PSYCHOLOGISTS AND PHYSICIANS IN THE BORDERLANDS OF SCIENCE, 1900-1942

By

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For my mother:

WILLIE MERLE PICKREN,
and in memoriam, BILL PICKREN,
You taught me to love and work.
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Abstract of Dissertation Presented to the Graduate School of the University of Florida in Partial Fulfillment of the Requirements for the Degree of Doctor of Philosophy

PSYCHOLOGISTS AND PHYSICIANS IN THE BORDERLANDS OF SCIENCE, 1900-1942

By

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The central argument of this dissertation is that psychologists availed themselves of opportunities offered to them in the first four decades of the twentieth century to expand the boundaries of their discipline. The expansion of the boundary between psychology and medicine through the creation of shared "borderlands" is the focus of this study.

Three case studies are used to illustrate the interaction of multiple factors in the development of these borderlands. The first case is located in time in the years, 1900-1920. Psychologists responded to the invitation of psychiatrists to work with them in the explication of mental disorders. Psychologists were able to move beyond the parameters of the invitation to secure a niche for their expertise in psychiatric settings.

The second case study presented here is the work of
psychologists during the 1920s in the borderland of sex research created by social concerns and philanthropic interests in social engineering. Although this area had traditionally been the preserve of medical expertise, psychologists were able to establish themselves as important contributors to the scientific understanding of sex and as competent managers of a large scientific enterprise.

The third case study concerns the work of psychologists in the development of a new borderland of science in the 1930s: psychosomatic medicine. Psychologists' contributed to this new field in two ways. First, they utilized an experimental methodology to investigate theories about human mental dysfunction. This work served as a bridge to physicians who were also interested in psychological factors in human illness. Secondly, psychologists contributed organizational leadership that provided an institutional framework for psychologists and physicians to jointly develop psychosomatic medicine.

This study takes a contextualist approach to the understanding of psychologists' involvement in borderland science. Attention is given to the social, intellectual, and practical contexts of this involvement. I rely on primary, archival, and secondary sources to fashion a narrative about a hitherto unexplored part of the modern story of psychology.
INTRODUCTION

All natural sciences aim at practical prediction and control, and in none of them is this more the case than in psychology today. We live surrounded by an enormous body of persons who are most definitely interested in the control of states of mind, and incessantly craving for a sort of psychological science which will teach them how to act. What every educator, every jail-warden, every doctor, every clergyman, every asylum-superintendent, asks of psychology is practical rules. Such men care little or nothing about the ultimate philosophic grounds of mental phenomena, but they do care immensely about improving the ideas, dispositions, and conduct of the particular individuals in their charge.¹

(William James, 1892)

The extension and expansion of psychology along a boundary shared with medicine through opportunities offered and taken by psychologists in the first four decades of the twentieth century is the concern of this work. The central argument of the chapters that follow is that these opportunities sprang from particular social contexts and that the growth of the relationship between psychology and medicine was due to the complex interaction of social, intellectual, and institutional factors. The three case studies used to illustrate this interplay are the movement of psychologists into psychiatric settings in the first two decades of the twentieth century, the involvement of psychologists in sex research in the 1920s, and the
contribution of psychological research in experimental psychopathology to the fashioning of the new interdisciplinary field of psychosomatic medicine in the 1930s.

These three case studies illustrate the importance of social and institutional factors in the growth of the discipline of psychology. The importance of disciplinary intellectual ideals in shaping that growth is also demonstrated in these three cases. I wish to show that these case studies are particularly good examples of disciplinary expansion through applied science. The move to application represented by these cases is indicative of the convergence of social and intellectual interests in the first part of the twentieth century and this history is a needed addition to the existing scholarly literature on the development of psychology as a science-based profession.

Recent scholarship in the field has moved from general histories of psychology to more specialized studies of particular facets of the discipline. These studies have tended to be sensitive to the social context of disciplinary growth. The expansion of psychology through application has been the focus of much of this recent, critical scholarship. Psychology's expansion along borders shared with education, forensics, advertising, and industry has been the subject of several recent studies. Scholarly studies of psychologists' interactions with physicians in mental testing,
developmental studies, and the neurosciences have added to our understanding of the growth of psychology through application. In the chapters that follow, I address some of the gaps in the historical understanding of psychologists’ involvement with the medical profession.

First, I discuss what is meant in this study by discipline, boundaries, and borderlands. I then turn to the boundaries of psychology and offer an account of where psychology’s boundary areas were at the turn of the century. The social and professional context of the movement toward application of psychological science is addressed, followed by a discussion of disciplinary expansion through application. Examples are offered in several areas. Penultimately, I turn to a general consideration of the boundaries of psychology and medicine in the period encompassed by the beginning years of this study. Finally, I offer a brief historiographical note on the approach I have taken in this study.

Disciplines, Boundaries, and Borderlands

Disciplines

A necessary precondition to the discussion of boundary work between disciplines is to define what a discipline is. Disciplines are differentiated communities of investigators who share subject matter, research problems, methods, and investigative tools. Disciplines exercise a certain measure of control over what counts as disciplinary knowledge and
exert authority over what qualifies an individual to participate in the discipline's knowledge-production process. Disciplines function to give their members a sense of institutional identity and to delineate the intellectual and social territory and resources that "belong" to that discipline. A discipline also structures the relationship between its members and the larger society, while at the same time the discipline is itself a product of that society.

Disciplines afford a useful opportunity to study both internal and external factors in science and the production of scientific knowledge. Scientific disciplines emerge in social contexts and are shaped by a diversity of factors that include intellectual and social processes. These processes may arise from within the discipline or may originate from the larger social domain. Charles Rosenberg has termed this interaction between society, disciplines, and expertise the "ecology of knowledge." As scientific disciplines emerge from the social matrix, their continued success is dependent upon their ability to adapt to changing social and intellectual demands. The valence of adaptation may vary; adaptation may mean a change in methodology or a movement to justify the discipline's existence in terms of practical utility. Successful adaptation requires that opportunities for enhanced survival must be taken. For example, Robert Kohler, in his
disciplinary history of biochemistry, has traced the responses to professional opportunities taken by biochemists in the formative years of the discipline. Depending upon their location, in departments of chemistry, biology, or medicine, the opportunities for personal and disciplinary success varied. Success depended on the biochemists' ability to adapt to their niche by finding ways to garner support and wisely utilize available resources.

Boundaries

What emerges from a careful reading of disciplinary histories is a notion that disciplinary boundaries serve to set off areas of inquiry from other, perhaps closely related, disciplines. That is, boundary lines are demarcations that allow for definition of problems and methods, as well as delineate professional identity. But, boundaries of scientific disciplines are not impermeable or impenetrable barriers. There is always the potential for exchange between disciplines.

Boundary exchanges may be likened to traffic flow across a border. At rush hour, there may be a constant movement of ideas and personnel as problems arise that demand the expertise of more than one discipline. At other times, the boundary may be closed to through traffic and the threat of hostile action may temporarily stop all exchanges. These boundary exchanges are perhaps most easily noticed among the disciplines that emerged from moral philosophy:
economics, sociology, psychology, political science, and philosophy. From a historical perspective, psychology's continued traffic with its parent and near neighbor, philosophy, as both struggled toward independent, institutional identity around the turn of the century, is an example of one type of permeable and problematic boundary. From a contemporary perspective, economics offers an example of a discipline with a high volume of cross-boundary exchanges.

Beyond the social sciences, disciplines basic to medicine also have had a history of permeable boundaries. For example, Robert Kohler has argued that biochemistry was "profoundly influenced by a diverse traffic in ideas and problems with neighboring disciplines." There was a steady flow of ideas and personnel between biochemistry and chemistry, physiology, and biology.

Historically, permeability of borders has also created contested areas between disciplines, whether the disciplines were biochemistry and its neighbors or psychology and the other sciences that emerged from moral philosophy. Contested areas are a characteristic of the dynamics of boundary-work. The likelihood of confrontations and controversies increases when disciplinary and professional identities are threatened by the overlap between disciplines. This may especially be the case when remuneration for services is part of the work of at least
one of the groups involved. The emergence of mental testing as a psychological specialty with broad applications to American life was a classic case of contested borderlands. The mental health field since World War II has at times suffered from a kind of "guerilla warfare" as issues of qualification and remuneration from third-party-payers have led to intense confrontations over the right to practice psychotherapy. The boundaries of science, then, to quote Thomas Gieryn, "are ambiguous, flexible, historically changing, contextually variable, internally inconsistent, and sometimes disputed." 

**Borderlands**

The permeability of disciplinary boundaries and the potential overlap of disciplines often create scientific borderlands, areas of investigation and/or practice that fall between disciplines. Impetus for the creation of borderlands comes from various sources. New methodologies or technologies may emerge that allow for the redefinition of a set of scientific problems in terms that require the resources of more than one discipline. For example, molecular biology was a scientific borderland that emerged as a response to new technology and methodology. Intellectual demands, such as the expressed desire for a unified science of evolutionary biology, may also create a scientific borderland, as V. B. Smocovitis has recently shown.
A unique example of a scientific borderland was the shared space of inquiry and practice created by psychiatry and psychology in the first years of the twentieth century, the subject of chapter one of this study. This borderland resulted from the desire of leading psychiatrists to incorporate the perceived experimental rigor of the New Psychology for the purpose of enhancing the status of psychiatry. The favorable response from psychology was an indication of the trend in the field at the time to seek legitimation through utility.15

Borderlands may also be created in response to social concerns, when the problems are perceived to fall within the purview of more than one discipline. One example of this was the borderland of sex research formed in the 1920s and a major focus of this study.

Increasing specialization may also, paradoxically, lead to borderland formation as researchers differentiate within their areas and find shared interests with workers from other disciplines. Julie Klein has called this approach to borderland formation "bridge-building," because it "preserves disciplinary identities."16 This was the case, as demonstrated in chapter three of this work, in the formation of psychosomatic medicine in the late 1930s as experimental psychologists found shared interests and concerns with psychoanalytically-oriented psychiatrists and physicians.
These exigencies are not exclusive; Lily Kay has shown the importance of social and institutional demands in the formation of the molecular biology borderland, and my chapter on sex research clearly indicates that it was a borderland that required the tools and methodologies of multiple sciences. Borrowing, collaboration, and interpenetration are common when multiple sciences are involved in the exploration of a borderland.

**Borderland Science and Foundations**

Since the 1920s, external funding sources have played a powerful role in the creation and shaping of borderland science. Indeed, it is probable that one agenda of philanthropies and other granting agencies has been to gain greater control over the direction of scientific research. Borderlands create uncertainty over authority, since the territory being explored "belongs" to more than one discipline. When uncertainty is created, funding agencies can provide direction and authority and, thus, play the role of power broker.17

Large-scale philanthropies were created in the first three decades of the twentieth century as American industrialists, such as John D. Rockefeller and Andrew Carnegie, turned from the accumulation of wealth to its distribution for public beneficence. From the establishment of the Carnegie Corporation in 1911 and the Rockefeller Foundation in 1913, one clear direction of this beneficence
was toward scientific research. Foundation managers, at the Carnegie, Rockefeller, and a miscellany of smaller philanthropies, sought to utilize their funds to ameliorate social ills, advance scientific knowledge, and build institutions through which their aims could be expedited. An impressive body of scholarship has been devoted to the delineation of these philanthropic aims in various disciplines, universities, and research programs. This research has shown that philanthropies, particularly the Rockefeller Foundation, exerted a powerful influence over the development of American social and natural sciences, especially in the interwar years.

American scientists, for their part, were not passive recipients of foundation funds. Many scientists and their institutions actively sought monies from the various philanthropies. For example, the astrophysicist, George Ellery Hale diligently pursued foundation funds, both for his scientific research and for the nascent National Research Council (NRC). In the 1920s, Yale University's president, James R. Angell, solicited funds from the Rockefeller Foundation to underwrite an experiment in the institutionalization of collaborative research, the Institute of Psychology. By the 1930s, the interests of foundations, academic institutions, science organizations, and many individual scientists were so enmeshed that it was
often difficult to determine the genesis of a particular research project.

One result of this convergence of foundation, academic, and scientists' interests, was the institutionalization of a collaborative ethos. Borderland science became more than a metaphor for this ethos, it was the "official" designation for multidisciplinary research and actively promoted by the NRC and philanthropies. As Glenn Bugos has shown, one key to obtaining philanthropic resources in the 1930s was presenting potential research projects under the rubric of "borderlands science." Projects as diverse as sex research, the effects of radiation, molecular biology, and psychosomatic medicine were funded and pursued as borderland science.

Examples abound of the emphasis on borderland research as the expressed or preferred style of science in the interwar period. Pnina Abir-Am has traced the influence of Rockefeller Foundation philanthropy on the development of the borderland between the physical sciences and biology. One of the numbers of the first volume of Perspectives on Science was devoted entirely to work in the borderlands of biology at the University of Chicago. A common theme of the five contributions to the special issue was the pursuit of interdisciplinary research at the University of Chicago in the interwar period. Much of this research was made possible by philanthropic grants to the individual
scientists involved. Characteristic of the work at Chicago was its potential for application to social problems. As I show in this study, application, actual or potential, was usually a vital part of the attraction for the scientists and the philanthropies involved in borderland science.

The multiple examples of borderland research at the University of Chicago suggest that practical problems, extra-institutional support, and the intellectual interest of individual scientists all contribute to the initiation and continuation of work along the boundaries of science. Similar interests and factors were at work in the emergence of psychology as an academic discipline and science-based profession in the first decades of the twentieth century.

**Boundary Work in Psychology**

The New Psychology stood poised on the brink of rapid expansion in 1900. Formed out of the physiological psychology of Wilhelm Wundt, the evolutionary theory of Charles Darwin, and moral philosophy, the new science had found a niche in the emergent American university. Its practitioners were imbued with the ideals of experimentalism: that real psychological science was that which had its basis in the laboratory. Psychologists were increasing in number every year, many of them certified by extended stays in German laboratories. By 1899, there were at least ninety recipients of the PhD in psychology in America.23 The number of laboratories for psychological
experimentation grew more slowly, from one in 1883, established by G. Stanley Hall at Johns Hopkins, to thirty-three in 1899. By 1900, the New Psychologists had a professional society, the American Psychological Association, and specialized publication outlets, the American Journal of Psychology, Pedagogical Seminary, Psychological Review, and the Psychological Monographs. All of these can be considered markers of success, indicators that psychologists had, at least, begun the establishment of a disciplinary identity. To echo Thomas Gieryn, the New Psychology was finding a niche in the intellectual ecosystem through the continuing processes of boundary-work designed to achieve an apparent differentiation of goals, methods, capabilities and substantive expertise.

The Problematic Boundaries of Psychology in 1900

Since the initiation of the new, experimental psychology in America, psychologists had sought to differentiate their discipline from the philosophically-oriented "old" psychology; to distinguish their expertise from that of physiology; and to distance their work from psychological practices considered non-scientific, such as psychical research. John O'Donnell has suggested that this "search for autonomy was essential to psychology's institutional survival and constitutes the keystone of its intellectual development after 1892."
The new versus the old psychology

Psychology was not new to America in 1875, when William James offered a course in "The Relations between Physiology and Psychology." Psychology had long had a place in American colleges, frequently being featured as the capstone course in the undergraduate philosophy curriculum. What was new in the last quarter of the nineteenth century was the institutionalization of psychology as a distinct academic discipline, with its own methods, rationale, and personnel. This New Psychology, as its practitioners called it, was different from the armchair speculations of the old psychology in its emphasis on experiment and laboratory.

The separation of psychology from philosophy was never clear-cut, despite Edward Buchner's claim in 1903 that "philosophy has slowly, but certainly lost psychology forever." Many psychologists retained a vital interest in philosophical issues and many philosophers sought validation of their ideas in the experimental methodology of the New Psychology. The relationship between the two disciplines was "problematic" and the issue of separation continued well into the twentieth century. Psychologists at the time, such as Buchner, however, had a stake in portraying themselves and their work as distinct from philosophy. Such distinction made the New Psychologists more fit in the competition for institutional recognition and resources in the changing academic ecosystem. Many psychologists took
every opportunity to portray their uniqueness and their capacity for making distinct contributions to society. This was especially true in the popular writing of psychologists at the turn of the century.

Psychologists frequently wrote for popular consumption in the fin-de-siècle. One use of popular media was to garner support for their science. Popular support was an integral component of their efforts toward legitimation, in that the university trustees and administrators who formulated academic policy were sensitive to the concerns of the informed public, especially since the children of that public made up the bulk of their institutions' student populations.\textsuperscript{30} The question of the difference between the New Psychology and the old was frequently presented in these popular pieces.\textsuperscript{31} Psychologists such as G. Stanley Hall, E. W. Scripture, and John Dewey vaunted the superiority of the new over the old psychology. For example, Scripture, the director of the psychology laboratory at Yale University, wrote that the knowledge gained by the old psychology amounted to "a mass of ingenious speculations." What the old tried to do, the new psychology was accomplishing, Scripture averred.\textsuperscript{32} G. Stanley Hall, the first American to receive a PhD in psychology, likened the old psychology to "boiled cobwebs" at its best and totally lacking in power, at its worst. The New Psychology, by contrast, was transformative for philosophy, "all-conditioning for
education, and full of promise for religion." Dewey praised the old for having begun to examine the complexity of the human mind, but the New Psychology had now the requisite equipment and methods to bring genuine advances in the investigation of mental phenomena. Through the use of such strategies, the New Psychologists sought to maximize their opportunities to distinguish themselves and their discipline from philosophy.

The new psychology and physiology

The methodological source of the New Psychology was physiology. Physiology also provided most of the apparatus used in early experimental work in psychology. This near-wholesale adoption of physiological approaches led to boundary problems when the Americans who traveled to Germany to study with Wundt adopted his methods and brought them back home. Hugo Münsterberg, a product of Wundt's laboratory, warned at the first meeting of the American Psychological Association in December, 1892, that "if psychology unites with physiology in order to cut loose from philosophy, it must lose more than it can gain." Münsterberg's warning was relevant because many of the experimental problems psychologists set for themselves were really physiological problems in flimsy disguise. This followed from Wundt and his training in physiology. Although Wundt included both an experimental and a historical-cultural approach to psychology, it was his
experimentalism that proved problematic for the development of psychology. Wundt's desire to be experimentalist in method and his commitment to sensory physiology as providing the basis for experimentation left him with little choice but to confine his research to the simplest sensations and perceptions, while attempting to confine the reports of those sensations and perceptions to the simplest possible language. This conflation of subject matter and methodology was at the base of the boundary problems between psychology and physiology in this period. O'Donnell has stated that "psychology's central problem in the 1890s was defining its boundaries between philosophy and physiology."36

It certainly seemed that way to George Stuart Fullerton in 1895. In an address to the American Psychological Association, Fullerton agreed that one public perception of psychologists was that they often did work which appeared purely physiological and acknowledged that the boundary line was difficult to draw between the two disciplines.37 The answer that Fullerton and other psychologists offered for this dilemma varied, but usually centered around the need to move away from simple sensory or perceptual processes to the experimental investigation of the higher mental processes. Little such work was done; what finally moved the New Psychology away from both philosophy and physiology was the pull toward utility. I return to this below.
The new psychology and psychical research

While the New Psychologists were willing to acknowledge a kinship with physiology and philosophy, they were not so willing to acknowledge their boundary with the realm of psychic phenomena. This boundary was populated by the advocates of spiritualism, seances, telepathy, and other psychic phenomena. David Leary has done an excellent job on the competition between the psychic phenomena adherents and the New Psychologists and how the psychologists sought to discredit psychical research. Leary emphasized the institutional or professional advantage sought by the New Psychology in this competition but provided little evidence of how the New Psychologists sought to persuade the educated public as to the merits of their case.

Joseph Jastrow, a Hall PhD and chairman of the psychology department at the University of Wisconsin, noted that psychology was still thought of by the "average educated man" as studying the same class of phenomena as psychical researchers. This after many of the New Psychologists had devoted considerable effort to delegitimizing psychic phenomena. The yardstick against which the psychical researchers were most often measured and found wanting was the scientific method. Hugo Münsterberg, in a clever debunking of psychic phenomena, did not dispute that psychical researches had produced interesting results. However, these results "are never incapable of a scientific
explanation." What the psychical researchers had failed to do, in Münsterberg's eyes, was follow proper scientific method. If they had, they would have discovered a natural, psychological, scientific explanation for their results. The New Psychologists consistently used language designed to undermine the credibility of psychical research. Such research was termed "twilight literature," a "fraud," a "desert of absolute stupidity," and contrary to "common sense." The most powerful argument, however, lay in the New Psychology's ability to explain the same phenomena in terms of science. In other words, what these phenomena needed in order to be rationally explained was the proper application of science. That the New Psychology was not wholly successful can be seen in the quote from Jastrow; despite their best efforts, psychology and psychic phenomena remained linked for much of the educated public.

Deborah Coon has suggested that one aim of the assault on psychical research by psychologists was the further legitimation of psychology as a science. Coon's view accords with my own, that psychology's acts of boundary definition vis-a-vis philosophy, physiology, and psychical research were attempts to make salient what was distinct and worthy in the New Psychology. Each of these neighbors posed threats to the disciplinary identity of psychology. By distinguishing their discipline, often at the expense of its near-neighbors, psychologists positioned themselves to take
advantage of new opportunities for expansion as the new century began.

Expansion through Application

A foreign observer of American psychology noted in 1913 that psychology was experiencing its greatest growth at its boundaries. Opportunities for expansion came along many of the boundaries that psychology shared with other emerging disciplines and in response to social and institutional demands. The greatest growth opportunities were in the direction of application.

