

Fall 1983

HENRY A MURRAY AND THE HARVARD
PSYCHOLOGICAL CLINIC, 1926-1938: A
STRUGGLE TO EXPAND THE
DISCIPLINARY BOUNDARIES OF
ACADEMIC PSYCHOLOGY

RODNEY GLENN TRIPLET

University of New Hampshire, Durham

Follow this and additional works at: <https://scholars.unh.edu/dissertation>

Recommended Citation

TRIPLET, RODNEY GLENN, "HENRY A MURRAY AND THE HARVARD PSYCHOLOGICAL CLINIC, 1926-1938: A STRUGGLE TO EXPAND THE DISCIPLINARY BOUNDARIES OF ACADEMIC PSYCHOLOGY" (1983). *Doctoral Dissertations*. 1407.

<https://scholars.unh.edu/dissertation/1407>

This Dissertation is brought to you for free and open access by the Student Scholarship at University of New Hampshire Scholars' Repository. It has been accepted for inclusion in Doctoral Dissertations by an authorized administrator of University of New Hampshire Scholars' Repository. For more information, please contact nicole.hentz@unh.edu.

INFORMATION TO USERS

This reproduction was made from a copy of a document sent to us for microfilming. While the most advanced technology has been used to photograph and reproduce this document, the quality of the reproduction is heavily dependent upon the quality of the material submitted.

The following explanation of techniques is provided to help clarify markings or notations which may appear on this reproduction.

1. The sign or "target" for pages apparently lacking from the document photographed is "Missing Page(s)". If it was possible to obtain the missing page(s) or section, they are spliced into the film along with adjacent pages. This may have necessitated cutting through an image and duplicating adjacent pages to assure complete continuity.
2. When an image on the film is obliterated with a round black mark, it is an indication of either blurred copy because of movement during exposure, duplicate copy, or copyrighted materials that should not have been filmed. For blurred pages, a good image of the page can be found in the adjacent frame. If copyrighted materials were deleted, a target note will appear listing the pages in the adjacent frame.
3. When a map, drawing or chart, etc., is part of the material being photographed, a definite method of "sectioning" the material has been followed. It is customary to begin filming at the upper left hand corner of a large sheet and to continue from left to right in equal sections with small overlaps. If necessary, sectioning is continued again—beginning below the first row and continuing on until complete.
4. For illustrations that cannot be satisfactorily reproduced by xerographic means, photographic prints can be purchased at additional cost and inserted into your xerographic copy. These prints are available upon request from the Dissertations Customer Services Department.
5. Some pages in any document may have indistinct print. In all cases the best available copy has been filmed.

**University
Microfilms
International**

300 N. Zeeb Road
Ann Arbor, MI 48106

8403940

Triplet, Rodney Glenn

HENRY A. MURRAY AND THE HARVARD PSYCHOLOGICAL CLINIC, 1926-1938: A STRUGGLE TO EXPAND THE DISCIPLINARY BOUNDARIES OF ACADEMIC PSYCHOLOGY

University of New Hampshire

Ph.D. 1983

**University
Microfilms
International** 300 N. Zeeb Road, Ann Arbor, MI 48106

**Copyright 1983
by
Triplet, Rodney Glenn
All Rights Reserved**

HENRY A. MURRAY AND THE HARVARD PSYCHOLOGICAL CLINIC, 1926-1938:
A STRUGGLE TO EXPAND THE DISCIPLINARY BOUNDARIES
OF ACADEMIC PSYCHOLOGY

BY

RODNEY G. TRIPLET

B. A. (Psychology), Stetson University, 1976

M.A. (Psychology), University of New Hampshire, 1980

A DISSERTATION

Submitted to the University of New Hampshire
in Partial Fulfillment of
the Requirements for the Degree of

Doctor of Philosophy
Graduate School
Department of Psychology

September, 1983

ALL RIGHTS RESERVED

(c) 1983

Rodney G. Triplet

This dissertation has been examined and approved.

William R. Woodward

Dissertation director, William R. Woodward
Associate Professor of Psychology

David E. Leary

David E. Leary, Associate Professor
of Psychology

Ellen S. Cohn

Ellen S. Cohn, Assistant Professor
of Psychology

Robert M. Mennel

Robert M. Mennel, Professor of History

R. Valentine Dusek

R. Valentine Dusek, Associate Professor
of Philosophy

March 31, 1983

Date

This dissertation is dedicated to my grandmother

MABEL C. SCHEUERMAN

who taught me that a sense of humor is the most
indispensable thing in life

ACKNOWLEDGEMENTS

Writing a dissertation is a paradoxical endeavor. It is at the same time an intensely solitary task and a group effort involving many collaborators. A number of people have provided me with invaluable assistance in the completion of this dissertation, and I want to take full advantage of this opportunity to express my gratitude to them.

Special thanks must first of all be given to Dr. Henry A. Murray. He has been much more than just a willing subject of my investigations. He has graciously granted me many interviews, telephone conversations, and exchanges of written correspondence. His cooperation and encouragement was a major factor in the emergence of a finished dissertation from the vaguely formulated idea I was toying with only two short years ago. He has become a role model whom I can only barely hope to emulate.

I also owe a great deal to the members of my committee at the University of New Hampshire. They have all contributed in their own unique ways to this work. That I have not always followed their advice and the examples they set for me is perhaps the greatest shortcoming of my work.

Whatever errors are found herein can thus be blamed on no one but myself. My advisor, William R. Woodward, introduced me to the study of the history of psychology. In particular, he has done much to stimulate my interest in social and institutional analyses of the development of the discipline. David E. Leary has been a great influence on those aspects of my work dealing with the theoretical analysis of major trends in psychology thought. He has given me a profound respect for intellectual history.

R. Valentine Dusek and Robert M. Mennel have done much to help me bridge the intellectual and methodological boundaries between psychology, philosophy, and history. This has proven crucial in my efforts, as a psychologist, to write an historical dissertation that hopefully transcends the traditional boundaries of disciplinary historiography. In contrast, Ellen S. Cohn has been responsible for keeping me in contact with my professional identification as a psychologist, and the implications of this for my academic career. Throughout the writing of this dissertation, her concise critiques have served as a pragmatic influence that has kept me from straying off on unnecessary, irrelevant, and time-consuming tangents. Finally, I would like to remember the late Ronald E. Shor. His input during the early stages of this work was greatly appreciated, and his untimely passing was deeply mourned.

I would like to thank R. Nevitt Sanford, M. Brewster Smith, Silvan Tomkins, and Robert W. White for giving so generously of their time in granting me interviews and commenting on sections of the dissertation. Eugene Taylor's efforts in helping me gain access to several important archival sources and interviews are greatly appreciated. My appreciation is also extended to the directors and staff of the Harvard University Archives; Rare Books, Countway Library of Medicine, Harvard University; the Rockefeller Foundation Archives; and the Archives of the History of American Psychology. Their assistance was instrumental in carrying out the archival research that formed the foundation of my work. Finally, I want to acknowledge those sources of financial support that made all this possible--the Graduate School, University of New Hampshire for the Summer Fellowship for Graduate Teaching Assistants, the Dissertation Fellowship, and the Central University Research Fund Grant; and the History of Psychology Foundation for the Research Fellowship.

There still remain several individuals who, though they may be listed last, will remain by no means least in my memories of this period of my life. My appreciation and love goes to Christena M. Durost, whose companionship was a refuge from the insecurities and frustrations of life as a graduate student, and whose expertise as a proofreader and

critic saved me from committing many high crimes against the English language. I also thank A. Richard Kindred, my undergraduate advisor at Stetson University for encouraging me to pursue a career in psychology. Finally, I will always remember Michael E. Gorman, Guy E. Cunningham, Loretta Mays, John S. VanSyckel, David B. Sugarman, Sandra L. MacWilliams, and Daniel W. Malloy for their warm friendship and support.

TABLE OF CONTENTS

DEDICATION iv
 ACKNOWLEDGEMENTS v
 ABSTRACT xii

CHAPTER

1. INTRODUCTION: BEYOND DISCIPLINARY HISTORIOGRAPHY 1

 Murray's Significance to Psychology 1
 Major Historical Themes to be Addressed 10
 Methodology of the Thesis 13
 Outline of the Thesis 15
 Reference Notes 17

2. PSYCHOLOGY EMERGES AS AN ACADEMIC DISCIPLINE IN THE AMERICAN UNIVERSITY 24

 Introduction 24
 The Professionalization of Academia 27
 Psychology Takes Its Place in the Modern University 36
 Psychology at Harvard and Its Institutional Status 54
 Reference Notes 58

3. MURRAY'S EDUCATIONAL BACKGROUND: TRAINING IN MEDICINE AND BIOCHEMISTRY 65

 Introduction 65
 Childhood and Education 66
 Research in Medicine and Biochemistry 74
 Conversion to Psychoanalysis 82
 Conclusion 92
 Reference Notes 94

4.	FUNDING THE HARVARD PSYCHOLOGICAL CLINIC: THE POWER OF MONEY AT HARVARD	103
	Introduction	103
	Founding the Harvard Psychological Clinic	103
	The Clinic Faces Financial Difficulties	113
	Aid From the Rockefeller Foundation Begins	118
	Conclusion	131
	Reference Notes	133
5.	CONTROVERSY AND THE CLINIC, 1926-1936: THE INSTITUTIONAL ISSUES OF HIRING, TEACHING, AND RESEARCH	139
	Introduction	139
	The Imperial Presidency at Harvard	140
	The Clinic Stagnates Under Prince: 1926-1928	145
	The Clinic Flourishes Under Murray: 1928-1936	150
	Controversy Surrounds Murray's Work	158
	The Incident of Borinq's Analysis	171
	Conclusion	174
	Reference Notes	176
6.	MURRAY'S THEORY AND RESEARCH: THE INFLUENCE OF HENDERSON, FREUD, AND JUNG	183
	Introduction	183
	Murray's Self-Education in Psychology	184
	Psychoanalytic Origins of the <u>Explorations</u>	187
	The Influence of Henderson and Whitehead	205
	Proposal of a New Methodology	210
	Conclusion	219
	Reference Notes	221
7.	THE DECISION TO PROMOTE MURRAY, 1936-1937: A CASE STUDY IN DISCIPLINARY BOUNDARY MAINTENANCE	228
	Introduction	228
	Faculty Promotion at Harvard Prior to 1934	229
	President Conant and the Harvard Meritocracy	234

Murray's Promotion and Tenure Review	245
Rockefeller Funding Resolves the Debate	256
Conclusion	259
Reference Notes	261
8. CONCLUSION: MURRAY'S ROLE IN DEFINING THE BOUNDARIES OF ACADEMIC PSYCHOLOGY	270
Introduction	270
Disciplinary Boundaries of Academic Psychology	271
Disciplinary Boundaries of Psychology at Harvard	275
Disciplinary Impact of Murray's Promotion	286
Conclusion	294
Reference Notes	298
BIBLIOGRAPHY	305
Archival Sources	305
Interviews	305
Writings of Henry A. Murray	306
Primary Sources	310
Secondary Sources	317

ABSTRACT

HENRY A. MURRAY AND THE HARVARD PSYCHOLOGICAL CLINIC, 1926-1938:
A STRUGGLE TO EXPAND THE DISCIPLINARY BOUNDARIES
OF ACADEMIC PSYCHOLOGY

by

Rodney G. Triplet

University of New Hampshire, September, 1983

The work of Henry A. Murray at the Harvard Psychological Clinic played a major role in shaping the face of modern American academic psychology. Prior to 1930, proponents of pure and applied psychological research debated the inclusion of personality and psychotherapy in the curriculum. Today, it is generally accepted that Murray was instrumental in broadening academic psychology's boundaries to include these topics. This dissertation details how the controversy surrounding Murray's promotion to Associate Professor in 1937 encapsulated the interplay between the intradisciplinary and extradisciplinary forces involved in psychology's efforts to define its intellectual, methodological, and institutional boundaries.

Initially, Murray's work was on the fringes of disciplinary and institutional acceptability. Murray was not trained in academic psychology, but rather in medicine and biochemistry. The Clinic's operations were subsidized by external grants and not from Harvard's own funds. Murray was also constantly in conflict with E. G. Boring, chairman of the Department of Psychology, concerning the administration of the Clinic. Furthermore, Murray rejected psychological reductionism, instead promoting research methods derived from medicine and a theory of personality influenced by psychoanalysis and L. J. Henderson's biological systems theory.

These factors made the outcome of Murray's 1936-1937 promotion and tenure review highly doubtful. However, the Rockefeller Foundation intervened and used its financial influence to secure Murray's promotion and ultimately to further the development of research on personality. This served to allow academic psychology to develop as a broadly defined field that would remain open to interdisciplinary influences.

CHAPTER 1:

INTRODUCTION:

BEYOND DISCIPLINARY HISTORIOGRAPHY

Murray's Significance to Psychology

The 1938 publication of Explorations in Personality: A Clinical and Experimental Study of Fifty Men of College Age marked Henry A. Murray's debut as one of America's leading academic personality theorists. This book was the crowning achievement of his early psychological career, a twelve-year period in which he assumed the directorship of the Harvard Psychological Clinic, and published fifteen articles on basic concepts and research methodologies in the psychology of personality.[1] However, when Murray embarked on his career as a psychologist in 1926, he did not intend to pursue a life of traditional academic research. Instead, he set out to totally revise the psychology curriculum. At this task he was partially successful. Today, the most popular courses offered by college and university psychology departments deal with the topics of personality theory, abnormal psychology, and psychotherapy.[2] That these topics are now accepted within the boundaries of academic psychology is partly a result of the work conducted by Murray at the Clinic.

Prior to Murray's pioneering research, abnormal psychology and psychotherapy were considered to be outside the domain of American academic psychology, and personality research was not conducted on any systematic basis. While academic psychologists constituted only a minority of the members of the profession, many felt that their position in institutions of higher education conferred a special status upon their work. As a result, they tended to look down upon the applied efforts of clinical psychologists and psychometricians, and on the work of fellow academics who expressed an interest in applied research. Furthermore, the proponents of a rigidly empirical and reductionistic psychology, as defined by the behaviorist polemics of John B. Watson and the psychophysical theory of Edward B. Titchener, were gaining prominence within academic circles. Especially at Harvard, the mood within the Department of Philosophy and Psychology was toward restricting research into the nature of human personality, in both its normal and abnormal manifestations, to the domain of practising clinical psychologists and psychiatrists.[3]

It is against this background and departmental politics that the significance of Murray's work becomes apparent. Murray served as a conduit for the transmission of European personality and clinical theories into American academic psychology. While trained as an orthodox Freudian analyst, he had as a graduate student made significant contributions to medical and biochemical research. This unusual

combination of skills and experience allowed Murray to translate the vagaries of psychoanalysis into a unified structure and couple it with a research methodology that would lend scientific respectability to the wholistic investigation of personality by experimental psychologists.[4]

Murray is also considered to be one of the greatest teachers of psychology in American academic history.[5] Among his most well known students and colleagues are R. Nevitt Sanford, Robert W. White, Erik H. Erikson, Jerome Frank, Robert Holt, Kenneth Keniston, Gardner Lindzey, Donald MacKinnon, Saul Rosenzweig, Edwin Shneidman, Silvan Tomkins, and Fredrick Wyatt.[6] Murray's teaching was characterized by the freedom he gave his students to pursue their own interests and fully develop their professional potential. He had, through his contact within the psychoanalytic community, seen the negative consequences of a mentor treating his theory and methodology as rigid doctrine. Instead, he wanted to encourage free and open debate at the Clinic by gathering around himself a loosely organized staff composed of individuals with diverse interests. From this environment of free inquiry, Murray's students were able to fully develop their intellectual potential and prepare for careers in which they would make many important and original contributions to the discipline.[7]

Murray has also made contributions to research methodology in both pure and applied settings. A review of the literature in psychological tests and measurements indicates that the Thematic Apperception Test (TAT), developed by Murray and Christiana Morgan in 1935, is the second most widely used projective test of personality and psychopathology.[8] The TAT is further distinguished by the wide range of applications in which it has been employed. Unlike the Rorschach Ink Blot Test which is limited almost exclusively to clinical use, the TAT has found wide acceptance among both clinicians and academic psychologists.[9] Murray's general research plan--what he referred to as the "multiform method"--has also been adapted by many industrial personnel psychologists to form the core of the employee assessment techniques in use by major American corporations.[10]

In recent years there has been a growing interest in applying many of Murray's ideas, especially those expressed in the Explorations, to current issues in psychology. There has been an increasing awareness among researchers in personality and social psychology that reductionistic research methodologies focusing on the investigation of molecular patterns of behavior have not produced relevant answers to the basic problem of psychology--the understanding of human nature.[11] Against this background of disciplinary crisis, Murray's example has begun to stand out as a viable alternative. Within the past four years

there have been two symposia at conventions of the American Psychological Association devoted to Murray's contributions to psychology. Also during this period, Michigan State University has initiated a bi-annual lecture series dedicated to Murray. Furthermore, interest in the current relevance of Murray's research is being expressed by European psychologists, as evidenced by ongoing research in England. [12]

Murray's impact on academic psychology, however, was not immediately evident to his contemporaries. References to his work in the psychological literature were limited at first to papers published by his students and colleagues. Prior to 1940, these were virtually the only citations Murray received, and it must also be noted that these papers were, for the most part, derived from the ongoing research for the Explorations. [13] During the 1940's Murray's work became gradually more widely known, with references beginning to appear in textbooks on the topics of general psychology, personality theory, and psychopathology. [14] By the end of the 1950's, Murray's reputation had become well established. References to his work could be found not only in the above mentioned sources, but also in a number of texts dealing with a wide variety of psychological topics, including works on applied psychology, psychometrics, and motivation theory. [15]

The explanation for this delayed reception is fairly complex. The research he had published prior to the release of the Explorations in 1938 had reached only a limited audience among psychologists. His articles in the Archives of Neurology and Psychiatry, for instance, had reached an audience composed primarily of physicians and psychiatrists, while the articles he published in the Journal of Psychology seem to have been ignored by most psychologists. This latter journal had been founded in 1934 by Carl Murchison for the purpose of providing psychologists with a source for rapid dissemination for research. Papers submitted for publication were not subjected to peer review, publication being contingent upon the author paying the printing costs. This tended to foster a suspicion (one that is still prevalent today) that the articles published in this journal were of lower quality than articles appearing in a journal that used a peer review selection process.[16] Only the four articles that Murray had published in the Journal of Abnormal and Social Psychology seem to have had any exposure among psychologists.[17]

The Explorations in Personality did not initially fare much better either. The first scholarly reviews were not positive. Richard Elliott was highly critical of the multiform method in a review appearing in the American Journal of Psychology in 1939. He felt that Murray's research methods were too unique, too far removed from accepted psychological practice to constitute a significant

contribution to the discipline. Elliott stated that:

the experimental program [of the Explorations] is not brought sufficiently into line with what has been done outside Harvard, a fault which taints all that the clinic [sic] did with idiosyncrasy, and worse. . . . One clear fact about the clinic group is that it is working in semi-detachment from the main traditions of differential psychology. They have derived little of positive value from this field and have so far contributed little to it.[18]

A similar conclusion was expressed in a review published in the American Journal of Sociology. In particular, Murray's work was criticized for the use of only fifty subjects in the study, and for the complete presentation of the data from only one subject. This, the reviewer claimed, cast serious doubts on the reliability and generalizability of Murray's methodology and findings.[19]

Elliott, however, was not totally critical of the Explorations. He felt that even with the methodological flaws, Murray's work did contain many valuable suggestions for new research ideas that might bear fruit if adequately pursued. For example, Elliott felt that psychoanalysts and psychiatrists would find the Explorations valuable reading.[20] Elliott would not, however, make any statement about the ultimate scientific impact of Murray's work. He would only venture to speculate that:

[a psychologist] is faced with a task too overwhelmingly great to justify him in refusing, either on a priori grounds, or on the grounds of his brief past experience, to make use of help that is offered him from any quarter, even by an often fantastic-seeming intraceptist. For if he does, then someday Henry Murray may be able to taunt him, or a Murray disciple in some later day may taunt his imitator, in the words used by Henry

IV of France: "Hang yourself, brave Crillon, We fought at Arques and you were not there!"[21]

Thus, recognition for Murray's work was to come slowly. Not until the late 1940's did the American psychological community take note of his contributions to the discipline. The most obvious reason for Murray's eventual recognition was the publication in 1948 of Assessment of Men. [22] This book summarized the research Murray conducted during the Second World War in developing techniques for the selection of agents for the Office of Strategic Services (OSS). OSS agents were often required to carry out dangerous missions behind enemy lines, and the United States government needed a means for selecting the most capable agents. Murray's contribution to this effort consisted of modifying the multiform method so that the individual assessment techniques were directly relevant to the tasks an OSS agent would have to perform. While Murray did not feel that this work had proven totally successful, many psychologists were impressed by this demonstration of the application of the multiform method to a specific selection task. This did much to generate renewed interest in the research presented ten years earlier in the Explorations. [23]

A more important, but less visible reason for Murray's growing prestige was a function of the graduate students who studied under him at the Clinic. These students left Harvard to make their own careers in academia, and through their work they spread the word about Murray. Many of those

students who pursued academic careers used the Explorations as a textbook when they taught courses on personality theory and psychopathology. They also conducted research derived from their specific contributions to the research at the Clinic.[24] In this way knowledge of Murray's methodology and theory spread gradually throughout academia. The prestige conveyed by a Harvard Ph.D. did much to place Murray's students in positions at other prominent universities, and they, in turn, produced their own students to carry the message further.

The continued access to graduate students was one of the most important conditions for the ultimate success of Murray in disseminating his approach to the study of personality. Graduate students were crucial for establishing the acceptance of Murray's work within academic circles. Graduate students were, in essence, the core of the "invisible college" that grew up around Murray and assured him his position as a leading figure in American psychology.[25]

In light of his contributions to the discipline, it is surprising that Murray has either been ignored or trivialized by previous histories of psychology. Current textbooks in the history of psychology make scant reference to either Murray or the Harvard Psychological Clinic.[26] Despite his close adherence to psychoanalytic theory in his work, Murray is not mentioned in any of the histories of the

movement.[27] Even though Murray was the director of the Clinic for almost thirty years, he is given only brief mention in Reisman's A History of Clinical Psychology, and the second edition of Boring's A History of Experimental Psychology. [28]

One reason for this neglect in the textbook literature is that Murray's work crossed the standard disciplinary boundaries. From the viewpoint of psychoanalysis, Murray was too eclectic. He did not belong to any of the usual orthodox psychoanalytic schools, striving instead to devise his own theory. Furthermore, his research orientation went counter to the strictly applied interests of the majority of American and European psychoanalysts.[29] Consequently, Murray's research orientation has resulted in his work being ignored by historians of clinical psychology, while his research is viewed as too clinically oriented for inclusion in histories of experimental psychology.[30] What this suggests is that historians of psychology have tended to follow traditional disciplinary and sub-disciplinary distinctions in their writings, and that theory and research not directly fitting these distinctions are overlooked.

Major Historical Themes to be Addressed

Clearly, the evidence for Murray's contributions to academic psychology, and the neglect of his work by previous histories, invites a thorough historical and theoretical

analysis. However, this analysis must not take the form of a presentistic search for the intellectual precursors of current psychological research and theory. A much more historically sensitive approach is called for. The particulars of Murray's career development involve a highly complex interaction of determining factors. One issue is that of Murray's desire to create an interdisciplinary, wholistic theory of human personality including elements of medicine, biochemistry, psychoanalysis, and academic research psychology. The disciplinary boundary issue of what topics were to constitute academic psychology, was represented in Murray's unorthodox educational credentials as an M.D. and Ph.D. biochemist who came to hold a major university position as a psychologist. Also involved here were questions surrounding his involvement in non-academic pursuits, such as the Boston Psychoanalytic Society, and in non-psychological groups like the Harvard Pareto Circle.[31] Murray's education and varied intellectual interests thus provide a significant contrast to the tendency of academic disciplines to establish and maintain their institutional basis through the delineation of specialized intellectual, methodological, and affiliational boundaries.[32]

Another issue has to do with the role of institutional and departmental politics in the development of Murray's career. This is seen especially in the efforts of Edwin G. Boring and Karl S. Lashley to have Murray removed from his position as the Clinic's director in 1936.[33] Particularly

important were the issues of individual faculty autonomy, versus centralized hierarchical departmental organization. Issues of the criteria for faculty promotion and tenure were also at stake. Harvard was in the midst of changing from an adherence to the "gentleman scholar" model to a "publish-or-perish" approach. This underlay the institutional debates surrounding the role of research specialization and productivity as indices of academic merit.[34]

Finally, the study of Murray's career raises the issue of the role of non-academic forces--in Murray's case the financial backing of the Rockefeller Foundation--in influencing disciplinary development. Those who give money rarely give it without attached conditions. They sometimes stipulate that their own particular interests be institutionalized by the recipient of their gift. This has made philanthropy a mixed blessing for academia that has, in some instances, resulted in a major reorientation in institutional policies. This is of particular relevance as the role of philanthropies in the development of the discipline has been generally overlooked by social histories of psychology.[35] Philanthropies and philanthropists have been involved with the setting of policy in American higher education ever since the founding of Harvard University in 1636, and their influence is greatest when institutions find themselves in periods of financial need. Harvard during the 1930's was feeling the impact of the Depression (although

not to the extent of many other colleges and universities), and this paved the way for external pressure to influence internal policy.[36]

Methodology of the Thesis

Given the number of issues to be raised in the present work, no single historical methodology will be adequate to present a complete analysis. Instead, three different historical approaches will be interwoven in the attempt to provide a complete and accurate account of Murray's research at the Harvard Psychological Clinic. The basis of the present work will be biographical. The details of Murray's life from his birth on May 13, 1893 to the publication of Explorations in Personality in 1938 will be discussed in terms of their relevance to his choice of psychology as a career. Upon this foundation, elements of social, institutional, and intellectual history will be added to further detail the development of his career; each methodology will contribute a unique perspective upon the whole thesis.

This is particularly important because through the use of multiple historical methodologies a more complete analysis can be made of the diverse factors involved in the origins of modern American psychology than if any one methodology alone was employed. The limitations of single methodology works can be easily seen in a brief analysis of

several recent books on figures of historical importance to psychology. The biography of Sigmund Freud by Ronald W. Clark, while acceptable as a popular biography, fails to offer a cogent analysis of either the intellectual origins of psychoanalysis or of the social and institutional factors in its development.[37] On the other hand, Frank Sulloway in his Freud: Biologist of the Mind is so intent upon making a particular intellectual argument that he either overlooks or ignores biographical and social-historical evidence that suggests an alternative analysis.[38]

An example of a more complete work in the existing literature is Dorothy Ross' biography of G. Stanley Hall.[39] In this work, Ross does intermingle elements of biography, intellectual history, and social history, but not with total success. In particular, the shortcoming of her social/institutional analysis lies in her failure to draw parallels between institutional politics at Clark University and American academia in general. Therefore, the reader is left with the impression that Hall's experience as President of Clark was an isolated incident, rather than a reflection of broader social trends in American psychology and American higher education.

Outline of the Thesis

Due to the number of issues being addressed, the present work is organized according to the major methodological issue(s) raised in each chapter. Chapter Two presents a discussion of the social and institutional background of academic psychology in the 1930's. Particular emphasis is given not only to the rise of academic psychology, but also to the general context of the development of a professionalized system of post-secondary education in America.[40] Chapter Three provides a detailed analysis of Murray's intellectual development, tracing his education and research in history, medicine, and biochemistry. Finally, the chapter concludes with an analysis of Murray's "conversion" to psychoanalysis and a career in psychology.

Chapters Four through Six deal with the operation of the Harvard Psychological Clinic from its inception in 1926 to the publication of Explorations in Personality in 1938. The financial status of the Clinic is covered in Chapter Four, with emphasis given to the close relationship that developed between Murray and the Medical Sciences Division of the Rockefeller Foundation. Departmental and institutional politics at Harvard are the focus of Chapter Five, while Chapter Six details the intellectual origins and interdisciplinary nature of Murray's theory and research. Taken as a whole, these three chapters are crucial to

applying the conceptual issues of academic professionalization and disciplinary boundaries to an understanding of the specific case of the development of personality theory and research by Murray at the Clinic at Harvard University.[41]

Finally, Chapters Seven and Eight present an analysis of Murray's 1937 promotion and tenure review, and the impact of Harvard's decision to continue Murray's contract upon the development of academic psychology. Chapter Seven presents a discussion of how conflicting intellectual, institutional, disciplinary, personal, and financial issues interacted in the promotion and tenure review process at Harvard. Chapter Eight focuses on the underlying assumptions of this process; raising and discussing the issue of how the disciplinary boundaries of academic psychology were established, and the role that external funding sources have played in this process. Finally, Chapter Eight seeks to bring together the various narrative and analytical strands to form a coherent understanding of the present thesis in its full complexity.

Reference Notes

1. Henry A. Murray, Explorations in Personality: A Clinical and Experimental Study of Fifty Men of College Age (New York: Oxford University Press, 1938). For a bibliography of Murray's publications from his early years at the Clinic see Edwin S. Shneidman (ed.), Endeavors in Psychology: Selections from the Personology of Henry A. Murray (New York: Harper & Row, 1981), p. 618. An early appraisal of Murray's contributions to psychology can be found in Richard M. Elliott, "Review of Explorations in Personality," American Journal of Psychology 52 (1939): 453-462.

2. See Henry A. Murray, "Psychology and the University," Archives of Neurology and Psychiatry 34 (1935): 803-817 for a statement of his views on traditional academic psychology. The latter point about the popularity of the "human oriented" psychology courses is derived from the following sources: Survey conducted by the Undergraduate Program Development and Operations Committee, Department of Psychology, University of New Hampshire, Durham, New Hampshire, 1978. Unpublished report; and Survey conducted by the Undergraduate Program Development and Operations Committee, Department of Psychology, University of New Hampshire, Durham, New Hampshire, 1980. Unpublished report. The former reported on a survey of student course preferences at the University of New Hampshire, while the latter reported on a survey of psychology course offerings at a random sample of American colleges and universities.

3. In particular see Edwin G. Boring, A History of Experimental Psychology (New York: The Century Co., 1929); John M. Reisman, A History of Clinical Psychology (New York: Irvington, 1976); and John M. O'Donnell, "The Crisis of Experimentalism in the 1920s: E. G. Boring and His Uses of History," American Psychologist 34 (1979): 289-295.

4. Edwin G. Boring, A History of Experimental Psychology, 2nd ed. (Englewood Cliffs, N.J.: Prentice-Hall, 1950), pp. 706-714; and Murray, Explorations, pp. 36-141. See also Chapters 3 and 6 for a discussion of Murray's graduate education and its relation to his psychological research.

5. Hiram Hayden, "Portrait: Henry A. Murray," The American Scholar 39 (1970): 124, 127-128.

6. Hayden, "Henry A. Murray," p. 127; and Henry A. Murray, "Preparations for the Scaffold of a Comprehensive System," in Psychology: A Study of a Science, vol. 3, ed. S. Koch (New York: McGraw-Hill, 1959), p. 28.

7. Murray, Explorations, p. xi; and Henry A. Murray, Personal interview, 11 November 1981.

8. Anne Anastasi, Psychological Testing, 3rd ed. (New York: Macmillan, 1968), pp. 499-502; Lee J. Cronbach, Essentials of Psychological Testing, 3rd ed. (New York: Harper & Row, 1970), pp. 651-658; and Jum C. Nunnally, Jr., Introduction to Psychological Measurement (New York: McGraw Hill, 1970), pp. 392-394.

9. Nunnally, Psychological Testing, pp. 393-394; and Leo Bellak, "Henry A. Murray: An Appreciation," Journal of Projective Techniques 22 (1958): 143-144. See also the entire issue in which the above article was published to appreciate the range of applications that have been developed for the TAT. Also, Bernard I. Murstein, Theory and Research in Projective Techniques (Emphasizing the TAT) (New York: Wiley, 1963); Edwin S. Shneidman, Walther Joel, and Kenneth B. Little, Thematic Test Analysis (New York: Grune & Stratton, 1951); and Morris I. Stein, The Thematic Apperception Test: An Introductory Manual for Its Clinical Use with Adults (Cambridge, Mass.: Addison-Wesley, 1955). For examples of research employing the TAT see J. W. Atkinson, Motives in Fantasy, Action and Society (Princeton, N.J.: Van Nostrand, 1958); J. W. Atkinson and N. Feather, A Theory of Achievement Motivation (New York: Wiley, 1966); and David C. McClelland, The Achievement Motive (New York: Appleton-Century-Crofts, 1953).

10. Douglas W. Bray, "The Assessment Center and the Study of Lives," American Psychologist 37 (1982): 180-189.

11. In particular, see Kenneth Gergen, "Social Psychology as History," Journal of Personality and Social Psychology 26 (1973): 309-320; and Richard Elms, "The Crisis of Confidence in Social Psychology," American Psychologist 30 (1975): 967-978.

12. The first American Psychological Association symposium was Rae Carlson (chair), "Symposium: Explorations in Personality Forty Years Later," (1978 American Psychological Association Convention, Toronto, Canada). The participants were Nevitt Sanford, "Reminiscences and Celebration;" Ernest R. Hilgard, "Explorations in Personality in Historical Perspective;" Seymour Epstein, "Explorations in Personality Today and Tomorrow: A Tribute to Henry A. Murray;" M. Brewster Smith, "Announcement;" and Henry A. Murray, "Response." A transcript of this symposium can be found in the Alan Elms Record, M1045, Archives of the History of American Psychology. Epstein's presentation was later published under the same title in American Psychologist 34 (1979): 649-653. Sanford's presentation has been published as a chapter in Nevitt Sanford, Learning After College (Orinda, Cal.: Montaigne, 1980), pp. 104-114. The second American Psychological

Association symposium was Edwin S. Shneidman (chair), "Symposium: Aspects of the Personological System of Henry A. Murray," (1981 American Psychological Association Convention, Los Angeles, California). The participants were Nevitt Sanford, "Murray's Personological System is Shaped by Numerous and Various Influences;" Silvan Tomkins, "Personology is a Complex, Life-Long, Never-Ending Enterprise;" Frank Barron, "Personology Requires a Multidisciplinary Approach and Special Techniques of Investigation;" Edwin S. Shneidman, "Personology Studies Living, Historical, Fictional, and Mythological Figures and Complexes;" and M. Brewster Smith, "Personology Encompasses a Wide Range of Concerns, Including Human Values." This symposium was held in part to publicize the publication of Shneidman's Endeavors, a compilation of the essential writings of Murray's psychology. The proceedings of the first Michigan State University Murray Symposium have been published as: A. I. Rabin, Joel Aronoff, Andrew M. Barclay, and Robert A. Jucker (eds.), Further Explorations in Personality (New York: Wiley, 1981). See Rae Carlson, "Personology Lives!" Contemporary Psychology 27 (1982): 7-8, for a review. European interest in promoting the multiform method as the best example of an intensive case study approach to personality research is evidenced in John McLeod to Rodney G. Triplet, 15 March 1982, letter in the present author's possession; and Murray, Interview, 8 July 1982.

13. Carl E. Smith, "A Study of the Autonomic Excitation Resulting from the Interaction of Individual Opinion and Group Opinion," Journal of Abnormal and Social Psychology 31 (1936): 138-162; Saul Rosenzweig, "A Basis for the Improvement of Personality Tests with Special Reference to the M-F Battery," Journal of Abnormal and Social Psychology 33 (1938): 476-488; R. Nevitt Sanford, "The Effects of Abstinence from Food Upon Imaginal Processes: A Further Experiment," Journal of Psychology 3 (1937): 145-159; Walter C. Langer, "An Experimental Critique of the Measures of Learning," Journal of Psychology 3 (1937): 195-221; Isabel Kendig and E. J. Shevack, "Studies in Perseveration: I. A Survey of Researches in Perseveration," Journal of Psychology 3 (1937): 223-230; Isabel Kendig, "Studies in Perseveration: II. Determining Factors in the Development of Compulsive Activity," Journal of Psychology 3 (1937): 231-246; Isabel Kendig, "Studies in Perseveration: III. The Upper Limen for Perseveration and Repetition," Journal of Psychology 3 (1937): 247-251; Robert W. White, "Prediction of Hypnotic Susceptibility from a Knowledge of Subjects' Attitudes," Journal of Psychology 3 (1937): 265-277; and Kenneth Diven, "Certain Determinants of the Conditioning of Anxiety Reactions," Journal of Psychology 3 (1937): 291-308.

14. J. P. Guilford, General Psychology (New York: Van Nostrand, 1939), pp. 473, 605; Silvan S. Tomkins, Contemporary Psychopathology: A Source Book (Cambridge,

Mass.: Harvard University Press, 1946), pp. xiii-xiv, 545; Edwin G. Boring, Herbert S. Langfeld, and Harry P. Weld, Foundations of Psychology (New York: Wiley, 1948), pp. 137, 497, 510; Henry E. Garrett, Great Experiments in Psychology (New York: Appleton-Century, 1941), pp. 121-124; and Gardner Murphy, Personality: A Biosocial Approach to Origins and Structure (New York: Harper, 1947), pp. 370, 562, 670, 672, 863.

15. Abraham A. Roback (Ed.), Present-Day Psychology: An Original Survey of Departments, Branches, Methods, and Phases, Including Clinical and Dynamic Psychology (New York: Philosophical Library, 1955), pp. 237, 492, 494, 550, 552, 554, 560, 862; Don E. Dulaney, Jr., Russell L. DeValois, David C. Beardslee, and Marian R. Winterbottom, Contributions to Modern Psychology (New York: Oxford University Press, 1958), pp. 229, 252; D. E. Klein, Abnormal Psychology (New York: Holt, 1951), pp. 68-69; Frank S. Freeman, Theory and Practice of Psychological Testing (New York: Holt, 1950), pp. 368, 417, 426, 434; J. Stanley Gray, Psychology Applied to Human Affairs, 2nd ed. (New York: McGraw-Hill, 1954), p. 271; McClelland, Studies in Motivation, pp. 63-74, 82, 168, 252, 273, 403-405, 413-123, 465; Raymond B. Cattell, Personality: A Systematic Theoretical and Factual Study (New York: McGraw-Hill, 1950), pp. 156, 162, 178-182, 206, 416; and Calvin S. Hall and Gardner Lindzey, Theories of Personality (New York: Wiley, 1957), pp. 157-205.

16. See in particular Richard M. Elliott to Edwin G. Boring, 29 September 1936, in the Edwin G. Boring Papers. The editorial policy of the Journal of Psychology is made unambiguously clear by the banner "IMMEDIATE PUBLICATION" that appeared prominently on the title page of each issue from Volume 1 (1934) through volume 4 (1937). Current issues deal with this point more discreetly, providing a rationale for the immediate publication policy, as well as a detailed schedule for the publication charges.

17. Gordon W. Allport to Edwin G. Boring, 4 August 1936; and William McDougall to Gordon W. Allport, 13 May 1936. Both letters are in the Gordon W. Allport Papers.

18. Elliott, "Review of Explorations in Personality," p. 454.

19. Kingsley Davis, "Review of Explorations in Personality," American Journal of Sociology 45 (1939): 130-131. An even more critical review can be found in Kimball Young, "Review of Explorations in Personality," American Sociological Review 4 (1939): 579-589.

20. Elliott, "Review," p. 461.

21. Ibid., pp. 461-462.
22. Henry A. Murray, Assessment of Men (New York: Holt, Rinehart, 1948).
23. Henry A. Murray, Personal interview, 3 August 1981; M. Brewster Smith, Personal interview, 28 August 1981; and Silvan Tomkins, Personal interview, 26 August 1981.
24. Sanford, Interview, 28 August 1981; White, Interview, 2 July 1981; Smith, Interview, 28 August 1981; Tomkins, Interview, 26 August 1981; and Henry A. Murray, Personal interview, 11 November 1981.
25. The term "invisible college" is meant to mean the formal and informal communication networks that serve to spread information about research and theory beyond the confines of the college classroom. These networks may take the form of journal readerships, conference attendances, etc. For a discussion see Diana Crane, Invisible Colleges: Diffusion of Knowledge in Scientific Communities (Chicago: University of Chicago Press, 1972); and Derek J. de Solla Price and Donald Beaver, "Collaboration in an Invisible College," American Psychologist 21 (1966): 1011-1018.
26. James B. Chaplin and T. S. Krawiec, Systems and Theories of Psychology, 3rd ed. (New York: Holt, Rinehart and Winston, 1968), pp. 613-620; Mary Henle, Julian Jaynes, and John J. Sullivan, Historical Conceptions of Psychology (New York: Springer, 1973), p. 254; and Gardner Murphy and Joseph K. Kovack, Historical Introduction to Modern Psychology, 3rd ed. (New York: Harcourt Brace Jovanovich, 1972), pp. 429-430.
27. Henri F. Ellenberger, The Discovery of the Unconscious: The History and Evolution of Dynamic Psychiatry (New York: Basic Books, 1970), no reference to Murray; Ruben Fine, A History of Psychoanalysis (New York: Columbia University Press, 1979), pp. 137, 330, 546; Ruth L. Munroe, Schools of Psychoanalytic Thought: An Exposition, Critique, and Attempt at Integration (New York: Holt, Rinehart and Winston, 1955), pp. 45, 142; and Paul Roazen, Freud and His Followers (New York: Knopf, 1976), pp. 296, 515.
28. Reisman, History of Clinical Psychology, pp. 208-209, 221, 231; and Boring, History of Experimental Psychology, 2nd ed. pp. 728-729.
29. Henry A. Murray, Personal interview, 13 July 1981; Henry A. Murray, Personal interview, 13 January 1982; and Henry A. Murray, Personal interview, 8 July 1982.

30. For a statement of their biases, see Reisman, A History of Clinical Psychology, pp. vii-viii; and Boring, A History of Experimental Psychology, 2nd ed., pp. ix-xvi.

31. Ives Hendrick, The Birth of an Institute: Twenty-Fifth Anniversary, The Boston Psychoanalytic Institute, November 30, 1958. (Freeport, Me.: Bond Wheelwright, 1961), pp. 15, 20-23; and Barbara S. Heyl, "The Harvard 'Pareto Circle,'" Journal of the History of the Behavioral Sciences 4 (1968): 218.

32. Bruce Kuklick, The Rise of American Philosophy: Cambridge, Massachusetts 1860-1930. (New Haven, Conn.: Yale University Press, 1977), pp. 451-480, 565-572; and Henrika Kuklick, "Boundary Maintenance in American Sociology: Limitations to Academic 'Professionalization,'" Journal of the History of the Behavioral Sciences 16 (1980): 201-219.

33. See Chapter 7 for a complete discussion of this issue.

34. James E. Conant, My Several Lives: Memoirs of a Social Inventor. (New York: Harper & Row, 1970), pp. 157-171. For an example of the debate on this issue see Gordon Allport to Edwin G. Boring, 17 January 1934; and Gordon Allport to Edwin G. Boring, 27 February 1936. Both letters are in the Edwin G. Boring Papers, Harvard University Archives, Nathan Marsh Pusey Library, Harvard University, Cambridge, Mass.

35. Everett Mendelsohn, "The Social Construction of Scientific Knowledge," in The Social Production of Scientific Knowledge, vol. 1, eds. E. Mendelsohn, P. Weinart, and R. Whitley (Dordrecht, Holland: D. Reidel, 1977), pp. 3-26; Allan R. Buss, "The Emerging Field of the Sociology of Psychological Knowledge," in Psychology in Social Context, ed. A. R. Buss (New York: Irvington, 1979), pp. 1-24; Kurt Danziger, "The Origins of Modern Psychology," in Psychology in Social Context, ed. A. R. Buss (New York: Irvington, 1979), pp. 27-45; and Franz Samelson, "Putting Psychology on the Map: Ideology and Intelligence Testing," in Psychology in Social Context, ed. A. R. Buss (New York: Irvington, 1979), pp. 103-168.

36. Jacques Barzun, The American University: How It Runs, Where It is Going. (New York: Harper & Row, 1968), pp. 138-170; Frederick Rudolph, The American College and University: A History. (New York: Knopf, 1962), pp. 177-200; Laurence R. Veysey, The Emergence of the American University. (Chicago: University of Chicago Press, 1965), pp. 165-170, 346-356; and Merle Curti and Roderick Nash, Philanthropy and the Shaping of American Higher Education. (New Brunswick, N.J.: Rutgers University Press, 1965).

37. Ronald W. Clark, Freud: The Man and the Cause (London: Jonathan Cape and Weidenfeld & Nicolson, 1980); and Rodney G. Triplet and William R. Woodward, "Review of Freud: The Man and the Cause by Ronald W. Clark," Annals of Science 39 (1982): 83-84.

38. Frank J. Sulloway, Freud, Biologist of the Mind: Beyond the Psychoanalytic Legend (New York: Basic Books, 1979); and Hannah S. Decker, "Analysis . . . Frank J. Sulloway. Freud, Biologist of the Mind," Isis 72 (1981): 638-642.

39. Dorothy Ross, G. Stanley Hall: The Psychologist as Prophet (Chicago: University of Chicago Press, 1975).

40. In particular, see Burton J. Bledstein, The Culture of Professionalism: The Middle Class and the Development of Higher Education in America (New York: Norton, 1976).

41. Bledstein, Culture of Professionalism, pp. 278-331; B. Kuklick, American Philosophy, pp. 565-572; and H. Kuklick, "Boundary Maintenance," pp. 201-219.

CHAPTER 2:
PSYCHOLOGY EMERGES AS AN ACADEMIC DISCIPLINE
IN THE AMERICAN UNIVERSITY

Introduction

During the final decades of the nineteenth century a major change was occurring in academia. The all-powerful college president who ruled paternalistically over his faculty and students was replaced by a complex bureaucracy overseen by legions of administrators. The old-time professor who frequently had to lecture on such diverse topics as physics and moral philosophy within the course of any given academic year, gave way to a new generation of eager young specialists fresh from their advanced training in the European graduate schools. No longer could a college education be thought to consist solely of the traditional classical curriculum. Increasing attention had to be paid to the advances in science and the growing awareness of the uniqueness and specialization of methods within the various disciplines. Academia was becoming professionalized.[1]

As research specialization increased, so did the distinctions between the various segments of the university population--students, faculty, and administration. Enrollment requirements were increasingly tightened, culminating in the near-universal acceptance of the successful completion of a prescribed high school course as mandatory for matriculation. This had the effect of increasing slightly the age of the average college student.[2] Faculty, also, were on the average older, reflecting the new emphasis on the Ph.D. or some other certificate of advanced study as a prerequisite for a professorship.[3] Administrators came to focus more on the financial expansion and operation of their institution, and less on fostering the moral well-being of the college community. Therefore, in much the same way that separate academic departments developed as a result of increasing specialization in research, the university bureaucracy became increasingly specialized. The number of Deans proliferated to head the various bureaucratic specialities. Full-time librarians were hired, and the Registrar often wielded more power than the President.[4]

The spirit of specialized intellectual pursuit that characterized the new universities quickly spread throughout American higher education. This was especially true among the new state universities. The stimulus for their growth was the passage of the Morrill Act of 1862, which put the income from the sale of federal lands at the disposal of the

states for use in the establishment of colleges emphasizing technical and agricultural training.[5] It was soon learned, however, that the audience for these practically-oriented courses was limited because of a deep-seated suspicion of formal education among the American working classes. Students came, instead, primarily from the burgeoning middle class. The sons (and eventually daughters) of the middle class flocked to the state colleges and universities seeking the educational and career opportunities afforded by a collegiate and graduate education, and hitherto available only to those within the financial and geographical reach of a few elite Eastern institutions.[6]

Psychology was undergoing changes at the turn of the twentieth century that paralleled the development of the modern university. These changes involved the recognition of psychology as a discipline distinct from philosophy in both content and methodology, and the establishment of an institutional basis for this new discipline. Prior to the 1870's, psychology was conceived of as a topic of philosophical speculation, focusing primarily on the issues of mental logic and personal ethics.[7] The rise of evolutionary theory, though, revolutionized the way scholars could investigate human action. By emphasizing the biological and historical origins of man's capabilities, it was now possible to study psychology as a natural science, rather than as philosophy.[8]

As psychology began its rise to disciplinary status, it was necessary for its founders to delineate its intellectual and methodological boundaries. This was an act necessary for psychology to justify its existence as a separate discipline.[9] There were, however, a number of distinct, and often competing, areas of research that laid claim to the title of "psychology." In particular, a tension soon developed between proponents of pure scientific research on the topics of sensation, perception, and physiological psychology, and advocates of applied research in the areas of education, psychometrics, and psychopathology.[10] Given this growing debate within the fledgling discipline, it was inevitable that controversies would arise over which approach to psychology would be institutionalized at any given university. This would serve to raise concerns over whether psychology would ever be able to achieve the status of a unified discipline.

The Professionalization of Academia

The state of American higher education during the early years of the twentieth century represented the consolidation of a series of reforms that had been vigorously pursued since the 1860's. The Civil War culminated a series of events that served to shake the country out of a backwards-looking idyll and thrust it forward into the industrial age. The traditional college curriculum that emphasized the development of moral character over issues of

intellectual growth found itself at odds with the pragmatic orientation of an increasingly technological society. No longer could the ideal college consist of "[the professor] on one end of a log and a student on the other." [11] The old curriculum was:

too narrow, elementary, and superficial. There was insufficient attention to the German-university ideals of free teaching, study, and research. There was insufficient attention to the technical and practical. The colleges were too sectarian, too undemocratic. Their psychology was faulty; their philosophy, wanting. [12]

One expression of this reform movement was the election of research-oriented men to the presidencies of a number of America's leading educational institutions. In 1869, Charles William Eliot was elected to the presidency of Harvard. Eliot was not overly concerned with Harvard's role in shaping the moral fiber of its students. The requirement that all students live in college housing was dropped, thus drastically reducing the paternal control of the college over the student's lives. In a similar move, Eliot also backed Harvard away from strictly controlling the students' intellectual life. Rigid curriculum requirements were replaced by the elective system, and this freedom to learn went hand-in-hand with the faculty's freedom to teach. Eliot's liberal ideals were coupled with (for the time) generous faculty salaries and benefits. In this way, Eliot was able to attract to Harvard many of the leading scientific researchers of the day. [13]

These educational reforms were also evidenced by the formation of new institutions dedicated to advanced research in the sciences and the applications of technology. In several instances, wealthy industrialists turned their philanthropic attentions towards the foundation of research-oriented universities modeled on the German university system. Examples of this included Johns Hopkins University, Stanford University, Clark University, and the University of Chicago.[14] Of these, Johns Hopkins was the prototype of the new style American university, with its emphasis on advanced graduate research training. Daniel Coit Gilman was appointed as the president of the as-yet unopened university in 1874, and was to guide it through its formative years until 1902. Gilman, however, did not impose a strict copy of the German university system upon his faculty and students. Instead, he fostered an atmosphere of intellectual freedom tempered only by a strong sense of dedication to intellectual achievement as demonstrated through original, scientific research.[15]

As a number of the American institutions of higher education took on the status of universities, new forms of institutional administration became necessary. Unlike the traditional college presenting only the liberal arts curriculum, the new university tried to be all things to all people. Especially in the land-grant state universities, the liberal arts undergraduate curriculum would be coupled with practical courses in agriculture, engineering, and

teacher training, as well as with graduate courses in the sciences.[16] This increase in course offerings, with the concomitant increase in enrollments and faculty, resulted in an expansion of administrative responsibilities. Given the emphasis on research, professors trained in the German universities found the activities of the day-to-day operation of the university distasteful. Increasing the ranks of the administration was thus necessary to insure that the mundane tasks of running the university were performed.[17]

Also, the pursuit of advanced research focused attention on the disorganized state of scientific knowledge. As scientific research became more technical during the nineteenth century, the traditional collegiate model of a unified approach to knowledge crumbled under the weight of the professors' desire to establish their reputations through their work. This disorganization was then institutionalized by bringing together under the heading of a "department" those researchers sharing common methodologies and topics of investigation. The establishment of departments thus served two purposes. First, it was a logical extension of the advanced training for the Ph.D. to recognize administratively a specific area of research, and second, once thus established, it served as a base from which researchers could further their professional careers.[18]

However, the move towards departmentalization of academia did not always proceed smoothly. The formation of new disciplinary boundaries meant, in some cases, the dissolution of existing scholarly domains and the split-up of older departments. Also, newly formed disciplines were sometimes unable to carve out a unique intellectual niche, and they were absorbed by more vital departments. An example of the latter was the demise of the Social Ethics Department at Harvard in 1931. The department had been inaugurated in 1906 under the leadership of the Unitarian minister Francis G. Peabody and funded by the New York philanthropist Alfred T. White for the purpose of studying the ethical problems of modern society and proposing solutions for them.[19] But, the department was never able to secure its status at Harvard. Questions were continually raised as to whether Social Ethics was in actuality a sub-area of theology, economics, or psychology. Even the appointment of the charismatic Richard C. Cabot as Peabody's successor in 1920 only served to delay the inevitable reorganization of the department. In 1928, Social Ethics was partially merged with the newly-formed Sociology Department. In 1931, Social Ethics was totally subsumed into the Sociology Department, as it was felt by the Harvard administration that sociology would be the stronger and more lasting of the two disciplines.[20]

The sense of professional identification fostered by academic and administrative specialization manifested itself at the inter- as well as intra-collegiate level. Various agencies and publications were founded in the late nineteenth century as a means of expressing group identifications. By 1900, journals and scientific societies catered to the specific interests of philosophers, mathematicians, chemists, biologists, physiologists, sociologists, and psychologists. Issues affecting all of academia were addressed by administrators in the forum provided by the Association of American Universities, while the faculty responded via the American Association of University Professors.[21] Thus, academicians were able to express their new status and roles in the university in three ways--the organization of academic disciplines into specialized departments, the designation of a variety of new administrative functions, and the foundation of topically defined national and international organizations.

By 1910, the American system of higher education had basically achieved its present form, as the period of greatest expansion had passed.[22] Effort was now directed towards consolidating the changes that had occurred. One area that held particular promise was the effort to provide a consistent set of standards for the elements of a college education, and thereby to determine what constituted a college. While first-rate institutions such as Harvard, Chicago, and the University of Wisconsin flourished, many

smaller, academically marginal colleges also managed to eke out a meager existence. Some of these marginal institutions even had pretensions to university status, offering a Ph.D. degree though having neither faculty nor facilities to support advanced training. Activities such as this were an obvious affront to the status and position of the established universities, and men in the position of Harvard's President Eliot and Chicago's President Harper called for and predicted the early demise of the small colleges.[23]

Not all changes, however, were the result of forces within the academic community. In particular, philanthropists have played a major role in shaping the development of American higher education. Philanthropy was instrumental in the founding of almost all American colleges and universities. The vast majority of the early colleges were church affiliated, and drew part of their support from the donations of local congregations. Other contributions came from wealthy landowners and merchants, as well as from the municipality in which the college was located. Donations, though, often came with strings attached. It was frequently stipulated that a college be located in a specific locale, that endowed professorial chairs in specific academic areas be created, and that many other special interests be catered to. Given the tenuous financial situation that most colleges frequently found

themselves in, donations were only rarely refused regardless of the conditions that were imposed.[24]

Towards the end of the nineteenth century, a major change took place that was to drastically effect American philanthropy. A few industrialists, such as John D. Rockefeller, Sr. and Andrew Carnegie, had amassed vast fortunes and began to turn their attention towards distributing their wealth for the betterment of society. The amount of their giving was so great that it served to effectively centralize American philanthropy and its impact on higher education. Their philanthropy was not motivated by purely altruistic goals, though. Their wealth had been made possible by the rapid mechanization of industry, and through their gifts they sought to seek answers to the one major unanswered industrial problem--the human problem.[25]

In 1905, both the Carnegie Foundation and John D. Rockefeller's General Education Board turned their attentions towards the proliferation of diploma mills and the generally low academic standards found in the smaller colleges. In particular, the Carnegie Foundation's efforts to establish a national pension fund for college and university faculty was to impose the first effective and enforceable definition of a college upon American higher education.[26]

To be considered eligible for pension funds, a college or university had to require (within reasonable limits) a four-year high school education involving a specified number of course credits as an entrance requirement for its students. It was also stipulated that all colleges offer a four-year curriculum with course offerings in a minimum of six different departments, and that the teaching staff consist of at least six full professors. Colleges catering to narrow-interest groups, such as denominational colleges offering only ministerial training, were thereby discouraged from applying for pension funds.[27]

Finally, the Carnegie Foundation set educational standards for the faculty. It was required that all department chairmen possess the Ph.D. in their field of specialization. This was a far more stringent standard than had previously been employed by the various accrediting agencies created during the 1890's to monitor the activities of American colleges and universities.[28] The effect of this requirement was to establish the Ph.D. as almost a prerequisite for a college teaching position. This, along with the increasing emphasis on specialization brought on by the departmentalization of college faculties, furthered the cause of professionalization within academia.

