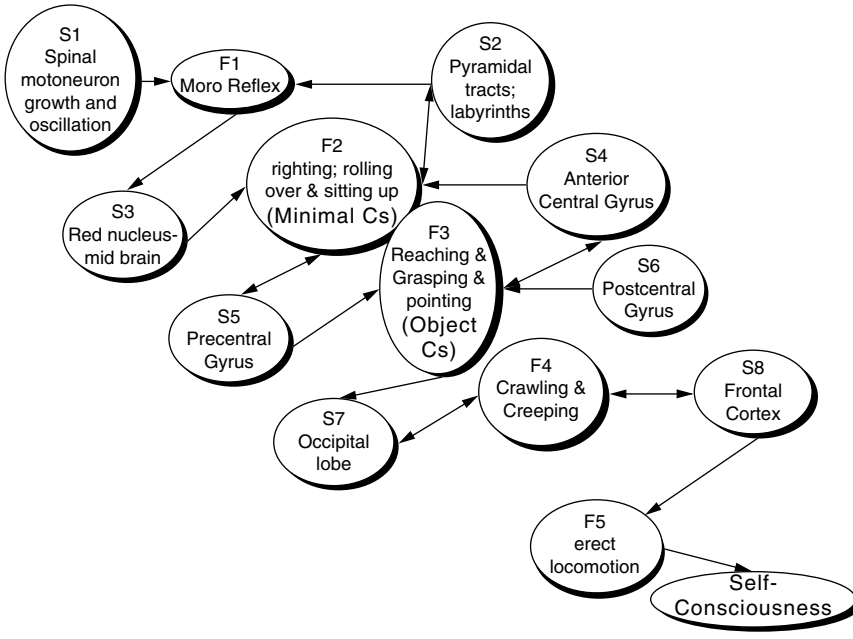

Figure 1. McGraw's Neurobehavioral Theory of Development and Consciousness

Motoneuron growth and oscillation along the spinal column commences a sequence of behaviorally mediated neuronanatomical and neuromuscular reorganizations that sustain successive forms of conscious activity, culminating in erect locomotion and self-consciousness, as depicted in Figure 1. Each functional pattern (F) constitutes the center of gravity of neural structures (S) undergoing differential growth (i.e., axons and dendritic connections), as illustrated by the neural complex S2–S5 encircling F2. Reciprocal interactions occurring, for example, between F4 (crawling and creeping), and neuranatomic structures S4 and S7 enable antecedent structures to influence subsequent patterns. Consequently, erect locomotion at F5 entails re-solving the problem of balance by drawing on the energy of consciousness generated by antecedent stages of neuromuscular development occuring previously.

Self-consciousness emerges sometime after the mastery of erect locomotion, when there is an explicit recognition of a causal relationship between self-initiated movement and the movement or manipulation of objects. McGraw found that the mastery of erect locomotion never presents exactly the same problem for each individual, because toddlers must

re-solve the problem of balance encountered during previous stages of neuromuscular development, the experiential circumstances of which vary considerably between children. Infants and young children do not attain conscious awareness and a conception of self at the same rate nor do they employ these tools the same way in learning processes because of differences in attitude, experience and neurobehavioral development.

Implications for Inquiry

The importance of McGraw's principles of development in Dewey's analysis of judgment in *Logic*: *The Theory of Inquiry* in 1938 cannot be overstated (Dalton and Bergenn, 1996). The distinctive pattern of development McGraw discovered furnished analogies that Dewey used to further elaborate the structure of judgment and function of inference in inquiry. These principles led Dewey (1986, p. 197–199) to argue that reasoning did not conform to invariant and non-reversible rules of inference, identity and contradiction. Scientific inquiry followed an indirect course, involving the formulation of functionally specific hypotheses that reflect the contingencies of interacting factors. The inferential force and generality of hypothetical propositions will depend on how well specified are the conditions that will result in the transformation of one kind of thing or event into another with a wider or narrower applicability depending on the direction of the argument.

McGraw's studies of infant locomotion furnished the evidence Dewey sought to support his argument that the focus and integrity of inquiry, like the equilibrium of the organism, is maintained despite the ever-changing dimensions of the problem under study (see McGraw and Breeze, 1941). For example, babies encounter many unexpected contingencies (i. e., furniture, slippery surfaces, etc.) that become limiting conditions that weaken the force or generality of their initial judgment that leads them to attempt to retrieve a rattle across the room. These contingencies affect the order in which they do things to get the rattle. Consequently, the methods involved in retrieving a rattle or attaining erect locomotion are not constant or preset but are contingent on the context in which they occur and the timing in which they are integrated into an existing repertoire of behavior. This example illustrates why Dewey (1986, pp. 452–453) argued that isolated facts make inferences possible only after they become interrelated or correlated through the positions they occupy within a whole series of propositions, giving inquiry a "cumulative force" towards a unified conclusion. Thus successful inquiry, according to Dewey, is demonstrated by whether the momentum from the measured steps taken to understand and overcome a problem by rearranging the relationships among its elements enables the

analysis to move forward without breaking stride or forfeiting previous attainments.

The Drawbacks of a Prominent Mentor

Developmental psychologists learned only recently about McGraw's close personal and professional relationship with John Dewey and the larger scientific context in which her studies took place (Dalton and Bergenn, (1994). Nevertheless, contemporary psychologists have questioned why they chose to keep their relationship confidential. For example, Janette Benson (1997) was surprised that one of McGraw's research associates believed erroneously that John Dewey had actually originated her study. She urged McGraw's biographers to reconsider the attitudes that prevailed at that time toward women scientists that led to McGraw's belated recognition. This is a legitimate concern and question but one whose answer is not straightforward. Dewey's first wife died in 1927, before Dewey's intense personal and professional relationship with McGraw began. However, Dewey remained sensitive to the issue that she was a former graduate student and that he was expected to fulfill the role of mentor. Dewey may have been more comfortable assuming a father-daughter relationship in his mentoring, as McGraw's comments attest, because of his ongoing collaboration with his daughter Evelyn. Dewey viewed scientific collaboration as a non-exclusive endeavor and actively sought the involvement of other colleagues in McGraw's research.

But maybe an explanation for Dewey's silence about his involvement in McGraw's studies, including his foundation role and numerous scientific acquaintances lies elsewhere. The dilemma Dewey faced in bringing science to bear on philosophic issues of utmost personal interest was the near impossibility of successfully accommodating the expectations and norms governing conduct associated with different roles without bringing them into conflict. Freud faced similar difficulties in gaining scientific acceptance of his controversial ideas about child development that put psychoanalytic orthodoxy in jeopardy. Dewey attempted to fill diverse roles as an academic philosopher, a foundation official, a mentor, a scientific collaborator, and a reformer. It is doubtful that he or anyone else could successfully reconcile *all* the conflicting demands imposed by these roles without the support and deference of incumbents from these different institutions. Dewey's visibility as a social reformer enabled him to strike an effective balance on the psychological tightrope between engagement and detachment because his agenda was no secret and no one challenged his intentions or sincerity.

Dewey's unrivaled reputation as a "public" intellectual perhaps made it possible for him to cross institutional boundaries that were blocked to others, because he lent prestige and legitimacy to the people and organizations with whom he was involved. But by collaborating with a former student, Dewey risked compromising the distance and reserve of a mentor and the objectivity of a scientist. Had Dewey allowed his involvement to become publicly visible, it is quite possible that this would have led the public to focus on Dewey's celebrity (and conceivably his conflict of interest as a trustee of a foundation supporting her work) and desire for posterity and discount McGraw's independence. This also risked diverting public discussion into the nature versus nurture controversy engulfing McGraw's research whose scientific basis was unresolved.

By acceding to his wishes for confidentiality, at least within Dewey's lifetime, McGraw gained unique insights in exchange for the notoriety that their relationship would have attracted. Psychologist Linda Acredolo (1998, p. 154) speculates that the secret to McGraw's success in a male dominated scientific era may have been her insatiable curiosity and "love with learning," a love that was so intense that she neglected to worry about anything else. McGraw demonstratively possessed a love for knowledge, an attitude that Dewey imparted, which may explain why McGraw called Dewey her "intellectual godfather."

GESELL AND THE MATURATION CONTROVERSY

McGraw attracted considerable public attention despite her confidential relationship with John Dewey. According to Paul Dennis (1989; 1994), no other psychological research was covered more continuously by the press and popular magazines than McGraw's experimental studies of Johnny and Jimmy Woods. The debate over whether behavior was influenced more by heredity or environment, fueled by the rivalry between John Watson and Arnold Gesell, aroused a palpable sense of expectancy among the public that McGraw's research would decisively resolve the issue (Dennis 1989). Given this atmosphere, it is not surprising that McGraw's most significant discoveries were overshadowed, because they could not be fit easily into the dichotomy of nature or nurture in development. McGraw complained years later (a fact confirmed by Dennis, 1994) that the press reported incorrectly that she had "conditioned" Johnny to enhance his performance and the gains were only temporary, thus supporting a maturational perspective. Disappointingly for McGraw, these reports found their way into textbooks with lasting effects.

Rivalry

Although Gesell and McGraw sought to transcend in their research the narrow confines of the nature-nurture debate, they were engulfed by it, contributing to their rivalry and diffidence. As a student, McGraw respected and admired Gesell's (1934) work. (McGraw 1926) came away from her first visit at Gesell's clinic in 1926 "with a renewed professional enthusiasm," reminding Gesell that, "It is after all this exchange of ideas that makes the wheels go round." McGraw (1927) followed up her visit the next year by unsuccessfully seeking employment on Gesell's staff. However, once McGraw assumed her position on the NCDS, she found it increasingly difficult to maintain cordial relations with Gesell. Gesell was justifiably put off by Tilney sending his subordinate, McGraw to review his experimental procedures (see McGraw 1972a), but acted unfairly by never citing any of McGraw's research—a slight that disappointed and annoyed McGraw—unwittingly contributing to the continuing confusion regarding their respective views (McGraw 1972a). Moreover, McGraw resented Gesell's medical degree that opened doors and commanded respect denied her as a female psychologist. Despite these personal differences, McGraw never publicly criticized or challenged Gesell's ideas that would have clarified their theoretical differences.

Coghill's Pivotal Role

The confusion surrounding McGraw's findings stemmed primarily from the fact that she and Gesell were strongly influenced by Coghill's research, but they interpreted it differently. Gesell believed Coghill to be saying that neural maturation must occur prior to behavior, while McGraw thought that Coghill viewed brain and behavior as reciprocally related through experience. Unfortunately for McGraw, Coghill, who also knew Gesell, never took an explicit position on the experience versus maturation debate, as Oppenheim (1978) points out, even though privately he did not agree with Gesell's views and considered early experience formative in neural development (Dalton, 2002a). Gesell considered the growth of the brain to be a maturational process controlled by genetic factors.

Coghill's claim that neural structures anticipate the acquisition of function no doubt strengthened Gesell's conviction that this was a correct interpretation of Coghill's theory. However, Coghill never used the terms 'maturation' or 'genes' to characterize neural growth processes, as he considered this to be an issue that could only be resolved by further research. (In fact, if Coghill's mind were made up on this issue, he would not have bothered to get involved in McGraw's research). In his eagerness to get

Coghill's approval, Gesell incorrectly interpreted and cited Coghill's findings as supporting his position when, in fact, no such agreement existed (Cognill, 1934). Esther Thelen (1987), an infant experimentalist (see below) has accepted this illicit appropriation as evidence that McGraw was essentially a maturationist even though the historical evidence speaks to the contrary.

Gesell also wrongly assumed that he and Coghill were in agreement that early stimulation does not accelerate the development of motor functions. Gesell & Thompson (1934, p. 309) observed somewhat tentatively that "Coghill doubts whether the appearance of a function like locomotion can be hastened in *Amblystoma* by exercise." Although this may have been true for an animal, which moves sparingly prenatally, Coghill did not rule out the possibility that experience hastens locomotor development in humans. He cited evidence that nerve cells differed in the level of sensitivity to excitation, and that dendrites grow in the direction of the sources of stimulation and continue to grow during adult life. He also stated that "the experience of the individual is a factor in determining the specificity of function of the constituent neurons" (Coghill, 1929, p. 98). But perhaps his most definitive statement about early experience was the following:

> It would be difficult to prove that the extension and perfection of particular patterns of response are not facilitated by its performance—for the functioning of growing neurons may facilitate or excite their growth and thus the perfection of the pattern may be hastened through its exercise" (Coghill, 1926, p. 132).

Disputing Cortical Influences

Gesell also believed that this process by which behavioral traits attain greater complexity began solely through mechanisms of reciprocal enervation and inhibition at the subcortical level. He did not think that these attainments involved cortical influences or entailed deliberate or purposeful control. Unlike Gesell, McGraw attempted to find out whether cortex is involved in early development. She found evidence that cortical inhibition is not only essential to voluntary action, but is indispensable to the formation of neuronal connections necessary for the coordination of feeling, movement, thought, and action. McGraw believed that cortical inhibition spreads selectively by alternating between upper and lower regions of the body, enabling infants and toddlers to gradually gain increased control over the amount, sequence and direction of their movement. As cortical functions spread, new behaviors emerge, creating new centers of dominance and the exaggerated or excessive exercise associated with the

acquisition of a new capability. Today, the preponderance of evidence from infant studies using EEG and Event Related Potentials indicates that cortex is involved from birth in motor and perceptual development (Bell, 1998; Johnson, 2000; Thatcher, 1997; Zelazo, 1998).

McGraw and Gesell's experimental designs and methods also differed fundamentally. For example, Gesell & Thompson (1934) began their observational studies at 4 weeks rather than from birth. They did not introduce special training until the experimental twin was 11–1/2 months (and after 14 months for the control twin) compared to 20 days for Johnny and Jimmy (see McGraw 1975). (Jimmy was not afforded special stimulation but he was tested in the same activities at the same interval as Johnny.) Gesell & Thompson (1934) also used repetitive training, administered tests that heavily emphasized vision, perception, and memory, and conducted no follow-up studies. Gesell's contention that training neither alters the sequence nor accelerates the processes of skill acquisition, and that delayed practice results in greater gains should not be surprising given these differences in methodology. The twins in Gesell and Thompson's studies had already attained some degree of sensori-motor development at the onset of their respective experimental training and testing programs.

CONTESTING McGRAW'S RESEARCH

Was McGraw a Maturationist?

McGraw's theoretical perspective has not been clearly understood by her successors. While acknowledging Gesell and McGraw's pioneering roles, infant researcher Esther Thelen, among other psychologists, has been critical of their work. These criticisms stem from honest differences in theory and methodology and different understandings of the historical context. In reflecting on their contribution, Thelen (2000, p. 388) contended that once major motor milestones were catalogued, "There seemed to be little left to do. Moreover, both Gesell and McGraw's theoretical positions appeared to lead to dead-ends in terms of further empirical studies." Gesell and McGraw's research can hardly be singled out as having brought about the death of motor studies, as their ideas never received a proper burial. Therefore, a close scrutiny of her portraits of these pioneers (especially McGraw) is in order to set the record straight.

Thelen argued that McGraw adopted a "more prescriptive neural-maturationist model" than Gesell and that only "Gesell's ideas are worthy of serious consideration," because she contended that Gesell anticipated

dynamic systems theory by emphasizing the self-generating patterning of behavior (Thelen, 1989, p. 10). More recently, Thelen (2000, P. 387) expressed agreement with this author (see Dalton, 1996) that "McGraw's legacy as a maturationist oversimplifies her more sophisticated view of development." But she still blames McGraw for having "put the role of maturation into the forefront." To be certain, McGraw pioneered methods that led to her important discoveries regarding early locomotor development. Thelen, Smith and their colleagues have benefited from her insights to make their own unique contributions to the science of early motor development. Developmental psychologists' understanding and assessment of McGraw's work is undergoing revision however, due to the belated but well deserved recognition as a scientific innovator as well as being Dewey's protégé and collaborator, (see Dalton and Bergenn, 1996; Dalton, 2002 b). This will undoubtedly contribute to a better appreciation of the broader historical context and significance of her work.

The Interaction of Structure and Function

There is one disputed point that deserves clarification, however, because of its importance to understanding McGraw's work as a whole. Thelen claims that McGraw's assertion that infant behavior does not become fully integrated until after the onset of cortical control is irrefutable evidence that she was a maturationist. She interprets McGraw as saying that the "immediate causes of new forms of behavior . . . were reorganizations in the nervous system as it matured and in a systematic and predictable fashion", and that therefore, according to Thelen, McGraw believed that "function emerged from structure and not the reverse" (1987, p. 6). Thelen (1996, p. 553) concludes that McGraw was unable to reconcile the 'tension' between her maturationism and experientialism. This interpretation is incorrect. McGraw (1943, p. 4) explicitly acknowledged that, "the problem of developmental or maturational relations between structure and function is more complex than the question of localization of function." McGraw was never divided on the relation between structure and function; she always considered them to be reciprocally related. Nor did McGraw ever argue that cortex 'caused' or 'determined' motor development. She neither observed nor posited a one-to-one relationship between brain structures and behavior (Dalton & Bergenn, 1998b). Instead, she contended that there are a plurality of neural structures supporting behavior, each of which overlap and influence behavior at different periods of motor development (see Figure 1).

Thelen (1987:13) believes that "neural maturation alone is an impoverished basis for a developmental theory" (Thelen 1987, p. 13). She contends

that her dynamic systems theory offers a more "parsimonious" account of developmental behavior because no *post hoc* references to pre-cognitive states, neural remodeling or programming, or fetal behavior is needed. She also wants to eliminate any references to developmental brain states, consciousness, or intentionality in her ambitious but incomplete line of inquiry. Admittedly, mind and consciousness remain controversial subjects. But there is mounting evidence from neuroimaging studies that the human brain supports conscious states of mind. (See Dalton and Baars, this volume). McGraw adopted the position that that brain and behavior interact–a position that Thelen also accepts. But McGraw also believed that consciousness influences how they are integrated and changed by experience.