Opportunities and opportunism

Psychologists were opportunists. As the new century dawned, many psychologists were so positioned professionally that they needed, and desired, to expand the boundaries of their discipline whenever and however they could. Employment pressures (by 1900 there were three PhDs for every laboratory), the desire that research be applicable to real life, and the leadership example of well-known psychologists like James McKeen Cattell were intra-disciplinary factors in the growth of psychological applications.

Social factors were also important in the movement toward application. Against the backdrop of the larger American scene, the development of psychology can be seen as part of a general rationalization of society. Along with a number of other new disciplines (e.g., economics, business
management), psychology grew and prospered as it responded
to the needs and demands of a society "in search of
order." As Robert Wiebe has pointed out, this was the
time of the rise of the expert who restricted himself to a
specialized function in order to increase his power and
secure his niche in the world. Many psychologists
believed that their discipline could make significant
contributions to bringing the kind of order, efficiency, and
productivity to human affairs that the natural sciences had
brought to the physical world. In so believing, they were
responding to a felt need from the larger community.

Psychologists also felt the demand for utility from
their professional location in academia. American
universities sought to provide trained personnel to fill the
new professional niches created by the demands for a more
efficient and orderly society. Efficiency (typically used
as a synonym for the traditional values of hard work and
thrift), social reform, and control were the promises of
these new professionals. Thus, social factors helped create
opportunities for the expansion of the New Psychology.
William James expressed this with his usual eloquence,
"What every educator, every jail-warden, every doctor, every
clergyman, every asylum superintendent asks of psychology is
practical rules." Psychologists were not short on promises for such
practical rules, nor were they dilatory in seeking
opportunities for application. As early as 1890, Harry Kirk Wolfe, another Wundt PhD, was writing about the usefulness of the New Psychology for education. G. Stanley Hall, at Clark University, was also praising the usefulness of psychology for education by 1890. But education was not the only field of opportunity perceived by psychologists in the 1890s. William Lowe Bryan, of Indiana University, was approached in 1896 by a railroad company to study the psychological processes involved in telegraphy. James McKeen Cattell’s development of mental tests in the 1890s and their use in the measurement of individual differences paved the way for a whole new avenue of application in a variety of fields. By 1900, application was a growth field for psychology. Opportunities to apply psychology were varied and as Madison Bentley, the Cornell psychologist, later recalled, "the pressure to extend psychology toward the practical arts came chiefly from those arts themselves and not from a growing abundance of facts and principles within psychology itself."

Recent scholarship has examined some of the instances of this growth through application. Matthew Hale, in his study of the Harvard psychologist, Hugo Münsterberg, has shown how Münsterberg moved from an emphasis on basic, laboratory psychology to extensive applications of psychology. Münsterberg’s practical applications of psychology included psychotherapy and forensics.
Münsterberg argued that psychologists must accept the practical needs of the world and find ways to apply their science to those needs.\textsuperscript{52}

John O'Donnell has charted the movement of psychologists into applied fields from their beginnings in the laboratory. This movement was partly due, he argues, to employment pressures. Applications of their science promised opportunities for personal and professional gain. O'Donnell cites the work of psychologists in industrial applications, advertising, and most of all, education.

Advertising was one of the early clients of psychology and captured the attention of one psychologist who went on to become an APA president, Harry Hollingworth. Industrial applications were not uncommon in this time. O'Donnell cites over 40 industry-funded psychological investigations in the years 1907 to 1916.\textsuperscript{53} But the greatest opportunity for expansion was in education.

"Applied psychology is in many a quarter the pay-vein that supports the mine. The educational application is the oldest and most comprehensive."\textsuperscript{54} These words of Joseph Jastrow’s reflect what more and more psychologists came to realize in the first years of the current century. O'Donnell estimates that by 1910, over three-quarters of American psychologists who were involved in applied work were involved in educational applications.\textsuperscript{55} Donald Dewsbury has suggested that many comparative psychologists
moved into education because it presented a better opportunity for present and future employment.56

Geraldine Joncich and Kurt Danziger have addressed the reasons why psychologists were so successful in applying their science to education. Psychologists were successful, they argue, because they were able to meet the needs of education professionals, especially administrators. School administrators needed data that could justify their decisions about allocation of funds, placement of students, and hiring of teachers. Educational psychologists provided them with data gathered in an experimental fashion that facilitated the needed justification by placing the administrative decisions within the framework of a quantifiable science. Geraldine Joncich has touched on this in her biography of Edward L. Thorndike.57 Kurt Danziger has elaborated on this point and extended it through his critique of the social construction of psychological practice. Danziger has argued that the use of aggregate data by educational psychologists was solely a response to the market demands of the educational system.58 Joncich and Danziger have placed the success of psychologists into the framework of an opportunity taken. Educational applications presented an opportunity for the use of psychological methods; psychologists who were prepared to seize that opportunity found a more secure niche in their intellectual ecosystem.
A primary way, then, that psychologists expanded their discipline in the beginning of the twentieth century was through seizing opportunities for application. Scholarly contributions such as those of Hale, O’Donnell, Joncich, and Danziger have focused on particular aspects of this expansion. My study is a contribution to this field of scholarship that examines the expansion of psychology along some of the boundaries it shared with medicine.

Psychology and medicine

The boundary that psychology shared with medicine was long and winding. In this history, only a few of the many boundary exchanges are examined. Mental testing, developmental studies, child guidance, and neuroscience are examples of borderland work between psychology and medicine that have benefitted from previous careful historical analyses. My study is an addition to this scholarship that addresses other boundary exchanges that provide fruitful insights into the convergence of social, intellectual, and institutional factors in the development of American psychology. What I hope to show is that psychologists and physicians moved from a baseline of mutual interest in each other’s intellectual territory to a borderland of shared research and practice in the first four decades of the twentieth century.

The New Psychologists were interested in the potential application of their fledgling science to
medicine. The popular writing of psychologists in this period is a reflection of their interest. From G. Stanley Hall to James R. Angell to Joseph Jastrow, the New Psychologists envisioned the expansion of psychology along the boundary it shared with medicine. Hall, in his usual grandiloquent style, informed the readers of the mainstream magazine, *Harper's Monthly*, that the New Psychology encompassed the study of the whole realm of mental alienation, from those clearly insane to the near-normal borderline states. James R. Angell told the readers of *The Chautauquan* in 1905 that the best work being done in mental medicine was founded upon psychological principles and suggested that this was likely to be even more so in the future. Joseph Jastrow suggested to the readers of a popular monthly medical medicine that physicians needed the insights gained from psychology in order to provide better treatment for their patients. Although these popular pieces are perhaps best understood as examples of rhetoric aimed at legitimating the New Psychology in the eyes of their readers, they also indicate the interest of psychologists in medical concerns.

Further examples of this interest may be found in Eugene Taylor's edition of William James's 1896 Lowell lectures on exceptional mental states. James was strongly interested in abnormal mental states, the province of mental medicine. Dorothy Ross has highlighted G. Stanley
Hall's interest in medicine. Hall even sought an honorary medical degree from Harvard to further his position at Johns Hopkins University. When Hall was at Johns Hopkins University, he served on the oversight board for a local asylum. James McKeen Cattell recalled that Hall was intensely interested in psychopathology at this time and frequently took his students to the asylum in order to illustrate various types of mental disorder. After Hall moved to Clark University in Worcester, Massachusetts, he involved his students in the same type of interaction at the nearby Worcester State Hospital. When the psychiatrist, Adolf Meyer, became the director of the Hospital, Hall engaged him to give lectures on psychopathology to Clark's psychology graduate students. These two leaders of American psychology, James and Hall, were vitally interested in the potential interchange between psychology and medicine, especially in the field of psychopathology.

There was interest from both sides of the boundary between psychology and medicine in the 1890s. An examination of psychological journals in the 1890s bears this out. Physicians, especially psychiatrists, contributed case studies and an occasional piece of research to the American Journal of Psychology or, after 1894, to the Psychological Review. For example, in 1892 William Noyes, a psychiatrist at the McLean Hospital, published his study of the knee-jerk response of a man suffering from dementia.
In every issue, psychologists provided reviews of recent medical literature, usually tomes on abnormal states. Occasionally, a psychologist would contribute a study on a medical topic. For example, George T. W. Patrick, a Hall PhD from Johns Hopkins, published a case study of dissociation in 1898. Two Radcliffe psychology students, Frances Drury and Clara Folsom, published their study on the adverse psychological effects of examinations on mental functioning. They sought to place their work within the explanatory framework of known mental disorders. The point is, the boundary between psychology and medicine was not unknown to either psychologists or physicians. The creation of a borderland of shared work and research, however, did not begin until the very end of the nineteenth century. Then, from similar impulses and opportunities as noted above in the cases of education, advertising, and industry, a very small beginning was made toward the formation of a scientific borderland. The contingencies that fostered the growth of that borderland, as well as the intellectual stimuli provided by the shared work of research and practice are the substance of the study that follows.

Historiographic Note

This study takes a contextualist approach to the understanding of the borderlands efforts of psychologists in the first four decades of the twentieth century. The work of psychologists and the members of the medical profession
they interacted with is interpreted in the light of professional, social, and institutional ideals. There is no formal dichotomy of internal vs. external factors, nor is the emphasis on the linear progression of psychological science. These categories of explanation are collapsed into the story I attempt to tell. My readers will find that attention is paid to both internal and external influences on the interaction of psychologists and physicians, although neither is privileged. Careful readers will also detect a degree of progressivism in my story. But they will also note that it is not an attempt to write history backwards. Such attempts tell stories of progress from "bad" or "they didn't know what we know now" science to "good" or "it basically agrees with our current understanding" science. Rather, progress is found in the continuity of characters and their ability to learn from past opportunities how to utilize the multiplicity of resources in their present situation. In that sense, my story is progressivist. My characters became better opportunists in the professional and social niche they found in the ecology of science.

My story, then, fits within the "new history" of psychology, as Laurel Furomoto articulated its characteristics in her seminal paper on the subject. That is, my story is critical rather than ceremonial, it attempts to contextualize its subject rather than retell the story of
"great men," and it relies on primary sources and archival
documents to fashion its argument.  

Notes

1. William James, "A Plea for Psychology as a Natural
Science," Philosophical Review 1 (1892): 146-153, quote on
148, emphasis in the original.

2. On disciplinarity, see Roger Geiger, To Advance
Knowledge: The Growth of American Research Universities,
1900-1940 (New York: Oxford University Press, 1986), pp. 20-
30; Thomas Gieryn, "Boundary-Work and the Demarcation of
Science from Non-Science: Strains and Interests in
Professional Ideologies of Scientists," American
the Boundaries," Social Epistemology 4 (1990): 267-280; and
David R. Shumway and Ellen Messer-Davidow, "Disciplinarity:

3. See Mitchell Ash, "Introduction," in Psychology in
Twentieth-Century Thought and Society, eds., Mitchell Ash
and William R. Woodward (New York: Cambridge University
Press, 1987), pp. 1-11. Also, Steve Fuller, "Disciplinary
Boundaries and the Rhetoric of the Social Sciences," Poetics

4. G. Lemaire, R. MacLeod, M. Mulkay, and P. Weingart,
"Introduction: Problems in the Emergence of New
Disciplines," in Perspectives on the Emergence of Scientific
Disciplines, eds., G. Lemaire, R. MacLeod, M. Mulkay, and P.

5. Charles Rosenberg, "Toward an Ecology of Knowledge: On
Discipline, Context, and History," in The Organization
of Knowledge in Modern America, 1860-1920, eds., A. Oleson and
J. Voss (Baltimore: Johns Hopkins University Press, 1979),
pp. 440-455.

6. Robert E. Kohler, From Medical Chemistry to Biochemistry:
The Making of a Biomedical Discipline (New York: Cambridge
University Press, 1982).

7. Gladys Bryson, "The Emergence of the Social Sciences
from Moral Philosophy," International Journal of Ethics 42
(1932): 304-323.

8. On psychology and philosophy, see Michael M. Sokal,
"Origins and Early Years of the American Psychological
111-122. Several of the chapters in William R. Woodward and


10. ibid., p. 7.

11. I thank Wilse B. Webb for offering this needed qualification to borderland science. The phrase is his.


17. This was pointed out to me by Toby Appel, "Personal Communication, 21 February 1995." Appel noted that the encouragement of interdisciplinary research in post-World War II science by government funding agencies served the purpose of empowering those agencies, not the scientists.


21. Abir-Am, "The Discourse of Physical Power."


29. See O'Donnell, Origins of Behaviorism, ch. 4.


34. Dewey, "The New Psychology."


36. Ibid., p. 146.


41. Ibid., pp. 72, 76, 78.


46. ibid.

47. James, "Psychology as a Natural Science," see n. 1.


57. Geraldine Joncich, The Sane Positivist: A Biography of Edward L. Thorndike (Middletown, CT: Wesleyan University,
1968).


CHAPTER 1
FELLOW LABORERS: CULTIVATING THE BORDERLANDS OF PSYCHOLOGY AND PSYCHIATRY, 1900-1920

Medicine for years had been an integral part of my life, especially of my vocational imaginings and hero worship. Almost certainly, except for the advice and aid of Josiah Royce and Hugo Münsterberg, I should have become a physician or surgeon and presumably a practitioner, instead of a psychobiologist.

(Robert Yerkes, psychologist, 1950)

Physicians are anxious to learn what psychology can teach regarding normal mental processes and to acquire the methods which psychologists have found useful in the investigation of these mental conditions.

(Shepherd Ivory Franz, psychologist, 1906)

Only the broadly trained physician can remain free of the charge of opening the door to the a-scientific if not anti-scientific... On the whole we do well, I think, to advise our psychological friends to choose, at least as an avocation, a medical training, which I hope will some day figure as a worthy source of general culture, and as a way to learn the human machine and its vicissitudes in the broader relations of life.

(Adolf Meyer, psychiatrist, 1912)

My aim in this chapter is to show how a borderland of scientific work was opened and developed by psychologists and psychiatrists in the first two decades of the twentieth century. In the first years of the century, a few psychologists took advantage of opportunities provided by psychiatry to apply their science in a new field. Once in this new borderland, psychologists worked assiduously to
find a secure niche where their services would be a vital part of mental medicine. The cultivation and growth of this borderland was due to a convergence of social, institutional, intellectual, and practical factors. From this small beginning in the first years of the century, the number of psychologists working in psychiatric settings increased significantly by 1920. I wish to show in this chapter how psychologists were able to expand the borders of their discipline through the application of their science to psychiatric problems and practice.

For scientific psychologists, one point of potential application for their discipline was through an alliance with the medical profession. Since medicine was already acknowledged as engaged in the care, if not the cure, of minds, psychologists sought ways to make themselves and their knowledge useful. Psychiatrists interested in the transformation of their specialty into a science-based profession perceived that a scientific psychology might provide clues to the understanding of such elusive diseases as dementia praecox. This mutuality of interest provided an impetus for a relationship that both benefitted and limited experimental psychology. The benefit came from the expansion of scientific psychology into new areas of application. The limitation was that psychologists had to remain in a subordinate role to psychiatry.
My three foci are: 1) the invitations extended by psychiatrists to experimental psychologists to utilize the methods of experimental psychology in the investigation of problems of mental disorder and the subsequent response of psychologists to that invitation; 2) the attempted extension of psychology into the medical school curriculum by psychologists and psychiatrists who perceived the benefits that training in psychology could provide future physicians; and, 3) the experimental work of psychologists in psychiatric settings over the course of the first two decades of the twentieth century. The social, institutional, intellectual, and practical contexts of the three foci are examined. By placing the developing relationship between psychologists and psychiatrists within a particular "ecology of knowledge," I hope to throw light on the multi-determined nature of disciplinary development.

Introduction

The years around the turn of the twentieth century were a period of social change in American history. The social, political, and professional order was being transformed; new scientific disciplines were being formed and professions were being reformed. In this period psychiatry and psychology developed a commonality of ethos and aim as both were in the process of expanding their boundaries. The relationship they developed was one of great complexity, shaped by philosophical and professional issues.
Philosophically, both psychologists and psychiatrists held the view that the way to understand mental phenomena, whether in the normal or abnormal mind, was through the use of scientific methods. Professionally, psychiatry and psychology needed each other, to advance their individual claims and to extend the boundaries of psychological science and practice.

Psychologists' Agenda for Application

At the beginning of the twentieth century, psychologists were in the process of severing their ties to philosophy and attempting to secure a niche for their discipline in American society. They sought a place for their work both in academia and in applied settings. Of the two main schools of psychology at this time, structuralism and functionalism, it was the functionalists who were more likely to pursue the application of their science. Functionalism grew out of the tradition of pragmatism in American science and life and was centered on mind as a tool of adaptation. It held as its corollary the possibility of utility. Structuralism was centered on the contents of the mind; there was no promise of application. Functionalists who sought to extend the boundaries of their science were eager to find partners who would need their knowledge and with whom there could be mutual benefit. Medicine was a natural partner, or so it seemed to some of these psychologists. An early application of psychology was
in that part of medicine, psychiatry, whose members were also attempting to redefine and reinvent their practice in terms of a scientific ideal.

Leaders in the development of the new experimental psychology, such as William James and G. Stanley Hall, expressed a great interest in abnormal mental states. For example, James, at Harvard, taught a course in what would today be termed abnormal psychology each year from 1893 to 1898. Hall, at both Johns Hopkins University and Clark University, lectured on mental pathology and involved his students in the observation of mental patients. This interest was generally shared in psychology. However, no psychologists were actively involved in working in a psychiatric setting until 1897. In that year William O. Krohn, a Yale PhD (1889), began a two-year stint as psychologist at Illinois Eastern Hospital for the Insane at Kankakee. Krohn established a laboratory, but published no research based on his laboratory work. Krohn left the hospital in 1899 and three years later entered medical school in Chicago. Boris Sidis, a Harvard PhD (1897), began a brief stint at the New York Pathological Institute under Ira Van Gieson either in 1897 or 1898. Sidis developed a laboratory for psychological investigations, but published little empirical research. Like Krohn, Sidis left psychology to enter medical school. Krohn and Sidis were the first psychologists to work in psychiatric settings, but
their work was that of pioneers in the borderland between psychiatry and psychology. The settling of the borderland and its full exploration did not come until 1904, when Shepherd Ivory Franz began his long career in psychiatric work. The possibility of a borderland between psychology and psychiatry was due, not only to psychologists' desire to expand the boundaries of their science, but also to the transformation of psychiatry and the consequent desire of psychiatrists to extend the range of their practice.

Medical Science and the Transformation of Psychiatry

Many psychiatrists, at the dawn of the new century, felt that their branch of the medical profession was twenty to thirty years behind the development of general medicine.\textsuperscript{11} Many leading psychiatrists sought to change this by placing psychiatry on a more scientific basis. Their model was the transformation of general medicine that had occurred with increasing rapidity during the nineteenth century.\textsuperscript{12}

The practice of medicine changed greatly in the last half of the nineteenth century. The development of the germ theory of disease by Pasteur, Koch, and their followers brought greater specificity to diagnosis and treatment of a variety of diseases. Concomitantly, the development of new diagnostic instruments also facilitated the improvement of both diagnosis and treatment. These developments also pointed toward the necessity of laboratory research in
sciences basic to medicine. As James Cassedy points out, the medical sciences changed in kind and in focus during this period. For example, anatomy changed as a result of developments in physiology and pathology. Increasingly, these sciences centered on the laboratory as the locus of their work.\textsuperscript{13}

The placement of medical practice on a more scientific basis led to greater success in the treatment of some diseases and raised the status of medicine in American society. These results were reciprocal with changes in medical education. The implications of the reform of medical education for psychology will be explored below. For psychiatry, these changes and reforms only highlighted the increasing distance between their specialty and mainstream medicine.

Psychiatry developed in America as a medical specialty in the context of custodial treatment for the insane. Early success (1830-1850) in the treatment of the insane in this context lent legitimacy and stature to psychiatry. During this time the professional association of hospital superintendents, the Association of Medical Superintendents of American Institutions for the Insane (AMSAII), enjoyed the respect and the interaction of their professional colleagues. In the second half of the nineteenth century, however, psychiatry became increasingly isolated from general medicine.
The institutional setting of the practice of psychiatry, the asylum, was physically and metaphorically removed from society. The physical separation of the asylum from society often contributed to a sense of isolation on the part of their superintendents. As the asylum population grew dramatically, there was an increasing emphasis on managerial and bureaucratic concerns over treatment concerns. Even when psychiatrists were able to focus on treatment, they often found that the treatments that had appeared effective earlier in the century proved to be much less ameliorative of mental suffering. The asylum became a place of hopelessness and appeared to many in the mainstream of medicine a symbol of pre-scientific medicine.

By the 1880s, some psychiatrists were working to change the image and the practice of psychiatry. They sought to emulate mainstream medicine by placing psychiatry on a scientific basis. In a symbolic move that indicated the new direction that psychiatry hoped to move, psychiatrists changed the name of their professional organization to the American Medico-Psychological Association. (It was changed to the current American Psychiatric Association in 1921.) Plans were made for the establishment of laboratories that would include the efforts of histologists, chemists, pathologists, and, in some case, experimental psychologists. It was hoped by the leaders in the transformation of
psychiatry that the causes and explanations of mental disorder would be provided through laboratory research.

The process that was begun at the end of the nineteenth century led to a redefinition of psychiatry. As attention was focused on the explanation of mental disorders, psychiatrists began moving away from their traditional role of institutional management. Increasingly, psychiatrists sought to make the broader community the locus of their work. As they expanded their boundaries, psychiatrists sought the assistance of other emerging fields cognate to their own. Experimental psychology was one of those fields.