The Rockefeller philanthropies addressed a wider variety of issues than did the Carnegie Foundation. Rockefeller endowments for liberal-arts undergraduate

institutions were primarily limited to Southern, all-black colleges. However, the example set by these gifts had the effect of stimulating donations from other sources for other Southern schools. This proved to have an overall positive impact on Southern education in general.[29] For the most part, the Rockefeller philanthropies focused on funding teaching and research on topics that were thought to have a particular importance for solving the problems of American industrial society. Particular attention was given to improving the standards of training in American medical schools, and to improving the quality of medical care available to the general public. Also, research in the social sciences was supported because of the promise these disciplines held for providing solutions to numerous social ills.[30]

Psychology Takes Its Place in the Modern University

It was in this context of professional specialization and practical application of knowledge that psychology evolved as a separate discipline within the American university. It is generally agreed that academic psychology had its origins in Germany during the 1850's as a result of the hybridization of philosophy and physiology. By combining the traditional subject matter of philosophy with the experimental methods of the natural sciences, it was thought that precise answers could be found for many questions concerning human mental activity. This new

discipline was christened by its founders as either empirical philosophy or physiological psychology.[31]

A few leading proponents of the "new psychology," such as Wilhelm Wundt, were able to secure professorial chairs within the German university system. Based on the status conferred by their academic position they were able to do much to promote their new discipline, although the major impact was to be felt abroad. Given the limited range of graduate training that existed in the United States prior to 1890, Germany provided almost the only source of advanced training in the discipline. The status accompanying a professorial chair was not lost upon the career-oriented American academics, and many went to Germany to receive the Ph.D. level training that would secure them positions in the many new American institutions of higher education. By 1900, except for a few psychologists who had taken their Ph.D.'s under William James at Harvard, all American academic psychologists had either studied in Germany, or studied under German-trained psychologists.[32]

While the Americans flocked to Germany for graduate training, they generally ignored or failed to see the many shortcomings of the German professorial system. In particular, they overlooked the inapplicability of the German intellectual caste system for the more egalitarian American colleges and universities. German academia was rigidly controlled by the recipients of the few

state-endowed professorial chairs, thereby creating a situation where theoretical and methodological doctrine was inflexibly controlled, practical applications of knowledge was discouraged, and opportunities for career advancement was severely limited. Because of the extreme inertia inherent in this system, psychology was not able to establish a secure, independent institutional basis in Germany until the 1940's.[33]

The Americans, on the other hand, were aggressively upwardly mobile in their career orientation. They were motivated by the wide range of professional opportunities that waited for them at home, and by the encouragement for research on applied topics to be found in the American universities. Thus, their work in psychology tended to focus on the application of the methodology of pure research to issues of practical concern, such as the measurement of individual differences and educational technology.[34]

The new psychology was quick to establish itself in American academia. G. Stanley Hall founded the first officially recognized psychological laboratory in this country at Johns Hopkins in 1883, and he also founded The American Journal of Psychology, the first scholarly psychological journal in this country, in 1887. By 1893, eighteen university psychological laboratories were in operation, at least a dozen more Americans had taken their Ph.D.'s in Germany, another journal had been inaugurated,

and most significantly, the American Psychological Association [APA] had been established.[35]

The formation of the APA was to prove the major force for the professionalization of American psychology. Basing the organization on established scientific societies, the founders of the APA:

sought to develop, and to utilize in the interest of the advancement of the new psychology, a single, national, multipurpose professional body. The [APA] was to symbolize the distinctiveness of their special field, give structure and corporate weight to their common goals, and promote, in a multiplicity of ways, both the scientific and social progress of their discipline.[36]

At this task the APA excelled. Official ties were quickly established between the APA and the American Society of Naturalists and the American Association for the Advancement of Science. Also, several journals, including the Psychological Review, the Psychological Index, and the Psychological Monographs were launched to serve as a means for the dissemination of psychological research and APA policy statements.[37]

At the same time that these external indices of a professional organization were established for the APA, the internal criteria for membership in the APA were being made increasingly stringent. At first, membership in the APA was open to virtually anyone who wanted to join. In 1906, however, membership criteria were tightened to require full-time participation in psychological research, and in

1916, it was required that applicants for membership supply information concerning their academic degrees.[38] Also by this time, the APA leadership was taking an active role in promoting psychology's disciplinary status in American colleges and universities, especially by decrying the subsidiary role that psychology played in most college curricula.[39]

In spite of their generally low institutional status, the research productivity of American psychologists soon overshadowed that of their German colleagues. From its beginning with the American Journal of Psychology, publication of psychological research in this country increased over 800 percent between 1886 and 1896. This equaled the research productivity of the more established German psychologists.[40] The rise in research activity reflected the influx of students into American universities who wanted to take advanced degrees in psychology under the German-trained Ph.D.'s. When G. S. Hall took the first American Ph.D. in psychology under William James in 1878, there were no official laboratories, no separate departments, no journals, and no professional organization to cater to psychologists in this country. The rush of students into psychology can easily be seen in the fact that by 1917 the APA was well established, twenty-one American psychological journals were being published, and thirty-five university psychology departments had produced close to

three hundred new Ph.D.'s. Academic psychology was here to stay.[41]

While psychology was solidly entrenching itself within the academic community, a great debate was ensuing over its disciplinary boundaries. Specifically, a few vocal proponents of the German model of pure research led a campaign to purge American psychology of its applied orientation. The classic example of this was Edward B. Titchener at Cornell University from 1892 to 1927. An Englishman by birth and a Germanophile by education and temperament, Titchener was the staunchest proponent in America of the view that psychology was the study of the elements of conscious experience. In his opinion, psychology was a pure science. It was to be free of all elements of philosophical speculation, focusing instead on the barest aspects of sensory phenomena. Psychology was not, in his opinion, to provide an explanation for the apparently meaningful interpretations that are made based upon sensory experience. Also, psychology as defined by Titchener had no room for the study of the applications of the knowledge gained through research. It was to be a pure science, and the applications of knowledge were to be left for the non-scientist practitioner or technician.[42]

While Titchener produced a number of devoted followers (he was personally charming and gregarious as well as possessing an exceptional intellect), his dogmatic

pronouncements on the discipline were agreed with by only a minority in psychology. The majority of American psychologists favored a more practically-oriented approach to research and theory. One of the leading examples of applied psychology was the functionalist school led from 1894 to 1920 by James R. Angell and John Dewey at the University of Chicago. Influenced largely by the Darwinian notion of adaptation and James's pragmatic philosophy, Angell and Dewey focused their psychological research on the ways in which consciousness aided the individual in functioning in a complex environment. Consciousness was not seen as made up of static elements to be analyzed, but as an ongoing activity. By emphasizing the adaptive relationship between the acting individual and his/her environment, the functionalists were quite open to studying the practical implications of psychology, and thus devoted much effort to the testing of individual differences and on research to educational issues.[43]

While this emphasis on the practical implications of psychology reflected the concerns of most American psychologists, applied psychology never became the central feature of any school or system. The closest thing to an applied school that existed was at Columbia University, and was directed by James McKeen Cattell from 1891 to 1917, and by Robert S. Woodworth from 1917 to 1942. Derived in large part from Cattell's deep personal interest in the study of individual differences in human capabilities, psychology at

Columbia was focused on the attempt to find the causes for, and significance of, human behavior. Instead of a dogmatic adherence to any specific theory or method, Cattell and Woodworth employed an eclectic approach to psychology combining both pure and applied research. As a result, psychology at Columbia came to be known as "general experimental psychology." [44]

Most research on applied topics, however, did not fall under any broad institutional or theoretical framework. For example, a popular child study movement was started by G. S. Hall in 1891, when he founded the journal Pedagogical Seminary and pioneered the use of questionnaires as a means of collecting vast volumes of data. While most of this research on children was performed by, and was of immediate interest to educators, Hall and his colleagues at Clark University also extended their work to address the problems of adolescent emotional adjustment. [45]

Initially related to the child study movement, research on the testing of human abilities was also a topic of great popular and academic interest. Alfred Binet's work to develop an accurate test of a child's intellectual ability was publicized widely in America between 1910 and 1916 by H. H. Goddard, L. M. Terman, and Robert M. Yerkes. [46] While the American adaptations of Binet's test figured prominently in research on childhood education, the outbreak of World War I served to extend intelligence testing to

adults. In order to separate the mentally incompetent from the multitude of draftees who were being processed into the Army, tests had to be developed that were both accurate and suitable for administration to large groups of men at a time. To this end, a group of psychologists under the direction of Yerkes were brought together to develop the Army Alpha and Beta Examinations. These tests were supplemented by a variety of vocational skills tests to aid in assigning mentally competent draftees to their proper jobs. After the war, many of these new measurement techniques were adapted for civilian use in both academic and industrial settings.[47]

Research techniques developed in the study of intelligence and vocational ability were also applied to the more general study of human personality. Largely in response to their repeated failure to discover any direct correspondence between emotional and physiological states, personality researchers turned increasingly to the use of questionnaire research methods after 1918.[48] The enormous quantities of data generated by these paper-and-pencil tests of intelligence, aptitude, and personality necessitated the development of new statistical techniques. In response, psychologists devised new applications for Robert A. Fisher's methods for determining the significance of the differences between sets of test scores. This was a development that was to be of great importance to the

increasing statistical sophistication of experimental psychology.[49]

However disorganized American psychology may have been, there were many psychologists who wished for a more conceptually and theoretically unified discipline. It was especially desired by many that even if psychology was to be practically oriented, that it should become more rigidly empirical, thereby making it more "scientific." A major impetus for this movement came during the 1880's from the writings of the British comparative psychologist C. Lloyd Morgan. His application of the law of parsimony and Darwinian evolutionary theory to the study of animal behavior led him to always propose the evolutionarily simplest explanation for behavior. For example, Morgan argued that there was no need to claim conscious motivation for animal behavior if it was possible to explain the behavior as the result of a simple biological reflex mechanism. His work, and similar lines of research being pursued by the German physiologist Jacques Loeb and the Russian physiologist Ivan P. Pavlov, caught the attention of American psychologists.[50]

Psychological research on animals began in this country with the publication in 1898 of Edward L. Thorndike's "Animal Intelligence: An Experimental Study of the Associative Process in Animals." [51] In this paper, Thorndike described how he had placed cats, dogs, and

chickens in a box or maze from which they could learn to escape through a process of trial-and-error. Various quantitative measures of the animals performance were used to monitor the rate at which it learned. What is usually considered to be Thorndike's most important finding was that success in performing the task (defined as escaping from the maze or box) served to greatly facilitate learning. Once the task had been successfully performed, the animal performed in a less trial-and-error fashion. This phenomena Thorndike called the "law of effect." [52] While he later abandoned this area of research to pursue a career in educational psychology, the cause of animal research was furthered by Margaret Washburn and Robert M. Yerkes. [53]

The essential feature of animal research was the reliance upon observable, quantifiable sources of data. Given the prevailing emphasis in American scientific circles on materialistic and positivistic theories, and the desire of many American psychologists to make their discipline more of a "hard" science, the time was ripe for an extension of the methods of animal psychology to the study of humans:

That American psychology was ready for such a program had been made abundantly plain by the hospitality it had shown functionalism. For the same conditions that had ensured for functionalism an interested hearing paved the way for behaviorism's more spectacular victory. . . . [Behaviorism] called upon its followers to fight an enemy who must be utterly destroyed, not merely to parley with one who might be induced to modify his ways. [54]

This program, this call to action known as behaviorism, was largely initiated by the publication in 1913 of John B. Watson's "Psychology as the Behaviorist Views It." [55] What Watson proposed was simple and straightforward. The subject-matter of psychology was to be observable behavior, not inferred conscious states or mental operations. Any research method could be used as long as it was objective. To this list was added in 1915 the Pavlovian concept of the conditioned reflex as the primary mechanism of behavior. [56]

Despite Watson's early retirement from academia in 1920 as a result of a personal scandal, the movement he founded spread rapidly through academic and popular psychology. [57] Many new Ph.D.'s were won over to the behaviorist position through its emphasis on scientific rigor and its rigidly environmentalist view of human nature, the latter of which appealed to American democratic social ideals. However, it became increasingly felt within psychology that Watson's radical view of behaviorism was naive and over-simplistic. Except for Karl S. Lashley (who took his Ph.D. under Watson), the second generation of behaviorists tended to expand their research into areas ignored by Watson. For example, Edward C. Tolman proposed in 1922 a psychological theory that he called purposive behaviorism. According to Tolman, while behavior could be purposive--that is, directed towards specifiable end states--it did not have to be conscious. Since the behavior and its goal could be observed, purpose could be "observed" in the behavior and

thus studied objectively.[58] A somewhat different approach was taken, beginning in 1925, by Clark L. Hull. He sought to combine the principle of conditioning with a deductive system derived from formal logic to develop a theory that would mechanistically account for all organismic phenomena.[59]

While a diversity of opinion existed as to what constituted academic psychology, there was general agreement that the discipline's intellectual boundaries be drawn to exclude certain topics. Even though psychology owed a great debt to philosophy, the consensus opinion within the discipline was that purely speculative approaches to the study of psychology were totally unacceptable. Psychology was conceived of as a science, and thus could only proceed on the basis of experimentally derived empirical data.[60] Even so, several prominent psychologists managed to maintain close, although not generally well publicized, ties with the philosophic community.[61]

Another area of research that was excluded from academic psychology was that of psychical research. Research on the topics of thought transference, precognition, and communication with the dead had been a major interest of many early American and British psychologists. However, interest in these issues had waned as no verifiable instances of psychic phenomena had been identified, and only numerous cases of fraud had been

uncovered. Finally, the rise of behaviorism with its rejection of mentalism in psychological theory and research served to remove the last ounce of respectability from psychical research.[62]

Other topics of psychological research were only grudgingly accepted within the discipline. In these cases the problems dealt with areas of research that were also claimed by other disciplines as well as by psychology. An example of this was the clinical study and treatment of the psychologically disturbed. Although many American psychologists were interested in these issues, most of the strictly clinical work was done by researchers trained in medicine. An example was the informal Boston School of abnormal psychology and psychotherapy. While the membership of the Boston School included such prominent psychologists as William James and Hugo Münsterberg, most of the research on and the practice of clinical psychology was conducted by men active in medicine. In particular, the leading members of the group were the psychiatrists Morton Prince and Boris Sidis, and the physiologist James J. Putnam.[63]

Some psychologists began to take tentative steps towards bringing clinical issues within academia. In 1896 Lightner Witmer founded a psychological clinic at the University of Pennsylvania, with the purpose of offering training in diagnostic and therapeutic techniques.[64] Several other universities followed suit; Iowa in 1908,

Clark in 1909. By 1914, clinics also existed at the universities of Cornell, Tulane, and Rutgers.[65] The training offered at these clinics, however, emphasized areas that have previously been discussed under the topic of applied psychology--mental tests and educational psychology. Little attention was given to the diagnosis and treatment of the seriously disturbed. Those psychologists wishing to study the latter had to turn to hospitals and medical schools for post-graduate internships in psychiatry.[66]

Even though the number of university clinics indicated that a demand existed for training in clinical psychology, this was not initially reflected in the membership rolls of the APA. In 1917, only sixteen of the APA's 307 members listed clinical psychology as their primary interest area.[67] At the same time, while the leadership of the APA was often critical of clinical research, the organization was inexorably involving itself in the regulation of clinical training and practise. This, of course, met with the opposition of the American psychiatric community. However, with regulatory precedents being set by a number of state legislatures, the APA moved in 1917 to establish professional criteria for clinical psychology.[68] This important step towards professionalization was followed by the establishment in 1919 of the Clinical Section as the first separate division of the APA. Clinical psychology was further legitimized in 1921, when the Journal of Abnormal Psychology expanded to include the publication of research

in experimental social psychology. This formed an important professional alliance between areas of applied and pure psychological research.[69]

Another instance of this process can be found in the gradual acceptance of psychoanalytic theory by academic psychologists. With the publication in 1895 of Studies in Hysteria, the work of Sigmund Freud began to be known by the American psychological and psychiatric community. Not surprisingly, the earliest references to and applications of psychoanalysis in this country came from the Boston School of abnormal psychology, in particular from Boris Sidis, J. J. Putnam, and William James.[70] These early adoptions of Freud's work, though, were often notable for the liberties that were taken with Freud's interpretations and uses of psychoanalysis. Putnam, Sidis, and others often reinterpreted psychoanalysis to more closely coincide with their own theories on subliminal states of consciousness and the therapeutic uses of hypnosis than a careful reading of Freud would have permitted. Also, there was an understandable reticence on the part of American psychologists and psychiatrists to wholeheartedly endorse Freud's views on infantile sexuality. This tended to keep psychoanalysis in the background of accepted theories of psychopathology.[71]

However, between 1905 and 1909, psychoanalysis gained greatly in popularity. One reason for this was Carl Gustav Jung's research on the word association test as a diagnostic tool. This technique appealed to the growing American interest in psychological testing and empirical research methods, as well as lending crucial support to the psychoanalytic theory of neurotic functioning.[72] Also, the invitation from G. S. Hall to Freud, Jung, Sandor Ferenczi, and Ernest Jones to participate in the festivities commemorating the twentieth anniversary of the founding of Clark University helped bring psychoanalysis to the attention of a wide psychological and popular audience.[73]

While psychoanalysis generated much interest among American psychologists at the time of the Clark conference, few, if any, incorporated it directly into their research activities. At best, as in the case of J. B. Watson and Clark Hull, attempts were made to show how behavioral theory could account for intrapsychic phenomena. Most psychologists were either neutral or outright antagonistic towards psychoanalysis. Even several early advocates, such as Morton Prince, had by 1920 become dissatisfied with psychoanalysis due in part to the increasing doctrinal orthodoxy of the American analysts, and withdrew support from efforts to gain converts to Freud's theory.[74] Only William McDougall, in his hormic theory of social instincts, showed any affinity for psychoanalysis, and his work was not

well received within academic circles due to controversies surrounding the validity of his research methods.[75]

Another factor influencing the acceptance of psychoanalysis by American psychologists was that of language. Due to problems with A. A. Brill's English translations of Freud's writings, many of the nuances of the psychological theory underlying psychoanalysis was lost.[76] Instead, only the therapeutic aspects of psychoanalysis came through. Given the tendency of American academic psychologists to consider psychotherapy to be outside the boundaries of their research interests, psychoanalysis was to have its greatest impact upon psychiatrists. Slowly, through cautious experimentation with Freudian therapeutic techniques, and through the continued advocacy of Jones and Brill in the psychiatric journals, a number of psychiatrists became converted to psychoanalysis. By 1920, psychoanalysis had become accepted as a therapeutic technique in many mental hospitals and psychiatric clinics. It had become part of the curriculum at many medical schools, and was at least mentioned in every textbook on psychopathology.[77] Therefore, while psychoanalysis had become a major psychiatric theory in just over a decade, perhaps the only contact an average academic psychologist had with Freud was in the general survey course in abnormal psychology.

Psychology at Harvard and Its Institutional Status

From its humble origins in the 1890's, academic psychology had by the 1920's grown into a vital, though somewhat disorganized and internally quarrelsome discipline. Many colleges and universities had independent psychology departments. Psychology Ph.D.'s were being turned out in increasing numbers, and research reports were flowing from the laboratories. One major institution, however, stood out as an anomaly in the rapid institutional rise of psychology. At Harvard University, psychology was in a state of decline. During the 1880's and 1890's, William James had made psychology a major focus of the Harvard Philosophy Department, and had come close to creating an indigenous alternative to the German experimental psychology. James's interests soon began to shift towards philosophical speculation, however, and by the turn of the century he had for all intents and purposes abandoned psychology altogether.[78]

In James's place, psychology at Harvard was handled by Josiah Royce and Hugo Münsterberg. Like James, Royce was primarily a philosopher, and Münsterberg, although he was the director of the psychological laboratory, also saw his work as ultimately leading to a major philosophical statement. Thus, no great internal pressures existed at Harvard to make psychology a department separate from philosophy.[79]

The decade from 1910 to 1920 was to cause sweeping changes in the face of psychology at Harvard. In 1910 James died, followed by both Royce and Münsterberg in 1916. This, and other faculty turnovers, left both psychology and philosophy at Harvard in a shambles.[80] The task of rebuilding the department to its former prominence fell to Harvard's new president, A. Lawrence Lowell. Elected to office in 1909, Lowell was to achieve fame as one of the greatest institutional builders in American academic history. He was not destined, however, to rise to greatness based on his actions to revitalize psychology. Early in the Philosophy Department's crisis of 1910-1916, Lowell set as his first priority the rebuilding of the philosophical faculty, leaving psychology to take second place.[81]

One effect of this decision was that psychology was to remain institutionally subordinate to philosophy. This made it virtually impossible for Lowell to hire a prominent psychologist to replace Münsterberg. The leading internal candidate, Robert M. Yerkes, an Assistant Professor at Harvard, resigned in 1917 because of Lowell's refusal to grant psychology independent departmental status. E. B. Titchener was briefly considered for the job, but the offer was withdrawn when he too demanded a separate department. Finally, J. M. Cattell, who had been fired from Columbia in 1917 because of his political views, was denied employment at Harvard for the same reason.[82]

In the absence of any clear program for psychology's development at Harvard, the laboratory continued to operate until 1920 under the direction of Assistant Professor Herbert S. Langfeld, and Instructor Leonard Ireland. In 1920, despite the rise of behaviorism and the empiricist orientation of psychology in general, the philosophy faculty persuaded Lowell to hire a theoretically-oriented psychologist to fill the vacancy left by Munsterberg. The British psychologist William McDougall was chosen for the position. Although he had built an international reputation on the basis of his 1908 An Introduction to Social Psychology and 1911 Body and Mind, his American colleagues were less than enthusiastic about his more recent research. The effect of Lowell's mistake in filling this position was that the continued existence of psychology at Harvard was put in serious jeopardy; a point that was underscored by the decreasing enrollment of graduate students and the diminishing prestige of the department among American psychologists.[83]

In 1922, Edwin G. Boring was called to the Harvard Psychological Laboratory as an Associate Professor. A disciple of Titchener's, Boring seized upon his appointment as an opportunity to carry out his mentor's unfulfilled mission to make psychology at Harvard a separate department. Furthermore, he was a ruthless political in-fighter when issues involving psychology's status were at stake. He managed to supplant McDougall as director of the laboratory

in 1924, and then forced Langfeld to seek employment elsewhere. Boring envisioned making psychology at Harvard a showcase for Titchener's view of the discipline as a pure science devoid of philosophical and practical overtones.[84] As much success as he had in getting his way within the Department, the fact still remained that he was not yet totally in control of psychology's destiny at Harvard. This point was made excruciatingly clear when Lowell established the Harvard Psychological Clinic under the direction of Morton Prince in January of 1926, entirely without Boring's knowledge or consent.[85] It was in this institutionally unstable situation that Murray found himself when he came to Harvard and academic psychology in 1926.

Reference Notes

1. Frederick Rudolph, The American College and University: A History (New York: Alfred A. Knopf, 1962), pp. 241-263; and Laurence R. Veysey, The Emergence of the American University (Chicago: University of Chicago Press, 1965), pp. 121-179. For an overview of the professionalization literature, see Susan F. Cannon, Science in Culture: The Early Victorian Period (New York: Dawson, 1978), pp. 137-165; Henrika Kuklick, "Boundary Maintenance in American Sociology: Limitations to Academic 'Professionalization,'" Journal of the History of the Behavioral Sciences 16 (1980): 201-219; and Purton J. Bledstein, The Culture of Professionalism: The Middle Class and the Development of Higher Education in America (New York: Norton, 1976), pp. 287-332.
2. Rudolph, American College, pp. 282-286.
3. Rudolph, American College, pp. 395-397; and Veysey, American University, pp. 294-302.
4. Rudolph, American College, pp. 300-405; and Veysey, American University, pp. 302-317.
5. Rudolph, American College, pp. 247-252.
6. *Ibid.*, pp. 254-263.
7. R. S. Peters, (Ed.), Brett's History of Psychology (Cambridge, Mass.: M.I.T. Press, 1965), pp. 143-225; Dorothy Ross, "The Development of the Social Sciences," in The Organization of Knowledge in Modern America, 1860-1920, eds. A. Oleson and J. Voss (Baltimore, Md.: Johns Hopkins University Press, 1979), pp. 109-112; and Sheldon M. Stern, "William James and the New Psychology," in Social Sciences at Harvard, 1860-1920: From Inculcation to the Open Mind, ed. P. Buck (Cambridge, Mass.: Harvard University Press, 1965), pp. 175-179.
8. Stern, "New Psychology," pp. 175-176; Ross, "Social Sciences," pp. 126-128; and Edwin G. Boring, A History of Experimental Psychology, 2nd ed. (Englewood Cliffs, N.J.: Prentice-Hall, 1950), pp. 240-244.
9. H. Kuklick, "Boundary Maintenance," pp. 203-204.
10. Boring, Experimental Psychology, pp. 410-420; and Edna Heidbreder, Seven Psychologies (Englewood Cliffs, N.J.: Prentice-Hall, 1933), pp. 113-151.

11. Rudolph, American College, p. 243.
12. Ibid., p. 245.
13. Seymour M. Lipset and David Riesman, Education and Politics at Harvard (New York: McGraw-Hill, 1975), pp. 91-99; Rudolph, American College, pp. 293-295; and Veysey, American University, pp. 256, 288, 391.
14. Rudolph, American College, pp. 244-245, 417-424; and Veysey, American University, pp. 302-311.
15. Francesco Cordasco, The Shaping of American Graduate Education: Daniel Coit Gilman and the Protean Ph.D. (Totowa, N.J.: Rowman and Littlefield, 1973), pp. 54-115; Rudolph, American College, pp. 269-275; and Veysey, American University, pp. 159-161.
16. Rudolph, American College, pp. 255-258.
17. Veysey, American University, pp. 308-311.
18. Rudolph, American College, pp. 389-401; and Veysey, American University, pp. 58-59, 162-183.
19. David B. Potts, "Social Ethics at Harvard, 1881-1931: A Study in Academic Activism," in Social Sciences at Harvard, 1860-1920: From Incultation to the Open Mine, ed. P. Buck (Cambridge, Mass.: Harvard University Press, 1965), pp. 93-118.
20. Ibid., pp. 120-128.
21. Rudolph, American College, pp. 405-407, 415-416, 438; and Veysey, American University, pp. 175-176, 356, 388, 416.
22. Veysey, American University, pp. 338-339.
23. Rudolph, American College, pp. 443-444; and Veysey, American University, p. 144.
24. Merle Curti and Roderick Nash, Philanthropy in the Shaping of American Higher Education (New Brunswick, N.J.: Rutgers University Press, 1965), pp. 42-59; Earnest V. Hollis, Philanthropic Foundations and Higher Education (New York: Columbia University Press, 1938), pp. 127-130; Rudolph, American College, pp. 430-433; and Veysey, American University, p. 313.
25. Curti and Nash, Philanthropy, pp. 212-237; and E. Richard Brown, Rockefeller Medicine Men: Medicine and Capitalism in America (Berkeley, Cal.: University of California Press, 1979), pp. 13-59.

26. Hollis, Philanthropic Foundations, pp. 127-130; Rudolph, American College, pp. 430-433; and Veysey, American University, p. 313.

27. Hollis, Philanthropic Foundations, pp. 136-139; and Rudolph, American College, pp. 431-432.

28. Hollis, Philanthropic Foundations, p. 137.

29. Raymond B. Fosdick, The Story of the Rockefeller Foundation (New York: Harper & Brothers, 1952), pp. 8-9, 99-100; and Curti and Nash, Philanthropy, pp. 171-185.

30. Fosdick, Rockefeller Foundation, pp. 30-70, 93-134, 192-236.

31. Boring, Experimental Psychology, pp. 27-31; Heidbreder, Seven Psychologies, pp. 71-76; and Joseph Ben-David and Randall Collins, "Social Factors in the Origins of a New Science: The Case of Psychology," American Sociological Review 31 (1966): 451-465.

32. Ben-David and Collins, "Origins of a New Science," p. 458.

33. Fritz K. Ringer, The Decline of the German Mandarins: The German Academic Community, 1890-1933 (Cambridge, Mass.: Harvard University Press, 1969), pp. 1-13, 376-384; and Ulfried Geuter, "National Socialism and the Professionalization of Psychology in Germany, 1933-1945," colloquium presented to the Department of Psychology, University of New Hampshire, 10 June 1983.

34. See especially Heidbreder, Seven Psychologies, pp. 201-233, 287-327. Other useful, but less accessible sources dealing with the establishment of psychology in America are Frank M. Albrecht, "The New Psychology in America: 1880-1895" (Ph.D. dissertation, Johns Hopkins University, 1960). See in particular pp. 38-58, 128-154, 186-220; and John M. O'Donnell, "The Origins of Behaviorism: American Psychology, 1870-1920" (Ph.D. dissertation, University of Pennsylvania, 1979). See especially pp. 1-67.

35. Thomas M. Camfield, "The Professionalization of American Psychology, 1870-1917," Journal of the History of the Behavioral Sciences 9 (1973): 66-67.

36. Ibid., p. 68.

37. Ibid., pp. 68-69.

38. Ibid., p. 71.

39. Ibid., pp. 71-72.
40. Ben-David and Collins, "Origins of a New Science," p. 453.
41. Camfield, "American Psychology," pp. 74-75.
42. Boring, Experimental Psychology, pp. 410-420; Heidebreder, Seven Psychologies, pp. 113-151; and Robert I. Watson, The Great Psychologists, 3rd ed. (Philadelphia: Lippincott, 1971), pp. 397-407.
43. Boring, Experimental Psychology, pp. 552-559; Heidebreder, Seven Psychologies, pp. 201-233; Watson, Great Psychologists, pp. 407-412; and John M. O'Donnell, "The Crisis of Experimentalism in the 1920's: E. G. Boring and His Uses of History," American Psychologist 34 (1979): 289-295.
44. Boring, Experimental Psychology, pp. 559-556; and Heidebreder, Seven Psychologies, pp. 287-327.
45. Boring, Experimental Psychology, pp. 517-524; and Watson, Great Psychologists, pp. 381-391. For an excellent, full-length biography of Hall, see Dorothy Ross, G. Stanley Hall: The Psychologist as Prophet (Chicago: University of Chicago, 1975).
46. Boring, Experimental Psychology, pp. 570-578; and Gardner Murphy, An Historical Introduction to Modern Psychology (New York: Harcourt, Brace and Co., 1929), pp. 348-349. See also Thomas M. Camfield, "Psychologists at War: The History of American Psychology and the First World War" (Ph.D. dissertation, University of Texas at Austin, 1969).
47. Boring, Experimental Psychology, pp. 575-576; and Murphy, Modern Psychology, pp. 351-355.
48. Murphy, Modern Psychology, pp. 373-381.
49. Ibid., pp. 381-386. For the classic work on statistical analyses of research data see Robert A. Fisher, Statistical Methods for Research Workers (London: Oliver & Boyd, 1925).
50. Boring, Experimental Psychology, pp. 622-631, 635-639; Heidebreder, Seven Psychologies, pp. 326-327; and Brian D. Mackenzie, Behaviourism and the Limits of Scientific Method (Atlantic Highlands, N.J.: Humanities Press, 1977), pp. 55-100.
51. Edward L. Thorndike, "Animal Intelligence: An Experimental Study of the Associative Processes in Animals," Psychological Monographs Whole no. 4 (1898).

52. Ibid. See also Boring, Experimental Psychology, pp. 562-563; Heidebreder, Seven Psychologies, pp. 295-296; and Watson, Great Psychologists, pp. 425-426.

53. Heidebreder, Seven Psychologies, p. 237.

54. Ibid., p. 239.

55. John B. Watson, "Psychology as the Behaviorist Views It," Psychological Review 20 (1913): 158-177.

56. Boring, Experimental Psychology, pp. 643-645; Heidebreder, Seven Psychologies, pp. 243-247; and Watson, Great Psychologists, pp. 429-430.

57. Boring, Experimental Psychology, p. 645; and Heidebreder, Seven Psychologies, pp. 259-260.

58. Boring, Experimental Psychology, pp. 645-648.

59. Ibid., pp. 651-653. See also Sigmund Koch, "Clark Leonard Hull," in Modern Learning Theory: A Critical Analysis of Five Samples, ed. A. J. Poffenberger (New York: Appleton-Century-Crofts, 1954), pp. 1-176; John A. Mills, "Hull's Theory of Learning as a Philosophical System: I. An Outline of the Theory; II. A Criticism of the Theory and its Relationship to the History of Psychological Thought," Canadian Psychological Review 19 (1978): 27-40, 116-127; and Rodney G. Triplet, "The Relationship of Clark L. Hull's Hypnosis Research to His Later Learning Theory: The Continuity of His Life's Work," Journal of the History of the Behavioral Sciences 18 (1982): 22-31.

60. Boring, Experimental Psychology, pp. 304-308; Heidebreder, Seven Psychologies, pp. 72-74, 119-126; and Ross, "Social Sciences," pp. 113-121.

61. For an excellent overview of this point, see Laurence D. Smith, "Behaviorism and Logical Positivism: A Revised Account of the Alliance" (Ph.D. dissertation, University of New Hampshire, 1983).

62. R. Laurence Moore, In Search of White Crows: Spiritualism, Parapsychology, and American Culture (New York: Oxford University Press, 1977), pp. 133-203.

63. Nathan G. Hale, Jr., "Introduction," in Psychotherapy and Multiple Personality: Selected Essays, by Morton Prince, ed. N. G. Hale, Jr. (Cambridge, Mass.: Harvard University Press, 1975), p. 4; and Nathan G. Hale, Jr. Freud and the Americans: The Beginnings of Psychoanalysis in the United States, 1876-1917 (New York: Oxford University Press, 1971), pp. 116-124.

64. John R. Reisman, A History of Clinical Psychology (New York: Irvington, 1976), pp. 41-43, 86-88.
65. Ibid., pp. 88-89, 129.
66. Reisman, Clinical Psychology, pp. 128-135; John C. Burnham, Psychoanalysis and American Medicine, 1894-1918: Medicine, Science, and Culture (New York: International Universities Press, 1967), pp. 60-81; and Hale, Freud and the Americans, pp. 116-173.
67. Reisman, Clinical Psychology, p. 128.
68. Ibid., pp. 130-132.
69. Ibid., p. 183. See also Gordon W. Allport, "The Journal of Abnormal and Social Psychology: An Editorial," Journal of Abnormal and Social Psychology 33 (1938): 6-7.
70. Burnham, Psychoanalysis and American Medicine, pp. 13-23; and Hale, Freud and the Americans, pp. 117-224. See also Nathan G. Hale, Jr., James Jackson Putnam and Psychoanalysis: Letters Between Putnam and Sigmund Freud, Ernest Jones, William James, Sandor Ferenczi, and Morton Prince, 1877-1917 (Cambridge, Mass.: Harvard University Press, 1971).
71. Burnham, Psychoanalysis and American Medicine, pp. 108-123, 180-213; and Hale, Freud and the Americans, pp. 274-312, 332-368.
72. Burnham, Psychoanalysis and American Medicine, pp. 128-133; and Hale, Freud and the Americans, pp. 194-195, 358-360.
73. Burnham, Psychoanalysis and American Medicine, pp. 126-127; and Hale, Freud and the Americans, pp. 3-23. Freud's lectures were published as Sigmund Freud, Five Lectures on Psycho-Analysis, trans. & ed. J. Strachey (New York: Norton, 1977).
74. Burnham, Psychoanalysis and American Medicine, pp. 180-213; and Hale, Freud and the Americans, pp. 280-285, 332-335, 355-356. Hull's interest in a behavioral reinterpretation of psychoanalysis found expression in John Dollard, Leonard W. Doob, Neal E. Miller, O. H. Mowrer, and Robert R. Sears, Frustration and Aggression (New Haven, Conn.: Yale University Press, 1939).
75. William McDougall, The Energies of Men: A Study of the Fundamentals of Dynamic Psychology (London: Methuen & Co., 1932), pp. v-ix, 305-316; and William McDougall, "William McDougall," in A History of Psychology in Autobiography, vol. 1, ed. C. Murchison (Worcester, Mass.: Clark University Press, 1930), pp. 208-211, 216-217.

See also Boring, Experimental Psychology, pp. 465-467.

76. Burnham, Psychoanalysis and American Medicine, pp. 140-147.

77. Burnham, Psychoanalysis and American Medicine, pp. 134-147, 145-146, 150-158; and Hale, Freud and the Americans, pp. 313-331, 434-461.

78. Boring, Experimental Psychology, pp. 508-517; Ben-David and Collins, "Origins of a New Science," pp. 457-458, 465; Heidebreder, Seven Psychologies, pp. 152-200; and Watson, Great Psychologists, pp. 355-364. The classic biography of James is Ralph B. Perry, The Thought and Character of William James, 2 vols. (Boston: Little, Brown, 1935).

79. Kuklick, American Philosophy, pp. 186-195.

80. *Ibid.*, pp. 407-414.

81. *Ibid.*, pp. 407-414. For a more sympathetic treatment of Lowell see Henry A. Yeomans, Abbott Lawrence Lowell: 1856-1943. (Cambridge, Mass.: Harvard University Press, 1948).

82. Kuklick, American Philosophy, pp. 413-441.

83. *Ibid.*, pp. 459-460, 461-463.

84. *Ibid.*, pp. 460-461.

85. See Chapter 4, pp. 103-111 and Chapter 5, pp. 140, 142-145 for a detailed discussion of this issue.

CHAPTER 3:
MURRAY'S EDUCATIONAL BACKGROUND:
TRAINING IN MEDICINE AND BIOCHEMISTRY

Introduction

Henry A. Murray was an anomaly among academic psychologists in the United States. By his own admission, he had virtually nothing in common with the vast majority of his professional colleagues. He stated in his autobiography that his

past history differed from all but a small fraction of one per cent (as a crude estimate) of the membership of the American Psychological Association (as it was then and it has been over the years, so far as I can tell) in most of ten respects.[1]

Murray then proceeded to list such factors as his education in private schools, his advanced training in medicine and biochemistry, his personal wealth, and his advocacy of psychoanalysis as examples of his professional idiosyncrasy. This is of particular importance in the context of the increasing specialization required for academic positions in American colleges and universities, and of the growing acceptance of defined disciplinary boundaries in academic disciplines.

Especially among psychologists at Harvard during the 1930's, issues of professional credentials and merit would come to play an important role in faculty promotion and tenure decisions. This would prove to be a significant factor in Murray's efforts to draw upon his interdisciplinary background in the creation of a broad, multifaceted approach to research and theory on human personality. That he was to eventually succeed in these efforts was, in large degree, dependent upon the intellectual and professional contacts he made prior to 1926. It were these experiences that laid the foundation for his "need-press" theory of personality, the "multiform method" of research, and the funding from the Rockefeller Foundation that secured his position at Harvard.

Childhood and Education

Murray was born on May 13, 1893, into a life of wealth and comfort. He was the second of three children. The eldest was a sister, and the youngest--a brother named Cecil--was to later prove instrumental in Murray's career development. The inheritance from his maternal grandfather, a highly successful merchant and president of the Mutual Life Insurance Company of New York, guaranteed Murray a life of ease.[2] His earliest memories reflect his privileged origins-- summers spent on Long Island, winters spent in one of the most fashionable neighborhoods in New York City, extended vacations in Europe. These memories do not reveal

any single decisive event that may have predisposed Murray to take up as an adult the study of the depths of the human psyche. Instead, only a few, relatively minor events can be seen as in any way foreshadowing his later choice of psychology as a career.[3]

Probably the most important event was his exposure in early life to relatives who had psychiatric disorders of varying degrees of severity. Two aunts from his mother's side of the family suffered from fairly severe psychological disorders. One had bouts of a paranoid psychosis, while the other had physiological symptoms resulting from a hysteric conversion reaction.[4] Also, Murray's mother was prone to anxiety-provoked migraine headaches. While apparently not suffering from any serious disorder, she was quite open about expressing her innermost feelings and anxieties to the rest of the family. Murray somehow felt responsible for her headaches, perhaps since he tended to feel that they accompanied periods of misbehavior on his part. In response, he developed a deep sense of empathy for her sufferings, and took it upon himself to try to alleviate her discomfort by being a "good boy." [5]

While Murray was thus sensitized to the suffering of the psychologically disturbed and to the need to relieve their suffering, his specific approach to psychology developed from a much different source. Murray's childhood heroes were the famous explorers of unknown territories,

exemplified by the Norwegian explorer Fridtjof Nansen and by Defoe's Robinson Crusoe. Murray feels that this later influenced his adherence to psychoanalytic approaches to the study of personality. In particular, he draws the parallel between the desire to explore the uncharted territories of the Arctic and Africa and his life's work in investigating the depths of the unconscious mind.[6]

These two factors--early exposure to psychologically disturbed people and an interest in exploring the unknown--probably predisposed Murray towards his later work in psychology. But this interest developed only gradually and indirectly, for Murray's career interests first turned toward research in medicine and biochemistry. Murray traces this indirect route to psychology through an undistinguished undergraduate career to brief interludes with surgery, physiology, biochemistry, and finally, embryology.[7]

Murray was educated at some of the most exclusive private schools on the East Coast. His elementary education was received at two small private schools in New York City, and his secondary education at Groton Academy in Massachusetts.[8] By his own admission, Murray was not much of a scholar. Instead, he preferred to devote most of his energy to athletics, although he did not excel there either.[9] However, Murray's lack of performance in scholarly activities was probably not totally due to lack of effort on his part. He claims that the educational

atmosphere at Groton was not conducive to effective learning. The instructors there tended toward the dogmatic and authoritarian, leaving little or no room for questioning or debate. Murray's interest in intellectual matters was captured only by the study of history, and he did win a prize in his senior year for a short paper on the life of John Brown of Osawatomie.[10] Murray further claims that the stifling classroom atmosphere, coupled with a social environment stressing competition for high grades, resulted in memorization, rather than the development of a critical intellect. Thus, the average Groton student went on to college simply because that was the expected thing for well-to-do young men to do. Murray followed this course as well, even though he had little desire to actively pursue intellectual goals.[11]

Murray graduated from Groton in May of 1911, and entered Harvard College that fall. True to his preparation at Groton, he spent his Harvard years in pursuit of the "Gentleman's C." [12] Students with money lived fairly well in Cambridge, most obtaining room and board in the prestigious "Gold Coast" section of Mount Auburn Street. College life was a time for social activities, sports, and an avoidance of academic pursuits.[13] Murray fit in well with the wealthy Harvard undergraduate society. He made the crew team. He courted, and eventually married, Josephine Rantoul, a Radcliffe student and heiress to the DuPont family fortunes. He majored in History, but was not

inspired by his studies. He even went so far as to avoid all classes he judged uninteresting. For example, he dropped out of Professor Hugo Münsterberg's introductory psychology class after two lectures, because he thought the course was irrelevant. Münsterberg began the semester with an examination of the physiology of the ear as it related to phenomena of auditory perception, and Murray could not see what this had to do with the way people thought and felt about their everyday existence, which he assumed to be the proper subject matter of psychology.[14]

However, as much as he did fit the stereotype of the playboy Harvard undergraduate, Murray began to feel discontented with the course he was following. While Harvard was a haven for the scions of the East Coast aristocracy, it did expose Murray to a wider range of experiences than he had known in New York or at Groton. Harvard maintained its tradition of academic freedom and free speech in the face of the growing, strident patriotic fervor that swept the country prior to its entry into World War I. Therefore, even though student opinion generally favored American intervention in the war, a vocal anti-war and socialist minority thrived on campus.[15] In this context of ongoing social and political controversy, Murray came to question his conservative and elitist upbringing and values. He decided to make a break with his past. Rather than pursue a career in his family's insurance business, Murray chose to follow his childhood interest in helping

people. He thus decided to take up a career in medicine.[16]

Murray entered the Columbia University College of Physicians and Surgeons in 1915, and received his M.D. in 1919. His initial intention was to become a practicing surgeon, and toward this end he began his studies by focusing on practical courses in surgery and anatomy. This plan, though, was cut short due to a problem with Murray's visual acuity. As a child, he had been troubled by a tendency for his eyes to cross. When physical therapy failed to produce an improvement in this condition by the age of nine, several of the orbital muscles of his eyes were surgically severed. While this ended the problem of his eyes crossing, it left another problem in its place. Now, Murray was unable to focus both eyes on a single object.[17] The extent of this new visual problem was diagnosed during his first year of medical school. Murray went to see Dr. Smith Ely Jelliffe, a prominent New York psychiatrist, about a problem he had with stuttering whenever he had to speak in public. Jelliffe immediately noticed Murray's problem with focusing, and suggested this as possibly being related to his speech impediment. While the psychological significance of this diagnosis was lost on Murray at that time, the realization of the extent of his visual impairment forced him to abandon his plans of becoming a surgeon.[18]

Since surgery was now out of the question as a career, Murray decided to change his area of concentration and devote himself to medical research. He began studying physiology under the direction of Dr. George Draper, and soon became one of Draper's research assistants.[19] Murray was associated with Draper while the preliminary research was being conducted for Draper's major publication on the relationship between physical constitution and disease. Draper was convinced that many physical disorders were the result of psychological, rather than biological, causes. He was also convinced that the psychological factors responsible for disease expressed themselves in the overall make-up of the body structure. Draper thought, therefore, that insights about an individual's personality could be gained by correlating observed psychological and immunological tendencies with measurable physical characteristics. This information could then be used in diagnosing and treating diseases.[20]

Draper was an excellent teacher, but Murray was still a slow learner. Draper saw that Murray possessed a keen mind, but that his Groton and Harvard education had not sufficiently prepared him to take on the critical, observant attitude that was necessary for excellence in scientific work. Draper suggested that Murray spend a summer at the Woods Hole Biological Laboratory in order to improve his powers of observation. Murray, however, did not follow up on this suggestion.[21]

While Draper failed to prod Murray into preparing himself sufficiently for a scientific career, several other factors combined to produce the same effect. One of Murray's closest friends in medical school was Alvan Barach, a brilliant scholar who was destined for a distinguished career in medical research.[22] Barach was the first Jew with whom Murray developed a close friendship, and their relationship provided a contrast in cultural backgrounds that forced Murray to question further the values he held as a result of his privileged upbringing. This self analysis led Murray to make a permanent break with the conservative attitudes of his family.[23]

The critical and questioning attitude he developed as a result of this self analysis was to be reinforced by the practical emphasis of the training Murray received during his last two years in medical school. The first two years had been devoted to course work, while the final two years were spent in the wards of New York's Presbyterian Hospital, working and studying alongside practicing physicians. As a major teaching hospital, the Presbyterian Hospital boasted many prominent specialists as members of its staff. An important feature of the training Murray received there was the "Grand Rounds." Each incoming patient was examined by the chief practitioner of each medical speciality area, with the examination being observed by the students. Upon completion of all the examinations, the specialists would confer, share their various insights, and through discussion

reach a diagnosis of the patient's problem.[24] From this diagnostic procedure Murray learned the value of being open to many divergent sources of information. He saw the importance of this for both medical practise and research, in that the careful analysis of different viewpoints was crucial for being able to claim some degree of certainty about one's own opinions. This was to prove significant for the development of the "Multiform Method" of research that he later pioneered at the Harvard Psychological Clinic.[25]

Research in Medicine and BioChemistry

Upon graduation from medical school in 1919, Murray began work on a Master's degree in Biology at Columbia University. This was a direct result of his decision to pursue a career in research instead of a medical practise. Since Murray was by now fully aware of the limitations of his educational background for a potential researcher, he saw the benefits to be gained in pursuing a research degree.[26] While at work on his Master's, Murray received a letter from his younger brother Cecil that was to change the course of his life. Cecil Murray was four years younger than Henry Murray, and was at that time finishing his degree in Biology at Harvard. Cecil wrote to inquire about his brother's opinion of the Harvard biochemist Lawrence J. Henderson. It was Cecil's opinion that Henderson was America's leading biochemist, but Henry had to admit that he

had never heard of Henderson, neither during his four years at Harvard, nor during his time at Columbia.[27]

Conversations with Cecil convinced Murray that Henderson was indeed a very important figure in biochemical research, and Murray decided that he should return to Harvard to study and work with Henderson.[28] First, though, he had to finish the work on his Master's degree. The year 1919-1920 was devoted to this, and to several other projects. Not only did Murray receive his Master's degree for research on the role of calcium in metabolism, he engaged in an investigation of the development of the cardiac loop in rabbit embryos, and published a paper based on this latter work.[29] Murray also published three other papers that year. One paper presented a report on the use of microscopic examination of biopsy material in the diagnosis of two unusual cases of skin cancer.[30] A second paper, coauthored with his friend Alvan Barach, presented clinical evidence of the development of tetanus as a side effect in cases of sprue, a tropical gastric-intestinal disease.[31] The final paper, coauthored with George King, was a brief report of the construction and use of a new instrument for measuring the rate of blood coagulation as an aid in the diagnosis of hemophilia.[32]

During the summer of 1920, Cecil Murray arranged for his brother to meet with Henderson at Harvard. Henderson was sufficiently impressed with Murray to offer him a

position as a part-time lecturer in physiology, and as a research assistant in Henderson's laboratory.[33] Murray was apparently offered this position for two reasons. Henderson had studied physical chemistry under Harvard's T. W. Richards, received his M.D. from Harvard in 1902, and had studied biochemistry with Franz Hofmeister at Strassburg. While Henderson had devoted his career entirely to scientific research, his medical training led him to admire the practicing physician who had been trained to be decisive, yet self-critical in his diagnosis. This was exactly the type of diagnostic training Murray had received under Draper and at the Presbyterian Hospital.[34] Also, Henderson thought Murray would make a good research assistant because he had some experience in blood-analytic work and other topics related to vascular functioning. This was important as Henderson was at that time engaged in a major investigation of the self-regulating chemical properties of the blood plasma that was to result in the 1928 publication of Blood: A Study in General Physiology. [35]

Henderson had first achieved prominence in American biochemical circles for his advocacy of the methodological and theoretical views of the French biochemist Claude Bernard, and the American physicist Josiah Willard Gibbs, in his own work on the acid-base equilibrium of the blood.[36] Henderson saw Bernard's work as crucial for establishing the self-regulating chemical properties of the blood, while he

saw Gibbs as pioneering a mathematical systems analysis of chemical properties.[37] While Bernard had studied only a limited set of chemical reactions in developing his concept of the consistency of the milieu intérieur, or internal environment, Henderson used Gibbs' systems model to extend Bernard's concept to conceive of all biological activity as a self-regulating system. For example, in his 1913 book The Fitness of the Environment, Henderson argued that life exists on earth not only due to the ability of organisms to adapt to the environment, but also due to the presence on earth of an environment that tends to remain constant and is uniquely suited for the support of life. Thus, he claimed that organic life and the environment formed a biological system, and that even relatively minor changes in the latter could result in the total eradication of the former.[38] Henderson later extended this view to his analysis of the structure of human society, greatly influenced by his study of the social theory of Vilfredo Pareto in the mid-1920's.[39]

A consequence of Henderson's systems approach to the study of biological phenomena was his belief in the necessity of a teleological, rather than a deterministic, explanation for the functions of any system. This was a direct elaboration on Bernard's and Gibbs's observations that a complex biological system could not be accurately described through a simple enumeration of its constituent chemical reactions.[40] Henderson argued that statistical

probability alone could not account for the evolution of living systems from all the chemical interactions that the environment made possible. Instead, he argued that a definite functional relationship existed between living organisms and their environment, and that this relationship was of a purposive nature.[41]

Thus, while Murray was working on detailed chemical investigations of the blood for Henderson, he was also being exposed to Henderson's views on biology as a teleological system. Henderson integrated this systems view of biology into his research methodology. Each day's activities began with Murray and a fellow research assistant, Franklin C. McLean, visiting a Cambridge slaughter house to collect several pails of fresh cattle blood. They would then rush back to Harvard and subject the blood samples to a series of chemical analyses. While they could at that time only describe their results verbally, Henderson was soon to develop a special technique he referred to as the "Cartesian Nomograph." [42] Using this technique, Henderson could graphically display the relationship between seven chemical compounds in the blood, showing how a change in any one compound affected the remaining six. An early presentation of this work occurred in 1920. Murray, McLean, and Henderson published a report describing the results of some preliminary research on the interrelations between the concentration of chloride compounds in the blood plasma and its degree of acidity.[43]

After completing his work with Henderson, Murray returned to New York in 1921 to undertake a two-year surgical internship at the Presbyterian Hospital.[44] This internship was to prove crucial in his turn from medicine and biochemistry to psychology. Ever mindful of Draper's admonitions that psychological factors lay at the root of many physical illnesses, Murray saw a great deal of evidence for this among his patients. In particular, he was impressed by the case histories of two patients; a mobster trying to kick a drug addiction and a politician suffering from polio. These two patients took Murray into their confidence, and from their self-revelations he gained a great appreciation for the role of their subjective experience in determining the course of their convalescence.[45]

While doing this internship, Murray maintained an active involvement in biochemical research with several colleagues from the Columbia University College of Physicians and Surgeons. This research was carried out under the direction of Dr. A. B. Hastings, the Assistant Sanitary Chemist of the United States Public Health Service, and focused on integrating the findings of Murray's earlier research on tetanus resulting from gastric-intestinal disorders with Henderson's work on blood chemistry. Their basic procedure was to experimentally induce tetanus in dogs, either by surgically closing the pylorus (the aperture between the stomach and the duodenum) or by removing the

parathyroid gland. Blood samples would then be taken periodically up to and including the time the subject had to be destroyed. The blood samples were then analyzed to determine the calcium content, pH level, hydrogen ion concentration, blood sugar level, non-protein nitrogen content, and the amount of urea in the blood plasma.[46] They concluded from this research that tetanus resulted from a disturbance in the blood pH level stemming from a gastro-intestinal blockage which prevented the reabsorption of acids found in the digestive fluid.[47] The importance of this experimental finding was underscored by the clinical observation of identical phenomena in several human tetanus patients.[48]

This research resulted in Murray's nomination in 1923 as an Assistant Member of the Rockefeller Institute for Medical Research in New York City.[49] Murray went to work there under the direction of Dr. Alfred E. Cohn, who was investigating the many biochemical, metabolic, and physiological changes that occurred in developing chicken embryos.[50] Cohn was an excellent example of a scientist who had managed to strike a working balance between his interest in pure research and a concern for the pragmatic impact of that research on the well-being of society. He was keenly aware of the social forces that indirectly influenced the course of scientific research. He was also appreciative of the fact that science was ultimately accountable to the greater society for the fruits of its

research.[51] The work that he and Murray conducted on the subject of embryological development was motivated by just such an awareness of the social importance of scientific knowledge, in this case, the need for knowledge about the processes involved in the early development of living organisms.[52]

This research required Murray to spend many long hours inside a large incubator studying the physical changes occurring in the developing embryo. This was accomplished by placing a small window in the egg shell and observing the egg's interior using a microscope. Also, numerous samples were extracted from within the egg in order to monitor the biochemical changes taking place.[53] Between 1923 and 1925, Murray and Cohn published three articles based on their work in the Journal of General Physiology and the Journal of Experimental Medicine under the general title of "Physiological Ontogeny."[54]

The first article dealt with the rate by which nutrient substances found in the egg shell were metabolized for use by the developing embryo.[55] The second article presented a mathematical description of the rate of embryonic growth, in terms of both gross weight and velocity of growth (the percentage increase in weight). Their findings indicated that while gross weight increased exponentially with age, the velocity of growth decreased, being inversely proportional to age.[56] The last of these three articles

dealt with a more detailed investigation of the decrease of growth velocity with age. Murray and Cohn found that the decrease in the growth rate was a function of the general condition of the organism, and not a feature of cells developing in isolation. By removing tissue samples from embryos in an advanced state of development and placing them in a fresh nutrient medium, they found that the tissue cultures would soon revert to the rapid rate of growth exhibited by embryos in an early state of development.[57]

Conversion to Psychoanalysis

The appointment to the Rockefeller Institute offered Murray more than just an opportunity to engage in serious research. The Institute was organized in such a manner as to encourage social and intellectual exchange among the staff. The staff met daily for lunch to discuss their research, or to hear an invited guest give an informal talk.[58] Murray was present at the luncheons, and had the opportunity to hear a number of renowned scientists expound upon their work. For example, in 1924 the behavioral psychologist John B. Watson, who was at that time a vice-president of the J. Walter Thompson advertising agency, gave a rather caustic talk on his view of what was good and bad in psychological theory and research.[59] Most of the time, however, the luncheon discussants were members of the Institute's staff, and the most heated arguments

occurred whenever Jacques Loeb and Alexis Carrel brought up the topic of biological theory.[60]

While both conducted research in biochemistry at the Institute, their interpretations of the same data differed drastically. Loeb, who had been trained in the German reductionistic approach to biology at the Universities of Berlin, Munich, and Strassburg, saw all activities of living organisms as resulting from the operation of rigidly mechanistic chemical processes.[61] Carrel, on the other hand, had taken his M.D. degree at the University of Lyon in France, and insisted that a mechanistic science overlooked the psychological and spiritual aspects of life. Thus, he claimed, mechanistic science could only present a partial view of reality.[62] Murray found this difference of opinion quite confusing. He wondered how two researchers who studied the same problems with the same experimental techniques could disagree so completely as to what their findings meant.[63]

Murray felt that the explanation of this situation was probably of a psychological nature, but what little knowledge he had of this discipline led him no closer to an answer. Aside from his extremely brief tenure in Professor Munsterberg's introductory psychology course, Murray's exposure to psychology consisted solely of one psychiatry course in medical school, an unsuccessful attempt at reading Freud's The Interpretation of Dreams, and a more successful

reading of William James's The Varieties of Religious Experience.^[64] However, as limited as his background in psychology was, his interest in the topic had been greatly increased by his knowledge of Draper's theories of psychosomatic illnesses and his interest in the plays of Eugene O'Neill. In particular, Murray had been extremely impressed by O'Neill's adept portrayals of the psychological motivations of his characters.^[65] As a result, Murray's interest in psychology was greater than outward appearances indicated, but the proper catalyst had not yet presented itself to produce a complete expression of this interest.