The Significance of Early Stimulation

The Emergence of Consciousness in Motor Development

Philip Zelazo has been willing to take a fresh look at McGraw's evidence and engage her at a methodological rather than conceptual level. His studies bring out more clearly the formative influence of conscious experience and the importance of stimulation in early brain development and behavior. Zelazo's studies provide a true appreciation of McGraw's seminal contributions about brain and behavior while underscoring the weaknesses of Thelen's conception of mind. Zelazo et al. 1972) showed that walking is accelerated through early stimulation. Zelazo (1983) also found that the simulation of infant stepping and kicking movements enhances perception and memory–capacities conducive to the subsequent attainment of erect locomotion. While agreeing about the positive effects of stimulation, Thelen (1983) denied that the attainment of erect locomotion involves deliberation or purposeful behavior arguing that kicking and stepping are stereotypical movements made possible solely by the removal of gravitational constraints.

Subsequent studies by Zelazo et al. (1993, p. 690) and his colleagues persuasively demonstrated that stimulated stepping also contributes to the emergence of consciousness within the first year. Infants who received this motor stimulation were able to control their movements willfully and deliberately. Zelazo credited McGraw with this initial discovery:

> Clearly the neonate enters the world with a vast repertoire of primary reflexes and more complex reflexive neuromotor pattern. If left unstimulated, many of these reflexive behaviors seem to become disorganized and disappear. This is not disputed; it is mostly the role of experience in this process that appears to have been undervalued. Moreover, our data more radically assert a higher level of control than anticipated

> previously by McGraw or others–even higher order control of reflexive neuromotor patterns . . . . It appears that the role of thought in action not only emerges at the end of the first year of life, as McGraw implied, but appears to direct the body ever more consciously from that point on (Zelazo (1998, p. 468).

Corroborating Studies of Early Experience

Unfortunately for McGraw, Zelazo's corroborating studies did not appear until the 1980s and 1990s, after years of misinformation had taken a heavy toll. Yet evidence was already mounting from studies of animals in the 1960s that indicated that McGraw was correct in her belief that enriched early experience alters brain development. Marc Rosenzweig et al. (1962; 1963) and his colleagues were the first neuroscientists in the early 1960s to find neurochemical and neuroanatomical evidence that experience changes brain function. Although vital to McGraw's studies, Rosenzweig's findings were not widely disseminated nor were they accepted by other scientists until Hubel and Weisel (1970) published the results of their studies that kittens deprived of vision showed a substantial reduction of cortical dendrites in the visual receptive field. Their studies emphasized the primacy of critical periods and mired scientists again in debates about genetic influences and the negative effects of deprivation, thereby detracting attention from the life-long positive brain effects of enriched experience (see Bruer and Greenough, 2001).

Studies mounted in the mid-1960s by Greenough (1976) and his colleagues led to an important distinction between *experience-expectant* and *experience-dependent* mechanisms of brain development that has enabled researchers to disentangle genetic and environmental influences and to reformulate the notion of critical periods (Greenough et al., 1993). While some brain growth occurs "expectantly" in advance of function, complex neural connections depend on experience for their exact configuration and performance, thus underscoring the *reciprocal* relationship McGraw posited between brain growth and behavior. Greenough and his associates also found that young rats exposed to enriched experience showed significant differences in brain growth compared to rats with normal experience. Black and Greenough (1998) identified specific changes in synapse growth in the dentate nucleus of the hippocampus of rats with enriched experience that were absent in rats with normal experience. Importantly, animal researchers have found that repetitive motor activity alone does not produce neural reorganization; skill acquisition involving motor attention and learning is necessary to tap neural plasticity and incur change (Plautz et al., 2000). This same finding that attention is the key to neurobiological change also has been demonstrated in human adults (Schwartz & Begley, 2002).

BRIARCLIFF AND BEYOND, 1950–1970

McGraw never had the opportunity, while conducting her research, to teach doctoral students in psychology at Columbia University to sustain her line of inquiry. Her collaboration with physicians and distinguished scientists, although critical to her important discoveries, did not lead to recognition in pediatric medicine. Disappointingly, Rustin McIntosh, Director of Babies Hospital during the 1930s and 1940s failed to credit her with key insights about infant growth processes and generally cited Gesell and other medical authorities (Weech et al., 1960). Gesell attained prominence as a medical authority in pediatric textbooks while McGraw's work was completely ignored. McGraw's book, *Growth, A Study of Johnny and Jimmy* received only one review by a female physician (Sherbon, 1938) that was published in *The Medical Women's Journal.*

Swimming Against the Freudian Tide

McGraw made the best of her extensive laboratory experience and her ability to communicate effectively about early development, when she was appointed in 1953 to teach undergraduates at Briarcliff, a women's college that closed in 1974. McGraw (1971) recounted using the infant laboratory to help students cultivate an "intuitive sensitivity" to infant development by correctly interpreting their pre-linguistic "signals" of growth. The observational protocols she used at Briarcliff were adapted from a "Mothers Manual" that McGraw drafted nearly two decades earlier. McGraw attempted unsuccessfully to publish the manual in the late 1930s as a popular guide for parents that would have predated Dr. Benjamin Spock's (1946) enormously successful book on baby care by several years. Spock adopted a Freudian stance that parents be "permissive" but consistent disciplinarians, who should never put any conditions on the love of their children. Ironically, McGraw's "Mother's Manual" was intended to restore self-confidence in child rearing that she believed Freudian psychology had taken from parents, by encumbering them with deep-seated anxieties and feelings of inadequacy about the love and care for their children.

McGraw did not share her colleagues' awe and curiosity for psychoanalysis and her negative attitudes probably were responsible for her stormy internship at the Columbia Institute for Child Guidance headed by Freudians David Levy and Lawson Lowrey (McGraw 1972a). McGraw was not opposed to exploring the clinical implications of growth processes, despite her misgivings about Freudian psychology (including Gesell's age-based norms). She believed that clinical judgments should

be drawn cautiously and based on a complete understanding of the complex interdependence of neural, behavioral, and environmental factors in development.

Extending Her Influence

Bert Touwen (1976), a developmental neurologist, took McGraw's reservations seriously that knowledge about abnormal functions depends crucially on understanding *normal* development. Touwen's (1971; 1976) research corroborates McGraw's discovery that normal infant development is marked by extensive variability in the timing and attainment of motor functions. Touwen (1978; 1994) contends that it is the capacity to vary rather than the ability to approximate a norm, as Gesell proposed, that is diagnostically significant for development. Touwen (1993, p. 11) argued "that the lack of ability to vary will inevitably lead to a lack of adaptive (both active and passive) capacities," singling out cerebral palsy, which Touwen observes, involves "the lack of sufficient and proper strategies that has led to non-adaptive and stereotyped motor behavior." Focusing on the human capacity for variability and its relationship to brain development is likely to better reveal the enormous resources for novel changes in the phenotype despite the relative outward similarity of human form and functions (Touwen, 1998).

McGraw also had the occasion to compare notes with developmental psychologist Lewis P. Lipsitt in the late 1970s regarding the timing and potential factors contributing to sudden infant death syndrome. Lipsitt (1979) contended that the period in early infancy (between 2 and 4 months) when transient reflex functions are waning with the onset of cortical control, as McGraw demonstrated, poses for some infants a special period of jeopardy and risk. Burns and Lipsitt (1991) argued that sudden infant death syndrome and perhaps, the failure to thrive both involve inadequate postural control and respiratory occlusion. These symptoms suggest possible deficits in neural functioning combined with deficient environmental supports (i.e., a lack of motor stimulation) compromising the ability of these infants to fully develop adaptive behaviors. McGraw concurred with Lipsitt that the period of special risk coincided with a critical period during infancy when the attainment of balance is instrumental to the mastery of motor activities such as sitting up and crawling. The susceptibility to sudden infant death can be measurably reduced, according to recent evidence from controlled longitudinal experiments, by introducing changes in sleeping posture. Babies who were placed on their backs while sleeping were less likely to be victims compared to infants who slept on their sides or stomachs. (Dewey et al., 1998a; Dewey et al., 1998b).

PROFESSIONAL BLUNDERS AND REENGAGEMENT

Renewed Interest in the1970s

McGraw remained inactive professionally in the field of child development from the late 1950's until 1976, when she accepted an invitation to receive a major award from the Society for Research in Child Development. McGraw was encouraged by this renewed interest that was sparked by the publication of her critical reflections (see McGraw, 1970) on the field of child development. Consequently, she undertook a remarkable strategy in the late 1970s, when research in infant motor development was perhaps at it lowest ebb, to revive interest in her work by confessing her professional blunders in an academic journal. She believed that her regrettable errors in her professional career may have contributed to needless confusion about her ideas and clouded the prospects of a science of growth. She eventually published the essay, "Professional and Personal Blunders in Child Development Research" in the *Psychological Bulletin* in 1985.

McGraw sent a draft of her "Blunders" article to several distinguished scientists that included: psychologists Lewis Lipsitt, Jerome Bruner, Gilbert Gottlieb and Berry Brazelton; neurobiologist Ronald Oppenheim; and Gerard Piel, founding publisher of *Scientific American*, among others. (Lipsitt, 1990; 1998 was instrumental during the 1980s in reviving an interest in her work among contemporary researchers). Although many cautioned McGraw against this approach, her colleagues considered her candor to be courageous, refreshing and likely to stimulate further introspection among researchers. McGraw succeeded in recapturing the attention of contemporary scientists while also unintentionally resurrecting the nature versus nurture debate that her earlier research sought to overcome.

Confessing Her Mistakes

In her "Blunders" article, McGraw singled out three episodes that are particularly relevant to the attempt in this book to understand why prominence and recognition carry special and sometimes unforeseen, intellectual burdens for those who advance novel ideas. First, McGraw admitted having the bad fortune of using the term "critical period" to characterize phases of behavioral transition when infants are particularly ready to respond to new challenges or experiences that advance their development. Embryologists first used the term "critical period" to denote special periods during early neural development when stimulation or its absence were crucial for further development. McGraw failed to make clear that she was not using the term to mean that the lack of stimulation would

entail deprivation. Instead she meant that there are many opportune times for motor or cognitive advancement in childhood in which stimulation can benefit performance. As noted before, developmental psychologists have tended to assume that critical periods are limited to early development when the brain is believed to be most plastic and that stimulation forgone during this time has negative consequences. Mounting neuroscientific evidence however, now indicates that many brain regions retain plasticity throughout life and that experiences forgone, in most instances, do not carry irreversible effects.

The stage theories popularized by Jean Piaget and Gesell contributed to the belief among psychologists that critical periods were time and phase locked events. McGraw did not subscribe to the stage theory conception of critical periods, arguing that developmental sequences are not preset but rather determined by the contingencies of experience. (For example, many infants walk without first having crawled.) She introduced special stimulation (i.e., challenging situations requiring ingenuity and coordination) during periods of motor transition that were intended to change the expected sequence by bringing about a more effective integration of new and old skills. Contemporary neuroscientists have used these techniques of special stimulation to help individuals overcome learning disorders by enabling them to reorganize somatosensory neural maps that alter the way they process and integrate sounds and visual cues (See Merzenich and Jenkins, 1995; Merzenich, 2001).

McGraw says that another "colossal" blunder that cost her dearly was her decision not to accept an invitation to submit a revised version of a chapter on 'the maturation of behavior' that was published in the first edition of Leonard Carmichael's *Manual of Child Psychology* in 1946. Her persuasive synthesis of the literature on early experience and the brain in the first edition was well received and influential. McGraw (1994, p. 63) recalled that the primary reason that she turned Carmichael down was because she mistakenly believed "that the disastrous dichotomy of maturation and learning was untenable and that perhaps it should not be re-aroused." This well intentioned but mistaken judgment not only offended Carmichael, but fueled the mistaken belief years later that Carmichael deliberated deleted the chapter.

In declining Carmichael's invitation, McGraw forfeited the mantle of intellectual leadership that was sorely needed during a period when motor studies were theoretically stagnant and researchers were struggling to define alternatives to Gesell's maturationism. McGraw could have rekindled old acquaintances with brain researchers, such as Conel, and become acquainted in a more timely way with Gilbert Gottlieb and Kuo's and Greenough's seminal studies of experience-dependent developmental

processes. She could have also established new contacts among leaders in the emerging field of neurobiology, such as Victor Hamburger and Rita Levi-Montalcini, who made important discoveries in the late 1950s about neural growth processes that led to the Nobel Prize for Levi-Montalcini (Oppenheim, 1994). More than anything else, McGraw deeply regretted her lapse of involvement for intellectual and professional reasons and tried hard to rectify it in her last years by addressing theoretical issues in journal articles and by cultivating new acquaintances among psychology's leading scientists. Through these efforts McGraw succeeded in reviving an interdisciplinary interest in her work among respected scientists and scholars who continue to find fresh insights from her work that challenge previous understandings.

CONCLUSION

This case study of Myrtle McGraw vividly illustrates that recognition as a pioneering scientist is problematically intertwined with how a body of ideas is understood and interpreted among a community of scientists over time. There are many obvious professional factors that could explain why McGraw experienced difficulty in her lifetime that center on the controversial and disputed nature of her discoveries and findings. Her gender was a liability during that era, she had no doctoral students to sustain her research methods and she failed to remain active in professional organizations. But McGraw was also a victim or her own farsightedness. She experimentally probed the effects of early stimulation on brain growth and behavior when there was little neurobiological evidence. Only years later has brain science caught up and vindicated her claims.

Trapped in the Nature Versus Nurture Debate

Importantly however, McGraw was ensnared in a theoretical debate, which predated her work. The nature versus nurture dichotomy confined the universe of theoretical discourse about development to well-defined opposing positions involving either hereditary or environmental determinism. McGraw defined development as involving the interaction of brain and experience, but many psychologists have construed neural growth as primarily a genetic phenomena and thus interpreted the brain's relationship to behavior as a causal one involving the primacy of neural maturation. McGraw was strategically disadvantaged because Gesell had already advanced his views about the brain before her work was published, and he instilled the belief that the only alternative to behaviorist

environmentalism was maturationism. McGraw's admitted failure to publicly challenge Gesell's theory reinforced the perception among psychologists that they were advancing essentially equivalent rather than rival theories. Engaging in a theoretical debate, although risky for a woman at that time, would have provided McGraw a timely opportunity to define her key terms more clearly and to better establish the significance of her findings, than attempting to do so decades later.

There is also evidence of an intellectual displacement that deserves discussion because it compounds the problems of interpretation that a body of work undergoes over time. McGraw and Gesell's mutual acquaintance with neuroanatomist George Coghill, whose ideas strongly influenced them, is problematic because so much depends on how Coghill's work is understood. This has the potentially adverse consequence of displacing the proper focus of attention on the prominent theorist and dwelling speculatively on perhaps unsolvable secondary influences. A beneficial side effect of renewed interest in an historical figure whose influence was overlooked may be that person will gain new importance and relevance. The down side is that the ideas of those singled out for their intellectual influence may be used to discredit the work of scientists who owe them an intellectual debt.

Intellectual Displacement

This phenomenon of intellectual displacement may be an unavoidable aspect of historical scholarship that is driven by the endless but often fruitful search in archives for new information that reveals previously unacknowledged influences and relationships. The scholarship on Freud took a different turn when new details about his intellectual relationship with (and potential debt to) Wilhelm Fliess turned up in their voluminous correspondence. Similarly, the revelation of McGraw's collaboration with John Dewey may contribute to a renewed interest in how this and other mentoring relationships contribute to a scientist's intellectual development and their debt to students and colleagues who collaborated in their research.

Reestablishing the Dewey Connection

Moreover, the continued disclosure of undocumented influences among prominent thinkers underscores that ideas have a tendency to become the possession of more than one mind and that originality may have more to do with synthesis than with invention. Dewey relied heavily on McGraw's studies and the research of many other scientists to fathom the interrelationship among ideas that Dewey integrated into an unrivaled

conception of mind and inquiry (see Dalton, 2002). I think McGraw forfeited a unique opportunity that many other Dewey scholars and former students seized; she failed to explicitly discuss the impact of Dewey's ideas on her research. Not only would this have attracted attention to her work, it would have forced the philosophers who monopolized the Dewey scholarship and psychologists who ignored Dewey's ideas, to take seriously his enduring interest in psychology and his conceptions of mind and inquiry.

This case also shows that while recognition requires merit, enduring prominence depends on professional and personal contingencies. McGraw attracted extraordinary media attention in the 1930s, but public awareness quickly faded and for several decades her studies attracted little attention or comment in scientific journals. Her reengagement in developmental psychology revived an interest in her contributions and achievements, but it also posed uncertain risks of being pulled back into nature versus nurture controversy that she desperately tried to overcome. When she did reengage with other scientists, she did so by acknowledging Dewey as her spiritual ally and intellectual mentor. Consequently, historians and psychologists are in a better position than before to evaluate Dewey and McGraw's scientific strategy for overcoming the nature-nurture conundrum. This debate is now receding as psychologists become aware of the burgeoning knowledge that humans possess the capacity to control their neurobehavioral development through experience.

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11

THE CONTEMPORARY RECONSTRUCTION OF DEVELOPMENTAL PSYCHOLOGY[1]

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> The essential truth is not the present fact but the entire process by which the present fact grew to be what it is.
>
> William Torrey Harris[2]

INTRODUCTION

Within the past 30 years, many developmental psychologists have moved away from a focus on cognitive development towards a cultural view of how children's mental development takes place. At the same time, not entirely by coincidence, there have been important new linkages between developmental psychologists and those who work on social programs and policies for children. I have been interested in both these trends of change in developmental psychology, and it seems most appropriate to consider

[1] Address given on the occasion of the Urie Bronfenbrenner Award, American Psychological Association Convention, Washington, August, 2000.

[2] "Editor's Preface, " in R. H. Quick (1890). *Essays on Educational Reformers*. New York: Appleton, p. ix.

them on this occasion, because Urie Bronfenbrenner was a leading figure in bringing both of them about.