An adumbration of the emergence of a borderland of shared work was the relationship that developed between Edward Cowles, an eminent psychiatrist, and G. Stanley Hall, an important leader in the development of experimental psychology. Edward Cowles, for many years the superintendent of the McLean Hospital outside Boston, was one of the most influential figures in the transformation of psychiatry. Cowles was so taken with the possibilities of the new experimental psychology that he came to Baltimore to study with Hall in the latter's last year (1887-1888) at the new Johns Hopkins University. Hall, the first person to receive a doctoral degree in psychology from an American university, was appointed Professor of Psychology and Pedagogy in 1884. In 1886 he was asked to assist in the oversight of nearby Bay View Asylum. Hall characterized his
appointment as a "locum tenens Superintendent, perhaps the only layman in medicine to occupy such a position."\textsuperscript{14}

Cowles came to study the New Psychology with Hall because he was interested in establishing a scientific basis for psychiatry and saw the new experimental approach to psychology as a potential partner for his own specialty.\textsuperscript{15} When Cowles returned to McLean, he established a research laboratory that included psychology among its topics of investigation. He employed as chief of the laboratory, William Noyes, a physician who had been a fellow with Hall at Johns Hopkins. While there was no psychologist qua psychologist on the laboratory staff until Shepherd Ivory Franz accepted a position there in 1904, psychological techniques of investigation were employed and the importance of a scientific psychology was acknowledged.\textsuperscript{16} Cowles stated that from the beginning the laboratory was for "experimental research in physiological-psychology, in the hope of ultimately adapting its methods to clinical and diagnostic uses."\textsuperscript{17}

Hall, in 1895, wrote a glowing account of the laboratory at McLean for the professional journal of American alienists and psychiatrists, the \textit{American Journal of Insanity}. In it he depicted the contributions of physiological psychology as vital to advances in the understanding of mental disease.\textsuperscript{18} Hall and Cowles's relationship was a precursor of the type of relationship
that held great potential for both psychiatry and psychology.

**Invitation to a New Borderland of Science**

As psychologists and psychiatrists moved toward a recognition of their mutual interests and possible mutual benefits, a dialogue between them emerged in the professional literature of the period. Psychologists and psychiatrists sought to define their relationship to each other and to define the boundaries of mental phenomena relevant to their respective fields. Professional and scientific journals of the day, such as the well established *American Journal of Insanity* and *Journal of Nervous and Mental Disease* or the more recently established *Psychological Bulletin* and *Psychological Review*, periodically published such discussions by leaders and workers from both psychiatry and psychology.

Many in the psychiatric and neurological community welcomed the new experimental psychology, as long as its practitioners did not cross the boundary into domain claimed by the psychiatrists and neurologists. Psychologists were welcomed as mental assayers, as laboratory men, whose purview was the elicitation of motor abilities and the elucidation of sensory and affective states, primarily among the "normal." Psychiatrists reserved the right of interpretation of experimental findings and treatment of the mentally disordered. Psychiatrists claimed that those who
were sick in mind or body lay within their province and that all others who claimed expertise in mental science must acknowledge their priority claim. At the turn of the century, psychologists were willing to move into a borderland with psychiatry on these terms for reasons of gaining acknowledgement of their fledgling discipline as a science. The initial invitation to develop the borderland between psychiatry and psychology came from psychiatry.

**Psychiatrists Extend an Invitation to Psychologists**

In 1896, Ira Van Gieson, the first director of the newly established Pathological Institute of the New York State Hospital, articulated a grand vision of mutuality and complementarity of all the sciences concerned with nervous and mental disease. In what he called the Correlation of Sciences, psychology, psychopathology, histology, cellular biology, anatomy, chemistry, pathology, and anthropology would together focus "a searchlight on the mysteries of mental disease." Van Geison was influenced in his thinking on this by a psychologist, Boris Sidis. Sidis, a Russian emigre, had been a student of William James and would later become a physician. As we shall see, Sidis had his own ideas about psychopathological states and the contributions that psychologists could make to their understanding. For the moment, however, what is of interest is that psychology was included among the sciences to be Correlated in Van Gieson’s grand plan.
Cowles and Van Gieson were not the only ones at the turn of the century to see the potential benefits of experimental psychology. Frederick Peterson, in his inaugural address as the new president of the New York Neurological Society, stated that, "in the investigation of the functional disorders of the brain also, there are fine conquests to be made by means of recent psychological methods," and that, "the neurologist may garner a vast number of extremely valuable data by the application of some of the principles and apparatus of the new physiological and experimental psychology to the investigation of his cases of organic brain disease."22 Adolf Meyer, an immigrant neurologist/psychiatrist from Switzerland who became the dean of American psychiatry in the first half of the twentieth century, acknowledged that the methods of experimental psychology were important in the understanding of mental diseases. Indeed, in the years 1895-1902, while he was pathologist and, later, Director of Clinical and Laboratory Work at Worcester State Hospital in Massachusetts, Meyer lectured in psychiatry and provided demonstration clinics for psychology graduate students at nearby Clark University. He and G. Stanley Hall, Clark's president, actively cooperated to build a working relationship between the hospital and the psychology students at the university.23
The attitude and public expressions of these leaders in American mental medicine indicated a willingness and a desire to involve experimental psychologists in the investigation of abnormal mental states. Many psychologists had an interest in mental pathology, as evidenced by the pages of psychological journals in this period. For some psychologists this was an opportunity to turn their interest into practice.

**Psychologists Respond to the Invitation of Psychiatrists**

**Boris Sidis and psychology as handmaiden to medicine**

"As I glance over the announcement of the many subjects to be brought before your attention and look at the long file of the names of lecturers, all with medical titles, I cannot help feeling grateful to you for the honor you have bestowed on me, a *mere psychologist*, by your kind invitation to read a paper on any subject in my line of work." These were the opening words of an address given by Boris Sidis to the American Medico-Psychological Association on May 25, 1899, in the city of New York.\(^{24}\) Sidis, as a student of William James, was very interested in psychical phenomena like dissociation, hypnotic states, and what he termed the "manifestations of the subconscious."\(^{25}\) It was his interest in such phenomena that comprised the bulk of his address. But central to Sidis' address were issues of significance to psychology and psychiatry. He began by pointing out that the demands of treating insanity had directed medical men to
psychology, so that medical men were "no longer afraid of anything mental." Psychology, he claimed, had left its philosophical cloud and "descended into the laboratories and is now demonstrating its truths by means of instruments and experiments." It was here, Sidis explained, that the nexus of medicine and psychology began: in the search for a scientific understanding of mental phenomena. He placed psychology in the position of the servant of medicine: A servant that would provide the experimental data that could form the basis for explicating complex mental states. This was a first, though indirect, attempt by a psychologist to suggest that psychology, the New Psychology, with its laboratory apparatus and experimental methods, was critical to the formation of a scientific basis for medical investigation of mental disorders. Those medical men interested in functional neuroses are, Sidis wrote, brought to

"the portals of the temple of psychology. Especially is this the case with the specialist in mental diseases. Daily is he confronted by phenomena, the source and mechanism of which can only be understood in the light of psychological analysis." Sidis was an important early member of a small group of psychologists that sought to extend the boundaries of psychology into areas once reserved for medicine. He sought such an extension, however, not on the basis of equality, but on the basis of psychology as a handmaiden to medicine.
Shepherd Ivory Franz and the cultivation of the borderland

Ultimately more influential in shaping the relationship of psychologists to the medical profession was Shepherd Ivory Franz. Franz earned both his undergraduate (A. B., 1894) and graduate degrees (PhD, 1899) at Columbia University in New York City. After graduation he was an assistant in physiology at Harvard Medical School for two years, then an instructor of physiology at Dartmouth Medical School. In the fall of 1903, Franz met Edward Cowles, the superintendent of the McLean Hospital, who was giving a series of lectures on psychiatry at Dartmouth Medical School. Their conversations led to an offer from Cowles of a position at McLean conducting research on mood disorders. Franz seized the opportunity and was appointed Pathological Psychologist at the McLean Hospital in 1904. His appointment marks the beginning of the formal development of the borderland between psychology and psychiatry.

Although Franz had no working knowledge of the problems of insane patients, his training in experimental methods in psychology and physiology proved fruitful in the investigation of pathopsychological states. His research in these topics will be addressed below.

After he began his work at the McLean Hospital, Franz published the first of what he called his "propaganda" pieces on the relation of psychology and medicine, particularly psychiatry. His "Psychological Opportunity
in Psychiatry" was published in 1906 in the recently established *Journal of Philosophy, Psychology, and Scientific Methods.* Franz, in his article, reflected on psychiatrist’s interest in the New Psychology and on the slowness of psychologists to reciprocate that interest. Few psychologists, he claimed, were working on problems of mental pathology. The published research on these problems was mostly by psychiatrists. Franz began his paper by noting that much light could be thrown on abnormal mental states by first examining normal mental states, which were the province of most experimental psychologists of the time. Franz here was echoing the approach of Emil Kraepelin, the Swiss psychiatrist who had studied extensively with Wilhelm Wundt and used Wundt’s methods in both normal and psychiatric populations.

Franz, in his article, was also interested in expanding the province of psychology. He took special care to elaborate how the study of abnormal mental processes could further the understanding of normal mental processes by asserting "that the study of the mentally abnormal will be of as great value to normal psychology as the study of the effects of extirpation and hyperactivity of various parts of the body has been to physiology." Franz then outlined how such study could prove productive in the major topics of normal psychology: sensation, perception, attention, memory, and emotion. The results of the application of experimental
psychology to abnormal states would also benefit psychiatry by providing a basis for diagnoses and prognoses. Franz, in a rhetorical move, grouped experimental psychology with anatomy, chemistry, and physiology as the sciences basic to the furtherance of psychiatry.

In one article, then, Franz argued for the importance of psychological investigation of the abnormal for both psychiatry and experimental psychology. He attempted to persuade psychologists that the study of the abnormal was a potentially fruitful field and to encourage psychiatrists that experimental psychologists stood ready and able to render a crucial service to their work.

If "Psychological Opportunity in Psychiatry" can be viewed as a rhetorical piece designed to interest other experimental psychologists and to persuade psychiatrists of the value of the experimental psychology, then his next "propaganda" piece can be seen as a further refinement of his rhetoric. In what was apparently an address to a gathering of psychiatrists, Franz concretely identified ways in which experimental psychology was needed for the advance of psychiatry. "On the Development and Need of Modern Psychiatry" was a careful statement of the boundaries and shared spaces of psychology and psychiatry. By the time the article was published (1908), Franz had moved to Washington, D. C., and taken the concurrent positions of Psychologist at the Government Hospital for the Insane and
Professor of Physiology and Experimental Psychology at George Washington University. The article reflected his greater experience with patients and his experimental investigation of psychopathology. It also reflected his experience with medical professionals, who were carefully guarding their territory.

Franz was careful to insist in the article that investigation of mental pathology in all its forms must be placed on an experimental basis. He wrote:

Psychiatry needs at the present time more careful studies in every line which promises any hope of information about the conditions in insanity. It needs the co-operation of the pathologist with his microtome and microscope. It needs the functional pathologist who will investigate the abnormal ways in which the nervous system does its work. It needs the specially trained psychologist who has methods that will be used for the differentiation of apparently similar mental reactions of individuals under our care. It needs the chemist who will discover some change in the blood or in the general metabolism of this class. And it needs the bacteriologist who will not only look for special micro-organisms in the different mental diseases, but who will cooperate closely with the chemist on such problems as toxins or antibodies in the insane. The combination and cooperation of all these different modes of determining changed functional efficiency are needed in psychiatry if we are to have the insanities investigated on broad scientific medical lines. When we get this combination we may hope for great advances.36

Franz’s recognition of the need for multiple approaches was not new, his call for such a combination of workers echoed the ideas of Ira Van Gieson, noted above. Complex disorders like dementia praecox, circular insanity,37 and dissociation had long puzzled psychiatrists and
neurologists. Psychologists, Franz argued, were critical to the explication of these disorders. Specifically, Franz asserted that the greatest service that psychologists could render the psychiatrist was to unravel the parts that are common to all types of what we call the same mental disease and those that are individual. It will be his opportunity also to determine the symptoms that mean in general a bad outlook, and those that indicate a mild attack.  

Differential diagnosis and prognosis of the insane were problematic for psychiatrists. Franz, with his experience, averred that the experimental method of psychology could help differentiate the mental conditions of individuals committed to psychiatric care.  

Not surprisingly, some physicians took umbrage at experimental psychologists' involvement in matters that were considered the sole province of medicine. Franz alluded to this in a wonderful analogy comparing work in mental pathology to the cultivation of farmland,  

This country borders on fields some of which have been cultivated for years, some of which are almost as new as that of psychiatry, but which have had more workers and are therefore more widely tilled. On one side is the field of neurology, and many of the neurologists have wandered in and out of the domain of psychiatry doing a little and getting much for each day's work. The psychologists in an adjoining field have been kept out by a few rabid protectionists and opponents of immigration or have not cared to barter with or to concede that there could be equivalent exchange of commodities...A few of the neurologists, pathologists, and psychologists have abandoned their own homes and have thrown in their lots with the psychiatrists, but the number is small. These have been well repaid by the fruits of
their labors when they have understood the character of the country.39

Franz also recalled boundary disputes with physicians in his 1932 autobiographical essay, "Psychiatrists considered me to be a psychologist, lacking the general medical knowledge and experience for psychiatry. Some psychologists said I was not a psychologist. Obviously I tended to occupy a 'No Man's Land' with whatever advantages that position conferred."40 But Franz was also a force for cooperation between psychologists and psychiatrists. He was elected an honorary member of the psychiatrists' professional society, the American Medico-Psychological Association, in 1908, because, he said, he "was the protagonist of the rapprochement of psychology and psychiatry."41

Despite the differences and disputes, psychologists had entered fields belonging to medicine by tradition and had come to stay. Boris Sidis had made the initial approach, but had left psychology to become a physician. Shepherd Franz was the first to forge a career as a psychologist closely identified with psychological medicine. His entrance opened the way for others to work in psychiatric settings. Franz was the first person to have a graduate student intern in a medical setting and his research was a signpost for future psychological research on medical topics and in medical settings.
Robert Sessions Woodworth: Physiology as the link between psychology and medicine

The tenor of the psychologists’ statements about their relationship with medicine changed subtly after Franz established himself at the Government Hospital for the Insane. Franz’s 1908 article reflected this change but so did two other articles published around the same time. The first was by Robert Sessions Woodworth. Woodworth earned both his BS and MS at Harvard University. While at Harvard he had acquired an interest in abnormal psychology from William James. Woodworth had been a fellow doctoral student with Franz at Columbia and, like Franz, was well-trained in physiology, even spending a year in England studying with Charles Sherrington. Woodworth, unlike Franz, had chosen a conventional career as a professor at Columbia. Nevertheless, he was invited to address the annual meeting of the American Medico-Psychological Association in June of 1906.

Woodworth, in his address, attributed much of the progress that had been made in psychology to its alliance with and dependence on physiology. These, Woodworth asserted, brought psychology and medicine closer together than they had been before. He was concerned specifically with the interaction of psychiatrists and academic experimental psychologists. Like Sidis, he began by placing emphasis on the priority of medicine. Woodworth stressed the contributions of psychiatry to understanding the normal
mind through the abnormal. However, he argued, experimental psychology could also contribute to a greater understanding on the part of psychiatrists concerning abnormal behavior. It could do so, Woodworth pointed out, through its use of the experimental method. He subtly suggested that psychiatric reliance on clinical observation might lead to superficial classification schemes and misdiagnoses. His tack in making this suggestion was through pointing out the unreliability of common observation. Experimental psychologists avoided these pitfalls by relying on systematic, controlled observations. Woodworth made this point as he asserted,

Here certainly it seems that psychology is in a position to be of service to psychiatry. The methods of investigation which have been invented, tested, and sifted in psychological laboratories, and the precautions which have been found necessary to correct in order to get reliable results, should be placed at the disposal of the experimental investigator in the hospital for the insane.43

Woodworth then went on to cite instances where psychological experimentation had proven to be useful. Studies of movement, association of ideas, and memory were foremost in usefulness for the psychiatrist. These studies had thrown light on differences between the normal and the abnormal and had also contributed to the amelioration of some mental disorders. Woodworth, like Franz (and probably thinking of Franz's research), argued for the usefulness of experimental psychology in making differential diagnoses, but he went one step further than Franz by asserting that there was a role
for psychology in patient treatment. For example, psychologists could help with patients who were suffering memory problems, by offering training in tasks known to improve attentional processes. Although Woodworth did not offer many specifics as to the usefulness of psychologists in patient treatment, he did suggest that enough had been learned by experimental psychologists to offer a bright future for anyone who would take the opportunity. He optimistically concluded his address with these words,

"Even so we may reasonably expect that the cross-fertilization of psychology by psychiatry and of psychiatry by psychology will result in a vigorous offshoot, a credit to our two sciences, a bond which will replace their former isolation, and a source of great enlightenment to both."44

It should be noted that Woodworth began his address by pointing out that in matters of mental disorder, psychiatry had the preeminence. He ended it by suggesting that the two were on an equal footing. Woodworth may have been premature in his appraisal of the relations between psychology and psychiatry. But, as he noted in his address, "the day of applied psychology is beginning."45 The confidence that Woodworth and Franz displayed about the contributions, potential and actual, of experimental psychology to a greater understanding of mental disorder and the role of psychologists in intervention with patients reflected the growing confidence of psychological science. The borderland between psychology and psychiatry had been opened and
psychologists were beginning to take advantage of this new opportunity to apply their science.

Frederic Lyman Wells: The dialogue of equality

Further indications of the expansion of psychology along the boundary shared with psychiatry were expressed in two articles on the relationship of psychiatry and experimental psychology written by Frederic Lyman Wells. Fred Wells was one of the most important individuals in the development of what is currently termed clinical psychology. An urbane, witty, extremely articulate man, Wells was known for his keen grasp of the experimental method in psychology and his clinical abilities with patients. When Franz left the McLean Hospital to move to Washington, he recommended Wells as his replacement. That Franz's recommendation of Wells was accepted speaks highly both of the work that Franz had accomplished and of the growing acceptance of a place for experimental psychology in a medical setting.

Wells, a native of Boston, received his PhD from Columbia in 1906 at the young age of twenty-two. His first position was at the McLean Hospital, where he served, except for a very brief period in 1911 when he took a position at the New York State Psychiatric Institute, until 1921. In that year he became Head Psychologist at Boston Psychopathic Hospital. His early research was on language, which may have naturally led him to his later work on word association in the insane.
In articles published in 1908 and 1910, Wells addressed the issues of the relationship of psychiatry and experimental psychology. Both articles appeared in psychiatric journals. Soon after he began his work at the McLean, Wells reviewed the extant literature on the methods and techniques of experimental psychology in conditions of mental pathology. His approach was qualitatively different from the work already noted of Sidis, Franz, or Woodworth. While their pieces were more clearly propaganda in tone, attempting to sell the idea of the usefulness of experimental psychology to psychiatry, Wells assumed the utilitarian value of his profession to psychiatry. That is, he began "Technical Aspects of Experimental Psychopathology" by assuming as fact what the others were still hopeful about. It was not a case of whether experimental methods were needed in psychiatry, but how those methods could be more clearly refined for psychiatric purposes. Wells changed the terms of the debate by subtly placing the two professions on an equal basis from the start. Where the others had been carefully deferential to psychiatry and medicine in general, Wells boldly, but subtly, assumed equality. He concluded his article by asserting that the future progress of the two sciences depended on their cooperation. Clinicians without psychologists were liable to error, while psychologists without clinicians were unable to interpret their experimental results.
In Wells' 1910 article, "The Experimental Method in Psychopathology," he made a case for a new branch of psychological science, experimental psychopathology. Wells acknowledged the limitations of an academic experimental psychology in the applied work of psychopathology. Specifically, Wells argued that the traditional research agenda of normal psychology: attention, memory, sensation, motor control, was not the place to begin the investigation of mental pathology. Rather, "the proper starting point for experimental investigations in psychopathology is the psychotic symptom itself." Given the correct starting point, the methods of experimental psychology were appropriate for uncovering the hidden processes of disorders like manic-depression and schizophrenia. Wells’s new science, he argued, blended the best of both clinical interpretation and experimental method. The article reflected the experience that psychologists were gaining in psychiatric work.

It is doubtful that Wells was actually arguing for the recognition of experimental psychopathology as a distinct science. His article, given as an address before the Ward’s Island Psychiatric Society in New York, can be more accurately viewed as an attempt to highlight the importance of scientific method to a group whose focus was mainly clinical: the diagnosis and treatment of persons suffering from some form of mental disorder. Psychiatrists had been
initially open to the new experimental psychology but many had begun to discount the utility and applicability of methods whose claim to validity appeared to rely on data generated by the normal mind in laboratories far removed from actual life, and even further removed from the conditions of the hospital for the insane. That Wells was a practicing psychologist in such a setting, and able to cite relevant research conducted in accord with experimental methods, made his case stronger. On a deeper level, however, Wells was establishing the mutuality of experimental psychology and psychiatry. Psychopathology must be investigated by methods both clinical and experimental; it was the purview not only of medical men, but of men whose training originated in the laboratories of academia and was refined and made useful in the living laboratories of the mental hospital.