This catalyst was to appear in the guise of Carl G. Jung's Psychological Types.^[66] Murray happened to be browsing in a New York bookstore on the same day in 1923 when the English translation of the book was released for sale. He picked up a copy, glanced quickly through it, and immediately knew that he had to read it. He bought the book and took the next two days off from work in order to read it.^[67] The reading of Psychological Types was an act of revelation for Murray. He felt that Jung's description of eight psychological types representing different modes of consciously dealing with one's environment allowed him to understand the arguments between Loeb and Carrel. If their "psychological types" were indeed different, then their arguments were a consequence of the different ways of perceiving reality that were characteristic of their personalities.^[68]

Murray immediately went on to read all of Jung's published writings, and went back a second time to read the works of Freud--this time successfully. He began attending lectures on psychology and psychoanalysis at New York's New School for Social Research.[69] He became so involved in the study of psychoanalysis that in 1924 he wrote to Jung in Zurich, Switzerland, asking if he could come to visit and discuss psychological theory. Jung responded affirmatively.[70]

Even though Murray was by now devoting the majority of his time and effort to the study of psychology, he was still actively engaged in his research at the Rockefeller Institute. His research with Cohn was progressing well, and he decided that he should take a year's leave of absence to take his Ph.D. in biochemistry. Since Murray's recent research was based in part on the work of Cambridge University's Nobel Laureate Professor of Biochemistry, Sir Frederick G. Hopkins, F.R.S., Murray applied for and received permission to travel to England to take his Ph.D. at Trinity College's Biochemical Laboratory.[71] This plan was also advantageous as it would give Murray an opportunity to travel to Switzerland to meet with Jung.

Murray left for England in September of 1924. A friend had given him a copy of Herman Melville's Moby Dick to read during the voyage, and Murray was taken by this book in much the same way that he had been affected by Jung's

Psychological Types. Murray was struck by the depth of Melville's characterizations, the complexity of his metaphors, and most important, by his literary premonition of many of the psychological insights of Freud and Jung.[72] This impression of Melville was further strengthened by his conversations with a fellow-passenger, the British surgeon and Melville scholar, Sir John Bland-Sutton.[73]

Once in Cambridge, Murray went to work at the Biochemical Laboratory. At first, his work centered on completing two experiments that had been begun at the Rockefeller Institute. The first experiment showed that as chicken embryos developed, the nutrients in the egg were metabolized and converted into embryonic tissue. This change occurred as an orderly progression through the following chemical conversions: materials in solution were converted into solids; inorganic compounds were converted into organic compounds; carbohydrates were converted into proteins; and finally, proteins were converted into fats.[74] The second study presented evidence critical of the assumption generally held by embryologists that as the growth rate decreased, the slowing metabolism was correlated with increasing tissue differentiation. Murray's research suggested, instead, that growth and differentiation occurred in separate phases, with periods of overall growth being followed by periods of tissue differentiation.[75]

Finally, Murray undertook the research for his Ph.D. This work focused in part on extending Hopkins's investigations of the role of the chemical glutathione in cell metabolism, to the overall development of the embryo. Murray's results indicated that glutathione was incorporated into the developing embryo at the same rate as other chemical compounds, indicating that metabolism developed according to the same schedule as did all other life processes.[76]

As productive as the year in Cambridge was for Murray in terms of research, he also found time to take advantage of the stimulating social environment there. Murray's co-workers in the Biochemical Laboratory were J. B. S. Haldane and Joseph Needham. These two men presented a contrast in ideologies that was nearly equal to that presented by Loeb and Carrel at the Rockefeller Institute. Haldane was an outspoken Marxist and atheist, while Needham generally avoided politics but professed a sincere belief in traditional Anglican Christianity as well as an intellectual interest in eastern philosophy.[77] Murray also enjoyed the company of several philosophers and literary scholars, including I. A. Richards, C. D. Broad, and C. K. Ogden, while in Cambridge.[78]

The high point of the year abroad, however, did not involve Murray's work towards the Ph.D. Rather, it involved his daily meetings with Jung that took place in Zurich

during his three-week Easter vacation from Cambridge.[79] Although Murray's intention had been merely to discuss with Jung a number of questions regarding some of the more complex theoretical issues raised in the Psychological Types, after only two days of polite talk the subject shifted dramatically. Jung was a very open and exuberant individual, and did most of the talking. It soon became clear that what Jung wanted to talk about would be the day's topic, and Jung simply did not want to talk about the Psychological Types. Instead, he wanted to discuss his recent work on the concept of the anima, the feminine element of each man's psychological makeup.[80]

This unexpected topic created an intense emotional response in Murray. Jung spoke of the anima--the "she who must be obeyed"--as a powerful directive force among one's psychological motivations.[81] This was a profound revelation to Murray, as it offered an explanation for his long-standing empathy for the sufferings of his mother and two aunts, and for several similar, more recent incidents.[82]

The rest of their three weeks together were spent analyzing a flood of psychological material that was released as a result of Murray's growing awareness of the feminine-intuitive aspect of his personality, especially as it contrasted with the masculine, rational, and scientific aspects that he emphasized in his medical and biochemical

research. This encounter left Murray a drastically changed man. He later described this experience in the following terms:

Dr. Jung [was] the first full-blooded, spherical--and Goethean, I should say--intelligence I had ever met. . . . We talked for hours, sailing down the lake and smacking before the hearth of his Faustian retreat [Jung's home at Bollingen on Lake Zurich]. "The great flood-gates of the wonder-world swung open," and I saw things that my philosophy had never dreamt of. Within a month a score of bi-horned problems were resolved. . . . I had experienced the unconscious.[83]

Murray came away from this encounter resolved to abandon biochemistry and to pursue a career in psychology.[84] He was, however, not someone who would walk away from prior commitments. He had to first complete his Ph.D. research and the remainder of his appointment to the Rockefeller Institute. Murray finished his research at Cambridge and returned to work in Cohn's laboratory in New York. Once back with Cohn, their embryological research progressed rapidly. First, Murray published a paper showing that previously determined relationships between temperature and biological functions depended upon the maintenance of the integrity of the internal environment of the living organism. Once this integrity had been disturbed, the previously observed relationships vanished.[85] Murray then published a second paper reporting his experimental replications of a number of important findings in embryology, but arguing that the data supported the view that embryonic development was characterized by orderly and

directed growth rather than by rigidly deterministic processes of chemical equilibria.[86] Finally, Murray reconfirmed by the use of newer and more precise experimental methods, his earlier finding that the metabolic rate of the embryo decreased with age.[87]

Even though his research was progressing well, Murray was spending less of his time in the laboratory, and more of it attending lectures on psychology and psychoanalysis at the New School for Social Research.[88] His changing interests found expression in an article he wrote for The Independent early in 1926.[89] This paper, while ostensibly written as a review of current psychological publications, provides an important insight into Murray's initial orientation to the discipline. What is immediately striking is that while he professed to present an eclectic survey of the psychological literature, it is implicitly clear that he considered only the works of a few theorists to be of any importance. Indeed, for the most part this article focused exclusively upon the area of abnormal psychology, and in particular upon Sigmund Freud's Collected Papers and Carl Gustav Jung's Psychological Types. [90] While he did direct the reader to several other sources-- notably the writings of Hart, McDougall, Prince, and Janet--these too dealt primarily with abnormal psychology.[91]

In light of Murray's opening statements in The Independent article expressing his breadth of knowledge of psychology, it is surprising that he glossed over the rest of the available psychological literature with only a reference to Koffka's The Growth of the Mind and to a survey textbook on biological and behavioral theories of psychology.[92] In place of any significant discussion of current work in experimental psychology, Murray directed the reader instead to the philosophical writings of C. K. Ogden and C. D. Broad, as well as to works on religion and literary criticism.[93]

Murray's new professional identity was thus established. He decided that he should transfer his medical and biochemical training to psychology, by becoming a psychotherapist and pursuing research on psychopathology in a clinic or hospital setting.[94] Cohn was naturally disappointed that Murray was planning to leave a promising future in biochemical research, but he did not try to dissuade Murray as he also had more than a passing interest in the therapeutic uses of psychoanalysis.[95] Therefore, when the invitation came in the spring of 1926 from Harvard's President A. Lawrence Lowell, asking Murray to be Dr. Morton Prince's assistant at the newly-formed Harvard Psychological Clinic, Murray left New York with Cohn's best wishes for his future success.[96]

Conclusion

Several important themes emerge from this discussion of Murray's youth and education that bear directly upon his career in psychology at Harvard. One theme is that of the sensitizing effects of early exposure to a particular phenomenon and the later choice of a career related to it. Murray was exposed early in his life to psychopathology--in one instance his own stuttering, and in other instances involving close family members. These experiences helped lead him first to a career in medicine, and later to a career in psychology. Another theme focuses on the direction his work in psychology would take once the career choice had been made. His research with L. J. Henderson showed Murray that more complete results could be obtained from systematic, wholistic research methodologies than that resulting from reductionistic strategies. This was to be perhaps the most important influence on his later work.

Murray's advocacy of psychoanalysis points out another major theme--his disdain for irrelevance in academic disciplines. Murray was opposed to traditional academic psychology because it did not address the questions of importance to the everyday lives of real people, whereas psychoanalysis not only raised these questions, but attempted answers as well. Finally, the theme of Murray's professional contacts takes on great importance. These contacts were not only significant in terms of their

intellectual impact, but also in regards to the personal and financial support they provided to Murray in his times of greatest need.

Reference Notes

1. Henry A. Murray, "Henry A. Murray," in A History of Psychology in Autobiography, eds., E. G. Boring and G. Lindzey (New York: Appleton-Century-Crofts, 1967), pp. 286-287; and Henry A. Murray, Personal interview, 28 April 1981.
2. Murray, "Henry A. Murray," pp. 296-298; Henry A. Murray, Videotaped interview with Eugene Taylor, 12 November 1981, Rare Books, Countway Library, Harvard University Medical School, Boston, Mass.; and Henry A. Murray, Personal interview, 1 December 1981.
3. Murray, "Henry A. Murray," pp. 287-291, 297-298.
4. Ibid., p. 288.
5. Ibid., pp. 299-300.
6. Ibid., pp. 300-301.
7. Murray, "Henry A. Murray," pp. 289-290; and Henry A. Murray, "Preparations for the Scaffold of a Comprehensive System," in Psychology: A Study of a Science, Vol. 3, ed., S. Koch (New York: McGraw-Hill, 1959), pp. 9-17.
8. Murray, "Henry A. Murray," p. 286; Murray, Interview, 28 April 1981; and Henry A. Murray, Personal interview, 13 July 1981.
9. Murray, "Henry A. Murray," pp. 286-287; and Murray, Interview with E. Taylor, 12 November 1981.
10. Murray, "Henry A. Murray," p. 287; and Henry A. Murray, "Groton and Adaptation," The Grotonian 35 (1919): 402-419, especially pp. 408-415. "John Brown of Osawatomie" is an archaic reference for the Captain John Brown who led the attempted insurrection at Harper's Ferry, West Virginia. For a biography see: Stephen Vincet Benet, John Brown's Body (New York: Holt, Rinehart and Winston, 1955); and Stephen B. Oates, To Purge this Land with Blood: A Biography of John Brown (New York: Harper & Row, 1970).
11. Murray, "Groton," pp. 412-413; and Murray, "Henry A. Murray," pp. 286-287.
12. Murray, "Henry A. Murray," pp. 286-287; and Murray, Interview with E. Taylor, 12 November 1981.
13. Seymour M. Lipset and David Riesman, Education and Politics at Harvard (New York: McGraw-Hill, 1975), pp. 153-155; and Henry A. Yeomans, Abbott Lawrence Lowell:

1856-1943. (Cambridge, Mass.: Harvard University Press, 1948), pp. 66-69, 125-127.

14. Murray, "Henry A. Murray," p. 290; Henry A. Murray, "What Should Psychologists Do About Psychoanalysis?" Journal of Abnormal and Social Psychology 35 (1940): 152; Murray, Interview, 28 April 1981; and Henry A. Murray, "Josephine Lee Murray 1894-1962," Radcliffe Quarterly, February 1965, pp. 9-10.

15. Lipset and Riesman, Education and Politics, pp. 133-138.

16. Murray, "Henry A. Murray," p. 303.

17. Murray, "Henry A. Murray," p. 301; and Robert W. Rieber, "Henry A. Murray," in The Roots of American Psychology: Historical Influences and Implications for the Future, eds., R. W. Rieber and K. Salzinger (New York: The New York Academy of Sciences, 1977), p. 321.

18. Murray, "Henry A. Murray," pp. 301-302.

19. Ibid., p. 288.

20. George Draper, Human Constitution: A Consideration of its Relationship to Disease (Philadelphia: W. B. Saunders, 1924). See in particular Chapter I, "General Considerations," pp. 17-45. Murray's work with Draper focused on the relationship between physical constitution and traits of personality. This was just one of the "four panels," or categories of information, that Draper thought a physician needed to have knowledge of prior to making a diagnosis. However, in Human Constitution Draper presented research involving only his first panel--the anatomic panel.

21. Murray, "Groton," p. 407.

22. Murray, "Henry A. Murray," p. 303. Barach became famous for developing the first safely usable oxygen tent. See his obituaries in The New York Times, 14 December 1977, p. D-17; and Time, 26 December 1977, p. 58.

23. Murray, "Henry A. Murray," p. 303.

24. Murray, "Henry A. Murray," pp. 288, 290-291; Murray, "What Should Psychologists Do?" p. 152; Murray, "Preparations," pp. 9-12; and Henry A. Murray, Personal interview, 14 October 1981.

25. Murray, "Henry A. Murray," p. 293; Murray, "Preparations," pp. 17-19; Murray, Interview 14 October 1981; and Henry A. Murray, Personal interview, 13 January 1982. See Chapter 6, pp. 210-211, 217-219 for a further discussion of this issue.

26. Murray, "Henry A. Murray," pp. 286-287; Murray, Interview with E. Taylor, 12 November 1981; and Rieber, "Murray," p. 321.

27. Murray, Interview with E. Taylor, 12 November 1981.

28. Ibid.

29. Henry A. Murray, "Studies in Calcium Metabolism and Other Related Subjects" (M.A. Thesis, Columbia University, 1920); and Henry A. Murray, "The Development of the Cardiac Loop in the Rabbit, with Especial Reference to the Bulboventricular Groove and Origin of the Interventricular Septum," American Journal of Anatomy 26 (1919): 29-39.

30. Henry A. Murray, "Two Unusual Cases of Melanocarcinoma," Proceedings of the New York Pathological Society 19 (1919): 28-37.

31. Alvan L. Barach and Henry A. Murray, "Tetany in a Case of Sprue," Journal of the American Medical Association 74 (1920): 786-788.

32. George King and Henry A. Murray, "A New Blood Coagulometer," Journal of the American Medical Association 74 (1920): 1452-1453.

33. Murray, Interview With E. Taylor, 12 November 1981; Rieber, "Murray," p. 321; and Henry A. Murray, Personal interview, 11 November 1981.

34. Bernard Barber, "Introduction," in On the Social System: Selected Writings, ed., B. Barber (Chicago: University of Chicago Press, 1970), p. 3; Walter B. Cannon, "Lawrence Joseph Henderson: 1878-1942," in Biographical Memoirs, Vol. 23 (Washington, D.C.: The National Academy of Sciences, 1945), pp. 31-58, especially pp. 32-33, 44, 48-50; Garland Allen, Life Science in the Twentieth Century (Cambridge: Cambridge University Press, 1975), p. 95; and Murray, "Henry A. Murray," p. 288.

35. Murray, "Cardiac Loop," pp. 29-39; King and Murray, "Coagulometer," pp. 1452-1453; and Lawrence J. Henderson, Blood: A Study in General Physiology (New Haven, Conn.: Yale University Press, 1928).

36. Cannon, "Henderson," pp. 33-42; Allen, Life Science, pp. 74, 99-100; John Parascandola, "Organismic and Holistic Concepts in the Thought of L. J. Henderson," Journal of the History of Biology 4 (1971): 63-113; and Barber, "Introduction," p. 4.

37. Cannon, "Henderson," pp. 33-42; Allen, Life Science; Parascandola, "Organismic and Holistic Concepts," pp. 63-113; and Barber, "Introduction," p. 4. Of special importance for the developments of Henderson's ideas were: Claude Bernard, Leçons sur les Phénomènes de la Vie Communs aux Animaux et aux Végétaux, 2 vols. (Paris: Faillière, 1878-1879); Claude Bernard, Introduction à l'étude de la Médecine Expérimentale (Paris: Baillière, 1865); and Josiah W. Gibbs, "On the Equilibrium of Heterogeneous Substances," Transactions of the Connecticut Academy of Arts and Sciences 3 (1875-1876): 108-248. For a review of Bernard's work see: James M. D. Olmstead and E. Harris Olmstead, Claude Bernard and the Experimental Method in Medicine (New York: Henry Schuman, 1952); and Francisco Grande and Maurice B. Virscher, Claude Bernard and Experimental Medicine (Cambridge, Mass.: Schenkman, 1967). Discussions of Gibbs's work can be found in Cannon, "Henderson," pp. 38-39, 43; Lawrence J. Henderson, The Order of Nature: An Essay (Cambridge, Mass.: Harvard University Press, 1917), pp. 125-138; James G. Crowther, Famous American Men of Science (New York: Norton, 1937), pp. 229-297; and Lynde P. Wheeler, Josiah Willard Gibbs: The History of a Great Mind (New Haven, Conn.: Yale University Press, 1951).

38. Lawrence J. Henderson, The Fitness of the Environment: An Inquiry into the Biological Significance of the Properties of Matter (New York: Macmillan, 1913). In particular see Chapters 1, 2, and 7, pp. 1-71, 249-273. It is important to note at this point that Parascandola differs from the interpretations of Cannon, Allen, and Bernard in claiming that Claude Bernard had little influence on the course of Henderson's work. Parascandola claims that all Henderson got from Bernard was a theoretical justification for his pre-existing research interests. See Parascandola, "Organismic and Holistic Concepts," pp. 81, 99.

39. Cannon, "Henderson," pp. 42-45, 49; and Lawrence J. Henderson, On the Social System: Selected Writings, ed., B. Barber (Chicago: University of Chicago Press, 1970). See in particular pp. 181-190.

40. Henderson, Fitness, pp. 282-300; and Henderson, Order, pp. 180-212.

41. Henderson, Order, pp. 3-9, 121-138.

42. Murray, Interview, 13 July 1981; Murray, Interview with E. Taylor, 12 November 1981; and Murray, Interview, 11 November 1981. An example of the use of the Cartesian Nomogram can be found in Henderson, Blood, pp. 97-98, 121-123.

43. Franklin C. McLean, Henry A. Murray, and Lawrence J. Henderson, "The Variable Acidity of the Hemoglobin and the distribution of Chlorides in the Blood,"

Proceedings of the Society of Experimental Biology and Medicine 17 (1920): 180-182.

44. Murray, "Henry A. Murray," pp. 287-288; Murray, "What Should Psychologists Do?" p. 152; and Rieber, "Murray," p. 321.

45. Murray, "Henry A. Murray," p. 288; and Murray, "What Should Psychologists Do?" p. 152. Given the description of the patient and the medical problem, there is reason to believe that the politician in question was Franklin D. Roosevelt, although Murray cannot disclose his patients' identities for reasons of professional ethics.

46. A. B. Hastings, Cecil D. Murray, and Henry A. Murray, "Certain Chemical Changes in the Blood after Pyloric Obstruction in Dogs," Journal of Biological Chemistry 46 (1921): 223-232; A. B. Hastings and Henry A. Murray, "Observations of Parathyroidectomized Dogs," Journal of Biological Chemistry 46 (1921): 233-256; Henry A. Murray, "The Chemical Pathology of Pyloric Occlusion in Relation to Tetany," Archives of Surgery 7 (1923): 166-196; and A. R. Felty and Henry A. Murray, "Observations on Dogs with Experimental Pyloric Obstruction," Journal of Biological Chemistry 57 (1923): 573-585.

47. Murray, "Chemical Pathology," pp. 195-196.

48. Henry A. Murray, "The Bicarbonate and Chloride content of the Blood in certain cases of Persistent Vomiting," Proceedings of the Society of Experimental Biology and Medicine 19 (1922): 273-275.

49. Rieber, "Murray," p. 321.

50. Murray, "Henry A. Murray," pp. 287, 289-290; and Murray, "Preparations," pp. 14-17.

51. Alfred E. Cohn, Minerva's Progress: Tradition and Dissent in American Culture (Port Washington, N.Y.: Kennikat Press, 1969), especially pp. 6-11; and Alfred E. Cohn, No Retreat from Reason: And Other Essays (New York: Harcourt, Brace and Co., 1931), especially pp. 11-13, 80-111.

52. Alfred E. Cohn and Henry A. Murray, "Physiological Ontogeny. I. The Present Status of the Problem," Quarterly Review of Biology 2 (1927): 469-470.

53. Murray, "Preparations," p. 13.

54. It is potentially important to note that the Journal of General Physiology was founded by Jacques Loeb and published by the Rockefeller Institute for Medical Research. It was in this journal that Murray published all but one of his embryological research papers. Since this

was the official "house organ" for the Institute, no doubt Murray had less trouble getting published than if he had not been a Member of the Institute.

55. Henry A. Murray, "Physiological Ontogeny. A. Chicken Embryos. II. Catabolism. Chemical Changes in Fertile Eggs during Incubation. Selection of Standard Conditions," Journal of General Physiology 9 (1925): 1-37.

56. Henry A. Murray, "Physiological Ontogeny. A. Chicken Embryos. III. Weight and Growth Rate as Functions of Age," Journal of General Physiology 9 (1925): 39-48.

57. Alfred E. Cohn and Henry A. Murray, "Physiology Ontogeny. A. Chicken Embryos. IV. The Negative Acceleration of Growth Rate with Age as Demonstrated by Tissue Cultures," Journal of Experimental Medicine 42 (1925): 275-290.

58. W. J. V. Osterhout, "Jacques Loeb: 1859-1924," in Biographical Memoirs, Vol. 13 (Washington, D.C.: The National Academy of Sciences, 1930), p. 325.

59. Murray, Interview, 13 July 1981; and Henry A. Murray, Interview with Gene F. Nameche, 4 November 1968. Transcript in the C. G. Jung Biographical Archive of the Frances G. Wickes Foundation, Rare Books, Countway Library, Harvard University Medical School, Boston, Mass.

60. Murray, "Henry A. Murray," p. 288; and Murray, "What Should Psychologists Do?" pp. 152-153.

61. Murray, "Henry A. Murray," p. 288; Osterhout, "Loeb," pp. 318-321, 329-334, 360-368; Jacques Loeb, The Mechanistic Conception of Life: Biological Essays (Chicago: University of Chicago Press, 1912), especially pp. 3-64; and Jacques Loeb, The Organism as a Whole: From a Physiochemical Viewpoint (New York: G. P. Putnam's Sons, 1916), especially pp. 1-13, 253-285.

62. Murray, "Henry A. Murray," p. 288; Theodore I. Malinin, Surgery and Life: The Extraordinary Career of Alexis Carrel (New York: Harcourt Brace Jovanovich, 1979), pp. 4, 109-123; and Alexis Carrel, Man the Unknown (New York: Harper & Brothers, 1939).

63. Murray, "Henry A. Murray," p. 288; and Murray, "What Should Psychologists Do?" pp. 152-153. For an overview of the mechanism-vitalism controversy in biology, see Hilde Hein, "The Endurance of the Mechanism-Vitalism Controversy," Journal of the History of Biology 5 (1972): 159-188; and Garland E. Allen, "T. H. Morgan and the Emergence of a New American Biology," Quarterly Review of Biology 44 (1969): 168-188.

64. Murray, "Henry A. Murray," pp. 288-290, 293.
65. Murray, "Henry A. Murray," p. 291; and Murray, Interview with G. F. Nameche, 4 November 1968.
66. Carl G. Jung, Psychological Types (New York: Harcourt, Brace & World, 1923).
67. Murray, "Henry A. Murray," pp. 288-289; Murray, "What Should Psychologists Do?" p. 153; and Murray, Interview with G. F. Nameche, 4 November 1968.
68. Murray, "Henry A. Murray," p. 289; and Murray, "What Should Psychologists Do?" p. 153.
69. Murray, "Henry A. Murray," p. 291.
70. Murray, Interview with G. F. Nameche, 4 November 1968.
71. Murray, "Henry A. Murray," p. 287; Murray, "Preparations," p. 9; Rieber, "Murray," p. 321. For the influence of the work done at the Cambridge Biochemical Laboratory under Hopkins on Murray's work, see Murray, "Chemical Pathology," p. 170n; Cohn and Murray, "Physiological Ontogeny. I," p. 492n; and Murray, "Physiological Ontogeny. II," pp. 33, 37n.
72. Murray, "Henry A. Murray" p. 287; Murray, Interview with E. Taylor, 12 November 1981; Edwin S. Shneidman, "Personology (as well as the Personologist) is Shaped by Numerous and Various Forces," in Endeavors in Psychology: Selections from the Personology of Henry A. Murray, ed., E. S. Shneidman (New York: Harper & Row, 1981), p. 5; and Hiram Hayden, "Portrait: Henry A. Murray," American Scholar 39 (1970): 126.
73. Murray, Interview with E. Taylor, 12 November 1981.
74. Henry A. Murray, "Physiological Ontogeny. A. Chicken Embryos. VII. The Concentration of the Organic Constituents and the Calorific Value as Functions of Age," Journal of General Physiology 9 (1926): 405-432.
75. Henry A. Murray, "Physiological Ontogeny. A. Chicken Embryos. VIII. Accelerations of Integration and Differentiation during the Embryonic Period," Journal of General Physiology 9 (1926): 603-623.
76. Henry A. Murray, "Studies in Physiological Ontogeny" (Ph.D. Dissertation, Cambridge University, 1928); and Henry A. Murray, "Physiological Ontogeny. A. Chicken Embryos. IX. The Iodine Reaction for the Quantitative Determination of Glutathione in the Tissues as a Function of

Age," Journal of General Physiology 9 (1926): 621-624. It is important to note the discrepancy between the date on the approval sheet for Murray's dissertation (1928) and the date given in the two major chronologies of Murray's career (Rieber and Shneidman both give 1927 as the date). Perhaps this reflects the delay between the oral defense of the dissertation and the receipt of the final written copy. This is a highly plausible hypothesis given Murray's general slowness in producing written work. See Chapter 7, pp. 235-237, 250-251.

77. Murray, "Henry A. Murray," p. 287; J. B. S. Haldane, Science Advances (New York: Macmillan, 1948); J. B. S. Haldane, The Causes of Evolution (Ithaca, N.Y.: Cornell University Press, 1966); and Henry Holorenschaw, "The Making of an Honorary Taoist," in Changing Perspectives in the History of Science, eds., M. Teich and R. Young (Dordrecht, Holland: D. Reidel, 1973), pp. 1-20.

78. Murray, "What Should Psychologists Do?" p. 153; and Murray, Interview with E. Taylor, 12 November 1981.

79. Murray, Interview with F. G. Nameche, 4 November 1968; Murray, "Henry A. Murray," p. 287; and Murray, "What Should Psychologists Do?" p. 153.

80. Murray, Interview with G. F. Nameche, 4 November 1968; and Murray, "What Should Psychologists Do?" p. 153.

81. Murray, Interview with G. F. Nameche, 4 November 1968.

82. Murray, Interview with G. F. Nameche, 4 November 1968; Murray, "Henry A. Murray," pp. 287, 291; and Murray, "What Should Psychologists Do?" p. 153.

83. Murray, "What Should Psychologists Do?" p. 153.

84. *Ibid.*, p. 153.

85. Henry A. Murray, "Physiological Ontogeny. A. Chicken Embryos. X. The Temperature Characteristic for the Contraction Rate of Isolated Fragments of Embryonic Heart Muscle," Journal of General Physiology 9 (1926): 781-788.

86. Henry A. Murray, "Physiological Ontogeny. A. Chicken Embryos. XI. The pH, Chloride, Carbonic Acid, and Protein Concentrations in the Tissues as Functions of Age," Journal of General Physiology 9 (1926): 789-803.

87. Henry A. Murray, "Physiological Ontogeny. A. Chicken Embryos. XII. The Metabolism as a Function of Age," Journal of General Physiology 10 (1926): 337-343.

88. Murray, "Henry A. Murray," p. 291.
89. Henry A. Murray, "What to Read in Psychology?" The Independent 118 (1927): 134; and Henry A. Murray, Personal interview, 14 October 1981.
90. Murray, "What to Read," p. 134; Sigmund Freud, Collected Papers, trans. Joan Riviere (London: Hogarth, 1925); and Jung, Psychological Types.
91. Murray, "What to Read," p. 134; Bernard Hart, The Psychology of Insanity (New York: Macmillan, 1912); William McDougall, Outline of Abnormal Psychology (New York: Charles Scribner's Sons, 1926); Morton Prince, The Unconscious: The Fundamentals of Human Personality, Normal and Abnormal (New York: Macmillan, 1921); Pierre Janet, Névroses et Idées Fixes (Paris: Baillière, 1898; and Pierre Janet, Les Obsessions et la Psychasthénie (Paris: Baillière, 1903).
92. Murray, "What to Read," p. 134; George A. Dorsey, Why We Behave like Human Beings (New York: Harper & Brothers, 1925); and Kurt Koffka, The Growth of the Mind: An Introduction to Child-Psychology, trans. R. M. Ogden (New York: Harcourt, Brace & Co., 1924).
93. Murray, "What to Read," p. 134; C. K. Ogden, The Meaning of Psychology (New York: Harper & Brothers, 1926); C. K. Ogden and I. A. Richards, Meaning of Meaning (New York: Harcourt, Brace & Co., 1923); C. D. Broad, The Mind and its Place in Nature (London: Routledge & Kegan Paul, 1925); and I. A. Richards, Principles of Literary Criticism (New York: Harcourt, Brace & Co., 1928).
94. Murray, Interview with G. F. Nameche, 4 November 1968.
95. Alfred E. Cohn to Alan Gregg, 7 May 1934, Rockefeller Foundation Archives, Series 200, Box 91, Folder 1095, Pocantico Hills, North Tarrytown, N.Y.; and Cohn, No Retreat, pp. 3-10.
96. Cohn to Gregg, 7 May 1934. For a discussion of the events surrounding Murray's call to the position at the Harvard Psychological Clinic, see Chapter 5, pp. 143-145.

CHAPTER 4:
FUNDING THE HARVARD PSYCHOLOGICAL CLINIC:
THE POWER OF MONEY AT HARVARD

Introduction

In Chapter 3 it was pointed out that the professional contacts that Murray had established during his graduate education in medicine and biochemistry were to play a crucial role in his career as a psychologist. One of the most important of these contacts was with Dr. Alan Gregg of the Medical Sciences Division of the Rockefeller Foundation. This relationship had begun during Murray's tenure as an Assistant Member of the Rockefeller Institute for Medical Research from 1923 to 1926. Murray was to later rely heavily upon this relationship when he turned to the Rockefeller Foundation in 1934 for aid for the financially floundering Harvard Psychological Clinic.

Founding the Harvard Psychological Clinic

Dr. Morton Prince was 72 years old when he approached his old friend President A. Lawrence Lowell of Harvard in January of 1926 with his plan to establish the Harvard

Psychological Clinic. As far as most men would be concerned, Dr. Prince had already lived a long, intellectually and professionally fulfilling life. He had received the A.B. from Harvard in 1875 and the M.D. from the same institution in 1879. He served on many Boston civic organizations, and held appointments with the Boston Dispensary and the Department of Nervous Diseases at the Boston City Hospital. However, while he initially had specialized in the diseases of the nose and throat, his interests soon shifted to psychopathology. This new interest was to become the driving force of the bulk of his professional life.[1]

Morton Prince was to become one of the founders of American psychopathology. He formed, along with William James, Josiah Royce, Hugo Münsterberg, Boris Sidis, and James J. Putnam, the nucleus of the informal Boston School of abnormal psychology and psychotherapy.[2] He published six books and scores of scientific papers on neurology and psychopathology, of which The Dissociation of a Personality, published in 1906, still stands as his greatest achievement and a seminal work on the topic.[3] Also in 1906, in response to the growing scientific interest in psychopathology, Prince founded the Journal of Abnormal Psychology, and was actively involved in the editorial supervision of this journal until his semi-retirement from the editorship in 1921.[4]

However much Dr. Prince was involved in his research and writing, he still devoted a large portion of his time to teaching. He was an instructor in neurology at the Harvard Medical School from 1895 to 1898, and was appointed Professor of Nervous Diseases at the Tufts Medical College in 1902, retiring as Professor-Emeritus in 1912. He was invited to give a series of lectures on abnormal psychology at the University of California in 1910, and at the Universities of Oxford, Cambridge, Edinburgh, and London in 1924.[5] Being an active teacher of both medicine and abnormal psychology brought home clearly to Prince the need to separate these two disciplines. He felt that if psychopathology were to be taught solely in the medical schools, the research necessary for the scientific advancement of the discipline would take a back seat to the practical concerns of operating a university hospital psychiatric ward. Instead, he felt that abnormal psychology could only be advanced scientifically in the atmosphere of academic freedom found in the university setting. With this goal in mind, he approached Harvard's President Lowell.[6]

Prince's plan for his "Psychological Clinic," as he related it to Lowell, was threefold:

- . . . A) Research. B) Systematic lectures on abnormal and normal dynamic psychology for undergraduates as a pre-medical course, and other students desiring to take them and graduate students of medicine as a post-medical course.
- C) Education and training of post-graduate students in medical psychology.[7]

The origins of this plan probably date back to 1880. In

that year Prince took his mother, who suffered from an unknown neurotic disorder, to Paris to consult with Jean Martin Charcot, the pre-eminent French psychiatrist. While there Prince had the opportunity to observe Charcot at work in the wards and lecture halls of the Salpêtrière, and to see the impact of dramatic clinical demonstration as a teaching tool. In fact, this experience most likely had a significant influence on initiating his life's work on abnormal psychology.[8]

While it was important to have the goals of the Psychological Clinic worked out before presenting the plan to President Lowell, it was far more important to have the finances for the Psychological Clinic already in hand. Since the early 1800's the fiscal policy at Harvard had been "every tub stands on its own bottom." [9] Academic departments, faculty chairs, and research laboratories were not funded from University monies. Basic expenses had to be covered by the interest from endowments given specifically to the department, chair, or laboratory. Once basic expenses had been covered, then the University would supply the share of the tuition-generated funds one's course enrollments justified. The necessity of having the funds already on hand before initiating any academic project sometimes tended to retard growth and development at Harvard. Particular instances of this occurred during the early years of the Harvard School of Education.[10] However, Prince had support for his planned Psychological Clinic in

the sum of \$75,344.18, later increased to \$140,645.92. Of this sum, \$25,000 came from Prince's brother, Frederick H. Prince, a wealthy Boston Lawyer, and the rest came from several anonymous sources.[11] It seems that while Frederick Prince was not particularly fond of Harvard as an institution, he was even less fond of the Internal Revenue Service, and by financially helping his brother's psychological clinic he would end up with a much desired tax shelter. This opened Harvard's door to Prince and his plans for establishing a psychological clinic devoted to the teaching and scientific investigation of abnormal psychology.

It was not hard for Prince to convince President Lowell to accept his proposal. The two men were old friends. Both belonged to the Boston Brahmin aristocracy. Both were excellent athletes during their undergraduate years at Harvard. Prince, who graduated in 1875, was instrumental in establishing football at Harvard, and Lowell, who graduated in 1877, was an outstanding distance runner.[12] Coming from the same social milieu, they shared similar views on many subjects, one of which was the need for Harvard to be at the forefront of all academic endeavors. Because of this shared view of Harvard's intellectual mission, and because of Prince's skills of persuasion, Lowell readily saw the merits of Prince's plan.

But even more important for Lowell was the fact that the money for the Psychological Clinic was already in hand. Lowell was perhaps the archetypal university fundraiser. During his presidency Harvard's endowment rose from around \$23,000,000 to over \$128,000,000, and this figure does not even include the additional millions of dollars given and expended as unrestricted funds or special purpose gifts.[13] When Lowell took over the Harvard presidency in 1909, the University was not in a sound financial condition. Faculty salaries were distressingly low due to lack of funds, and the expense of operating and maintaining the physical plant only strained the budget further. Instituting new bookkeeping procedures helped the situation somewhat, but nothing could take the place of the never-ending quest for new funds. Lowell carried out the greatest program of building in the history of the University. At this task Lowell excelled, for the University's endowment grew dramatically under his direction.[14]

However, as important as raising money was for Lowell, he maintained a discreet New Englander's reticence about publicizing this aspect of his presidency. It was as if there was something inherently distasteful about a man of learning involving himself with matters of business and finance.[15] This point is corroborated by Professor Nevitt Sanford of the Wright Institute, who recalls that during his student years at Harvard in the 1930's: "I knew that money

was important in New England, the evidence being that it was never talked about." [16]

As one anonymous critic put it, "Morton Prince bought his professorship." [17] Lowell appointed him Professor of Abnormal Psychology for a one year term, and handed the entire matter over to the Department of Philosophy and Psychology for implementation. [18] All this came as quite a surprise to the Harvard philosophers and psychologists, as this was the first that they had heard of the negotiations between Prince and Lowell. A debate ensued between the psychologists Edwin G. Boring and William McDougall, and the philosophers, headed by Alfred N. Whitehead, over how, if at all, Prince's planned teaching clinic could be put into operation. Boring was not convinced that abnormal psychology was a clearly distinct field of study separate from normal psychology and therefore able to support a separate professorship. Whitehead also had several reservations about this project. His first reservation followed Boring's line of reasoning. If the terms of the endowment were too narrowly defined, then the Department might find itself in the position of being unable to live up to the endowment's terms if the discipline of abnormal psychology developed into something substantially different in the future. Whitehead's second reservation was that as chairman of the Department, he felt uncomfortable about accepting the responsibility of overseeing a clinic where psychologically disturbed individuals would be undergoing

treatment. This fear was mostly allayed by the assurance that the Clinic would not compete with the Department of Hygiene in administering to the counseling needs of the student body, and that therapeutic services would be restricted to carefully selected cases of educational value.[19]

The terms of the endowment became the only stumbling block for the creation of the Psychological Clinic. Whitehead recalled a case at London University where a prize fund for scholarly research concerning the reign of King William II became unusable after a century of research exhausted the topic.[20] Also, they were all aware of the outcome of the McKay Bequest Affair of 1904-1917. The inventor Gordon McKay had bequeathed his entire estate, estimated to be worth approximately \$23,000,000, to Harvard for the specific purpose of creating an institute of applied science. President Lowell felt that this goal could best be achieved through a merger with, and transfer of funds to, the young Massachusetts Institute of Technology, thus avoiding wasteful duplication of effort. Several attempts to put this merger into effect were made, generating great emotion both pro and con among the respective faculties and the Boston-Cambridge community. Finally, in November of 1917, the Massachusetts Supreme Court ruled that a Harvard-MIT merger would be illegal, as it would violate the intent of McKay's will.[21] With these precedents in mind,

the Harvard philosophers and psychologists had to make their decision.

Boring, McDougall, Whitehead, and colleagues realized that while some changes would have to be made in the terms of the endowment in order to prevent the Psychological Clinic from becoming a white-elephant like the King William II prize, Prince would have to get virtually everything he wanted from this position. The pressure was on the Department of Philosophy and Psychology since Prince had hinted that he would turn his attentions to Yale if Harvard did not satisfactorily accept his offer. Boring suggested that the phrase "and Dynamic Psychology" be added to the title of Prince's professorship. This added sufficient ambiguity to the endowment, and would allow it to be used for research in other areas, such as human personality, as well as abnormal psychology. This suggestion was agreeable to all the parties involved, and in the Fall Semester of the 1926-1927 academic year the Harvard Psychological Clinic became a reality.[22]

The Clinic was initially housed in two rooms that were borrowed from the Psychological Laboratory on the top floor of Emerson Hall. Emerson Hall was at that time the headquarters of the Department of Philosophy and Psychology. Prince was the sole full-time faculty member, and on the suggestion of Professor Lawrence J. Henderson of the Chemistry Department, and with the approval of the

Department of Philosophy and Psychology, Dr. Henry A. Murray was brought in as Prince's research assistant.[23]

Space was a problem for the Harvard psychologists. For several years course enrollments and faculty research had had to be artificially limited as a result. The formation of the Psychological Clinic now only made matters worse. Space was found in an old, University-owned house at 19 Beaver Street in Cambridge, and the Clinic was relocated to that address. This was a fortunate move, because in 1929, it was learned that the will of the late Edgar Pierce, an 1895 Ph. D. of Hugo Münsterberg, left \$872,802 to the Department of Philosophy and Psychology, stipulating that a portion of the income be used for the development of a psychological laboratory. Had the Clinic not been moved two years earlier, the added pressure to expand the work of experimental psychology would have forced a move at that point.[24]

The years 1927-1929 were marked by great changes at the Clinic. Prince was reappointed to another one year term in 1927, but his failing health forced him to retire as Director of the Clinic in 1928. Henry Murray, after serving two years as Prince's assistant, was appointed as the new Director and promoted to the rank of Assistant Professor. This change of leadership was to have a profound effect on the development of new lines of research at the Clinic, which will be the focus of the next chapter. Due to

Lowell's grand plan for the construction of residential houses for Harvard's students, the Clinic had to be relocated again during the summer of 1929, this time from the house on Beaver street, to more spacious and convenient quarters at 64 Plympton Street in Cambridge.[25] For the immediate future, however, these changes were overshadowed by a national catastrophe that would ultimately threaten the very existence of the Clinic. On October 29, 1929, a panic selling wave swept the New York Stock Exchange. By the end of that day's trading, 16,410,030 shares of stock had exchanged hands, and the total dollar loss was estimated to be in the area of \$30,000,000,000. The Great Depression had begun.

The Clinic Faces Financial Difficulties

The Psychological Clinic had always operated on a shoestring budget. The income from the endowment amounted to over \$6,500 a year. Initially this was enough to cover the Director's salary of \$4,500, leaving \$1,000 for Murray's salary, and over \$1,000 for other expenses. Fortunately for the fiscal solvency of the Clinic, both Prince and Murray were men of independent means, and they paid many of the Clinic's expenses out of their own pockets. As a result, the Clinic's budget was able to effectively exceed the income from its endowment. For example, when Murray became Director of the Clinic in 1928, the Clinic's expenses had risen to the point that he had to use over half of his

salary just to meet these expenses, which included a full-time secretary, books, scholarly journals, research supplies, and pay for research subjects.[26] Also, limited support for the Clinic's small group of graduate students came in the form of one \$1,000-a-year assistantship from the Department of Philosophy and Psychology.[27] However, if the Clinic was to fulfill its stated purpose of training qualified students in abnormal psychology, another source of financial support had to be located in order to help the rest of the Clinic's graduate students through the final years of their education.

Help first came from the Committee on Industrial Fatigue at the Harvard Business School. This group was operating under a grant from the Rockefeller Foundation to study the effects of fatigue on worker productivity, the culmination of their efforts being the 1933 study of the Western Electric Company's Hawthorne Plant at Chicago, Illinois.[28] In 1930, \$2,000 of the Committee's funds were transferred to the Clinic to establish four research fellowships of \$500 each. This support continued through the 1933-1934 academic year. Five graduate students were thus supported during their training at the Clinic: Walter C. Langer, Saul Rosenzweig, Robert N. Sears, Carl E. Smith, and David R. Wheeler.[29] Also, from September 1932 through June 1935, the Committee provided the Clinic with \$2,500 a year for the support of Dr. William G. Barrett as a clinical fellow and half-time instructor.[30]

However, by 1933, the full impact of the Depression was making itself felt on the nation and on Harvard. While millions lost their jobs and life savings in the closings of factories and banks and millions more stood in soup lines, Harvard tightened its belt and struggled on. The Widener Library operated on reduced hours. The athletic budget was drastically cut. Scholarship funds were unable to keep up with the increasing needs of students, and as a response, a \$40,000 surplus in the dining hall budget was used to establish a student employment program. In large part due to the fund-raising abilities of President Lowell, Harvard was in no danger of financial collapse, as were many other institutions of higher education. However, James B. Conant, who succeeded Lowell as Harvard's President in 1933, was incorrect when he claimed that there was "no likelihood of salaries being cut."^[31] At the Clinic, the income from its endowment had dwindled from over \$7,500 to under \$5,600 a year, and the situation showed all signs of worsening. The Department of Philosophy and Psychology had to reduce the amount of the Clinic's one assistantship to \$400 a year. Even worse, the Department's offer to pick up part of Murray's salary out of their unrestricted funds was rejected by the Harvard administration. There was even some concern that the Clinic might have to be transferred to the control of the more financially sound Harvard Medical School if the situation did not dramatically improve. Murray, whose salary had risen to \$5,500 a year as part of a

University-wide program to raise faculty pay, voluntarily took a cut in salary in order to keep their doors open. Even so, virtually nothing was left in the budget to pay the rest of the Clinic's expenses. As a last resort, Murray paid these expenses out of his own pocket.[32] At this point Murray began to explore a number of sources of potential funding.

Murray first appealed for aid to the Department of Neuropathology of the Harvard Medical School. In an unpublished paper entitled "Researches conducted at the Harvard Psychological Clinic during the year 1933," Murray argued the case for financially helping the Clinic. He began the paper by briefly summarizing the origins and aims of the Clinic. Then he devoted the bulk of the paper to a discussion of the general research orientation and specific topics of investigation of the Clinic's ongoing work. The main point of the paper, though, was Murray's plea for financial assistance. After citing the inadequacy of the Clinic's endowment, and its precarious present financial condition, Murray asked for \$5,400 from the Department of Neuropathology. Of this sum, \$1,500 was to go for a secretary's salary, \$700 for supplies, \$300 for paying research subjects, \$400 for books and journals, and \$2,500 for research fellowships.

To support this request, Murray stressed four points. One, he emphasized the positive potential outcome of the combined commitment to research, therapy, and teaching at the Clinic. Two, he pointed out that the theoretical outlook of the work conducted at the Clinic combined traditional academic psychology with the recent advances of medical psychology. Three, mindful of the potential biases of his audience, Murray downplayed his ties to psychoanalytic theory, emphasizing instead his interest in the social-psychological theories of Jean Piaget, Kurt Lewin, and A. R. Luria. Finally, Murray noted the fact that the Clinic was the only place where pre-medical students could learn abnormal psychology from an active practitioner of psychotherapy.[33]

This application was to prove fruitless. Murray had foreshadowed the reason for the rejection in his own proposal when he wrote that, "financial conditions have made it impossible for the University to increase the budget of the Clinic." [34] The financial situation was hitting everyone hard at Harvard. The Department of Neuropathology simply did not have the resources to assist the Clinic, however ably Murray had demonstrated his need. Murray was able to generate some private support for the Clinic through his numerous social and professional contacts, though not enough to meet the deficit.[35] In 1935, Murray made his campaign for private funds public. In an article for the Harvard Alumni Bulletin he wrote:

So many persons have inquired: "The Harvard Psychological Clinic? What sort of place is that? What do you do there? Why is it at the College?" that I am prompted--now that something can be said--to answer publicly in the BULLETIN. Moreover, if by any chance a statement from the Clinic of its function, works, and aims could win the economic backing which it might not be unreasonably desirous of, then such a statement should be made. For, in the hope of eking out an insufficient income, the Clinic has been long enough a beggar at the door of Charity.[36]

Only one record exists of how successful Murray's appeal for private financial support was, and according to it, only \$800 was generated in this manner.[37] However, extensive records do exist of Murray's success in obtaining support from the Medical Division of the Rockefeller Foundation--support that was to insure the survival of the Clinic.

Aid From the Rockefeller Foundation Begins

In turning to the Rockefeller Foundation, Murray had the full support of Boring, and the advantage of being on personal terms with Dr. Alan Gregg, Director of the Medical Services Division. Dr. Gregg had been associated with the Rockefeller Institute for Medical Research while Murray was engaged in embryological research there in 1923-1926.[38] This must have helped to reduce the anxiety that any prospective grant-seeker feels, when Murray first wrote to Gregg on February 6, 1934:

If you knew of a psychological laboratory-clinic where the most original and promising research in this country was being done, and where the best opportunity for pre-medical and

pre-psychiatric training in the elements of normal and abnormal psychology prevailed, and if you also discovered that this laboratory-clinic was the most impoverished organization of its kind in this country, would you be inclined to assist it?

I have been told by seven or more visitors from various parts of Europe and America that the statements contained in the preceding paragraph are precisely and scientifically correct. My own opinion is that they are a bit too modest, but modesty is such an imbedded trait with me that I have always found it difficult to give a proper value to anything with which I am associated.

In dead earnest, I should like to know whether it would be possible for you to visit the Harvard Psychological Clinic some time in the near future. It is a very young thing but I think it has made a good start. Its future, however, is precarious. We are running on a shoestring. . . [39]

No doubt Murray would not have been so immodestly "modest" had it not been for his previous acquaintance with Gregg. To support the request made in his letter, Murray sent to Gregg under separate cover a brief proposal stating the ongoing research and financial needs of the Clinic. Interestingly, this proposal was exactly the same document that Murray had already submitted to the Harvard Department of Neuropathology. [40]

This time, however, the proposal would bear fruit. Murray's success was probably not a result of his personal relationship with Gregg, as all grant requests had to be reviewed and approved by the Foundation Board, thus guarding against personal favoritism as a deciding factor. However, what was important for Murray getting the grant was that he

had the support of some very important people for his work at the Clinic. Evidence for this support came almost immediately in the form of a letter to Gregg from Dr. Alfred E. Cohn, Murray's former mentor at the Rockefeller Institute. On May 7, 1934, Cohn wrote:

Four years of very close cooperation, intellectually at least as intimately as experimentally, assured me that Murray was no ordinary person. He belongs to the order, to me the most admirable in lovers of learning, the disinterested amateur, the private scholar. The wealth of Murray's learning, his sympathy, his insight, his ingenuity, are not the gifts which are to be found often in any walk of life; they seem to me to be extraordinarily rare in our own. These he has been devoting, not to the well worn avenues along which he would no doubt have won certain success, but to those trails through dark woods, the darker because we have chosen to regard them as essentially beclouded, along which he has convinced himself that it was his duty to move. He saw there, with his unusually good and broad training in the methods of natural science, an opportunity, by the use of all his accomplishments, of bringing into focus natural and pathological phenomena and scientific method, out of the welding of which a step in advance of contemporary understanding might issue. But it was not merely a vague and pious eclectic enterprise which suggested activity to Murray; through years of unremitting reflection and of testing, he has come upon psychological conceptions, the analysis of which would, had he the opportunity, demonstrate the value of a method of study. His recent report is evidence of the careful development of his thought.[41]

A more emphatic or effective letter of support it would have been impossible to write.

Gregg's initial response to Murray's request was understandably very favorable, though he noted in his letter to Murray that the first step in approving a grant to the

Clinic would involve an inspection of their facilities. While Gregg looked forward to this visit as a means of renewing his acquaintance with Murray, he could not say definitely when he would have the time for a trip to Harvard.[42] Finally, Gregg was able to propose April 6, 1934, as the date of his visit, much to the distress of Murray, who had planned to be out of town on that date. In desperation, Murray wrote a hurried, hand-written note to Gregg stating that:

I am so anxious to see you that I shall gladly rearrange my schedule in anyway that is necessary. It happens that April 6th is about the worst day you could have chosen. In the first place it is Easter Vacation at Harvard, & the men here will be home writing papers rather than experimenting in the laboratory. In the second place I had planned to spend that week in New York with my mother who is very much depressed at this moment--my father having just died. I don't feel that I can leave her alone. What can be done? Can you stay over until Monday, April 9th? Or can you see me in New York? The condition of the Clinic is rather desperate, & I don't want to leave a stone unturned, so that I shall stay here if you cannot possibly change.[43]

Fortunately for Murray, Gregg was able to reschedule the visit.