Developmental psychology's new orientation towards social development and social policy is well under way but, nevertheless, it may be helpful to examine it historically. The contemporary reconstruction, I believe, has deeper roots and is more complexly organized than is immediately apparent. Some political revolutions look sudden but then a careful historical examination shows tbat the revolutionary changes grew out of slower and more evolutionary changes. (See, for example, DeTocqueville's (1955) classic analysis of the events in France that led up to the French Revolution and that, in fact, produced a number of the changes in French society that are usually attributed to the Revolution).

The contemporary reconstruction of developmental psychology is usually understood to have been begun by events of the 1960s. The brilliant Soviet psychologist, L. S. Vygotsky has been the major theoretical inspiration for the cultural view of child development. Though Vygotsky lived and died well before 1962, it was in that year that the first translation of Vygotsky's *Thought and Language* exposed Americans to the depth and quality of Vygotsky's thinking and began a rising tide of influence on American developmental psychology that remains strong today.

The movement of developmental psychologists towards work on social programs and policies was precipitated by the War on Poverty of the 1960s. There were, to begin with, foundation-sponsored demonstration programs of the 1960s—experimental preschools such as those of Cynthia and Martin Deutsch in New York City, Gray and Klaus's preschool and subsequently Susan Gray's DARCEE program in Nashville, Bereiter & Engelmann's preschool in Urbana, David Weikart's preschool in Ypsilanti, Michigan, to name a few of the more prominent efforts. These paved the way for federal programs for children such as Head Start, Follow Through, and the school-based projects funded under the Elementary and Secondary Education Act. Accompanying these educational innovations in time and spirit were the television programs for poor children, *Sesame Street* and *The Electric Company.* Developmental psychologists participated in the development of all of these programs[3] and those participations have changed developmental psychology itself.

My purposes in the present talk are twofold—first, to show that the reconstruction of developmental psychology that began in the 1960s stood upon an infrastructure that had been building for 100 years before that time and, second, to argue that we can today better manage the reconstruc-

[3] For a recent analysis of the several kinds of participation of developmental psychologists in the creation and management of Head Start, see White & Phillips (2000).

tion if we recognize the true complexity of its intellectual and institutional architecture.

SOCIAL CONCEPTIONS OF HUMAN DEVELOPMENT IN THE 19th CENTURY

We usually understand theory building in developmental psychology to have grown easily and naturally out of 19th century biology. After Charles Darwin completed his work on the origin of species, he turned towards the descent of man and studies of human development. He published a baby biography giving observations of his infant son and a monographic study of the expression of the emotions of animals and men. Around Darwin, there was much scientific work on comparative neurology, comparing the nervous systems of various species with each other. An evolutionary lineage of the human nervous system was constructed and from that lineage it was readily apparent that a rough recapitulationism can be found both embryologically and in the post-natal development of a baby's nervous system. Phylogenetically older parts of the human brain serving "lower" brain functions mature before phylogenetically newer areas serving "higher" brain functions. So a child in developing "intelligence" more and more fully expresses the hierarchical structure of its nervous system.

In the 19th century, the comparative psychology of George Herbert Romanes set forth an evolutionary lineage of animal behaviors (Romanes, 1883, 1884) and this analysis in turn led him to propose stages of mental development in children (Romanes,1889). Others saw the possibility of developmental stages. Similar stage schemes were proposed by Ivan Sechenov in Russia and James Mark Baldwin in the United States (White, 1983) and, for that matter, in the 20th century formulations of Freud and Piaget.

Comparative neurology and psychology pointed towards a fundamental orderliness in children's mental development but at the same time it tended to bias the study of such development, keeping it close to the methods, conceptualizations, and theoretical issues of concern to animal psychologists. How do children acquire all those things that animals do not have—language, culture, the sophisticated use of the objects, technologies, social institutions, and ethical codes of everyday human society? Such questions tended to be pushed to the side because of an intense interest in the question of how and how much humans differed from animals. But there was a distinguished group of late 19th and early 20th century American psychologists who wrote about the social character of human development.

Four psychologists of the late 1800s and early 1900s set forth powerful theoretical formulations about the social development of human beings: Josiah Royce, James Mark Baldwin, George Herbert Mead, and John Dewey. Josiah Royce is usually not thought of as a psychologist and ought to be. Royce wrote books about psychology, taught courses about psychology, and in 1902 was elected President of the American Psychological Association. He had a well-thought-through theory of the social origins of human motives, ethics, and self-conceptualization. His theory of human moral development (Fuss, 1965; Clendenning, 1999; McDermott, 1969) is, in my estimation, far more complex, sensitive and persuasive than the contemporary formulations of Piaget and Kohlberg. Royce wrote about psychotherapy in the preFreudian era. He had a direct influence on contemporary theorists of social development such as Baldwin and Mead and on individuals who would establish some of the significant social action of his time—Felix Adler, who would found the Ethical Culture movement and Elmer Southard, Bullard Professor of Neuropathology of Harvard Medical School, who would promote the professional practice of social work.[4]

The theories of social development of James Mark Baldwin and George Herbert Mead have been resurfacing in developmental psychology in recent years, and I would predict they are going to receive much more serious consideration in the future. John Dewey is well-remembered in education but not in psychology though he was a leader among the psychologists of his own time. Dewey was not, strictly speaking, a theorist of human development, but he was intensely interested in it. His writings about the Chicago Laboratory School contain sophisticated observations about what happens when the child tries to connect his or her life to the life of surrounding society. He was a silent partner with Myrtle McGraw in many of her classic observations of the neuromuscular development of the human infant. Dewey (1917) argued, as Mead (1909) had done before him, that American psychology departments ought to include social psychology. In the end he wrote an introduction to social psychology; he saw all human qualities as rooted in social situations and, in fact, indefinable without a consideration of the social situation in which they arise (Dewey, 1920).

Like Moses, John Dewey took American psychologists towards the promised land, but he never quite got to enter it himself. I suspect that when American psychology achieved a reasonably stable political organization after WWI, this organization permitted only a limited number of approaches to social psychology, those consistent with experimental

[4] Southard & Jarrett (1922), giving thanks to Royce, would set forth a curious good-vs.-evil approach to social work.

psychology and behaviorism—the kind of social psychology carefully and restrictively formulated in Floyd Allport's (1924) *Social Psychology*. Proponents of less-favored approaches to social psychology were gently pushed back towards philosophy or sociology. We have to consider what happened to American psychology in the 1920s and 1930s when the social developmentalisms of Royce, Baldwin, Mead, and Dewey began to be unremembered.

Can the important, cutting-edge thinking of one generation be unremembered in the next? Consider the interesting case of Pierre Janet. Henri Ellenberger (1970) devotes a large chapter in his *Discovery of the Unconscious*, to a review of Pierre Janet's substantial contributions to psychology and dynamic psychiatry in order, he says, to remind his readers of that work. Ellenberger (1970, p. 407) remarks that the work of Janet seems to have been visited by Lesmosyne, the goddess of oblivion, while Mnemosyne, the goddess of memory, favored that of Janet's great rival, Sigmund Freud. Perhaps Lesmosyne similarly visited our four theorists of human social development. Or perhaps there were some earthly reasons why they began to be forgotten in the 1920s and 1930s.

TRANSITIONS OF THE 1920s AND 1930s

The period of the 1920s and 1930s was a time of great change in American psychology. The standard story is that Psychology had been consciousness-centered but now it "went behavioristic"—going through, some would say, a paradigm shift. The turning of American psychologists towards behaviorism is generally understood as a positive response to John B. Watson's dramatic call for a behavioristic psychology. But this is a broad-brush interpretation of what was, in fact, a rather complex, extended, and confusing time of transition for American psychologists.

To begin with, there were a number of behaviorisms in the 1920s (Samelson, 1985). Most Americans were attracted by one or another methodological behaviorism rather than Watson's flamboyant ideological behaviorism. Such psychologists were quick to pick up the research methods originated by Pavlov and Thorndike. They responded to the scientific agendas, programs for the systematic study of behavior, put forward by Tolman, Hall, or Skinner. They took the formulations of the philosophers of science as prescriptions for the proper conduct of psychological science and, as we shall see, for the ultimate unification of American psychology. The majority of American psychologists participated in the building of what Sigmund Koch (1985) has called an Age of Theory, and that took a while to get going.

But psychologists were not alone in turning towards a conservative scientific program. A movement towards positivism is registered almost simultaneously in the histories of all the social sciences in the 1920s and early 1930s. The histories of sociology, political science, economics, and history generally do not mention this interesting coincidence. Such histories offer possible explanations depending on factors peculiar to each discipline, as do histories of psychology. But one can suspect that there were social and political factors outside the scope of any of the individual disciplines that encouraged the growth of relatively narrow, conservative scholarly or scientific programs. Something like a Boring *zeitgeist* was at work.

Within psychology, it should be noted, the movement towards behaviorism solidified slowly, and it came out of a period of extended uncertainty. The discipline of psychology was, curiously, winning on the outside and struggling on the inside. On the outside, there was growth, influence. There were growing numbers of psychologists in the university and, outside it, in work on education, personnel selection, industrial psychology, mental health, and child guidance (Napoli, 1981). The findings and conclusions of psychologists were being given serious consideration in public discussions of American society and government.

One example of this is to be found in some public intellectuals' discussion of the problems of democracy. Westbrook's (1991) biography of John Dewey traces the history of an attack on participatory democracy that seems to have been brought on by findings of American psychologists in the 1920s. The findings of massive intelligence testing in WWI said to some that many Americans were deficient in intelligence. (Newspapers would remark, "The average American has the intelligence of a 13-year-old. Psychologists familiar with the scoring of the Stanford-Binet will know the source of that remark. One news paper went a little further, saying "The average American is below normal in intelligence.") Freudian theory seemed to say that unconscious, instinctive, and irrational forces governed much of human thought. Behaviorism seemed to say that simple, mechanical, automatic mechanisms controlled much that human beings did. Studies of the effects of propaganda in WWI and of voting behavior seemed to show that a large number of people were susceptible to irrational, emotional appeals. Perhaps most Americans were not fit to govern themselves. Barnes (1924, p. 373) said that psychology

> has given scientific confirmation to the old Aristotelian dogma that some men are born to rule and others to serve, and makes it clear that we can have no efficient and progressive social system unless we

> recognize the real value of leadership and make it possible for the actual intellectual aristocracy to control society.

Elton Mayo (1923) suggested that most social behavior was determined by irrational factors, the "night mind", and that democratic politics only served to enhance the influence of the night mind on American social life.

Public intellectuals might make flamboyant political proclamations using the findings of American psychology. But most psychologists in the 1920s, like the members of a number of other disciplines, were pulling back from political prophecy and advocacy. The postures of "value-free science" and "dustbowl empiricism" were emerging. We do not remember today quite how vulnerable American professors used to be. At the turn of this century, American professors could be fired for being insufficiently religious, or of the wrong religion, or because their politics, patriotism, or morality were judged objectionable by someone above them, or simply because a college president wanted his salary for another purpose.[5]

The negotiations around the establishment of the American Association of University Professors in 1915 arrived at the beginnings of a bargain. Professors had a shield and were defended provided their public pronouncements arose out of their expertise and were representations of what they knew according to their scholarly and/or scientific methods. Professors were safe provided they spoke about matters that were within their own specialty. What this encouraged was narrowing, an enhanced

[5] One reason why "physiological psychology," the brass-instruments laboratory of Wundt and Titchener, became prominent at the beginning of American psychology was that it seemed to be safe. A letter of a friend of George Herbert Mead, Henry Northrup described Mead's decision to study physiological psychology in Leipzig in the following terms. *Physiological psychology is a science as yet very much in the air . . . and poor George was utterly at a loss to know how to begin. Every professor whom we visited here [Leipzig] had a different piece of advice. . . . We had given the whole thing up in despair. George thinks he must make a specialty of this branch because in America, where poor, hated unhappy Christianity, trembling for its life, claps the gag into the mouth of Free Thought and says "Hush, hush, not a word or nobody will believe in me anymore," he thinks it would be hard for him to get a chance to utter any ultimate philosophical opinions savoring of independence. In Physiological Psychology on the other hand he has a harmless territory in which to work quietly without drawing down upon himself the anathema and excommunication of all-potent Evangelism. You understand, of course, that I am not speaking as an enemy of Christianity or religion here. I am attacking the preposterous system by which the sects in America have taken possession of the higher education everywhere so that no mathematical, chemical or mineralogical fact can get into the world, and come in contact with susceptible youth, without having received the official methodistical or congregational pat on the back. It has ruined higher education in America, or, to speak more exactly, has prevented there being any philosophy that can breathe that prison atmosphere and live. It yearns for the mountain tops and unobstructed vision.* (Miller, 1973, p. xvii.)

technicalism, and an increasing disinclination of academics to enter into public political debates.[6,7]

If the discipline of Psychology was winning on the outside, it was experiencing a somewhat disconcerting disintegration on the inside. Psychology was breaking apart into a number of schools. Americans (Murchison, 1925, 1930; Heidbreder, 1930; Woodworth, 1931) wrote books counting the schools, disagreeing somewhat as to their number. Heidbreder (1930, p. 3) began her survey of Psychology's schools with the remark:

> It is something of a paradox that systems of psychology flourish as they do on American soil. Psychology, especially in the United States, has risked everything on being science; and science on principle refrains from speculation that is not permeated and stabilized by fact. Yet there is not enough fact in the whole science of psychology to make a single solid system.

European psychologists reacted even more strongly to the pluralism, writing about Psychology's "crisis" or "chaos" (Driesch, 1925; Buhler, 1927; Vygotsky, 1927; McDougall, 1930). Psychologists were vitally interested in scientific prospectuses that might point towards some meaningful coherence of their field, and this was a significant (and often overlooked) factor in their interest in methodological behaviorism.

It was reasonably clear in the 1920s that psychologists interested in social development had not formulated a cooperative empiricism through which researchers might collectively explore the phenomena of human social development. For that matter, the research technologies available for the study of any kind of child development were distinctly limited—the questionnaires used by G. Stanley Hall, the tests and measurements used in the era of the child development movement. Some, subscribing to the philosophy of science of the time, accepted the doctrine that there is a

[6] A number of factors led American social scientists to turn away from political advocacy and prophecy. Purcell (1973) argues that in the 1920s "methodology replaced moralism." People came to believe that knowledge, efficiency, and scientific planning were the only true agents of social reform.

[7] Increasing the political pressure on American academics is the fact that a "Red Scare", or a concern about the "Red Menace", welled up strongly in the United States in the years right after WWI, cresting between 1919–1921 but producing aftereffects well into the 1920s. What seems to have brought it on was: (1) the aftermath of American experience in WWI; (2) the Russian Revolution and a concern that Bolshevism would spread to the United States . . . a concern no doubt enhanced by the fact that in the early days Communist rhetoric emphasized the exporting of revolution; (3) a collection of American Socialist and Communist parties, probably pretty small; (4) some scary strikes and terrorist bombings; (5) an uneasiness about the large number of foreign-born immigrants in the United States (Allen, 1931/1997).

unity of science. Under this doctrine, given time, the further development of the more scientifically "mature" parts of psychology, sensation and perception, laboratory-based studies of conditioning and learning—ought to produce a beneficent fallout—research techniques and "composition laws" that would advance our understanding of developmental, social, and personality psychology and bring all psychology together within a scientific framework of stimulus-response theory.

But there were other psychologists who felt that distinctly different research approaches would have to be found to deal with human social and cultural development. The establishment of such approaches would take time and some genuine creativity. Experimental psychologists, it must be remembered, had formed much of their scientific program by adopting research methods taken from earlier European laboratories of physics, physiology, astronomy and medicine (Boring, 1956). A research program to address human social development would have to be invented and the invention would have to venture into new, dark, somewhat legendary scientific territory.

There has been a long-standing argument that a fully adequate scientific approach to human behavior would require two distinctly different kinds of psychologies. The argument that the study of humans and human affairs cannot be conducted using the methods of the natural sciences goes back to Kant. During the 19th century, a number of German philosophers—Windelband, Rickert, Dilthey—attempted to characterize an alternative scientific program might be possible to study human affairs—to create a *geisteswissenschaft* or a *kulturwissenschaft*. Over the past two centuries, a number of important figures in the history of psychology—John Stuart Mill, Wilhelm Wundt, Hugo Munsterberg—have argued for the necessity of two psychologies—a "bottom-up" psychology in which lower-order thoughts and behaviors are formed out of elementary atomic sensations, ideas, reflexes, etc., and a "top-down" psychology in which higher-order thoughts and activities are formed by the social and cultural milieu of the individual (Cahan & White, 1962; Jahoda, 1993).

How do you create a cooperative scientific program directed towards social development? I have argued elsewhere that a group of investigators seeking to engage in cooperative investigations need a set of "social proof structures", cooperative arrangements that allow them to look at the same universe of phenomena and to identify entities that can be examined by all observers. They need procedures that will help them to separate the observer from the observed. And they need a commonly available dimensional scheme or schemes so that they can map the entities and processes under observation and propose temporal, spatial, or causal orderlinesses (White, 1976, 1977).

Beginning in the 1930s, there was the substantial development of research procedures, methods, and conceptual formulations to serve as a framework for the serious study of a child's social and cultural development and to provide the foundation for a "top-down" cultural psychology. Time will not permit me to undertake a review of the long, slow, heterogeneous development of the research procedures of the "soft" psychologies in the 20th century, but I will comment on a few programs that have been of particular help in providing foundations for a serious research exploration of children's social and cultural development.