Underlying the overt statements in the literature surveyed to this point was an attitude about the nature of reality and how it could best be understood. Men whose work lay in abnormal mental states believed that the secrets of such states would yield to scientific methods. From psychiatrists such as Adolf Meyer to psychologists such as Frederic Wells, this was a shared world view. The relationship of psychologists and psychiatrists developed around this view. Psychologists, like Franz and Wells, were able to translate the rhetoric of psychology as science into
practical investigative results that placed them and their profession on a more egalitarian basis with psychiatry and other medical sciences. Although there were true differences of method and technique between psychiatrists and psychologists in their common workspace, the differences were similar to those between psychiatry and physiology, or other of the basic medical sciences. Science and its methods, whether anatomical, physiological, pathological, or psychological was the proper path.

**Boundary Expansion: Psychologists and Psychopathy**

The essential relationship between psychologists and psychiatrists had been established by 1910. Psychologists assessed the patients' sensory and motor abilities as an aid to diagnosis and treatment by the psychiatrist. Psychologists also conducted research of their own on the patient population and guided the research of a growing number of doctoral students and interns. This research will be surveyed below. Psychiatrists and psychologists had worked out a mutually beneficial arrangement by 1910. That relationship was to grow as both groups sought to extend the range of their practices.

Shepherd Franz noted in 1915 that "the employment of professional psychologists in psychiatric institutes in this country is not only far from general but rather the exception."50 In 1910, Wells and Franz were the two most prominent of the very few practicing psychologists in
psychiatric settings. But the situation changed over the course of the 1910s. In this section, I show that psychologists were able to further expand their work in the borderland they shared with psychiatry. This opportunity for extension was made possible by the efforts of psychiatrists to extend the range of their authority to cover not just the clearly insane and abnormal, but to also cover the near-normal.

In 1913, Robert Yerkes appealed to physicians to recognize the usefulness of comparative psychology in the understanding of human behavior. Yerkes, a Harvard PhD (1902), had a long and illustrious career in psychology and a longstanding interest in abnormal behavior. In 1913, he accepted a position of psychologist at the new Boston Psychopathic Hospital. Yerkes, like Franz, Woodworth, and Wells before him, touted the need for an objective, experimental psychology and made his case that his specialty, comparative psychology, was just that. His special point was that such study would prove useful in the explication of human problems, like nervousness and neuroses. But, in many ways, Yerkes was preaching to the converted. Psychiatrists and other medical personnel were convinced that psychology had something to offer in the study and treatment of mental pathology. That Yerkes was employed at a new type of psychiatric institution was proof enough of that.
Psychiatrists and psychopathy

The move of psychiatry from a nearly exclusive focus on treatment and care of the insane to one that encompassed a wider range of disorders and a rearrangement of professional priorities in research and practice has been well-documented. Psychiatrists sought to expand the boundaries of their knowledge about mental disorders and to ameliorate human suffering. But the motive of extending professional hegemony and, hence, gaining a greater degree of cultural authority has also been imputed to psychiatrists. Certainly, psychiatrists did seek to extend the boundaries of their practice by moving to include as psychiatric disorders those behaviors that, while not indicative of insanity, were different from or exaggerated versions of what was accepted as normal. A new term, psychopathy, was coined for this wider range of behaviors and attitudes that differed only from the normal quantitatively, not qualitatively.

One type of extension of psychiatric practice that illustrated both these motives was the psychopathic hospital. The psychopathic hospital was envisioned as a way of providing medical assistance to those who were not insane, but suffering an acute attack of insanity, or who were experiencing temporary confusional or delirious states, or who needed observation and assessment for determination of whether to commit them to a hospital for the insane. The
biographer of E. E. Southard, the first director of the Boston Psychopathic Hospital, opened in 1912, described the purpose of that institution as "a clearing house for borderline mental and nervous cases, it aimed to provide an emergency refuge, often voluntary, for those whose maladjustment to the ordinary stress of life might fortunately prove temporary." Albert M. Barrett, a former associate of Adolf Meyer at the Worcester State Hospital in Worcester, Massachusetts, was the first director of the earliest such institution, the Psychopathic Hospital at the University of Michigan, opened in 1906. By 1920, at least four psychopathic hospitals were in operation. Psychologists were staff members in at least three of these institutions, Boston, Michigan, and Baltimore. In each of these three institutions, research into mental disorders was part of their mission. Louville Emerson, another student of William James, and one of the first psychologists to practice psychotherapy on a full-time basis, was on the staff of the Psychopathic Hospital at the University of Michigan until 1911, when he became a staff member at Massachusetts General Hospital under the direction of James Jackson Putnam. Robert Yerkes was the Chief Psychologist at the Boston Psychopathic Hospital from 1913 to 1917. At the Phipps Psychiatric Clinic, psychologists John B. Watson and Knight Dunlap collaborated with Adolf Meyer, the Clinic director, in the training of medical students.
Psychologists were important members of these medical institutions, providing assessment of patients, instruction, and in at least the case of Emerson, psychotherapy for patients.

Robert Yerkes and the Boston Psychopathic Hospital

Robert Yerkes described his activities at the Boston Psychopathic Hospital in the following terms:

As from various educative and advisory services at the P. H., I attempted: (1) to develop and improve methods of mental examination and measurement suitable for psychiatric purposes: and (2) the study of certain aspects of deficiency and derangement of ideational behavior. Under methodology the most notable achievements of my group were, I think, the development and standardization of the Point Scale method of measuring and rating intellectual abilities, and the devising and application to human examining of the 'multiple choice' method.\(^58\)

Yerkes had been a long time friend of Ernest Southard, the director of the Boston Psychopathic Hospital. They moved in the same intellectual and social circles. Yerkes and Southard were both members of the "Wicht Club," a group of young instructors at Harvard that also included psychologists Edwin B. Holt and Roswell P. Angier, the physiologist Walter B. Cannon, the chemist Gilbert Lewis, and the physicists George W. Pierce and Harry W. Morse.

Yerkes brought his "objective method" developed in his comparative studies to his work at the hospital. He believed that "psychology is the science upon which psychiatry, as the art of dealing with mental ills, chiefly depends."\(^59\) In fact, he argued that psychiatry is not a
science but a medical art whose focus is that of treatment.

Yerkes described the division of labor at the Psychopathic Hospital by using as an example a delinquent boy:

An adolescent boy whose persistent disregard for property rights had brought him before the Juvenile Court might be referred to the hospital by the judge for examination and report. As soon as practicable after arrival he would be examined by a psychiatrist to discover indications of mental defect, disorder, or other mental abnormalities. He would then, if necessary, be referred for psychological examination to determine his intelligence and emotional level, personality type, and to discover peculiarities of behavior suggestive of a psychopathic personality. Meanwhile the "psychiatric social worker" would inquire into his social adjustments, success at school or work, the nature of his heredity, family, home, and community environment, companionships, and habits.60

In Yerkes' view, it was the psychologist who was best able to determine psychopathy. While many psychiatrists styled themselves as psychopathologists and insisted that to be such one had to have had medical training, here Yerkes placed the determination of psychopathy at least partially under the umbrella of legitimate psychological services.

With the conception of psychopathy, however vague in practice it was, psychiatrists extended the range of what constituted their professional domain. They did not do so, however, without the aid of psychologists. Psychologists and their services had become important to psychiatry. Because psychiatrists were not trained in mental testing or the assessment of normal sensory and motor functioning, they
were dependent on the skills of the psychologists who were so trained.\textsuperscript{61} As Yerkes argued,

"the more familiar one is with the normal, whether it be defined as the usual or the suitable, in human structure, behavior, experience, and societal relations, the better qualified he should be to understand and deal serviceably with the abnormal or the pathological."\textsuperscript{62}

As pointed out above, Shepherd Franz had argued the reverse, that normal functioning might best be understood by observing and measuring the abnormal. When the differentiation to be made was between the normal and insane, Franz's argument was an appealing one to both psychiatrists and psychologists. Now that the domain of professional interest had shifted to deviations from normality, but not insanity, that is, psychopathy, it was paramount that the normal be known. Where psychiatrists had once claimed that laboratory psychology, with its focus on the normal human mind, was limited in its usefulness to psychiatry, now psychologists trained in experimental methodology were needed to aid in the extension of psychiatric hegemony.

The demand for psychologists as assessors of mental states was evident in another way. Opportunities for expansion of psychology in the borderland with psychiatry increased as psychology graduate students, or recent graduates, began to serve as interns or assistants in medical settings such as hospitals for the insane and psychopathic hospitals. Shepherd Ivory Franz, when he
accepted the positions at George Washington University and the Government Hospital for the Insane, began the practice of placing his graduate students at the hospital for training purposes. While the beginning of psychological internships was extremely modest and did not become widespread until after World War II, nevertheless, in 1916 there were five psychologists serving as interns or assistants under Yerkes at Boston Psychopathic Hospital alone. The nature of their research activities while serving in these positions will be considered below. It is apparent that psychologists had become important to psychiatry in ways that were not present or evident in the first decade of the twentieth century. The reverse is also true, psychologists needed the relationship with psychiatry to expand their share of the burgeoning psychological discourse and professional practice in psychology.

From 1910 to 1920, the complexity of the relationship of psychologists and psychiatrists grew. Psychiatrists and psychologists needed each other, to advance their individual claims and to extend the boundaries of their science and practice. For example, mental tests were important for the professional expansion of psychology and the extension of psychiatric hegemony. Mental testing became a focus of professional contention between psychologists and psychiatrists. But the relationship persisted despite the contention, even though psychiatrists lost the battle over
who should be able to determine intelligence. But a great deal is missed if the only focus of scholarship is the controversy and contention between these two disciplines. Whether the focus of observation was the normal or abnormal mind, psychiatrists and a growing number of psychologists had important contributions to make. Intellectual and professional interests kept them allied with each other in the borderland of science they had both developed.

**Psychology and Medical Education**

When men like Edward Cowles, Adolf Meyer, Frederick Peterson, and Ira Van Geison touted the benefits of the new experimental psychology, they created a climate of inclusion for psychology into medicine. One avenue of inclusion was in medical education. With the reform of the medical profession and the emergence of an emphasis on scientific training as the basis of the profession, a debate was opened on questions of content of the medical curriculum. The question of the inclusion of experimental psychology became a part of the debate.

One voice in the debate came from those who were sympathetic to psychology. Another consideration came from experienced physicians who understood that psychological states and processes in the patient and in the care and treatment of the patient were important. Yet another influence was the reaction on the part of some physicians to what they perceived as an overemphasis on somatic
explanations for all human disease and dysfunction, including mental diseases. In the last years of the nineteenth century and the first years of the twentieth, a number of physicians and psychologists, some of them trained in the new medicine, argued for the consideration of "psychic" factors in disease, especially mental disease. These voices were important in the debate over the inclusion of psychology in the medical school curriculum.

In this section, I show that there were several similarities or links between psychology and the new scientific medicine. I briefly discuss the agenda of medical school curricular reform and then turn to a consideration of the various position papers on the question of inclusion of psychology. Last, I examine the symposium of psychology and the medical curriculum held in 1911 and its subsequent fate. I argue that the effort to include psychology illustrated the growth and increased salience of the new discipline.

Similarities and Links between Psychology and Medicine

The links between medicine and psychology in the opening years of this century were greater in number than might at first seem apparent. These links were important in the attempt to include psychology in the medical school curriculum. They provided points of contact that facilitated the dialogue between the representatives of psychology and medicine.
One link was through physiology. Kurt Danziger has pointed out that the new psychology, developed in Germany and Americanized by men who went to Germany to train in the new discipline, owed its conceptual and methodological life to experimental physiology. The psychologist, Robert Sessions Woodworth noted in an address to psychiatrists in 1906 that, "Psychology has had a new birth; and medicine, if not its new mother may at least be called its grandmother, since it is to the physiologists that the new line of development is principally due." Graduate education in psychology, as in medical school, usually included course work in physiology and many psychologists pursued further work in physiology.

A second similarity between medicine and psychology was the shared emphasis on laboratory work as requisite for training and identity as a science. Just as the best medical schools came to require extensive laboratory work as part of the medical school curriculum, so did the best graduate schools in psychology require laboratory experimentation of their graduate students. Hilgard has argued that "the importance of the laboratory in attaining acceptance [of psychology] as a science by other scientists cannot be overestimated."

Conceptually and practically related to the emphasis on the laboratory, a third link was the shared reliance on instrumentation as the preferred way of divining the unseen
but crucial determinants of disease, in the case of medicine, or mental functioning and capacities, in the case of psychology. In medicine, advances in instrumentation allowed for more precise measurement of such bodily processes and states as temperature, blood pressure, and heart rate, thus moving physicians more toward a reliance on technology for diagnosis. As Paul Starr pointed out, this led to a "growing recognition of the inadequacy of the unaided and uneducated senses in understanding the world." Psychologists, too, relied upon new instruments to ascertain human (and animal) psychological functioning. So much was this the case that William James deplored the rise of "brass instrument psychology." The popular writings of psychologists in this period clearly illustrate the reliance of the new science on a wide variety of laboratory instruments. For example, G. Stanley Hall in a popular piece on the New Psychology published in Harper's Monthly in 1901, liberally sprinkled photographs of various psychological apparatus throughout the article, as he described in his inimitable grandiose style the scientific wonders of the New Psychology. While it may be argued that popularizations were not a part of psychologists' science, it must be remembered that psychologists very much needed to sell their product as science and themselves as scientists to a public ready to accord important privileges to scientists. Popularization and its rhetoric, I would
argue, was one vital part of the (successful) attempt by psychologists to gain public recognition of their legitimacy as scientists and to gain acceptance of their product: psychological knowledge. Laboratories and laboratory instrumentation as accoutrements of science, while important in the development of psychological science, were also effective as discursive aids in an era when scientism was a dominant part of the social and professional discourse.

The use of such rhetoric forms a fourth link between medicine and psychology. In the reform of medical education, the rhetoric of science was used to advance strategies of reform. From 1893, when the Johns Hopkins University School of Medicine opened, until the latter part of the 1910s, one of the strongest arguments for medical reform was the need to place it on a thoroughly scientific basis. The medical profession was overpopulated in numbers, both in practice and in medical schools. Too many physicians were too poorly trained to take advantage of the advances in medical therapeutics and were unable to benefit from the advances then resulting from medical research. The prestige, profits, and future of medicine were compromised by these undereducated and poorly trained practitioners. This situation was due, the reformers argued, to the low standards of medical schools, particularly proprietary institutions who were dependent on student fees for survival. What was needed, on the one hand, was a reduction
in the number of institutions and students, and, on the other hand, an increase in the quality of instruction. Higher quality of instruction meant more preclinical teaching in the sciences considered basic to medicine, with attendant laboratory work, so that the medical student was a medical scientist upon completion of medical school.

Here is a case where advances in knowledge and rapid changes in technology combined with a political/economic agenda of reformers to bring about significant change in a profession. Scientific, social, and political/economic factors were inextricably linked. My point is that medical reformers used these factors to achieve their agenda. Their rhetoric of "scientific medicine" was successful in persuading the consuming public, political leaders (who controlled licensing), the administrators of universities, and fellow professionals that their agenda of reform offered the best of the possible alternatives. In psychology and in medicine, the scientific method, with its emphasis on experimentation and its locus the laboratory, formed the basis for the rhetorical claims to legitimacy and, in medicine, it undergirded the calls for a medical education that would produce medical scientists.

Transformation of the Medical Curriculum

Medical education at the beginning of the twentieth century was increasingly centered around the several sciences considered basic to medicine: anatomy,
bacteriology, biochemistry, histology, pharmacology, physiology, and pathology. According to Paul Starr, this was the result of a gradual and halting reform of the medical school curriculum that began around 1840. The reorganization of Harvard Medical School (1871) and the establishment of the Johns Hopkins University Medical School (1893) along lines emphasizing both clinical instruction and medical research were landmarks of the reform and signposts to the future of medical education. Standards of admission were eventually raised by all the leading medical schools and the length of training was expanded from two years to three years and then to four years.

One result of the raising of standards was the extinction of most of the proprietary medical schools, which led to a reduction in the number of new physicians entering the marketplace, and thus, a reduction in competition. Another result, of course, was better trained physicians. The ideal of medical education became two years of preclinical instruction in the sciences considered basic to medicine, with laboratory work in the second year, followed by two years of clinical instruction.

The emphasis on instruction in basic science, the extension of the school year, and the increase in the number of years required to earn the medical degree, all contributed to the placement of medical education on a par with graduate education. These were important ingredients
in the closer association of medical schools with universities. As a result, members of the new academic disciplines, such as psychology, that saw themselves as scientific were often brought into close contact with the new scientific medicine.

In this period of reform, the question of what to include in the medical curriculum was open for debate. Even with the extension of medical education from two years to four years, there was limited space for inclusion of new subjects and what new subjects to add was hotly debated. What was not debatable was that whatever the content of the curriculum it must be scientific and/or of clinical utility. Into this flux, came the calls for inclusion of psychology in the medical curriculum.

Physicians' Calls for the Inclusion of Psychology in the Medical Curriculum

In 1898, Charles K. Mills, a prominent Philadelphia neurologist, published a plea for a laboratory of neurology and psychology in the medical school of the University of Pennsylvania. The immediate context was the university's emphasis on the establishment of laboratories for research in the various sciences. Mills wanted the medical school to have its own teaching laboratory, with a dual emphasis on neurology and experimental psychology.

Lightner Witmer, a psychologist, had established a psychological clinic at the University of Pennsylvania in 1896. But Witmer’s clinic focused on the treatment of
children, with special reference to school problems. What Mills had in mind was different, he wanted a laboratory in the medical school where experimental investigations, both neurological and psychological, of nervous diseases could be conducted. He also proposed that the laboratory be used as a training site for medical students who had chosen neurology as their specialty. Mills envisioned two benefits of the laboratory: scientific research that would advance neurological and psychological knowledge and neurologists trained to understand the psychological processes of their patients.81

The following year, 1899, the psychologist Boris Sidis wrote, "It is to be hoped that the time is not far off when every medical college of high standing will institute courses in psychology and psychopathology." 82 Two years later at least one medical school did begin offering a course in psychology as part of its curriculum. Tufts Medical School began offering a course in "normal medical psychology" in the 1901-02 academic year.

George Dearborn, with an MD (1893) and a PhD (1899, Psychology) from Columbia University, was the instructor. He taught the course for seven years and then merged it with a course in psychopathology given by the physician, Morton Prince. Dearborn had been a fellow graduate student at Columbia with Shepherd Franz and received his doctorate in
the same year as Franz. His later work involved both eugenics and neuropsychiatry.

Dearborn was an early promoter of psychology in the medical school curriculum. In his first article on the subject published in Science in 1901, Dearborn urged the adoption of psychology as a counter to the over-emphasis on materialism then rampant, in Dearborn's view, in medical schools. The medical student, Dearborn charged, is taught only what he can "feel with his hands or see through the microscope." Dearborn, though he used the term "scientific psychology," did not make a case for the new, experimental psychology in which he must have been trained at Columbia. His real concern seemed to be that of humanizing medical education through a recognition of the importance of man as a psychological being. Physicians, he said, must be practical psychologists and acquainted with the normal human mind. Psychiatrists and neurologists were the specialists who needed this training more than other medical students. Dearborn asserted that to "graduate a psychiatrist without this knowledge is like pretending to qualify a general practitioner without teaching him physiology." Dearborn suggested a weekly lecture, without laboratory, during the first part of the fourth year of medical school. Topics that should be covered in this conceptualization included hysteria, neurasthenia, dementia praecox, paranoia, and sex, as well as more mundane topics
such as habit formation, emotions, and sensation. Dearborn's list reads like a mixture of normal psychological subjects then being investigated in academic laboratories and the topics of abnormal psychology then perplexing psychiatry and neurology. Presumably, Dearborn taught this type of psychology course for the next seven years.

Dearborn, at the end of that seven years, continued to maintain that medical students needed instruction in psychology. In 1909, he acknowledged the "failure of academic psychology to completely 'make good' in the laboratories" and asserted that psychologists needed to demonstrate the practical relevance of their discipline to medical problems. What the medical curriculum really needed from psychology, in Dearborn's view, was two things: 1) an explication of normal mental processes so that a physician could recognize a deviation from the normal and 2) an explanation of the psychological factors in the relationship of doctor and patient, especially as this affects disease and recovery from disease. A psychology able to make these contributions should be welcomed into the reformed medical curriculum, Dearborn concluded.

Dearborn was not the only physician to plead for medical training in psychology. T. Wesley Mills, a Canadian physician who was also a professor of physiology at Montreal's McGill University, made a plea in 1908 at the annual meeting of the American Medico-Psychological
Association for the inclusion of a practical psychology in the medical curriculum. Mills argued that especially in psychiatry, physicians should be trained in the workings of a normal mind before they could understand the workings of an abnormal mind. He used as an analogy, clinical medicine. Here, Mills said, the medical student receives instruction in normal physiological processes and then is taught how to recognize any deviation from that norm. Medicine, Mills asserted "is applied physiology, etc., to which he [the physician] must now add applied psychology." The road to be followed for this to occur was the addition of psychology to the medical curriculum. Mills acknowledged the importance of adding psychologists to the staffs of asylums, but argued that greater benefit would come when medical students were also trained in psychology, which he termed a medical science. With a flourish of rhetoric, Mills finished by declaiming, "The time has come when the physician should be a psychologist...and when psychology should become part of the curriculum of every medical student." The comments that followed his presentation suggest that many in attendance also felt a need for some instruction in normal psychology. What physicians who were concerned about this issue wanted, however, was not a dry, academic psychology, but one that was suited to their needs. What psychologists wanted was not always the same thing, as we shall see.
Psychologists' Efforts to Place Psychology in the Medical School Curriculum

American psychologists were active advocates of the inclusion of their new psychology into the medical curriculum. Joseph Jastrow, the chairman of the department of psychology at the University of Wisconsin and another of the popularizers of psychology, stressed that physicians and their patients would profit by better understanding the psychological factors present in the relationship of the physician and patient. He emphasized that psychological factors were at play in all human conditions and that the better the physician understand these factors the more effective and efficient his treatment of the patient would be. Particularly important, Jastrow urged was that the physician remember that he was treating a person and not just a disease. 87 James Mark Baldwin, an early proponent of genetic (developmental) psychology and the chairman of the revived psychology department at Johns Hopkins, argued in 1906 that it was the "duty" of the medical profession to incorporate the relevant findings of recent psychology into medical training. Especially important, Baldwin asserted, were the emphasis on the role of mental functions in suggestion and the formation of habits and the developmental sequences of human life. 88 Baldwin was a proponent of the functionalist approach in psychology. For functionalists, mind was for adaptation to the environment, external or
internal, and the physician could benefit from understanding normal mental processes. That is, physicians could benefit from understanding how the normal mind adapted to disease, what the role of the mind was in disease.