As it turned out, there was no need for Murray to have been so anxious about showing off the Clinic to Gregg. Any decision about funding the Clinic would have to be postponed until the fall of 1934, as President Conant was in the process of reviewing the organization of the Department of Philosophy and Psychology. Depending on what changes, if any, were to be made in the organization of the Department,

the Clinic's official status, and hence need for support, could be drastically altered.[44] With this in mind, Gregg made his first visit to the Clinic on April 27, 1934, recording his impressions later that night in his diary. After a fairly lengthy discussion of the Clinic's facilities and Murray's general research plans, Gregg made the following comment on the Clinic's financial condition:

Endowment left by Dr. Prince provides now for only a small part of the expenditures of the Inst., i.e. Murray's salary (reduced) plus bal. of \$118. . . . Urgent need will arise next year.[45]

Despite the urgency of the need, Gregg could only advise Murray to sit tight and await the outcome of Conant's departmental review.

Conant's decision was to be characteristically slow in coming. The independent review committee appointed by Conant during the first weeks of his Presidency was not to meet until October of 1934, and no report was expected until November at the earliest. No change could therefore be expected in the Clinic's departmental or financial status until the fall of 1935, and it was obvious that the Clinic might not be able to financially survive that long on its endowment. With this in mind, and at the urging of Dr. Stanley Cobb, Professor of Neuropathology at Harvard and a friend of both Murray and Gregg, Gregg decided to act on Murray's request for aid.[46]

While inclined to financially support Murray's Clinic, Gregg felt that the Clinic's researchers would have to support themselves at least for the immediate future. Murray responded to this suggestion by arguing forcefully for aid for his colleagues and graduate students. In a letter dated September 26, 1934, Murray wrote to Gregg that:

You said last spring that you were not interested to hear about the research work. I did not quite understand what you meant by that. Why shouldn't financial help be given to those experimenters who have the most promising researches in mind, and have developed the best methods of pursuing them? There are a multitude of first-rate psychological or psychiatric laboratories in America which are organized up to the nines. On paper, they seem to be first-rate; they seem to be working in conjunction with related departments, etc. But they are failures because no effective imaginations inhabit them.[47]

Gregg remained unconvinced on this issue, though. The Rockefeller Foundation had not at that time clearly defined its approach to the field of psychology, and therefore could consider only a short-term grant. Also, since the fellowship money from the Committee on Industrial Fatigue was to continue through June, 1935, no further consideration of this issue would be necessary until then.[48]

Gregg made a second visit to Harvard on October 3-4, 1934. This time his purpose was twofold: to press Conant for a decision on the Clinic's official University status, and discuss with Murray the limited nature of any possible support for the Clinic. In regard to his first purpose, Gregg was not successful. While Conant had no objections to Rockefeller Foundation support for the Clinic, he had no

comment about his plans for the Clinic's future at Harvard. Gregg's visit with Murray was more productive, however. Not only was Murray convinced of the rationale of limiting the amount of support he could expect from the Rockefeller Foundation, but he was also able to impress upon Gregg even more convincingly the urgency of the Clinic's financial situation.[49]

Based on the information gathered during Gregg's visit to Harvard, the Board of the Rockefeller Foundation acted quickly. On October 19, 1934, the following proposal was considered and approved:

GRANT-IN-AID, NEW YORK

OBJECT-Purchase of scientific apparatus and equipment for psychological testing, and compensation for special statistical and secretarial services in connection with the research in normal and abnormal psychology of Doctor Henry A. Murray, Jr., and his associates in the Psychological Clinic of Harvard University at Cambridge, Mass.

AMOUNT-\$2,450

FORM OF PAYMENT-Treasurer of Harvard University; \$1,000 as of November 1, 1934, and balance upon presentation of bills or salary expenses under headings noted herewith. . .

A grant of \$2,450 to the Psychological Clinic of Harvard University as a contribution to the purchase of scientific apparatus and equipment, and the special expenses of statistician and secretarial service is approved. This grant to be made available from November 1, 1934; any unexpended balance as of October 31, 1935, to be returned to the Foundation. Equipment purchased to be considered property of the Psychological Clinic of Harvard University.[50]

The document was signed by Alan Gregg, Director of Medical

Services; George J. Beal, Comptroller of the Rockefeller Foundation; and Max Mason, President of the Rockefeller Foundation.

The particulars of the Board's decision were appended to the above document. As supporting evidence for their decision, the Board noted that:

Dr. Stanley Cobb, Dr. Alfred Cohn and Dr. L. J. Henderson all have spoken of Dr. Murray and his work in terms of unusual earnestness and approval, and successive visits to the clinic show that it is composed of a group of workers within the field of psychology whose breadth of training and viewpoint, whose sincerity and modesty and whose common sense are not to be found easily elsewhere.[51]

Also, the financial need of the Clinic was cited along with the specific itemization of the grant's budget as follows:

The Clinic was endowed by the late Dr. Morton Prince. Previously the income was \$6,900 annually. It is now \$5,500. Dr. Murray's salary was fixed at \$5,000, but of this he has used \$3,000 annually for routine expenditures during the past seven years. He cannot afford to support the clinic further to this extent. . . . The principal financial difficulty is to bear the cost of necessary equipment and running expenses during the current academic year.

The utilization of aid proposed is as follows:

Expenses of installation of sound recording room and apparatus	\$1,000	
Purchase of apparatus for measuring skin temperature and muscular tremor	450	
Equipment for children's clinics and cost of intelligence and aptitude tests	400	
Employment of statistical and secretarial assistance	600	

	\$2,450	[52]

With this grant, a long relationship between the Harvard Psychological Clinic and the Rockefeller Foundation began.

The Harvard Psychological Clinic was to be, except for the small income from the Prince endowment, totally funded by the Rockefeller Foundation until 1945. In that year the Harvard social psychologists split away from the Psychology Department to join with Harvard's cultural anthropologists and empirical sociologists in forming the Department of Social Relations. The Clinic became part of this new department and shared in its endowment and University funding.[53] For the present, however, only the funding of the Clinic through 1938 will be considered.

Murray was understandably overjoyed to learn of the Rockefeller Foundation grant of October 19, 1934. But in writing Gregg to officially accept the award, Murray was already thinking about the future needs of the Clinic:

I was of course delighted to receive your letter informing me of the grant-in-aid which was voted to the Psychological Clinic. . . This aid will make it possible for us to carry on our work this year and bring our preliminary studies to a close. . .

In our conversation you suggested that you might be willing to propose to your Committee that a grant for one year, with the possibility of renewal for two more years, amounting for about \$8,000, might be awarded the Clinic. . . After hearing of your willingness to help us this year, I am hesitant to push the other matter, but the situation is now as it was when you expressed your willingness to put our case before the Committee.[54]

But, with Gregg's approval, push this matter he did, and with greater monetary success than before.

At the March 25, 1935, meeting of the Board of the Rockefeller Foundation it was noted that:

No material change in the financial situation has occurred since the [initial] grant was made. It is now certain that the annual contribution of \$4,500 from the Committee on Industrial Fatigue will not continue after June, 1935, and Doctor Murray has been informed by the Director of the Department of Education that his request for funds from the university has had to be declined. Thus, the only assured income for 1935-1936 is the \$5,500 yielded by the Prince endowment and which provides Doctor Murray's salary. Doctor Murray will continue to contribute \$3,000 of this to the expenses of the clinic, but he needs in addition at least \$6,000, of which \$3,000 would go for his running expenses, secretarial assistance, apparatus, supplies, etc., and \$3,000 for part-time staff and research workers.[55]

In response to this demonstration of need, the Board approved a grant of \$5,000 to the Clinic for the 1935-1936 academic year. This time the grant included funds for the payment of research assistants.[56]

This grant, though, was for one year only, and did not include the extension clause that Murray had originally sought. This meant that a whole new grant proposal would have to be made again the following year. The reason for the Rockefeller Foundation's refusal to provide long-term support was quite simple. Harvard's President Conant had still not decided upon the future of the Clinic. It was decided that the Foundation would consider no further aid to the Clinic until Conant made his decision.[57] This

information was passed on to Conant by Gregg. Conant responded on April 24, 1936, by writing to Gregg that:

I should like to have the decision in regard to the future of Harry Murray put off for another year. . .

In view of these circumstances, I should greatly appreciate it if the Rockefeller Foundation were willing to continue for one more year on the same basis the support of the Psychological Clinic; and this letter may be regarded as a formal application to you for such support.[58]

While this could hardly have been the desired response from Conant, Gregg and his colleagues at the Rockefeller Foundation knew that a decision on their part would have to be made nonetheless. Three weeks prior to Conant's letter, Murray had paid a visit to the Rockefeller Foundation offices in New York. He was apprised of the position the Foundation was taking towards the granting of further support. Murray responded by predicting that the University would allow the Clinic to die if the grant was not continued. He added further that he would ask for no more aid after the 1936-1937 academic year if Conant had still not made his decision about the Clinic.[59] Faced with these alternatives, the decision was made on May 15, 1936, to extend for another year the \$5,000 grant to the Clinic. In an appendix to the grant award it was noted that: "No commitment for future aid is implied," but that if Conant decided in favor of permanently establishing the Clinic at Harvard, "a request for aid over a period of years will be presented in 1937." [60] This secured the existence of the

Clinic for the immediate future, and put the pressure on Conant to finally make his decision.

The fate of Murray and the Clinic was finally decided in March of 1937. Murray was promoted to the rank of Associate Professor but was not tenured. Instead, he was appointed to a ten year term, with an administrative review of his record in five years. Tenure was not denied, but merely delayed. This final recognition of academic standing and professional security was at last bestowed on Murray in 1948, when he was advanced to the rank of full Professor.[61] Murray now set as his primary goal for his sabbatical the completion of the manuscript of a book based on the work he had been directing at the Clinic since Prince's retirement. This manuscript was to become the Explorations in Personality published in 1938. In order that Murray might finish this manuscript, Conant agreed to give Murray a sabbatical and leave of absence for two years without pay.[62]

With Murray gone for the 1937-1938 and 1938-1939 academic years, the financial need of the Clinic was significantly changed. The \$2,000 a year that Murray kept from the income of the Prince endowment as his salary could now be used to help defray the operating expenses of the Clinic. The main need now was for the hiring of someone to carry on the new research program that Murray recently initiated, as he planned to spend most of his sabbatical in

Europe. Preferably, this person was to be a young, energetic researcher familiar with psychoanalytic theory and methods, and would be hired at the level of an instructor or research assistant. Dr. Walter Dyk, formerly of the Boston Children's Hospital and the Boston Department of Public Health, was chosen by Murray for this position. Murray made a request of the Rockefeller Foundation to provide a \$2,500 a year fellowship for Dyk for the two years that Murray would be on sabbatical, and the Foundation agreed to do so. Now, with Dyk in charge of the Clinic's new research program, and Robert W. White, a 1937 Ph.D. from the Clinic, acting as the Clinic's chief administrative officer, Murray was free to take his leave.[63]

In the last letter that Murray sent to Gregg before leaving on his sabbatical, he wrote concerning the backing that the Clinic had received from the Rockefeller Foundation. In this letter he stated:

. . .how grateful we are for past benefits. A good part of our work would have been impossible without your backing and financial aid.[64]

It would be impossible to state the situation at the Clinic more concisely or more accurately. The total amount of aid the Clinic received from the Rockefeller Foundation from 1934 to 1939 was \$17,450.[65] Without this aid the Clinic would not have been able to function on the income from the Prince endowment for very long past 1934. More important though, was the non-monetary support that Murray received from Alan Gregg. It will be shown in the next chapter that

it was Gregg's personal support for Murray's work that secured his promotion to associate professor, and along with it the continued existence of the Psychological Clinic at Harvard.

Conclusion

Two very important issues are raised in this discussion of the establishment and financial situation of the Harvard Psychological Clinic. One issue is that of Boring's and President Conant's reluctance to support research in applied psychology at Harvard. This would prove to be a major stumbling block in the way of Murray's promotion to Associate Professor in 1937, and will be discussed in detail in Chapters 7 and 8.

The second issue deals with the role played by Murray's professional contacts in securing his position at Harvard. Lawrence J. Henderson was instrumental in obtaining a position at Harvard for Murray, first as Prince's assistant, and later as his successor. This point will be discussed further in Chapter 5. Henderson's Industrial Fatigue Research Group also provided vital financial support for the Clinic's operations during 1930-1934. Another important contact was Dr. Stanley Cobb of Harvard. Cobb was important as an influential reference in Murray's efforts to obtain funding from the Rockefeller Foundation, and he would later prove to be a major supporter of Murray during the

1936-1937 promotion and tenure review. Finally, Murray's greatest ally was Dr. Alan Gregg of the Medical Sciences Division of the Rockefeller Foundation. His support of Murray's work guaranteed the financial survival of the Clinic. Furthermore, as shall be detailed in Chapter 7, Gregg was to use his financial influence to convince President Ccnant to promote Murray to Associate Professor in 1937.

Reference Notes

1. Nathan G. Hale, "Introduction," in Psychotherapy and Multiple Personality: Selected Essays, ed. N. G. Hale, Jr. (Cambridge, Mass.: Harvard University Press, 1975), pp. 1-2; Henry A. Murray, "Dr. Morton Prince: A Founder of Psychology," Harvard Alumni Bulletin, 23 January 1930, pp. 491-492; Henry A. Murray, "Morton Prince: Sketch of his Life and Work," Journal of Abnormal and Social Psychology 52 (1956): 291-292; Morton Prince to A. Lawrence Lowell, 26 January 1926. A copy of this letter is in the Gordon W. Allport Papers, Harvard University Archives, Nathan Marsh Pusey Library, Harvard University, Cambridge, Mass.; Abraham A. Roback, "Morton Prince, 1854-1929," American Journal of Orthopsychiatry 10 (1940): 177-178; and E. W. Taylor, "Morton Prince, M.D., LL.D.: 1854-1929," Archives of Neurology and Psychiatry 22 (1929): 1031-1033.

2. Hale, "Introduction," p. 4.

3. Hale, "Introduction," pp. 5-11, 16; Otto M. Marx, "Morton Prince and the Dissociation of a Personality," Journal of the History of the Behavioral Sciences 6 (1970): 120-130; Murray, "Dr. Morton Prince," pp. 492-493; "Morton Prince: Sketch," pp. 291-293; and Taylor, "Morton Prince," pp. 1033-1034.

4. Gordon W. Allport, "The Journal of Abnormal and Social Psychology: An Editorial," Journal of Abnormal and Social Psychology 33 (1938): 3-10.

5. Murray, "Dr. Morton Prince," p. 493; "Morton Prince: Sketch," p. 293; Roback, "Morton Prince," p. 178; and Taylor, "Morton Prince," p. 1031.

6. Murray, "Dr. Morton Prince," p. 495; "Morton Prince: Sketch," p. 295; and Taylor, "Morton Prince," p. 1035.

7. Prince to Lowell, 26 January 1926.

8. Hale, "Introduction," p. 2; Murray, "Morton Prince: Sketch," pp. 291-292; and Henry A. Murray, Personal interview, 14 October 1981. Privately, Murray suggests that Charcot became a hero-figure for Prince, and Prince always spoke glowingly of Charcot and the Salpetriere, and even attempted to imitate Charcot's style of lecturing.

9. James E. Conant, My Several Lives: Mercis of a Social Inventor (New York: Harper & Row, 1970), p. 126.

10. Arthur G. Powell, The Uncertain Profession: Harvard and the Search for Educational Authority (Cambridge, Mass.: Harvard University Press, 1980), pp. 149-205.

11. Edwin G. Boring, Psychologist at Large (New York: Basic Books, 1961), pp. 43-44; A. Lawrence Lowell, "Report of the President," in Reports of the President and The Treasurer of Harvard College: 1925-26 (Cambridge Mass.: Harvard University, 1927), p. 33; "Report of the President," in Reports of the President and the Treasurer of Harvard College: 1926-27 (Cambridge, Mass.: Harvard University, 1928). p. 33; A financial audit of the Clinic's endowment for the years 1926-1936 can be found in an undated mimeographed document in the Gordon W. Allport Papers. The three preceding references are located in the Harvard University Archives.

12. Hale, "Introduction," p. 17; Murray, "Morton Prince: Sketch," p. 291; Henry A. Murray, Personal interview, 13 July 1981; Roback, "Morton Prince," p. 177; and Henry A. Yeomans, Abbott Lawrence Lowell: 1865-1943 (Cambridge, Mass.: Harvard University Press, 1948), pp. 39-42.

13. Yeomans, Lowell, p. 245.

14. Samuel E. Morison, Three Centuries of Harvard: 1636-1936. (Cambridge, Mass.: Harvard University Press, 1936), pp. 449-450; and Yeomans, Lowell, pp. 219-244.

15. Yeomans, Lowell, p. 233.

16. Nevitt Sanford, "Reminiscences and Celebrations," Paper read as part of the symposium "Explorations in Personality Forty Years Later," Rae Carlson, Chairman, Annual Convention of the American Psychological Association, Toronto, Canada, 31 August 1978, p. 3. A transcript of this symposium can be found in the Alan Elms Record, M1045, Archives of the History of American Psychology, University of Akron, Akron, Ohio.

17. *Ibid.*, pp. 3-4.

18. Lowell, "Report of the President, 1925-26," p. 32.

19. Boring, Psychologist, p. 44; Henry A. Murray, "Professor Murray Describes Department of Abnormal Psychology," Harvard Crimson, 12 January 1929, p. 4; and Henry A. Murray, Personal interview, 3 August 1981.

20. Boring, Psychologist, p. 44.

21. Morison, Three Centuries of Harvard, p. 471; and Yeomans, Lowell, pp. 259-271.
22. Boring, Psychologist, p. 44.
23. Edwin G. Boring to Karl S. Lashley, 8 April 1937, the Edwin G. Boring Papers, Harvard University Archives.
24. Edwin G. Boring, "The Psychological Laboratory," in Reports of the President and the Treasurer of Harvard College: 1925-1926 (Cambridge, Mass.: Harvard University, 1927), p. 240; "The Psychological Laboratory," in Reports of the President and the Treasurer of Harvard College: 1926-1927 (Cambridge, Mass.: Harvard University, 1928), p. 244; A. Lawrence Lowell, "President's Report," in Report of the President: 1929-1930 (Cambridge, Mass.: Harvard University, 1931), p. 31. The above sources are located in the Harvard University Archives; Also, Henry A. Murray, "The Harvard Psychological Clinic," Harvard Alumni Bulletin, 25 October 1935, p. 142; and Murray, Personal interview, 14 October 1981.
25. Edwin G. Boring, "The Psychological Laboratory," in Reports of the President and the Treasurer of Harvard College: 1928-1929 (Cambridge, Mass.: Harvard University, 1930), p. 240; Edwin G. Boring to William McDougall, 27 January 1927, in the Edwin G. Boring Papers. Both of the above sources are in the Harvard University Archives; Morison, Three Centuries of Harvard, pp. 445-446; Jennings B. Sanders, A College History of the United States, Vol. 2 (Evanston, Ill.: Row, Peterson & Co., 1962), pp. 372-374; and Yeomans, Lowell, pp. 165-198, 219-229.
26. Alan Gregg, "Memorandum of Interview with Dr. Henry Murray, Psychological Clinic, Harvard College, October 4, 1934," Rockefeller Foundation Archives, Series 200, Box 91, Folder 1095, Pocantico Hills, North Tarrytown, New York.
27. Edwin G. Boring to Henry A. Murray, 16 January 1931; and Edwin G. Boring to Henry A. Murray, 29 September 1931. Both of these sources are in the Edwin G. Boring Papers, Harvard University Archives.
28. Elton Mayo, The Problems of Industrial Civilization (New York: Macmillan, 1933).
29. Henry A. Murray, "Researches conducted at the Harvard Psychological Clinic during the year 1933," Harvard University Archives.
30. Ibid.

31. Conant, My Several Lives, pp. 115-118.
32. Edwin G. Boring to Henry A. Murray, 8 January 1932; Edwin G. Boring to Henry A. Murray, 18 April 1932; Edwin G. Boring to Henry A. Murray, 2 March 1934. The above three sources are in the Edwin G. Boring Papers, Harvard University Archives, and Alan Gregg, "Diary, Friday, April 27, 1934," Rockefeller Foundation Archives, Series 200, Box 91, Folder 1095.
33. Murray, "Researches conducted at the Harvard Clinic, 1933."
34. *Ibid.*, p. 1.
35. *Ibid.*, pp. 22-25.
36. Murray, "The Harvard Psychological Clinic," pp. 142-143.
37. Henry A. Murray to Edwin G. Boring, undated (probably sometime in April of 1934), in the Edwin G. Boring Papers, Harvard University Archives.
38. Edwin G. Boring to Henry A. Murray, 29 January 1934, in the Edwin G. Boring Papers, Harvard University Archives; "Alan Gregg Dies; Medical Leader," New York Times, 21 June 1957, p. 25; "Public Health Statesman," Time, 26 November 1956, p. 50; and M. C. Winternitz, "Alan Gregg, Physician," Science-126 (1957): 1279.
39. Henry A. Murray to Alan Gregg, 6 February 1934, Rockefeller Foundation Archives, Series 200, Box 91, Folder 1095.
40. Henry A. Murray, "Researches conducted at the Harvard Psychological Clinic during the year 1933," Rockefeller Foundation Archives, Series 200, Box 91, Folder 1095.
41. Alfred E. Cohn to Alan Gregg, 7 May 1934, Rockefeller Foundation Archives, Series 200, Box 91, Folder 1095.
42. Alan Gregg to Henry A. Murray, 16 February 1934, Rockefeller Foundation Archives, Series 200, Box 91, Folder 1095.
43. Henry A. Murray to Alan Gregg, 27 March 1934, Rockefeller Foundation Archives, Series 200, Box 91, Folder 1095.
44. Alan Gregg, "Diary, 27 April 1934," Rockefeller Foundation Archives, Series 200, Box 91, Folder 1095.

45. Ibid.
46. Edwin G. Boring to James B. Conant, 10 May 1934, in the Edwin G. Boring Papers; and Stanley Cobb to Alan Gregg, 24 September 1934, Rockefeller Foundation Archives, Series 200, Box 91, Folder 1095.
47. Henry A. Murray to Alan Gregg, 26 September 1934, Rockefeller Foundation Archives, Series 200, Box 91, Folder 1095.
48. Gregg, "Memorandum of interview with Murray, 10/4/34."
49. Alan Gregg, "Diary, October 3, 1934;" and "Diary, October 4, 1934," Rockefeller Foundation Archives, Series 200, Box 91, Folder 1095.
50. "Grant-in-aid, New York," Rockefeller Foundation Archives, Series 200, Box 91, Folder 1095.
51. Ibid.
52. Ibid.
53. Boring, Psychologist, p. 67.
54. Henry A. Murray to Alan Gregg, 5 November 1934, Rockefeller Foundation Archives, Series 200, Box 91, Folder 1095.
55. "Grant-in-aid, New York," Rockefeller Foundation Archives, Series 200, Box 91, Folder 1095.
56. Ibid.
57. Ibid.
58. James B. Conant to Alan Gregg, 24 April 1936, Rockefeller Foundation Archives, Series 200, Box 91, Folder 1096.
59. Robert A. Lambert, "Diary, Thursday, April 2, 1936," Rockefeller Foundation Archives, Series 200, Box 91, Folder 1095.
60. "Grant-in-aid, New York," Rockefeller Foundation Archives, Series 200, Box 91, Folder 1095.
61. Hiram Haydn, "Portrait: Henry A. Murray," American Scholar 39 (1970): 127; and Edwin S. Shneidman, "Chronology," in Endeavors in Psychology: Selections from the Peronology of Henry A. Murray, ed. E. S. Shneidman (New York: Harper & Row, 1981), pp. 615-616.

62. Henry A. Murray to Alan Gregg, 23 March 1937, Rockefeller Foundation Archives, Series 200, Box 91, Folder 1097.

63. Murray to Gregg, 23 March 1937; Henry A. Murray to Robert A. Lambert, 10 May 1937; and George W. Gray to Robert W. White, 30 November 1937. All the above material is located in the Rockefeller Foundation Archives, Series 200, Box 91, Folder 1097.

64. Henry A. Murray to Robert A. Lambert, 18 May 1937, Rockefeller Foundation Archives, Series 200, Box 91, Folder 1097.

65. George W. Gray, "Sifting the Ways of Psychoanalysis," Rockefeller Foundation Archives, Series 200, Box 91, Folder 1097. This manuscript was privately published in a newsletter distributed to the Trustees of the Rockefeller Foundation.

CHAPTER 5:

CONTROVERSY AND THE CLINIC, 1926-1936:

THE INSTITUTIONAL ISSUES OF HIRING, TEACHING, AND RESEARCH

Introduction

Parallel to the Clinic's struggle for financial survival, another drama was shaping up concerning Murray's professional credentials for holding an academic position in psychology. The fact that Murray, who had absolutely no formal training in academic psychology, was appointed to a major position at Harvard raises three important issues. The first deals with the role played by President Lowell of Harvard University in making faculty appointments and setting departmental policy. In particular, were these decisions based on the merit of the job candidate or policy proposal, or were contacts within the "old boy" network the crucial factor? In contrast to this were Boring's attempts to reform psychology at Harvard by emphasizing the importance of professional credentials and academic merit as measured by research productivity as the criteria for faculty hiring, promotion, and tenure. Finally, the issue of departmental power politics is analyzed in terms of

Boring's efforts to shape psychology along pure experimentalist lines in the face of administrative apathy and opposition.

The Imperial Presidency at Harvard

President Lowell accepted Morton Prince's offer of an endowment to create the Harvard Psychological Clinic without consulting anyone about the proposal. Furthermore, Lowell consulted with no one before appointing Prince as the first director of the Clinic. The initial arrangement to create the Clinic and hire Prince was the result of a verbal agreement between Prince and Lowell that was concluded during the first few days of January, 1926. This verbal agreement was then ratified almost immediately when Lowell presented the plan to the Harvard Corporation on January 4, 1926.[1] It is significant to note that it was not until three weeks later, on January 26, that Prince officially communicated to Lowell the conditions of the endowment, and not until three weeks after that, on February 19, that Prince transferred the endowment from his bank account to the Harvard Treasurer's Office.[2]

It was commonplace that Harvard presidents had this much autonomy from bureaucratic red tape in running the affairs of the University. For example, also in 1926, Lowell obtained a gift in excess of \$10,000,000 from Edward S. Harkness to build residential houses for all Harvard

undergraduates simply because he could give Harkness an immediate yes or no answer to the offer. Harkness had initially offered the gift to his alma mater, Yale University, but had turned instead to Harvard when informed by Yale's President James R. Angell that the gift could not be accepted until approved by the Yale Board of Trustees and by the faculty.[3] Harkness was apparently a very impatient man, and Lowell was able to use this character trait to his, and Harvard's, advantage. Yet another case illustrates the near-legendary autonomy of the Harvard Presidency in the 1920's and 1930's. In the summer of 1933, President-elect James B. Conant visited Oxford and Cambridge Universities in England in order to get some background knowledge of the educational system that had served as the model of his predecessor's institutional reforms. At one point during his visit, he was greeted by an English colleague who exclaimed:

Oh, you are the new president of Harvard; I understand that you will be all-powerful and, unlike an English vice chancellor, can do exactly what you like. If you have the authority I am told you have, I give you one piece of advice: Draw your salary and stay right here in Europe![4]

At Harvard, when the President made a policy decision, the decision technically had to be approved by two separate bodies: the Corporation and the Board of Overseers. The approval of the President's decisions by the Corporation was virtually guaranteed. The Corporation was comprised of only six members plus the President, and since it was empowered

to fill its own vacancies no chance existed of the status quo being disrupted. Approval of the President's decisions by the Board of Overseers was nearly as certain. Though the Board of Overseers was a much larger and more politically diverse body, its chartered role of providing advice and consent was largely short-circuited by the rapidity with which the President and Corporation could make and implement policy. In essence, then, the Board of Overseers was little more than a rubber stamp for the decisions of the President and Corporation, much to the consternation of many members of the Board. This is a state of affairs that has continued virtually unchanged up to the present day.[5] So, while something of an overstatement, the remark of the English Don to Conant is an indication of the feeling of awe that the rest of academia had for the power of the Harvard Presidency.

Naturally, the hiring of Prince and the formation of the Harvard Psychological Clinic by order of President Lowell was not particularly well received by the Department of Philosophy and Psychology. There was some debate within the Department as to whether the Clinic's endowment could even be accepted under the terms of its offer. Eventually, a compromise was achieved that allowed the Clinic to begin operation in the fall of 1926.[6] Boring, in his autobiography, glossed over the fact that the plan to create the Clinic originated without the knowledge or consent of the Department of Philosophy and Psychology, claiming

instead a major role for himself in its founding. In 1960 he wrote:

It is interesting to see the deliberate forces of History working here through the wills of Prince and me. Dynamic psychology was getting itself born near the turn of the century, when Freud in Vienna was discovering the role of the unconscious and Kulpe in Wurzburg the imagelessness of thought. Prince's act was designed to help bring the two trends of psychology together, just as Janet had tried to do in Paris back in 1895. Prince helped History along, but he was also a consequence of History's inevitability. By the 1920's many psychologists were beginning to think along similar lines, especially those who wanted psychology to do more about motivation of "human nature" or what was called personality. Similarly I thought that I was making a novel suggestion about the addition of the word dynamic to the deed of the gift, whereas I too was being little more than History's agent.[7]

Regardless of the reasons for the Clinic's existence at Harvard, the fact remained for all concerned that it did exist and that Prince, the psychologists, and the philosophers would all have to cooperate if the venture was to succeed.

Once the Clinic had been officially established, Prince's first order of business was to appoint an assistant to help him with administrative, research, and teaching duties. While Prince had retired from teaching psychopathology at the Tufts University Medical School in 1912, he had kept in touch with that institution in the meantime. However, he knew of no one from there whom he could call on to be his assistant. All his former colleagues and students from Tufts had gone on into medical

practice, and as he wanted the Clinic to focus on research rather than practice, none of these people seemed appropriate.[8] Seeing that Prince was having difficulty in choosing an assistant, Professor Lawrence J. Henderson, Harvard's preeminent biochemist and also a close friend of President Lowell, suggested that Lowell recommend to Prince the name of Dr. Henry A. Murray.[9]

By all the standard criteria of faculty appointments, Murray appeared to be an unlikely candidate for the position. He was an M.D., his M.A. was in Biology, and his Ph.D. was in Biochemistry. He was, however, a close friend of Henderson. Murray's friendship with Henderson had begun in 1920 when he assisted in Henderson's research on the chemical make-up of the blood, and had continued via Henderson's frequent visits to the Rockefeller Institute for Medical Research in New York City.[10] Based on their personal relationship, Henderson felt confident to make the proposal to Lowell that Murray be appointed as Prince's assistant. Lowell accepted Henderson's suggestion, and invited Murray to come up to Harvard from New York for an interview. During the interview, Murray impressed everyone with his personal charm, wit, knowledge of psychology, and background in scientific research. Everyone, that is, except Prince. Prince liked Murray as a person, but was quite critical of psychoanalysis, and felt that Murray was far too psychoanalytically oriented to be able to fit in with the program of instruction and research that he had

planned for the Clinic. But Prince was outvoted by the rest of the Department who felt that Murray's background in experimental research would bring a needed element of scientific rigour into the Clinic's program.[11] So with this matter settled, the Clinic began operation.

The Clinic Stagnates Under Prince: 1926-1928

From the very start, the relations between the Clinic and the rest of Harvard's psychologists were marred by controversy. First came the issue of how the limited space on the top floor of Emerson Hall was going to be shared by the Clinic and the Psychological Laboratory. The conditions of the endowment specified that the Clinic be given two rooms for its own use, and that it have full access to the facilities of the Psychological Laboratory.[12] This put quite a strain on the already over-extended facilities for psychology. For the past several years Boring had been continually complaining about the fact that course enrollments and ongoing research had had to be artificially limited due to lack of classroom and laboratory space.[13] What brought the matter to a head, though, was Prince's insistence that the two rooms that had been given over to the Clinic were totally inadequate. Prince felt that much more space was needed if the Clinic was to fulfill its stated purpose. He went as far as to suggest that the Clinic needed at least six to eight rooms, and looked upon Boring and the rest of the Harvard psychologists as being

ungenerous in providing only the space stipulated by the Clinic's endowment.[14]

Prince's teaching also proved to be controversial. He based his lecture style on that of the French neurologist, Jean Martin Charcot, using classroom demonstrations of hypnotic phenomena and of patients suffering from psychological disturbances to illustrate the various topics under discussion. However, Prince's demonstrations proved to be far less dramatic than Charcot's. A former patient of Prince's was summoned from her new home in St. Louis to return to Cambridge in order for Prince to display her split personality to the class. Having put a great deal of time and effort into helping her integrate her personalities into a functioning unit, Prince was understandably reluctant to have her break completely "apart" again. So he would have her dissociate only slightly during class, but not enough to provide an effective demonstration.[15] Aside from the ineffectiveness of his demonstrations, Prince's teaching drew substantial criticism for other reasons. He was becoming rather forgetful in his old age, and would frequently repeat the same points over and over again in class unless some student was able to muster the courage to interrupt him and point out the situation. Also, Prince always arrived late for class, and would compensate for this by holding the class long past its scheduled time. The students found Prince to be poorly organized and unprepared for class, and they felt he devoted an inordinate amount of

attention to minor details in the material he was covering.[16] As a result, the class, which began with an enrollment of eighty students, was down to only twenty students by the end of the semester.[17]

A certain amount of friction also arose from the contrast between Prince and Murray's manner of professional conduct and that of Boring and the rest of the Department's psychologists. Prince and Murray were members of the wealthy elite, while their psychology colleagues were men who had worked their way up the academic ladder from humble beginnings. There was a certain degree of resentment for the way Prince and Murray openly lived "the good life." Also, they tended to flaunt their status as medical doctors around the Department. Prince and Murray made a point of always wearing their white doctor's coats whenever they were in Emerson Hall, a gesture that for some unknown reason did not sit well with the rest of the psychologists.[18]

Furthermore, there was some concern that Prince and Murray were not taking their professional academic duties seriously enough. Prince, complaining of poor health, showed up only to teach his class and did not engage in any research. Murray frequently was not around at all, and had a bad habit of forgetting to show up for appointments, even when they had been made well in advance. However, some hope was held out for Murray since he at least was actively

pursuing several research problems and had several graduate students who were enthusiastic about working with him.[19]

What was perhaps the most troubling thing about the Clinic for the rest of the Department, though, was Prince's habit of bypassing established Departmental procedures and going straight to President Lowell whenever he wanted anything for the Clinic.[20] As far as Boring, Whitehead, and the rest of the Department of Philosophy and Psychology were concerned, this constituted a direct violation of the conditions under which the endowment for the Clinic had been accepted. The conditions of the endowment specifically stated that the Clinic was to conduct "research and instruction to be carried on under the Faculty of Arts and Sciences," and was to be "a special department with research and instruction in psychology," and that furthermore this special department was a "sub-department of abnormal and dynamic psychology under the present Psychological Department." [21] Under these terms, Prince should have taken his requests through regular Departmental channels instead of going directly to the President. But go over the head of the Department he did, and as a result of their long-standing friendship Lowell gave Prince everything he asked for. In this manner Prince obtained the remodelling of one of the Clinic's rooms in Emerson Hall and the extension of his initial one-year contract to add a second year to his term as the Clinic's director. Additionally, in

1927, the University gave the Clinic the use of a house at 19 Beaver Street in Cambridge for its headquarters.[22]

The Clinic's move from Emerson Hall to the Beaver Street address resulted in a degree of isolation for the Clinic that was to have both good and bad effects. On the negative side, the Clinic was to be effectively cut off from any close collaborative work with the rest of the Department, a situation that was to become a major issue when Murray's promotion and tenure case was being decided in 1937. On the positive side, though, the departure of Prince and Murray from Emerson Hall helped to reduce some of the immediate friction that existed between them and the rest of the Department. Also, the Beaver Street house was large enough for Prince to have the six to eight laboratory rooms that he felt he needed in order to conduct his planned research. As a result of the expanded facilities, two research projects on hypnotic phenomena were initiated in the fall of 1927 under Murray's direction. The effect of hypnosis on the galvanic skin response (GSR) was conducted by Murray, MacKinnon, and Oxnard, while Lundholm, Mitchell, and Weiss studied the effect of hypnosis on conditioned reflexes.[23] This marked the beginning of the Clinic's research program.

The Clinic Flourishes Under Murray: 1928-1936

Prince's health continued to fail throughout the 1927-1928 academic year. He had always had trouble negotiating the three flights of stairs up to his classroom in Emerson Hall, and had even collapsed several times from the exertion. By the spring of 1928, his health had deteriorated to the point that he had to turn most of his teaching over to Murray. Prince would teach one class a week while Murray would teach the other two. The rest of the time Prince stayed at his home on Beacon Street in Boston, writing and seeing his patients.[24] It finally came to the point that Prince knew that he would have to retire as the Clinic's director at the end of the 1927-1928 academic year. The issue then arose as to who would be Prince's replacement. As much as Prince had disagreed with Murray over theoretical questions, he felt that Murray was the most qualified person to succeed him.[25] There were two reasons for this. One, the Clinic's endowment specifically stipulated that the director should be "an experienced physician and psychologist trained in laboratory technique." [26] Murray fit these criteria well; he was both an experienced physician and a trained laboratory scientist. Finally, Murray had spent two years as Prince's assistant so he already knew how to carry out the Clinic's program of research and instruction. If nothing else, appointing Murray as Prince's successor would therefore be the most expedient choice.

However, Boring led the rest of the Department of Philosophy and Psychology in opposing Murray as Prince's successor. While they had initially favored his appointment as Prince's assistant, a number of questions had arisen during the two intervening years about his qualifications for advancement. The most serious originated when it was learned, after Murray had begun his duties as Prince's assistant, that his Ph.D. was in Biochemistry and not in Psychology. It had naturally been assumed that when Henderson recommended Murray to President Lowell as a good psychologist, that Murray was in fact trained as a psychologist and had been involved in biochemical research only as a side-interest. While Boring and the rest of the Department admitted their negligence in not having checked Murray's credentials more closely at that time, they were not about to let this matter slip by them again. Also, there was growing concern over Murray's interest in psychoanalysis. It had originally been thought that Murray would be more scientifically rigorous than Prince, but now his interest in the theories of Freud and Jung raised serious questions about this assumption.[27] Boring, as director of the Psychological Laboratory, argued for finding someone else to replace Prince who would not only meet the requirements of the Clinic's endowment, but who would also fit in better with the increasingly experimental orientation of the work being conducted in the Laboratory.[28] The final decision, however, was up to President Lowell, and true to

his past performance, he sided with his old friends Prince and Henderson, and appointed Murray to succeed Morton Prince as director of the Harvard Psychological Clinic beginning with the start of the 1928-1929 academic year.[29]

Once he had taken over as the Clinic's new director, Murray moved rapidly to exert his authority and set a new course for the Clinic's operations. His first move was to phase out the medical orientation of the Clinic's course materials. The clinical demonstrations that had been so important to Prince were immediately eliminated. Hypnosis, which had been the primary method of research for Prince, was phased out more gradually because of graduate student research that was still in progress. In place of the medical/clinical orientation of Prince's lectures, Murray put his emphasis on teaching psychology graduate students psychoanalytic interpretations of the abnormal and normal human personality. In terms of his research activities, Murray increasingly focused on developing and implementing new research techniques to test his theoretical ideas for a descriptive ego psychology.[30] It is a testimony to the strength of Murray's personality and to the degree of charisma he exuded, that he was singlehandedly able to totally reshape the character of the Clinic in just a few years. The extent to which he was successful in this task can be seen in his 1935 article in the Harvard Alumni Bulletin. He stated in this article that the Clinic had three goals:

(1) to impart by lectures and demonstrations a systematic knowledge of abnormal psychology to qualified students; (2) to conduct researches in the field of dynamic (behavioral)[sic] psychology; and (3) to study and treat patients suffering from emotional disorders.[31]

While superficially similar to the goals originally proposed by Prince in 1926, the Clinic under Murray was oriented significantly more towards the needs of students of psychology, rather than towards the needs of students of medicine.

Murray's style of teaching was markedly different from that of Prince. Despite a tendency to stutter, Murray was an extremely popular and stimulating lecturer. Instead of driving students away, as did Prince, Murray had more students trying to enroll in his courses than he could accommodate.[32] His primary teaching responsibility was a course entitled "Psychology 24--Abnormal and Dynamic Psychology," which was listed as being primarily for graduate students, though open to undergraduates with the permission of the instructor. This course was offered every year, as was a course entitled "Research in Abnormal Psychology," which was open only to advanced graduate students. Other courses were also offered from time to time by Murray. He taught a seminar in personality theory during the 1927-1928 academic year. In both of the next two years he taught a course on individual psychology. In the 1933-1934 academic year Murray team-taught with Gordon Allport a course on the psychology of human action and

motivation. Also, Murray collaborated from time to time with various psychologists from the Department in supervising undergraduate Honors Thesis research and graduate student independent studies.[33]

For the first three years of the Clinic's existence, all of its courses were taught in Emerson Hall along with the rest of the Department of Philosophy and Psychology. The 1928-1929 academic year--Murray's first as the Clinic's Director--marked the last year that the Clinic's courses were taught in Emerson Hall.[34] When the Clinic was moved a second time to the house at 64 Plympton Street during the summer of 1929, the basement of the house was remodelled and made into a lecture hall with a capacity of forty-five students. From this time on, all of the Clinic's classes met in this room and its capacity became the major limiting factor in determining the size of Murray's classes. Starting in 1929-1930 and continuing through the following decade, enrollment in Psychology 24 hovered between 39 and 56 students. The number admitted in any given year was dependent on only two factors: Murray's refusal to enroll unprepared and/or unmotivated students, and the willingness of the enrolled students to either sit on the floor or stand in the aisles when all of the available seats had been filled. Under Murray's instructorship Psychology 24 was consistently one of the most popular psychology courses at Harvard. Only the Department's two introductory psychology

courses and Gordon W. Allport's social psychology course attracted more students.[35]

The Clinic's research output also greatly expanded under Murray's leadership. While detailed records of the research conducted at the Clinic exist only up through 1933, it is obvious that the Clinic had become an important center for graduate student research in psychology at Harvard. From a beginning with only two research projects under way during the 1927-1928 academic year, research activity expanded to five studies in 1928-1929, six studies in both 1929-1930 and 1930-1931, ten studies in 1931-1932, and finally to seventeen studies during the 1932-1933 academic year.[36] Perhaps the best analysis of the Clinic's role during the first seven years of its existence in the growth of psychological research at Harvard was made by Boring in his report to the Harvard President of the activities in 1932-1933 of the Psychological Laboratory. In this report he presented the following chart:

	Number of Researches	
	1926-1927	1931-1932
Human Normal Psychology	12	25
Abnormal and Dynamic Psychology	3	17
Animal Psychology	2	10
Educational Psychology	6	8
Social Psychology	0	7
	--	--
	23	67 [37]

Boring also noted that:

Abnormal and dynamic psychology, favored by the founding of the Psychological Clinic, has come during this period to represent about a quarter of the research in the Laboratory.[38]

The reason for the rapid growth of research in the Clinic was quite simple. The Clinic under Murray's leadership had quickly gained a reputation within the Harvard community as a socially vibrant, intellectually invigorating center of learning. Initially, a small group of graduate students had been attracted to the Clinic, lured by rumors of Murray's adherence to psychoanalytic theories of personality. Generally, these students had found themselves dissatisfied and intellectually stifled by the predominantly Titchenerian brand of experimental psychology favored at the Psychological Laboratory in Emerson Hall. They wanted something more relevant to the concerns of everyday human experience than the study of psychophysics and animal psychology could offer.[39] Once at the Clinic, however, they quickly learned that Murray had much more to offer them than just an introduction to psychoanalytic theory, for the Clinic was a place where divergent ideas could be discussed in lively debate, and where research could be conducted in an atmosphere of exuberant camaraderie.[40]

Life and work at the Clinic was centered in the library of the old house at 62 Plympton Street in Cambridge that had become the Clinic's headquarters after its second move in

1929. This room served many functions. It was a place for the Clinic's graduate students to study; it was the place where their weekly seminars and colloquia were held; and it was the site of the Clinic's daily luncheons and annual Christmas parties. The daily luncheons, in particular, became one of the major sources of intellectual stimulation for those connected with the Clinic. Coming from a wealthy New York family, Murray was acquainted with many prominent individuals from a number of different walks of life. Murray drew on these acquaintances to engineer a steady stream of luncheon guests, all guaranteed of an appreciative audience, regardless of what they wanted to talk about.[41]

Murray brought to Clinic luncheons such Harvard luminaries as the biochemist Lawrence J. Henderson, the philosopher Alfred N. Whitehead, and the sociologist Talcott Parsons. There was also a standing invitation for any of Murray's literary, artistic, or intellectual friends to drop in whenever they happened to be in the Boston area. In this way people such as Felix Frankfurter, Conrad Aiken, Katherine Cornell, Bertrand Russell, and Paul Robeson became frequent luncheon guests. The Clinic also served as the informal meeting place of the Boston Psychoanalytic Institute, and through this connection the luncheons were sometimes graced by the presence of the emigre analysts Hanns Sachs and Franz Alexander.[42] Perhaps the best description of the intellectual conviviality that characterized the Clinic was given by Nevitt Sanford at a

symposium held in Murray's honor at the 1978 Convention of the American Psychological Association. To a standing-room-only audience, he recalled that the Clinic:

presented a kind of civility that has long since been lost. Also, diversity of point of view among people in the same department was still valued, as a means for finding the truth.

I remember an interchange between Harry [Murray] and Fred Skinner at one of our weekly colloquia. There was a discussion of levels of analysis. Some one had drawn some examples of levels on the blackboard. At the top was the person, below that the syndrome or complex, then the need or trait, then the conditioned response, then, I suppose, the nervous impulse. Skinner, who as a graduate student had already worked out most of the ideas that would make him famous, thought one ought to focus at about the level of the conditioned reflex. Harry said he thought we should go higher.

Skinner said, "But I don't want to go higher."

Harry: "I know, you're afraid you'll go to heaven."

Skinner, I think, enjoyed the joke as much as anybody. As a matter of fact we saw him at the Clinic more than a few times--once he gave a formal report on one of his studies of verbal behavior. He thought it would interest us, and it did.[43]

Controversy Surrounds Murray's Work

Even though Murray's teaching was a vast improvement over Prince's; even though Murray was actively pursuing research whereas Prince had not; even though Murray had made the Clinic into a vital center of learning; relations with the Emerson Hall psychologists did not improve dramatically. Murray, the Clinic, and almost everything that went on there

remained highly controversial. For example, there was the issue of Murray's teaching. It was an undisputed fact that as an instructor, Murray was far more popular than Prince, and more popular than most of the faculty of the Department of Philosophy and Psychology as well. However, this very popularity raised questions about the quality of his teaching. Clifford H. Moore, Dean of the Faculty of Arts and Sciences at Harvard, launched an investigation of Murray's teaching of Psychology 24 in the spring of 1930 because it was rumored that Murray's popularity was due to his being an excessively lenient teacher. Moore was concerned that there were too many undergraduates enrolled in a course that was catalogued as being primarily for graduate students. Also, he was suspicious that a large number of these undergraduates did not have a sufficiently high class ranking to allow them to be admitted to such an upper-level course. Surprisingly, however, even though Moore raised these allegations and investigated Murray's teaching of the course, no solid evidence was ever found to substantiate any of these charges.[44]

The problem seems to have been an unwritten policy at Harvard that courses taught by new Assistant Professors should be exceedingly difficult. It was reasoned that if a course by a new faculty member was to be a valuable learning experience, it had to be difficult, and hence, unpopular. Any course taught by a new faculty member that was popular

with the students was therefore immediately suspect as being too easy.

While Murray was able to assure Dean Moore that his concerns were unfounded, the whole incident did not sit well with the rest of the Department. Boring, the de facto head of psychology at Harvard, preferred that everyone keep a low profile as far as administrative controversies were concerned. It was his opinion that "it would have been better for Murray and the Department if the question had not been raised."^[45] However, questions like this were to be raised again and again, especially after James B. Conant became the President of Harvard in 1934. Under Conant's administration greater importance was placed on excellence of research than on excellence of teaching as a measure of a faculty member's professional merit. Therefore, the issue of Murray's popularity as a teacher continued to be a source of conflict between the Clinic and Emerson Hall.^[46]

The research conducted at the Clinic also became a point of contention with the Emerson Hall psychologists. Abnormal psychology was recognized by the Department as a legitimate area of specialized research. The majority of the research carried out at the Clinic was therefore done by graduate students in fulfilling the requirements for the Ph.D. in psychology at Harvard. While their research was done at the Clinic under Murray's supervision, they were still students in the Department and they had to fulfill all

of the Department's graduation requirements. This meant that most of the courses that the Clinic's graduate students had to take were taught by the experimental psychologists in Emerson Hall. Their only courses under Murray at the Clinic were Psychology 24 and perhaps several independent studies.[47] Even though the Clinic's graduate students went through virtually the same course of instruction as did those students working in the Psychological Laboratory, their preparation as psychologists was constantly being questioned by Boring and his colleagues. For example, in reviewing the final draft of one of Murray's student's Ph.D. thesis, Boring commented that he:

lacks perspective, that he does not know what psychologists know, that he mixes the new with the old. So often he explains naively all about what every first year graduate student must know and what should therefore be taken for granted.[48]

This critical attitude towards the graduate student research at the Clinic even extended into the manner in which the final oral defense was conducted by the Emerson Hall psychologists. The examining committee for each thesis student consisted of the supervisor of the student's work, and two other members of the Department who were appointed by the Department Chairman to act as "readers." While there is no evidence that an actual conspiracy existed to place readers from the Psychological Laboratory who were hostile to the Clinic on the examining committees of Clinic students, there was a marked tendency for the examination to focus largely on questions concerning the legitimacy of the

research topic and the scientific merit of the research methods used.[49]

The quality of the research conducted at the Clinic was not the only thing that was questioned. Questions were raised on several occasions as to whether the work performed at the Clinic was morally acceptable to the Harvard community. Shortly after the Clinic had begun operation, there were rumors circulating around Harvard that Prince and Murray were planning to "conduct a systematic investigation of certain enigmatical obsessions prevalent in the Faculty," and there is some reason to believe that the Clinic's move out of Emerson Hall may have been expedited in response to this rumor, as a means of preventing the implementation of the supposed plan.[50] Even more controversial, though, was Murray's adherence to psychoanalytic interpretations of psychological phenomena and the use of hypnosis as a research tool at the Clinic.

Harvard in the late 1920's and early 1930's was both socially and politically conservative, or, as Murray put it in a 1929 Harvard Crimson article: "a coclbed of conservatism." [51] Psychoanalysis in particular was seen by many as more voodoo than science, and its emphasis on the role of sexuality as the foundation of psychological functioning made it even more suspicious among "proper" Bostonians. Consequently, if anyone connected with the Clinic, be he or she a student, staff member, or research

subject, developed any personal problems, questions were instantly raised about the propriety of the Clinic's work.