THE TESTS AND MEASUREMENTS PROGRAM

The predominant activity of the institutes and centers of Child Development movement from the 1920s to the 1940s was addressed to efforts at quantitative descriptions of the activities and qualities of developing children, norm-gathering. The Child Development movement is often dismissed in a friendly way, as a well-meaning but largely ineffectual scientific program. The movement was relatively short-lived. Its research activities were predominantly descriptive, and its methods and methodology was largely that of the tests and measurements program formulated in early 20th century by educational and applied psychologists. The Child Development movement was a largely atheoretical effort. Some of what energized it were some overoptimistic views about the possibility of mathematical measurement of human psychological qualities, some the practical needs of the emerging whole-child professions in the early decades of this century. I think the movement deserves a second look and Emily Cahan and I have been trying to do just that. If a community of investigators is going to cooperatively examine the personality, social activities, and cultural participation of growing children, then that community needs to find a way to find features or qualities of that child's activities that all can look at together in some agreed-upon way . . . to give those features or qualities what Donald Campbell would call "entatitivity". Perhaps the Child Development movement took some necessary, dull steps towards finding personal and social things for developmental psychologists to look at and think about together.

DEVELOPING IDIOGRAPHIC AND DISCOURSE-ANALYTIC PROCEDURES

Psychoanalysis exerted an enormous influence on the growth of psychology as a human science in the 20th century, and there has been

relatively little historical effort given to exploring how this was so. A fascinating thesis by Lazar (1994) traces some of the movement of psychoanalysis into American university life. In 1930, Franz Alexander was appointed Professor of Psychoanalysis at the University of Chicago, the first professorship of psychoanalysis in any American university. Leading social scientists at Chicago—Park and Burgess in Sociology, Sapir in Anthropology, Lasswell in Political Science—welcomed Alexander's appointment, because they thought psychoanalysis would offer a case study method for the study of the individual and culture or society. When the Alexander appointment did not work out, a number of Chicago faculty left Chicago to participate in the founding of Yale's Institute of Human Relations. That Institute, in turn, served as a significant precedent for the formation of Harvard's Department of Social Relations. There were many channels of psychoanalytic influence on American academic life, but we can follow the chain of institutional events traced by Lazar (1994) towards points of origins for major programs in mid-20th century developmental psychology. Robert Sears' antecedent-consequent analyses of child training and personality was born at Yale's Institute of Human Relations, as was John and Beatrice Whiting's studies of culture and personality. More broadly, psychoanalysis helped open the door to case study methods and discourse-analytic approaches in developmental psychology.

EFFORTS TO MAP OUT THE ECOLOGY OF HUMAN DEVELOPMENT

If we are to understand how a child enters into society, sooner or later we have to find significant ways of characterizing in some detail the environment to which the child must adapt and the questions or problems that environment holds to out to the child. Note that the environments traditionally posited and dimensionalized in early paradigms of psychological research are completely inadequate for this kind of research program. The environment of the 19th-century "brass instruments" experimental psychology, and the environment of contemporary research on perception, is a world of fundamental physical energies, more or less precisely specified. The environment of the evolutionary biologists from whom the first theories of the developing child's "intelligence" were put forth, is an environment of fields and rivers and oceans and jungles . . . to use Herbert Spencer's phrase, "Nature red in fang and claw." Try to spin a theory of how a child reacts to fourth grade arithmetic, or a first date, or a computer-based program of instruction, out of observations on those worlds. Beginning in the 1930s, with Martha Muchow's work in Germany (Wohlwill,

1985), there were pioneering efforts to describe in some detail the psychological environment of the small child. We owe to Barker & Wright (1954) and their associates at the University of Kansas, and to Urie Bronfenbrenner (1979) at Cornell, the working out of careful, detailed descriptions of the world the small child experiences, to which it adapts, and which sooner or later it begins to reconstruct and rebuild as a participating member of its society.

THE INVENTION OF INTERESTING AND PROVOCATIVE RESEARCH PROCEDURES

Sooner or later, somebody is going to have to study the individual motives and group dynamics of research-producing communities of investigators. Everyone who has done research knows something about the mixture of holy and unholy motives that drive investigators and condition their choices of problems, about fads, about hot issues, about tribalisms, about sources of technical, financial, and political support. In the 20th century, a number of creative investigators and their associates have been particularly fertile and influential in bringing forth, again and again, research procedures and problems that have been followed, and that have given the research community new windows with which to explore and ponder phenomena of children's personality and social development. The names of Kurt Lewin and Michael Cole come instantly to mind as major influences for the research communities of their time. But here a word needs to be said about L. S. Vygotsky and the nature of his influence on American psychology.

COEVOLUTIONARY THEORIES OF HUMAN EVOLUTION

We come full circle in the last decade, when conceptions of human evolution have been set forth that give room to the place of language and culture in a growing child's environment. Donald (1991) and Deacon (1997) have set forth coevolutionary visions of human evolution, in which the emergence of the human species has been seen as a process linked with the emergence of human language and culture. We cannot study the evolution of human intelligence without studying the growing intelligences of the language, myths, cultural practices, and technologies within which humans live. This has profound implications for our understanding of child

development, and Nelson (1996) has begun to explore those implications in her recent treatment of children's language and cognitive development.

THE REEMERGENCE OF HUMAN SOCIAL DEVELOPMENT IN THE 1960s

Let us return, finally, to a reconsideration of events of the 1960s. Historians of cognitive science (Gardner, 1985; Baars, 1986) have argued that there was a "cognitive revolution" in the late 1950s. It is clear that at the time there was a movement of experimental psychologists away from behaviorism and towards cognitive studies. But there was more to the "cognitive revolution" than that. In a sense, Psychology became undone again in the 1960s, split up, as it had been in the 1920s. With the setting aside of behaviorism, the vision of an ultimate unification of Psychology set forth by the philosophy of science of the behavioristic era was abandoned. Now Psychology resigned itself to being pluralistic, multiparadigmatic.

Among developmental psychologists, something like the long-projected second psychology began to emerge. Two clear signs of this were, exactly, the beginning influence of Vygotsky in the 1960s and the turning of developmental psychologists towards work on social programs and policies.

Vygotsky was one of that small number of Europeans who were dismayed by Psychology's pluralism in the 1920s. He wrote a book titled *The historical meaning of the crisis in Psychology* in 1927 (Vygotsky, 1927/1997). He recognized that at the core of the pluralism there were two competing visions of human psychology, a natural-scientific approach and the approach of a human or cultural science. He set forth to build a research program that would address psychology as a cultural science. The core of any human-scientific approach to the study of human psychology, Vygotsky felt, had to be developmental psychology.

We know Vygotsky as a brilliant theorist of human cultural development, but what came over to the United States in Vygotsky's name in the 1960s was considerably more than that. Vygotsky had begun to build an innovative research program in the post-Revolutionary Soviet Union. After his death in 1936, his friends and pupils—scrambling, living by their wits in Stalinist Russia—managed to advance that research program. What came to the United States as a sophisticated philosophy of human social development at least partially "naturalized" for service to a community of cooperative investigators. Americans had their own empirical and methodological resources by then, considerable ones, but still it helped in building receptivity to Vygotsky that you could get your hands on his ideas.

One of the more important proponents of a second psychology in the 19th century had been John Stuart Mill. His "ethology"[8] would look at the effect of a human's social and cultural environment on the person's motives character and personality. Such a science, Mill thought, might serve as a foundational science for education and the social design of the circumstances of human lives. It is interesting that in the 1960s developmnental psychologists found themselves involved in a variety of projects addressed to the conception, management, and evaluation of poverty programs having exactly those kinds of aims.

ADVANCING THE RECONSTRUCTION

We have examined some of the historical circumstances underlying the contemporary reconstruction of developmental psychology. How might such an examination help us to advance that reconstruction?

1. It seems to me we should begin to examine more seriously the conceptions of human social development historically set forth by Josiah Royce, James Mark Baldwin, George Herbert Mead, and John Dewey . . . and, while we are at it, by Cooley and William McDougall. We have by now spent about 40 years working through conceptions of human development set forth by Vygotsky in 10 very active years of intellectual leadership in the late 1920s and early 1930s. Perhaps we can enlarge our pool of theoretical resources.
2. We need to extend the mappings of the human environment, the world to which the small child is born and in which it lives, given to us by the Kansas group and by Bronfenbrenner. And we need to see that environment as something the child learns to modify, regulate, reconstruct, build, and rebuild as the child grows to maturity.
3. We need to study the codevelopment of a child and its environment.
4. How do we study the behaviors of children and their parents in their everyday environments? Can we leave the laboratories of the university and simply make everyday environments our laboratories? I'm skeptical about that possibility. People are too busy living their lives in stores, factories and homes to allow researchers to invade their time and their privacy for scientific purposes. We will have to offer services to people—clinical or educational services,

[8] Mill's name for his cultural psychology. The correspondence of that name to our name for biology's 20th century study of animal behavior is, as far as I know, coincidental.

work on social programs or policies—as the vehicle for our study. And we may have to face the fact that applied developmental psychology can have quite basic meanings for our understanding of human development.

5. The customs and the practices by which we have traditionally pursued our research have reflected older ideals of solitary achievement—the professor who teaches his or her classes in the morning and in the afternoon does the research and publishes the papers that advance developmental psychology. But much of the work that developmental psychology needs to do in the everyday environment must be the work of larger, cooperating research groups. We need to adjust our programmatic and career ideals to new forms of research.
6. We need to formulate for our students and ourselves some historical and political accounts that set forth the kinds of methods we favor in cultural psychology and the ideas we serve. I hope this talk has taken a step towards doing just that.

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Part 5

Career Pathways and Professional Impact

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12

BREAKING THE SILENCE: HELEN BRADFORD THOMPSON WOOLLEY

Katharine S. Milar

Helen Bradford Thompson Woolley was one of America's "first generation" of women experimental psychologists (Scarborough & Furumoto, 1987). She carried out the first experimental investigation of sex differences in psychological characteristics (Thompson, 1903b), was one of the first psychologists in the United States to be employed by a public school (Milar, 1999), was the first psychologist and the first woman to serve as president of the National Vocational Guidance Association (Brewer, 1942), and was also a leader in the pre-school, nursery school movement of the early 1920s. If Woolley is known at all, it is for her sex differences work (see Rosenberg, 1982; Rossiter, 1982), which formed only a very small part of her career. While there are a few good brief accounts of her life (Scarborough & Furumoto, 1987; Zapoleon & Stolz, 1971), other accounts have either underestimated her accomplishments (Stevens & Gardner, 1982) or distorted them (Lewin, 1988). This chapter offers a more complete description of Woolley's life and work and discusses some of the reasons for her obscurity. A number of authors have examined factors influencing the omission of women psychologists from histories of the discipline until relatively recently (e.g., Furumoto & Scarborough, 1986; Russo & Denmark, 1987; Scarborough & Furumoto, 1987). Some of those factors are relevant to the relative obscurity of Woolley, but in Woolley's case an additional factor,

mental illness, played an important role. I argue that the impact of mental illness on her behavior and the response of her friends and former colleagues to her illness led to the silence that has cast a shadow over her accomplishments and has been a major factor in the lack of recognition her work has received.

BEGINNINGS AND EARLY CAREER

Helen Bradford Thompson was born November 6, 1874 in Englewood, Illinois, the second of three daughters of David Wallace Thompson and Isabella Perkins Faxon Thompson. David Thompson was a traveling shoe salesman and erstwhile inventor; Isabella was the daughter of an Illinois storekeeper. Helen graduated from Englewood High School in 1893, first in her class with an average of 97 (out of 100) for her four years. At graduation she read her valedictory essay "The Advance Towards Individual Freedom by the Aid of Invention," a tribute to the contributions of science to civilization and a reflection of her interests and inclinations. Her excellent high school record enabled her to win a scholarship to the University of Chicago and, by living at home, she was able to attend. Her older sister, Lillian, was forced to withdraw from the University of Michigan due to lack of funds. Even though during the period from 1890–1910, college women comprised only 2.2 to 3.8 percent of their age cohort (Solomon, 1985), both Helen's older and younger sisters eventually earned college degrees indicating the importance placed on education by her family (Fowler, 1976; Thompson, 1893; Woolley, 1936)

During her junior year at Chicago, Helen Thompson was offered scholarships in both physics and psychology and chose the psychology scholarship. After receiving her Bachelor's degree in 1897, she was offered a fellowship for graduate work in psychology within the department of philosophy and studied with James R. Angell, John Dewey, and George Herbert Mead as well as neurologist Henry H. Donaldson. Angell encouraged his students to study philosophy and neurology as well as psychology and Thompson published papers in all three fields while still a student at Chicago (Angell &Thompson, 1899; Thompson, 1899; 1900; Tufts & Thompson, 1898). Angell felt so strongly about the significance of their study of physiological changes accompanying emotional stimuli, sensory stimuli and mental activity that he devoted two and a half pages of his autobiography to a description of it (Angell, 1936).

Thompson completed her Ph.D. in 1900, summa cum laude. Chicago graduate John B. Watson had a similar course of study at the university, but evidently was not as highly regarded as his predecessor. "I received

my degree Magna Cum Laude and was told, almost immediately, by Dewey and Angell that my exam was much inferior to that of Miss Helen Thompson who graduated two years before me with a Summa Cum Laude. I wondered then if anybody could ever equal her record. That jealousy existed for years" (Watson, 1936, p. 274). Her dissertation research compared the performance of 25 men and 25 women students at the university on "motor ability, skin and muscle senses, taste and smell, hearing, vision, intellectual faculties and affective processes" (Thompson, 1903b, p. 5). Rather than reporting average performance, she described the complete distribution of scores on each test for each sex. As she said of this work, "The topic aroused public interest and the work was given newspaper publicity. The title of the thesis was Psychological Norms in Men and Women. The use of the word 'norm' as a noun was new and unfamiliar. My thesis was reported in various distant newspapers not as psychological norms, but as psychological forms, and once as psychological worms in men and women" (Woolley, 1936, p. 2).

The qualities that characterized her experimental work throughout her career are evident in this dissertation: detailed (even tedious) descriptions of the method of administration of each test, together with the possible sources of errors in measurement, and results presented in the form of distributions of scores on each test for each sex. She found that men had the advantage in most of the tests of motor skills whereas women overall showed finer sensory discrimination. On the tests of intellectual faculties, she found women slightly better at memory and association tasks and men better at tests of ingenuity. In her conclusions, Helen Thompson departed considerably from the accepted theories of the day. Geddes and Thompson (1890) accounted for male female differences based on the characteristics most beneficial evolutionarily for the sperm and the egg. With great restraint, Helen pointed out the illogic in the biological analogies and suggested that other explanations were as logical as genetic ones—namely environmental differences: "... the psychological differences of sex seem largely due, not to difference of average capacity, nor to difference in type of mental activity, but to differences in the social influences brought to bear on the developing individual from early infancy to adult years" (Thompson, 1903b, p. 182).

Eventually published as *The Mental Traits of Sex* (1903), her work received mixed reviews. Two reviewers, while acknowledging the importance of such careful experimental work questioned whether the women in her sample were representative of their sex (Anonymous, 1903; Dolson, 1905). One wrote: "... the college woman is not exactly comparable with the college man. She is usually the cleverest girl of her family, urged by ambition or poverty or incompatibility of temper to leave her home; he goes to

college as a matter of course" (Anonymous, 1903, p. 236). Other reviewers, however, found her conclusion momentous. Thomas (1907) stated "Her findings are probably the most important contribution in this field and her general conclusion on differences of sex will, I think, hold also for differences of race . . . " (p. 435).

After completion of her degree, Thompson was awarded a European Fellowship by the Association of Collegiate Alumnae (later the AAUW); she studied first in Berlin with Carl Stumpf and Arthur König and then at the Sorbonne with Eduard Toulouse and Pierre Janet. Elizabeth Mead, president of Mount Holyoke College and a relative of George Herbert Mead, had offered Helen Thompson a teaching position at the college upon her return from Europe; she began teaching there in the fall of 1901 (Woolley, 1936; Zapoleon & Stolz, 1971).

Because of her training, Helen Thompson proposed to Mount Holyoke's new president Mary E. Woolley that a psychological laboratory be established at the college. "No modern psychology had at that time been taught at Mount Holyoke College . . . Courses in 'mental philosophy' had been given, but they were adjuncts of religion rather than of science. President Woolley had at hand a small gift of money presented to the college for the purpose of proving the existence of the soul. President Woolley knew of no more direct way of approaching the problem than by scientific experiment on the soul. Accordingly I was allowed to use the money to establish a laboratory of experimental psychology" (Woolley, 1936, p. 3). According to the student newspaper, the 'small' gift to which she referred was $1000.00; surely not considered a small sum for the time. Thompson purchased an electric color wheel, a chronoscope, the Hering apparatus for testing color blindness, a set of tuning forks and resonators from Paris and brain models and charts (*The Mount Holyoke*, 1903–1904). During the period Thompson was directing the laboratory, no less than $200 per year was allocated by the Trustees for laboratory expenses. In the years following her departure when the laboratory was under the direction of Samuel P. Hayes, smaller and smaller allocations were noted in the Board minutes and finally only "unexpended balance"(Board of Trustees, 1902–1908).

Thompson was professionally productive during the Mount Holyoke years in several different subject areas. In addition to the publication of her dissertation (Thompson, 1903b), she completed a philosophical chapter in a book edited by John Dewey (Thompson, 1903a), an experimental study on judgements of surface area on the skin (Thompson & Sakijewa, 1901), and a study of after-images with Kate Gordon, who had overlapped with Thompson at the University of Chicago and who joined the faculty of the psychology department at Mount Holyoke in 1904 (Thompson & Gordon, 1907). In addition, Thompson "communicated" a study by her student

Grace Fernald under the rubric "Studies from the Psychological Laboratory of Mount Holyoke College" (Fernald, 1905). Thompson also participated in college life in other ways, appearing as one of seven princesses in a faculty dramatic production in 1903 starring biologist Cornelia Clapp and English professor Jeanette Marks ("Living Pictures,"1903).