Robert Woodworth and Shepherd Franz were two other psychologists with strong functionalist leanings who, as noted above, were involved in the development of links with medicine. Woodworth commented on the role of psychology in the training of psychiatrists by noting that if psychiatrists wanted to conduct experimental investigations of psychological phenomena, they would be best equipped to do so by being trained in a psychological laboratory. His remarks may be taken as an indication of one of the roles that psychologists saw themselves as playing in medical education, particularly psychiatry.

One problem that was soon apparent to interested parties on both sides was the distance, both literal and figurative, between them. Despite the fact that medical schools were increasingly positioned as part of a larger university, many were still physically situated at some distance from the university campus. The training site for experimental work in psychology was usually located in the typical academic setting, while the material for investigation and the training site for physicians were located near or in hospitals or insane asylums. If it was important for physicians to be trained or at least made
acquainted with psychological knowledge during their years in medical school, then some way would have to be found to bridge the distance. It was not likely that medical students were going to be sent to psychology departments for such training, so it became more and more evident, at least to psychologists, that psychology would need to be placed in the medical curriculum.

For psychologists and for physicians what was needed was a way to bridge the gap between psychological research and medical practice. If psychology was to find a place in the medical curriculum this need would have to be met.

Shepherd Franz was just the kind of person needed to help bridge the gap between medicine and psychology. He had developed a solid reputation for his research on cerebral localization and he conducted research on topics of interest to psychiatry. Over the course of his career he had developed clinical skills that demonstrated his grasp of how to conduct mutually beneficial relationships with physicians. Franz addressed the gap between clinical training and psychological research after a few years experience with psychiatrists in mental hospitals. Franz deplored the fact that many medical students finished their training without ever having seen a case of insanity and some finished without any instruction at all in nervous and mental diseases. But Franz argued that medical students also needed training in psychology for reasons other than an
understanding of psychiatric disorders. Like Dearborn, Jastrow, and Woodworth, Franz advocated the importance of physicians understanding the basic psychological functions in general medical practice or in their chosen specialties. The fault, Franz said, lies in the emphasis on structure over function in the student's medical education. One way to remedy that was through instruction in psychological functions to medical students.  

The APA Symposium on "The Relations of Psychology and Medical Education"

In order to promote the integration of psychology into the medical curriculum, Franz organized a symposium on "The Relations of Psychology and Medical Education," held in December, 1911 at a joint meeting of the American Psychological Association (APA) and the Southern Society for Philosophy and Psychology, at the Government Hospital for the Insane in Washington, DC. According to the APA's Secretary-Treasurer, W. V. Bingham, the symposium was the best attended event of the entire meeting. The participants in the symposium were the psychologists Shepherd Franz and John Watson, and the physicians Adolf Meyer, E. E. Southard, and Morton Prince.

Franz opened the symposium by asserting the independence of psychology as a science. Given the cultural as well as professional/academic accentuation on "Science" at this time, it is apparent that Franz wanted the acknowledgement of his medical colleagues that what was
occurring in this meeting was a discussion between two sciences, a discussion between equals. Franz, as he had done in earlier articles, deplored the continuing emphasis in medical schools on structural anatomical studies: anatomy, histology, pathology. He noted, however, that there had been some changes over the last few years as evidenced by the renewed interest in mental states in disease and attributed this to the competition that medical practitioners were receiving from Christian Scientists, Emmanuelists, and their sort, who stressed the power of the mind in their cures.  

Franz also acknowledged that unusual disorders like neurasthenia and hysteria had contributed to this renewed interest in mental processes. Although Franz did not articulate it in his address, interest in such conditions was due in no small part to the dissemination of the ideas of Sigmund Freud and psychoanalysis.

What was needed, Franz argued, was instruction in normal psychology. Franz granted that, here and there, a few medical schools offered courses in psychology and that psychological topics were discussed in other courses (anatomy and physiology of the nervous system), but charged that such instruction did not provide an adequate foundation for the applied sciences of the medical school curriculum. Psychology was an independent science and should not be a mere addendum to physiology, psychiatry, or neurology.
The inclusion of scientific psychology in the medical school curriculum was justified in three ways, Franz asserted. The first was that physicians must deal with patients who have mental processes and not just physical processes. An understanding of psychology would aid the physician in the diagnosis and treatment of patients. Secondly, psychology had proven to be an aid in nervous and mental diseases by assisting the specialist in differential diagnosis and by providing an understanding of the psychological factors present in psychotherapy. Lastly, psychology should be a part of the medical curriculum because of its research relevance to physiology and pharmacology. Franz concluded his address by conceding that what medicine wanted from psychology was a psychology that was practical, not the one that most physicians perceived, probably accurately, to be the product of sterile, academic laboratories. But Franz argued, the onus was on the physicians to utilize psychological research. If they would do so, they would find that psychological researchers would respond with practical applications of their science to medicine. There was common ground for both to stand on, Franz asserted, and psychologists and physicians must be prepared to work together in order for the medical student to receive the best training possible.  

All of the symposium papers agreed that psychology merited inclusion in the training of physicians, although
each one expressed different views about the place of psychology in the medical curriculum. Adolf Meyer, arguably the most prominent psychiatrist of his day, argued for a functionalist, practical psychology. Meyer acknowledged that "the importance of psychology is guaranteed," but asserted that what medical students needed was not textbook psychology. A psychology of the whole person, in all their biologic reactions, was demanded by Meyer. Rather than add a course in psychology to the already crowded curriculum, Meyer wanted to add psychological material to courses in physiology and pathology. And he wanted the instruction to be handled by a physician, not a psychologist.

Ernest Southard, the Boston psychiatrist and first director of the Boston Psychopathic Hospital, echoed Meyer's call for a practical, functional psychology. He linked psychology and physiology and urged that the joint study of these two subjects would help bridge the gap between research psychology and research medicine.

John Watson made specific suggestions for a course in psychology for medical students. Watson made his address at a time when he was developing his own agenda for the future direction of psychology. The course that should be offered, in Watson's view, would be oriented to the practical concerns of the physician, yet would also incorporate the laboratory emphasis of the psychologist.
The ideal, Watson argued, would be for a course in psychology as part of the third year of medical school. The course would meet three times weekly and would be comprised of two psychological laboratory hours where the future physician would learn how to assess such psychological phenomena as memory, emotion, association, and habit formation, and one hour of didactics. Watson echoed Meyer's call for the necessity of training physicians to treat the whole person. Psychology would aid in this just as any other biological science would help. In Watson's scheme, all medical students would participate in his ideal course. Those interested in a career in psychiatry would then learn the material peculiar to their specialty in clinical settings with a sure "foundation in practical psychology." Morton Prince, the Boston psychopathologist and early exponent of psychotherapy, began his portion of the symposium by asserting that there was no relation between normal psychology and psychopathology. For Prince, an understanding of psychopathology was not to be gained from an understanding of normal psychological processes. Rather, normal psychological processes were more likely to be elucidated by the study of abnormal mental processes. Like Meyer and other physicians, Prince criticized normal psychology for failing to have practical application. He suggested a "New Psychology" for medical students; a psychology that would have as its focus unconscious factors
and therapeutic intervention. Such a course, Prince asserted, would be supplemental to the type of course suggested by Watson.\textsuperscript{100}

\textbf{Aftermath of the Symposium}

The APA symposium reflected the interest and importance that academic psychologists attached to the expansion of their discipline. The potential inclusion of psychology in the medical school curriculum presented a fine opportunity for expansion and it would raise the visibility and stature of American psychology. Leading psychiatrists were also interested in placing more emphasis on psychological topics in medical training. After the symposium, the dialogue about inclusion was continued in the professional literature. The APA also constituted a special committee on psychology and medical education.

\textbf{The dialogue on medical education and psychology}

Meyer continued to press his case for the teaching of psychology by physicians in his presidential address before the American Psychopathological Association in May of 1912.\textsuperscript{101} Meyer argued that only physicians were qualified to teach a psychology that would meet the needs of medical students. If psychologists wanted to be involved, then they would need to pursue medical training to equip them for service to physicians. Meyer flip-flopped the argument of the psychologists. They had argued that physicians needed psychological training to better prepare them to care for
their patients. Meyer wanted psychologists to undergo medical training if they wanted to teach physicians. In a statement that indicates his view on who was best prepared to guide psychological discourse, Meyer wrote,

"Only the broadly trained physician can remain free of the charge of opening the door to the a-scientific if not anti-scientific. On the whole we do well, I think, to advise our psychological friends to choose, at least as an avocation, a medical training..."\(^{102}\)

The symposium and Meyer’s later statements are examples of the ongoing debate and development of the medical-psychological relationship. The content and emphasis of any curriculum, including the medical, has power to shape the interests and practice of those who participate in it. The debate over the inclusion of psychology in the medical curriculum reflected the same issues of the control and direction of the broader psychological discourse that were discussed above. It was a crucial part of the relationship of psychologists and physicians. The psychologists involved, Franz and Watson, argued for a normal psychology, taught by psychologists, oriented around the disciplines’ traditional subjects: habit-formation, association, emotion, fatigue, memory, and the like. The physicians, Meyer, Southard, and Prince, all wanted a practical psychology oriented to the needs of physicians and, especially, the needs of those who wanted to specialize in psychiatry. The physicians wanted, as well, medical control over the content of psychological instruction.
In 1913, E. Stanley Abbot presented the results of a survey he had made on the inclusion of psychology in the medical school curriculum. Abbot, a Harvard MD and long-time assistant at McLean Hospital, had a long history of interaction with psychologists. He had been at McLean when Shepherd Franz was hired and was there when Fred Wells replaced Franz. He and Wells had collaborated on research and he was a frequent author of articles published in psychological journals. Abbot surveyed 85 medical schools, excluding Class C schools). Of the 58 replies that he received, only three medical schools required a course in psychology for admission, while eight advised some type of course. In the medical curriculum, only four schools required a course in normal psychology, while thirty ignored it altogether. Two schools required a course in abnormal psychology and two offered such a course as an elective. Eleven schools offered instruction in psychology in other courses, while seven planned to do so. Abbot found that the total number of schools requiring, advising, or teaching psychology was 26 out of the 58 that replied. Of course, as Abbot noted, these numbers do not indicate the type, the quality, or the quantity of psychological instruction. They do indicate that psychology as a fit subject for inclusion in medical instruction was not ignored in the competition for curricular space. What appeared to discomfit Abbot, however, was the lack of acknowledgement of psychology as
basic to medicine, in general, and psychiatry, in particular. As he put it, "in no curriculum that I have seen has psychology been mentioned as one of the fundamental sciences."\textsuperscript{103}

Abbot, like the participants in the symposium organized by Shepherd Franz, called for a practical, applied psychology, suited to the medical student's needs. The course he envisioned would have included instruction in all the usual topics of normal psychology, with a passing acquaintance in psychological experimentation for the medical student. Abbot, again like Meyer and Southard, indicated that preference as to instructor should be given to a physician, although he did not rule out a psychologist as instructor, if he was knowledgeable in medicine.

Fred Wells, Abbot's colleague at McLean Hospital and a pioneer in the borderland between psychology and psychiatry, wrote a piece for \textit{Popular Science Monthly}. His article was a response to the issues raised in the APA symposium about the type of psychology needed in medical schools. Wells stated at the outset that the onus was on psychology to provide medical students with "something they can use."\textsuperscript{104} Medicine, he pointed out, deals with problems of human suffering, while psychology is concerned with scientific investigation of psychological phenomena not necessarily related to medical problems. It is no good, Wells argued, for psychologists to expect medical schools to include a
psychology that was not oriented to the practical needs of medicine. He went on to suggest what topics in normal psychology might be of use in medical training. Psychological work in memory, the psychogalvanic reflex, the free association, and the work-curve could all prove their usefulness to the medical student, in Wells’s view. But Wells was pragmatic, rather than dogmatic. If these topics did not suit, then whatever psychological instruction as did prove useful should be included.

Perhaps Wells’s most important suggestion, and one that will be discussed further below, was that medicine needed a psychology that was concerned with the whole person. That is, psychology would offer most to medical education if psychologists would focus on the human personality. "The key-word to what medical psychology should be, and what academic psychology has not been, is, in fact, personality." Wells anticipated here a new direction of psychological investigation; one in which psychologists moved in significant numbers beginning in the 1920s. Wells was making the point that a focus on personality was not the sole province of any current group. The psychoanalysts, the psychiatrists, and the university based researchers could all contribute to personality research with potential fruitfulness for medical education and understanding.

To teach psychological medicine, Wells claimed, one should
be conversant with and able to judge of the methods of experimental psychology in reference to their application to the analysis and interpretation of symptoms; and, on the other hand, able to recognize and elucidate the more general questions now stated dynamically.\textsuperscript{106}

Wells, then, saw psychologists who had experience in medical issues as qualified to teach a course in medical psychology. Like Abbot, he was not dogmatic about whether a physician or psychologists provided instruction. What was important to these men was that medical students receive instruction in a psychology that was relevant to their profession.

The APA surveys the place of psychology in medical training

The American Psychological Association continued to pursue the subject of the inclusion of psychology in the medical curriculum after the 1911 symposium, by forming a Committee on Psychology and Medical Education. Its members were Shepherd Franz (Chairman), E. E. Southard, and John Watson; all three had participated in the symposium.

The Committee sent a questionnaire to all the medical schools of the United States and Canada. The Committee received replies from 71 out of 114 medical schools. The Committee then subdivided the replies according to the AMA Council of Medical Education classification of the schools: A+, A, B, C. This classification was devised by the Council on Medical Education of the AMA and published in 1913. A+ schools were those that provided the highest level of medical training; A schools were those whose training was
acceptable, but needed improvement; B schools were those that needed overall improvement if they were to be acceptable; and C schools were those whose training was so inferior as to be completely unacceptable.¹⁰⁷

The survey sent out by the Committee sought to discover interest in psychology, what amount of instruction was currently given to psychological topics, what amount, if any, of such instruction was planned for the near future, and who did the instruction in psychology when such courses were offered.

Overall, the Committee must have been discouraged. While a significant number of replies indicated that psychology was thought to be of some importance, few schools offered instruction in it, although ten planned to do so in the near future. Additionally, there appeared to be great confusion on the part of those replying as to what was meant by psychology, which reflected the confusion within the discipline itself. As to the question of instruction in psychology, a few indicated that relevant course work was handled by psychologists from associated academic departments. Other forms of interaction that were indicated included borrowing apparatus and admitting psychology students to special lectures in psychopathology. The one encouraging note came from the data from the schools rated as A+. Several of these schools already offered instruction
in some form in psychology, while several more planned to do so.

The Committee made several recommendations based upon the results of their survey. Medical education in psychology should be practically oriented, related in some definite fashion to the needs of physicians. These courses should be taught either by psychologists or jointly by psychologists and qualified physicians. The courses offered, while practical in aim, should include instruction in laboratory or experimental work. Franz concluded his report on the work of the Committee by calling for close cooperation between psychologists and physicians:

Since both classes have many common interests it would appear wise that the knowledge of psychologists should be utilized by physicians and that in turn the experience of more physicians might be made available for the advancement of psychology and psychopathology.  

**Psychology and Medical Education: Expansion of the Boundaries of Psychology?**

When seen within the larger context of the reform of medical education and the contested content of the medical curriculum, the suggestions and insights of Franz, et al., can be understood as united in their plea for a place for psychology in the medical curriculum. These participants all were invested in the importance of psychological instruction in the medical school.

That those who were leaders in the reform of medical education did not share their views can be seen in the
comments of an internist, Walter James (1918) and a psychiatrist, William Graves (1914). Graves lamented the lack of instruction in psychological science, arguing that it left those who wanted careers in neurology and psychiatry without an adequate information base. Walter James argued that the reason that so many physicians were ignorant of mental disorders and mental factors in disease states was the lack of medical school instruction. Physicians, he argued, simply were not trained to pay attention to psychological processes or to understand them. The comments of James and Graves reflect the problematic status of psychological sciences in the first two decades of the twentieth century. Neurology and psychiatry, of which psychology can be understood as a subset, were marginal to the leaders of medical education.

There were exceptions to the exclusion or marginality of psychological sciences in medical schools, but they were few in number. For example, Adolf Meyer at Johns Hopkins was able to push his agenda of psychobiology into the medical curriculum. But a more formal inclusion of psychology in medical education had to wait in most places until a real utility was found for a psychological approach to issues that were of real concern to the medical profession. Two of those issues, sexuality and psychosomatic disorders, are the foci of later chapters of this work.
A fitting footnote to this section comes from an address given by one of the principal players in the development of psychologists' involvement in the issues of sexuality and psychosomatic disorders, Robert Mearns Yerkes. Yerkes was a transitional figure in the relationship between psychology and medicine. He was instrumental in pushing the boundaries of psychology as a science and as a profession. In 1920 his address as the Chairman of Section I of the American Association for the Advancement of Science (AAAS) focused on the place of psychology in medical education. Touting his own variant of psychology, psychobiology, Yerkes echoed the pleas of earlier advocates for a place for psychology in the medical curriculum when he urged that his subject be considered a basic medical science. In his view, psychobiology was more inclusive than psychopathology, for it considered humans in their totality and approached the study of humans as a strictly biological phenomenon. A course in psychobiology, he asserted, "would constitute a natural bridge between physiology and psychiatry." Yerkes acknowledged the reality of an already overcrowded medical curriculum and proposed that, at first, his course in psychobiology be voluntary. Beyond the initial course, Yerkes envisioned advanced training in medical applications of psychobiology for interested students; applications to problems of psychiatry and preventive medicine and hygiene were two salient proposals. Yerkes, who was by training a
psychologist, suggested that his proposed courses be taught by "a thoroughly competent biologically trained psychologist." As we shall see, Yerkes' proposal, in modified form, became a reality at Yale Medical School more than a decade later.\textsuperscript{111}

At approximately the same time that Yerkes made his address to the AAAS, he submitted a proposal to Adolf Meyer for the establishment a professorship in psychobiology in the Johns Hopkins Medical School.\textsuperscript{112} Yerkes argued that instruction in psychobiology would offer medical students an opportunity to learn psychological methods in an approach grounded in biology. Practically, instruction would be given through elective lectures and demonstrations of psychological methods of measurement and application.

Yerkes also suggested in his proposal to Meyer that a new approach to graduate training in psychology be developed around psychobiological principles. Yerkes argued that this would allow for greater interaction between medicine and psychology. This kind of graduate program, Yerkes asserted, would produce graduates who were thoroughly trained in the application of professional psychology and in scientific psychology. Yerkes' proposal was turned down. Adolf Meyer circulated the proposal to his colleagues in the medical school and in the existing psychology department. The response was very unfavorable.\textsuperscript{113} No professorship was established. However, what is clear from Yerkes' proposal
and the correspondence is the increased salience of American psychology. Yerkes was confident in his science. In his view psychology had demonstrated its relevance for a wide range of human problems.

At the end of the first two decades of this century, the relationship between psychology and medicine was complex and multifaceted. Physicians were concerned with the application of basic sciences to human problems; most psychologists were concerned with the development of their discipline as a natural science. Those psychologists and physicians who worked the borderlands between the two fields realized that each could make significant contributions to the endeavors of the other. Medical education was one nexus that was considered vital to such cross-fertilization of their respective work. That these workers failed to fully realize their vision does not obviate the growth of understanding that did occur. Their efforts set the stage for increased collaboration between physicians and psychologists in the next two decades and beyond. Indeed, collaboration was realized in areas not foreseen clearly by the laborers of the first twenty years of the new century. And the role of psychology in medical training did become a reality, due in no small part to the preparation made for it by men like Franz, Wells, Abbot, and Yerkes.
Psychologists' Research on Medically Relevant Topics

Position papers and negotiations to include psychology in the medical curriculum notwithstanding, the work of psychologists in the first two decades of the twentieth century was research. It is here where the public statements of psychologists concerning their usefulness or relevance to medical concerns can be evaluated. What psychologists said is, of course, interesting, but what they did is ultimately more important.

In this section I review the research of psychologists on medically relevant topics in the first twenty years of the twentieth century. I argue that this research was important to the development and expansion of psychology as a science-based profession. The work was suggestive, laden with potentiality; it concerned what might be done with experimental psychology. While other experimental psychologists were expanding their discipline into education and other practical settings, the work of psychologists in psychiatric settings helped move experimental psychology into that borderland also occupied by medical investigators and practitioners. As such, their work was a signpost to a future when psychological methods would play an important role in the interpretation and treatment of mental disorders and when "abnormal psychology" would fall mostly within the province of those who were academically trained psychologists.
If the broad course of psychological research in this area is charted, two main paths are traceable. The first path to be followed by psychologists interested in medical topics led to the laboratory and the research was either conducted there or with laboratory instruments (or adaptations of those instruments) in clinical settings. In the second decade of this century, psychologists began to follow the second path: mental testing using psychometric methods. Such research became an increasingly important approach to the investigation of medically relevant mental phenomena. In general, these two paths reflected trends in the broader science of psychology. That is, for most psychologists in the first years of this century, research meant laboratory work or its equivalent. The second decade of this century saw the emergence of psychometric research with its focus on individual differences. By 1920, a number of psychologists were employing psychometric methods in psychiatric research.