As Murray stated in the same article:

If a Radcliffe student suffers from morbid depression and a copy of McDougall is found on her shelves, her plight is put at the back door of the Psychological Clinic.[52]

Given the concerns raised by the very nature of the work being conducted at the Clinic, whenever some problem did arise the Department and the university administration responded swiftly and severely. An example of this can be seen in regards to Murray's use of hypnosis as a research tool at the Clinic. The use of hypnosis in research was a highly controversial topic. At the same time that Murray was starting his research at the Clinic, Clark L. Hull was coming under a lot of fire from the Yale University administration for his hypnosis research at the Yale Institute of Human Relations. Put simply, the Yale administration felt that hypnosis was far too dangerous to be used in psychological experimentation with normal human subjects. It was thought that there was too great a risk of the subject experiencing some permanent psychological damage, and Hull was forced to relent under this pressure and abandon hypnosis as a research method.[53] Murray was well aware of the situation faced by Hull at Yale through his personal contacts with the New Haven medical community. However, despite warnings that his research was not immune to similar criticism, Murray pressed on with hypnosis

research at the Clinic. These criticisms arose when a Radcliffe student, who had been participating as a subject in a hypnosis study in 1931, complained that sexual advances had been made to her by a graduate student researcher while she was in a trance state. These charges were taken very seriously by the Harvard administration, and Murray was called before Dean Murdock to account for his student's behavior. The Department also took action in this case. Even though there was little concrete evidence against the student, Boring gave him the option of either withdrawing from Harvard, or facing the possibility of having charges pressed against him. The student chose the former option, and Boring settled the case once and for all by refusing to allow Murray ever again to use hypnosis in any research project involving the use of Harvard or Radcliffe students as subjects.[54]

Murray also found himself at the center of a storm of controversy concerning the administrative affairs of the Clinic. Unlike Prince, Murray did not purposely try to circumvent the Departmental chain of command in seeing to the daily affairs of the Clinic's operation. However, the physical isolation of the Clinic from Emerson Hall served, in this case, to disrupt the regular administrative channels. Faced with vague and sometimes contradictory instructions about how to administer the Clinic, Murray would act in good faith only to discover that he had unwittingly overstepped his authority, or in some other way

committed an administrative faux pas. Technically, since the Clinic was an auxiliary to the Psychological Laboratory, all of the Clinic's policy and budgetary decisions had to be approved by Boring, the Laboratory's Director.[55] Boring, though, had as early as 1932 communicated to Murray his desire not to be bothered by being consulted on every issue that came up at the Clinic. Boring felt that he had enough to worry about in running the Psychological Laboratory, so he gave Murray virtually free rein in running the Clinic.[56]

The effect of this was that Murray did as he pleased at the Clinic, and if some important question of policy or finance arose, he would confer directly with the Harvard University administrators who were ultimately responsible for handling the matter. This system worked well as long as the interests of the Clinic and the Psychological Laboratory did not conflict. Given the different aims of the Clinic and the Laboratory, just such a conflict was soon to arise. For example, on January 14, 1932, Boring sent Murray a brief note which, among other things, suggested that the Department of Philosophy and Psychology might be willing to pick up part of Murray's salary out of their unrestricted funds. In a sense, Boring was saying that the Department was ready to make a "spiritual adoption" of Murray and the Clinic.[57] But Murray had received no further communication concerning this by April, when Departmental and Laboratory budgets for the upcoming academic year had to be submitted

to the Dean. As a result, Murray went ahead as he had done before, and submitted his own budget independent of the Department. Unbeknownst to him, though, the Department had included his salary in their own budget request as Boring had earlier indicated they would. Dean Murdock of the Harvard Faculty of Arts and Sciences was left with the problem of having to reconcile these conflicting budget proposals, and he decided to follow the established precedent and pay Murray's salary solely from the Clinic's endowment.[58]

This infuriated Boring, and he vented his feelings on Murray. While denying that he had any desire to have personal power over Murray or the Clinic, he reversed his earlier position and demanded that Murray use the Department as the channel for all official university business. Boring stressed this point by arguing that the Clinic:

(1) . . . can continue in the way that Morton Prince and the President [Lowell] started it off, as an independent little venture which sinks or swims by itself largely in relation to what extra money you can find for it. (2) Or it can continue to increase its liaison with the Department so that it shares in the fortunes of the Department generally. I can contemplate either possibility, but I prefer (2). You say that you prefer (2). All right, but then you must not do things that make (2) difficult.[59]

A similar problem occurred in 1934, this time concerning Murray's use of money given by the Harvard Business School's Committee on Industrial Fatigue to aid several of the Clinic's graduate students. Two of the

students who were receiving this aid also held assistantships in the newly separate Department of Psychology. Murray was informed by Dean Edsall of the Business School that anyone receiving money from two or more Harvard sources would have to have their appointment confirmed by Mr. Hunnewell, the Chairman of the Harvard Corporation, and that Murray would have to write to him directly on this matter. But first, the question arose as to what official title should be recommended for these two students when Murray wrote to the Corporation. Murray conferred with Lawrence J. Henderson and Dr. Elton Mayo of the Industrial Fatigue Committee, and they reached the decision that the two students be given the title of Research Fellow as this did not directly conflict with any other official Harvard title. With this issue settled, Murray sent his letter to Mr. Hunnewell.[60]

In the meantime, though, Boring had written to Dean Murdock of the Faculty of Arts and Sciences recommending Murray's two students for the position of Departmental Research Assistant.[61] Also, it turned out to be the case that Mr. Hunnewell did not actually have the authority to confirm University appointments, and that he only passed all such letters on to Dean Murdock. The Dean noted the discrepancy in Boring's and Murray's letters, and returned the whole matter to Boring for clarification.[62]

As a result, both Boring and Dean Murdock wrote angry letters to Murray for fouling up the appointment procedure by not going through the proper channels.[63] Murray angrily responded that as far as he knew he had gone through the proper channels, and that he could not understand why, no matter how hard he tried, he was always seen as being in the wrong.[64] This outburst prompted Boring to re-evaluate his position on the matter, and after reviewing all the facts on this case, he realized that Murray had acted properly after all. Boring wrote an apology to Murray for his earlier letter, and the two were then able to come to an agreement about administering the Clinic. Put simply, Murray was to retain virtual autonomy in directing the Clinic's operations, but Boring was to be kept informed by memoranda of important matters of policy or finance. In this way, conflicts like the above could be avoided, ensuring a minimum of disruption to Murray's and Boring's usual routines.[65]

One final administrative issue caused problems between Murray and Boring. Since the Clinic's endowment paid for little more than Murray's salary, he had to rely on volunteer help to supplement the research efforts of himself and his graduate students. Some of these volunteers came from within the Harvard community, either from the Psychological Laboratory or from other departments on campus. The rest of the volunteers were individuals from around the Boston area who were interested in the work

Murray was conducting.[66] It was the non-Harvard volunteers that were problematic. Boring frequently had to remind Murray that as a University facility, the Clinic could not allow non-University personnel to conduct research there. However, this was an issue that was never resolved to Boring's complete satisfaction, as Murray usually arranged for these volunteers to enroll as part-time students taking only an independent study under Murray. This allowed them to continue their research with little alteration to their actual relationship with Harvard.[67]

The general atmosphere fostered by Murray at the Clinic also produced a fair amount of friction between Murray and the Emerson Hall psychologists. Murray's upper-class origins and life-style continued to be a point of contention with Boring and his fellow experimental psychologists. The prevailing opinion at Harvard was that scientific excellence demanded personal sacrifice. According to Murray some faculty went as far as to purposely fail all upper-class graduate students who came their way, because they felt that a wealthy student could never develop the sense of self-sacrifice necessary for excellence in scientific work.[68] Everything that Murray did flew in the face of this opinion. He lavishly furnished the Clinic with antiques and works of art. The Clinic's daily luncheons, weekly colloquia, and frequent parties were known as some of the outstanding social events on campus.[69] Boring and his colleagues were quite simply jealous of the way Murray

managed to live the good life while the deepening Depression forced everyone else to scrimp and save just in order to get by.[70]

Not only was Murray's life-style radically different from the norm at Harvard, he also appeared to some to be working less hard than most others as well. This only served to deepen the already existing resentment of Murray's personal wealth and social charm. Murray had two private retreats, a farm in Topsfield Massachusetts, and a cottage in New York's Thousand Islands, to which he would frequently retire to write and plan for future research. During some periods, he was away from the Clinic a good deal of the time, and no one, except for the Clinic's secretary Mrs. Ingalls, would know where he was or when he would be back.[71] Instead of teaching summer courses in order to make extra money, as most of the Harvard faculty needed to do, Murray preferred to have one of his advanced graduate students teach Psychology 24 during the summer session, so he could take time off to vacation with his family and do some writing. Even with his summers free, Murray felt that he needed more time off from Harvard in order to conclude some research that had been put aside for a couple of years. So, in September of 1931, Murray asked for, and was granted, a temporary leave of absence for the upcoming month of January.[72] All this led Boring to accuse Murray of not being sufficiently serious about his work and of not

fulfilling his share of the Department's teaching responsibilities.[73]

The Incident of Boring's Analysis

In August of 1935, however, an opportunity arose for a personal reconciliation between Murray and Boring. Starting in the fall of 1934, Boring underwent an analysis with Hanns Sachs, a leading psychoanalyst, and a friend and occasional collaborator of Murray's. Boring felt that he had reached the end of his professional creativity. He seemed to be unable to produce work of any quality since the completion of the manuscript of his book, The Physical Dimensions of Consciousness in 1932. Anxiety had mounted over this inability to work until, at the encouragement of his wife and close friends, Boring had turned to Sachs for professional help. While Boring felt that he got along well personally with Sachs, he came to no great insight into his problem as a result of the analysis. His feeling that the analysis had been a failure only dramatically increased when on June 20, 1935, Sachs announced that he was terminating the analysis, claiming that as much progress as was possible had been made, and that he wanted to vacation in Europe for the months of July and August. In desperation, Boring turned to Murray to help him through this stressful period.[74]

Murray was spending the summer at his retreat in the Thousand Islands area of New York when Boring's plea for help arrived. While sympathetic to his plight, Murray did not want to conduct an analysis by mail. Therefore, Murray wrote to Boring suggesting that perhaps the best thing that he could do would be to get away from Cambridge for a few days. Murray then suggested that Boring come up for a visit to the Thousand Islands for a week, and that there the two of them could discuss the situation in an undisturbed, relaxed setting. Murray did, however, venture a guess in this letter as to what might have been Boring's major problem with the analysis:

As far as your analysis goes I will make a guess that with all your hopes & good intentions & with kindly old Sachs sitting behind his comfortable paunch, you never got around to a real explosion of your heaped up, sorely provoked aggression. You never forced all of the malice out of your nature--I am assuming for the time being that you have as much as I have--you never fired it right before his eyes. The right thing to do in the fall is to gather your forces & tell Sachs how he, (as all the Freudians do for that matter) led you on only to leave you high and dry with no nourishment in sight. You must mass all the hatred and injured hopes in your being & in several sessions give him the works--don't omit a single grievance, not one.[75]

Boring took Murray up on his offer of spending a week in the Thousand Islands, leaving Cambridge on August 17, 1935 to rendezvous with Murray in Clayton, New York.[76] Once at Murray's island retreat, Boring apparently decided to modify Murray's advice to vent his wrath on Sachs. Instead, Boring vented his anger on Murray. Donald W.

MacKinnon, R. Nevitt Sanford, and Erik Homburger [Erikson], three of Murray's co-workers from the Clinic who were also visiting at the time, were witnesses to this episode. Boring later recounted the incident as follows:

I went to see [Murray] partly in order to damn psychoanalysis (and I did DAMN it to him, MacKinnon, Sanford and Homburger assembled, all in one evening, in the middle of the St. Lawrence).[77]

. . . we had it out. I said: "But how does it change personalities? How can self knowledge alone do it? And there's no technique. And it's vague, much like the technique of any spiritist's mind reading. And the unconscious is not so far beneath after all." Oh, I said it, and they fell silent; and then I realized that that very evening was August 20, the second month [after Sachs terminated the analysis]. And we expected the heavens to be rent asunder and the fire of insight to descend, and it was just like any other evening with an argument on.[78]

Like his analysis, Boring's chance to achieve a personal reconciliation with Murray ended a failure in an atmosphere of bitterness and unfulfilled hopes.

This last incident was to prove significant in determining the nature of the arguments surrounding Murray's promotion to Associate Professor in 1937. While Murray had guided the Clinic to great advances in teaching and research, these advances were clouded in controversy. In the end, it all came down to a debate centered on highly emotional issues among the members of the ad hoc committee appointed by President Conant to review Murray's case for promotion. Boring, as Chairman of the Department of Psychology, was of course appointed to this ad hoc

committee, and his feelings about the outcome of his analysis weighed heavily upon him then. That this situation would arise was on Boring's mind when he had first written to Murray for help in 1935 and said, "how does this effect my attitude toward analysis in the University and what can I do to be sure that I am fair?"[79]

Conclusion

Boring and Murray were caught in a professional "love hate" relationship that would eventually threaten Murray's career at Harvard. Boring was adamantly opposed to the old "gentleman scholar" approach to academia, as no recognition of earned professional merit was acknowledged therein. Given the persistent rumors that Prince had used his wealth and friendship with President Lowell to influence the founding of the Clinic and the awarding of his professorship and his lack of professional productivity, it is easy to see why his relations with Boring were strained. Likewise, Murray was in many ways a representative of the old academic order. Murray was independently wealthy, and thus did not feel the need to fight his way up the career ladder in order to survive. Also, he obtained his position through personal contacts, rather than by demonstrating his merit by having the accepted academic credentials and research expertise. Furthermore, Murray tended not to follow the departmental chain of command regarding administrative decisions

involving the Clinic, thus threatening Boring's sense of control over the development of psychology at Harvard.

Murray did, however, possess a number of qualities that were totally in line with Boring's conception of an academic psychologist. Murray established a reputation as a dynamic teacher who consistently attracted more students than he could accommodate. Murray's enthusiasm carried over to the laboratory as well. By the mid-1930's Murray was responsible for directing more graduate student research than any other psychologist at Harvard. Murray also transformed the Clinic's colloquia and daily luncheons into a leading forum for intellectual exchange at Harvard.

Murray's successes as an academic were not able to outweigh the criticisms leveled against him though. While he could demonstrate through his teaching and research that his lack of an advanced degree in psychology was of little consequence, there still remained the disciplinary boundary issue of his advocacy of psychoanalysis. Boring was opposed to psychoanalysis for two reasons. First, psychoanalysis was a form of applied psychology, and Boring rejected the inclusion of applied topics in the academic curriculum. Second, Boring had had a negative personal experience with psychoanalysis. This further jaundiced his view of the theory and of its practitioners, and threatened his ability to remain totally objective regarding decisions involving Murray and the Clinic.

Reference Notes

1. A copy of the Harvard Corporation's resolution to accept Prince's offer of the endowment to fund the Harvard Psychological Clinic can be found in an undated mimeographed document in the Gordon W. Allport Papers, Harvard University Archives, Nathan Marsh Pusey Library, Harvard University, Cambridge, Mass.

2. Morton Prince to A. Lawrence Lowell, 26 January 1926; A. Lawrence Lowell to Morton Prince, 17 February 1926; and Morton Prince to Charles F. Adams, 19 February 1926. Copies of these letters are to be found in an undated mimeographed document in the Gordon W. Allport Papers, Harvard University Archives.

3. James B. Conant, My Several Lives: Memoirs of a Social Inventor (New York: Harper & Row, 1970), p. 109; Samuel E. Morison, Three Centuries of Harvard: 1636-1936 (Cambridge, Mass.: Harvard University Press, 1936), pp. 476-477; Henry A. Murray, personal interview, 14 October 1981; and Henry A. Yeomans, Abbott Lawrence Lowell: 1856-1943 (Cambridge, Mass.: Harvard University Press, 1948), pp. 183-195.

4. Conant, My Several Lives, pp. 98-105.

5. *Ibid.*, p. 92.

6. Edwin G. Boring, Psychologist at Large (New York: Basic Books, 1961), pp. 44-46; "The Psychological Laboratory," in Reports of the President and the Treasurer of Harvard College: 1925-26 (Cambridge, Mass.: Harvard University, 1927), p. 240; and Joseph Lee, "Report of the Committee to Visit the Department of Philosophy and Psychology," in Reports of the Visiting Committees of the Board of Overseers of Harvard College: For the Academic Year 1932-1933 (Cambridge, Mass.: Harvard University, 1933), p. 151. Both of the above sources are in the Harvard University Archives. See also Chapter 4, pp. 108-111 for a further discussion of this debate.

7. Boring, Psychologist at Large, pp. 44-45.

8. Henry A. Murray, videotaped interview with Eugene Taylor, 12 November 1981, Rare Books, Countway Library of Medicine, Harvard Medical School, Boston, Mass.

9. Edwin G. Boring to Karl S. Lashley, 8 April 1937, in the Edwin G. Boring Papers, Harvard University Archives.

10. Henry A. Murray, Personal interview, 13 July 1981; and Murray, Interview, 14 October 1981; and Henry A. Murray, Personal Interview, 8 July 1982.

11. Boring to Lashley, 8 April 1937; Murray, Interview, 13 July 1981; Interview, 14 October 1981; Eugene Taylor, Personal Interview, 7 November 1981; and Robert W. White, Personal interview, 2 July 1981.

12. Prince to Lowell, 26 January 1926.

13. Boring, "The Psychological Laboratory, 1925-26," p. 240.

14. Edwin G. Boring to William McDougall, 27 January 1927, in the Edwin G. Boring Papers, Harvard University Archives.

15. Edwin G. Boring to William McDougall, 25 October 1926, in the Edwin G. Boring Papers, Harvard University Archives; Murray, Interview, 14 October 1981; and White, Interview, 2 July 1981.

16. Boring to McDougall, 25 October 1926; and Henry A. Murray, "Morton Prince: Sketch of his Life and Work," Journal of Abnormal and Social Psychology 52 (1956): 292.

17. Boring to McDougall, 25 October 1926.

18. White, Interview, 2 July 1981.

19. Boring to McDougall, 21 January 1927; R. Nevitt Sanford, Personal interview, 28 August 1981; Silvan Tomkins, Personal interview, 26 August 1981; and White, interview, 2 July 1981.

20. Boring to McDougall, 21 January 1927.

21. Prince to Lowell, 26 January 1926.

22. Boring to McDougall, 21 January 1927; and Edwin G. Boring, "The Psychological Laboratory," in Reports of the President and the Treasurer of Harvard College: 1926-27 (Cambridge, Mass.: Harvard University, 1928), p. 244, in the Harvard University Archives.

23. Boring, "The Psychological Laboratory, 1926-27," p. 245.

24. Boring to McDougall, 27 January 1927; and Edwin G. Boring to William McDougall, 13 June 1927, in the Edwin G. Boring Papers, Harvard University Archives.

25. Boring to Lashley, 8 April 1937.
26. Prince to Lowell, 26 January 1926.
27. Boring to Lashley, 8 April 1937; Murray, Interview, 14 October 1981; and White, Interview, 2 July 1981.
28. Boring to Lashley, 8 April 1937.
29. Ibid.
30. Henry A. Murray, "Professor Murray Describes Department of Abnormal Psychology," Harvard Crimson, 12 January 1929, p. 4; "The Harvard Psychological Clinic," Harvard Alumni Bulletin, 25 October 1935, pp. 142-149; Interview, 13 July 1981; and White, Interview, 2 July 1981.
31. Murray, "Harvard Psychological Clinic," p. 143.
32. Edwin G. Boring to Gordon W. Allport, 3 May 1930, in the Edwin G. Boring Papers; Harvard Crimson, 1 May 1936, quoted in Gordon W. Allport to Edwin G. Boring, 4 August 1936, in the Gordon W. Allport Papers; and Henry A. Murray, "Henry A. Murray," in A History of Psychology in Autobiography, volume 5, eds. E. G. Boring and G. Lindzey (New York: Appleton-Century-Crofts, 1967), p. 287.
33. For a listing of the courses taught by Murray between 1927 and 1937, consult the "Report of the Faculty of Arts and Sciences," in the Reports of the President and the Treasurer of Harvard College for the years 1927 through 1928, and in the Report of the President of Harvard College for the years 1929 through 1937, all of which are in the Harvard University Archives.
34. Boring to McDougall, 25 October 1926; Boring to McDougall, 13 June 1937; and Moore, "Report of the Faculty, 1928-29," p. 76.
35. Boring to Allport, 3 May 1930; Harvard Crimson, 1 May 1936; and Murray, "Harvard Psychological Clinic," pp. 143, 145. Also, see the "Report of the Faculty of Arts and Sciences" in the Reports of the President and the Treasurer of Harvard College for the years 1927 through 1929, and in the Report of the President of Harvard College for the years 1929 through 1937 for the enrollment figures for Psychology 24.
36. See Edwin G. Boring's "The Psychological Laboratory" reports in the Reports of the President and the Treasurer of Harvard College for the years 1926 through 1929, and in the Report of the President of Harvard College for the years 1929 through 1933 for a listing of the psychological research in progress. The requirement that

each laboratory at Harvard submit a separate annual report of its work was abolished when James B. Conant became the Harvard President in 1934.

37. Edwin G. Boring, "The Psychological Laboratory," in Report of the President of Harvard College: 1931-32 (Cambridge, Mass.: Harvard University, 1933), p. 270, in the Harvard University Archives.

38. Ibid., p. 270.

39. Henry A. Murray, Personal interview, 28 April 1981; "Murray Describes Department of Abnormal Psychology," p. 4; R. Nevitt Sanford, Personal interview, 28 August 1981; "Reminiscences and Celebrations," Paper read as part of the symposium "Explorations in Personality Forty Years Later," Rae Carlson, Chairman, Annual Convention of the American Psychological Association, Toronto, Canada, 31 August 1978. A transcript of this symposium can be found in the Alan Elms Record, M1045, Archives of the History of American Psychology, University of Akron, Akron, Ohio; and White, Interview, 2 July 1981.

40. Donald W. MacKinnon, "Facts in Psychological Leadership," one of a series of questionnaires completed by prominent American psychologists as part of a study designed to find out what personal and institutional factors were important in their career development, in the Lauren Wispé Papers, M698-M699, Archives of the History of American Psychology; Sanford, Interview, 28 August 1981; Sanford, "Reminiscences and Celebrations;" and White, Interview, 2 July 1981.

41. Hiram Hayden, "Portrait: Henry A. Murray," The American Scholar 39 (1970): 123-124, 127, 131-132; MacKinnon, "Facts in Psychological Leadership;" Murray, "Henry A. Murray," pp. 294-295; "Murray Describes Department of Abnormal Psychology," p. 4; "Harvard Psychological Clinic," pp. 145-146; Personal interview, 1 December 1981; Sanford, Interview, 28 August 1981; "Reminiscences and Celebrations;" and White, Interview, 2 July 1981.

42. Murray, Interview, 1 December 1981; Sanford, Interview, 28 August 1981; "Reminiscences and Celebrations;" and White, Interview, 2 July 1981.

43. Sanford, "Reminiscences and Celebrations," pp. 5-6; and B. F. Skinner, "B. F. Skinner," in A History of Psychology in Autobiography, vol. 5, E. G. Boring and G. Lindzey, eds. (New York: Appleton-Century-Crofts, 1967), p. 398, for a further discussion of the amicable relationship between Murray and Skinner.

44. Boring to Allport, 3 May 1930.
45. Ibid.
46. Edwin G. Boring to Gordon W. Allport, 24 March 1930; and Gordon W. Allport to Edwin G. Boring, 27 February 1936. Both of the above are in the Edwin G. Boring Papers, Harvard University Archives.
47. Edwin G. Boring to Gordon W. Allport, 7 December 1929, in the Edwin G. Boring Papers, Harvard University Archives; Murray, Interview, 13 July 1981; Interview, 3 August 1981; Sanford, Interview, 28 August 1981; M. Brewster Smith, Personal interview, 28 August 1981; Silvan Tomkins, Personal interview, 26 August 1981; and White, Interview, 2 July 1981.
48. Edwin G. Boring to Henry A. Murray, 25 April 1934, in the Edwin G. Boring papers, Harvard University Archives.
49. Sanford, Interview, 28 August 1981; and White, Interview, 2 July 1981.
50. Murray, "Murray Describes Department of Abnormal Psychology," p. 4; and "Harvard Psychological Clinic," p. 142.
51. Murray, "Murray Describes Department of Abnormal Psychology," p. 4.
52. Ibid.
53. Clark L. Hull, "Clark L. Hull," in A History of Psychology in Autobiography, volume 4, eds. E. G. Boring, et. al. (Worcester, Mass.: Clark University Press, 1952), p. 152; "Psychology of the Scientist: IV. Passages from the 'Idea Books' of Clark L. Hull," ed. Ruth Hays, Perceptual and Motor Skills 15 (1962): 848, 849-850, 852; Rodney G. Triplet, "The Relationship of Clark L. Hull's Hypnosis Research to His Later Learning Theory: The Continuity of His Life's Work," Journal of the History of the Behavioral Sciences 18 (1982): 22-31; and Griffith W. Williams, "Clark L. Hull and His Work on Hypnosis," International Journal of Clinical and Experimental Hypnosis 1 (1953): 2.
54. Henry A. Murray to Edwin G. Boring, Undated (most likely early January 1931); Henry A. Murray to Edwin G. Boring, 28 January 1931; Edwin G. Boring to Henry A. Murray, 29 January 1931; Edwin G. Boring to Henry A. Murray, 6 February 1931; Edwin G. Boring to Henry A. Murray, 29 September 1931; and Edwin G. Boring to Henry A. Murray, 11 December 1931. All of the above are in the Edwin G. Boring Papers, Harvard University Archives. Also,

Murray, Interview, 3 August 1981.

55. Prince to Lowell, 26 January 1926. See also pp. 148-149 of this chapter for a discussion of the origins of this issue.

56. Edwin G. Boring to Henry A. Murray, 23 March 1932, in the Edwin G. Boring Papers, Harvard University Archives.

57. Edwin G. Boring to Henry A. Murray, 14 January 1932, in the Edwin G. Boring Papers, Harvard University Archives.

58. This sequence of events can be reconstructed from a reading of Edwin G. Boring to Henry A. Murray 18 April 1932, in the Edwin G. Boring Papers, Harvard University Archives.

59. Ibid.

60. Henry A. Murray to Edwin G. Boring, 21 September 1934, in the Edwin G. Boring Papers, Harvard University Archives.

61. Edwin G. Boring to Henry A. Murray, 17 September 1934, in the Edwin G. Boring Papers, Harvard University Archives.

62. Edwin G. Boring to Henry A. Murray, 22 September 1934, in the Edwin G. Boring Papers, Harvard University Archives.

63. Boring to Murray, 17 September 1934. Murdock's letter is quoted in part in Edwin G. Boring to Henry A. Murray, 26 September 1934, in the Edwin G. Boring Papers, Harvard University Archives.

64. Murray to Boring, 21 September 1934.

65. Boring to Murray, 22 September 1934.

66. Murray, "Harvard Psychological Clinic," pp. 143-146; Interview, 13 July 1981; and White, Interview, 2 July 1981. For a detailed discussion of the Clinic's financial situation, see Chapter 4.

67. Boring to Murray, 29 September 1931; Edwin G. Boring to Henry A. Murray, 13 October 1932; and Henry A. Murray to Edwin G. Boring, 20 October 1932. The above are in the Edwin G. Boring Papers, Harvard University Archives.

68. Murray, Interview, 13 July 1981.

69. Sanford, Interview, 28 August 1981; and White, Interview, 2 July 1981.

70. Sanford, Interview, 28 August 1981; Tomkins, Interview, 26 August 1981; and White, Interview, 2 July 1981.

71. Tomkins, Interview, 26 August 1981.

72. Edwin G. Boring to Gordon W. Allport, 3 May 1929; Edwin G. Boring to Henry A. Murray, 17 November 1931; Henry A. Murray to Edwin G. Boring, 1 December 1931; and Edwin G. Boring to Henry A. Murray, 27 September 1932. The above are in the Edwin G. Boring Papers, Harvard University Archives.

73. Edwin G. Boring to Gordon W. Allport, 24 March 1930, in the Edwin G. Boring Papers, Harvard University Archives.

74. Edwin G. Boring, "Was this Analysis a Success?" Journal of Abnormal and Social Psychology 35 (1940): 4-16; Psychologist at Large, pp. 53-55; Edwin G. Boring to Henry A. Murray, 3 August 1935, in the Edwin G. Boring Papers, Harvard University Archives; and Julian Jaynes, "Edwin Garriques Boring: 1886-1968," Journal of the History of the Behavioral Sciences 5 (1969): 107-108.

75. Henry A. Murray to Edwin G. Boring, Undated (response to Boring to Murray, 3 August 1935), in the Edwin G. Boring Papers, Harvard University Archives.

76. Ibid. This was the only time that Murray could meet with Boring before the start of the fall semester.

77. Edwin G. Boring to Gordon W. Allport, 11 September 1935, in the Edwin G. Boring Papers, Harvard University Archives.

78. Edwin G. Boring to Richard M. Elliott, 9 February 1936, in the Edwin G. Boring Papers, Harvard University Archives.

79. Boring to Murray, 3 August 1935.

CHAPTER 6:
MURRAY'S THEORY AND RESEARCH:
THE INFLUENCE OF HENDERSON, FREUD, AND JUNG

Introduction

The preceding two chapters have focused on Murray's work at the Harvard Psychological Clinic in terms of issues of institutional and professional significance. In particular, the means by which the Clinic was established, funded, and institutionally organized, as well as Murray's professional credentials and conduct have been discussed in detail. The present chapter focuses on the disciplinary boundary issue of the acceptability of Murray's theoretical views and research interests for academic psychology.

Murray claimed that "there was nothing original about his [sic] ideas."^[1] Nonetheless, they offended the academic sensibilities of Lashley and Boring, and caused Allport to refer to him as "independent of opinion" and to criticize him for his "brusque opposition to academic psychology."^[2] Clearly, something about Murray's approach to psychology was very controversial.

What was "new" was not the elements of Murray's theory and research methods, but their implementation within academic psychology. Murray crossed the then accepted disciplinary boundaries of academic psychology by basing his work on concepts derived from psychoanalysis, medicine, and biochemistry. This fact was to become an issue of major importance in the debate that developed surrounding his promotion to Associate Professor in 1937.

Murray's Self-Education in Psychology

Murray was not a psychologist when he came to the Clinic in 1926, but he was to develop into one by 1938. During this period of time a tremendous amount of self-education took place. This process was, however, selective. Murray studied only those psychologists he found relevant to his conception of the discipline. These theorists can be characterized by the common themes of a dynamic motivational system and unconscious mental processes. Not only do these two themes dominate Murray's work, but they can be largely derived from the writings of Freud and Jung. Murray thus possessed a relatively limited background in traditional academic psychology. It will be shown, instead, that he drew upon his graduate education in the life sciences for his programmatic theoretical and methodological ideas. These were to form the basis of his work at the Harvard Psychological Clinic.

Analysis of Murray's early work at the Clinic confirms the initial limits imposed by his lack of formal training in psychology. These limitations are particularly clear in the papers he published prior to 1934. In this early period, Murray's research appears to be of a highly derivative nature, demonstrating clearly his previously stated interest in the works of Prince, James, Jung, and Freud. It also reflects the fact that he had just taken over the directorship of the Clinic, and therefore was committed to overseeing the completion of several research projects that had been initiated under Prince's sponsorship.

The first psychological research paper that Murray published was a lengthy summary of the hypnosis research that he had conducted in collaboration with Herbert Barry and Donald W. MacKinnon.[3] This research was simply an extension of the program that had been begun at the Clinic by Prince. Frequent references are made in this paper to Prince, citing him in regard to data supporting the importance of rapport with the hypnotist as a factor influencing hypnotizability, and in reference to clinical data suggesting a possible correlation between hypnotizability and susceptibility to hysteric conversion-reaction neuroses.[4] The purpose of this paper, though, was to determine if hypnotizability could be conceived as a personality trait associated with other phenomena of psychological dissociation. In this regard, Murray referred frequently to Janet's work on dissociation,

Jung's Psychological Types, and McDougall's Outline of Abnormal Psychology. [5] Experimental tests of dissociation were designed and conducted by MacKinnon, but contrary to their expectations no relationship was found between hypnotizability and dissociation. [6] Similarly, no significant relationship was found between hypnotizability and other identifiable personality traits, such as McDougall's submission instinct or Jung's extraversion trait. [7]

The direction of some of Murray's early published research was also dictated by the interests of his students, and not by any programmatic research plan. The students working with Murray at the Clinic were all pursuing their Ph.D. degree in Psychology at Harvard. They took the same courses in experimental psychology as did the rest of the graduate students in Emerson Hall. The only difference was that Murray's students also took courses and conducted their research at the Clinic. Since his students spent as much time at Emerson Hall as at the Clinic, their research was often more traditionally experimental than clinical in orientation. [8] Examples of this student-originated research include the articles Murray published with H. A. Wolff and C. E. Smith on the psychology of humor. [9] They interpreted their data as indicating that a lack of a mirth response to a joke could be used as a reliable indicator of the degree to which the subject identified with the "butt" of the joke. Another example was the brief report Murray

and D. R. Wheeler published in 1937 based on data that they had collected five years earlier.[10] This paper presented an analysis of 1300 dreams dealing with the kidnapping of the Lindbergh baby. Reports of dreams had been solicited a few days after the kidnapping, and were analyzed for factual content after Bruno Hauptmann had been convicted of the crime.[11] Only seven dreams contained any significant amount of factual information, thus leading Murray and Wheeler to conclude that the clairvoyance of dreams was no more than a popular misconception.

Psychoanalytic Origins of the Explorations

These papers, however, gave no indication of the developing programmatic approach to theory and research that was to result in the Explorations of 1938, and form the basis of his psychological career. In 1932, Murray, in collaboration with a number of co-workers, embarked on a major long-term investigation of human personality. This marked the point when he actually began to think of himself as an independent research psychologist. In an unpublished report he described this project in the following terms:

During the past year the members of the Clinic staff have collaborated in an investigation of the total personality. Each member has worked with a particular experimental technique, by means of which he created for the subjects a type of stimulus-situation or environmental pressure designed to bring into prominence a particular aspect of personality. The persons who served as subjects were tested in turn by each experimenter. By means of the data collected in this way, it was possible to study and compare the reactions of

single individuals to many varied situations. An attempt could then be made to construct an intelligible abstract portrait of each individual.[12]

The most important feature of Murray's developing theory of personality was his reliance on psychodynamic theories as the source of many of his concepts. This point was made explicit by Murray in his dedication of the Explorations to:

SIGMUND FREUD
whose genius contributed the most fruitful
working hypotheses . . .
and to
CARL G. JUNG
whose writings were a hive of great
suggestiveness,

in the numerous references to the writings of Freud and Jung, and in the extensive use of psychoanalytic terminology throughout the book.[13] This point was stressed further when he wrote in a 1959 analysis of his work that:

I came to psychology via Jung's Psychological Types and his Psychology of the Unconscious, the first of which initiated my interest in types of human nature, the second, my interest in unconscious processes as revealed by mythologies and religious imagery as well as in the more central and integral transformations of personality. What I gained from Freud was somewhat more specific and applicable in practice and, in due course, became so much a part of my regular and irregular modes of thought that there have been times when I took his huge gift for granted.[14]

A more provocative statement of this was made in "Psychology and the University," which Murray published in 1935 in the Archives of Neurology and Psychiatry. [15] In this article, he openly stated his advocacy of

psychoanalysis as the only approach to the science of psychology that adequately addressed the total complexity of human behavior. He called for the inclusion of psychoanalysis within the academic curriculum. This, he argued, would not only provide a needed counterpoint to the academic psychologists who "are looking critically at the wrong things," but would also lend scientific rigor to the psychoanalysts who "are looking with reeling brains at the right things." [16]

Murray had passed a draft of the manuscript of this paper around to a number of his friends and colleagues prior to its submission for publication. He was concerned not only about the critical reception of his views on psychoanalysis, but also that some of the statements he was making would offend his fellow academic psychologists. In particular, he was worried about how experimentalists would react to his characterization of psychological research as having:

contributed practically nothing to the knowledge of human nature. It has not only failed to bring light to the great, hauntingly recurrent problems, but it has no intention, one is shocked to realize, of attempting to investigate them. Indeed--and this is the cream of a wry jest--an unconcerned detachment from the natural history of ordinary mortals has become a source of pride to many psychologists. [17]

The comments Murray received about this manuscript were quite mixed. Dr. Alan Gregg, Murray's financial backer at the Rockefeller Foundation, said, "don't tone down your

article: I like pickles that taste of brine." [18] A contrasting opinion was expressed by Harvard's Edwin G. Boring, who warned Murray that he "would be persona non grata in the APA" if the paper was published. [19] Murray, however, decided that the issues raised in the paper needed to be aired even if they proved to be unpopular. In a letter to Gregg he stated that:

I feel that a bit of indignation at the shameless provincialism of the separate classes of psychology is appropriate at the present time, and I can better afford to make myself the goat than many others who feel as I do but whose very existence depends on academic favor. [20]

By 1938, Murray's opposition to the theories and methods of traditional experimental psychology had hardened to the point where he would in the Explorations describe his academic colleagues as:

peripheralists. . . . [They] have an objectivistic inclination, that is, they are attracted to clearly observable things and qualities. . . . In this respect they are positivists. . . . If [they] ever do indulge in speculations about what goes on within the brain, they usually fall back upon the conceptual scheme which has been found efficient in dealing with simpler partial functions. They resort to mechanistic or physiological explanations. . . . [They] have come to adopt the extreme behavioristic point of view. These are the holy zealots, the modern puritans of science. . . . The peripheralists are . . . men addicted to the methodology of science. Being chiefly interested in what is measurable, they are forced to limit themselves to relatively unimportant fragments of the personality. [21]

Given his adherence to psychodynamic theories, it is important to note that while Murray had initially been converted to a career in psychology as a result of his

personal contact with Jung and had remained a devoted friend and follower, Murray did feel that Jung's theory did not readily lend itself to scientific investigation. He saw Jung's work as being far too speculative and philosophical to be defined in the concrete terms necessary for research application.[22]

The 1937 paper on clairvoyant dreams is important in this respect because Murray's rejection of the clairvoyance hypothesis marked a significant departure from the Jungian influence. Jung's concept of synchronicity--the acausal interaction between events separated in space and time and yet giving rise to a meaningful psychological state--had been viewed as an explanation for paranormal phenomena such as clairvoyance.[23] In rejecting the hypothesis that dreams have a significance beyond the immediate experience of the dreamer, Murray was in effect rejecting a major aspect of the Jungian concept of the collective unconscious and its dynamic role in the psychological growth of the individual. As Murray later said, "at no time. . . was I a good Jungian, a good Freudian, a good Adlerian, or a good schoolman of any breed." [24] Even considering the great personal and intellectual debt that he owed to Jung, Murray was too well trained as a scientist to let such prejudices stand in the way of his data.

Instead, Murray turned to the writings of Freud for concepts that could be translated into testable hypotheses for empirical study. These concepts, according to Murray, were:

(1) evidences of the theory of unconscious psychic processes and their effects, (2) evidences of the determining importance of early family relations and of the experiences of childhood, of the persistence of complexes established in those years, (3) countless illustrations of the multifarious manifestations of the sex drive, (4) divisions of the personality into id, ego, and superego (conventional constructs), [and] (5) definition of several mechanisms--repression, isolation, denial, etc.--that operate in the service of adjustment.[25]

In the Explorations Murray also discussed in detail his adoption of the psychoanalytic tripartite division of the personality. His position was orthodox Freudian in regards to the Id as "the generic term under which all innate drives are subsumed," and the Superego as the "organization of 'Do's' and 'Dont's' preached and perhaps practiced by the parents, asserted to be the only 'Right,' sanctioned by religion and. . . internalized as a complex institution, known commonly as conscience." [26]

Another concept that Murray borrowed from Freud's work was that of projection. This issue was discussed in particular detail in a 1933 article which presented the results of a study on the effect of one's emotional state upon the process of judging others' personalities.[27] Subjects who were asked to make a personality judgement based on a photograph of the stimulus person after having

experienced a fear provoking event, made judgements of a significantly more malicious nature than did subjects who did not experience fear prior to making the judgement.[28]

The importance of this paper for Murray's work was his use of G. F. Stout's concept of apperception to describe the process whereby a sensory stimulus had meaning attributed to it.[29] This process was contrasted with simple cases of perception which Murray limited to the conscious recognition of pattern in the sensory stimulus. Murray argued that in many cases the process of apperception was unconscious; the apperceiving person was not aware of the reason he or she attributed specific meaning to the sensory stimulus. This, he speculated, could also account for the misinterpretation of sense data when elements of an unrelated emotion are unknowingly projected onto the perceived stimulus. In this study, Murray saw his findings as supporting this interpretation of the apperceptive effect of fear on judging personalities, since the subjects generally claimed to be unaware of feeling particularly fearful while in the act of making the personality judgements. The significance of this discussion went well beyond the interpretation of this specific set of data, for Murray publicly raised for the first time the conceptual issues that were to provide the basis for his work with Christiana D. Morgan in developing the Thematic Apperception Test (TAT) in 1935.[30]

Morgan, a client and student of Jung's during Murray's 1925 visit to Zurich, had achieved notoriety in Jungian circles for her ability to produce "visions" of enormous symbolic wealth. Returning to her family home in Cambridge in 1927 to set up a therapeutic practise, Morgan renewed her acquaintance with Murray. This led to her appointment as a part-time assistant at the Clinic, an association that would last for fifteen years. Because of her visionary experiences, Morgan and Murray recognized the importance of imagination and fantasy in revealing hidden psychological processes. However, they also felt that few people could take such flights of fantasy unaided. Therefore, they set out to develop a structured technique to assist the subjects' imaginal processes.[31]

Murray and Morgan first presented their work on the TAT in the Archives of Neurology and Psychiatry. They stated that the rationale behind the technique lay in:

the well recognized fact that when some one attempts to interpret a complex social situation he is apt to tell as much about himself as he is about the phenomena on which attention is focused. . . . The process involved is that of projection--something well known to analysts.[32]

In using the TAT, subjects were shown a series of ambiguous pictures:

each of which depicts a different dramatic event, with the instructions to interpret the action in each picture and give an imaginary reconstruction of the preceding events and the final outcome.[33]

After presenting the procedure for using the TAT, Murray and Morgan then interpreted the TAT responses of two subjects.

They felt that these responses were indicative of their subjects unconscious motivational states, and the interpretations made heavy use of orthodox Freudian terminology.[34]

The process of apperception, or, the assignment of meaning to perceptual stimuli, can be traced to Murray's previous paper on the effect of fear on personality judgements, wherein the psychoanalytic conception of projection was contrasted with Whitehead's views on perception.[35] In the paper on the TAT, however, the psychoanalytic position was stressed exclusively by Murray and Morgan. The responses/fantasies that were reported by their subjects were interpreted using psychoanalytic terms whenever possible. They felt that this was appropriate because:

This... represents the beginning of a classification--the initial step in the construction of any science--and the practice should be continued until all important fantasies have been so recognized.[36]

Murray's main divergence from the orthodox Freudian position occurred in his discussion of the Ego. He did not view the Ego as subservient to the Id and Superego. Instead, he emphasized the ability of a "strong Ego" to engage in "conscious, freely-willed acts" including the formation of long-term goals and interests and the active mediation of conflicts between innate drives and social conventions.[37]

Murray, therefore, found the orthodox psychoanalytic view of drive as merely the expression of sexual and aggressive tendencies to be far too limited. Based on his earlier research on embryonic development and on his study of Jungian theory, Murray felt that the goal directedness of behavior was as often proactive as it was reactive. Behavior was not just a function of blind response to basic urges and/or their socially-conditioned inhibitions, but often involved working toward long-term goals implying expectations about the future status of the organism.[38] For this reason Murray concluded that the writings of McDougall on human "instincts" were more appropriate to the study of personality than Freud's theory of sexual motivation. McDougall's lengthy list of instincts provided for both proactive and reactive forms of motivation. Murray was aware, though, of the inherent and potentially insoluble biological problems that accompanied the assumption of specific inherited human patterns of thought and behavior. This problem consisted of having to posit the existence of a genetic factor for each trait. The way around this problem, according to Murray, was to maintain the directional component of the concept of instinct, while assuming that only those instincts directly related to physiological survival had a biological bases. The rest of the instincts could then be assumed to be of psychological origin, having arisen as the result of the socialization process.[39] Having thus satisfied his concerns about the

problems with McDougall's concept of instinct, Murray then reinterpreted McDougall's theory to form the basis of his own model of human motivation.[40]

In Murray's opinion, however, human action was a complex phenomena that involved much more than just motives. Actions invclved both the motivational state of the subject and the stimulus-environment in which the subject existed, the former being referred to by the term need and the latter by the term press. Murray did point out that if the need-press mdel was used to describe discrete events, the resultant analysis would be more like behavioral stimulus-response psychology than psychoanalysis.[41]

The introduction of the need-press model of human action was the most significant theoretical contribution of Murray and Mergan's paper on the TAT. The need-press model was developed further in a 1936 publication, "Basic Concepts for a Psychology of Personality." [42] In this paper Murray reaffirmed his intent to analyze the functional organization of human acticn in terms of needs and press. Needs were further defined as the conscious or unconscious perception of an internal state (primary or biogenic need), or external situation (secondary or psychogenic need). Needs served to organize behavior towards achieving a specific end state by means of an appropriate pattern of behavior. This directional organization of behavior was further described as not necessarily involving conscious purpose on the part

of the acting organism, but as being potentially similar to physical phenomena occurring as a function of the process of chemical equilibria and/or the Second Law of Thermodynamics.[43]

Murray again noted the similarity between his concepts of need and press and the behavioral concepts of stimulus and response. However, he now made the distinction between stimulus and response, which were generally used to describe situations involving a single environmental excitation followed by a single reflexive reaction, and need and press, which referred to organized trends of action occurring in a more inclusive environmental context, or press. Murray thus defined press as a temporal gestalt of stimuli that are perceived as a whole because they either have the same subjective meaning to the perceiver, or because they represent different aspects of a larger perceptual event. In either case, the awareness of press is ultimately connected for the perceiver with the development of psychogenic needs.[44] Murray saw this distinction between need-press and stimulus-response as crucial for the investigation of human personality because:

The stimulus-response pattern is adequate for reflexes (fixed relations), but when, as is usually the case, it is a matter of complex behavior, we must use the p[ress]-n[eed] formula.[45]

Murray anchored this discussion of the need-press model of personality within a theoretical framework of central controlling psychological processes. Arguing that the debates between behaviorists and introspectionists (here loosely defined as anyone studying consciousness) had divided psychology into two exclusive camps, Murray called for a new synthesis that could account for the physiology of behavior as well as for subjective experience.[46] Murray's proposed synthesis centered on the concept of regnancy, which he defined as "the organized aggregate of mutually dependent [brain] processes which momentarily determines the functioning of the organism 'as a unit.'"[47] The regnancy involved the neural processing of sensory and motor impulses and was dependent on the physical condition of the brain and its supporting fluid medium. Consciousness was seen as an aspect of some regnant processes, a view similar to the double-aspect solution to the mind-body problem. Murray was quick, though, to point out that a double-aspect view of consciousness did not mean that all regnant processes had to necessarily have conscious correlates, thereby concluding that regnant processes could also be unconscious.[48] Therefore, the need-press model of personality was interpreted by Murray as reflecting an organized pattern of neural activity governing a person's behavior during a given segment of time. Personality thus became analyzable as a system of interrelated processes, a position reminiscent of the biochemical theories of Henderson and Cohn, in

particular Henderson's view of blood chemistry as a system regulated by the presence of hemoglobin, and the physical theory of Gibbs.[49]

The concept of need was treated in further detail in the 1937 article, "Facts which Support the Concept of Need or Drive." [50] Murray first focused on the distinction between the action (he used here the term *actone* to refer to action *qua*-action) instigated by a need, and the effect of the action. Murray made the point, basing his argument on the work of B. F. Skinner, that "effects are more fundamental to life and occur more regularly than any observable action pattern." [51] Therefore, needs were seen as more readily analyzable from an investigation of the outcomes of behavior than from behavior itself.

Murray proceeded from this point to argue that studying the effects of behavior led one to the conclusion that behavior was ultimately organized and directed, and that psychology could not then develop an intelligible account of human action without a recognition of the concept of need. [52] Furthermore, since needs were directed towards effects and the means to the effects were of only secondary importance, the directional influence of the need was of a finalistic rather than mechanistic nature. This, Murray felt, clearly placed the concept of need within the conceptual framework of thermodynamics and biological equilibria. [53]

After restating these basic issues in the Explorations, Murray proceeded to argue for the theory of needs requiring a psychological explanation of energy. Drawing on the work of McDougall, he declared that not only was there an energetic aspect to needs, but that the transformation of a need into action involved the expenditure of energy as well. Murray then proposed the hypothesis that needs and actions both drew their energy from a common source, possibly related to the basic processes of metabolism.[54]

Murray then presented a lengthy (100 page) chapter devoted to the description of a new taxonomy of personality. Murray felt that the personality researchers of his day generally held a far too limited scope for their research. Individual researchers tended to focus on a single topic, with the discipline-wide result of a fragmented view of personality characterized by a hodge-podge of classification schemes. In response to his perception of these problems, Murray felt that a new systematic taxonomy was needed. This, he argued, would bring psychology in line with biology and the rest of the life sciences, which he considered to be a more appropriate disciplinary model for psychology than physics.[55]

Murray acknowledged that the taxonomy he was proposing was at best tentative, but he was convinced that this first step had to be taken.[56] In all, Murray proposed a total of twenty needs (notationally identifiable by the prefix "n")

and their attitudinal counterparts as follows:

1. nAba = nAbasement (Abasive attitude).
2. nAch = nAchievement (Achievant attitude).
3. nAff = nAffiliation (Affiliative attitude).
4. nAgg = nAggression (Aggressive attitude).
5. nAuto = nAutonomy (Autonomous attitude).
6. nCnt = nCounteraction (Counteractive attitude).
7. nDef = nDeference (Deferent attitude).
8. nDfd = nDefendance (Defendant attitude).
9. nDom = nDominance (Dominative attitude).
10. nExh = nExhibition (Exhibitionistic attitude).
11. nHarm = nHarmavoidance (Fearful attitude).
12. nInf = nInfavoidance (Infavoidant [shy] attitude).
13. nNur = nNurturance (Nurturant attitude).
14. nOrd = nOrder (Orderly attitude).
15. nPlay = nPlay (Playful attitude).
16. nRej = nRejection (Rejective attitude).
nSec = nSeclusion (Seclusive attitude). This need has been taken as the opposite of Exhibition, not as a separate variable.
17. nSen = nSentience (Sentient attitude).
18. nSex = nSex (Erotic attitude).
19. nSuc = nSuccorance (Succorant attitude).
nSup = nSuperiority (Ambitious attitude). This need is considered to be a composite of Achievement and Recognition [see also Exhibition].
20. nUnd = nUnderstanding (Intellectual attitude).[57]

Once Murray had set forth this list of needs he then discussed each one in much further detail. He gave a complete definition of each need in terms of its intended effects, emotional states, and trait names. Each need was also discussed in terms of the press, sensory modalities, and physical and verbal actions most commonly associated with it. Finally, each need was discussed in regards to its relation to other needs. Particular attention was given to issues of fusion and conflict between different needs, and to the relative priorities among them.[58]

Needs and press, however, were not the ultimate level of psychological analysis for Murray. Instead, he proposed the more wholistic concept of thema in his 1934 papers on the psychology of humor. Thema was defined as "the dynamic constitution of the total situation." [59] Murray argued that the unit of observation for psychological research should not be the most elemental, yet observable event, but a larger and more intuitively meaningful event. For example, the subject of the joke and the reaction to it was the level of analysis in these studies, rather than specific elements of the joke and specific elements of the response. Put another way, the joke was a meaningful whole, and its effect could not be adequately assessed through using a reductionistic research methodology. Likewise, in Murray and Morgan's 1935 article on the TAT, the responses of the subjects were seen as representing a complex meaningful event, or thema. [60]

While this argument was reminiscent of the Gestalt critiques of experimental psychology, and also of Murray's interest in the psychological insights afforded by a study of literature, the concept of thema was more closely related to Alfred North Whitehead's philosophy of organism. Specifically, Murray has cited Whitehead's discussion of an event as an organic combination of perceptual entities in support of the concept of thema. [61]

The theory of personality espoused by Murray in the Explorations can thus be seen as a synthesis of a number of different theories--"the incorporation into the sphere of academic concern of a large portion of Freud's theoretical system integrated with contributions from Jung, Adler, McDougall, Lewin, and others." [62] What held this assemblage of theories together for Murray was a metatheory of human nature that he developed from his research in biochemistry. As he later stated:

what seemed both most obvious and most important about the interior of the embryo were (a) the givenness, the inherent spontaneity, of its cellular activities and (b) the continuous sequence of orderly metamorphoses. . . which resulted from these activities, and hence the necessity of including formative (constructive) processes in one's scheme of variables. . . .

Also, it appeared that organic processes are not only primarily endogenous, autonomous, and proactive (initiated and sustained from within. . .), but especially in the early stages of development are. . . not perfectly coordinated with other processes, not constantly directed toward the achievement of effects extrinsic to themselves. [63]

As a result of this view of the organism as a self-directed, not always perfectly integrated, but growing entity, Murray emphasized the developmental study of process and creativity in personality. He put this view into practice through the use of long-term investigations of small groups of subjects where projective tests of imaginal processes were combined with the standard experimental techniques and clinical diagnostic procedures to analyze the dynamics of the structure and transformations of

personality.[64] These transformations of personality were thought by Murray to occur within a generally homeostatic framework. Based on his study of Henderson's The Order of Nature, and their collaborative research on the chemical equilibria of the blood, Murray was convinced that the functioning of the human organism was hierarchically organized to achieve a physical, psychological, and social stability that yet remained flexible enough to allow for growth and adaptation.[65]

The Influence of Henderson and Whitehead

Analysis of this theory raises several issues concerning the influence of Whitehead's philosophy on Murray's work. First, it indicates that Whitehead's influence was wider, but probably less crucial, than has previously been thought. Murray was never a close friend of Whitehead's, knowing him only through the circle of Harvard intellectuals that gathered around L. J. Henderson. Murray made a point of keeping in daily contact with Henderson, a practice that dated to the year he had spent working in Henderson's laboratory in 1920, and he no doubt first met Whitehead during one of these visits. This personal contact convinced Murray to undertake a study of Whitehead's philosophical writings.[66]

The standard interpretation of Whitehead's influence on Murray is that Whitehead's proposal of an organismic metaphysics in Process and Reality was the source of Murray's insistence on studying individual personalities as meaningful thematic totalities.[67] The 1933 paper on the effects of fear upon personality judgements also shows that Whitehead's influence was not limited to Murray's reading of Process and Reality. [68] Murray was attracted as well to Whitehead's discussions of the causal efficacy of symbolic stimuli, that is, the ability of perceptual events to automatically arouse internal physiological states that could then have emotional labels attached to them. This provided philosophical support for the conceptual foundation of the process of apperception.

It seems incorrect, however, to credit Whitehead as the major source of Murray's ideas. While Whitehead's emphasis on an organismic metaphysics did influence Murray's views on the thematic totality of personality, an even greater precedent for this position can be found in the work of L. J. Henderson. This is a point that Murray has made but not drawn attention to. For example, in 1959 he stated that:

Perhaps my most influential basic model is that of biochemical metabolism. . . [which] has led me to conceptualize. . . systems of participant entities and participating processes. [69]

Henderson viewed the blood as a complex chemical system of interacting variables, where a change in one variable would

set in motion a process of equilibrium that would alter the other variables in such a way as to bring the entire system back to its original state.[70] In a similar vein, Murray and Alfred E. Cohn saw the process of embryonic development as being a function of the total condition of the developing organism.[71] There was a conceptual similarity between these models of biological processes and Whitehead's philosophy of organism. Henderson noted this similarity, and became a champion of Whitehead's philosophy among the scientific community, partly to proclaim a philosophical justification for his own interactionist research methodology.[72]

This can also be demonstrated through an examination of the introduction to the Explorations in Personality of 1938. Murray began by stating that:

This Book is Gratefully Dedicated
by its authors
to . . .
LAWRENCE J. HENDERSON
whose expositions of scientific procedure
established a methodological standard,
to
ALFRED N. WHITEHEAD
whose philosophy of organism supplied the
necessary underlying generalities.[73]

Murray then proceeded in the body of the book to state that Henderson's methodological influence was instrumental in determining the nature of the theory of personality that was under investigation;

In defining [our variables] and building up our theory of the total personality, we attempted to proceed systematically according to certain principles. . . . In working out our method of

approach we were greatly influenced by Professor L. J. Henderson of Harvard.[74]

In contrast to this explicit statement of intellectual indebtedness, Murray's references to Whitehead in the Explorations show him to be supporting, not determining Murray's methodology and theory. For example, Murray cited Whitehead in regards to his emphasis on studying individuals rather than group averages, his avoidance of mechanistic concepts, and his views on the dynamic nature of motivation.[75] In each of these cases, however, Murray cited other sources as his primary references and only used Whitehead to provide philosophical support for his positions. The emphasis on studying individuals rather than group averages Murray derived from his experience with medical diagnostic practice and also from Kurt Lewin's views on statistical averages obscuring the different motives behind apparently similar responses.[76] Murray's rejection of mechanistic explanations in favor of dynamic interpretations of motivation were credited to the writings of McDougall and the psychoanalysts, as well as to the works of Claude Bernard, Henderson, and Walter Cannon on the dynamic equilibria of living systems. Murray even credited the biological theories of E. S. Russell and W. E. Ritter (as well as the indirect influence of Josiah Willard Gibb's systems theory as transmitted through Henderson) as sources for his organismic theory of personality.[77]

Furthermore, Murray and Whitehead differed greatly in their views on the organization of human personality. Murray held that personality was highly structured; that it consisted of conscious and unconscious elements; that it was composed of the functional units of id, ego, and superego; and that it could further be analyzed as a system of various needs and press. Whitehead, on the other hand, was uninterested in the structure of personality. He was interested, instead, in searching for an understanding of what he termed the "self-identical Soul-Substance." [78] By this Whitehead meant that his major concern was in discovering what accounted for the formless experience of personal unity, an experience that transcended states of consciousness and transitory needs.

As a result of this analysis it must now be argued that previous authors have overestimated Whitehead's influence on Murray's psychological research. Murray, like Henderson, used Whitehead's philosophy of organism primarily to support theoretical and methodological beliefs that he already held. Murray derived his ideas from established biochemical and psychological sources as well as personal experience, and then used Whitehead's writings to demonstrate philosophical support and intellectual authority for his views.