In 1897, as a senior undergraduate at the University of Chicago, Helen had become engaged to Paul Gerhardt Woolley who, after receiving his B.S. from Chicago a year earlier, was studying medicine at The Johns Hopkins University. Woolley received his M.D. in 1900, then served as resident house officer at Hopkins hospital before becoming a Fellow at McGill in 1901 (Cattell & Brimhall, 1921). In the wake of the Spanish American War and the establishment of a colonial government in the Philippines, the United States set up scientific laboratories to help in the development of the islands' natural resources and in the control of animal and human diseases. The Bureau of Government Laboratories was established in Manila and in January, 1903, Paul Woolley embarked for the Phillippines and his position as chief pathologist (Fischer, 1938).

In August, 1905, Helen met Paul in Yokohama, Japan where they were married at the home of the consul general. Upon their return to Manila, Helen Thompson Woolley was appointed as psychologist to the Bureau of Education. Only three years of schooling were required for children in the Philippines and Woolley's task was to determine experimentally the particular three years of age that would be optimal educationally. She decided to use visual and auditory memory tests on the children, but when she began to analyze her data she discovered a problem: "the Philippinoes (sic) did not know how old they were. The age records of school children sometimes stood still for several years, sometimes went forward normally, and from time to time went backward" (Woolley, 1936, p. 3). Attempts to track down the ages of the children through christening records were frustrated by haphazard record keeping and Woolley eventually had to exclude much of her data. She finally recommended the ages of 9 to 12 years as the optimal years for school. In addition to this research, she also taught psychology to teachers at the summer normal school (Woolley, 1936).

In April, 1906, Paul Woolley became director of the serum laboratory in Phrapatom, Siam. Although initially excited about his opportunity, by September he complained to his friend William A. Wherry who had returned to the States, "The worst is that I am so alone and with only the literature to talk with, I get morbid on the subject of my own acquirements. . . . I don't think I can stand it for more than my contract time. Can't you get control of a laboratory and take me in, or will you come out here for what I am getting-3000 and a house?" (Fischer, 1938, p. 118). Helen

Woolley also was feeling isolated; even reading and writing posed some difficulties as she described in 1907: "I am writing in a remote corner of the earth where I have no library facilities, and only a few of the leading current periodicals, which reach me irregularly and whose files extend back but a year or two" (Woolley, 1907, p. 329). Prince Dam Rong offered her the position of director of the Siamese department for dealing with juvenile delinquents; however by this time Helen was pregnant. With no hospitals and no obstetricians in Siam, Paul insisted that she return to the U.S. for the birth. She traveled alone on a journey that took seven weeks and arrived at her parent's home only six weeks before Eleanor Faxon Woolley was born (Woolley, 1936).

On leave from Siam in the spring of 1908, Paul Woolley, Helen and nine month old Eleanor lived briefly with his parents, John Grenville Woolley and Mary Veronica Gerhardt Woolley before Paul officially resigned his position in Siam to become an associate professor of pathologic anatomy at Creighton Medical School in Omaha, Nebraska. The birth of Eleanor and multiple moves did not prevent Helen Woolley from remaining professionally active. In Omaha, she offered private classes in psychology to women. While living with her parents, she had noted Eleanor's preference for a pink rattle rather than a blue one, and began some observations and experiments on the baby's color perception. In addition to recording Eleanor's color preferences, she also recorded the hand she used in making her color choices. Two papers resulted from these observations, one on color vision (Woolley, 1909) and one on the development of handedness (Woolley, 1910a). These articles show Woolley's familiarity with the work of Millicent Shinn (1900) and reflect the same kind of attention to detail and to possible sources of error in testing that characterized her research on sex differences. For example, concerned that the lack of uniform texture in the color discs she was using could influence Eleanor's performance, Woolley obtained some better ones from Angell and John Watson at Chicago (Woolley, 1909).

In 1909, at the age of 34, Paul Woolley was appointed Professor of Pathology and director of the laboratories of the Cincinnati Hospital, Cincinnati, Ohio. His was the first full-time appointment in the new medical center being established there (Kehoe, 1963). Helen Woolley obtained a position teaching one philosophy course a semester at the University of Cincinnati including logic, ethics and introduction to philosophy. In addition, she completed the first of her extensive reviews of the psychological literature on sex differences in which she stated her opinion of the research quite forcefully. "There is perhaps no field aspiring to be scientific," she declared, "where flagrant personal bias, logic martyred in the cause of supporting a prejudice, unfounded assertions, and even sentimental

rot and drivel, have run riot to such an extent as here" (Woolley, 1910b, p. 340).

Although she had certainly been professionally productive, there is no coherent body of work represented in Woolley's publications between her time at Chicago and her arrival in Cincinnati. Her marriage, the frequent moves, and the lack of any type of regular position prevented her from developing a coherent research program. Her publications are opportunistic, and the fact that she pursued publication opportunities through the writing of reviews as well as through the study of her own daughter's behavior suggest an ambition to establish a name for herself in the field. The year 1911 marked the beginning of the most significant period in the professional and personal life of Helen Woolley. She became a member of a group of rather extraordinary professional women in Cincinnati who were active in a variety of social, political and education reforms. One of them, social economist, M. Edith Campbell, was to have a substantial impact on Helen Woolley's life and work, providing her with a project and a direction for her efforts which was to bring Woolley national recognition.

VOCATIONAL GUIDANCE PSYCHOLOGIST

In 1910, the State of Ohio passed one of the most progressive child labor laws in the country (Kelley, 1911). Children between the ages of 14 and 16 years who had completed the equivalent of fifth grade could withdraw from school to go to work. However, until the age of 16 years, a child had to obtain a "working certificate" for every job held and either had to be in school or regularly employed. The public school was placed in charge of the issuance of the working certificates to insure that the type of job and the working conditions conformed to the restrictions established by law (Freiberg, 1911; Woolley, 1913–14).

Edith Campbell along with Edward N. Clopper of the National Child Labor Committee, saw in this new regulation the opportunity to obtain a body of statistical information on the effects of child labor. With the help of a group of local philanthropists led by Jacob Schmidlapp, they launched a five-year study designed for the purpose of establishing a scientific basis for vocational guidance. They proposed to gather physical and psychological data from children who left school to go to work and a control group of children who remained in school (Clopper, 1910; Milar, 1999; Phillips, 1950).

In March, 1911, Helen Woolley was appointed director of the Bureau for the Investigation of Working Children, which later became known as the Vocation Bureau. (Woolley & Fischer, 1914; Woolley, 1926a; Zapoleon,

Figure 1. Helen Bradford Thompson Woolley and daughter Eleanor, 1909. (Courtesy of Ann Fowler Rhoads)

1985). She was initially reluctant to accept the full-time director's position because she had a three-year old child at home, but Edith Campbell was eager enough to have Woolley that she personally found a trained person to take care of little Eleanor. In taking on this position, Woolley became one of the first psychologists employed by a public or private school; moreover, she was among the first psychologists engaged in applying psychology in a non-academic setting.

Woolley was drawn to the research with working and school children by a number of factors. An emphasis on applied research was growing among social scientists as part of the Progressive movement (Burnham, 1960; Fitzpatrick, 1990). The University of Chicago in the late 19th and early 20th century was a magnet for these middle-class progressive reformers. Jane Addams and Florence Kelley were pushing a progressive agenda at Hull House where Woolley volunteered as a student (Fitzpatrick, 1990; Sklar, 1985; Woolley, 1938b). John Dewey, with whom Woolley had studied, was a vocal advocate of educational reform. A new class of university educated women in the sciences and social sciences were eager to use their expertise for political and social action in areas such as child labor, prison reform, juvenile delinquency, and the cause of women wage earners (see Fitzpatrick, 1990 for more extensive discussion). Woolley was interested in social justice and welfare issues as evidenced by her involvement with a number of causes during the 12 years she lived in Cincinnati: suffrage, child

welfare, and the control of prostitution. Both Dewey and Addams visited her at her home in connection with those causes (Fowler, 1976; Zapoleon & Stolz, 1971).

As a psychologist, Woolley's original goals in taking on the project with working and school children were to establish a battery of tests appropriate for adolescents, to develop norms for those tests as a way to address a general perception that the Binet tests were inadequate for children above the age of 14, and she sought to establish a relationship between test performance and job performance (Woolley, 1912a). Accomplishing these three goals was a monumental undertaking. Woolley and her staff at the Vocation Bureau began with a sample of 800 14-year old children who applied for working certificates in 1911–12. A control group of 713 children who remained in school were used for comparison. Each child was given battery of physical and psychological tests once a year until the age of 18, and, with the recruitment of additional controls, the total number of cases included in the study was 5483 (see Milar, 1999 for further details).

The analysis of the data from this study was a staggering task. Results from the first two years of testing the working children appeared in a monograph in 1914, which contained a total of 90 tables and 144 figures in its 247 pages (Woolley & Fischer, 1914). Woolley herself, with the help of friend and co-worker Charlotte Rust Fischer apparently carried out all the data analysis in spite of a rather considerable distraction. Charlotte Fischer wrote in her diary on January 12, 1914, "began going to Helen's for 6 hrs daily-until paper is finished." On January 30th, "Helen Woolley not so well but worked all day on curves." Finally on March 5, 1914: "Charlotte Woolley born 11:15 p.m." (Fischer, 1914).

In 1915, Woolley published her new scale of measurement for adolescents based on the two years of data gathered from the working children (Woolley 1915b). However, by December, 1915, a preliminary comparison of the data from the working children with the control group of children remaining in school showed that the school children were superior on every measure and that this difference seemed to be greater at 15 years than at 14 years. This led to the conclusion, "The scale of measurements already published for working children, is accordingly too low for children in general" (Woolley & Fischer, 1916, p. 91). In the final report of the project not published until 1926, Woolley no longer claimed that her test battery is a good one for use with adolescents, suggesting instead that Terman's revision of the Binet tests, the Yerkes-Bridges Point Scale, or other tests developed since her research began might be better choices (Woolley, 1926a, p. 720).

In spite of her failure to develop a new test battery for adolescents, Woolley was a passionate promoter of the usefulness of experimental

psychology in the public school to diagnose a child's abilities, to help identify the causes of failure, and especially to identify general level of ability. She articulated her vision of educational policy based on physical and psychological tests to various professional groups including the APA, National Education Association, Conference on Charities and Corrections (later the National Conference of Social Work), and the National Vocational Guidance Association (see, for example, Woolley, 1912 a,b; 1913a,b; 1915a, 1923). Woolley was truly a pioneer of applied psychology.

In December, 1916, of the 307 members of the APA, only 35 were engaged in work that was not primarily teaching . Women faced special challenges; in 1916, 39 women accounted for 12.7% of the membership of APA (Cattell, 1917). Academic positions were difficult for women to obtain except at women's colleges and even that option was not available for married women (Furumoto & Scarborough, 1986; Rossiter, 1982). Cattell (1917) reported that nine married women were among the 11 APA members working in areas entirely unrelated to psychology. Because of the difficulty in finding academic positions, women, single and married, were among the first applied psychologists, but even by 1916 Cattell found that of the APA members not engaged in teaching, only 16 (5% of the total membership) were doing work connected with schools, penal institutions, or hospitals. Woolley was one of these 16, and further, was sufficiently respected in vocational guidance circles to serve in 1918 as second vice president of the National Vocational Guidance Association. In 1921 she became the first woman and the first psychologist to serve as president of the association (Brewer, 1942).

Under her direction, the work of the Cincinnati Vocation Bureau continued to expand. The budget grew from $5000 in 1911 provided exclusively through private sources, to $60,000 by 1922, two-thirds of which came from public monies (Woolley, 1922). The psychological laboratory activities were restricted to research until 1916 when it began to be used to test children for placement in classes for "defectives" (Woolley, 1922; 1926a). By 1921 when Woolley left Cincinnati, the laboratory of the Vocation Bureau was testing all children for placement in special classes, for exclusion from school, all behavior problem children referred for examination and children referred by the juvenile court. Children admitted to the School for Crippled Children were all tested, as were children being considered for placement in the "rapidly moving class" for superior children (Woolley, 1918). As compared with other psychological clinics of the time, the Vocation Bureau had a more balanced emphasis on both research and providing remedial help for the individual child (Children's Bureau, 1925). In addition Woolley's Vocation Bureau also explored institutional solutions such as establishing special classes for developmentally delayed

and gifted children, a new development at the time (Fagan, 1992; Smith, 1914).

THE MERRILL-PALMER SCHOOL

Paul Woolley never settled back into life in Cincinnati after World War I and left in 1920 to direct the National Pathologic Laboratory of Michigan and then to do diagnostic work with the Detroit Clinical Laboratory (Kehoe, 1963, p. 268). Convinced by the Vocation Bureau's work with "defective" children that, at age 12, they were assigned too late to special classes, in 1917, Woolley became part of a four-year experiment to assign first and second grade children to an "observation class" (Woolley & Ferris, 1923). This signalled a shift in her interests from adolescents to younger children. In 1921, in order to join her husband, Helen accepted a position as psychologist and assistant director of the Merrill-Palmer School where she would work with nursery school children. Three hundred people attended a farewell banquet held for her at the Sinton Hotel, at which the Woman's City Club announced the establishment of a scholarship in her name in "recognition . . . of the services Mrs. Woolley had rendered to the public schools of the city and of her experience and scientific research in . . . the problems of childhood . . . , by which she has rendered the name of Cincinnati conspicuous as an educational center throughout the country" ("Scholarship," 1921).

The shift from a primary concern with adolescents to a concern with younger children occurred in many of the child specialists in the United States at that time. The child-study movement of G. Stanley Hall, which could roughly be said to have been dominant from the 1880's to about 1910, focused primarily on school-age children and adolescents. This gave way to the post-war child development movement, the rise of interest in the nursery school child and in parent education (Cahan, 1991; Schlossman, 1976). The mental hygiene movement with its emphasis on the importance of childhood experience in later mental health was also ascending (see Richardson, 1989 for further discussion). Michigan lumber heiress Lizzie Merrill Palmer gave $3 million to establish a school for "Motherhood and Home Training." A corporation was organized in 1918, with seven men trustees handling the estate and six women directors placed in charge of educational policy (Diack, 1922). They selected home economist Edna Noble White as director of the Merrill-Palmer School who, concerned that preparation for motherhood should include training in child care, traveled to England to visit English nursery schools. She brought back with her Miss Emma Henton, an experienced nursery school teacher and White

determined in "our own experiment the mental phases of child development should receive especial consideration and Mrs. Helen Thompson Woolley was secured to direct the organisation(sic) and development of this piece of work" (Merrill-Palmer, 1921).

With Woolley as assistant director, courses in child care and training were supplemented with laboratory work in the nursery school. The curriculum was designed with two objects in mind: "general instruction for women in the problems of childhood, and . . . the training of specialists, either teachers or research students . . . Our conviction is that every woman should during her education, have instruction in the care and management of children, either as mother or as teacher, as social worker or merely as relative" (Merrill-Palmer, 1922). The Merrill-Palmer School was one of the first laboratory nursery schools in the United States and, as Woolley (1936) described it, "attracted a great deal of attention. Visitors thronged to it from all over the United States and from abroad. . . . The school seemed . . . to demonstrate a new and fruitful approach to education"(p. 6).

Experimental studies involving development and use of mental tests with children between 18 months and 5 years of age as well as personality studies were carried out with the children enrolled in the nursery school. Woolley herself did very little of the kind of testing which had occupied her in Cincinnati. She did report the standardization of the performance of 3-, 4- and 5-year old children on some Montessori apparatus, but the actual testing was conducted by her friend and graduate student, Elizabeth (Bess) Cleveland (Woolley & Cleveland, 1923). This was the beginning of the Merrill-Palmer Scales for Children, but the majority of that standardization must be credited to Rachel Stutsman Ball. In a paper presented at APA in 1924, Woolley expressed reservations about the validity of mental tests of young children because of the variablity in scores obtained at retesting. Yet she continued to assert that a superior environment, created by nursery schools or parent education, could produce substantial gains in IQ (Woolley, 1925c).

In addition to her limited work on mental testing, she published some personality case studies on "Agnes" (Woolley, 1925a); "David" (1925b); and "Peter" (1926c). The majority of her publications during this period are the texts of speeches given to various groups, most frequently teachers or social workers (e.g., Woolley, 1923; 1924; 1926b). In the same way that her speeches during the Cincinnati period promoted the establishment of psychological laboratories associated with public schools, in these speeches to parents and teachers Woolley was spreading what Schlossman (1981) has characterized as "the gospel of child development." highlighting the benefits of nursery school for both parents and children. An examination of her speaking schedules for the years 1923–1925 shows that she was usually

absent from Detroit several times a month. In 1925 alone, she reported giving over 50 speeches to a total of more than 8,000 individuals during the year. This schedule would have precluded much in the way of research conducted personally by Woolley at the nursery school (Schedule, 1923; 1924; 1925). In addition to her work at Merrill-Palmer, as vice president of the AAUW from 1923–25 and chair of their Committee on Educational Policies, Woolley was instrumental in initiating the AAUW program on preschool and elementary school children. This was a controversial program according to Lois Hayden Meek who was hired to administer it. There was a belief among some of the AAUW members including Virginia Gildersleeve, president of Barnard and Ellen Pendleton, president of Wellesley, that the program was a bad idea: "they felt that this was a lowering of the standards for women, that child development would become a woman's field, and that there would again be this differentiation which they worked so hard, since 1882 to get rid of" (Stolz, 1977, p.21). In spite of these objections, Woolley working with Edna Noble White and Lawrence Frank obtained funds from the Laura Spelman Rockefeller Memorial to support the organization of AAUW women into child study groups. Under the direction of Lois Meek more than such 300 groups were organized.

TEACHERS COLLEGE

Woolley's contact with the Laura Spelman Rockefeller Memorial on behalf of the AAUW as well as Merrill-Palmer's status as a model for preschool education contributed to her being offered the position of director of the Institute for Child Welfare Research at Teachers College, Columbia University in May, 1925. Teachers College was the first institution to receive an award from the Laura Spelman Rockefeller Memorial (LSRM) for child study and parent education. Subsequently the Memorial funded centers for research on young children at Iowa, Toronto, Minnesota, Yale and Berkeley. Woolley was the only woman to serve as a director of one of these major research centers.