I first review the laboratory-based research of psychologists in medical settings or on topics of medical concern. Next, the use of word association tests in the assessment of mental disorders is examined; I argue that the association tests served as a transitional method to the use of psychometric methods. I then review the psychometric research of psychologists in psychiatric settings or who
addressed topics of interest to medical professionals in their research.

**Laboratory-based Psychological Research in Medical Settings**

At the turn of the twentieth century, there were important leaders in both neurology and psychiatry who expressed great hopefulness that the methods of the new experimental psychology would throw light on the problems of mental disorder. Encouraged by the work of Emil Kraepelin in Europe, American researchers saw experimental work as critical to the elucidation of a scientific basis for understanding mental disorders. Kraepelin extended the methods he had learned in the psychological laboratory of Wilhelm Wundt to psychiatry and to his monumental classification of mental disorders. In America at this time very little work was done in this area using the methods of experimental psychology. One example of such work was a study done by William Noyes, a psychiatrist at McLean Hospital and one-time fellow with G. Stanley Hall at Johns Hopkins. Noyes published a lengthy study on the knee-jerk reflex of an elderly patient diagnosed with terminal dementia. He used apparatus that was then beginning to be used in psychological laboratories and he was clearly influenced by the attitude about psychological research espoused by Hall. Psychologists did not become involved in this research until the end of the nineteenth century and it was not until after the turn of the century that
psychologists began to produce a substantive body of research that attempted to explicate the mental phenomena present in mental disorders. This small body of laboratory-based research was generated by a few individuals in the first two decades of the twentieth century. Many of these individuals went on to careers in psychology far removed from their psychiatric research; a few spent their lives in work that helped lay the groundwork for the emergence of what is now clinical psychology.

One of the first psychologists to conduct research in mental disorders was Boris Sidis, a student of William James. Sidis worked with Ira Van Geison at the New York Pathological Institute for three years around the turn of the century. Sidis was strongly influenced by the French clinical tradition and its approach to mental states through hypnotism. The clinical research of Pierre Janet in France on dissociative states was pursued in this country by Sidis. He published his Psychology of Suggestion in 1898 and followed it in 1902 with Psychopathological Researches: Studies in Mental Dissociation. Sidis reported that he used extensive laboratory apparatus in his research, but his books were philosophical speculations about the meaning of personality. One enduring contribution of Sidis was his placement of mental disorders within the natural order of the world and so amenable to scientific investigation. Though mysterious, these phenomena would eventually yield to
the patient investigations of psychologists as they applied
the objective methods of science.

Soon after the publication of *Psychopathological Researches* Sidis returned to medical school and spent the remainder of his life as a physician. Sidis did maintain his interest in mental phenomena, especially his interest in dissociation. His later career was marked by an interest in the role of hypnosis and the hypnoidal state in psychotherapy.

**Shepherd Ivory Franz: Manic-depression and the laboratory**

More central to the application of the methods of experimental psychology to psychiatric disorders was the research of Shepherd Ivory Franz. When Franz accepted the position of Pathological Psychologist at the McLean Hospital, he had already published research results in the area he became best known for: studies of cerebral function. After his appointment at McLean, Franz continued this line of research while also pursuing research in the more clinically relevant phenomena that were more directly connected to his position in a mental asylum. Like Sidis, Franz placed the various phenomena of mental disorder within the natural order and, thus, amenable to experimental investigation. His articulated rationale for such research was that by studying psychological processes in the psychologically disordered, the psychological processes of the normal would be elucidated. Franz asserted that "much
valuable information [about normal psychological processes] could be obtained from a study not only of the defects, but also of the exaggerations and inconsistencies in the insane and feeble-minded." Among his first published research on clinically relevant topics were three studies on manic-depressive insanity.

In these studies, Franz hoped to demonstrate the "fruitfulness and importance of the application of psychological and physiological methods in psychiatry." All the studies reported in this series were conducted in the laboratory of McLean Hospital. The role of laboratory instruments to explicate mental functioning was well-established in experimental psychology by this time, but such instrumentation was not part of the standard armamentarium of the psychiatrist. Psychiatrists had developed a tradition of reliance on clinical observation in order to make diagnoses, provide treatment, and predict the course of their patients' disorders. Franz, while not discounting the importance of clinical observation, tried to demonstrate the pertinence of laboratory work to the development of a more accurate clinical assessment of the patient.

In all three of the studies, Franz assessed the patients' performance on standard laboratory tasks. These tasks had been developed for use with a non-psychiatric population. They included reaction time (both simple and
observation." He also sought to correlate changes in degree of depression with changes in retardation.

The results in this first study were somewhat equivocal. There was a slight correlation between changes in depression and changes in rate of tapping: as depression lessened, the rate of tapping increased. However, Wells also found that not all the retardation could be accounted for in terms of depression and so he suggested that retardation appears in three forms or aspects: depression as primary with retardation as a secondary feature, retardation as primary, and retardation independent of emotional condition. Wells asserted that depression with retardation probably occurs more frequently than the others. He interpreted this result as meaning that, in normals, it is a common feature of adjustment or attempted adjustment by the individual to some feature of the external world, such as trauma. In those who are manic-depressive it is also an attempted adjustment, but in their cases, it is due to internal, probably physiological, disturbances.

Wells extended his subject pool in a further study employing tapping time as a measure of psychomotor retardation. He added data gathered from patients who had received diagnoses of dementia praecox, general paralysis, mania, and depression without mania. In this study, Wells continued to press his point about the necessity of psychological experimentation as a supplement or corrective
to mere clinical observation.\textsuperscript{131} He wrote, "The phenomena which seem to be associated with motor retardation in these experiments have appeared most strikingly in cases in which little if any retardation would have been evident to ordinary clinical examination."\textsuperscript{132} It was Wells's conclusion that the tapping test offered a way toward an improved method of differential diagnosis among these diverse clinical disorders.

Wells continued to do laboratory research through the 1910s, although he increasingly relied on psychometric methods to describe the individual differences he found.\textsuperscript{133} For example, in 1919 Wells published a group of studies titled "Psychological Parerga," in which he utilized both laboratory and psychometric methods.\textsuperscript{134} Laboratory methods were used to investigate pathological performance on standard psychological tests, including comparisons between normals and manic-depressives on reaction times to various sensory stimuli and the ability to follow directions. Other studies included in the report were more oriented to general issues of personality and learning and included more typically psychometric tests. The mixture of approaches in these studies reflected Wells's increasingly eclectic approach, as he sought to illuminate mental functioning in a clinically relevant manner and were also indicative of the trend toward more clearly psychometric methods in clinical research.\textsuperscript{135}
choice), tapping, rapidity of reading, marking out letters, time of adding, and accuracy of movement.

One study, "Time of Mental Processes," was a comparison of performance among depressed, manic, and normal patients. Franz found that the depressed patients' reaction times were longer than normals, but the reaction times of those in the manic condition were not shorter. The variability in reaction time, Franz concluded, did not differ widely among normals, manics, or depressives. Franz offered a two-fold explanation: first, that habits are as critical among the insane as among the normal population; second, that psychophysical processes, mainly in the peripheral nervous system, are at the root of the differences between normals and manic-depressives.

Franz and G. V. Hamilton, a psychiatrist at McLean who wanted training in experimental psychology, examined the effects of exercise on patients' performance on laboratory tasks. They found that exercise significantly increased reaction time. In general, Franz reported that those patients who participated in his research studies showed improvement. This, to Franz, suggested that such experimentation had therapeutic value in addition to its value as an explicative method.

Frederic Lyman Wells: From academic laboratory to clinical application

Franz's successor at the McLean Hospital, Frederic Lyman Wells, continued the use of traditional experimental
methods to explicate the relationship between mental states in the insane and the normal. From 1908 to 1920 and beyond, Wells sought to elucidate psychopathology through the experimental method. Initially, Wells's work was based on the use of laboratory apparatus (or adaptations thereof). His earliest work in what he preferred to call experimental psychopathology was within the standard framework of topics thought suitable for general psychological investigation, habits, psychomotor function, attention, etc.

Like Franz, Wells employed measures of psychomotor performance as indicative of normal and abnormal mental processes. Wells was careful to link his investigations not only to the standard practices and topics of his fellow psychologists, but also to the investigations of those few psychiatrists who were experimentally investigating psychological disorders. For example, in his work on psychomotor functions, he linked his investigations to those of August Hoch, a prominent psychiatrist who had trained with Emil Kraepelin. By establishing this historical link, Wells sought to place his work within the context of accepted psychiatric research. His intention was to extend this line of investigation by applying what he considered the superior methodology of experimental psychology. Also, like Franz, Wells desired to bring greater objectivity and accuracy to psychiatric research and treatment than could be obtained by ordinary clinical observation.
Wells had critiqued experimental methods in psychopathology in an article published soon after obtaining his position at McLean and so was well aware of the strengths and weaknesses of such research. In his critique he had argued for the establishment of norms of performance on the various tests, against which the performance of mental patients could be measured. In his new position Wells set out to establish just such a baseline on the tapping test, a measure of repeated voluntary movements thought to be relatively free of higher cortical processes. As he phrased it, "an important object of the experiments was to obtain normal data for comparison with pathological cases unamenable to more rigid experimental control." Once Wells established an acceptable baseline, he then conducted a similar series of experiments on patients who had received a diagnosis of manic-depressive insanity.

In the first study published from the data collected, Wells's subjects were all in the depressed phase of manic-depression. Psychomotor retardation, the slowing of responses to stimuli, was considered the crucial symptom of manic-depressive insanity. Psychiatrists used clinical observation to determine the extent of such retardation. Wells argued that by testing these patients with his method he would be able to detect retardation and "changes in its amount so slight as to escape ordinary clinical
A consistent theme in Wells’s research was adjustment. By adjustment, Wells meant how well an individual was able to adapt to the conditions of the immediate environment. Wells sought to discover psychological functions or dysfunctions as they related to "the conditions of actual life," and he increasingly sought to describe the conditions that led to lack of adjustment to these conditions that he saw as characteristic of individuals committed to the institutions that he worked in. In a study of choice reactions in dementia praecox and manic-depressive cases conducted jointly with H. A. Sturges, Wells justified their research by noting that life is a series of choices, reactions that help the individual adjust to the exigencies of day-to-day living. Healthy individuals, he argued, make choices that lead to well-adapted, happy lives. On the other hand, "Discontent, failure, mental disease, are expressions of bad, ill-adapted, false systems of psychomotor adjustment." This emphasis placed Wells within the evolutionary tradition in American psychology, as well as providing links between his work and the emerging behaviorist tradition. One historian of psychology has termed the psychologists who became involved in applied settings in education, industry, and medicine, "architects of adjustment." Wells was, perhaps, the first such architect.
Miscellaneous laboratory research

Wells and Franz, as pioneers of clinically relevant psychological research, understood the limitations of traditional laboratory approaches in psychiatric settings. For example, patients could not always be brought to the laboratory. Nor, once there, could they be counted on to understand or cooperate with the investigators. Both Franz and Wells found it necessary to adapt standard psychological laboratory tests and apparatus to the exigencies of clinical work.¹⁴¹ As Franz wrote in his 1912 Handbook, "Certain psychological methods have been found to be too complex to be used with a number of patients, and for clinical purposes others require too great an amount of time because of the elaborate apparatus and adjustments."¹⁴²

One person who attempted to circumvent the limitations of the psychological laboratory for psychiatric research was L. C. Gatewood, a graduate student in psychology at Ohio State University. Gatewood believed that certain psychological processes that were disturbed in various forms of mental disorder could not be investigated in a psychological laboratory. He adapted several traditional laboratory tests for use with asylum patients, so that they could be administered where the patients were, rather than in a laboratory.¹⁴³

In 1909, Gatewood sought to experimentally test Kraepelin’s theory of progressive mental deterioration in
dementia praecox. That is, as the course of the disease progresses, one would expect to see an increasing disturbance in the psychological processes of attention, association, perception, memory, judgement, motor efficiency, and emotionality. Gatewood tested these processes in normals and in patients at an asylum. He attempted to control extraneous sources of error by testing his subjects at regular intervals, using the same methods in the same way each time. Gatewood found significant differences between normals and dementia praecox patients. He concluded that one constant factor that marked the dementia praecox subjects was the deterioration of intellectual functions.144

Gatewood's work was an extension of experimental methods that had been developed in the laboratory. He found that the apparatus had to be modified in order to be workable, but once modified such apparatus could be used to investigate mental processes in a clinical, rather than laboratory, setting.

Two other notable studies published in the 1910s reflected the influence and direction of Franz and Wells more directly: Edwin G. Boring's "Learning in Dementia Praecox" and Edward K. Strong's "A Comparison Between Experimental Data and Clinical Results in Manic-Depressive Insanity."145 Edwin G. Boring spent time with Shepherd Franz at the Government Hospital for the Insane, where he did
research with patients suffering from dementia praecox, and Edward K. Strong worked with Fred Wells during Wells's brief tenure at the Psychiatric Institute on Ward's Island, New York. Strong compared clinical and experimental data on manic-depressives in multiple phases of their condition, while Boring examined whether or not dementia praecox patients could acquire habits that might lead to their useful occupation while in residence at mental institutions. Psychological research with psychiatric patients was not central to the later careers of either of these men. Their work in psychiatric settings reflected their interest in psychopathology. It also reflected the increased opportunities for psychologists in psychiatric settings. Such opportunities had not been available to graduate students in 1900. The work of psychologists like Franz and Wells had expanded the boundaries of psychology.

**Association Tests and Pathological Mental States**

Association tests began to be used with psychiatric populations in America around 1910. In some ways, the association test was intermediary to the earlier emphasis on standard laboratory tests and the later psychometric approaches. Like the standard tests, it had a history in psychological testing and, like psychometric tests, it did not rely on elaborate instrumentation or a specific locale for its administration.
Francis Galton, the English counter **extraordinaire** and cousin of Charles Darwin, was the originator of the association test.\(^{1}\) The German physiological psychologist, Wilhelm Wundt, routinized the use of it in the investigation of the normal mind. The test soon became part of the standard repertoire of the new experimental psychologists, as well as finding a place in the developing methodology of psychoanalysis. Emil Kraepelin utilized tests of association as one of the many experimental instruments in his multitudinous researches. But it was the through the work of the Swiss physician, Carl Jung, that the test passed into frequent usage as a tool for the investigation of psychopathological states.

Jung, while working with the noted Swiss psychiatrist, Eugen Bleuler, in the first decade of the twentieth century, used association tests to uncover hidden psychological complexes. He observed that some words seemed to be laden with emotional content for some individuals and that this, characteristically, caused a delay in response time. This, he thought, pointed toward hidden complexes. He categorized the modes of responding to the tests into three types, each with its own characteristic complexes. Jung viewed the association test as a way of scientifically investigating Freudian concepts.\(^{1}\)

In America, Jung’s work was embraced by a small but growing number of individuals interested in psychoanalysis
and by the few psychologists who were working in clinical settings. For example, the possibility that a delay in response to the stimulus word might indicate the presence of unconscious factors appeared to hold fascination for several researchers. Vassar's Margaret Floy Washburn and her student, Hazel Leach, published a study on association reactions and mental diagnosis and concluded that long delays in responding probably indicated either disturbed affect or the presence of an emotional complex.\textsuperscript{148} Shepherd Franz and Fred Wells also followed Jung's lead and employed the association test in research on the clinical population.

In 1909, Franz and William Alanson White published the results of their researches with the association test.\textsuperscript{149} They noted that, while the test was useful in eliciting emotionally charged material, the subjective mental state of the subject must also be considered as a potentially confounding variable. In a follow-up study published the next year, Franz reached a similar conclusion and noted that subjective nature of the responses made it extremely doubtful for clinical use.\textsuperscript{150}

Franz moved away from this type of research after the publication of the latter article. He did include, however, lengthy discussions of the association method in the two editions of his \textit{Handbook of Mental Examination Methods}. He cited what was common psychological understanding at this time: when there was a delay in reaction time with the
response to the stimulus word, it usually indicated that the first response, the "immediate" one, had been suppressed in favor of a "mediate" response, one further removed from the emotion elicited by the stimulus word. As he wrote in the second edition of the Handbook, "It is by reason of this increase in time that certain reactions which appear to be logically normal are detected to be abnormal."\textsuperscript{151} And, he noted, these "mediate" reactions occur more frequently among the insane.

Fred Wells wrote extensively about the association method in research with normals and those confined to mental hospitals. Along with Robert Woodworth, he wrote the report on association tests for the American Psychological Association’s Committee on the Standardizing of Procedure in Experimental Tests.\textsuperscript{152} Their report was mainly focused on the use of association tests in the investigation of the normal mind. However, they noted that the free association test could be used to detect hidden, diagnostically relevant material in suspected cases of psychopathology.

Wells explored the uses of the association test for a variety of tasks. For example, he addressed the usefulness of the association method in classifying the psychological types suggested by Carl Jung and found limited support for its use in this way.\textsuperscript{153} In a popular review published in 1914, he concluded that the use of association tests with those suffering from mental disorders was of limited
usefulness in diagnosis or treatment. Among those with dementia praecox, for example, association tests elicited neologisms and irrelevant associations, while among manic-depressives the responses were about the same as among the normal population.¹⁵⁴

Grace Kent, one of the first women psychologists to work in a psychiatric setting, was responsible for the delineation of the psychometric properties of association tests. Kent received her master's degree in psychology under Carl Seashore at the University of Iowa in 1904. Kent eventually completed her doctorate under the direction of Shepherd Ivory Franz. Soon after completing her master's degree, she accepted a position in the Insane Department at the Philadelphia Hospital. She remained there a little over a year and then moved to Danvers State Hospital in New York, where she worked for three years. It was at Danvers that she collaborated with A. J. Rosanoff, Danvers' clinical director, on a massive study of word association among normals and the insane. Their study was published in two parts in 1910.¹⁵⁵

Kent and Rosanoff selected one hundred words as their stimuli for association and had over one thousand "normal" male and female subjects, ranging in age from eight to eighty, free associate to them. No reaction time was taken for the responses. They then compiled frequency tables indicating the different reactions to each word. Kent and
Rosanoff classified the responses on the basis of their statistical frequencies, rather than on an a priori philosophical basis, as had been done by others. This move was, at once, both practical and rhetorical. It was practical because there was no obfuscation of meaning from a philosophical bias, nor any loss of data because it did not fit such a scheme. It was rhetorical because their approach was "empirical" and "objective," two words that carried great weight in the transformation of psychiatry and psychology into clinical sciences.

The main point of Kent and Rosanoff's study, however, was to use the association test as a method of detecting and predicting psychopathology. In order to do this, they compared the responses of normals with the free associations of those institutionalized at Danvers for various mental disorders, including dementia praecox, manic-depressive insanity, paranoid conditions, general paresis, and epilepsy. While there were differences, the authors had to conclude that the test was not pathognomonic:

By the application of the association test, according to the method here proposed, no sharp distinction can be drawn between mental health and mental disease; a large collection of material shows a gradual and not an abrupt transition from the normal state to pathological states.\textsuperscript{156}

Despite Kent and Rosanoff's conclusion, research into the possible usefulness of the association test in detecting abnormal or unusual emotional or psychological states continued. The research of Wells mentioned above was an
example of this continuing line of research. Edward Strong, in the study reported above, included association tests in his research. He did not find the tests to be especially helpful in differentiating among clinical groups. Herbert Langfeld, later to be a president of the APA, employed association tests in an investigation of the ability of dementia praecox and manic-depressive individuals to voluntarily suppress their responses. He, too, found few differences between normals and those diagnosed as insane.

This pattern of results was repeated in a few other studies over the second decade of the twentieth century. Clark Hull, later a leader of neo-behavioral theory building, conducted two studies of word association among the insane. His 1917 study investigated differences between normals and three types of insane subjects on tests of formation and retention of associations. He concluded that it is difficult for the insane to form associations. Of greater interest is the 1921 study on free associations. Here, Hull examined the notion of hidden complexes and their revelation through the free association technique. He employed Jung’s list of words (with slight variations) and utilized reaction time as an indicator of emotional complexes. His subjects were 50 normal men and 50 normal women. Hull found that free association reactions did appear to signify the presence of latent emotionality. However, the results on reaction time were more equivocal.
Hull found that there was not a sharp distinction between long reaction times and intermediate ones. Overall, his study provided some support for free associations as a useful diagnostic marker for the presence of emotional complexes.

Hull's work with the association method in this period was tangential to his later career as a theorist. The method of free association in clinical research also proved tangential. The association method as a diagnostic aid was never firmly established. Interest in it diminished over the course of the 1910s, as psychometric methods came into vogue and were applied to the investigation of individuals who were considered psychologically disordered. Its significance lay in its role as a transitional method. It did not require elaborate laboratory apparatus and it allowed for individual differences. Though the early hope in it was found to be misplaced by most American investigators, the association method was important in pointing the way toward methods that freed psychologists from their dependence on the laboratory.