Proposal of a New Methodology

As has been shown, Murray's theory of personality was wholistic, emphasizing the essential mutual relationship of the whole person with the elements of his or her personality. This made it necessary for Murray to develop a research methodology that would adequately address the whole person. The way to do this, he concluded, was:

to design methods appropriate to the variable we wished to measure; in case of doubt, choosing those that crudely revealed significant things rather than those that precisely revealed insignificant things.[79]

A number of general principles were proposed by Murray to guide the course of his research at the Clinic. Subjects were to be studied in a variety of situations by different researchers using appropriate experimental and diagnostic techniques, with the stipulation that the experimental situation be as life-like as possible and that the subjects not know the true purpose of the experiment. Each experiment was to be observed by a second, hidden researcher. This would provide a means of obtaining an estimate of the reliability of the primary researcher's collection and interpretation of the data. Finally, the subjects were to be given an opportunity to voice their own reactions to the experiments in order to elicit data that might not have been uncovered during the formal parts of the studies.[80]

Murray further proposed the study of fantasy as a major means of collecting data on the dominant themes of a subject's personality. Since themes were seen as complex entities composed of needs and press and organized by regnancies, he reasoned that a systems model of research was most appropriate. Multiple tests of fantasies providing converging sources of data, Murray claimed, were necessary for achieving descriptive certainty about the subject's personality.[81] He described in detail fifteen tests of fantasy, including such techniques as free association, picture completion, the TAT, and direct questioning. He also discussed the order of the presentation of the tests, and suggested the use of multiple experimenters to control for experimenter bias.[82]

Murray then commented on several methods for validating the results of these tests of fantasy. Again, the emphasis was on the use of multiple methods as the basis of his research methodology. Correlating the results of the various tests with each other and with biographical data provided the first step in validating the experimental results. If these initial validity checks proved positive, further tests could be made where fantasy themes would be used to predict future behaviors and unreported past behaviors. These predictions could then either be confirmed or disconfirmed through collecting more detailed biographical data, or by experimentally eliciting the new behavior of interest.[83]

This research program was overseen by a group of experienced researchers--composed of Murray, Dr. William G. Barrett, Robert W. White, Saul Rosenzweig, H. Scudder Mekeel, Christiana D. Morgan, and Erik Homburger [Erikson]--called the Diagnostic Council.[84] The researchers who were chosen for membership on the Diagnostic Council all had had either long-term working relationships with Murray, and hence substantial familiarity with his theoretical and methodological views, or experience as psychotherapists. This reflected Murray's opinion that the primary guarantee of the validity of their research findings came from pooling the superior judgmental skills of a number of trained diagnosticians. In Murray's words:

The psychologist is and will always be the final judge of all questions pertaining to personality. No fine instrument can replace him. . . . Now, since in any group of experimenters there will always be some who have greater aptitude or how are more experienced than others, it is advisable to establish a diagnostic hierarchy. By weighting the opinions of the more competent, one gets the full benefit of superior judgements as well as of many judgements.[85]

For the research of the Explorations, fifty subjects were recruited in groups of thirteen through an advertisement placed with the Harvard Employment Office. All but fourteen were students at Harvard. They were contracted to come individually to the Clinic for three or four hours of experimentation a week in return for a payment of forty cents an hour. In all, the entire research program took around thirty-five hours to complete, which meant that

each subject was studied over a period lasting about two months.[86]

Each subject first was interviewed by Murray in order to determine if he was serious about participating conscientiously in the study. No subjects were judged to be insincere, so all were instructed to begin the first phase of the study by writing a fifteen page autobiography.[87] A strict format was to be followed in writing the autobiographies. Murray had all the subjects address the following issues: Family history, including parents, siblings, and other relatives; personal history, emphasizing early childhood development; school and college history; sexual experiences; other major life experiences; and finally, aims, aspirations, and opinions of self-worth.[88] After completing the autobiography, each subject met briefly with the Diagnostic Council so that an initial impression could be formed to aid in proposing hypotheses about the subject's personality to be tested in the experiments to follow.[89]

After these initial sessions, the research began in earnest. The subjects were first involved in a series of structured interviews, each designed to tap a different topic of importance. H. Scudder Mekeel asked the subjects about their childhood memories and family relations. Issues of sexual development were raised in discussions with William G. Barrett. Merrill Moore inquired about the

subjects' current personal problems. Finally, the subjects engaged in "casual" conversation with Eleanor C. Jones in order to elicit information that might have been missed in the more structured interviews.[90]

A series of paper-and-pencil questionnaires comprised the next phase of the research program. Karl Kunze administered a test designed to measure the extent to which the subjects' current attitudes influenced their predictions about future events. A test of the subjects' degree of psychological insight was developed by Murray, and he supervised the use of this test along with the use of Vernon and Allport's "Study of Values." The subjects' abilities and special aptitudes were assessed using a questionnaire designed by Ruth Peterson and Edward Inglis. Finally, the last of the tests completed by the subjects was developed by Kenneth Diven to measure their appreciation of various aesthetic experiences.[91]

The fourth phase of the research program consisted of a series of tests designed to measure the subjects' behavioral responses to various stimuli. Robert White tested their susceptibility to hypnotic suggestion. The subjects' performance on a series of repetitious tasks was correlated with their predictions of future success by Jerome Frank as a measure of their levels of aspiration. Saul Rosenzweig measured their tendency to repress memories of failure on an experimental task as compared to memories of success. This

test was used in another study conducted by E. H. Trowbridge, to determine how much difference there would be between immediate memory of failure, and memory of failure five weeks later. Donald W. MacKinnon and James A. Christenson carried out a series of interrelated experiments designed to measure the subjects' ethical standards and their willingness to violate societal prohibitions. Walter C. Langer studied simple sensorimotor learning in order to determine which personality variables were correlated with it. The final test of the subjects' behavioral responses was conducted by Carl E. Smith and Kenneth Diven. They studied conditioned emotional responses by pairing an auditory stimulus with an electric shock, and then monitoring the subjects' autonomic reactions using the galvanic skin response and the finger tremor response.[92]

The main part of the research for the Explorations came in the fifth phase, which was devoted to various projective tests of imaginal processes. The emphasis here was on the attempt to uncover the inhibited and repressed needs and feelings of the subjects, sources of data that would not likely be revealed by the preceding experimental techniques. For this purpose Christiana D. Morgan and Murray developed and administered the TAT. The subjects were presented with a series of twenty ambiguous pictures and given the following instructions:

This is a test of your creative imagination.
I shall show you a picture and I want you to make
up a plot or story for which it might be used as

an illustration. What is the relation of the individuals in the picture? What are their present thoughts and feelings? What will be the outcome? Do your very best.[93]

Other projective tests that were used included the Beta Ink Blot, Similies, and Minister's Black Veil Tests developed by David R. Wheeler; the Musical Reverie Test designed by Karl Kunze; Erik Homburger's Dramatic Productions Test; and the Rorschach Test administered by Samuel J. Beck.[94]

Concluding the experimental sessions was a series of miscellaneous procedures. Most of these were pilot studies for new questionnaires measuring some specific ability, interest, or opinion, and were generally not found to be of any use in providing new and useful data.[95] Two of these techniques, though, were found to be of use. Saul Rosenzweig developed a procedure by which reactions to a frustrating experience could be measured in a controlled setting, and Maria Rickers-Ovsiankina conducted clinical observations of controlled social interactions.[96]

During the course of the experimental sessions, the Diagnostic Council met regularly to discuss the most recent research findings and to reformulate the personality hypotheses that had been proposed for each subject at the initial conference. At the conclusion of the research program:

a meeting, usually lasting five or six hours, was held on each subject. At this meeting each experimenter read a report of his session with the subject. A specially appointed 'biographer' conducted the meeting. He opened with a short summary of the findings, made comments on each

report, and concluded with a psychograph, or reconstruction of the subject's personality from birth. After the psychograph was read, there was general discussion and, at the end, the markings of the subject on each variable were discussed and finally established by majority vote.[97]

One complete case study of a single subject is presented in the Explorations. The space devoted to this subject (98 pages) is testimony to the thorough, but time-consuming, nature of Murray's approach to psychological research.[98]

In Murray's own opinion, the research methodology he presented in the Explorations--the "multiform method of assessment, . . . which consists of multifarious procedures administered by multifarious specialists"--was his greatest intellectual achievement.[99] The importance of this methodology to Murray can be seen in the fact that virtually half of the Explorations was devoted to descriptions of the research techniques used and the way the data was distilled into a description of each subject's personality. This methodology, like the underlying theory, was a direct outgrowth of Murray's biochemical research with Henderson in 1920. Henderson saw all natural phenomena as being analyzable into a single interactive system.[100] Murray carried this belief in the usefulness of a systems analysis with him when he changed careers in 1926 to become Morton Prince's assistant at the Clinic. The systems model of research was then slowly but surely implemented in Murray's psychological research through the development of many

different techniques to achieve converging information about the different aspects of the total personality.

However, it was not possible for Murray to implement this systems model of psychological research simply by measuring each subject using many different techniques. A method for analyzing and integrating the vast amounts of data thus generated also needed to be developed, because techniques for the statistical analysis of multiple sources of data were not readily available at that time.[101] For a solution to this problem, Murray turned to his training in medicine. He felt that it would be:

possible for a group of trained collaborators. . . to make a reasonably complete examination, formulation, and appraisal of the whole person. . . . This objective is achieved over and over again on the physiological level by practitioners of medicine.[102]

In developing his methodology, Murray used the model of the teaching hospital's "Grand Rounds" and applied it to the psychological research setting. Each experimenter at the Clinic acted like a medical specialist in being responsible for the application of a specific research technique, and in forming a hypothetical personality "diagnosis" based on his or her data. These research findings and personality hypotheses were then discussed by the Diagnostic Council in much the same way the hospital's specialists would discuss the diagnoses of their co-workers. Through group discussion each member of the Diagnostic Council had the opportunity to critique the personality formulations presented by the other

Council members, as well as having to respond to others' criticisms of his own hypotheses. In this way the Council members could converge on an analysis of each subject's personality that was acceptable to all.[103]

Conclusion

In summary, Murray drew heavily upon his training in the life sciences and medicine in developing the psychological theory and research methodology presented in the Explorations. He saw his approach to the study of personality as being in opposition to mainstream academic psychology. He was interested in "people, their doings and their ills," in contrast to those psychologists who were "addicted to the methodology of science. . . [who] limit themselves to relatively unimportant fragments of the personality." [104] He called for a broadening of the intellectual boundaries of academic psychology to include psychoanalysis and medicine in an eclectic combination with the most useful and humanly relevant aspects of traditional academic psychology. In this effort Murray drew heavily upon his training in the life sciences, and on his interest in psychoanalytic theory.

Murray's magnum opus was, as the title made clear, an exploration, a tentative first step towards a comprehensive study of human personality taken by a man who was also taking his first steps within the discipline. As a new

venture, the research program of the Explorations was fraught with problems and limitations. But this, Murray felt, was as it should be. He believed that all new scientific programs (and scientists) had to start somewhere, even if:

we . . . ventured further than our scientific consciences advised. We did not always hug the solid shore of fact, but sometimes let reflection take us out to sea--'very much at sea,' will be our critics' comment. For us this loosening of constraints was a desirable refreshment which allowed the inclusion of impressions that were beyond the subtlety and flexibility of our abstractions. It did not delude us into thinking that we were voyaging on terra firma. But now we have come to a landing, as far up the beach as possible; there to write our findings in the sand, since we have nothing to carve in stone, our work being merely a prelude.[105]

Reference Notes

1. Henry A. Murray, "Henry A. Murray," in A History of Psychology in Autobiography, vol. 5, eds. E. G. Boring & G. Lindzey (New York: Appleton-Century-Crofts, 1976), p. 294.
2. Gordon W. Allport to Edwin G. Boring, 4 August 1936, in the Gordon W. Allport Papers, Harvard University Archives, Nathan Marsh Pusey Library, Harvard University, Cambridge, Mass.
3. Herbert Barry, Jr., Donald W. MacKinnor, and Henry A. Murray, "Hypnotizability as a Personality Trait and its Typological Relations," Human Biology 3 (1931): 1-36.
4. Ibid., p. 23.
5. Ibid., pp. 3-4, 7, 17-18, 19-20.
6. Ibid., pp. 27-34.
7. Ibid., pp. 17-22.
8. Edwin G. Boring to Gordon W. Allport, 7 December 1929; Edwin G. Boring to Henry A. Murray, 25 April 1934. Both letters are in the Edwin G. Boring Papers, Harvard University Archives; Henry A. Murray, Personal interview, 13 July 1981; Henry A. Murray, Personal interview, 3 August 1981; M. Brewster Smith, Personal interview, 28 August 1981; Silvan Tomkins, Personal interview, 26 August 1981; and Robert W. White, Personal interview, 2 July 1981. For a further discussion see Chapter 5, pp. 160-162.
9. H. A. Wolff, Carl E. Smith, and Henry A. Murray, "The Psychology of Humor: 1. A Study of Responses to Race-Disparagement Jokes," Journal of Abnormal and Social Psychology 28 (1934): 341-365; and Henry A. Murray, "The Psychology of Humor: 2. Mirth Responses to Disparagement Jokes as a Manifestation of an Aggressive Disposition," Journal of Abnormal and Social Psychology 29 (1934): 66-81.
10. David R. Wheeler and Henry A. Murray, "A Note on the Possible Clairvoyance of Dreams," Journal of Psychology 3 (1937): 309-313.
11. Ibid., p. 309.
12. Henry A. Murray, "Researches Conducted at the Harvard Psychological Clinic During the Year 1933," Series 200, Box 91, Folder 1095, in the Rockefeller Foundation Archives, Pocantico Hills, North Tarryton, N.Y.

13. Henry A. Murray, Explorations in Personality: A Clinical and Experimental Study of Fifty Men of College Age (New York: Oxford University Press, 1938), p. v.

14. Henry A. Murray, "Preparations for the Scaffold of a Comprehensive System," in Psychology: A Study of a Science, vol. 3, ed. S. Koch (New York: McGraw-Hill, 1959), p. 36.

15. Henry A. Murray, "Psychology and the University," Archives of Neurology and Psychiatry 34 (1935): 803-817.

16. Ibid., p. 811.

17. Ibid., p. 805. That Murray had solicited comments about this paper can be ascertained from Murray, "Henry A. Murray," p. 294; and Henry A. Murray to Alan Gregg, 9 October 1934, Series 200, Box 91, Folder 1095, in the Rockefeller Foundation Archives.

18. Alan Gregg to Henry A. Murray, 10 October 1934, Series 200, Box 91, Folder 1095, in the Rockefeller Foundation Archives.

19. Murray, "Henry A. Murray," p. 294.

20. Henry A. Murray to Alan Gregg, 9 October 1934, Series 200, Box 91, Folder 1095, in the Rockefeller Foundation Archives.

21. Murray, Explorations, pp. 6-9.

22. Murray, Interview with F. G. Nameche, 4 November 1968; Murray, "Preparations," p. 36. This point is only hinted at in the dedication of the Explorations (p. v.) when Murray referred to:

CARL G. JUNG
whose writings were a hive of great
suggestiveness.

23. Carl Gustav Jung, Synchronicity: An Acausal Connecting Principle, trans. R. F. C. Hull (Princeton, N.J.: Princeton University Press, 1973). See especially pp. v-vii, 15, 78n, 85, 107, 110.

24. Murray, "Preparations," p. 13.

25. Murray, "Preparations," pp. 36-37.

26. Murray, Explorations, pp. 135-137.

27. Henry A. Murray, "The Effect of Fear upon Estimates of the Maliciousness of Other Personalities," Journal of Social Psychology 4 (1933): 310-329, reprinted in

Endeavors in Psychology: Selections from the Personology of Henry A. Murray, ed. E. S. Shneidman (New York: Harper & Row, 1981), pp. 275-290.

28. Ibid., pp. 280-285, 290.
29. Ibid., p. 275.
30. Ibid., pp. 275-280, 286-289.
31. Henry A. Murray, "Postscript: Morsels of Information Regarding the Extraordinary Woman in Whose Psyche the Following Visions Were Begot," in C. G. Jung, The Visions Seminar, book 2 (Zurich: Spring Publications, 1976), pp. 517-521.
32. Christiana D. Morgan and Henry A. Murray, "A Method for Investigating Fantasies: The Thematic Apperception Test," Archives of Neurology and Psychiatry 34 (1935): 289-306, reprinted in Endeavors in Psychology: Selections from the Personology of Henry A. Murray, ed. E. S. Shneidman (New York: Harper & Row, 1981), pp. 390-391.
33. Ibid., p. 391.
34. Ibid., pp. 392-407.
35. Murray, "The Effect of Fear: 2," pp. 277-279.
36. Morgan and Murray, "Thematic Apperception Test," p. 394.
37. Murray, Explorations, pp. 137-138.
38. Murray, Explorations, pp. 135-136, 137-141; and Murray, "Preparations", pp. 20, 37-38.
39. Murray, Explorations, pp. 24, 37-38, 69, 84, 94-95; Murray, Interview, 11 November 1981; and Murray, "Preparations," p. 13.
40. Murray, Explorations, p. 84; Murray, Interview, 11 November 1981; and Smith, Interview, 28 August 1981.
41. Morgan and Murray, "Thematic Apperception Test," pp. 394-395.
42. Henry A. Murray, "Basic Concepts for a Psychology of Personality," Journal of General Psychology 15 (1936): 241-268.
43. Murray, "Basic Concepts," pp. 241, 255-260; and Murray, "Preparations," pp. 15-16.

44. Murray, "Basic Concepts," pp. 255-260, especially pp. 257-259.
45. Ibid., pp. 258-259.
46. Ibid., pp. 241-245.
47. Ibid., pp. 245-246.
48. Ibid., pp. 247-250.
49. Ibid., pp. 250-255. See also p. 17 above, and Chapter 3, pp. 76-78.
50. Henry A. Murray, "Facts Which Support the Concept of Need or Drive," Journal of Psychology 3 (1937): 27-42.
51. Ibid., p. 31.
52. Ibid., pp. 27-32, 39-42.
53. Ibid., pp. 39-40.
54. Murray, Explorations, pp. 129-134.
55. Ibid., pp. 5-11, 142-144.
56. Ibid., pp. 143-144.
57. Ibid., pp. 144-145.
58. Ibid., pp. 152-277. An example of Murray's treatment of the need for achievement is found on pp. 164-165:
59. Wolff, Smith, and Murray, "Psychology of Humor: 1," p. 343. In this case, as with the study on clairvoyant dreams, while the topic of research originated with the student co-authors, Murray provided the theoretical framework for the discussion of the results.
60. Morgan and Murray, "Thematic Apperception Test," p. 394.
61. Charles D. Laughlin, "Discussion: The Influence of Whitehead's Organism upon Murray's Personology," Journal of the History of the Behavioral Sciences 9 (1973): 252-253; and Murray, "Preparations," pp. 22, 31.
62. Murray, "Henry A. Murray," p. 304.
63. Murray, "Preparations," p. 15.

64. Murray, "Henry A. Murray," pp. 303-304.
65. Murray, "Henry A. Murray," pp. 289-290; Murray, "Preparations," pp. 17-19; Murray, Interview, 13 July 1981; Murray, Interview 14 October 1981; and Murray, Interview, 11 November 1981.
66. Laughlin, "Whitehead's Influence," p. 251; and Murray, Interview, 13 July 1981.
67. Laughlin, "Whitehead's Influence," pp. 251-254.
68. Murray, "Effect of Fear," p. 276; Alfred N. Whitehead, Symbolism: Its Meaning and Effect (New York: Macmillan, 1927), p. 88; and Alfred N. Whitehead, Process and Reality: An Essay in Cosmology (New York: Macmillan, 1929).
69. Murray, "Preparations," p. 23.
70. Lawrence J. Henderson, Blood: A Study in General Physiology. (New Haven, Conn.: Yale University Press, 1928). See also Chapter 3, pp. 76-79 for a further discussion.
71. Alfred E. Cohn and Henry A. Murray, "Physiological Ontogeny. A. Chicken Embryos. IV. The Negative Acceleration of Growth Rate with Age as Demonstrated by Tissue Cultures," Journal of Experimental Medicine 42 (1925): 275-290.
72. Lawrence J. Henderson, "A Philosophical Interpretation of Nature," Quarterly Review of Biology 1 (1926): 289-294; and Garland E. Allen, Life Science in the Twentieth Century. (Cambridge: Cambridge University Press, 1975), pp. 103-106.
73. Henry A. Murray, Explorations in Personality: A Clinical and Experimental Study of Fifty Men of College Age (New York: Oxford University Press, 1938), p. v.
74. Ibid., p. 25, 25n.
75. Ibid., pp. viii, 68, 72.
76. Ibid., p. viii.
77. Ibid., pp. 36-39, 39n.
78. Alfred N. Whitehead, Adventures of Ideas. (New York: Macmillan, 1935), pp. 239-241. For a discussion of Murray's views on the organization of personality, see pp. 192, 195-203 above.

79. Murray, Explorations, p. 26.
80. Ibid., pp. 26-28.
81. Henry A. Murray, "Techniques for a Systematic Investigation of Fantasy," Journal of Psychology 3 (1937): 115-143, reprinted in Endeavors in Psychology: Selections from the Personology of Henry A. Murray, ed. E. S. Shneidman (New York: Harper & Row, 1981), pp. 366-372, 381-382.
82. Ibid., pp. 372-381.
83. Ibid., pp. 385-388.
84. Murray, Explorations, pp. 29, 399-412.
85. Ibid., p. 27.
86. Murray, "Researches Conducted at the Clinic During 1933," pp. 4-5; and Murray, Explorations, pp. xi, 397.
87. Murray, Explorations, p. 397.
88. Ibid., pp. 397, 412-420.
89. Ibid., pp. 399-412.
90. Ibid., pp. 421-431. It must be noted here that each researcher was responsible for writing up the published description of his or her experimental technique.
91. Ibid., pp. 431-453.
92. Ibid., pp. 453-529.
93. Ibid., pp. 529-545. Quote taken from p. 532.
94. Ibid., pp. 545-582.
95. Ibid., pp. 582-585.
96. Ibid., pp. 585-603.
97. Ibid., pp. 29, 604-615.
98. Ibid., pp. 615-702. Robert W. White was chosen as the biographer for "The Case of Earnst," and was responsible for writing this case study. It is interesting to note that Murray used the term "biography" in reference to the case study reports. Possibly this reflects Murray's earlier interests in history and biography. See Chapter 3, pp. 69-70 for details.

99. Murray, "Henry A. Murray," pp. 303-304.

100. Murray, Explorations, pp. v, 25, 25n; Murray, "Henry A. Murray," pp. 289-290; Murray, "Preparations," pp. 17-19; Murray, Interview, 13 July 1981; Murray, Interview, 14 October 1981; Murray, Interview, 11 November 1981; Lawrence J. Henderson, The Fitness of the Environment: An Inquiry into the Biological Significance of the Properties of Matter (New York: Macmillan, 1913), pp. 282-300; and Lawrence J. Henderson, The Order of Nature: An Essay (Cambridge, Mass.: Harvard University Press, 1917), pp. 180-212.

101. Anthony J. Rucci and Ryan D. Tweney, "Analysis of Variance and the 'Second Discipline' of Scientific Psychology: A Historical Account," Psychological Bulletin 87 (1980): 166-184. Specifically, R. A. Fisher developed the ANOVA technique in 1925, but it was not used in psychological research until 1934, and then did not gain wide acceptance until 1948. In the Explorations, Murray and Richard Wolf did present several correlational analyses of interrater reliability. See in particular pp. 266-280. This discussion was reprinted in Richard Wolf and Henry A. Murray, "An Experiment in Judging Personalities," Journal of Psychology 3 (1937): 345-365. There is reason to believe that this chapter was added in response to criticisms that Murray had ignored any attempt to objectively test the validity and reliability of his research methods. See, Karl S. Lashley to James B. Conant, 6 January 1937, in the Gordon W. Allport Papers.

102. Murray, "Preparations," p. 11. See also, Murray, Explorations, pp. 703-704.

103. Murray, Explorations, pp. 26-27, 29, 604-606, 703-704; and Murray, Interview, 14 October 1981.

104. Murray, "Preparations," p. 9; and Murray, Explorations, p. 9.

105. Murray, Explorations, p. 703.

CHAPTER 7:

THE DECISION TO PROMOTE MURRAY, 1936-1937:

A CASE STUDY IN DISCIPLINARY BOUNDARY MAINTENANCE

Introduction

In the preceding five chapters a number of issues have been detailed concerning the development of American psychology, its status as an academic discipline at Harvard University, and the institutional and theoretical origins of Murray's work at the Clinic. The present chapter will focus on six of these issues and their impact on the debate surrounding Murray's promotion to Associate professor in 1937. Specific attention will also be given to detailing the relative importance of each factor in the decision that was ultimately reached.

The first two issues of importance deal with the generally middle-class orientation of the American university, and the concomitant emphasis on the practical applications of college training and research. Contrasted with this was the orientation of academic psychology at Harvard, an orientation noted for its rejection of application in favor of narrowly defined experimental

research. Issues three and four address the institutional factors involved in the operation of a research group like that directed by Murray at the Clinic. On one hand, there were the administrative criteria by which faculty hiring decisions were made, and teaching and research effectiveness were evaluated. On the other hand, there were influences resulting from the support that institutions received from external funding sources that sometimes ran counter to the policies of the local administration. Finally, the fifth and sixth issues concern Murray's lack of formal training in psychology, and the manner by which he drew on his education in the life sciences to create an interdisciplinary approach to theory and research on human personality.

Faculty Promotion at Harvard Prior to 1934

When he became Prince's successor in 1928, Murray was made an Assistant Professor and appointed to a three-year, renewable contract. As Harvard at that time had no defined policy dealing with faculty promotions, Murray could expect that his stay at the Assistant Professor level might be quite lengthy. Advancement to the tenured rank of Associate Professor was primarily dependent on the availability of guaranteed funding for a permanent position. Therefore, unless the Clinic was able to secure a substantially larger endowment, or administrative changes occurred to allow the Clinic to share in the Psychological Laboratory's

unrestricted funds, Murray could not anticipate advancement.[1]

This was the future faced by most of the junior faculty at Harvard. Advancement generally depended on the death or retirement of senior faculty, especially after the Depression had greatly reduced the rate at which new endowments were being received. Three-year term followed three-year term, until at long last one was advanced or one's contract was not renewed. By the mid-1930's, however, contract non-renewal had increasingly become the usual means of dealing with the junior faculty. The Harvard administration rationalized these terminations as being in the best interest of the individuals involved. Since there was little chance of advancement at Harvard, it was argued that the only way junior faculty members could further their careers was to seek a better job at another institution.[2] There was, however, a serious inconsistency in this argument. At the same time that junior faculty members were being terminated, ostensibly for their own good, to seek employment elsewhere, the Harvard administration was taking advantage of the economic crisis created by the Depression to recruit promising scholars from among the ranks of the many unemployed.[3] By 1933, this situation had deteriorated to the point where it was widely agreed that corrective measures had to be taken. Unfortunately for Murray, these measures were to raise the most serious obstacles to his continued presence at Harvard.

In November of 1933, A. Lawrence Lowell announced his intentions to retire from the Harvard Presidency at the end of the academic year.[4] While nearly universally praised for his successes as a fundraiser and builder of great institutional edifices, for several years there had been growing criticism of his faculty hiring practices. This criticism had even spread to the ranks of the Harvard Corporation. An eminent scholar specializing in English history, Lowell had based his plans for the development of Harvard upon the model provided by England's Oxford and Cambridge Universities. This led him to emphasize undergraduate instruction as a primary goal. To achieve this goal Lowell instituted the house plan and the tutorials.[5] Under this system, Harvard undergraduates were provided room and board in University residence halls, and their education was monitored through frequent meetings with a tutor who was responsible for only a few students.[6]

The success, or failure, of the tutorial system depended on Lowell's ability to attract to Harvard Young scholars willing to be tutors, or to take on tutoring along with the usual Assistant Professor's duties. But, by making the tutorial system work, Lowell fueled his critics' concerns that he was neglecting the overall quality of faculty scholarship at Harvard. It was rumored that the desire and ability to tutor were far more important than research excellence for getting, and holding, an appointment at Harvard. This, coupled with Harvard's haphazard system

of promotion and tenure, raised concerns that Harvard was following a course that would lead it from its status as the preeminent scholarly center in the United States to that of a second-rate university.[7] These concerns were reflected in a joke popular among the Harvard faculty in the early 1930's, the punchline of which was: "the highest academic honor is to receive a call to Harvard--and to refuse it." [8]

One of Lowell's most vocal critics was James B. Conant, Chairman of the Department of Chemistry at Harvard. A proponent of the German, rather than the English, model of the university, Conant argued for increasing Harvard's emphasis on scholarly research and decreasing the emphasis on undergraduate instruction. Reflecting back upon this period of time, Conant wrote in his autobiography that:

A university . . . was a collection of eminent scholars. If the permanent professors were the most distinguished in the world, then the university would be the best university. The quality of those appointed to life positions was therefore fundamental. If a man was made a professor for personal reasons, as I believed was happening, or even for being a helpful member of a department or for devotion to Harvard, then Harvard was to that extent betraying its trust. Out of my own experience as a scientific investigator, I knew how few positions there were in 1932 in the United States where a professor could carry on research. To fill one of these positions with a second-rate person was to betray a trust--to be guilty of almost criminal negligence.

. . . Of course, I was not speaking or, indeed, thinking solely in terms of natural scientists. I knew from my friends in the humanities and social sciences what was happening in these areas, and I thought the present policy was leading Harvard downhill.[9]

Conant's outspoken criticisms of Lowell's faculty hiring practices quickly brought him to the attention of the members of the Harvard Corporation. Several members of the Corporation who held similar views, in particular Charles P. Curtis and Robert Homans, began to promote Conant as Lowell's successor. They stressed the point that Conant would bring to the Presidency the spirit of reformation so desperately needed if Harvard was to remain a vital intellectual community.[10] Curtis and Homans were aided in this effort by Lawrence J. Henderson. Henderson was related by marriage to Conant's wife, and through his various social and professional contacts with Conant had concluded that, of the several candidates under consideration for the position, Conant was best suited emotionally to make the difficult decisions that faced the Harvard President. Henderson then used his personal prestige to influence the Corporation towards favoring Conant as the next President of Harvard.[11]

Lowell had removed himself from any active role in choosing his successor, but was understandably concerned about the growing support for Conant's candidacy. As well as being opposed to the tutorial system, Conant had repeatedly expressed little concern for, or knowledge of, University affairs not dealing directly with the Chemistry Department. Not only did Lowell fear for the continuation of the policies he had worked hard to implement, but also for the future of the liberal arts at Harvard.[12] After

much persuasion by Henderson, though, Lowell finally resigned himself to the growing support Conant had among the members of the Corporation. This cleared the last obstacle that could have prevented Conant's election. On April 24, 1934, the Harvard Corporation unanimously elected Conant as Harvard's new President.[13]

President Conant and the Harvard Meritocracy

While the realities of the position quickly forced Conant to moderate his plans to reform Harvard, he did not hesitate to make it quite clear what his ultimate priorities were. In the "Report of the Committee to Visit Harvard College" for the academic year 1933-1934, it was noted that:

President Conant has announced that he intends to direct his most earnest and continuous thought to the problem of maintaining and strengthening the faculty. . .

Now a faculty can be strengthened only by the injection of new blood into its academic body. One way is to bring in distinguished scholars and teachers from other centers of learning. This method eliminates some of the incalculable risks of faculty appointments since it makes possible the appointment of "winners," after they have won. . .

This is one aspect of the faculty problem, but there is another, which also deserves attention. Some provision should be made for the orderly promotion of brilliant young scholars and teachers who are already connected with the University. . . Harvard is particularly fortunate in having such a body of reserves to draw upon. For various reasons, however, the University has been slow to take advantage of this special opportunity.[14]

Conant chose to pay special, personal attention to this latter issue in order to ensure that research excellence became the primary standard of faculty performance. Regarding the promotion of Harvard's junior faculty, he stated that:

Over a period of years the new appointments and promotions to permanent positions determine the fate of any college or university. In bringing his recommendations to the Corporation for action and to the Board of Overseers for confirmation, the President must be prepared to exhibit the evidence on which he has based his decisions. Such matters cannot be delegated to deans of faculties or departmental chairmen,--they constitute the first charge on the President's time. As far as possible I shall endeavor to sit with the ad hoc committees which in most of our faculties consider each appointment. With the advice of these competent groups I trust that a discerning judgement may be passed on our younger men and that those who are promoted or called to professorships may be worthy followers of their predecessors who have made this University great.[15]

With this statement, a merit-based promotion and tenure policy was instituted at Harvard.

Murray had been appointed to a second three-year term as Assistant Professor in 1931, without any serious questions being raised about his qualifications for the position. However, when he came up for reappointment again in 1934, Murray was concerned that there was a real possibility that his contract would not be renewed. Questions were being raised in administrative circles about Murray's lack of research publication. From 1928, when Murray became the Clinic's Director, through 1931, only

three articles were published based on research being conducted at the Clinic.[16] No further publications were to issue from the Clinic until 1933, by which time Murray had become aware of the possibility that his lack of publication might hinder his chances for reappointment.[17] To remedy this problem, Murray, his colleagues, and students at the Clinic had commenced upon a frenzy of writing articles for publication. Beginning in 1933 and continuing through 1934, a total of nine publications were produced by members of the Clinic. Murray was an author of three of these papers, but by far the most prolific of the Clinic's writers was Saul Rosenzweig, author of six of the publications.[18]

However, the questions raised concerning Murray's lack of publication could not be entirely answered through a series of journal articles. The major unanswered question concerned the publication date of the magnum opus which Murray intended to be the result of his various studies of personality. This work was to be based on a new approach to psychological research that he was developing at the Clinic. He had been announcing its imminent publication since 1931, but by 1934 it had yet to appear. Sometimes, Murray referred to this project as involving one book. At other times, he referred to it as being so large and complex as to require two or three books. At various times, he had promised that the book(s) would be finished in anywhere from six months to two years. But among his critics, the point

remained that this was a promise he seemed unable to fulfill.[20]

Boring, as Murray's immediate supervisor, was at first sympathetic to his publication problems. Boring himself had been suffering from a severe writer's block ever since the publication of his The Physical Dimensions of Consciousness in 1932, and he personally intervened on Murray's behalf with the Harvard administration.[21] As a result of his efforts, Boring was able to assure Murray that:

I do not expect that the University is likely to fire you at the end of your present term for lack of productivity. I think it is going to see that you had a very difficult undertaking to put through and that you have done what is about humanly reasonable; but I think it is also going to continue to put pressure upon you for personal productivity in addition to the work of your students. My personal expectation is that next winter you will be reappointed for three years at \$5500, and that everybody will hope that this book will be out long before the problem has to be considered again.[22]

This was, in fact, the University's decision. Murray expressed his gratitude to Boring in an effusive, hand-written letter, and began a concerted effort to finish his book as quickly as possible.[23]

However, Conant's election to the Harvard Presidency in 1934 initiated a series of events that was to profoundly influence the next review of Murray's appointment in 1937. One of Conant's first acts as President was to appoint an interdepartmental committee to study the status of psychology at Harvard and to make recommendations as to how

its status could be improved. The committee reconfirmed the finding of the 1933 Committee to Visit the Department of Philosophy and Psychology, and recommended that Psychology be made a department separate from Philosophy. Also, the committee recommended that the new Department of Psychology could best be strengthened by hiring a leading figure in experimental psychology for a permanent Professorship. The committee's choice for this new Professorship was Karl S. Lashley, a neuropsychologist at the University of Chicago.[24]

Boring had always viewed his call to Harvard in 1922 as a sort of sacred mission to make psychology there rigorously scientific, primarily by promoting the psychophysical research of his mentor, Edward B. Titchener of Cornell University:

Harvard commanded my imagination. I do not fully know why. I had been involved in Titchener's affair with Harvard in 1917, and I had from long indoctrination by Titchener gained the conviction that Harvard was potentially the strongest place in America, that Hugo Munsterberg had debased psychology there, and that psychology needed to be brought clearly into the scientific circle and rescued from the philosophers who still dominated it.[25]

Now he was free to act at last. With Psychology a separate department, Boring began with an administrative iron-hand to direct its development into the research-oriented department he had long envisioned. Boring's intent to reshape psychology at Harvard along rigorously scientific lines can be seen not only in his efforts to bring Lashley to Harvard,

but also in the fact that he rated the faculty appointments of S. S. Stevens and (much later) B. F. Skinner as among his greatest administrative achievements. Both Stevens and Skinner shared Lashley's views on the necessity for psychology being a radically empirical, reductionistic science. [26]

Even though the Department of Psychology at Harvard took a decidedly more radically experimentalist course in 1935, Murray did pick up two very strong and influential supporters who were to prove invaluable the next time his contract came up for renewal. Murray's strongest supporter in the Department of Psychology was Gordon W. Allport. Allport stood out against the radically empirical orientation of the newly independent Department. A pioneer in research into the structure of human personality, Allport felt that his career chances had nearly been ruined in 1922 when Titchener, in front of many of America's leading experimental psychologists, had publicly condemned Allport's early research as being unscientific and worthless. Undoubtedly, Boring, who was in attendance at the conference at which these remarks were made, took note of his mentor's comments. [27]

Nevertheless, Allport did receive a position at Dartmouth College in 1926. Then, in 1928, a psychology position became available in the Department of Philosophy and Psychology at Harvard as a result of William McDougall's

resignation the previous year. Boring's choice for filling this position was Karl S. Lashley, while the philosophers wanted Allport, who in the previous year had successfully taught social ethics in the Department on a one year contract. Boring was allowed to try for his choice first. However, the attempt to lure Lashley away from the University of Chicago failed. Then, at the insistence of the Harvard philosophers, Boring extended the offer of an Assistant Professorship to Allport.[28]

While no doubt concerned about Boring's long-term reaction to his research, Allport accepted this offer for three reasons. First, a call to Harvard signaled that after the rebuff of seven years earlier, his work was at last getting some recognition. Second, Harvard had a long tradition of humanistic psychology that Allport wanted to help maintain. Finally, Allport was quite excited about the work that Murray was conducting at the Clinic, and foresaw the possibility of close collaborative research even though he had serious personal doubts about the validity of Murray's psychoanalytic orientation to personality theory.[29]

Murray's other important supporter was Dr. Stanley Cobb. Cobb was Professor of Neuropathology at the Harvard Medical School, Director of the Harvard Neurological Unit of the Boston City Hospital, and Chief of Psychiatry at the Massachusetts General Hospital.[30] On top of his very heavy

professional responsibilities, Cobb maintained an active avocational interest in psychoanalysis. This interest had its origins when Cobb was analyzed while in graduate school. Cobb was one of the charter members of the Boston Psychoanalytical Society, which held its first meeting at the Harvard Psychological Clinic on October 29, 1930.[31] This mutual interest led to the formation of a deep friendship between Cobb and Murray.

Cobb was a member of the committee appointed by Conant in 1934 to review the status of psychology at Harvard. Early in the committee's deliberations, Cobb sensed that the new Department of Psychology under Borinq's leadership was going to focus heavily on radically empirical psychology, and move away from research on more humanistic topics. As the Clinic was at that time experiencing serious financial difficulties that threatened its very existence, Cobb felt that he had to take some decisive action to help ensure that Murray's work would continue. Since Murray had just recently applied to the Medical Sciences Division of the Rockefeller Foundation for financial assistance, Cobb decided that he could aid Murray most effectively by vigorously supporting this application.[32]

Cobb had been negotiating with Alan Gregg, Director of the Medical Sciences Division of the Rockefeller Foundation about securing a large grant to make Harvard the leading psychiatric center in the United States. Cobb's plan

involved, among other things, the establishment of close ties between the Harvard Psychological Clinic and his new psychiatric unit.[33] Gregg was quite interested in this plan. Having met Freud during his visit to the United States in 1909, Gregg was very enthusiastic about the future of psychoanalysis in America, and under his direction the Medical Sciences Division of the Rockefeller Foundation was focusing on providing funding for psychiatric research and psychoanalytic training centers.[34]

Therefore, Gregg was inclined to grant Murray's request for aid, but had originally intended to wait until the committee on psychology had concluded its deliberations before acting.[35] Cobb, however, urged immediate action in order to ensure the Clinic's future at this critical transition in the development of psychology at Harvard:

If you have to make any decision about aid for Dr. Murray, I for one, would urge that you go ahead and act on that as an individual item on its own merits. As you know, I rate these merits very highly.[36]

Gregg took Cobb's advice, and provided the Clinic with the first of a series of grants-in-aid.[37]

Even though Murray's position at Harvard was thus temporarily secured, at least from a financial point of view, his most vehement critic was soon to arrive in Cambridge. Boring and the rest of Conant's committee on psychology considered Karl S. Lashley the most renowned psychologist in the United States, and they felt that

calling him, successfully this time, to a Professorship at Harvard would be the most significant act the new Department of Psychology could make.[38] This fit in perfectly with Boring's plans for increasing the empirical orientation of the Department, and he personally took charge of the secret negotiations to bring Lashley to Harvard.[39] This second attempt succeeded, largely because Lashley had become dissatisfied with the way the administration of the University of Chicago was interfering with his research activities. Lashley wanted to be left alone to carry out his work, and severely resented all the paperwork and financial accounting that he was responsible for at Chicago. Boring promised Lashley that Harvard would provide a much more congenial atmosphere in which he could conduct his research unencumbered by administrative details, and Lashley succumbed to this offer.[40]

However, when Lashley arrived at Harvard in the fall of 1935, he turned out to be far more vocal in his advocacy of a radically empirical psychology than even Boring was willing to accept. While a brilliant researcher and theorist, Lashley was narrow minded to the point of irrationality when it came to discussing points of view that differed from his own.[41] For example, he had begun a psychoanalysis under Franz Alexander before he left Chicago, but had angrily terminated the analysis after a few sessions for unknown reasons. He then became publicly very anti-psychoanalytic, and even tried everything in his power

to have Alexander fired from his adjunct faculty position at the University of Chicago.[42] Furthermore, not only did Lashley take a very critical view of Murray's and Allport's personality research, he even questioned Boring's right to call his own research scientific. A student of John B. Watson, and a staunch proponent of Watson's program for a radically empirical psychology, Lashley thus considered Boring's research on the nature of conscious experience to violate his strict criteria for a scientific psychology. This belief in extreme scientism, while alienating several members of the Department of Psychology, soon made Lashley a personal favorite and adviser to President Conant, who held a similar view about the necessity of precise, quantitative science for the growth of human knowledge.[43]

Largely as a result of Lashley's presence, the third scheduled review of Murray's appointment in 1937 promised to be far more controversial than previous reviews. Also, Boring, as Chairman of the Department, was now free to take issue with Murray about a number of administrative matters over which they had clashed during the preceding nine years.[44] Not only was this review of Murray's appointment to center on theoretical and administrative issues, an even greater obstacle threatened to block his reappointment. Despite the repeated promises of previous years, and even though seven more articles had been published and five others were awaiting publication from the Clinic, Murray's book was still far from completion.[45]

Murray's Promotion and Tenure Review

The first evidence of a noticeable cooling in relations between Boring and Murray came early in 1936. In January, Murray proposed to the Department his intention to sponsor a series of six lectures by Hanns Sachs on psychoanalytic theory, to be held at the Clinic. This proposal was, not unexpectedly, strenuously opposed by Lashley on the grounds that it would amount to an official University recognition of what he considered to be a non-scientific psychology.[46] What was significant, though, was that Boring did not attend these lectures, even though he had himself been analyzed by Sachs and had just three months earlier attended a series of lectures given by Sachs in the home of Dr. Cobb.[47]

Boring's actions were to become even more curious as Murray's review approached. As part of Harvard's upcoming tercentenary celebration, a number of distinguished scholars from around the world had been invited to Cambridge to receive honorary degrees. Each department had the opportunity to invite several scholars to receive this honor, and the Department of Psychology sent its invitations to Pierre Janet, Jean Piaget, and Carl G. Jung.[48] Murray wanted to have a Departmental luncheon at the Clinic as a special entertainment for their guests, and wrote to Boring to get his approval of this idea.[49] Boring had a surprisingly two-faced response to this plan. While writing to Murray about his enthusiastic support for the proposed

luncheon, he wrote to Lashley that he thought the whole thing was a poor idea.[50] Finally, when Boring visited the Clinic late in the spring of 1936 to gather evidence for Murray's upcoming review, his growing coolness towards Murray and the Clinic could no longer be concealed. After the visit Murray was so angry that he could not restrain himself from directly confronting Boring. He wrote:

It doesn't take a psychologist to see that during the last week you have grasped at every scrap of evidence or of a word (conscience!) which might be used as a case against the Clinic & have overlooked many points for the Clinic. In your words I find everything treacherously twisted. Why do you make out a bad case (damning with faint praise &c) for one of your own children?[51]

No record exists of Boring ever having responded to Murray's accusations. Instead, with Allport's assistance, he set about the task of soliciting a number of letters from prominent psychologists, asking for an evaluation of Murray and his work at the Clinic.[52]

While Boring had initially wanted the upcoming review to focus on the issue of whether Murray would be reappointed to another three-year term, President Conant insisted otherwise. At the urging of Dr. Cobb, and in accordance with his new "up-or-out" policy on faculty contract reviews, Conant stipulated that the review was to focus on whether Murray was to be promoted to the rank of Associate Professor and granted tenure.[53] Given the increased importance that Conant had placed on Murray's upcoming review, the letters solicited by Boring and Allport were to be of crucial

importance. In all, twenty-one letters evaluating Murray's work were received, and the response was overwhelmingly favorable. Only four letters were critical of Murray, and these were notable for the vagueness of their criticisms, and the general lack of knowledge their authors expressed of Murray's work. For example, Richard M. Elliott, Chairman of the Psychology Department at the University of Minnesota, wrote that he was:

wondering if as many undergraduates aren't harmed as are helped by the clinic's [sic] frank espousal of personal--what shall I say--comfort? good form? elegance? anyway something that is not of the essence of the laboratory spirit, nor akin to the asceticism of scholars outside the genteel tradition (Harvard? Cambridge and Oxford?).

How should I know about it all? I have not seen a printed word from the clinic.[55]

The seventeen positive letters were a different matter. In general, their authors were far better informed about the work Murray was conducting. They were also far more specific in terms of their praise and their criticisms than were the authors of the negative letters.[56] In a letter to Boring, Allport summarized the positive letters as making the following points:

	Number of times mentioned
Inter-disciplinary value; cultural medicinal and psychological synthesis; breaking down barriers	13
Favorable influence outside of Harvard	10
Depth and adequacy of approach to the baffling problems of personality	9
Ingenuousness in framing problems and in devising appropriate methods of research	8
Development continuing; future promising; eminent contributions predicted	7
Excellent teaching	7

Work unique; unlike any other institution	7
Murray's personal enthusiasm, his zeal	6
Specific published researches praised	5
Importance of problems chosen for research	5
Scientific caution; high standards in publication	5
Clinic best place for training men in field	3
Would like Murray at other institutions	2
Helpfulness to students with personal problems	2 [57]

To this list, Allport added three points of his own:

(1) Two merits frequently cited in the letters should be viewed together, viz., the inter-disciplinary value of the Clinic, and its unique position among American institutions. In private conversations with me Professor Sapir of Yale remarked that the Clinic was doing what the Institute of Human Relations was supposed to be doing at Yale, only far more successfully. This is likewise my personal opinion. When we consider the difference in the amount of financial backing the two institutes have the comparison is most striking. If Harvard wants to relate its sciences and break down barriers it ought not eliminate what to date is perhaps its most successful single achievement in this direction.

(2) If Harvard does not have the Clinic, what will it have? Or if Murray does not head the Clinic, who will head it? So far as I have information on men and institutes in abnormal psychology I can think of nothing that would be as good for us as Murray. We might obtain specialized neurologists and psychiatrists, but they would not fill the requirements of teaching, research, and cultural integration that abnormal psychology requires within our department.

(3) . . . Statistics of the department would show that Murray is doing a lion's share of the teaching. He seems to have nearly as many graduate students as the rest of the staff put together.[58]

The letters supporting Murray came from a number of prominent psychologists, including B. F. Skinner, Kurt Lewin, and William McDougall. Prominent non-psychologists

such as Dr. Stanley Cobb of Harvard and Dr. Alfred E. Cohn of the Rockefeller Institute for Medical Research also wrote in support. Carl G. Jung argued in person for Murray's promotion while a guest of honor at the Harvard tercentenary celebration in August of 1936.[59] But, according to Allport, the most persuasive letter of support was written by Murray's financial backer at the Rockefeller Foundation, Dr. Alan Gregg. He wrote:

Murray has had a superior training in biology. He has a wider experience than most psychologists and this may mean something if "the last thing that a deep sea fish could discover is salt water." He is an ingenious deviser of experimental methods. There are fields of inquiry in psychology as in medicine where human material is available for study only in exchange for advice and therapy and Murray has the technical qualifications, experience and an adequate sense of responsibility for such situations. He has had a real influence on premedical students and will continue to have it. He is honest, dogged and unselfish. His friends are deeply attached to him by admiration as well as affection. His defects appear to be a slowness or cloudiness in self-expression, an inadequate productivity and a certain tenacious independence of current opinion. In the competitive aspects of scholarly life these may be serious handicaps: I could wish, however, that there were more generally in this country, and especially at Harvard, an interest in seeing a scholar find solutions to what he regards as problems. It is in that procedure that England has seen grave additions to learning coming from independent and, if you wish, amateur scholars.

Of Murray's value I have no doubt. He matures slowly and has I think not yet produced his best. His mind is not brilliantly articulate. But his motivations are powerful and continuing. Whether he will make a significant addition to ideas and thought cannot be asserted but his contribution to the development of others and the example he sets in attempting to comprehend personality and the meaning of different approaches to it is a contribution I should be extremely sorry to see neglected or depreciated.

Medicine and psychiatry are beginning to despair of such a contribution from psychology. Without it psychology appears to be a collection of so highly diverse and differentiated, disparate interests as to be called more correctly psychologies, and to suffer from the undigested multiplicity of repeated additions to its body of knowledge. We do not need more of such multiplicity.[60]

In spite of the overwhelming support Murray received in the solicited letters, the opinion within the Harvard Department of Psychology was turning decidedly against Murray's reappointment. Lashley was arguing for making the Department's orientation strictly biological, and for banishing all socially and psychoanalytically oriented research from Harvard.[61] While Boring publicly claimed to be neutral on this issue, the manner in which he privately discounted the credibility of the letters supporting Murray's work indicated that he had already made up his mind on the matter.[62] Although claiming that he could still be swayed by evidence favorable to Murray, it was clear that the only evidence Boring considered to be relevant would be the immediate publication of Murray's book. Boring saw this as a matter of general principle. Evaluating the significance of a faculty member's magnum opus was the main criterion used by Boring in implementing Conant's "up-or-out" promotion and tenure process. Boring could not accept an exception to this in Murray's case.[63]

The pressure was now put on Murray to either have his book accepted for publication by the end of 1936, or at the very least, to submit a draft to President Conant prior to the upcoming deliberations of the ad hoc committee that was to be appointed to review his case.[64] In a desperate effort to meet this deadline, Murray ordered his students and colleagues to quickly prepare their contributions to the book, and then went into seclusion to work on the manuscript.[65] Allport, even though he was on sabbatical leave that year, was aware of how tenuous Murray's position had become in the Department, and also of the threat that Lashley posed to the future of his own research. He wrote to Murray pledging his total support in the upcoming debate:

It ought to be perfectly clear to all concerned that any change in the Clinic except in the direction of greater stability would be disastrous for Harvard. Still I presume we have both learned that worthwhile things cannot be taken for granted. They have to be fought for. . . . If . . . I am called upon for consultation in December [the tentative date for beginning the review of Murray's contract], I shall be more than willing to carry on the advocacy in person. If any issue should arise where you think I might be of service, please let me know. [66]

This was an extremely magnanimous gesture on Allport's part, since his anticipated collaboration with Murray had never developed, and their relations had instead developed into a low-keyed rivalry.[67]

Conant appointed the ad hoc committee to consider Murray's promotion and tenure on December 9, 1936. The committee consisted of Allport, Boring, and Lashley as

representatives of the Department of Psychology; Dean Birkhoff representing the Faculty of Arts and Sciences; Dr. Cobb and Dean Burwell as representatives of the Medical School; and President Conant, who would have the tie breaking vote if such was necessary. The first and only meeting of the committee was scheduled for lunch at Conant's house on December 23.[68] While only apocryphal accounts exist of this meeting, there is evidence to suggest that the debate was quite heated. Apparently, the meeting began with Allport delivering an impassioned speech to the effect that Murray was the intellectual heir of William James in the development of psychology at Harvard. He cited in detail how both James and Murray came to psychology through the study of medicine, and also how they were both ultimately concerned with how psychology could address philosophical questions about the nature of human existence. Allport concluded by stating how Murray's continued presence at Harvard was crucial for the further development of a humanistically oriented psychology.[69]

At the conclusion of Allport's speech, Lashley rose to address the committee. In contrast to Allport, his speech is said to have been short and sarcastic, his thesis being that if Murray was the heir to William James, this had to be interpreted in light of James having been a disgrace to the efforts to develop a scientific psychology in the United States. At this point, the meeting is said to have gotten totally out of hand. Conant took Lashley's insult of James

as an attack on the intellectual heritage of Harvard that he was trying to preserve through his efforts as President, and angrily left the room.[70] While the particulars of this confrontation cannot be independently confirmed, the confrontation itself is quite credible: Lashley later admitted that he had fallen from Conant's favor, and he was later considered unsuitable for appointment to a prestigious University Professorship.[71]

Somehow, out of this chaos, the committee was able to arrive at an evaluation of Murray. Its conclusion, though, was highly equivocal in its recommendation to Conant, and reflected the fact that the committee was evenly split over whether to promote or fire Murray. In their final vote, Allport, Cobb, and Dean Burwell voted for Murray being promoted and tenured, while Boring, Lashley, and Dean Birkhoff voted for his termination.[72] The task of writing the committee's report to Conant fell to Allport and Lashley, as they represented the extremes of opinion within the committee. Their report focused on a critique of the two chapters of Murray's book that had been completed prior to the committee's deliberations. Allport and Lashley agreed that in his book Murray was attempting to present a totally new methodological approach to the study of personality. Instead of the usual, piecemeal investigation of one element of personality at a time, Murray was attempting to study personality in its entirety through combining in one analysis the results of many different

experiments carried out by different experimenters, all studying one subject at a time. While Allport and Lashley acknowledged that Murray's earlier, published research was acceptable to all on the committee as methodologically sound, the work represented in the book was scientifically controversial.[73]

As far as Allport and Lashley were concerned, Murray was at least partly justified in rejecting the traditional reductionistic approach to the study of personality, for it only resulted in a partial description of a highly complex subject. But, Murray's new approach to research methodology was seen as raising more problems than it solved. Three problems in particular were pointed out. First, Murray was criticized for inventing new terms for personality variables when established terms for the same concepts were already in wide use. Second, Murray was criticized for the predominantly psychoanalytic orientation of his research concepts, in the face of sometimes limited justification for their use. Finally, and most damaging for Murray, there were serious questions raised about the validity and reliability of the experimental methods he devised for this work, and about his lax approach to checking and correcting these problems. Blame for this was put on Murray's naivete in the use of statistical data analysis. Many of his conclusions seemed, to the committee, to be more a matter of clinical interpretation than of clear empirical findings. Furthermore, the manner in which Murray integrated the

findings of the various researches that made up each personality study was considered equally naive. Rather than expressing the findings as an average of the research data, Murray had all the collaborating researchers meet as a group and argue out a single-analytical conclusion. The committee declared that this allowed for the possibility of incorrect conclusions held by a few influential researchers, such as Murray or one of his leading collaborators, overriding correct conclusions held by less influential researchers as a result of the dynamics of the group interaction.[74]

Allport and Lashley concluded the committee's report by stating that while these problems existed with Murray's work, it was at least as good, if not much better, than much of the research being conducted elsewhere on the same topic. Ultimately, the decision on Murray involved a value judgement of the scientific merits of personality research in general. Furthermore, Allport and Lashley concluded that this value judgement was a highly personal matter and that it could not be adequately expressed in a consensus opinion.[75] Just how personal this judgement could be was expressed in the separate follow-up letters to the committee's report that both Allport and Lashley wrote to Conant. Allport concluded that in regards to Murray's work:

The chief merit . . . is that it pushes forward the frontiers of research in the psychology of personality. Nowhere else in the literature will one find in a single study such breadth of perspective, so many ingenious methods, and such intensive treatment of individual life.

The criticisms we have made of Murray's work are not, in my judgement, fatal. . . .

It must be borne in mind that Murray is working in an almost virgin territory where chicanery and system-making have always prevailed. He brings a more critical mind to his subject than did Charcot, Prince, Freud, or Jung. . . .