Woolley claimed that the generous salary offer was the inducement for her to leave Detroit. Paul Woolley had left in 1923 for California where he could receive better treatment for tuberculosis which he had contracted as a result of his laboratory work in Detroit. Helen claimed that his insurance and an Army pension were used to defray the costs of his illness, leaving her with complete financial responsibility for her two daughters (Woolley, 1936). There is, however, some evidence that Helen Woolley was not as beloved at Merrill-Palmer as she had been in Cincinnati. Edna Noble White reflected, "She has always been difficult to work with and her colleagues

were in a perpetual state of nerves and resentments as long as she was here" (White, 1935). The extent to which these tensions contributed to Woolley's willingness to take the New York position cannot be determined.

A number of subsequent events produced cumulative effects with tragic consequences for Woolley. Bess Cleveland, with whom Helen and Charlotte had lived since Paul's departure and whose personal and professional relationship Helen characterized as "one of the most valuable, and one of the best enjoyed experiences of my life" died of cancer in December, 1925 (Woolley, 1936, p. 7). Teachers College wanted Woolley to begin work in January, 1926, but Helen wanted to smooth the transition at Merrill-Palmer so she commuted every two weeks between Detroit and New York from January until June. In April, 1926, there were hints that Woolley was finding her new position difficult. In a memorandum of an interview with James Russell, Lawrence Frank of the LSRM reported: "I told Dean Russell that I had been approached by several of Dr. Woolley's friends with a request that I help her out of the difficulties she was facing and that as nearly as I could understand the matter, Dr. Woolley's temperamental aversion to any disputes or self-assertion put her at considerable disadvantage at Teachers College" (Frank, 1926). Charlotte Fischer noted in her diary, "Helen says doesn't know if she can hold out-losing weight can't sleep" (Fischer, 1926). In late May an abdominal tumor was discovered and in June Helen had a hysterectomy and appendectomy. Some time during this period Paul sued for divorce which he subsequently withdrew when Helen told him that Dean James Russell at Teachers College had indicated that it would jeopardize her position. (Paul did obtain a Mexican divorce in 1929 and married a woman named Millie Thompson) (Woolley, 1934b).

In September, 1926, Helen moved to New York and began an even more intensive period of work at the Child Welfare Institute. By January, 1927 she was exhausted and took a trip to Atlantic City with Charlotte Fischer who noted in her diary on January 11, 1927 "Came home immediately after dinner & Helen had the most frightening shaking fit" (Fischer, 1927). Charlotte arranged for Helen to go to a sanitarium in Summit NY for a "rest cure" and took charge of Helen's daughter Charlotte; Eleanor was a student at Bryn Mawr (Fischer, 1927; Woolley, 1927). By February, 1927, deeply depressed and actively suicidal, Helen was transferred to the Four Winds Sanitarium in Ketonah, NY where she remained, the suicidal tendencies at times requiring nurses around the clock (Russell, 1928). As her doctor, Charles I. Lambert reported later, "Her illness ran the usual course of a depression, with reaction in the opposite direction, in which she was elated, over-active, very self-assertive and dominant, and in this phase she left 'Four Winds' against advice" (Lambert, 1930a). That was in December,

1927, and Helen went immediately to Cincinnati to see her daughter Charlotte. In her diary, Charlotte Fischer described Helen's manner during this visit as "very exaggerated" and reports her getting lost in the city, having a picture framed and charged to the Fischers and other uncharacteristic behavior (Fischer, 1927).

Teachers College was very generous in their treatment of Woolley. Not only did they continue her salary, but they paid her medical bills which amounted to over $11,000 (Russell, 1928). In March, 1928, supported by Rockefeller funds, she left for Europe to visit child research institutes in Brussels, Geneva, and Vienna, and to observe some English nursery schools. Charlotte Fischer's diary entries in September after her return indicate continuing difficulties. Woolley purchased a school wardrobe for her 14-year old daughter that included, "4 pairs of spike heel shoes with lizard and snake skin trimming; seven silk and velvet afternoon dresses . . . an opera coat of silver brocade . . . & a suitcase full of chiffon stockings. Paranoia? if ever there was a case" (Fischer, 1928). Also in September, in spite of Dr. Lambert's reservations, Woolley returned to Teachers College to direct the Institute. Lambert reported, "My reasons for thinking she would be unable to meet the responsibilities of her position were, first, her own emotional instability and lack of self control in the face of situations which would tend to thwart her; second, what appeared to be a relative impairment of retention and recent memories . . . ; third, the personnel with which she had surrounded herself was somewhat individualistic . . . and during her absence what organization had existed had disintegrated and she was faced with a rather trying administrative year. I feared she would be unable to meet it with credit to herself" (Lambert, 1930a).

Lois Hayden Meek, with whom Woolley had worked at the AAUW, was hired in the fall of 1929, at Woolley's suggestion, to take over some administrative duties. Those who were eager for Woolley to succeed greeted her arrival with enthusiasm. One professor wrote, "I cannot tell you what a great relief the news is to all the women on the faculty here. We feel you will be Mrs. Woolley's salvation and that she will have a real opportunity to keep steady when supported by your sympathetic cooperation. She is a big woman, and anything you can do toward loving and assisting her into continued efficiency will be . . . a great contribution." (Stolz, 1976, p. 7.). To her chagrin, Meek was unable to provide enough help for Helen and in February, 1930, Dean William Russell requested Woolley's resignation. As Lois Meek Stolz (1977) described it later, "This has always been one of my great regrets because I was devoted to her . . . I've always felt that we did not have quite long enough for Dr. Woolley to make use of the kind of support I gave her, though there's no question that she had failed during that period" (p. 128).

In his request for her resignation, Dean Russell wrote, " It was most unfortunate that so soon after you had assumed administrative responsibility for the Institute you suffered a severe illness which took you away for a year and a half; and that you were forced to resume your duties before you had time for adequate convalescence. . . . It is only after a great deal of careful thought and consideration that the Board of Trustees finds itself compelled to re-organize the Institute . . . " (Russell, 1930a). He encouraged Woolley to devote herself to writing or clinical work and offered to arrange a partial salary for a year or two while she adjusted to the change.

Helen Woolley was only 55 years old, considered by her contemporaries one of the nation's top child psychologists, and yet she never worked again. She tendered her resignation to Dean Russell on February 13, 1930 to be effective as of September 1; however, in a personal interview and letter sent later that day, Russell makes clear that Woolley's association with the Institute is over immediately: " . . . I must insist (1) that you no longer interview the parents of the children in the Institute; (2) that you no longer visit classes in the Institute; and (3) that so far as possible, you avoid the Institute"(Russell, 1930b). Interestingly, there is little adverse reaction by Woolley to this harsh communication, at least initially. In fact, Dr. Lambert sent a message to Russell on February 17 recounting a talk with Woolley. "She particularly appreciated how splendidly she had been treated by you and the administration and realized the embarrassing situation in which you found yourself with reference to the college responsibilities as well as to her personally" (Lambert, 1930b). She was not to remain quietly grateful, however.

In April, 1930, Helen wrote to her old friend and mentor James Angell, now president of Yale University, to seek his help in finding another position. In her letter she maintained that Russell asked for her resignation because his trustees were objecting to paying her such a high salary—, "doubtless in part because a woman was receiving it, though that he did not say;" however, at the time of writing, she worried that Russell was focusing instead on the poor quality of her teaching and administrative work. She denied any problem with her teaching and administration and asked for Angell's help (Woolley, 1930). Receiving Helen's permission to ask Russell himself about the reasons for requesting her resignation, Angell investigated and was convinced that there had been difficulties with her teaching. Observing that "by reason of over-work, or some other form of strain, your nervous and mental balance had been very seriously disturbed," Angell asked Woolley to provide him with a psychiatric evaluation of her condition before he attempted to help her find a position (Angell, 1930).

In response, Woolley sought an evaluation by William A. White of Washington, D.C. who did not see her in person, but corresponded with her and with Dr. Lambert. White forwarded Lambert's letter to Angell along with his own observations. Lambert was not sanguine about Woolley's readiness to resume serious academic work, but also admitted that his association with Teachers College made him less than objective about her situation. Woolley had begun an argumentative correspondence with Dean Russell in May, 1930 about salary, housing and the quality of her teaching. "At the present time, she is carrying a rather large sized chip on her shoulder about Dean Russell's treatment of her, and is talking about it to her own great disadvantage"(Lambert, 1930a). White, believing from Woolley's letter to him that she was better than Lambert suggested, urged Angell to give her an opportunity (White, 1931).

Writing from Columbia, John Dewey reported to Angell that in a conversation with Woolley in early February, 1931, he was impressed with "her complete balance" and that he believed "[s]he had rather a raw deal . . . ", but Angell disagreed with Dewey suggesting rather that Teachers College had been very generous to Woolley and that "[s]he is herself, I think, wholly unaware of the extent to which her memory is affected, and is indisposed to accept the evidence of her friends about it" (Angell, 1931a; Dewey, 1931). Through Angell's influence, however, in March, 1931, Woolley was offered a job in New Haven studying the behavior of visitors to museums which she declined, giving as an excuse that she had very poor visual perception and memory and felt she was not capable of doing the job. Angell expressed his disappointment suggesting that the position was less taxing than teaching and would "furnish a tangible demonstration of the stabilization of your health, without involving any teaching organization in what might seem to them an unjustifiable risk" (Angell, 1931b; Woolley, 1931a).

By late April, 1931, Woolley's resentment of Dean Russell was forcefully expressed in an 11–page typescript, titled "The Experience of Helen T. Woolley in being employed in Teachers College, Columbia University, and in being dismissed from Teachers College." She personally delivered it to his secretary with a note that she intended to send the statement to every administrator with whom she applied for a job. The statement contained a number of errors reflecting impaired recollection which Russell in his reply of May 7th tried to point out. The errors included stating that she gave a speech in Atlantic City in February of 1930 which was actually given in Cleveland in 1929 and persisting in her claim that Russell told her she was being asked to resign for financial reasons. Russell reiterated that "the progress of your recovery was insufficient at the time to make the work of the Institute all that it should have been. . . . It was a painful duty

for me" (Russell, 1931; Woolley, 1931b). Unmoved by Russell's assertions, Woolley sent this statement to a number of people with requests to help her find a new position, among them Angell.

In January, 1933, Woolley consulted Dr. Adolf Meyer the well-known Johns Hopkins psychiatrist (Lidz, 1985) for an evaluation of her mental health. Meyer reported the results of his examination to Dr. Lambert. According to Meyer, Woolley showed no indication of depression but had developed an "obsession of vindication" concerning Dean Russell and Teachers College. "She came to me for a clear bill of health, but the points I drew her attention to made no impression. She objects to my speaking of obsession of vindication." Meyer urged her to take any work she could to show whether her obsession would or would not interfere with her job performance. Further, he pleaded with her to stop sending out her statement, [Woolley, 1931b] but "...I have failed completely on this point and also with suggestions concerning possibilities of errors of memory" (Meyer, 1934).

The obsession did not abate. Over a period of another five or six years Woolley continued to send letters recounting her treatment by Teachers College. In 1934 she wrote another statement which she referred to as her "form letter," giving more of a life story but including three and a half pages on Teachers College. In this statement, too, there are errors. She gave the date of her resignation as February 1931 rather than February 1930 and further claimed that Dean Russell had admitted the truth of her account of her resignation from Teachers College. This is a complete falsehood; Russell (1934) instead urged that if she was going to circulate a statement of her experiences at Teachers College, she append his letter of May 7, 1931 in which he pointed out the errors in her claims. Undeterred, she sent this form letter to friends and to prospective employers (Woolley, 1934b). She produced another typescript in 1936, "An Autobiography of Helen T. Woolley" which is a delightful document up to the point that she begins recounting her experience at Teachers College. Woolley sent these statements to friends, prospective employers and total strangers. She told the story of her treatment to Burkes Landed Gentry to explain why she couldn't purchase their Coronation Edition, to the Eugene Field Society to explain why she couldn't make a charitable contribution; she even recounted her story in a letter to the IRS with her tax return (Woolley, 1937; 1941).

Her friends, colleagues, and family were concerned and some tried to help. Grace Fernald wrote to Angell, "For some time, my beloved Helen Thompson Wooley (sic) has been a matter of great concern for me...." (Fernald, 1931). Helen's sister Lillian queried Angell in May, 1935 about the possibility of obtaining a Carnegie pension for her: "I feel sure she

will never again be able to do any work, for she has not recovered entirely . . . " (Thompson, 1935). In 1936, Ethel Waring, Myrtle McGraw and Bess Cunningham who had been students of Woolley's at Columbia, applied for funds from the Josiah Macy, Jr. Foundation to bring Helen to Cornell and allow her to complete a book she wanted to write on young children. This effort was unsuccessful, but Waring managed to get Woolley an appointment as "Resident Doctor" for a few months to allow her access to children and to the library (Waring, 1936). Adolf Meyer queried Waring about Woolley's behavior and in her response Waring suggested that age might be a factor in Woolley's difficulties finding a position. Waring noted that "She lives so largely in the past both professionally and personally that it comes with a bit of a shock to me . . . " In conversations, Waring related that Woolley seldom went more than a few minutes without mentioning her husband. She observed " . . . Perhaps it is only another indication of age that she lives so largely in her happy past with her husband before his illness and separation. It is as though she denied all that has happened since those more fortunate days" (Waring, 1937).

Woolley's correspondence during this time and subsequently suggests that age was not the primary problem; she continued to suffer both emotional and cognitive impairment. In addition to recounting over and over the tale of Teachers College, she accuses both William Russell and later Edna White of convincing people not to aid her (e.g., Woolley, 1938a). Eleanor Woolley Fowler described her mother as becoming "progressively more paranoid." (Fowler, 1976). As evidence of the cognitive impairment, Woolley, one of only 25 psychologists to have been certified by the American Psychological Association for clinical practice (Fernberger, 1932), in 1938 took a written examination for clinical psychologist for the state of Connecticut. She wrote for three hours on three questions, didn't complete the fourth and did so poorly that she was not invited for an oral examination (Woolley, 1938c). It is also the case that Woolley's happy memories of her marriage probably did not reflect reality. She attributed the cause of her breakdown in 1927 to her husband's "spiritual desertion of me. I had felt him slipping away from me for some time. He had almost ceased writing to me" (Woolley, 1934a). Angell (1931c) in a letter to former colleague Harvey Carr about Woolley alluded to an earlier mental breakdown, " . . . it is true that Mrs. Woolley had a very serious nervous breakdown, not wholly unlike, I imagine, the one she had in her early professional career, after she was at Mount Holyoke." Woolley, herself makes no mention of any earlier illness. She left Mount Holyoke in 1905 to marry Paul Woolley. Eleanor Woolley Fowler recalled finding a letter from her father, Paul, to his father, John G. Woolley asking whether he had to go through with his marriage to Helen. If Angell is correct (and there is no

other documentary evidence of an earlier breakdown), it may be that the first breakdown occurred surrounding an attempt by Paul to avoid marrying Helen. Eleanor characterized the marriage as "unfortunate" and asserted, "I'm sure that my father was not in love with her. She was in love with him" (Fowler, n.d.).

CONCLUSION

Helen Thompson Woolley died at her daughter Eleanor's home in Havertown Pennsylvania on December 24, 1947 of a disecting aortic aneurysm. As Angell put it, "Helen Woolley's case is altogether tragic" (Angell, 1935). She had been an enormously influential psychologist from her early work on sex differences to her attempts to use psychological research in the causes of vocational guidance, child welfare, nursery school education, and mental hygiene. Some of the reasons for history's relative silence about Woolley's achievements are related to her sex and some to the nature of her contributions.

As Furumoto (1987) showed, women who received their PhDs at the beginning of the 20th century were comparable to their male peers in their acceptance into professional organizations such as APA, and in measures of eminence such as the award of a starred entry in *American Men of Science*. (Woolley's entry was starred in the 1921 edition). The occupational status of women, however, was not comparable. As mentioned above, women were under-represented among academics and if they did hold academic positions, they were at lower ranks. Women were better represented among the applied psychologists. Napoli (1981) has pointed out that even with the post-World War I increase in interest in applied psychology, as of 1930, men were still in the minority of applied psychologists. When the American Association of Clinical Psychologists was chartered in 1917, among the 45 charter members were 13 women. Since women comprised only about 13% of the APA, but nearly one-third the membership of this clinical group, this is a good indication of their representation in clinical psychology (Routh, 1994).

By 1939, Finch and Odoroff found about equal numbers of men and women in clinical psychology, but men outnumbered women in industrial psychology, and there were three times as many women as men in school psychology. Unfortunately school psychologists were the least esteemed among applied psychologists because their ranks included a number of "mental testers" who could administer Binet tests but did not have other professional qualifications (Furumoto, 1987). As the numbers of women in a profession increase, the status of that profession tends to decrease

(Ostertag, 1992) and Woolley was in a profession dominated by women, first as a school psychologist and later associated with the nursery school movement. Longevity of reputation tends to be reserved for those who were prominent in higher status professions or who made significant theoretical contributions to their field.

Woolley's theoretical contributions were minimal. She found few sex differences in intellectual characteristics and attributed those she did find to environmental rather than biological differences between the sexes. This is certainly a significant finding, but it has been more significant to contemporary feminist scholars than it was at the time, and some feminist scholars have treated this as Woolley's only contribution to psychology. For example, Lewin (1988) dismisses all of Woolley's applied career, referring to her as "an occasional free-lance child development expert"(p. 65). Similarly, Woolley's monumental work in vocational guidance had no impact on the field. Reviews called it a "painstaking contribution" (Healy, 1926, p. 496) and expressed doubt about the usefulness of her approach to other vocational bureaus (Ryan, 1918). The results of Woolley's research on working children led her to anticipate the questions about criteria for classifying an individual as feeble-minded before the revelations of the Army testing program of World War I, but this discovery has been overlooked (See Milar, 1999). Stevens and Gardner (1982) ignore her testing work in Cincinnati, and categorize her as primarily a clinical psychologist.