**Psychometric Research with the Psychiatric Population**

The work of Kent and Rosanoff with their frequency tables of word associations was an attempt to standardize and bring objectivity to an assessment method that was marked by subjective interpretations of subjects' responses. Their work was an example of a movement in
psychology toward psychometric soundness in mental tests of all types. Although the American Psychological Association (APA) had been concerned enough about standardization of experimental procedure to establish a committee in 1906 to promote consistency in such procedures, the APA had done little to assure psychometric soundness in the rapidly proliferating mental tests.\textsuperscript{159} As Richard von Mayrhauser points out, the concern with test validity and accuracy and the concurrent concerns with standardization and establishment of adequate norms for assessing any individual's performance became central issues in the nascent mental testing movement during the 1910s. The debate over these issues continued into the next decade and was codified by a new term, \textit{psychometrics}.\textsuperscript{160} There is a rather large literature, from this period and currently, that addresses the main issues of the debate.\textsuperscript{161} Charles Spearman, the British psychologist noted for the development and application of complex statistics in psychological research, weighed in heavily on the need for standardization of psychological tests in work with psychotic patients.\textsuperscript{162} However, there is little historical scholarship that addresses the role of psychometric testing in a psychiatric population.

\textbf{Robert Yerkes and the Point Scale}

History may rate as my major service to the Psychopathic Hospital the development of the Point Scale and of certain other more specialized methods of practical mental measurement and of research in
psychopathology. All of these instruments were designed with the special need of psychopathic hospitals in mind.¹⁶³

These words of Robert Yerkes were part of his reminiscences of a lifetime of research and service in psychology. While others played the starring roles in the development of mental testing among the feebleminded, Yerkes certainly was the leader in the parallel developments in the psychiatric setting.¹⁶⁴ His work at the Boston Psychopathic Hospital serves as a cogent illustration of the application of psychometric tests to problems of psychopathology. He and his assistants, principally James Bridges, Rose Hardwick, and Cecilio Rossey, developed the Point Scale as an alternative to the Binet scale and argued for its greater usefulness among a clinical population. As he and Bridges stated in 1914, they were concerned with assisting the physicians at the Hospital in determining the "mental status of socially troublesome individuals."¹⁶⁵

When Robert Yerkes was asked by E. E. Southard to accept a half-time position of staff psychologist at the new Boston Psychopathic Hospital, he was relatively inexperienced in clinical application of psychological principles. Animal psychology, a natural science, was his first interest and he viewed himself as a practitioner of scientific psychology with relatively little interest in its application. However, under pressure from his superiors at Harvard to devote more of his time to practical applications
of psychology, Yerkes accepted the post at Boston Psychopathic and for the next four years spent his afternoons "adventuring in psychopathology." His principal accomplishment, in his own eyes, in his four years at the Hospital was the development of a test of intelligence and intellectual ability that was suitable for the individuals that he and his assistants saw daily in the clinical setting.

Yerkes later described his initial task at the Hospital as primarily consisting of determining the mental age of the patients seen by the attending physicians. His only tool to accomplish this end was the recently developed Binet scale of intelligence. Yerkes found the Binet scale to be inadequate and ill-suited for psychiatric work. Its chief faults, he argued, were that it lacked standardization, it was designed primarily to be used with schoolchildren, and its method of all-or-none scoring failed to capture the full range of intellectual ability in many patients. Yerkes and his colleagues set out to devise a method to obviate these shortcomings.

Yerkes found in James Bridges and Rose Hardwick able assistants. Bridges, a Canadian, was a Harvard doctoral candidate at the time that he began working with Yerkes at the hospital. Hardwick had received her master's degree from Smith College in 1909 and was interested in educational assessment issues throughout her career. They, along with
Cecilio Rossey, were prototypes of clinical interns among psychologists. Under Yerkes' supervision they performed psychological assessments of new patients in both in-patient and out-patient settings.

Hardwick and Bridges collaborated with Yerkes in the development of the Point Scale. The Point Scale was based upon the Binet scale, with revisions in arrangement of tests and method of scoring that its authors argued made it both simpler and time-saving to administer. Yerkes, Bridges, and Hardwick offered an extended rationale for their new assessment device, along with directions for its use and its standardization with both normal and abnormal populations, in *A Point Scale for Measuring Mental Ability* (1915). As Fred Wells pointed out some twelve years later, the Point Scale was clearly more psychometrically sound than any version of the Binet scale then available. While Yerkes and colleagues were not the first to pay attention to the psychometric properties of tests, they were among the first to develop a test with as much attention to its psychometric properties as to its face validity.

Results of the Point Scale with a psychiatric population

The prestigious *Boston Medical and Surgical Journal* published a guide to the use of the Point Scale by Hardwick, in which she contrasted and compared the scale with Goddard's revision of the Binet scale and gave administrative directions. Echoing Yerkes, she referred
to the fact that, of all the tests then in use, the Yerkes-Bridges Point Scale was the only one developed specifically to meet the needs of a Psychopathic Hospital.\textsuperscript{172} Hardwick expressed concern that procedures for administration should be carefully followed and all relevant data be gathered for each subject. Her stated reason for this requirement was that it was necessary to improve the psychometric soundness of the scale, as well as to ensure an accurate estimate of the subject's intellectual ability.\textsuperscript{173} She also noted the difficulty of administering the scale in the noisy and busy hospital setting.

In \textit{A Point Scale for Measuring Mental Ability}, Rose Hardwick reported the results of using the new Point Scale with 155 individuals seen in the out-patient service at Boston Psychopathic Hospital. Most were between the ages of 12 and 17. Hardwick found significant differences in reasons for referral between her younger subjects and her older ones. Subjects under 12 were typically referred for "mental" reasons, while the older ones were more commonly referred for social reasons. "Mental" referrals were usually due to problems in school (e.g., the child was not able to keep up with other students). "Social" referrals for the older subjects were usually due to problems in social adjustment (e.g., the individual had difficulty adjusting to a work environment). Hardwick noted the role of environmental factors in many of the referrals to the
hospital and candidly acknowledged that the poor living conditions of most of the patients were probably the single most important contributing factor in their referral.174

Hardwick used her results to argue for the greater accuracy of the Point Scale over the Binet scale. She and her colleagues, Yerkes and Bridges, argued that their scale allowed for a fine-grained distinction among various groups. Among the individuals Hardwick tested, she did indeed find a few whose mental age was differently reckoned depending on the scale used. However, she was unable to make clear just how the determination of mental age was clinically useful or how it might lead to a more suitable intervention in the person's life.

Hardwick worked with the initial version of the Point Scale, developed for individuals between the ages of three and fifteen. Yerkes and Cecilio Rossey, an intern in psychology at the Boston Psychopathic Hospital, were at work on a version of the Point Scale suitable for testing adults when the United States entered the First World War.175 This ended Yerkes' involvement with the development of the Point Scale, but the work was continued by others. Since the Point Scale was developed in a psychiatric setting it became an investigative tool in the hands of other psychologists interested in intelligence and psychosis.

Josephine Curtis, who had assisted in the development of norms for the adult Point Scale and who became the
Psychologist in Charge at the Boston Psychopathic Hospital after Yerkes left, in 1918 applied the scale in an extensive study comparing the "feeble-minded" and the insane among the patient population of the hospital. Subjects included individuals with diagnoses ranging from "psychopathic personality" to manic-depressive to psychoneurosis to unclassified paranoid. She found that the range of scores obtained from her mentally disordered subjects was less than that obtained from the feeble-minded or normal children, but that these individuals showed increased deterioration of intellectual function with the passage of time. Curtis concluded from her results that the adult version of the Point Scale could serve as "an aid in differentiating between feeble-mindedness and deterioration."\textsuperscript{176}

In the same year that Curtis presented her work, Sidney Pressey and his future wife, Luella Cole, published further results of their investigations of intellectual function, again using the Point Scale.\textsuperscript{177} Pressey previously had used the Point Scale to investigate intellectual deterioration in dementia praecox and alcoholic patients.\textsuperscript{178} Both studies had been carried out under the supervision of Yerkes at Boston Psychopathic Hospital. In the 1917 paper, Pressey had suggested that among this population the scatter (variation) of the subject's scores might indicate their deterioration from previous levels of functioning. In the Pressey and Cole collaboration, Pressey revised the previous conclusion
to include the possibility that what the scatter of "irregularity" of the scores indicated was interference with performance on the test caused by their mental condition. The authors ended by noting the potential value of using the scale as an aid in differentiation between feeble-mindedness and deteriorating psychosis.

The Binet Scale in psychiatric testing

The Binet Scales were not as widely used in the testing of individuals with severe psychopathology in the 1910s as was the Yerkes-Bridges Point Scale. This may have been in part, because its early applications in this country were with the feeble-minded and school children. For example, Henry Goddard at the Vineland Training School did extensive testing with the Binet scale with those suffering from amentia.\textsuperscript{179}

Sidney Pressey investigated the variation in scores on the Binet scale with subjects at the Boston Psychopathic Hospital as a comparison to the same phenomenon on the Point Scale.\textsuperscript{180} Josephine Foster, nee Curtis, utilized memory scales from the Binet in a study also conducted at Boston Psychopathic with a variety of subjects diagnosed with various forms of psychopathology.\textsuperscript{181} She concluded that "there is no evidence that the insane give responses in the memory span for digits or sentences which are markedly different from those given by normal persons."\textsuperscript{182} Harry Hollingworth administered a form of the Binet scale adapted
for group use to soldiers committed to a military hospital during World War I. He concluded that previous level of intelligence was predictive of neurotic symptomatology. In 1920, Fred Wells and C. M. Kelley reported equivocal results with the Stanford-Binet on over 100 cases newly admitted to the McLean Hospital. For some disorders, particularly those thought to be due to organic dysfunction, IQ scores obtained were markedly lower than normals and other disordered individuals. For those individuals diagnosed as having dementia praecox and manic-depression, the scores were not consistently lower than normals. The authors cautioned, as Wells had done twelve years previously, that the cooperation of the subjects was critical in obtaining valid test results.

The Binet scales were more difficult to administer to a mentally disordered population than the Yerkes' scale. They also required more time and as the involvement of psychologists in psychiatric work increased both in quality and in kind, assessment devices were sought that allowed for the most efficient use of the psychologists' time.

Research Results, Psychologists, and Psychiatry

The research literature of this period is not overly impressive or extensive. Academically trained psychologists, prepared to examine the normal white male human mind, had difficulty translating their training into clinical usefulness. Despite their difficulties,
psychologists persisted in applying psychological methods and techniques to the field of psychopathology. Just as they had done in the series of position papers on the relationship between psychology and medicine and the concurrent papers on the need to include psychology in the medical curriculum, psychologists offered a rationale for their psychopathological research that was designed to further their share of the fields of labor that bordered their discipline and that of medicine.

Shepherd Ivory Franz used the results of his laboratory testing of psychiatric patients noted above to address both his medical audience and his colleagues in psychology. To the psychiatrists, Franz claimed that his results supported his contention that the methods of experimental psychology were a valuable corrective and addition to the traditional methods of psychiatry. They were corrective, in that the laboratory data were more accurate and reliable than clinical observation alone. They were additive, in that the methods employed in the psychological laboratory allowed for an explanation based upon current conceptions of physiological/psychological processes. To his colleagues in psychology, Franz could point to the application of accepted experimental research methods in a new setting. In addition, his work was within the parameters of conventional psychological topics and methods: reaction time, movement, attention, etc. Reaction time as a measure of psychological
processes was already a traditional method of experimental psychology. Thus, the work of Franz, as reported in these three studies, occupied the borderland between psychology and medicine. Through these studies, Franz attempted to address the professional and scientific demands of both psychiatrists and psychologists, as well as justify his own position as a psychologist in a psychiatric setting.

Of perhaps more fundamental importance, Franz’s work, based on the use of laboratory instrumentation, was of a conceptual piece with research in many fields that indicated a "growing recognition of the inadequacy of the unaided and uneducated senses in understanding the world."185 Franz indicated that this was so in understanding the world of mental disorder. Since psychiatrists, following their traditional method of reliance on clinical observation, had so few successes in explicating mental disorder and even fewer in effective therapeutic intervention, the door was left open to psychologists, who were able to enter the field formerly reserved to medical professionals and find a permanent place there. Franz, like the psychologists who followed, brought the aura of science; he, and others who came after, presented research based upon experimental methodology and data gathered from the use of instruments designed to probe beneath the surface of human mental functioning and discover the recondite sources of mental disorder.
Like Franz, Wells addressed both a psychological and psychiatric audience, hoping to gain credence for the application of psychological methods and laboratory apparatus in a medical setting among both groups. Wells made his case that experimental psychological methods, such as he employed in the tapping test, offered a quantitative, reliable approach to greater accuracy in diagnosis and treatment of manic-depressive insanity, as well as increasing psychiatric understanding of the disorder. To this end, he offered an extended description of his experimental procedures and included extensive tables of quantitative results.

On the one hand, these maneuvers can be understood in the light of the attempt by many psychologists of this time to appropriate the quantitative approach of the natural sciences; mental phenomena that could be measured were more substantial than mental phenomena that could only be discussed in philosophical terms. On the other hand, Wells’s approach can be understood as an attempt to increase the standing of experimental psychologists like himself with the medical (psychiatric) establishment. Even though the experimental results of the psychologists were often equivocal, they were frequently more useful than the standard approaches of many psychiatrists.

Psychologists' work with free association and psychometric tests is more difficult to place in context.
Franz, Wells, Kent, and Rosanoff all concluded that the free association test was problematic in psychiatric work. While interesting differences between the insane and the normal were often found, a subject's responses were not pathognomonic. The development of mental tests and the attendant emphasis on their psychometric properties gave hope for their clinical usefulness. And clearly, psychologists made a place for themselves in psychiatric work with their expertise in mental testing. A psychometrically sound test was, as Fred Wells put it, "a psychological experiment which is standardized." The emphasis placed on norms and standardization carried the weight of a supposed objectivity and obviated the difficulties that had been found with the free association test. They were a new "technology based upon psychological science." Despite their promise, the results after 6-7 years of use, were equivocal and somewhat disappointing. Like the free association tests, intelligence tests yielded interesting differences between normal and not-normal individuals. And like the association tests, intelligence tests failed to be of much use for differential diagnosis. They were, as one reviewer remarked, too blunt an instrument.

Probably the most important result of the use of mental tests in clinical settings was the increase in the number of psychologists who were drawn into clinical work. Many of
these individuals entered clinical settings via an internship under a more senior psychologist. Grace Kent worked with Shepherd Franz at the Government Hospital in Washington and later was a mentor for a new generation of budding psychologists. Among that new generation was David Shakow, the "father" of modern clinical psychology. Robert Yerkes had a number of psychologists intern with him at Boston Psychopathic Hospital. The list includes Bridges and Hardwick, who have already been mentioned. It also includes Josephine Curtis Foster, who had a distinguished career in clinical and educational psychology. Sidney Pressey also served as an intern under Yerkes and went on to very distinguished career in psychology. There were several others not as well known but who were part of the growing number of psychologists doing practical work in a medical setting.

Conclusion

The twentieth century began with budding promise of a working relationship between the fields of psychology and medicine, particularly psychiatric medicine. Position papers were published by both psychologists and physicians touting the mutual benefits their relationship would bring. Both shared in the hopefulness of a better world through science.

Psychology clearly had more to gain from the relationship than medicine. Psychologists laid claim to the
mantle of "experimental science" to distinguish their field of endeavor from the old philosophical psychology and sought to apply their science to a number of practical concerns. Among those concerns were areas within medical practice. In Franz's metaphor, there were fallow fields ready for tilling that lay on the boundaries of psychology, psychiatry, and neurology. Though few in number, there were psychologists who began working those fields. The first psychologists to work in traditional medical settings, Franz and Wells, were individuals who had extensive training in physiology and were conversant with medical discourse. Both Franz and Wells published "propaganda" pieces designed to impress psychiatrists and fellow psychologists with the benefits of mutual cooperation.

Secondly, the attempt to place some version of academic, experimental psychology into the medical curriculum occupied a noticeable amount of space in both medical and psychological journals and even led to a special conference on the topic and to an official committee of the APA to address the issue. Revision of the medical curriculum was greatly debated in the first ten to fifteen years of this century. It was an opportune time to place psychology into the debate and fight for its inclusion in the curriculum. Significantly, it was not only psychologists who wanted to include it but some prominent physicians as well. This reflected the tremendous strides
that academic psychology had made in gaining respectability among the scientific and professional communities. That the attempt by and large failed at the time should not obscure the gains made by the inclusion of psychology in the debate over the medical curriculum. A fair number of the better medical schools did eventually make a place for psychology or required it in the undergraduate preparation. More importantly, the debate raised the profile of a fledgling science at a time when that was sorely needed. Psychologists were brought together with their medical colleagues in ways that proved important for future collaboration.

Finally, psychologists actually did work that was relevant to medical, especially psychiatric, practice. From the research of Boris Sidis with Ira Van Geison to that of Josephine Curtis at Boston Psychopathic Hospital under the leadership of E. E. Southard, psychologists demonstrated that they had something useful to offer. Experimental psychologists brought their methods to bear on practical problems of psychiatric concern. We judge too harshly if we minimize their results. In many ways they helped psychiatry move toward the mainstream of medicine. In particular, psychologists brought a rigor to their research that was needed by psychiatrists. In place of rambling, unfocused "clinical observations," psychologists offered data gathered from experimentation. While some of their conclusions and a
number of their methods eventually proved unreliable, their attempts at applying experimental methods to psychiatric problems encouraged psychiatrists to do the same. Edward Cowles, the psychiatrist most responsible for bringing experimental psychology into an alliance with psychiatry, acknowledged this in his 1916 account of the development of the McLean Hospital.189

The development of mental tests and their application to a wide range of the population, including the psychiatric population, brought even more psychologists and psychiatrists into contact with each other. In the 1910s, psychologists made a niche for themselves as testers, a niche that often proved too confining. The results of their testing among those with severe psychopathology were equivocal, but the role they played in expanding the range of psychiatry to include those suffering from milder forms of abnormal or subnormal behavior was largely accomplished through their testing. Though significant numbers of psychiatrists engaged in turf battles with psychologists to limit the psychologists' place in what the psychiatrists perceived as medical domains, by and large the battle for priority in testing was won by the psychologists by the close of the 1910s.

In the first two decades of this century, psychologists established a place for themselves as colleagues of a sort of the medical profession. They used the rhetoric of
cooperation and mutual benefit to respond to the conversation initiated by a few important medical professionals. Psychologists expanded this conversation to include a place for themselves in the training of physicians and, by doing so, gained a wider audience for their rhetoric. Lastly, psychologists did the research that placed them into a cooperative enterprise with medical researchers who also wanted to elucidate the workings of the human mind and body. As the 1920s began, psychologists were poised to expand their share of the scientific borderlands they had worked so hard to cultivate. In the remainder of this monograph I explore two such borderlands: human sexuality and psychosomatic illness.

Notes

1. Robert Mearns Yerkes, The Scientific Way [manuscript autobiography], p. 90. Yerkes Papers, Manuscripts and Archives, Sterling Library, Yale University, New Haven, CT. The quote refers to Yerkes' graduate training at Harvard, 1897–1902.


4. There is an extensive and growing literature on the application of psychology in the period covered by this chapter. In my opinion, John M. O'Donnell, The Origins of Behaviorism, American Psychology, 1870-1920 (New York: New York University Press, 1985) is the best. O'Donnell, however, pays scant attention to psychologists' links with medicine.


12. I rely on the accounts given by Gerald Grob and Roger T. Mulder for the material on the transformation of psychiatry.


16. Shepherd Ivory Franz noted that Cowles "opened his hospital to a layman specialist in psychology and had him appointed to the position of investigating psychologist." Who this "layman specialist" was is not stated and I have been unable to discover his identity. Franz, "On the Development and Need of Modern Psychiatry," *Bulletin of the Ontario Hospitals for the Insane* 2 (1908): 57-58.


18. I have drawn on the following sources for this paragraph: Dorothy Ross, G. Stanley Hall; G. Stanley Hall, "Laboratory of the McLean Hospital, Somerville, Massachusetts," *American Journal of Insanity* 51 (1895): 358-

19. I use men here because it was the term in general use at the time.


21. According to Adolf Meyer, Van Gieson was strongly influenced by Sidis. See Meyer, "Thirty-five Years of Psychiatry in the United States and Our Outlook," CP 2:1. Van Gieson calls the work of Sidis "brilliant" in "Correlation of Sciences," p. 757. Sidis was the chief psychological investigator at the Pathological Institute while Van Gieson was in charge.


25. ibid., p. 50. On James's interest in these topics, see Eugene Taylor, William James on Exceptional Mental States (New York: Charles Scribner's Sons, 1982).

26. ibid., pp. 42-43.
27. At the same time other psychologists were making similar claims for the application of psychology to education, advertising, and industry. This was to become the trend of psychology in the first decade of the twentieth century. One psychologist who opposed application, E. B. Titchener, nevertheless, acknowledged that psychology was moving toward the practical in an address given at the twenty year celebration of Clark University in 1909. See E. B. Titchener, "The Past Decade in Experimental Psychology," American Journal of Psychology 21 (19): 404-421. Ironically, his remarks were made to the same audience that Sigmund Freud addressed in his only visit to the United States.


29. Sidis was also a member of the "Boston Group" that met weekly in the home of the psychopathologist, Morton Prince, and that also included the psychologists William James and G. Stanley Hall and the physicians James Jackson Putnam and E. E. Southard. Among the many topics that this group reportedly discussed, psychopathology and psychotherapeutics were prominent. See Sandford Gifford, "Psychoanalysis in Boston: Innocence and Experience," in Psychoanalysis, Psychotherapy, and the New England Medical Scene, 1894-1944, ed., George E. Gifford, Jr. (New York: Science History Publications, 1978), pp. 325-345.