I earnestly hope that you will not now permit the humanistic tradition in psychology at Harvard to be imperiled and destroyed. The critical standards of the "exact sciences," admirable in their own right, are not catholic enough in outlook to serve as the norm for the newer science of the human mind.[76]

Lashley took a position directly opposite that of Allport. In his letter he stated that:

. . . I am impressed, on the contrary, by the failure to meet satisfactorily any one of the . . . objective checks . . . by what I can interpret only as a sort of intellectual dishonesty where statistical tests for reliability are used and then glossed over as of little importance when they do not show the desired reliability, and by the supercilious attitude which Murray and his students show towards the available methods of validating their results.

. . . stripped of his verbiage, his major concepts are elementary and his elaboration of them is in terms of orthodox psychoanalytic theory.

Our [Lashley and Allport] disagreement in evaluating the work presents, in a way, the conflict between the older humanistic and philosophical psychology and the attempt to evolve a more exact science through an objective and biological approach.[77]

Rockefeller Funding Resolves the Debate

The final decision was now up to Conant, and he knew that no matter what his decision was, it would not be popular with everyone. The faculty of the Medical School

and the Department of Philosophy had already gone on record as being highly supportive of Murray's promotion and tenure.[78] Allport, realizing the threat to his own research implicit in Lashley's criticism of Murray, made known his intention to resign from Harvard and take a position at Clark University if Murray was not promoted and tenured.[79] However, Lashley insisted that anything less than Murray's immediate termination would be taken as an affront to his professional integrity, and would necessitate his resignation and return to his former position at the University of Chicago.[80] The situation remained deadlocked until late in January of 1937. Boring suggested to Conant that Murray be promoted to Associate Professor, but not granted tenure. Instead, Murray should be guaranteed two five-year appointments in which to prove the merits of his work, since by the end of ten more years it should be obvious to all whether or not his work was significant.[81] The only problem with this plan was that, since Murray was not to be tenured, no University funds could be used to supplement the Clinic's meager endowment. It was determined that the implementation of Boring's plan would require a long-term commitment from the Rockefeller Foundation to continue providing funding for the Clinic. Unfortunately, this matter could not be concluded until Alan Gregg returned from a business trip to Europe in early March.[82]

Fortunately for Murray, Dr. Cobb had kept in close touch with Gregg as the debate surrounding Murray's contract review was developing. Through Cobb, Gregg was aware of the various arguments for and against Murray, and of the consequences of a negative decision by Conant.[83] Therefore, when Gregg was finally able to confer with Conant on March 12, 1937, he was prepared to vigorously reassert the points he had made in his letter of the previous summer, and to express the Rockefeller Foundation's continued interest in supporting Murray's work at the Clinic.[84] While Gregg did not specifically guarantee that financial support could be expected for the next ten years, Conant considered his expression of interest a sufficient guarantee. On the morning of March 17, 1937, he announced his decision to appoint Murray to two five-year terms as Associate Professor.[85]

As it turned out, only Lashley was unwilling to accept this compromise solution. He continued to argue for Murray's termination and again threatened to resign if his demand was not met. He was unwilling to accept Boring's advice that he had upheld his professional integrity by virtue of his vote in the ad hoc committee and his letter to Conant, and that his duty to the University now consisted in implementing Conant's decision to the best of his ability.[86] Lashley continued to insist that his integrity was in jeopardy, and was only dissuaded from resigning when Conant offered to appoint him as a Research Professor and

remove him from all teaching and administrative obligations within the Department of Psychology.[87]

Murray left on a two-year sabbatical leave at the end of the 1936-1937 academic year, leaving the Clinic in the care of former student and research collaborator, Robert W. White.[88] After a one-year vacation in Europe with his family, Murray returned to Cambridge to put the finishing touches on his book, which was to be entitled Explorations in Personality: A Clinical and Experimental Study of Fifty Men of College Age. [89] Once the manuscript was completed, however, Murray ran into trouble finding a publisher for it. After having the manuscript rejected by Appleton-Century-Crofts, Harvard University Press, and Yale University Press, Murray was introduced by his friend, Supreme Court Justice Felix Frankfurter, to Paul Dillert, the American Editor of the Oxford University Press.[90] Although the reviewer to whom Dillert sent the manuscript--an anonymous but violently anti-psychoanalytic Philadelphia psychiatrist--argued for rejection, Murray and Frankfurter were able to convince Dillert that the book warranted publication.[91]

Conclusion

Explorations in Personality was finally published in October of 1938. Murray was at that time back in Cambridge working on a grant application to the Rockefeller Foundation

that would provide funding for the eight years that remained in his appointment as Associate Professor.[92] The bitterness of the debate of 1936-1937 had subsided, and Murray and Boring were again on cordial terms.[93] The time for accusations and argument was past, and a new beginning was in order. Boring best captured the spirit of this new beginning when he wrote to Murray after receiving his complimentary copy of Explorations in Personality. He said:

Any word of evaluation of this opus--now--from me to you-- after all that has been said and argued between us--would be superfluous. You and I differ in scientific taste and in philosophic preference. About such things there is no better and worse, except the dubious sanction of the expert opinion which is so seldom articulate. But some things are plain. You already have an enthusiastic following. You were not built to be other than a leader. And you will have opponents and derogators. The book is too positive and vital not to stir criticism. You must have wanted all that, in a way at least; and certainly you'll have it. You ought to be congratulated for it.[94]

Reference Notes

1. The terms of Murray's initial appointment can be ascertained from Edwin G. Boring to Henry A. Murray, 23 January 1933, in the Edwin G. Boring Papers, Harvard University Archives, Nathan Marsh Pusey Library, Harvard University, Cambridge, Mass.; and Stanley Cobb to Alan Gregg, 8 May 1936, Series 200, Box 91, Folder 1096, Rockefeller Foundation Archives, Pocantico Hills, North Tarrytown, New York. See also, James B. Conant, My Several Lives: Memoirs of a Social Inventor (New York: Harper & Row, 1970), pp. 157-171; and Chapter 4, pp. 113-116.

2. Conant, My Several Lives, pp. 157-171; Edwin G. Boring to Henry A. Murray, 29 October 1934; Edwin G. Boring to Gordon W. Allport, 17 January 1934; and Gordon W. Allport to Edwin G. Boring, 17 January 1934. The above letters are in the Edwin G. Boring Papers.

3. Allston Burr, "Report of the Committee to Visit Harvard College," in Reports of the Visiting Committees of the Board of Overseers of Harvard College: For the Academic Year 1933-34 (Cambridge, Mass.: Harvard University, 1935), pp. 225-226. In the Harvard University Archives.

4. Conant, My Several Lives, p. 81; and Henry A. Yeomans, Abbott Lawrence Lowell: 1856-1943 (Cambridge, Mass.: Harvard University Press, 1948), p. 535.

5. Seymour M. Lipset and David Riesman, Education and Politics at Harvard (New York: McGraw-Hill, 1975), pp. 142-150, 153-155; Samuel E. Morison, Three Centuries of Harvard: 1636-1936 (Cambridge, Mass.: Harvard University Press, 1936), pp. 441-445, 476-479; and Yeomans, Lowell, pp. 147-158, 165-198, 295-307.

6. Conant, My Several Lives, p. 82; Morison, Three Centuries of Harvard, pp. 447-448; and Yeomans, Lowell, pp. 147-158.

7. Conant, My Several Lives, p. 83; Lipset and Riesman, Education and Politics at Harvard, pp. 153-155; Morison, Three Centuries of Harvard, p. 441; and Yeomans, Lowell, pp. 282, 286.

8. Burr, "Report of the Committee to Visit Harvard College," p. 223.

9. Conant, My Several Lives, p. 83.

10. Ibid., pp. 81-83, 87-91.

11. Henry A. Murray, Personal interview, 28 April 1981; and 11 November 1981. Also, Henry A. Murray, Videotaped interview with Eugene Taylor, 12 November 1981, in the Rare Books Collection, Countway Library of Medicine, Harvard University Medical School, Boston, Mass.

12. Conant, My Several Lives, pp. 88-89.

13. Ibid., p. 89.

14. Burr, "Report of the Committee to Visit Harvard College," pp. 222-223.

15. James B. Conant, "Report of the President," in Reports of the President of Harvard College: 1933-1934 (Cambridge, Mass.: Harvard University, 1935), p. 7.

16. Henry A. Murray to Edwin G. Boring, 27 March 1931; Henry A. Murray to Edwin G. Boring, undated (probably mid-December, 1931); Henry A. Murray to Edwin G. Boring, undated (probably late-January, 1933); and Boring to Murray, 23 January 1933. The above are located in the Edwin G. Boring Papers. The publications from the Clinic during this period of time were: Herbert Barry, Jr., "The Role of Subject-Matter in Individual Differences in Humor," Journal of Genetic Psychology 35 (1928): 112-128; Herbert Barry, Jr., "A Test for Negativism and Compliance," Journal of Abnormal and Social Psychology 25 (1931): 373-381; and Herbert Barry, Jr., Donald W. MacKinnon, and Henry A. Murray, "Hypnotizability as a Personality Trait," Human Biology 3 (1931): 1-36.

17. Murray to Boring, undated (probably late January, 1933).

18. Henry A. Murray, "The Effect of Fear upon Estimates of the Maliciousness of other Personalities," Journal of Social Psychology 4 (1933): 310-329; Saul Rosenzweig, "Preferences in the Repetition of Successful and Unsuccessful Activities as a Function of Age and Personality," Journal of Genetic Psychology 42 (1933): 423-441; Saul Rosenzweig, "The Experimental Situation as a Psychological Problem," Psychological Review 40 (1933): 337-354; Saul Rosenzweig and A. G. Koht, "The Experience of Duration as Affected by Need-Tension," Journal of Experimental Psychology 16 (1933): 745-774; Saul Rosenzweig and G. Mason, "An Experimental Study of Memory in Relation to the Theory of Repression," British Journal of Psychology 24 (1934): 265-274; Harold Wolf, C. E. Smith, and Henry A. Murray, "The Psychology of Humor," Journal of Abnormal and Social Psychology 28 (1934): 341-365; Henry A. Murray, "The Psychology of Humor, II: Mirth Responses to Disparagement Jokes as a Manifestation of an Aggressive Disposition," Journal of Abnormal and Social Psychology 29 (1934): 66-81; Saul Rosenzweig, "A Suggestion for Making Verbal Personality

Tests More Valid," Psychological Review 41 (1934): 400-401; and Saul Rosenzweig, "Types of Reaction to Frustration," Journal of Abnormal and Social Psychology 29 (1934): 298-300.

19. Murray to Boring, 27 March 1931; Murray to Boring, undated (probably late January 1933); and Boring to Murray, 23 January 1933.

20. Henry A. Murray to Edwin G. Boring, 1 November 1934, in the Edwin G. Boring Papers; Henry A. Murray, "Researches Conducted at the Harvard Psychological Clinic During the Year 1933;" and Alan Gregg, "Memorandum of Interview with Dr. Henry Murray, Psychological Clinic, Harvard College, October 4, 1934." The preceding two items are located in the Rockefeller Foundation Archives, Series 200, Box 91, Folder 1095.

21. Boring's intervention is acknowledged in, Henry A. Murray to Edwin G. Boring, undated (probably late-March, 1934), in the Edwin G. Boring Papers. In regards to Boring's writer's block see: Edwin G. Boring, Psychologist at Large (New York: Basic Books, 1961), pp. 53-55; and Julian Jaynes, "Edwin Garriques Boring: 1886-1968," Journal of the History of the Behavioral Sciences 5 (1969): 107-108. See also, Chapter 5, pp. 171-173.

22. Boring to Murray, 21 January 1933.

23. Murray to Boring, undated (probably late-March, 1934).

24. Boring, Psychologist at Large, pp. 55-56; Edwin G. Boring to James E. Conant, 10 May 1934, in the Edwin G. Boring Papers; and Joseph Lee, "Report of the Committee to Visit the Department of Philosophy and Psychology," in Reports of the Visiting Committees of the Board of Overseers of Harvard College: For the Academic Year 1932-33 (Cambridge, Mass.: Harvard University, 1933), p. 151, in the Harvard University Archives.

25. Boring, Psychologist at Large, p. 33.

26. Boring, Psychologist at Large, pp. 65-66; and Jaynes, "E. G. Boring," pp. 107-109.

27. Gordon W. Allport, "Gordon W. Allport," in A History of Psychology in Autobiography, volume 5, eds., E. G. Boring and G. Lindzey (New York: Appleton-Century-Crofts, 1967), p. 9.

28. Allport, "G. W. Allport," pp. 13-14; Karl S. Lashley to Edwin G. Boring, 20 January 1928; Karl S. Lashley to Edwin G. Boring, 13 January 1928; and Edwin G. Boring to Gordon W. Allport, 27 February 1929. The above

are in the Edwin G. Boring Papers.

29. Allport, "G. W. Allport," pp. 8, 14; Edwin G. Boring to Gordon W. Allport, 16 April 1929; Edwin G. Boring to Gordon W. Allport, 3 May 1929. The above letters are in the Edwin G. Boring Papers; and Murray, Interview, 11 November 1981.

30. Henry K. Beecher and Mark D. Altschule, Medicine at Harvard: The First Three Hundred Years (Hanover, N.H.: University Press of New England, 1977), pp. 384-385.

31. Ives Hendrick, "The Birth of an Institute," in The Birth of an Institute: Twenty-Fifth Anniversary, The Boston Psychoanalytic Institute, November 30, 1958, ed. I. Hendrick (Freeport, Maine: Bond Wheelwright, 1961), pp. 16, 17f, 23f, 35.

32. Stanley Cobb to Alan Gregg, 24 September 1934, in the Rockefeller Foundation Archives, Series 200, Box 91, Folder 1095.

33. Beecher and Altschule, Medicine at Harvard, pp. 400-401; Edwin G. Boring to Karl S. Lashley, 6 March 1935, in the Edwin G. Boring Papers; Stanley Cobb to Alan Gregg, 24 April 1934; and Alan Gregg, "Diary," 3 May 1934. The above two items are in the Rockefeller Foundation Archives, Series 200, Box 91, Folder 1095, and represent but a small selection of the material therein pertaining to this issue.

34. Edwin G. Boring to Walter Hunter, 7 December 1936, in the Edwin G. Boring Papers; Hendrick, "Birth of an Institute," p. 14; Raymond B. Fosdick, The Story of the Rockefeller Foundation (New York: Harper, 1952), pp. 123-134; and Saul Benison (ed.), "The Reminiscences of Dr. Alan Gregg" (New York: Oral History Research Office, Columbia University, 1958), p. 28.

35. Alan Gregg, "Diary," 27 April 1934," Rockefeller Foundation Archives, Series 200, Box 91, Folder 1095. See also Chapter 4, pp. 121-122.

36. Cobb to Gregg, 24 September 1934.

37. "Grant-in-aid, New York," Rockefeller Foundation Archives, Series 200, Box 91, Folder 1095. This document was signed on 19 October 1934. See also Chapter 4, pp. 124-126.

38. Frank A. Beach, "Karl Spencer Lashley: June 7, 1890-August 7, 1958," in Biographical Memoirs, volume 35 (New York: Columbia University Press, 1961), pp. 173-174; and Boring, Psychologist at Large, pp. 55-56.

39. Edwin G. Boring to Karl S. Lashley, 26 February 1935; Karl S. Lashley to Edwin G. Boring, 28 February 1935; Boring to Lashley, 6 March 1935; and Karl S. Lashley to Edwin G. Boring, 23 March 1935. The above are located in the Edwin G. Boring Papers.

40. Edwin G. Boring to Richard M. Elliott, 17 January 1935; and Lashley to Boring, 28 February 1935. Both letters are in the Edwin G. Boring Papers.

41. "Factors in Psychological Leadership," a questionnaire designed as part of a research project by Lauren Wispe to determine what personal and educational factors were important in the professional development of prominent American psychologists. Respondents who provided personality ratings of Lashley when he was their mentor: Arthur G. Bills, Dorwin Cartwright, Carlyle F. Jacobsen, Charney Landis, Nathan W. Shock, and Calvin P. Stone. In the Lauren Wispe Papers, M698-M699, Archives of the History of American Psychology, University of Akron, Akron, Ohio. Of the above, all but Cartwright worked with Lashley before he came to Harvard. In his pre-Harvard days, Lashley was seen as being generally open minded, as long as the topics under discussion did not include psychoanalytic or Gestalt theory. Apparently, his views had narrowed considerably by 1935. For a further discussion of this point see: Edwin G. Boring to Gordon W. Allport, 18 January 1937, in the Edwin G. Boring Papers.

42. Henry A. Murray, Personal interview, 13 July 1981.

43. Murray, Interview, 13 July 1981; Interview, 11 November 1981; Conant, My Several Lives, pp. 84-85; Edwin G. Boring to Richard M. Elliott, 16 September 1936; Karl S. Lashley to Edwin G. Boring, 29 March 1935; Edwin G. Boring to Karl S. Lashley, 12 January 1937; and Edwin G. Boring to Gordon W. Allport, 14 January 1937. The above letters are in the Edwin G. Boring Papers.

44. See Chapter 5, pp. 164-169 for a further discussion of this issue.

45. Henry A. Murray to Edwin G. Boring, 22 January 1936, in the Edwin G. Boring Papers; and Edwin G. Boring to Alan Gregg, 11 May 1936, Rockefeller Foundation Archives, Series 200, Box 91, Folder, 1096.

46. Murray to Boring, 22 January 1936.

47. Edwin G. Boring to Henry A. Murray, 20 November 1935; and Henry A. Murray to Edwin G. Boring, 6 February 1936, in the Edwin G. Boring Papers.

48. The Tercentenary of Harvard College: A Chronicle of the Tercentenary Year, 1935-1936 (Cambridge, Mass.: Harvard University Press, 1937), pp. 218-220, 457-458.

49. Henry A. Murray to Edwin G. Boring, 24 July 1936, in the Edwin G. Boring Papers.

50. Edwin G. Boring to Henry A. Murray, 27 July 1936; and Edwin G. Boring to Karl S. Lashley, 28 July 1936. Both letters are in the Edwin G. Boring Papers. Boring had a tendency to identify with both sides in most all debates. This is corroborated by Jaynes, "E. G. Boring," p. 108; Henry A. Murray, Personal interview, 1 December 1981; and Silvan Tomkins, Personal interview, 26 August 1981.

51. Henry A. Murray to Edwin G. Boring, undated (probably sometime between early-February and late-May, 1936), in the Edwin G. Boring Papers.

52. Boring to Gregg, 11 May 1936; and Boring to Elliott, 16 September 1936, are examples of the solicitation of letters of reference for Murray's contract review. Further examples are to be found in the Gordon W. Allport Papers in the Harvard University Archives. For example, Gordon W. Allport to Kurt Lewin, 1 May 1936; Gordon W. Allport to Mark May, 7 May 1936; Gordon W. Allport to William McDougall, 22 April 1936; and Gordon W. Allport to David Shakow, 29 April 1936.

53. Stanley Cobb to Alan Gregg, 8 May 1936, Rockefeller Foundation Archives, Series 200, Box 91, Folder 1096; and Conant, My Several Lives, pp. 157-158.

54. Gordon W. Allport to Edwin G. Boring, 4 August 1936, in the Gordon W. Allport Papers.

55. Richard M. Elliott to Edwin G. Boring, 29 September 1936, in the Edwin G. Boring Papers.

56. Allport to Boring, 4 August 1936.

57. Ibid.

58. Ibid.

59. Skinner's letter is referred to in Elliott to Boring, 29 September 1936. Kurt Lewin to Gordon W. Allport, 4 May 1936, in the Gordon W. Allport Papers, is a very brief letter of support. A more detailed recommendation was sent on 26 May 1936, of which only the cover letter survives. Also in the Gordon W. Allport Papers is William McDougall to Gordon W. Allport, 13 May 1936. A copy of Stanley Cobb to Edwin G. Boring, 7 May 1936; and a summary of Alfred E. Cohn to Edwin G. Boring,

undated (probably early-May, 1936) can be found in the Rockefeller Foundation Archives, Series 200, Box 91, Folder 1096. Jung's meeting with Conant during the tercentenary celebration is discussed in Murray, Interview, 13 July 1981; and Murray, Interview, 1 December 1981.

60. Alan Gregg to Edwin G. Boring, 22 June 1936, in the Rockefeller Foundation Archives, Series 200, Box 91, Folder 1096. Allport offers his evaluation of the letter in Allport to Boring, 4 August 1936; and Gordon W. Allport to Henry A. Murray, 11 August 1936, in the Gordon W. Allport Papers.

61. Lashley to Boring, 29 March 1935; and Edwin G. Boring to Karl S. Lashley, 14 January 1937, in the Edwin G. Boring Papers.

62. Boring to Elliott, 16 September 1936; Boring to Gregg, 11 May 1936; and Allport to Boring, 4 August 1936. Allport states that he was motivated to write this letter because of Boring's dismissal of the letters supporting Murray.

63. Boring to Gregg, 11 May 1936; Edwin G. Boring to Alan Gregg, 23 June 1936, Rockefeller Foundation Archives, Series 200, Box 91, Folder 1096. The increasing emphasis on research excellence as the primary criterion for faculty advancement at Harvard under President Conant is discussed in Gordon W. Allport to Edwin G. Boring, 27 February 1936, in the Edwin G. Boring Papers. Boring's refusal to deviate from general principles in administering the Department's affairs is discussed in Boring to Allport, 17 January 1934; Allport to Boring, 17 January 1934; and Jaynes, "E. G. Boring," p. 107.

64. Edwin G. Boring to Henry A. Murray, 28 October 1936, in the Edwin G. Boring Papers. This letter is apparently a follow-up to a verbal communication of the order to Murray to finish his book by December, 1936.

65. Henry A. Murray to Alan Gregg, 21 October 1936, Rockefeller Foundation Archives, Series 200, Box 91, Folder 1096; Murray, Interview, 14 October 1981; and R. Nevitt Sanford, Personal interview, 28 August 1981.

66. Allport to Murray, 11 August 1936.

67. Murray, Interview, 11 November 1981; and M. Brewster Smith, Personal Interview, 28 August 1981.

68. James B. Conant to Gordon W. Allport, 9 December 1936, in the Gordon W. Allport Papers.

69. Murray, Interview, 13 July 1981.
70. Ibid.
71. Karl S. Lashley to Edwin G. Boring, 7 April 1937; and Edwin G. Boring to Gordon W. Allport, 7 April 1937. Both letters are in the Edwin G. Boring Papers.
72. Edwin G. Boring to Karl S. Lashley, 8 April 1937, in the Edwin G. Boring Papers.
73. Gordon W. Allport and Karl S. Lashley to James B. Conant, 6 January 1937, in the Gordon W. Allport Papers.
74. Ibid.
75. Ibid.
76. Gordon W. Allport to James B. Conant, 5 January 1937, in the Gordon W. Allport Papers.
77. Karl S. Lashley to James B. Conant, 6 January 1937, in the Gordon W. Allport Papers.
78. Edwin G. Boring to Gordon W. Allport, 19 January 1937, in the Edwin G. Boring Papers; and Boring to Lashley, 8 April 1937.
79. Gordon W. Allport to Edwin G. Boring, 21 January 1937, in the Edwin G. Boring Papers; and Boring to Lashley, 14 January 1937.
80. Karl S. Lashley to Edwin G. Boring, 7 April 1937; Boring to Lashley, 8 April 1937; Edwin G. Boring to Richard M. Elliott, 6 April 1937; and Edwin G. Boring to Gordon W. Allport, 1 April 1937. The above are in the Edwin G. Boring Papers.
81. Boring to Lashley, 8 April 1937.
82. Edwin G. Boring to Gordon W. Allport, 6 February 1937; and Edwin G. Boring to Gordon W. Allport, 18 February 1937. Both are in the Edwin G. Boring Papers.
83. Cobb to Gregg, 8 May 1936; and Robert A. Lambert, "Monday, Oct. 12, 1936, Boston," Rockefeller Foundation Archives, Series 200, Box 91, Folder 1096.
84. Edwin G. Boring to Gordon W. Allport, 17 March 1937, in the Edwin G. Boring Papers; and Alan Gregg, "Diary, March 12, 1937-- Boston," Rockefeller Foundation Archives, Series 200, Box 91, Folder 1097.

85. Boring to Allport 17 March 1937; and Memo from Gordon W. Allport, 29 April 1937, in the Gordon W. Allport Papers.

86. Boring to Lashley, 8 April 1937.

87. Lashley to Boring, 7 April 1937; Boring to Elliott, 6 April 1937; and Boring to Allport, 1 April 1937.

88. Edwin G. Boring to Gordon W. Allport, 22 March 1937, in the Edwin G. Boring Papers. See also Chapter 4, pp. 129-130.

89. Edwin G. Boring to Henry A. Murray, 10 October 1937, in the Edwin G. Boring Papers.

90. Robert A. Lambert, "Diary, Tuesday, October 26, 1937," Rockefeller Foundation Archives, Series 200, Box 91, Folder 1097; and Henry A. Murray, Personal interview, 3 August 1981.

91. Murray, Interview, 3 August 1981; and Henry A. Murray, "Henry A. Murray," in A History of Psychology in Autobiography, volume 5, eds. E. G. Boring and G. Lindzey (New York: Appleton-Century-Crofts, 1967), p. 305.

92. Henry A. Murray to Edwin G. Boring, 5 April 1938, in the Edwin G. Boring Papers.

93. Edwin G. Boring to Henry A. Murray, 14 April 1938; and Henry A. Murray to Edwin G. Boring, 15 April 1938. Both letters are in the Edwin G. Boring Papers.

94. Edwin G. Boring to Henry A. Murray, 25 October 1938, in the Edwin G. Boring Papers.

CHAPTER 8:
CONCLUSION: MURRAY'S ROLE IN DEFINING THE
BOUNDARIES OF ACADEMIC PSYCHOLOGY

Introduction

The maintenance of disciplinary boundaries does much to provide for the internal stability and institutional status of an area of academic inquiry. Through the process of theoretical debate, the acceptability of certain topics for the discipline is established. Further sources of intellectual control exist in the process of educating new members for disciplinary status, in the making of faculty hiring and tenure decisions, and in the editorial policies of professional journals.[1] However, the term boundary maintenance is perhaps not a totally accurate description of the process by which the limits of an academic discipline are set. Boundary maintenance implies that the intellectual limits are rigidly set and enforced. This is simply not the case.

Disciplinary boundaries are, for the most part, permeable, allowing for interdisciplinary influences. Disciplinary boundaries are also not stationary, as is demonstrated by the radical redefinitions that have occurred during periods of "scientific revolution." Also, disciplinary boundaries must be flexible enough to accommodate the frequent emergence and demise of limited-domain research topics. This is particularly true in psychology, which in recent years has been characterized by decreased emphasis on all-inclusive theories and a reliance on the development of "mini-theories" of personality and social behavior.[2]

Disciplinary Boundaries of Academic Psychology

The flexibility of disciplinary boundaries is particularly obvious in the instance of psychology. Traditionally, the origins of modern academic psychology have been seen as rooted in a hybridization of philosophy and physiology. Philosophy provided the subject matter and institutional base in the German universities, while physiology provided the empirical methods for the fledgling discipline.[3]

However, the "role hybridization" model of the origins of modern psychology is overly simplistic. The new discipline was more than a simple blend of philosophy and physiology. Researchers from a number of different

disciplines, including medicine, biology, and education as well as philosophy and physiology, began to turn their attention to issues of psychological importance during the middle years of the nineteenth century. While their theories and methods were varied, they shared the view that an empirical psychology of consciousness and volition was of practical importance, and stood near the top of the Comtean hierarchy of the sciences. This allowed the founders of modern psychology to consciously promote the emerging discipline and press for a separate institutional base within the universities.[4]

Early psychological thought addressed many issues of sociological and anthropological importance. Examples of this include Sigmund Freud's Totem and Taboo and Wilhelm Wundt's massive Völkerpsychologie. [5] Important extradisciplinary influences on psychology continue to come from physiology (neuropsychology), medicine (psychiatry), and zoology (comparative psychology). [6] Furthermore, psychologists and philosophers continued to maintain intellectual ties long after the two disciplines began to separate institutionally. William James, for example, always saw himself as a philosopher first, and as a psychologist second. He maintained that while the two disciplines differed on methodological grounds, philosophy's emphasis on the metaphysical basis of experience gave it conceptual priority over psychology. [7] Edward C. Tolman derived much of the conceptual underpinnings for his theory

of purposive behaviorism from the writings of neo-realist philosophers.[8] Also, Clark L. Hull maintained an active, if sometimes superficial, involvement with the Vienna Circle logical positivists during the 1930's and 1940's. More significantly, during this time period Hull was developing his behavioral theory upon a conceptual foundation derived from Newton's Principia Mathematica and implemented using the terminology of formal logic.[9] This continuing interaction between philosophy and psychology, however, has been greatly obscured by the vocal, anti-philosophical pronouncements of such prominent psychologists as Edward B. Titchener and John B. Watson.[10]

However, disciplinary boundaries do not always remain permeable and movable. This is especially seen whenever a discipline's institutional status is in jeopardy, or in an early stage of formation. The latter instance is revealed in an recent analysis of articles published in the American Journal of Sociology between 1895 and 1915. Initially, there was a great deal of debate among sociologists about the intellectual constitution of the emerging discipline. As much as one fourth of each volume's articles dealt with concerns over what sociology was in terms of its disciplinary identity. This publicly aired identity crisis subsided as American sociology gradually came under the intellectual direction of Albion Small and his colleagues at the University of Chicago.[11]

Similarly, the early years of American psychology were characterized by the expression of concerns over intellectual content and institutional status. Intellectual control was firmly held by five men--James McKeen Cattell, George T. Ladd, James Mark Baldwin, G. S. Fullerton, and William James--and by their hand-picked successors. These men were also instrumental in efforts to establish psychology in institutions of higher education. Under the aegis of the American Psychological Association, the young discipline's leaders lobbied to establish ties between psychology and older, more established disciplines, and to secure their position in American colleges and universities.[12]

Situations external to a discipline may also cause a restriction in intellectual boundaries. Periods of economic crisis place strains on academic institutions, potentially resulting in the demise of departments that are seen as not contributing sufficiently profitably to the fulfillment of the institution's educational mission. Such periods result in a renewed emphasis on the traditional topics and problems of the discipline by its leading members. In this way the discipline strives to recertify its right to remain within the institutional setting. This is largely a reflection of the middle-class orientation of the modern academic. An academic position and hard work as evidenced by research productivity confers status upon the individual scholar. Thus, the loss of a discipline's institutional position

would signify not only a loss of employment for the affected academic, but a significant loss of status as well. This serves as a powerful motivation for the individual scholar to publicly support and work within the discipline's boundaries.[13]

Disciplinary Boundaries of Psychology at Harvard

Psychology at Harvard during the 1920's and 1930's clearly demonstrated this relationship between the institutional security of a discipline and the restrictiveness of its intellectual boundaries. While psychology had been established within the American university system in general for almost forty years, psychology at Harvard did not become independent of the Department of Philosophy until 1934.[14] This, in itself, implies less than complete security for the discipline. Furthermore, the Depression was placing serious financial constraints upon the new Department of Psychology. As all departments at Harvard were funded by separate endowments, falling interest rates meant reduced departmental budgets.[15] This combined institutional and financial insecurity of psychology at Harvard set the stage for a tightening of disciplinary boundaries. The department had to strengthen its position as an important, unique, and coherent field of study. The vigor with which this was attempted and the specific direction it took reflected the

theoretical and methodological biases of the Department's ranking members, Edwin G. Boring and Karl S. Lashley.

Murray, however, stood out in contrast with the move for restricting psychology's disciplinary boundaries. His sense of personal and professional identity was not constrained by his disciplinary role as an Assistant Professor of Psychology. He actively maintained close contacts with artists, literary scholars, and statesmen of varied intellectual orientations. He counted among his friends such notables as the biochemist Lawrence J. Henderson, the writers Conrad Aiken and Eugene O'Neill, the actor and singer Paul Robeson, and the United States Supreme Court Justice Felix Frankfurter. Many of these men not only were guests of Murray's in Cambridge, but gave colloquia at the Clinic as well.[16] Beginning in 1932, Murray was also involved with the interdisciplinary Pareto Circle led by Henderson, and counting among its members the sociologists George C. Homans, Talcott Parsons, and Crane Brinton.[17]

Murray's interdisciplinary interests were also prominent in his psychological research. The major theoretical influences on his work were derived from psychoanalysis and dynamic psychology, in particular, the writings of Freud, Jung, and McDougall. Also, Murray derived the basis of his research methodology from his graduate training in the disciplines of medicine and biochemistry.[18]

The staff at the Clinic provided further indication of the interdisciplinary nature of Murray's work. As he described it in the introduction to the Explorations in Personality, the staff consisted of:

poets, physicists, sociologists, anthropologists, criminologists, physicians; of democrats, fascists, communists, anarchists; of Jews, Protestants, Agnostics, Atheists; of pluralists, monists, solipsists; of behaviourists, configurationists, dynamicists, psycho-analysts; of Freudians, Jungians, Rankians, Adlerians, Lewinians, and Allportians.[19]

What brought this divergent group of researchers together was the desire to pool their talents under Murray's leadership in the search for a more complete understanding of the workings of human personality. That this interdisciplinary approach to the study of personality was viewed favorably by some of Murray's colleagues could be seen in the letters sent in support of his promotion to Associate Professor. The breadth of his theoretical and methodological orientation to psychology was emphasized as a major strength by the psychologists Gordon Allport and Mark May, and by the physician Dr. Stanley Cobb. Furthermore, Alan Gregg of the Rockefeller Foundation's Medical Sciences Division cited Murray's work as one of the few aspects of psychology that would prove to be of interest to the medical community.[20]

However, not all agreed with this assessment of Murray. Whereas Murray represented an interdisciplinary approach to psychological theory and research, Boring and Lashley strove

to restrict psychology at Harvard to the study of a few, rigidly defined topics. Boring held religiously to the opinion of his mentor, Edward B. Titchener of Cornell University, that psychology at Harvard had been seriously tainted by its lengthy subordination to philosophy, and that drastic steps were needed if the newly formed department were to achieve a stature equivalent to that of Harvard in general. For Boring, these steps involved limiting the scope of psychology to the study of sensation and perception.[21] Lashley similarly believed that psychology at Harvard had an overly philosophical orientation. He did not, however, share Boring's views about what corrective measures to take. Based on his experience as a graduate student of John B. Watson, Lashley concluded that psychological research at Harvard should be reorganized along behavioral and physiological lines.[22]

Boring and Lashley differed further with Murray over the disciplinary acceptability of psychoanalysis. All three had had personal experience with psychoanalysis, but had reached radically different conclusions about its validity as a theory and as a form of therapy. Boring's and Lashley's experiences had been negative. Neither felt that the problems that had prompted them to seek psychoanalytic help had been resolved by the analysis. As a result, both came away from their analysis with a deep resentment for psychoanalysis and its practitioners.[23] Murray, on the other hand, found his analysis to result in a great personal

revelation, answering many questions he had had about himself and about human motives in general. Murray came away from his analytic experience dedicated to a new career and life goals.[24]

Yet another point of contention was the question of methodology. Boring and Lashley also shared similar views on what constituted the appropriate research methods for psychology. They believed that psychology had to model itself on the physical sciences. Research had to involve the strict control of all relevant variables. Only one independent variable was to be manipulated at a time, and the effect of this manipulation was to be measured through the observation of a single dependent variable.[25] This stood in direct contrast with Murray's wholistic approach to research. Murray believed that reductionistic, single variable research methods were inappropriate for the study of human personality. He saw personality as resulting from the interaction of numerous factors, both internal and external to the individual, and manifesting itself through numerous means. Therefore for Murray, research had to progress through the simultaneous study of a number of independent and dependent variables.[26]

The conjunction of the disciplinary insecurity of psychology at Harvard with these differences of opinion between Murray on one hand, and Boring and Lashley on the other, meant that several important issues in the debate

surrounding Murray's promotion and tenure review of 1936-1937 centered on the acceptability of Murray's theoretical orientation and research methods to the discipline of experimental psychology. Interestingly, though, only once during this debate was the issue of academic freedom raised in Murray's defense. Gordon Allport was concerned that his own research in the area of personality and social psychology would eventually jeopardize his position at Harvard if Boring and Lashley had their way and had Murray let go. Allport saw Murray's case as setting the precedent for the fate of all research not meeting Boring and Lashley's approval. Because of this, Allport argued that Murray's right to pursue whatever research topics he was interested in was protected by the express desire of the Harvard administration to support the academic ideals of unencumbered innovation and excellence in research.[27]

The ideal of academic freedom, though, was only tangentially relevant to this case. Academic freedom as it has been interpreted by, and applied to American higher educational institutions, serves to protect the individual scholar from arbitrary firing as a result of political or philosophical considerations. No limits are placed on the ability of academics to exclude other academics from their ranks as long as the decision is based on purely disciplinary considerations.[28]

Murray's personal politics or philosophy were never at issue. The question facing Boring and Lashley was of the disciplinary acceptability of Murray and his work, and their right to exclude him from psychology on these grounds was guaranteed by the concept of academic freedom. Thus, Lashley's threatened resignation upon Murray's promotion was quite understandable. He was rightly concerned that extradisciplinary forces had intervened in the promotion and tenure review process and had deprived him of his right to judge the acceptability of Murray's work.[29]

The restrictionist stance that Boring and Lashley took in regards to Murray's promotion and tenure review may also be interpreted in light of the tenuous institutional status of psychology at Harvard. While having the distinction of being one of the first centers for psychological research in the United States, psychology at Harvard had fallen upon hard times. Efforts to establish an independent department of psychology had been thwarted by the indifference of President Lowell towards psychology, and by the unexpected deaths of William James, Josiah Royce, and Hugo Munsterberg between 1910 and 1915. This left psychology staffed by junior faculty and part-time instructors, and institutionally subordinant to the Department of Philosophy until 1934. Only with the retirement of President Lowell was it possible for the institutional separation of psychology and philosophy to occur. While Lowell had been a proponent of the British tutorial model of higher education,

his successor James B. Conant favored the German research university model and supported the notion that disciplinary boundaries as reflected by research topics form the basis of departmental separation.[30]

Once psychology became independent of philosophy at Harvard, Boring, with the help of Lashley, set about the task of restoring the reputation of Harvard within the American psychological community. Harvard's status among psychologists had dropped precipitously since 1915 as measured both by the number of Ph.D.'s awarded and by peer reviews of departmental eminence.[31] Boring and Lashley knew also that psychology was not highly valued by the new Harvard President. Conant had appointed a special committee to evaluate the position of psychology vis-à-vis the other sciences at Harvard. The report of this committee did not impress Conant. In particular, he was discouraged by the committee's inability to even agree upon what constituted the discipline of psychology, and came away unconvinced that psychology would ever be able to attain the status of a bona fide academic discipline.[32]

Thus, Boring and Lashley were placed in the position of having to establish the credibility of a discipline that was seen as being on the fringe of the Harvard scientific community. Murray's position was complicated by the fact that he stood at the fringe of Harvard psychology. From the very beginning when he was hired in 1926 controversy had

surrounded him. Questions had been raised concerning his lack of the standard academic credentials for a position in psychology. Further questions concerned the theoretical validity of psychoanalysis, and concerning the ethical implications of Murray's use of hypnosis in his research.[33] The Clinic also operated semi-autonomously of the Department of Psychology. The Clinic was housed in separate quarters several blocks from the Department. The Clinic was funded separately, and Murray was not always required to go through departmental channels when dealing with the Harvard administration.[34]

These issues became of crucial importance when the economic disaster of the Depression threatened the ability of the Clinic to survive on the income from its endowment. A proposal to support the Clinic partially from general departmental funds fell through because of the administrative confusion that resulted from the Clinic's independent status. This gave rise to speculation that the administration of the Clinic would be transferred from the Department of Psychology in the College of Arts and Sciences to a department in the Medical School. It was proposed that this move would serve two purposes. First, it would place the Clinic under the administrative control of a more financially secure department, thus insuring its continued existence. Also, Boring and Lashley felt that such a move would be disciplinarily appropriate. They both considered abnormal and personality psychology to be an applied area of

research deriving much of its theoretical input from psychiatry. Therefore, they argued that Murray and the Clinic would fare best if allied directly with medicine, rather than remaining attached to a department that emphasized reductionistic rather than wholistic research methodologies, and rejected even the slightest hint of practical application.[35]

The institutional status of Murray's work within psychology at Harvard was also jeopardized by his failure to fit the established role model of the academic scientist. Beginning in the last quarter of the nineteenth century, American higher education came to be increasingly identified with middle-class values and goals. The college and university curricula shifted its focus from the shaping of the moral character of a social elite, to the professional training of the middle-class. This reflected a basic value assumption of American society that success was not to be achieved through membership in a hereditary elite, but through individual effort and intellectual capability. Thus, American higher education redefined its curricula to cater to a middle-class seeking avenues for self betterment, and the academic career quickly became identified as one of these avenues. Once the aspiring middle-class academic entered the profession, further career advancement was predicated upon the individual's demonstration of merit and hard work through publication in professional journals and success in teaching.[36]

Murray simply did not fit this definition of the professional academic. His position in society was not a function of his status as a scholar, but a result of his personal wealth and connections. Therefore, he felt free to challenge the traditional conceptions of academic psychology instead of being constrained to compete for peer recognition within accepted disciplinary boundaries.[37] This orientation carried over to his daily activities at the Clinic. The furnishings there were a statement of Murray's appreciation of fine art and antiques. Daily luncheons for the Clinic's staff and invited guests served as "a stimulus for convivial intellectual discourse. Work, for Murray, was something from which to take frequent breaks. He considered the quality of his writing to be far more important than its quantity, and he would take frequent short vacations from the Clinic to reflect upon his ongoing research and his plans for future investigations.[38]

This mode of behavior stood in direct contrast with that of Boring and Lashley, and with the values espoused by other leading figures in the discipline. Both came from middle-class backgrounds. So, for both, the attainment of prominence in an academic career marked a substantial gain in social status.[39] Within this social and institutional context they agreed that academic status was measured through research productivity. Of the two, Lashley was the more prolific researcher, publishing numerous journal articles, whereas Boring concentrated more on producing a

few books and on his responsibilities as a journal editor. This is not to suggest, though, that Boring was not concerned about his own efforts to publish. He feared greatly for the continued advancement of his career while suffering from a temporary writer's block between 1932 and 1934.[40]

Disciplinary Impact of Murray's Promotion

Therefore, Murray's promotion to Associate Professor revolved around the resolution of three basic issues. First was the question of where Murray's work stood in regards to the accepted disciplinary boundaries of academic psychology. This was closely related to the second issue--the institutional status of the Clinic and the Department of Psychology at Harvard. Finally, there was the problem of the manner in which Murray fulfilled his role as a professional scholar/scientist. In all three areas Murray challenged the status quo. He self-consciously questioned traditional definitions of academic psychology, calling instead for a total reconceptualization of teaching and research in the field. The Clinic existed at the institutional fringe of a young and insecure department threatened by administrative indifference and a worsening domestic economy. Finally, through his personal conduct Murray rejected the middle-class values that formed the basis of twentieth century academia. Given these facts,

Boring and Lashley could reach no conclusion other than to recommend that Murray's contract with Harvard be terminated.

What would otherwise have been a relatively straightforward review was significantly complicated by Harvard's policy of composing the review committee of faculty drawn from several different disciplines. In Murray's case, the review committee included faculty from the medical school as well as from psychology. Furthermore, the medical school faculty chosen to participate had a special appreciation of interdisciplinary applications of medical and biological theories and methods. Thus, they were highly supportive of Murray and argued for his promotion and tenure. Their strong support, meeting with the determined opposition of Boring and Lashley resulted in the committee's inability to reach a consensus concerning Murray's promotion or termination.[41]

It was at this point that the involvement of the Rockefeller Foundation became crucial. The Rockefeller Foundation had a history of providing financial support for interdisciplinary research in the social sciences, particularly in the application of the social sciences to solve the many human problems engendered by the rapid development of an industrial-technical Western economy. The Laura Spelman Rockefeller Memorial, founded in 1918, shifted the focus of its philanthropy exclusively to the social sciences upon the appointment of Beardsly Fuiml, a former

student of James R. Angell, to its directorship in 1922. His vision for the Memorial was to promote research and training in the social sciences through the establishment of centers where representatives of various disciplines could collaborate in the investigation of specific social issues. The Memorial funded a variety of projects including Dr. Elton Mayo's industrial fatigue studies at Harvard, the Yale Institute of Psychology (later named the Institute of Human Relations), and social work schools at the University of Chicago, Tulane, and elsewhere.[42] These funding policies were continued when the Memorial was merged with the the Rockefeller Foundation in 1929 and a Social Sciences Division was formed under the direction of Edmund E. Day.[43]

The Medical Sciences Division of the Rockefeller Foundation shared this appreciation of interdisciplinary research. An outgrowth of the Rockefeller Foundations early efforts to improve medical training standards and public health facilities (and to directly reduce employee absenteeism and increase work productivity), the Medical Sciences Division under the direction of Dr. Alan Gregg began in 1933 to concentrate its efforts on promoting psychiatric training and research. The rationale for this move was that while great advances were being made in the prevention and treatment of physical illness, the state of knowledge concerning mental disorders was woefully inadequate.[44] The funds of the Medical Sciences Division

were used to create departments of psychiatry at the universities of Chicago, McGill, and Tulane; to fund research at Worcester State Hospital in Massachusetts, Northwestern University, and Harvard; to create the Chicago Institute of Psychoanalysis; and to fund Kinsey's pioneering research on human sexuality.[45]

Therefore, while Murray's work at the Clinic received what was a comparatively small amount of financial support from the Rockefeller Foundation, his work embodied the ideals the Foundation had for the development of the social sciences, and for the integration of psychological knowledge into medicine for the purpose of treating mental illness. Furthermore, Murray was far more successful in implementing an interdisciplinary model of social science research and theory than were most of the more well supported beneficiaries of the Rockefeller Foundation.[46] This was because Murray, unlike his counterparts at other institutions, had been trained in different disciplines and thus did not approach his work from a perspective constrained by traditional intellectual boundaries.

The difficulty, at this point, lies in interpreting President Conant's acceptance of the Rockefeller Foundation's offer to continue funding Murray and the Clinic in exchange for Murray's promotion to Associate Professor. Harvard, like all American colleges and universities, had a history of institutional policy being set in part by the

desires of its benefactors. Rarely did donations come without some conditions attached.[47] The founding of the Clinic in 1926 by Morton Prince is a case in point. Harvard's "every tub on its own bottom" policy mandating the separate funding of each department, faculty chair, or laboratory increased the potential influence of wealthy contributors. Prince took advantage of this to stipulate that the Clinic be institutionally based in the College of Arts and Sciences instead of in the Medical School, and that he be appointed as its first director.[48]

This, however, created the dilemma that faced President Conant when he was forced to cast the deciding vote on Murray's promotion and tenure review. On one hand, the terms of the Clinic's endowment required him to maintain it as a part of the Department of Psychology. The offer of continued support from the Rockefeller Foundation served to insure that the Clinic could continue to function according to these terms in spite of the dwindling income from its endowment. On the other hand, Conant had publicly committed himself to the dismantling of the "old boy" system of faculty appointments in favor of allowing the individual disciplines greater freedom in hiring and firing based on their evaluation of research merit and productivity.[49] Furthermore, Conant was already on record as agreeing with Boring and Lashley that Murray's credentials and research were inappropriate for a continuing contract with Harvard.[50]

Therefore, Conant's decision to promote Murray represented a financially-motivated retreat from his meritocratic promotion and tenure system. The significance of this retreat is underscored by the fact that while Conant later boasted in his autobiography about his firm application of a merit-based promotion and tenure decision to a contemporaneous case in the Harvard Political Science Department, he remained pointedly silent about his decision on Murray.[51] While the Rockefeller Foundation secured Murray's position at Harvard, it thwarted Boring and Lashley's efforts to pass independent and unencumbered judgment on him. This is what particularly angered Lashley and resulted in his threat to resign his position in protest. Whereas middle-class academics initially justified their existence by the practical application and popular appeal of their work, a major portion of their professional identity and pride centered on the institutional independence of their discipline.[52] In this instance, the activities of the Rockefeller Foundation, aided and abetted by Conant, deprived Harvard psychology of this freedom.

In this way the guarantee of continued funding from the Rockefeller Foundation served to resolve the debate over the disciplinary and institutional issues that were at the core of the controversy surrounding Murray's work at the Harvard Psychological Clinic. This resolution was to prove, however, to be temporary. With the creation of the Department of Social Relations in 1945, the issue of

disciplinary boundaries was raised again and old wounds were reopened. This time Boring, with the help of S. S. Stevens, was able to prevail in his quest for limiting psychology at Harvard to the study of a few topics, and both Murray and Allport were pressured into relocating in the new department.[53]

In the interim, though, Murray was able to accomplish much at Harvard. His work steadily gained recognition, so that by 1948 his status among psychologists was solidly enough established for Harvard to grant him tenure.[54] While he did not succeed in his stated plan to radically redefine teaching and research in academic psychology, his presence within the discipline did provide a broadening influence on its intellectual boundaries. His work demonstrated that it was possible to operationalize and empirically test concepts drawn from the clinical practise of psychoanalysis. This opened the way for future researchers to legitimately pursue investigations in this area free of concern that their work would be deemed unacceptable on grounds of theory or methods.[55] Murray's deliberate incorporation of concepts from medicine and biochemistry in his theory of personality reflected his belief that man could not be studied from a reductionistic viewpoint, but had to be conceived instead as a complex, interactive, and wholistic system. This view was to provide an essential cornerstone in the rise of the humanistic psychology movement during the early 1950's.[56]

Murray's work also contributed to a redefinition of the methodological limits of academic psychology. Again, this was accomplished through the introduction into psychology of concepts from other disciplines. By drawing on diagnostic techniques from medicine and experimental procedures from biochemistry, Murray was able to propose a method for studying the interaction of the numerous independent factors underlying human personality. The significance of this achievement lies in the fact that although statistical techniques for analyzing multiple sources of causation in experimental data had been available since 1925, their use did not become generally accepted among psychologists until after 1950.[57]

Murray also succeeded in demonstrating the practical applications of his research methods. This was accomplished by Murray's involvement with the Office of Strategic Services during the Second World War. By modifying the multiform method he first employed in the Explorations in Personality, Murray was able to devise a practical method for selecting men to conduct espionage and commando raids behind enemy lines. His OSS activities then served to draw attention to his earlier work, thus greatly aiding in the growth of his reputation and use of his techniques among other psychologists.[58]

It would be incorrect, however, to assume that Murray's broadening influence on the discipline was solely a function of the support he received from the Rockefeller Foundation. He received funding not only because of his personal contacts (although his friendship with Gregg did make it easier for him to communicate directly with the Foundation), but because his program of research was independently judged to be worthy of funding. While it is true that external funding secured his position at Harvard, the growth of his reputation within the discipline of psychology rested upon the strength of the ideas. His publications were reviewed and accepted on the basis of the arguments and evidence he presented. Finally, he attracted highly motivated graduate students because of the intellectual stimulation and guidance he provided.[59]

Conclusion

In conclusion, Murray was an iconoclast among academic psychologists. He began his career by challenging the traditional definition of the discipline, and retired as one of its most respected figures. Along the way he played a prominent role in the development of research and theory on human personality, and in doing so he served to keep psychology a broadly defined, eclectic discipline that would remain open to influences from allied fields. Such has been his contribution to psychology.

However, his struggle to remain at Harvard--focusing on the promotion and tenure decision of 1936-1937--has provided a contribution to the historical study of psychology. In particular, this study has presented a analysis of the institutional development of the discipline. This dissertation has focused on the never ending process of defining the intellectual boundaries of academic psychology at Harvard during the 1920's and 1930's. This process has involved an interplay between intellectual and institutional forces within the discipline and university, and financial forces from outside. None of these three forces alone were sufficient to determine Murray's fate at either Harvard or in the larger context of the psychological community. Instead, a combination of internal and external issues were inexorably intertwined.

Therefore, Murray's long-term legacy to psychology lies perhaps in the example he set for what the discipline could be--a discipline unselfconsciously open to a wide variety of influences, both from within psychology and from other disciplines. This is a goal that psychology has yet to attain. While the attempts of people such as Boring and Lashley to restrict psychology to the study of a limited domain of topics have failed, Murray's and Allport's goal of psychology as a broadly-defined, eclectic discipline has not been completely achieved either. In fact, Murray's and Allport's view of psychology as a humanistic discipline has also spawned a restrictive subfield, frequently referred to

as the "Third Force." [60] There is still a persistent undercurrent of debate within the discipline as to what psychology is, and who has the right to call themselves a psychologist. Especially in regards to the distinctions drawn between academic and clinical psychology, certain sub-disciplinary boundaries have even been codified in the by-laws of the American Psychological Association. [61]

What has developed, then, is a situation where theoretical and methodological diversity is superficially accepted for the discipline as a whole, but that individual researchers tend to professionally isolate themselves within their own narrow area of specialization. This has resulted in psychology being a fragmented discipline characterized by limited communication among its members, and occasional outbursts of intramural warfare. This is a situation that must change if psychology is to remain vital as an academic and applied discipline, not to mention it ever achieving the degree of interdisciplinary openness that characterized Murray's work. As William Bevan, President of the APA during 1982, succinctly put it:

Modern psychology is a house of many mansions. Our survival as a social institution depends upon our occupying not only the room called basic science, but the other rooms as well. We must establish for ourselves an effective circulation pattern among the rooms and a collective life within them. What we now urgently need in psychology is a greater diversification of roles and far better communication among the players of those roles than is at present the case. [62]

A potential solution to this nagging problem was offered by Murray's work. As he pointed out in the preface to the Explorations, the purpose of his research was to "inquire into the nature of man." [63] Issues of theory and methodology were ultimately of secondary importance to this one overarching. To the extent that psychologists recognize that their specialized research projects are ultimately directed towards the understanding of larger human concerns, they are following Murray's example. In this manner, the issue of disciplinary boundaries and their maintenance yields to the hope of transcending and integrating disciplinary perspectives.

Reference Notes

1. Henrika Kuklick, "Boundary Maintenance in American Sociology: Limitations to Academic 'Professionalization,'" Journal of the History of the Behavioral Sciences 16 (1980): 201-219; Bruce Kuklick, The Rise of American Philosophy: Cambridge, Massachusetts 1860-1930 (New Haven, Conn.: Yale University Press, 1977), pp. 565-572; and Everett Mendelsohn, "The Social Construction of Scientific Knowledge," in The Social Production of Scientific Knowledge, vol. 1, eds. E. Mendelsohn, P. Weinart, and R. Whitley (Dordrecht, Holland: D. Reidel, 1977), pp. 3-26.

2. Thomas S. Kuhn, The Structure of Scientific Revolutions, 2nd ed., enl. (Chicago: University of Chicago Press, 1970).

3. Joseph Ben-David and Randall Collins, "Social Factors in the Origins of a New Science: The Case of Psychology," American Sociological Review 31 (1966): 451-465; and Edwin G. Boring, A History of Experimental Psychology, 2nd ed. (Englewood Cliffs, N.J.: Prentice-Hall, 1950), pp. 3-21.

4. Dorothy Ross, "The Development of the Social Sciences," in The Organization of Knowledge in Modern America, 1860-1920, eds. A. Cleson and J. Voss (Baltimore: Johns Hopkins University Press, 1979), pp. 107-138.

5. Sigmund Freud, Totem and Taboo, trans. J. Strachey (London: Routledge & Kegan Paul, 1950); and Wilhelm Wundt, Volkerpsychologie, 10 vols. (Leipzig: Engelmann, 1900-1920).