It was the force of Woolley's personality as much as her intellect that distinguished her. She was passionate about the welfare of women and children which is reflected not only in her speeches but also in her work with suffrage groups, the League of Women Voters, AAUW, and other organizations. Her prominence was primarily due to her role as a promoter of the usefulness of psychology in association with public schools, and later her insistence on the importance of the nursery school and of the training of women in child development.

I believe one major reason for her obscurity today is the mental illness that ended her career. Unlike John B. Watson or James Mark Baldwin who were fired for immoral behavior, the cause of Woolley's dismissal produced changes in her personality and her behavior. Waring (1937) remarked in her letter to Adolf Meyer that although Woolley's appearance had not changed, she "fails to impress" people unless they know ahead of time who she is. The implication of this statement is that prior to her illness Woolley commanded attention. The changes in her behavior induced by the mental illness drove her friends and colleagues away—even Charlotte Fischer eventually ceased all interaction with the Woolleys (Fischer, 1929). As Edith Campbell wrote to a friend, in September, 1948, "Did you hear of Helen Woolley's death last Christmas?. . . The exquisite beauty, the brilliant

mind, the high fame gone- and the rest is silence-" (Campbell, 1948). Woolley's mental illness led to the silence that has cast a shadow over her accomplishments. This chapter is an attempt to break that silence and recover the story of this complex, brilliant, tragic woman.

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Part 6

The Dynamics of Scientific and Cultural Change

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13

PROMINENCE, SCHOOLS OF THOUGHT AND SOCIAL TRANSFORMATION

Thomas C. Dalton

FACTORS INFLUENCING PROMINENCE

The contributors to this book have identified numerous factors that affect the likelihood of attaining prominence in psychology or other disciplines. While these include diverse elements that involve personal, intellectual, institutional, professional and political considerations, I think they usefully can be grouped into 6 broad categories to assess how they interact and contribute to understanding the processes of intellectual and cultural change. These categories include: proprietary control; cognitive orientation; research style and funding; rivalries; intellectual leadership and boundary crossing; and science and social transformation. I make no pretense of providing an exhaustive analysis. Instead, I offer some impressions and pose some questions, drawing on recent studies of the personal and social dynamics of intellectual change. My hope is that this will inspire subsequent researchers to use more rigorous methods to better understand the dynamic relationship among prominence, schools of thought and movements for social change.

Proprietary Control

Originality and Synthesis

Some psychologists effortlessly get credit for ideas that often are not original with them. It seems pertinent then to determine when a discovery or idea is recognized as truly novel and when the assignment of ownership is appropriate and legitimate. Dean Simonton argues that great psychologists who maintain their high standing over time do so because of the enduring impact of their ideas as measured by the frequency of citation of their publications. Psychologists in this select group tend to be concentrated among those who made pioneering contributions early on in the history of the field. They tend to be founders of schools of thought that endure and have attracted widespread attention and commentary. Their possible debt to previous thinkers may be difficult to trace however, because that would involve assessing the influence of philosophers and scientists who predate the formation of the discipline. Freud and Dewey are cases in point because they creatively reinterpreted respectively Darwin's ideas about human phylogeny and Hegel's conception of mind that have become disputed. Freud surrounded himself with followers who pledged allegiance to orthodoxy and thus thwarted efforts by critics, such as Jung and Adler to challenge his status as a founder or to advance alternative schools of thought (Sulloway, 1979). Dewey's rapture with Hegel alienated his fellow pragmatists Peirce and James, whose criticisms eventually forced Dewey to withdraw explicit references to Hegel, even though Dewey continued to convert Hegel's phenomenology of mind into testable scientific hypotheses about the brain and human judgment (Dalton, 1997; Dalton, 2002).

Importanly, as Simonton observes, great psychologists get recognized for formulating well defined easily understood theoretical positions that persuasively address popular or controversial public issues. But their challenge is how to sustain public visibility and continue to capture the public limelight. The resources of prominence gained from a creative and original idea may be depleted over time if recipients stray from their intellectual point of departure. For example, B. F. Skinner seems to have attracted increased criticism in his later career for his outspoken views about the limitations of human freedom and dignity that were implicit in his conception of operant conditioning. Conversely, the reputations of original minds grow larger when their ideas are applicable to new fields of knowledge, as is the case with Otto Selz and Gerald Edelman.

But conversely, the significance of a discovery can be oversold undermining the discoverer's credibility. Dewsbury's amusing description of James McConnell's excesses in promoting and attempting to popularize

his notion of "memory molecules" from his research on flatworms lost credibility because he overextended the results of his studies. In doing so, McConnell alienated the scientific establishment and hastened the disappearance of citations of his work in the journal literature. And Myrtle McGraw's difficulties highlight one other salient point. Her notoriety for being mistakenly credited with adopting a theory that she actually challenged attracted attention for the wrong reasons and sidetracked her career into frustrating efforts at "disownership."

An important asset possessed by only a few "pioneers" or "founders" is their ability to cleverly combine or synthesize prevailing ideas into a coherent and novel theory. The case is often made that important ideas are rediscovered, (or that they are co-discovered, e. g., pragmatism and evolution) and that the zeitgeist or general climate of the times favored the emergence of a theoretical point of view that defeats claims of ownership. The idea of human evolution was discussed and disputed long before Darwin entered the debates. His unique contribution was the explication of a testable theory that could consistently explain the diverse evidence. But only when Dewey and Baldwin brilliantly attempted to synthesize phylogenetic and ontogenetic perspectives did the individual developmental implications of Darwinian evolution become more clear to psychologists and social scientists.

Similarly, William James brilliantly summarized and synthesized psychological research conducted several decades before his *Principles of Psychology* was published in 1890. James' *Principles* had a philosophical edge attacking the metaphysics of Cartesian faculty psychology and the realism of British associationists. Charles Peirce and Dewey shared with James the honor of being co-discoverers of pragmatism, who wanted to advance philosophy and science without declaring allegiance to some higher truth. James and Dewey best expressed the philosophical spirit that led American psychologists to embrace experimentalism. But ironically neither James nor Dewey (nor Baldwin) accepted or endorsed the inevitable split between philosophy and psychology that doomed the scientific study of mind and consciousness. As Rand Evans (and see Evans, 1981) argues, James was well on his way to formulating a naturalistic conception of mind and consciousness underpinning his pragmatist conception of belief and knowldege. Dewey also was motivated by his disenchantment with psychological theory to advance a theory of mind that would unite developmental and social psychology. But White concludes that psychologists' failure to investigate the relationship between these three realms of mind, consciousness and society led to disappointing policy consequences decades later. And as Corballis and Lea astutely observe, developmentalists and social psychologists rediscovered the significance of mind as an object of comparative

study only when researchers investigated the mind reading capabilities of chimpanzees.

Novel Methods and Context

A compelling incentive to engage in the day-to-day but laborious practice of science is the reward of recognition for making an important discovery. The Nobel Prize is the ultimate expression of the value that nations place on scientific innovation and ingenuity. But the prize is given retrospectively only after a long period in which the scientist's methodological and theoretical impact can be properly assessed. A crucial consideration in scientific fields is whether the methods involved in an initial discovery contribute to additional discoveries that increase understanding of the phenomena under investigation. Admittedly, it takes time for new methods to sink in and be accepted. But behaviorism introduced a slavish adoption of stimulus and response methods that emphasized the virtue of replication rather than innovation, which may have contributed to its demise. As Wozniak points out, some ideas are doomed to forever remain on the drawing boards for lack of appropriate methods. Discoveries that involve controversial methods can also delay acceptance and recognition, Wozniak contends, because of a lack of intellectual preparedness. As Dalton and Baars illustrate, scientists were not persuaded by initial experimental evidence that experience alters brain growth and function. Several more decades of experimentation were required to furnish corroborating evidence by different methods for opinion to change.

There also is a tendency among psychologists and social scientists that Dewsbury documents to decontextualize their data by converting problematic results into accepted phenomena. This draws more attention to and citations of theoretical or conceptual considerations rather than consideration of methods and evidence that would support alternative hypotheses and tests (see Cozzens, 1985 and Cole, 1983). In their chapter, Tracy, Robins and Gosling show how phenomenon, such as fear, are subject to reinterpretation when a dominant school is challenged. Behaviorists reduced fear and phobias to conditioned responses. Then psychoanalysts construed fear as displaced anxiety. This gave way to the view that fear is a cognitive appraisal of a threat. In each instance fear was redefined to conform to new perspectives and interpretations of the evidence.

McGraw was a victim of contextual misinterpretation by being too closely identified with Gesell and maturationism by her successors who failed to notice important differences in her methods and findings. Even the identity of a profession can be jeopardized by inaccurate and misleading historical judgments about their member's motives and methods. That is why Popplestone contends that psychologists are unlikely to accept as

objective theoretical assessments of their professional impact by outsiders who decontexualize the field's history and reduce psychologists' motives to the acquisition of power and characterize their methods as instruments for social control.

Institutional Support and Professional Connections

Strong institutional ties and professional connections, while clearly conducive to increased visibility, do not always smoothly translate into intellectual recognition and enduring prominence. Several other factors come into play involving personal style, interpersonal relationships and social norms that affect the transition from initial recognition to long term esteem. Otto Selz's estrangement from the professional and intellectual culture of Nazi Germany was devastating. But Selz's failure to cultivate support for his ideas left them adrift until his cognitive heirs realized their true value. Helen Woolley and Myrtle McGraw shared a common prestigious mentor John Dewey, who provided them access to extensive institutional connections at different times in their careers. Woolley excelled on her own in her early career as an educational reformer. But she became increasingly dependent on her network of supporters to sustain her career who were forced to abandon her, when her mental instability clouded her judgment, resisting and alienating her supporters. Baldwin opted for self-exile rather than incur ostracism, "loser rejection" and isolation from his esteemed colleagues. McGraw enjoyed a close but confidential relationship with Dewey and reaped the benefit of his numerous scientific acquaintances in her novel research. But when these connections disappeared, McGraw lost her association with an intellectual mentor that, if publicized, may have proved decisive in enabling her to successfully clarify and defend her unique contributions. McGraw learned perhaps too late that timeliness and decisiveness are crucial to correct impressions and that renewed professional acclaim does not immediately rectify previous strategic blunders.

Cognitive Orientation

Almost from its inception, the field of psychology has been divided among those who adopt quantitative and qualitative approaches and who pursue basic and applied research. These contrasting perspectives periodically produce tension among psychologists about the proper focus of the profession as a whole. Coan's (1979) study of prominence, which relied on the rating of 54 eminent psychologists by 232 experts, indicates that ever since the inception of the field, psychologists have adopted one of two general theoretical orientations. These orientations impose different

restrictions on conceptualization and generalizations about psychological phenomena. Some psychologists favor objective, quantitative, elemental, impersonal, static and exogenist perspectives, while others adopt subjective, qualitative, holistic, personal, dynamic, and endogenist elements. For example, psychologists who adopted objective, elemental and exogenist perspectives included Pavlov, Watson, Skinner and Hull. Those psychologists who adopted contrasting perspectives included respectively Bretano, Adler and Piaget (subjective); Koffka, Allport and Lewin (holistic); and Freud, Hall and McDougall (endogenous). The correlations that Coan (1979, p. 59) found among the six factors indicated the influence of a higher order or general factor of objectivism versus subjectivism that is indicative of the natural versus human science orientation.

Simonton (2002) used Coan's data to examine how the citation measure of contemporary influence correlated with the theoretical and methodological orientation represented by the general factor. Simonton's findings are interesting. Eminent psychologists who received the highest total citations scored lowest on the general factor. Included in this group are those who adopt subjective, qualitative and holistic views, such as James, Dewey, Freud, Adler, Allport and Rogers. The recipients of the next highest total citations included eminent psychologists who scored highest on the general factor, such as Skinner, Watson and Thurstone. They tended toward objective, quantitative and elemental views and took extreme positions. Finally, a third group of psychologists that were located at the bottom of a backward J curve had taken more moderate positions and thus experienced a declining influence. This propensity among psychologists and other social scientists to take extreme positions is not evident among natural scientists described by Dalton and Baars, who sought collaborative and integrative approaches to understanding the brain and mind. That is why Simonton (2002, p. 189) best expresses the dilemma that confronts psychologists and their discipline:

> To become the greatest of the great psychologists in the eyes of posterity, moderation may be an invariable vice. So, the long-term progress of the field may sometimes prove inconsistent with the long-term prominence of its participants. Great psychologists do not always make psychology a great science

Simonton's analysis can't tell us definitively which type of philosophical stance toward psychology best sustains long-term influence in the field. But he speculates that low scorers in the first group produced more literary and memorable writings that included descriptive but personalized terms, such as Freud's "oedipal complex or "James' metaphor of "stream

of consciousness or the "Baldwin effect." James Mark Baldwin was singled out for originating the idea that development functions as an evolutionary principle of selection, Dewsbury observes, even though Lloyd Morgan and H. F Osborn independently came out with the same idea. According to Simonton, these evocative terms tend to be closely identified with their author while impersonal terms like "reaction time" rarely trigger the memory of its originator, F. C. Donders. However, impersonal terms may be more easily assimilated, such as Dewey's concept of "interaction." This term embodies a unique way of understanding natural relationships without imputing linear causality, such as that between heredity and environment or structure and function, that is now a widely accepted principle even though rarely associated with Dewey's name.

Several scholars have proposed provocative general theories that examine the developmental sources of prominence. (See the discussion of rivalries below). These personal accounts (see Gardner, 1993 and Eysenk, 1995) and more general historical comparative studies (see Sulloway, 1996 and Simonton, 2002) analyze the personality traits and experiences that contribute to individual cognitive orientation. Freud and contemporary attachment theorists contend that early adverse emotional experiences negatively affect individual motivation and subsequent development. Family adversity, however, is not necessarily a negative factor. Wundt and Watson overcame the early loss of a parent and Maslow endured family tensions and conflict and subsequently excelled. Dewey believed that the capacity to learn from experience and failure is a valuable skill, but the pressures for professional conformity weigh heavily against risk-taking and recognition. Mentors and supportive colleagues can make a difference whether initiative is encouraged or discouraged, as indicated by Schmitt, Woolley and Yerkes' contrasting experiences. The novelty and diversity of early experiences can enhance creativity and the capacity to recognize and select variations that make a difference. But this requires a tolerance of ambiguity and uncertainty and personal persistence.

Research Style and Funding

The conduct of experimental research has historically provided a potential pathway to prominence. The great German laboratories of the nineteenth century became centers of training for many pioneering American psychologists who subsequently acquired fame (Ben-David & Collins, 1966). Watson obtained his laboratory training at the University of Chicago from American neurologists Donaldson and Child, but he conducted experiments with few collaborators and neglected the brain. Yerkes

encountered resistance from the Rockefeller Foundation when he tried to be true to the German tradition by imposing hierarchical control at his primate laboratory. Even though James Mark Baldwin received training in Wundt's laboratory, he had a low regard for experimental research and ultimately advanced an epistemological rather than developmental theory of human evolution (Wozniak, 1998). In contrast, although declining postgraduate study in Berlin and Leipzig, Dewey embarked with McGraw on perhaps the most ambitious study ever undertaken by an American philosopher involving the development of brain and behavior. Lawrence Frank, a former student and protégé and Rockefeller and Macy Foundation officer, was in sync with Dewey because he shared Dewey's vision of the interdisciplinary conduct of science and interactive conception of mind, brain and behavior. Robert Yerkes also enjoyed an initial affinity with the Rockefeller Foundation, which supported the development of his primate research center. But the scientific tide shifted away from psychobiology to molecular biology stranding Yerkes, who had few supporters or collaborators who might have helped him make this transition successfully.

Scientists enjoyed a considerable degree of freedom to pursue research until institutional sources of funding and organizational support underwent fundamental change. These changes have altered the modes of attaining recognition and prominence. Prior to WWll, large corporations and philanthropic organizations sponsored and funded most of the extramural academic research conducted in institutions of higher education. After the WWll, the United States government provided a growing proportion of funds that steadily increased. During the first half of the twentieth century, the Rockefeller and Carnegie Foundations, the Commonwealth Fund and many other philanthropic foundations strongly supported innovative, interdisciplinary basic scientific and medical research that included psychology and the social sciences. But after the war, the National Science Foundation, the National Institutes of Health and the Defense Department increasingly channeled funds into peer-reviewed applied research projects affecting national security. This was period in which specialization was favored over more generalist approaches. This bureaucratic environment discouraged intellectual creativity and support of idiosyncratic ideas that defied easy classification. The Josiah Macy, Jr. Foundation conferences and the Laurentian conferences on brain and mind filled an important intellectual niche during this period by encouraging prominent scientists to think about unconventional and controversial subjects. Not until the early 1990s were initiatives undertaken by the public sector to rectify this imbalance and to promote cross-disciplinary collaboration in basic and applied research.

Rivalries

Randall Collins (1998) contends that personal and intellectual rivalries have played an important role in the western and eastern philosophical traditions. He views rivalries as products of densely structured vertical and horizontal networks of opposing schools whose leaders and followers engage in competition for dominance and positions of prominence available to those who advance novel ideas. His evidence is compelling that philosophers have engaged in theoretical disputes through the centuries that allocate attention, distribute intellectual capital and emotional energy, produce adherents, and bestow recognition in predictable and recurring patterns of interpersonal interaction. This is not to say that there are impersonal forces or determinants that are working behind the backs of the participants. Quite to the contrary, the struggle for intellectual ascendancy consists of a self-conscious, strategic deployment of arguments, evidence and garnering of institutional support intended to capture and retain popular attention and interest.