6. Ross, "Social Sciences," pp. 107-112; Boring, Experimental Psychology, pp. 622-631; Edna Heidebreder, Seven Psychologies (Englewood Cliffs, N.J.: Prentice-Hall, 1933), pp. 326-327; Brian D. MacKenzie, Behaviourism and the Limits of Scientific Method (Atlantic Highlands, N.J.: Humanities Press, 1977), pp. 55-100; and John F. Reisman, A History of Clinical Psychology (New York: Irvington, 1976). Further support for this can be found in examining any current introductory psychology textbook. For example, Rita L. Atkinson, Richard C. Atkinson, and Ernest R. Hilgard, Introduction to Psychology, 8th ed. (New York: Harcourt Brace Jovanovich, 1981), pp. 4-29.

7. Ross, "Social Sciences," p. 122; and William James, The Principles of Psychology, vol 2 (New York: Henry Holt & Co., 1890), pp. 312-315.

8. Laurence D. Smith, "Behaviorism and Logical Positivism: A Revised Account of the Alliance" (Ph.D. dissertation, University of New Hampshire, 1983); and Edward C. Tolman, "Edward Chace Tolman," in A History of Psychology in Autobiography, vol. 4, eds. E. G. Boring, H. S. Langfeld, H. Wenner, and R. M. Yerkes (Worcester, Mass.: Clark University Press, 1952), pp. 325-330.

9. Smith, "Behaviorism and Logical Positivism;" R. B. Ammons, "Psychology of the Scientist: II. Clark L. Hull and his 'Idea Books,'" Perceptual and Motor Skills 15 (1962): 800-802; and Rodney G. Triplet, "The Relationship of Clark L. Hull's Hypnosis Research to his Later Learning Theory: The Continuity of his Life's Work," Journal of the History of the Behavioral Sciences 18 (1982): 24.

10. Edward B. Titchener, "The Problems of Experimental Psychology," American Journal of Psychology 15 (1905): 220-224; Edwin G. Boring, "Edward Bradford Titchener, 1867-1927," American Journal of Psychology 38 (1927): 489-506; and John B. Watson, "Psychology as a Behaviorist Views It," Psychological Review 20 (1913): 158-177.

11. Kuklick, "Boundary Maintenance," pp. 204-206.

12. Thomas M. Camfield, "The Professionalization of American Psychology, 1870-1917," Journal of the History of the Behavioral Sciences 9 (1973): 67-69.

13. Kuklick, "Boundary Maintenance," pp. 203-204; and Burton J. Eledstein, The Culture of Professionalism: The Middle Class and the Development of Higher Education in America (New York: Norton, 1976), pp. 1-45, 287-331.

14. Kuklick, American Philosophy, pp. 180-195, 405-416; Edwin G. Boring, Psychologist at Large (New York: Basic Books, 1961), pp. 55-56; Edwin G. Boring to James B. Conant, 10 May 1934, in the Edwin G. Boring Papers, Harvard University Archives, Nathan Marsh Pusey Library, Harvard University, Cambridge, Mass.; and Joseph Lee, "Report of the Committee to Visit the Department of Philosophy and Psychology," in Reports of the Visiting Committees of the Board of Overseers of Harvard College: For the Academic Year 1932-1933 (Cambridge, Mass.: Harvard University, 1933), p. 151, in the Harvard University Archives.

15. James B. Conant, My Several Lives: Memoirs of a Social Inventor (New York: Harper & Row, 1970), pp. 115-118, 126.

16. Henry A. Murray, Personal interview, 1 December 1981; R. Nevitt Sanford, Personal interview, 28 August 1981; R. Nevitt Sanford, "Murray's Clinic as a Place to Learn," in Learning After College, ed. C. Comstock

(Orinda, Cal.: Montaigne, 1980), pp. 107-112; and Robert W. White, Personal interview, 2 July 1981.

17. Barbara S. Heyl, "The Harvard 'Pareto Circle,'" Journal of the History of the Behavioral Sciences 4 (1968): 318.

18. For a detailed discussion of this issue, see Chapter 6. See also Henry A. Murray, "Preparations for the Scaffold of a Comprehensive System," in Psychology: A Study of a Science, vol. 3, ed. S. Koch (New York: McGraw-Hill, 1959), pp. 9-38.

19. Henry A. Murray, Explorations in Personality: A Clinical and Experimental Study of Fifty Men of College Age (New York: Oxford University Press, 1938), p. xi.

20. Gordon W. Allport to Edwin G. Boring, 4 August 1936, in the Gordon W. Allport Papers, Harvard University Archives; and Alan Gregg to Edwin G. Boring, 22 June 1936, Series 200, Box 91, Folder 1096, in the Rockefeller Foundation Archives, Pocantico Hills, North Tarrytown, N.Y.

21. Boring, Psychologist at Large, pp. 33, 65-66; and Julian Jaynes, "Edwin Garriques Boring: 1886-1968," Journal of the History of the Behavioral Sciences 5 (1969): 107-109.

22. Edwin G. Boring to Gordon W. Allport, 18 January 1937; Edwin G. Boring to Richard M. Elliott, 16 September 1936; Karl S. Lashley to Edwin G. Boring, 29 March 1935; Edwin G. Boring to Karl S. Lashley, 12 January 1937; and Edwin G. Boring to Gordon W. Allport, 14 January 1937. The above are all in the Edwin G. Boring Papers. Also, Henry A. Murray, Personal interview, 13 July 1981; and Henry A. Murray, Personal interview, 11 November 1981.

23. Edwin G. Boring to Gordon W. Allport, 11 September 1935, in the Edwin G. Boring Papers; Edwin G. Boring, "Was this Analysis a Success?" Journal of Abnormal and Social Psychology 35 (1940): 4-16; and Murray, Interview, 13 July 1981.

24. Henry A. Murray, "What Should Psychologists Do About Psychoanalysis?" Journal of Abnormal and Social Psychology 35 (1940): 153.

25. Karl S. Lashley to James B. Conant, 5 January 1937, in the Gordon W. Allport Papers.

26. Murray, Explorations, pp. 5-23.

27. Gordon W. Allport to Edwin G. Boring, 21 January 1937; and Edwin G. Boring to Karl S. Lashley, 14 January 1937. Both letters are in the Edwin G. Boring Papers.

28. American Association of University Professors, "Academic Freedom and Tenure: 1940 Statement of Principles and Interpretive Comments," AAUP Bulletin 64 (1978): 108-112.

29. Karl S. Lashley to Edwin G. Boring, 7 April 1937; and Edwin G. Boring to Karl S. Lashley, 8 April 1937. Both letters are in the Edwin G. Boring Papers.

30. Kuklick, American Philosophy, pp. 405-416; and Boring, Psychologist at Large, pp. 55-57.

31. Kuklick, American Philosophy, pp. 459-463.

32. Boring, Psychologist at Large, pp. 55-56; Edwin G. Boring to James E. Conant, 10 May 1934, in the Edwin G. Boring Papers; and Murray, Interview, 13 July 1981.

33. Edwin G. Boring to Karl S. Lashley, 8 April 1937; and Edwin G. Boring to Henry A. Murray, 29 January 1931. Both letters are in the Edwin G. Boring Papers. Also, White, Interview 2 July 1981.

34. Edwin G. Boring, "The Psychological Laboratory," in Reports of the President and the Treasurer of Harvard College: 1926-27 (Cambridge, Mass.: Harvard University, 1928), p. 244, in the Harvard University Archives; and Edwin G. Boring to Henry A. Murray, 23 March 1932, in the Edwin G. Boring Papers.

35. Edwin G. Boring to Henry A. Murray, 8 January 1932; Edwin G. Boring to Henry A. Murray, 18 April 1932; and Edwin G. Boring to Henry A. Murray, 2 March 1934. The above are in the Edwin G. Boring Papers. Also, Karl S. Lashley to James B. Conant, 6 January 1937, in the Gordon W. Allport Papers.

36. Bledstein, Professionalism, pp. 287-309.

37. Henry A. Murray, "Henry A. Murray," in A History of Psychology in Autobiography, vol. 5, eds. E. G. Boring and G. Lindzey (New York: Appleton-Century-Crofts, 1967), pp. 297-298; and Henry A. Murray to Alan Gregg, 9 October 1934, Series 200, Box 91, Folder 1095, in the Rockefeller Foundation Archives.

38. Sanford, Interview, 28 August 1981; White, Interview, 2 July 1981; and Silvan Tomkins, Personal interview, 26 August 1981.

39. Boring, Psychologist at Large, pp. 8-11; and Frank A. Beach, "Karl Spencer Lashley: June 7, 1890-August 7, 1958," in Biographical Memoirs, vol. 35 (New York: Columbia University Press, 1961), pp. 163-168.

40. Boring, "Was this Analysis a Success?" pp. 4-16; Boring, Psychologist at Large, pp. 53-55; and Jaynes, "E. G. Boring," pp. 107-108.

41. Boring to Lashley, 8 April 1937; and Gordon W. Allport and Karl S. Lashley to James B. Conant, 5 January 1937, in the Gordon W. Allport Papers.

42. Raymond B. Fosdick, The Story of the Rockefeller Foundation (New York: Harper, 1952), pp. 192-209; E. Richard Brown, Rockefeller Medicine Men: Medicine and Capitalism in America (Berkeley, Cal.: University of California Press, 1979), pp. 105-109; and Elton Mayo, The Problems of Industrial Civilization (New York: Macmillan, 1933).

43. Fosdick, Rockefeller Foundation, pp. 200-203.

44. *Ibid.*, pp. 123-144.

45. *Ibid.*, pp. 126, 130-131.

46. Allport to Boring, 4 August 1936. Allport comments in this letter about a conversation he had recently had with Dr. Mark May of the Yale Institute of Human Relations, comparing Murray's work with that being conducted at Yale.

47. In particular see the discussion of the McKay Bequest to Harvard in Samuel E. Morison, Three Centuries of Harvard: 1636-1936 (Cambridge, Mass.: Harvard University Press, 1936), pp. 259-271.

48. Morton Prince to A. Lawrence Lowell, 26 January 1926. A copy of the original is in the Gordon W. Allport Papers.

49. Allston Burr, "Report of the Committee to Visit Harvard College," in Reports of the Visiting Committees of the Board of Overseers of Harvard College: For the Academic Year 1933-34 (Cambridge, Mass.: Harvard University, 1935), pp. 222-223; and James B. Conant, "Report of the President," in Reports of the President of Harvard College: 1933-1934 (Cambridge, Mass.: Harvard University, 1935), p. 7. Both of the above are in the Harvard University Archives.

50. Murray, Interview, 13 July 1981; Murray, Interview, 11 November 1981; Conant, My Several Lives, pp. 84-85; Edwin G. Boring to Richard M. Elliott, 16 September 1936; Karl S. Lashley to Edwin G. Boring, 29 March 1935; Boring to Lashley, 12 January 1937; and Boring to Allport, 17 January 1937. The above letters are in the Edwin G. Boring Papers.

51. Conant, My Several Lives, pp. 157-171. Conant cites the firing of Dr. J. Raymond Walsh and Dr. Alan R. Sweezy from positions in the Department of Economics in March of 1937 as the first application of his "up-or-out" promotion and tenure policy.

52. Bledstein, Professionalism, pp. 290-309.

53. Boring, Psychologist at Large, pp. 65-67; and Murray, Interview, 13 July 1981. Needless to say, both have a different interpretation of why the move to the Department of Social Relations took place.

54. Murray, "Henry A. Murray," p. 294.

55. Calvin S. Hall and Gardner Lindzey, Theories of Personality, 3rd ed. (New York: Wiley, 1978), pp. 235-238; and Salvatore R. Maddi, Personality Theories: A Comparative Analysis, 2nd ed. (Homewood, Ill.: Dorsey Press, 1978), pp. 42-45, 292, 474-479.

56. Salvatore R. Maddi, "Humanistic Psychology: Allport and Murray," in Concepts of Personality, eds. J. M. Wepman and R. W. Heine (Chicago: Aldine, 1963), pp. 162-205; Floyd W. Matson, "Humanistic Theory: The Third Revolution in Psychology," The Humanist 31 (1971): 7-11; and Murray, Interview, 1 December 1981.

57. Anthony J. Rucci and Ryan D. Tweney, "Analysis of Variance and the 'Second Discipline' of Scientific Psychology: A Historical Account," Psychological Bulletin 87 (1980): 166-184.

58. Henry A. Murray, Assessment of Men (New York: Holt, Rinehart, 1948); Henry A. Murray, Personal interview, 3 August 1981; M. Brewster Smith, Personal interview, 28 August 1981; and Tomkins, Interview, 26 August 1981.

59. Alan Gregg to Henry A. Murray, 16 February 1934, Series 200, Box 91, Folder 1095, in the Rockefeller Foundation Archives; Sanford, Interview, 28 August 1981; White, Interview, 2 July 1981; Tomkins, Interview, 26 August 1981; and Hiram Hayden, "Henry A. Murray," The American Scholar 39 (1970): 127-128.

60. Floyd W. Matson, "Humanistic Theory: The Third Revolution in Psychology," Humanist 31 (1971): 7-11.

61. American Psychological Association, "Specialty Guidelines for the Delivery of Services by Clinical Psychologists," American Psychologist 36 (1981): 640-651; and American Psychological Association, "Ethical Principles of Psychologists," American Psychologist 36 (1981): 633-638.

62. William Bevan, "Integrity, Image and Social Action," APA Monitor, October 1982, p. 5.

63. Murray, Explorations, p. vii.

BIBLIOGRAPHY

Archival Sources

- Akron, Ohio. University of Akron. Archives of the History of American Psychology. Alan Elms Record, M1045.
- Akron, Ohio. University of Akron. Archives of the History of American Psychology. Lauren Wispe Papers, M698-M699.
- Boston, Massachusetts. Harvard University Medical School. Countway Library. Rare Books. C. G. Jung Biographical Archive of the Francis G. Wickes Foundation.
- Boston, Massachusetts. Harvard University Medical School. Countway Library. Rare Books. Videotaped interview between Henry A. Murray and Eugene Taylor, 12 November 1981.
- Cambridge, Massachusetts. Harvard University. Nathan Marsh Pusey Library. Harvard University Archives. Gordon W. Allport Papers.
- Cambridge, Massachusetts. Harvard University. Nathan Marsh Pusey Library. Harvard University Archives. Edwin G. Boring Papers.
- Cambridge, Massachusetts. Harvard University. Nathan Marsh Pusey Library. Harvard University Archives. Henry A. Murray Papers.
- Pocantico Hills, North Tarrytown, New York. Rockefeller Foundation Archives. Series 200.

Interviews

- Murray, Henry A. Cambridge, Massachusetts. Interview. 28 April 1981.

- , Cambridge, Massachusetts. Interview. 13 July
1981.
- , Cambridge, Massachusetts. Interview. 3 August
1981.
- , Cambridge, Massachusetts. Interview. 14 October
1981.
- , Cambridge, Massachusetts. Interview. 11
November 1981.
- , Cambridge, Massachusetts. Interview. 1 December
1981.
- , Cambridge, Massachusetts. Interview. 13 January
1982.
- , Cambridge, Massachusetts. Interview. 8 July
1982.
- Sanford, R. Nevitt. Los Angeles, California. Interview.
28 August 1981.
- Smith, M. Brewster. Los Angeles, California. Interview.
28 August 1981.
- Tomkins, Silvan. Los Angeles, California. Interview. 26
August 1981.
- White, Robert W. Marlborough, New Hampshire. Interview. 2
July 1981.

Writings of Henry A. Murray

- Murray, Henry A. "Groton and Adaptation." The Grotonian 35
(1919): 402-419.
- , "The Development of the Cardiac Loop in the
Rabbit, With Especial Reference to the Bulboventricular
Groove and Origin of the Interventricular Septum."
American Journal of Anatomy 26 (1919): 29-39.
- , "Two Unusual Cases of Melanocarcinoma."
Proceedings of the New York Pathological Society 19
(1919): 28-37.
- , "Studies in Calcium Metabolism and Other Related
Subjects." M.A. thesis, Columbia University, 1920.
- , "The Bicarbonate and Chloride Content of the
Blood in Certain Cases of Persistent Vomiting."

Proceedings of the Society for Experimental Biology and Medicine 19 (1922): 273-275.

- "The Chemical Pathology of Pyloric Occlusion in Relation to Tetany: A Study of the Chlorid, Carbon Dioxid and Urea Concentrations in the Blood." Archives of Surgery 7 (1923): 166-196.
- "Physiological Ontogeny. A. Chicken Embryos. II. Catabolism. Chemical Changes in Fertile Eggs during Incubation. Selection of Standard Conditions." Journal of General Physiology 9 (1925): 1-37.
- "Physiological Ontogeny. A. Chicken Embryos. III. Weight and Growth Rate as Functions of Age." Journal of General Physiology 9 (1925): 39-48.
- "Physiological Ontogeny. A. Chicken Embryos. VII. The Concentration of the Organic Constituents and the Calorific Value as Functions of Age." Journal of General Physiology 9 (1926): 405-432.
- "Physiological Ontogeny. A. Chicken Embryos. VIII. Accelerations of Integration and Differentiation During the Embryonic Period." Journal of General Physiology 9 (1926): 603-621.
- "Physiological Ontogeny. A. Chicken Embryos. IX. The Iodine Reaction for the Quantitative Determination of Glutathione in the Tissues As a Function of Age." Journal of General Physiology 9 (1926): 621-624.
- "Physiological Ontogeny. A. Chicken Embryos. X. The Temperature Characteristic for the Contraction Rate of Isolated Fragments of Embryonic Heart Muscle." Journal of General Physiology 9 (1926): 781-788.
- "Physiological Ontogeny. A. Chicken Embryos. XI. The pH, Chloride, Carbonic Acid, and Protein Concentrations in the Tissues as Functions of Age." Journal of General Physiology 9 (1926): 789-803.
- "Physiological Ontogeny. A. Chicken Embryos. XII. The Metabolism as a Function of Age." Journal of General Physiology 10 (1926): 337-343.
- "What to Read in Psychology." The Independent 118 (1927): 134.
- "A Case of Pinealoma with Symptoms Suggestive of Compulsion Neurosis." Archives of Neurology and Psychiatry 19 (1928): 932-945.

- "Studies in Physiological Ontogeny." Ph.D. dissertation, Cambridge University, 1928.
- "Professor Murray Describes Department of Abnormal Psychology." The Harvard Crimson, 12 January 1929, p. 4.
- "Dr. Morton Prince: A Founder of Psychology." Harvard Alumni Bulletin, 23 January 1930, pp. 490-495.
- "The Effect of Fear upon Estimates of the Maliciousness of Other Personalities." Journal of Social Psychology 4 (1933): 310-329.
- "The Psychology of Humor: 2. Mirth Responses to Disparagement Jokes as a Manifestation of an Aggressive Disposition." Journal of Abnormal and Social Psychology 29 (1934): 66-81.
- "Psychology and the University." Archives of Neurology and Psychiatry 34 (1935): 803-817.
- "The Harvard Psychological Clinic." The Harvard Alumni Bulletin, 25 October 1935, pp. 142-149.
- "Facts which Support the Concept of Need or Drive." Journal of Psychology 3 (1936): 27-42.
- "Techniques for a Systematic Investigation of Fantasy." Journal of Psychology 3 (1936): 115-143.
- "Basic Concepts for a Psychology of Personality." Journal of General Psychology 15 (1936): 241-268.
- "Visceral Manifestations of Personality." Journal of Abnormal and Social Psychology 32 (1937): 161-184.
- Explorations in Personality: A Clinical and Experimental Study of Fifty Men of College Age. New York: Oxford University Press, 1938.
- "What Should Psychologists Do About Psychoanalysis?" Journal of Abnormal and Social Psychology 35 (1940): 150-175.
- Assessment of Men. New York: Holt, Rinehart, 1948.
- "Morton Prince: Sketch of His Life and Work." Journal of Abnormal and Social Psychology 52 (1956): 291-295.

- , "Preparations for the Scaffold of a Comprehensive System." In Psychology: A Study of a Science. Vol. 2, pp. 7-54. Edited by Sigmund Koch. New York: McGraw-Hill, 1959.
- , "Josephine Lee Murray 1894-1962." Radcliffe Quarterly, February 1965, pp. 9-10.
- , "Henry A. Murray." In A History of Psychology in Autobiography. Vol. 5, pp. 285-310. Edited by Edwin G. Boring and Gardner Lindzey. New York: Appleton-Century-Crofts, 1967.
- , "Postscript: Morsels of Information Regarding the Extraordinary Woman in Whose Psyche the Foregoing Visions Were Begot." In The Visions Seminars. Book 2, pp. 517-521. Edited by Carl Gustav Jung. Zurich: Spring Publications, 1976.
- , "Indispensables for the Making, Testing, and Remaking of a Personological System." In The Roots of American Psychology: Historical Influences and Implications for the Future, pp. 323-331. Edited by Robert W. Rieber and Kurt Salzinger. New York: The New York Academy of Sciences, 1977.
- , "Response." Paper presented at the annual meeting of the American Psychological Association, Toronto, Canada, August 1978.
- Barach, Alvin J. and Murray, Henry A. "Tetany in a Case of Sprue," Journal of the American Medical Association 74 (1920): 786-788.
- Barry, Herbert, MacKinnon, Donald W., and Murray, Henry A. "Studies in Personality: A. Hypnotizability as a Personality Trait and its Typological Relations." Human Biology 3 (1931): 1-36.
- Cohn, Alfred E., and Murray, Henry A. "Physiological Ontogeny. A. Chicken Embryos. IV. The Negative Acceleration of Growth with Age as Demonstrated by Tissue Cultures." Journal of Experimental Medicine 42 (1925): 275-290.
- Cohn, Alfred E., and Murray, Henry A. "Physiological Ontogeny. I. The Present Status of the Problem." Quarterly Review of Biology 2 (1927): 469-493.
- Felty, A. R., and Murray, Henry A. "Observations on Dogs with Experimental Pyloric Obstruction: The Acid-Base Equilibrium, Chlorides, Non-Protein Nitrogen, and Urea of the Blood." Journal of Biological Chemistry 57 (1923): 573-585.

- Hastings, A. B., Murray, Cecil D., and Murray, Henry A. "Certain Chemical Changes in the Blood After Pyloric Obstruction in Dogs." Journal of Biological Chemistry 46 (1921): 223-232.
- Hastings, A. B., and Murray, Henry A. "Observations on Parathyroidectomized Dogs." Journal of Biological Chemistry 46 (1921): 233-256.
- King, George, and Murray, Henry A. "A New Blood Coagulometer." Journal of the American Medical Association 74 (1920): 1452-1453.
- McLean, Franklin C., Murray, Henry A., and Henderson, Lawrence J. "The Variable Acidity of Hemoglobin and the Distribution of Chlorides in the Blood." Proceedings of the Society for Experimental Biology and Medicine 17 (1920): 180-182.
- Morgan, Christiana D., and Murray, Henry A. "A Method of Investigating Fantasies." Archives of Neurology and Psychiatry 34 (1935): 289-306.
- Wheeler, D. R., and Murray, Henry A. "A Note on the Possible Clairvoyance of Dreams." Journal of Psychology 3 (1937): 309-313.
- Wolf, R., and Murray, Henry A. "An Experiment in Judging Personalities." Journal of Psychology 3 (1937): 345-365.
- Wolff, H. A., Smith, C. E., and Murray, Henry A. "The Psychology of Humor: 1. A Study of Responses to Race-Disparagement Jokes." Journal of Abnormal and Social Psychology 28 (1934): 341-365.

Primary Sources

- Allport, Gordon W. "The Journal of Abnormal and Social Psychology: An Editorial." Journal of Abnormal and Social Psychology 33 (1938): 3-13.
- "Gordon W. Allport." In A History of Psychology in Autobiography. Vol. 5, pp. 3-25. Edited by Edwin G. Boring and Gardner Lindzey. New York: Appleton-Century-Crofts, 1967.
- American Association of University Professors. "Academic Freedom and Tenure: 1940 Statement of Principles and Interpretive Comments." AAUP Bulletin 64 (1978): 108-112.

- American Psychological Association. "Ethical Principles of Psychologists." American Psychologist 36 (1981): 633-638.
- American Psychological Association. "Specialty Guidelines for the Delivery of Services by Clinical Psychologists." American Psychologist 36 (1981): 640-651.
- Atkinson, J. W. Motives in Fantasy, Action and Society. Princeton, New Jersey: Van Nostrand, 1958.
- Atkinson, J. W., and Feather, N. A Theory of Achievement Motivation. New York: Wiley, 1966.
- Barry, Herbert, Jr. "The Role of Subject-Matter in Individual Differences in Humor." Journal of Genetic Psychology 35 (1928): 112-128.
- "A Test for Negativism and Compliance." Journal of Abnormal and Social Psychology 25 (1931): 373-381.
- Bernard, Claude. Introduction, à l'étude de la Médecine Expérimentale. Paris: Baillière, 1865.
- Leçons sur les Phénomènes de la Vie Communs aux Animaux et aux Végétaux. 2 Vols. Paris: Baillière, 1878-1879.
- Bevan, William. "Integrity, Image and Social Action." APA Monitor, October 1982, p. 5.
- Boring, Edwin G. "Was this Analysis a Success?" Journal of Abnormal and Social Psychology 35 (1940): 4-16.
- Psychologist at Large. New York: Basic Books, 1961.
- Broad, C. D. The Mind and its Place in Nature. London: Routledge & Kegan Paul, 1925.
- Carrel, Alexis. Man the Unknown. New York: Harper & Brothers, 1939.
- Cattell, Raymond B. Personality: A Systematic Theoretical and Factual Study. New York: McGraw-Hill, 1950.
- Cohn, Alfred E. No Retreat from Reason: And Other Essays. New York: Harcourt, Brace and Co., 1931.
- Minerva's Progress: Tradition and Dissent in American Culture. Port Washington, New York: Kennikat Press, 1969.

- Conant, James B. My Several Lives: Memoirs of a Social Inventor. New York: Harper & Row, 1970.
- Diven, Kenneth. "Certain Determinants of the Conditioning of Anxiety Reactions." Journal of Psychology 3 (1937): 291-308.
- Dollard, John, Doob, Leonard W., Miller, Neal E., Mower, O. H., and Sears, Robert R. Frustration and Aggression. New Haven, Connecticut. Yale University Press, 1939.
- Dorsey, George A. Why We Behave like Human Beings. New York: Harper & Brothers, 1925.
- Draper, George. Human Constitution: A Consideration of its Relationship to Disease. Philadelphia: W. B. Saunders, 1924.
- Fisher, Robert A. Statistical Methods for Research Workers. London: Oliver & Boyd, 1925.
- Freud, Sigmund. Totem and Taboo. Translated by James Strachey. London: Routledge & Kegan Paul, 1950.
- . Five Lectures on Psycho-Analysis. Translated and edited by James Strachey. New York: Norton, 1977.
- Gibbs, Josiah W. "On the Equilibrium of Heterogeneous Substances." Transactions of the Connecticut Academy of Arts and Sciences 3 (1875-1876): 108-248.
- Haldane, J. E. S. Science Advances. New York: Macmillan, 1948.
- . The Causes of Evolution. Ithaca, New York: Cornell University Press, 1966.
- Hart, Bernard. The Psychology of Insanity. New York: Macmillan, 1912.
- Henderson, Lawrence J. The Fitness of the Environment: An Inquiry into the Biological Significance of the Properties of Matter. New York: Macmillan, 1913.
- . The Order of Nature: An Essay. Cambridge, Massachusetts: Harvard University Press, 1917.
- . Blood: A Study in General Physiology. New Haven, Connecticut: Yale University Press, 1928.
- . On the Social System: Selected Writings. Edited by Bernard Barber. Chicago: University of Chicago Press, 1970.

Hull, Clark L. "Clark L. Hull." In A History of Psychology in Autobiography. Vol. 4, pp. 143-162. Edited by Edwin G. Boring, H. S. Langfeld, H. Wenner, and R. M. Yerkes. Worcester, Massachusetts: Clark University Press, 1952.

----- "Psychology of the Scientist: IV. Passages from the 'Idea Books' of Clark L. Hull." Edited by Ruth Hays. Perceptual and Motor Skills 15 (1962): 803-882.

James, William. The Principles of Psychology. Vol. 2. New York: Henry Holt & Co., 1890.

Janet, Pierre. Névroses et Idées Fixes. Paris: Faillière, 1898.

----- Les Obsessions et la Psychasthénie. Paris: Baillière, 1903.

Jung, Carl Gustav. Psychological Types. New York: Harcourt, Brace & World, 1923.

----- Synchronicity: An Acausal Connecting Principle. Translated by R. F. C. Hull. Princeton, New Jersey: Princeton University Press, 1973.

Kendig, Isabel. "Studies in Perseveration: II. Determining Factors in the Development of Compulsive Activity." Journal of Psychology 3 (1937): 231-246.

----- "Studies in Perseveration: III. The Upper Limen for Perseveration and Repetition." Journal of Psychology 3 (1937): 247-251.

Koffka, Kurt. The Growth of the Mind: An Introduction to Child Psychology. Translated by R. M. Ogden. New York: Harcourt, Brace & Co., 1924.

Langer, Walter C. "An Experimental Critique of Measures of Learning." Journal of Psychology 3 (1937): 195-221.

Loeb, Jacques. The Mechanistic Conception of Life: Biological Essays. Chicago: University of Chicago Press, 1912.

----- The Organism as a Whole: From a Physiochemical Viewpoint. New York: G. P. Putnam's Sons, 1916.

Mayo, Elton. The Problems of Industrial Civilization. New York: Macmillan, 1933.

McClelland, David C. The Achievement Motive. New York: Appleton-Century-Crofts, 1953.

McDougall, William. Outline of Abnormal Psychology. New York: Charles Scribner's Sons, 1926.

----- "William McDougall." In A History of Psychology in Autobiography. Vol. 1, pp. 191-223. Edited by Carl Murchison. Worcester, Massachusetts: Clark University Press, 1930.

----- The Energies of Men: A Study of the Fundamentals of Dynamic Psychology. London: Methuen & Co., 1932.

Ogden, C. K. The Meaning of Psychology. New York: Harper & Brothers, 1926.

Ogden, C. K., and Richards, I. A. Meaning of Meaning. New York: Harcourt, Brace & Co., 1923.

Prince, Morton. The Unconscious: The Fundamentals of Human Personality, Normal and Abnormal. New York: Macmillan, 1921.

Report of the President of Harvard College: 1929-1930. Cambridge, Massachusetts: Harvard University, 1931.

Report of the President of Harvard College: 1930-1931. Cambridge, Massachusetts: Harvard University, 1932.

Report of the President of Harvard College: 1931-1932. Cambridge, Massachusetts: Harvard University, 1933.

Report of the President of Harvard college: 1932-1933. Cambridge, Massachusetts: Harvard University, 1934.

Report of the President of Harvard College: 1933-1934. Cambridge, Massachusetts: Harvard University, 1935.

Report of the President of Harvard College: 1934-1935. Cambridge, Massachusetts: Harvard University, 1936.

Report of the President of Harvard College: 1935-1936. Cambridge, Massachusetts: Harvard University, 1937.

Report of the President of Harvard College: 1936-1937. Cambridge, Massachusetts: Harvard University, 1938.

Reports of the President and the Treasurer of Harvard College: 1925-1926. Cambridge, Massachusetts: Harvard University, 1927.

Reports of the President and the Treasurer of Harvard College: 1926-1927. Cambridge, Massachusetts: Harvard University, 1928.

Reports of the President and the Treasurer of Harvard College: 1927-1928. Cambridge, Massachusetts: Harvard University, 1929.

Reports of the President and the Treasurer of Harvard College: 1928-1929. Cambridge, Massachusetts: Harvard University, 1930.

Reports of the Visiting Committees of the Board of Overseers of Harvard College: For the Academic Year 1932-1933. Cambridge, Massachusetts: Harvard University, 1934.

Reports of the Visiting Committees of the Board of Overseers of Harvard College: For the Academic Year 1933-1934. Cambridge, Massachusetts: Harvard University, 1935.

Richards, I. A. Principles of Literary Criticism. New York: Harcourt, Brace & Co., 1928.

Rosenzweig, Saul. "The Experimental Situation as a Psychological Problem." Psychological Review 40 (1933): 337-354.

..... "Preferences in the Repetition of Successful and Unsuccessful Activities as a Function of Age and Personality." Journal of Genetic Psychology 42 (1933): 423-441.

..... "A Suggestion for Making Verbal Personality Tests More Valid." Psychological Review 41 (1934): 400-401.

..... "Types of Reaction to Frustration." Journal of Abnormal and Social Psychology 29 (1934): 298-300.

..... "A Basis for the Improvement of Personality Tests with Special Reference to the M-F Battery." Journal of Abnormal and Social Psychology 33 (1938): 476-488.

Rosenzweig, Saul, and Koht, A. G. "The Experience of Duration as Affected by Need-Tension." Journal of Experimental Psychology 16 (1933): 745-774.

Rosenzweig, Saul, and Mason, G. "An Experimental Study of Memory in Relation to the Theory of Repression." British Journal of Psychology 24 (1934): 265-274.

Sanford, R. Nevitt. "The Effects of Abstinence from Food Upon Imaginal Processes: A Further Experiment." Journal of Psychology 3 (1937): 145-159.

Skinner, B. F. "B. F. Skinner." In A History of Psychology in Autobiography. Vol. 5, pp. 387-413. Edited by Edwin G. Boring and Gardner Lindzey. New York: Appleton-Century-Crofts, 1967.

Smith, Carl E. "A Study of the Autonomic Excitation Resulting from the Interaction of Individual Opinion and Group Opinion." Journal of Abnormal and Social Psychology 31 (1936): 138-162.

"Survey of the Undergraduate Program Development and Operations Committee." Department of Psychology, University of New Hampshire, Durham, New Hampshire, 1978.

"Survey of the Undergraduate Program Development and Operations Committee." Department of Psychology, University of New Hampshire, Durham, New Hampshire, 1980.

The Tercentenary of Harvard College: A Chronicle of the Tercentenary Year, 1935-1936. Cambridge, Massachusetts: Harvard University Press, 1937.

Thorndike, Edward L. "Animal Intelligence: An Experimental Study of the Associative Processes in Animals." Psychological Monographs Whole no. 4 (1898).

Titchener, Edward B. "The Problems of Experimental Psychology." American Journal of Psychology 15 (1905): 220-224.

Tolman, Edward C. "Edward Chace Tolman." In A History of Psychology in Autobiography. Vol. 4, pp. 323-339. Edited by Edwin G. Boring, H. S. Langfeld, H. Wenner, and R. M. Yerkes. Worcester, Massachusetts: Clark University Press, 1952.

Watson, John B. "Psychology as the Behaviorist Views It." Psychological Review 20 (1913): 158-177.

White, Robert W. "Prediction of Hypnotic Susceptibility from a Knowledge of Subject's Attitudes." Journal of Psychology 3 (1937): 265-277.

Whitehead, Alfred N. Symbolism: Its Meaning and Effect. New York: Macmillan, 1927.

..... Process and Reality: An Essay in Cosmology. New York: Macmillan, 1929.

..... Adventures of Ideas. New York: Macmillan, 1935.

Wundt, Wilhelm. Völkerpsychologie. 10 Vols. Leipzig: Englemann, 1900-1920.

Secondary Sources

- "Alan Gregg Dies; Medical Leader." The New York Times, 21 June 1957, p. 25.
- Albrecht, Frank M. "The New Psychology in America." Ph.D. dissertation, Johns Hopkins University, 1960.
- Allen, Garland E. "T. H. Morgan and the Emergence of a New American Biology." Quarterly Review of Biology 44 (1969): 168-188.
- , Life Science in the Twentieth Century. Cambridge: Cambridge University Press, 1975.
- "Alvin J. Barach." The New York Times, 14 December 1977, p. D-17.
- "Alvin J. Barach." Time, 26 December 1977, p. 58.
- Anastasi, Anne. Psychological Testing. 3rd ed. New York: Macmillan, 1968.
- Barron, Frank. "Personology Requires a Multidisciplinary Approach and Special Techniques of Investigation." Paper presented at the annual meeting of the American Psychological Association, Los Angeles, California, August 1981.
- Barzun, Jacques. The American University: How It Runs, Where It is Going. New York: Harper & Row, 1968.
- Beach, Frank A. "Karl Spencer Lashley: June 7, 1890-August 7, 1958." In Biographical Memoirs. Vol. 35, pp. 163-204. New York: Columbia University Press, 1961.
- Beecher, Henry K., and Altschule, Mark D. Medicine at Harvard: The First Three Hundred Years. Hanover, New Hampshire: University Press of New England, 1977.
- Bellak, Leo. "Henry A. Murray: An Appreciation." Journal of Projective Techniques 22 (1958): 143-144.
- Ben-David, Joseph, and Collins, Randall. "Social Factors in the Origins of a New Science: The Case of Psychology." American Sociological Review 31 (1966): 451-465.
- Benet, Stephen Vincet. John Brown's Body. New York: Holt, Rinehart and Winston, 1955.
- Bledstein, Burton J. The Culture of Professionalism: The Middle Class and the Development of Higher Education in America. New York: Norton, 1976.

- Boring, Edwin G. "Edward Bradford Titchener, 1867-1927." American Journal of Psychology 38 (1927): 489-506.
- . A History of Experimental Psychology. New York: The Century Co., 1929.
- . A History of Experimental Psychology. 2nd ed. Englewood Cliffs, New Jersey: Prentice-Hall, 1950.
- Boring, Edwin G., Langfeld, Herbert S., and Weld, Harry P. Foundations of Psychology. New York: Wiley, 1948.
- Bray, Douglas W. "The Assessment Center and the Study of Lives." American Psychologist 37 (1982): 180-189.
- Brown, E. Richard. Rockefeller Medicine Men: Medicine and Capitalism in America. Berkeley, California: University of California Press, 1979.
- Burnham, John C. Psychoanalysis and American Medicine, 1894-1918: Medicine, Science, and Culture. New York: International Universities Press, 1967.
- Buss, Allan R. "The Emerging Field of the Sociology of Psychological Knowledge." In Psychology in Social Context, pp. 1-24. Edited by Allan R. Buss. New York: Irvington, 1979.
- Camfield, Thomas M. "Psychologists at War: The History of American Psychology and the First World War." Ph.D. dissertation, University of Texas at Austin, 1969.
- . "The Professionalization of American Psychology, 1870-1917." Journal of the History of the Behavioral Sciences 9 (1973): 66-75.
- Cannon, Susan F. Science and Culture: The Early Victorian Period. New York: Dawson, 1978.
- Cannon, Walter B. "Lawrence Joseph Henderson: 1878-1942." In Biographical Memoirs, Vol. 23, pp. 31-58. Washington, District of Columbia: The National Academy of Sciences, 1945.
- Chaplin, James B., and Krawiec, T. S. Systems and Theories of Psychology. 3rd ed. New York: Holt, Rinehart and Winston, 1968.
- Clark, Ronald W. Freud: The Man and the Cause. London: Jonathan Cape and Widenfeld & Nicolson, 1980.
- Cordasco, Francesco. The Shaping of American Graduate Education: Daniel Coit Gilman and the Protean Ph.D. Totowa, New Jersey: Rowman and Littlefield, 1973.

- Crane, Diana. Invisible Colleges: Diffusion of Knowledge in Scientific Communities. Chicago: University of Chicago Press, 1972.
- Cronback, Lee J. Essentials of Psychological Testing. 3rd ed. New York: Harper & Row, 1970.
- Crowther, James G. Famous American Men of Science. New York: Norton, 1937.
- Curti, Merle, and Nash, Roderick. Philanthropy and the Shaping of American Higher Education. New Brunswick, New Jersey: Rutgers University Press, 1965.
- Danziger, Kurt. "The Origins of Modern Psychology." In Psychology in Social Context, pp. 27-45. Edited by Allan R. Buss. New York: Irvington, 1979.
- Davis, Kingsley. "Review of Explorations in Personality." American Journal of Sociology 45 (1939): 130-131.
- Decker, Hannah S. "Analysis . . . Frank J. Sulloway. Freud, Biologist of the Mind." Isis 72 (1981): 638-642.
- de Solla Price, Derek J., and Beaver, Donald. "Collaboration in an Invisible College." American Psychologist 21 (1966): 1011-1018.
- Dulaney, Don E., Jr., DeValois, Russell L., Beardslee, David C., and Winterbottom, Marian R. Contributions to Modern Psychology. New York: Oxford University Press, 1958.
- Ellenberger, Henri F. The Discovery of the Unconscious: The History and Evolution of Dynamic Psychiatry. New York: Basic Books, 1970.
- Elliott, Richard M. "Review of Explorations in Personality." American Journal of Psychology 52 (1939): 453-462.
- Elms, Richard. "The Crisis of Confidence in Social Psychology." American Psychologist 30 (1975): 967-978.
- Epstein, Seymour. "Explorations in Personality Today and Tomorrow: A Tribute to Henry A. Murray." Paper presented at the annual meeting of the American Psychological Association, Toronto, Canada, August 1978.
- "Explorations in Personality Today and Tomorrow: A Tribute." American Psychologist 34 (1979): 649-653.
- Fine, Rubin. A History of Psychoanalysis. New York: Columbia University Press, 1979.

- Fosdick, Raymond B. The Story of the Rockefeller Foundation. New York: Harper & Brothers, 1952.
- Freeman, Frank S. Theory and Practise of Psychological Testing. New York: Holt, 1950.
- Garrett, Henry E. Great Experiments in Psychology. New York: Appleton-Century, 1941.
- Gergen, Kenneth. "Social Psychology as History." Journal of Personality and Social Psychology 26 (1973): 309-320.
- Geuter, Ulfried. "National Socialism and the Professionalization of Psychology in Germany, 1933-1945," Colloquium presented to the Department of Psychology, University of New Hampshire, Durham, New Hampshire, 10 June 1983.
- Grande, Francisco, and Virscher, Maurice B. Claude Bernard and Experimental Medicine. Cambridge, Massachusetts: Schenkman, 1967.
- Gray, J. Stanley. Psychology Applied to Human Affairs. 2nd ed. New York: McGraw-Hill, 1954.
- Guilford, J. P. General Psychology. New York: Van Nostrand, 1939.
- Hale, Nathan G., Jr. Freud and the Americans: The Beginnings of Psychoanalysis in the United States, 1876-1917. New York: Oxford University Press, 1971.
- James Jackson Putnam and Psychoanalysis: Letters Between Putnam and Sigmund Freud, Ernest Jones, William James, Sandor Ferenczi, and Morton Prince, 1877-1917. Cambridge, Massachusetts: Harvard University Press, 1971.
- "Introduction." In Psychotherapy and Multiple Personality: Selected Essays, by Morton Prince, pp. 1-18. Edited by Nathan G. Hale, Jr. Cambridge, Massachusetts: Harvard University Press, 1975.
- Hall, Calvin S., and Lindzey, Gardner. Theories of Personality. New York: Wiley, 1957.
- Hall, Calvin S., and Lindzey, Gardner. Theories of Personality. 3rd ed. New York: Wiley, 1978.
- Henderson, Lawrence J. "A Philosophical Interpretation of Nature." Quarterly Review of Biology 1 (1926): 289-294.
- Hayden, Hiram. "Portrait: Henry A. Murray." The American Scholar 39 (1970): 124-128.

- Heidbreder, Edna. Seven Psychologies. Englewood Cliffs, New Jersey: Prentice-Hall, 1933.
- Hein, Hilde. "The Endurance of the Mechanism-Vitalism Controversy." Journal of the History of Biology 5 (1972): 159-188.
- Hendrick, Ives. The Birth of an Institute: Twenty-Fifth Anniversary, The Boston Psychoanalytic Institute, November 30, 1958. Freeport Maine: Bond Wheelwright, 1961.
- Henle, Mary. Jaynes, Julian, and Sullivan, John J. Historical Conceptions of Psychology. New York: Springer, 1973.
- Heyl, Barbara S. "The Harvard 'Pareto Circle.'" Journal of the History of the Behavioral Sciences 4 (1968): 316-334.
- Hilgard, Ernest R. "Explorations in Personality in Historical Perspective." Paper presented at the annual meeting of the American Psychological Association, Toronto, Canada, August 1978.
- Hollis, Earnest V. Philanthropic Foundations and Higher Education. New York: Columbia University Press, 1938.
- Holorenschaw, Henry. "The Making of an Honorary Taoist." In Changing Perspectives in the History of Science, pp. 1-20. Edited by M. Teich and R. Young. Dordrecht, Holland: D. Reidel, 1973.
- Jaynes, Julian. "Edwin Garriques Boring: 1886-1968." Journal of the History of the Behavioral Sciences 5 (1969): 99-112.
- Kendig, Isabel, and Shevack, B. J. "Studies in Perseveration: I. A Survey of Researches in Perseveration." Journal of Psychology 3 (1937): 223-230.
- Klein, D. B. Abnormal Psychology. New York: Holt, 1951.
- Koch, Sigmund. "Clark Leonard Hull." In Modern Learning Theory: A Critical Analysis of Five Samples, pp. 1-176. Edited by A. J. Poffenberger. New York: Appleton-Century-Crofts, 1954.
- Kuhn, Thomas S. The Structure of Scientific Revolutions. 2nd ed., enl. Chicago: University of Chicago Press, 1970.

Kuklick, Bruce. The Rise of American Philosophy: Cambridge, Massachusetts 1860-1930. New Haven, Connecticut: Yale University Press, 1977.

Kuklick, Henrika. "Boundary Maintenance in American Sociology: Limitations to Academic 'Professionalization.'" Journal of the History of the Behavioral Sciences 16 (1980): 201-219.

Laughlin, Charles D. "Discussion: The Influence of Whitehead's Organism upon Murray's Personology." Journal of the History of the Behavioral Sciences 9 (1973): 251-257.

Lipset, Seymour M., and Riesman, David. Education and Politics at Harvard. New York: McGraw-Hill, 1975.

Mackenzie, Brian D. Behaviourism and the Limits of Scientific Method. Atlantic Highlands, New Jersey: Humanities Press, 1977.

Maddi, Salvatore R. "Humanistic Psychology: Allport and Murray." In Concepts of Personality, pp. 162-205. Edited by J. M. Wepman and R. W. Heine. Chicago: Aldine, 1963.

----- Personality Theories: A Comparative Analysis. 3rd ed. Homewood, Illinois: Dorsey Press, 1978.

Malinin, Theodore I. Surgery and Life: The Extraordinary Career of Alexis Carrel. New York: Harcourt Brace Jovanovich, 1979.

Marx, Otto M. "Morton Prince and the Dissociation of a Personality." Journal of the History of the Behavioral Sciences 6 (1970): 120-130.

Matson, Floyd W. "Humanistic Theory: The Third Revolution in Psychology." The Humanist 31 (1971): 7-11.

Mendelsohn, Everett. "The Social Construction of Scientific Knowledge." In The Social Production of Scientific Knowledge. Vol. 1, pp. 3-26. Edited by Everett Mendelsohn, P. Weingart, and R. Whitley. Dordrecht, Holland: D. Reidel, 1977.

Mills, John A. "Hull's Theory of Learning as a Philosophical System: I. An Outline of the Theory." Canadian Psychological Review 19 (1978): 27-40.

----- "Hull's Theory of Learning as a Philosophical System: II. A Criticism of the Theory and its Relationship to the History of Psychological Thought." Canadian Psychological Review 19 (1978): 116-127.

- Moore, R. Laurence. In Search of White Crows: Spiritualism, Parapsychology, and American Culture. New York: Oxford University Press, 1977.
- Morison, Samuel E. Three Centuries of Harvard: 1636-1936. Cambridge, Massachusetts: Harvard University Press, 1936.
- Munroe, Ruth L. Schools of Psychoanalytic Thought: An Exposition, Critique, and Attempt at Integration. New York: Holt, Rinehart and Winston, 1955.
- Murphy, Gardner. An Historical Introduction to Modern Psychology. New York: Harcourt, Brace and Co., 1929.
- Personality: A Biosocial Approach to Origins and Structure. New York: Harper, 1947.
- Murphy, Gardner, and Kovack, Joseph K. Historical Introduction to Modern Psychology. 3rd ed. New York: Harcourt Brace Jovanovich, 1972.
- Murstein, Bernard I. Theory and Research in Projective Techniques (Emphasizing the TAT). New York: Wiley, 1963.
- Nunnally, Jum C. Introduction to Psychological Measurement. New York: McGraw-Hill, 1970.
- Oates, Stephen B. To Purge this Land with Blood: A Biography of John Brown. New York: Harper & Row, 1970.
- O'Donnell, John M. "The Origins of Behaviorism: American Psychology, 1870-1920." Ph.D. dissertation, University of Pennsylvania, 1979.
- "The Crisis of Experimentalism in the 1920's: E. G. Boring and His Uses of History." American Psychologist 34 (1979): 289-295.
- Olmstead, James M. D., and Olmstead, E. Harris. Claude Bernard and the Experimental Method in Medicine. New York: Henry Schuman, 1952.
- Osterhout, W. J. V. "Jacques Loeb: 1859-1924." In Biographical Memoirs. Vol. 13, pp. 318-401. Washington, District of Columbia: The National Academy of Sciences, 1930.
- Parascandola, John. "Organismic and Holistic Concepts in the Thought of L. J. Henderson." Journal of the History of Biology 4 (1971): 63-113.
- Perry, Ralph E. The Thought and Character of William James. 2 vols. Boston: Little, Brown, 1935.

- Peters, R. S., ed. Brett's History of Psychology. Cambridge, Massachusetts: M.I.T. Press, 1965.
- Potts, David B. "Social Ethics at Harvard, 1881-1931: A Study in Academic Activism." In Social Sciences at Harvard, 1860-1920: From Inculcation to the Open Mind, pp. 91-128. Edited by Paul Buck. Cambridge, Massachusetts: Harvard University Press, 1965.
- Powell, Arthur G. The Uncertain Profession: Harvard and the Search for Educational Authority. Cambridge, Massachusetts: Harvard University Press, 1980.
- "Public Health Statesman." Time, 26 November 1956, p. 50.
- Rabin A. I., Aronoff, Joel, Barclay, Andrew M., and Jucker, Robert A., eds. Further Explorations in Personality. New York: Wiley, 1981.
- Reisman, John M. A History of Clinical Psychology. New York: Irvington, 1976.
- Rieber, Robert W. "Henry A. Murray." In The Roots of American Psychology: Historical Influences and Implications for the Future, pp. 321-322. Edited by Robert W. Rieber and Kurt Salzinger. New York: The New York Academy of Sciences, 1977.
- Ringer, Fritz K. The Decline of the German Mandarins: The German Academic Community, 1890-1933. Cambridge, Massachusetts: Harvard University Press, 1969.
- Roazen, Paul. Freud and His Followers. New York: Knopf, 1976.
- Roback, Abraham A. "Morton Prince: 1854-1929." American Journal of Orthopsychiatry 10 (1940): 177-184.
- Roback, Abraham A., ed. Present-Day Psychology: An Original Survey of Departments, Branches, Methods, and Phases, Including Clinical and Dynamic Psychology. New York: Philosophical Library, 1955.
- Ross, Dorothy G. Stanley Hall: The Psychologist as Prophet. Chicago: University of Chicago Press, 1975.
- "The Development of the Social Sciences." In The Organization of Knowledge in Modern America, 1860-1920, pp. 107-138. Edited by A. Oleson and J. Voss. Baltimore, Maryland: Johns Hopkins University Press, 1979.

- Rucci, Anthony J., and Tweeny, Ryan D. "Analysis of Variance and the 'Second Discipline' of Scientific Psychology: A Historical Account." Psychological Bulletin 87 (1980): 166-184.
- Rudolph, Frederick. The American College and University: A History. New York: Knopf, 1962.
- Samelson, Franz. "Putting Psychology on the Map: Ideology and Intelligence Testing." In Psychology in Social Context, pp. 103-168. Edited by Allan R. Buss. New York: Irvington, 1979.
- Sanford, R. Nevitt. "Reminiscences and Celebrations." Paper presented at the annual meeting of the American Psychological Association, Toronto, Canada, August 1978.
- "Murray's Clinic as a Place to Learn." In Learning After College, pp. 104-114. Edited by C. Comstock. Orinda, California: Montaigne, 1980.
- "Murray's Personological System is Shaped by Numerous and Various Influences." Paper presented at the annual meeting of the American Psychological Association, Los Angeles, California, August 1981.
- Sanders, Jennings B. A College History of the United States. Vol. 2. Evanston, Illinois: Row, Peterson & Co., 1962.
- Shneidman, Edwin S. "Personology Studies Living, Historical, Fictional, and Mythological Figures and Complexes." Paper presented at the annual meeting of the American Psychological Association, Los Angeles, California, August 1981.
- Shneidman, Edwin S., ed. Endeavors in Psychology: Selections from the Personology of Henry A. Murray. New York: Harper & Row, 1981.
- Shneidman, Edwin S., Joel, Walther, and Little, Kenneth B. Thematic Test Analysis. New York: Grune & Stratton, 1951.
- Smith, Laurence D. "Behaviorism and Logical Positivism: A Revised Account of the Alliance." Ph.D. dissertation, University of New Hampshire, 1983.
- Smith, M. Brewster. "Announcement." Paper presented at the annual meeting of the American Psychological Association, Toronto, Canada, August 1978.
- "Personology Encompasses a Wide Range of Concerns, Including Human Values." Paper presented at

the annual meeting of the American Psychological Association, August 1981.

Stein, Morris I. The Thematic Apperception Test: An Introductory Manual for Its Clinical Use with Adults. Cambridge, Massachusetts: Addison-Wesley, 1955.

Stern, Sheldon M. "William James and the New Psychology." In Social Sciences at Harvard, 1860-1920: From Incultation to the Open Mind, pp. 175-222. Edited by Paul Fuck. Cambridge, Massachusetts: Harvard University Press, 1965.

Sulloway, Frank J. Freud, Biologist of the Mind: Beyond the Psychoanalytic Legend. New York: Basic Books, 1979.

Taylor, E. W. "Morton Prince, M.D., LL.D.: 1854-1929." Archives of Neurology and Psychiatry 22 (1929): 1031-1036.

Tomkins, Silvan. Contemporary Psychopathology: A Source Book. Cambridge, Massachusetts: Harvard University Press, 1946.

----- "Personology is a Complex, Life-Long, Never-Ending Enterprise." Paper presented at the annual meeting of the American Psychological Association, Los Angeles, California, August 1981.

Triplet, Rodney G. "The Relationship of Clark L. Hull's Hypnosis Research to His Later Learning Theory: The Continuity of His Life's Work." Journal of The History of the Behavioral Sciences 18 (1982): 22-31.

Triplet, Rodney G., and Woodward, William R. "Review of Freud: The Man and the Cause by Ronald W. Clark." Annals of Science 39 (1982): 83-84.

Watson, Robert I. The Great Psychologists. 3rd ed. Philadelphia: Lippincott, 1971.

Wheeler, Lynde P. Josiah Willard Gibbs: The History of a Great Mind. New Haven, Connecticut: Yale University Press, 1951.

Williams, Griffith W. "Clark L. Hull and His Work on Hypnosis." International Journal of Clinical and Experimental Hypnosis 1 (1953): 1-3.

Winternitz, M. C. "Alan Gregg, Physician." Science 126 (1957): 1279.

Veysey, Laurence R. The Emergence of the American University. Chicago: University of Chicago Press, 1965.

Yeomans, Henry A. Abbott Lawrence Lowell: 1856-1943.
Cambridge, Massachusetts: Harvard University Press,
1948.

Young, Kimball. "Review of Explorations in Personality."
American Sociological Review 4 (1939): 579-589.

-