The studies presented in this book illustrate how the center of gravity of theoretical debates about key psychological phenomena reflects the changing institutional leverage and professional status of prominent leaders of contending schools of thought. The attributes of character that contribute to eminence, according to Collins, include a desire to solve problems, a keen understanding of one's intellectual predecessors, an uncanny ability to anticipate where the action next will be and a sense of balance and psychological distance from present concerns. Collins and Simonton may differ on how to define and measure prominence. But they concur that prominence is a natural phenomenon that can be explained by using a combination of personal, biographical data, publication and research impact data and information about professional networks that put prominence in individual, social and institutional context.

Conceptual Oversimplification

Rival philosophical ideas have profoundly influenced psychology as an academic and scientific discipline. Several contributors to this volume have shown how contrasting views about the evolution and continuity of species, the role of nature and nurture, the subjectivity of mind and consciousness and the social basis of human development have shaped psychology's theoretical discourse and scientific agenda from the beginning. These contested themes have ancient roots that are supported by theories that tend to occupy the extreme ends of the continuum. As Jerome Kagan (1998) noted, the public finds seductive ideas that oversimplify human behavior, such as the belief that the first two or three years of life

determines a child's future development. This conception of experience is fueled by the disputed idea that early development involves irreversible critical periods even though there is much evidence to the contrary (Bruer, 2001). As McGraw's career attests, this conceptual conflict has perpetuated the nature versus nurture debate in theoretical discourse even though the interaction of heredity and environment is now widely accepted. It may seem pointless and unfair today to characterize McGraw as a maturationist, or to reduce psychology's history to a few prominent but controversial individuals who are unrepresentative of the field as a whole. Yet Wozniak, Popplestone and Dalton show that retrospective judgments about scientific predecessors are often based on oversimplified charicatures of their key arguments and that of their rivals. Nevertheless, it is much simpler to hang on to a problematic stereotype for rhetorical, discursive reasons than to challenge its historical applicability.

The Dynamics of Competing Schools

Several essays in this volume have examined how rivalries affect prominence. What are the possible psychological origins or social dynamics of intellectual and professional rivalries? Collins (1998) contends that rivalries embody not only individual tendencies to debate and dispute ideas, but reflect the relative scarcity of top academic positions and the limited space available for novel ideas to get attention at any one time. He believes that alliances spearheaded by eminent leaders of contending schools of thought produce temporary coalitions of like-minded adherents who struggle to maintain or gain ascendancy and control over leading academic and scientific institutions and journals. The currency of intellectual influence and institutional power is circulated and exchanged through interpersonal networks involving mentors and students who transmit, interpret, advance and defend ideas over time.

The evidence presented in this book suggests that the formation of alliances and coalition building appear particularly crucial when a new discipline is being formed and when a new school of thought is struggling to define itself and gain recognition. The creation and ascendancy of an academic discipline requires a strong intellectual and professional identity, institutional support, theoretical unity, the continuous production of graduate students who secure positions in prestigious universities and the collective will to fend off institutional competitors. However, the processes by which well-established schools of thought are challenged and ultimately superceded in their dominance involve a prolonged period of critical theoretical reexamination, changing views about key phenomena to be explained and piecemeal revisions and defections by key adherents rather than complete abandonment.

For these reasons, forming a school of thought and sustaining its influence by absorbing competitor's ideas are likely to be more successful than for challengers to displace or replace it with an alternative. Coan's (1979) interesting factor analysis supports the contention that schools undergo *cyclical* periods of popularity and decline. Their eventual reemergence sometimes involve a blending or synthesis of contending perspectives. Introspection and experimentalism shared a precarious co-existence in the limelight before the turn of the last century until Titchener, James and Dewey's influence waned and Freud, Thorndike, Binet and Pavlov attracted increased interest and adherents respectively to their psychoanalytic, learning and reflexological theories. Then from 1910 through the 1930s, Watson's behaviorism grew more dominant. During this time, behaviorism coexisted with psychoanalysis and gestalt psychology made large inroads in American thought through Köhler, Wertheimer and Koffka's influence.

However, by the early 1940s, Hull and Lewin cleverly combined behavioral and psychoanalytic perspectives that attracted increased interest. Tolman and Miller also introduced a cognitive orientation and Hebb and Lashley demonstrated the emerging importance of brain science in learning and information processing theory that had waned in previous decades. Despite this intermingling of different theoretical perspectives, Skinner reintroduced an uncompromising behavioral theory that remained dominant through the 1960s. Skinner's success in overshadowing rival theories is hard to explain unless other factors are considered that buttressed his efforts, particularly the attempts by logical positivists to eliminate as "subjective" all reference to mental states involving attributions of intentionality and consciousness (see Baars, 2003 and Dalton, 2003). However, by assuming a highly visible role as champions of behaviorist theories, Skinner and his allies were put on the defensive, forcing them to invest more time responding to critics than making new discoveries. These theoretical disputes ultimately weakened behaviorism's hold on scientists.

Psychological and Social Origins of Rivalries

There is abundant evidence presented in this book that prominence thrives in networks dedicated to competing perspectives, but there are also numerous examples of interdisciplinary movements led by prominent scientists who cross boundaries and cultivate consensus rather than accentuate theoretical differences. Collins contends that the history of philosophy can be understood best through the sociology of knowledge. He believes that the processes of argumentation and quest for intellectual influence can be explained partly by recurring patterns of ritualized institutional expression. But this does not explain very well the psychological and

developmental factors (see cognitive orientation) that impel individuals to seek recognition for their intellectual attainments by challenging the status quo. A Darwinian perspective may do a better job in this regard.

Frank Sulloway (1996) argues that the roots of intellectual rivalry can be traced to the dynamics of birth order, sibling rivalry and intergenerational competition for social recognition and esteem. In his longitudinal statistical study, Sulloway found that birth order influences the receptivity to innovation and change in a number of ways by affecting temperament, niche strategies, social and political attitudes, and openness to new experiences. Children are confronted with a struggle for parental recognition and support not unlike those faced by hatchlings that must compete, sometimes fiercely, for food and room to grow in a crowded nest and flourish as adults. Firstborns are favored by their circumstances to be temperamentally selfish, to be assertive, persistent and ambitious, to adopt conservative attitudes toward social and political change and to be wary of the value of new experiences. Laterborns tend to be more tolerant and intrepid. They seek challenges and take risks. They also tend to be socially unconventional, favor radical political views and are more open to new experiences. Accordingly, firstborns are more likely to propose and support theories or accept discoveries that leave status structures and conventions unchallenged while laterborns favor new ideas that challenge the status quo.

Sulloway argues that the phenomenon of sibling rivalry strongly influences the "life cycle" through which controversial scientific innovations, discoveries and theories are assimilated and accepted. Importantly, Sulloway challenges Kuhn's theory of scientific revolutions at several key junctures. For example, Kuhn (a firstborn who challenged conventional views about he conduct of science) contended that revolutionary scientific change happens primarily through a non-rational process of conversion to a new way of thinking. But Sulloway found that most of the variance in adoption tendencies was associated with pre-existing differences in attitudes towards innovation not conversions over time. This phenomenon of preadaptation is more consistent with Darwinian evolution whereby birth order promotes diversity and different strategies to understand the world.

Whether or not Sulloway's theory accurately predicts rivalries among psychologists must await further analysis. A preliminary analysis by Terry (1989), who used the *History of Psychology in Autobiography* series as its data source, found a significant birth order effect, as nearly 52 percent of prominent psychologists were firstborns or only children. Hall, James, Washburn, Calkins and Binet were firstborns as was Skinner, Piaget, Gesell and Freud. They appear to fit Sulloway's profile of personality and achievement fairly well, although James became increasingly critical of laboratory methods.

Wundt, Dewey and Watson were laterborns but Wundt and Watson's profile seem divided between early careers dedicated to challenging the status quo and then defending their perspectives from challengers in their later careers. Dewey's philosophical and political stances were more closely aligned contributing to a more consistent laterborn profile. Interestingly, McGraw was a firstborn who fits much better the expected profile of a laterborn, such as Woolley.

Despite these and other exceptions to birth order predictions, Sulloway does make telling criticisms of Kuhn's theory that are useful. For example, Sulloway contends contrary to Kuhn, that radical scientific revolutions in science are rarely preceded by a crisis. In his survey of 28 scientific innovations, Sulloway found that laterborns were typically half a century ahead of firstborns in their willingness to endorse radical innovations. Alternatively, crises are more likely to precede technical revolutions when the evidence of breakdown is clear but the solutions are slow to emerge. Although laterborns are the first to rebel in this instance, firstborns will be more cautious and wait until new techniques show real promise in solving a problem before acknowledging the urgency of a situation.

Finally, Sulloway challenges Kuhn's notion that scientists who advance opposing theories are unable to understand each other's perspectives because of incommensurate conceptual schemes. Laterborns who favor a radical theoretical perspective that challenges conventional thinking apparently do not experience incommensurability, but rather adeptly translate back and forth between old and new ways of thinking. Sulloway attributes Darwin's success (a laterborn) in advancing his theory of evolution in part, to his ability to show that evolution and creationism were commensurate and that only evolution could consistently explain significant gaps in our knowledge of how seemingly diverse animals are related as species.

This capacity to bridge competing perspectives is evident in Premack and Woodruff's discoveries of chimpanzee mind reading. Corballis and Lea describe how this spurred comparative studies of theory of mind, capabilities that have narrowed conceptual differences between scientists regarding the continuity of species. As Dalton and Baars and van Strien and Fass indicate, Herbert Simon, a later born, and other pioneers in cognitive psychology deftly demonstrated that behaviorists could embrace this new perspective without completely jettisoning behavioral premises. Similarly, Schmitt and Jasper (firstborns and apparent exceptions to birth order dynamics) found a common language to enable communication between physics, neurobiology and many other disciplines that stood in the way of an integrated study of mind and consciousness.

Intellectual Leadership and Boundary-Crossing

Most prominent psychologists have attained notoriety by formulating theories whose generality is limited to a narrow range of phenomena. Only a few have advanced ambitious theories, described in this book, that encompass phenomena, such as mind and human inquiry, which attract researchers in different fields. Still others have migrated to the fields of psychology and neuroscience from other disciplines and proposed novel ways to view familiar phenomena. Some scientists and theorists stand a better chance of attaining prominence that stake out a narrow domain in which their talents and merit can be properly assessed by experts in the field. Skinner, Watson, Hull and Thorndike seem to fit in this mold. But Gardner (1993) argues that those aspiring to make important breakthroughs must be willing to challenge conventional ideas, cross domains, and be prepared to endure a sometimes prolonged status as a pariah figure at the margins of a chosen field of knowledge or practice. The founders of German and American psychology seem to fit the latter career strategy. Wundt was a physiologist, Freud was a neurologist and James was a physiologist and philosopher. But each created new roles in the fields of psychology and psychiatry. Similarly, the early pioneers in cognitive psychology came from outside the discipline, such as Noam Chomsky, Jerry Fodor and George Miller (Baars, 1986). How do we assess the opportunities and challenges that these different strategies pose for introducing new perspectives, attaining prominence, or extending intellectual influence into other fields of inquiry?

Baars (1986) describes how psychologists and scientists in other fields played different but strategically essential roles in the ascendancy and dominance of cognitive science in psychology. The terms he uses to describe these different roles include "adapters," "persuaders," and "nucleators" (and synthesizers—one other role discussed before). These roles help us assess the relative effectiveness of those scientists and theorists, described in this book, who have pursued different pathways to prominence, particularly those individuals who have bridged different fields.

Some scientists and intellectuals are better at adapting ideas to fit new circumstances rather than trying to persuade colleagues to accept novel, untested theories. Adapters are able to anticipate and adjust with the changing times. They sometimes develop new methods that increase understanding of human neural or cognitive functions that are considered significant but poorly understood. They may not get recognition until the significance of their contribution becomes apparent many years later.

Otto Selz and Herbert Jasper seem to have been successful adapters. Selz saw a way around behaviorist logic and the epistemological

conundrums of the Würzburg School and gestaltists to demonstrate how thought becomes productive. The computing revolution eventually provided the right context for Selz's ideas. Jasper introduced but did not perfect techniques to obtain electrical recordings of the brain that enabled neuroscientists to view the brain in its entirety rather than as separate localized functions to better understand the reticular processes supporting attention. Jasper employed these same adaptive (and persuasive) skills to lead his colleagues into a post-WWII era that required accommodation, international cooperation and collaboration for world peace. Institution building may not generate personal recognition and accolades but the science establishment is certainly better off with robust mechanisms for communication and collaboration.

Scientists who are adept at persuasion present another style for the advancement of intellectual change. Baars (1986) contends that Ulric Neisser and Ernest Hilgard typify this role model. These psychologists were nonconformists who became disenchanted with behaviorism and brilliantly enunciated the principles of cognitive psychology before this became a well-defined research program. Persuaders have the ability to present a vision and lead others to adopt it before fully comprehending its implications. This requires communication skills and sustained interpersonal interaction. Warren McCulloch, who led the Macy conferences on cybernetics possessed these skills but he alienated neurobiologists whose support he needed to advance models of machine intelligence. Francis Schmitt was more successful in this role (as was Jasper) because he persuaded molecular biologists and physicists that they would be more scientifically productive by joining forces to explore the "biophysics of the mind." Schmitt was enormously successful in this endeavor even though he attracted little public notoriety for his leadership. He assembled a network that has been sustained without insisting on being credited as its founder.

Philosophers through the centuries are held in awe for the depth of their thinking even though many of their ideas are elusive and defy easy comprehension. That is why the ability is rare to develop a clear and concise theory that attracts attention among practitioners in both the natural and social sciences and humanities. Clearly Darwin and Freud deserve recognition for forming the nucleus of a cluster of related ideas whose evocative metaphors and theoretical principles about evolution and the human mind have had widespread cultural impact. There are several contemporary scientists who also fit well the role of "nucleators," because their ideas and concepts have acquired a breadth and depth of meaning and stimulated research in realms well beyond their disciplinary origins. Noam Chomsky and Herbert Simon bridged the natural and social sciences by furnishing

respectively a theory of language and model of information processing that inspired research by linguists, psychologists, anthropologists, mathematicians, computer scientists and neuroscientists among others. Similarly, Gerald Edelman has captured the imagination and interest of diverse scientists and scholars in the humanities with his theory of "neural Darwinism." Edelman's theory makes the brain and mind accessible to science and its workings comprehensible to non-scientists. These many examples underscore an important point that the debts accumulated from attaining prominence are redeemable when seminal ideas become the common possession of society and culture.

Science and Social Transformation

The production of knowledge involves a close but unstable relationship among educational institutions, industry, government and foundations. Each sector has its own vested interests in its "special" knowledge and jealously guards its right to advance its own vision of how best to employ this knowledge for social betterment. Sometimes each sector's interests come into conflict when the norms, ethics and expectations governing professionals in different institutions are disputed or challenged. The philosopher Karl Popper contended that the best antidote to despotism is an open society and democracies excel at publicizing and politicizing disputed issues, including scientific ones. This enables the public to hold officials accountable for their actions but it also puts scientists in the uncomfortable position of having to justify their knowledge and to be held accountable for the social consequences of that knowledge.

Before WWl, Thomas Edison epitomized the average American's image of a scientist as an independent, brilliant but isolated and aloof laboratory-based researcher. American's were relatively unconcerned with the practice of science and political leaders devoted little attention to the organization of science, its relationship to education and public accountability. Dewey led the pragmatist social movement to reform public education by introducing an experiential and experimental approach to learning. Dewey believed that schools and communities could enlist science in intelligent action dedicated to democracy and the public good. For his part, Dewey believed that the pattern of scientific inquiry he endorsed is "foreshadowed" in the developmental processes in which humans acquire judgment. Accordingly, Dewey considered mind and inquiry natural phenomena that could be understood scientifically. From his perspective individual development, the conduct of science and democratic governance followed complementary pathways but whose convergence required human guidance and intervention.

In Germany, the debate between physicists Ernst Mach and Max Planck about the ends of science and its role in public education also attracted considerable public attention from 1908–1913 (Fuller, 2000). Mach was an instrumentalist who wanted science to be publicly accessible, educationally functional and capable of achieving an economy of thought and behavior. Plank took a different tact. He was a realist who asserted the independence of science and its need to protect its own professional identity and values. Plank was wary of democratizing influences, which he believed would jeopardize the practice and unity of science. Selz participated in the German debate and strongly endorsed the democratic agenda that Mach advocated and that Dewey espoused in America.

Kuhn resumed this debate in the 1960s about how the practice of science reflects the underlying consensus about its claims to knowledge. Psychologists, among other academically trained professionals, have encountered increased but conflicting pressures since the turn of the twentieth century to justify their professional status by invoking their unique claims to knowledge, while also collaborating with other professionals to fulfill their responsibility for human betterment. In his chapter, Popplestone complains that psychologists have been judged harshly by outsiders for promoting their own professional interests without adequate intellectual or political justification. Popplestone's compliant is justified because the field of psychology is not monolithic. It consists of a diversity of subfields, contending theories and different views about the profession and its role in public policy (Dewsbury, 1996). The subdisciplines of psycholoogy continue to proliferate and take new forms that it is anybody's guess what the field will be like in another 100 years (Evans, 1999). Non-psychologists who criticize psychology for supporting clinical and therapeutic approaches also are unfair because psychologists have been divided for years about how to balance basic and applied research and the need to promote human welfare—a debate that goes to the heart of the issue of professional identity. Moreover, psychologists can hardly be convicted of being parochial or imperialistic about their special knowledge because historically the field was "founded" and transformed by individuals whose initial discoveries and theoretical contributions were made in other fields (see Ross, 1967 and Baars, 1986).

Scientific discoveries and theoretical breakthroughs confer advantages of prestige and notoriety that quite properly go to the discoverer or co-discoverers. This is a powerful incentive for those involved in the conduct of science. But this attainment should not be construed as evidence of the superiority of the field of knowledge that sponsored the discovery. Instead, the true worth of such an achievement should be measured in terms of how much collateral research is generated in other scientific fields,

how much collaboration is encouraged and whether different sectors and communities are engaged in democratic processes of social betterment.

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