32. See note 2.

33. For a contemporary account of Kraepelin's approach, see August Hoch, "Kraepelin on Psychological Experimentation in Psychiatry," American Journal of Insanity 52 (1895-1896): 387-396. Hoch, a physician, had conducted psychological
experiments at McLean Hospital in the mid to late 1890s. Cowles had sent him on a tour of the new experimental laboratories of psychology in Europe to help prepare him for his research at McLean.

34. Franz, "Psychological Opportunity," p. 562. It should be noted that Franz had done such work in physiology at both Harvard and Dartmouth.

35. See note 16.


37. Dementia praecox was the term used for schizophrenia. Circular insanity referred to what we now call bi-polar disorder or manic-depression.


39. ibid., pp. 42-43.


41. Ibid., p. 107.


44. ibid., p. 37.

45. Ibid., p. 28.


48. Wells, "Experimental Method," p. 405. L. E. Emerson, a Harvard PhD under William James and one of the first psychologists to practice psychotherapy in a medical setting, made a similar point about the inadequacies of academic experimental psychology in relation to psychopathology, in a letter written in 1911 to James

49. See, for example, George Dearborn, "Medical Psychology." Medical Record 75 (1909): 176-178.

50. Shepherd Ivory Franz, "The Functions of a Psychologist in a Hospital for the Insane," American Journal of Insanity 72 (1916): 458. Franz delivered this as an address to a joint meeting of the APA and Section H of the American Association for the Advancement of Science (AAAS) in August of 1915. It is interesting to note that this was in the same year that Franz was awarded an honorary M.D. by George Washington University.

51. Grace Kent was employed as a psychologist at Kings Park State Hospital in New York from 1907-1910. She then took an internship with Franz at the Government Hospital for the Insane in Washington, DC. Psychologists were by 1910 also established professionally in the care of the feeble-minded. These institutions were also presided over by physicians, but that is another story and is recounted in other places. See Leila Zeilander, "Education, Evangelism, and the Origins of Clinical Psychology: The Child-Study Legacy," Journal of the History of the Behavioral Sciences 24 (1988): 152-165; the various chapters in Psychological Testing and American Society ed., Michael Sokal (New Brunswick, NJ: Rutgers University Press, 1987) are also relevant to an understanding of psychologists' interactions with the medical profession. See, also, JoAnne Brown, Definitions of a Profession.


57. According to Grob, *Mental Illness*, the four were the Psychopathic Hospital at the University of Michigan, the Boston Psychopathic Hospital, Pavilion F at the Albany (New York) Hospital, and the Henry Phipps Psychiatric Clinic at the Johns Hopkins Hospital in Baltimore. Adolf Meyer, however, cites psychopathic hospitals in Iowa City, Iowa and Denver, Colorado, "Thirty-five Years of Psychiatry," p. 9.


60. ibid., p. 144. Psychiatric social work was a new mental health field instigated by the collaboration of Southard and Mary Jarret. The basic premise of the field was that mental disorder, delinquency, and other deviations from social norms were due primarily to environmental causes.

61. Psychiatrists and the medical profession in general acknowledged their lack of facility with various types of what came to be known as mental tests. However, one exception to this may be found in a series of articles written by a physician, J. Victor Haberman. Haberman argued cogently and powerfully for the role of psychiatrists and neurologists in mental testing. See J. Victor Haberman, "Probing the Mind: Normal and Abnormal," *Medical Record* 92 (1917): 927-933, and "Probing the Mind: The Intelligence and Its Examination," *Medical Record* 93 (1918): 839-849.


63. Grace Kent was a doctoral student under Franz in 1910-1911. Her dissertation was one of the first by a psychologist to use data collected from patients in a mental hospital.

64. See, for example, *Psychological Testing and American Society*.

65. Psychiatrists, in general, were not gracious losers. The reactions of many can only be described as bitter, rancorous, and spiteful. Name-calling was not uncommon.
Adolf Meyer said that the term "clinical psychologist" was absurd and Walter Fernand used the phrase, "so-called clinical psychologist," both were quoted in a Note from the annual meeting of the New York Psychiatrical Society published in the Medical Record 92 (1917): 833. The same society circulated a recommendation to all the relevant professional organizations and journals condemning the activities of "clinical psychologists" in the diagnosis of feeble-mindedness and in other "medical" situations. One recommendation read as follows:

We recommend that the Society express its disapproval and urge upon thoughtful psychologists and the medical profession in general an expression of disapproval of the application of psychology to responsible clinical work except when made by or under the direct supervision of physicians qualified to deal with abnormal mental conditions.

The circular was printed in the Psychological Bulletin 14 (1917): 224-225. Shepherd Franz offered a stinging rebuttal in the pages that followed. Shepherd Franz, "Psychology and Psychiatry," ibid., 226-229. This was certainly one of the low-water marks in the relationship between psychologists and psychiatrists.


68. Psychologists frequently taught physiology courses. A case in point is that of Shepherd Franz, who taught physiology at Dartmouth Medical School for three years and later became a professor of physiology at George Washington University.

69. See O'Donnell, Origins of Behaviorism, p. 124, where he argues that psychological experimentation was used by the first generation of psychologists to certify their competency as scientists and then abandoned. This was certainly not true of the second generation of psychologists.


74. G. Stanley Hall, "The New Psychology," Harper’s Monthly 103 (1901): 727-732. The maestro of popularization of psychological laboratory apparatus and its uses and benefits to psychology and the public was without doubt Edward Wheeler Scripture, the laboratory chief at Yale. In numerous popular articles and two popular texts, Thinking, Feeling, Doing and The New Psychology, Scripture illustrated (literally) the ever-growing number of instruments available to the scientific psychologist to measure reaction-time, attention, memory, and any number of other psychological processes in humans and animals. He further advertised the benefits that such measurements would bring to society. The New Psychology would, with the aid of such apparatus, be able to transform even the delinquent boy into a "true man." For an example, see E. W. Scripture, Thinking, Feeling, Doing (Meadville, PA: Chautauqua-Century, 1895); idem, The New Psychology (New York: Scribner’s, 1897); and idem, "Cross-education," Popular Science Monthly 56 (1899): 589-96.

75. Rhetoric is used here in a broad sense, meaning all the ways in which language is used to achieve certain effects and to persuade an audience to accept or reject ideas, theoretical positions, or courses of action. I use rhetoric in the sense that Kenneth Burke discussed it in, A Grammar of Motives and A Rhetoric of Motives (Cleveland: Meridian Books, 1962). For a study of rhetoric in another context, see Wade E. Pickren, "A Lamp Unto Our Feet, A Light Unto Our Way."

76. Starr, Social Transformation of American Medicine.

77. See Kohler, Medical Chemistry to Biochemistry, pp. 121-157, for an in-depth analysis of medical education reform and its linkage to the basic medical sciences.
That this was one of the aims of the American Medical Association is suggested in Bevan, "Cooperation in Medical Education," p. 1176.

That this ideal was achieved at other medical schools is unclear. Certainly, the leading schools used the rhetoric of scientific medicine exemplified in this ideal in order to attract the best students, but many did not have full time faculties until after the Flexner report of 1910. For example, in 1909 George Washington University Medical School, where Shepherd Franz held an appointment in physiology, did not have any full time faculty members, see Flexner, Medical Education, p. 201.

There was wide disparity and divergence in the curriculum of medical schools in the first twenty years of this century. In 1895, the Association of American Medical Colleges instituted basic requirements of instruction for its members. An immediate result of these requirements was a decrease in the number of members of the Association. By 1905, the revitalized and increasingly powerful American Medical Association (AMA) issued standards for what it considered an "ideal" medical curriculum. The AMA continued to publish its standards for an ideal curriculum each year in its Journal. For an example, see "The American Medical Association Standards of Medical Education," Journal of the American Medical Association 53 (1909): 544-549.

Nevertheless, it was nine more years before these standards were widely adhered to.


ibid., p. 133.


89. Woodworth, "Psychiatry and Experimental Psychology," p. 34.


91. The symposium papers were published in the *Journal of the American Medical Association* 58 (1912): 909-921. The order of presentation in JAMA was, Shepherd Ivory Franz, "The Present Status of Psychology in Medical Education and Practice," idem., pp. 909 911; Adolf Meyer, "The Value of Psychology in Psychiatry," idem., 911 914; E. E. Southard, "Psychopathology and Neuropathology: The Problems of Teaching and Research Contrasted," idem., pp. 914 916; John B. Watson, "Content of a Course in Psychology for Medical Students," idem., pp. 916 918; Morton Prince, "The New Psychology and Therapeutics," idem., pp. 918 921. It is not clear that this was the order of presentation.


93. It must have been galling for Franz and other psychologists who were committed to psychology as a science to admit that one reason for medicine's interest in their subject was due to such "pseudo-psychology." Psychologists had fought hard to distinguish their science from what they termed quackery. That they had not won the war was evident here and, indeed, is evident today. A visit to any general bookstore will suffice to demonstrate that psychology is lumped together with books on metaphysics, self help, relationships (e.g., "Why Women Love Men Who Hate Them"), astrology, crystals, and pyramids. What this says about psychology is not clear, perhaps the kind of psychology that most members of the public want is not one based on science.

95. Franz, "Present Status."


97. Shortly after the December meeting at which this symposium was held, Watson made a frontal attack on introspectionist psychology in an address delivered at Columbia University. The address was later published as, "Psychology as the Behaviorist Views It," *Psychological Review* 20 (1913): 158-177. Though often cited as creating a maelstrom of reaction and credited with playing a critical role in changing the direction of psychology, the article, and Watson's views, actually received little notice. See Franz Samelson, "Struggle for Scientific Authority: The Reception of Watson's Behaviorism, 1913-1920," *Journal of the History of the Behavioral Sciences* 17 (1981): 399-425.

By 1920, however, the thrust of Watson's ideas had been instrumental in the reorientation of many psychologists toward a practical, measurable psychology and away from a philosophical, mind-centered approach.

98. Watson offered an alternative to this laboratory-oriented three hour course: a lecture course covering the more important topics in psychology with the emphasis on function, not theory. Watson's important topics included fatigue, emotional factors, and the Binet-Simon tests.


105. ibid., p. 180.
106. ibid., p. 182.


110. Walter B. James, "Problems that Mental Disorders Present to the Medical Profession," *Medical Record* 93 (1918): 353.


112. Robert M. Yerkes, "Memorandum on Psychobiological Research, Prepared with Special Reference to the Johns Hopkins University," Yerkes Papers, Manuscripts and Archives, Sterling Library, Yale University, New Haven, CT. Although the memorandum is undated, the correspondence about it dates from the Fall and Winter of 1920-1921.

113. For an example, see Raymond Pearl to Robert Yerkes, 29 November, 1920, Yerkes Papers, Manuscripts and Archives, Sterling Library, Yale University, New Haven, CT.


116. See above, pp. 4-9.

117. For a review of Kraepelin’s experimental work see August Hoch, "Kraepelin on Psychological Experimentation in Psychiatry," *American Journal of Insanity* 52 (1895-96): 387-396. Hoch had been appointed the director of the Psychological Laboratory at the McLean Hospital by Edward
Cowles. Cowles then sent him on a trip to Europe to study with various psychiatric and psychological researchers.


119. See notes 5 and 24.


122. idem., "Effects of Exercise," 239.


126. Wells, "Technical Aspects of Experimental Psychopathology."


128. ibid., p. 437.
129. idem, "Studies in Retardation as Given in the Fatigue Phenomena of the Tapping Test," American Journal of Psychology 20 (1909): 38-59; and, "Motor Retardation as a Manic-Depressive Symptom," American Journal of Insanity 66 (1909): 1-52. In the first article, Wells argued for the superiority of his use of the tapping test as an investigative tool in cases of psychomotor retardation. Comparing his work with that of Hoch and Hutt, he wrote, "the most reliable results are probably to be expected from the test which is intrinsically the most accurate psychological measure," p. 52.


131. Wells, "Motor Retardation as Manic-Depressive Symptom."

132. ibid., p. 3, emphasis added.

133. idem., "The Systematic Observation of the Personality in its Relation to the Hygiene of Mind," Psychological Review 21 (1914): 295-333. Wells’s summary of his insights was published as Mental Adjustments in 1917. In many ways, Wells’s ideas set the stage for the development of personality research by psychologists, as well as providing an impetus to a psychology of adjustment that is still characteristic of most of the psychotherapeutic work of American psychologists.


135. Wells also conducted a great deal of research that was unrelated to his work as a pathological psychologist at the McLean Hospital. For example, see, F. Lyman Wells & C. M. Kelley, "Briefer Studies from the Psychological Laboratory of the McLean Hospital," Journal of Applied Psychology 3 (1919): 172-195. Despite the title, these studies were mostly psychometric in methodological approach.


139. ibid., p. 81.


142. ibid., p. viii.


144. ibid., p. 71.


156. ibid., p. 373.


159. See, for example, Woodworth and Wells, "Association Tests."


164. For a well-reasoned account of the uses of mental testing among those labeled as feebleminded, see Leila Zenderland, "The Debate Over Diagnosis: Henry Herbert Goddard and the Medical Acceptance of Intelligence Testing," in Sokal, Psychological Testing, pp. 46-74. A succinct account of the general role played by Yerkes in mental testing, one not limited to the psychiatric setting, see James Reed, "Robert M. Yerkes and the Mental Testing Movement," ibid., pp. 75-94.


166. O'Donnell cites the correspondence between Yerkes and Hugo Munsterberg as evidence of the pressure exerted upon Yerkes by his department and by the Harvard administration to address more practical and human concerns in his research. See O'Donnell, Origins of Behaviorism, pp. 191-195. The quote is from Yerkes, "Testament," p. 152.


170. Face validity refers to the supposed or apparent content of an assessment device. For example, a depression inventory that asks about depressed mood has face validity. It might not, however, have any value in assessing depression.


172. ibid., p. 819.

173. ibid., p. 819.


175. Robert M. Yerkes, "A Point Scale for the Measurement of Intelligence in Adolescent and Adult Individuals," Boston Medical and Surgical Journal 176 (1917): 564-573. One study that used the Point Scale with adults was Sidney L. Pressey and Luella W. Cole, "Are the Present Psychological Scales
Reliable for the Examination of Adults?" Journal of Abnormal Psychology 13 (1919): 314-323. Pressey and Cole concluded that more work needed to be done if the Point Scale was going to be suitable for testing normal adults.


182. ibid., p. 153.


185. See p. 35.

186. Wells, Mental Tests, pp. 2-3.
187. ibid., p. 1.


189. Edward Cowles, "Research in Pathological Psychology and Biochemistry," American Journal of Psychology 28 (1917): 117-140. This was reprinted from a psychiatric journal.
CHAPTER 2
CONTROLLING SEX IMPULSES: PSYCHOLOGISTS AND THE COMMUNITY OF SEX RESEARCHERS, 1921-1930

Only historical perspective can enable the reader of this story to appreciate the natural reluctance of individuals and institutions in the early 1920s to undertake, or be in any way associated with, studies of human sexual phenomena. In many quarters it was deemed bad taste or indecent to inquire into the phenomena of sexual life and their significance or to speak and write frankly of sexual behavior.

(Robert Yerkes, 1950)¹

Nature herself is not divided into a physical world, a chemical world, a biological world; she is a unit. These artificial distinctions have been introduced for convenience and because of our inability to see the whole field at once. It is as if the whole field of knowledge was originally a desert. When the rain fell on this plot, the water flowed off in streams and rivulets, digging its channel deeper and deeper but leaving the intervening space relatively untouched. The place to dig is now in these intervening spaces.

(Karl Compton, 1927)²

In this chapter I argue that psychologists extended their disciplinary boundaries in the 1920s by work in the new borderland of sex research. One observer noted in 1913 that American psychology was growing most at its boundaries; that remark was certainly true as the 1920s began.³ Psychologists were buoyed by their success in the mental testing of American military personnel during World War I. Their testing effort had received a great deal of publicity and had served to heighten the public and professional...
profile of psychology. Psychologists at the power center of their discipline had gained important roles in the national science scene, e.g., Robert Yerkes at the National Academy of Sciences' National Research Council (NRC) and James R. Angell at the NRC and the Carnegie Corporation. The application of psychological knowledge and methods in education, industry, business, and psychopathology had served to provide an increasing measure of legitimacy for the discipline. As the new decade began, psychologists were poised to extend the boundaries of their discipline. One direction this extension took was in the new borderland of sex research. I wish to demonstrate in this chapter that psychologists seized the opportunity made possible by social concerns about sexual behavior to extend the range of their professional authority; that they did so in two ways: by providing viable "expert" psychological knowledge about sex and by demonstrating that they could be productive initiators, collaborators, and managers of a large scientific enterprise.

The focus of this chapter is the sex research funded by the NRC's Committee for Research in Problems of Sex (CRPS). Issues and questions of sexual behavior, traditionally the preserve of medicine, became a topic of scientific investigation by workers from several disciplines in the 1920s: biologists, anatomists, physiologists, endocrinologists, and psychologists. The impetus that led
to the formation of the CRPS came from a felt social need to understand and control human sexuality and from new forms of institutional support. Sexuality was perceived to lie at the crux of a number of social ills: prostitution, venereal disease, racial degeneracy, and the perceived decline of the family. The change in patterns of American life caused by industrialization and urbanization had profoundly affected family life, including marital relations. A new form of institutional support for sex research came from the Rockefeller Foundation through its Bureau of Social Hygiene. Foundation officers encouraged managed, cooperative research as a long-range strategy to advance human knowledge and ameliorate social ills. The control of "sex impulses and acts" fell under this strategic umbrella. It is important to remember that these social, scientific, and intellectual factors developed against the backdrop of the rationalization of American society that had begun with the Progressive movement in American politics.

My aim in this chapter is to show that the agendas of psychologists, fellow scientists, and foundation managers converged in such a way to form a new borderland of scientific investigation and that psychologists used this convergence as an opportunity to expand the range of their discipline along a shared boundary with medicine. I argue that this expansion is exemplified by the pattern and types of research conducted by psychologists from 1920-1930.
Specifically, I argue that psychologists employed established research strategies in new ways. First, I show that the pattern of collaboration between a physician (psychiatrist) and psychologist already established through the work of Franz, Wells, Yerkes, and others in the years 1904-1920 was employed to explicate norms of sexual behavior. Second, I demonstrate that animal research strategies, already usefully employed by a number of psychologists to elucidate diverse psychological processes, were utilized by psychologists to throw light on basic underlying mechanisms of sexual behavior. Third, I show that comparative approaches informed the attempts of the CRPS to understand sex factors in marital adjustment. Fourth, I demonstrate that the widespread interest in individual differences, with its attendant focus on psychometrics, found expression in sex research as an attempt to explain gender differences in non-intellectual traits.

This new borderland of sex research was not like some unknown continent just waiting to be discovered. Rather, it was a land constructed by social exigencies. In order to provide a context for the expansion of psychology through sex research, I wish to explicate the role of social concerns about sexuality in the century's first two decades in providing an impetus for sex research in the 1920s. A thorough contextualization must also include the changing
climate of scientific research after World War I and the role of philanthropic foundations in bringing social concerns and scientific investigations together.

Introduction

Sexuality and Social Hygiene, 1908-1920

Social pressures and historical events converged in the first two decades of the twentieth century to illuminate a need for a scientific understanding of human sexuality. Human sexuality had traditionally been the intellectual and professional preserve of medical experts. Vern Bullough has noted that "increasingly [by the nineteenth century] sexual discussion was dominated by the medical community" and that every physician was expected to advise his patients about sexual matters. John and Robin Haller have shown that Victorian Americans came to rely on their physicians for advice on sexual problems, as well as treatment for physical conditions. The Hallers argue that physicians came to play the role of priest as well as physician in the lives of their patients and that members of the medical profession were preservers of the moral and social status quo. In the first years of the twentieth century, physicians played a dominant role in determining public discourse about sexuality and were an integral part of calls for social reform of sexual standards. The social hygiene and anti-vice movements were led by physicians, though middle and upper-middle class women also played leading roles. The
emergence of social hygiene societies represented a change in attitudes about sexuality; a change from near total silence and double standards for men and women to one of greater public discussion of sexual matters. Christina Simmons argues that these changes reflected a redeployment of sexuality designed to rehabilitate men's sexuality and undermine the power of women as they moved out of the Victorian order of male-female relationships.  

Chief among the reform issues of the social hygiene movement were concerns about venereal disease and its devastating impact on marriages and children, prostitution, and the double standard of sexual conduct that sanctioned men's use of prostitution. In the 1910s concern about a white slave trade for prostitution also became a part of the social hygiene movement. Birth control became an issue in the same decade.  

Physicians were involved in all aspects of these movements.

Prior to the 1920s, most literature about sex and most sex research was done by physicians. In Europe, the work of a number of physicians addressed questions of homosexuality, fetishism, masturbation, and the relation of sexual "deviancy" to mental illness. Some physicians, such as Karl Ulrichs and Magnus Hirschfield, were apologists for homosexuality. While their writings were available to American physicians, the work of Havelock Ellis was the most widely disseminated medical writing on sex. His research
consisted of the compilation of case histories drawn from literature, historical records, and solicited sexual accounts from friends and their sexual contacts. Though controversial, his work was important in changing American views about sexual behavior in the first years of this century.

Despite the traditional primacy given to medical opinion on sexuality, other voices also spoke to American concerns about sex. A modest, but important, body of psychological insights entered the public conversation about sex. Sigmund Freud, a physician by training, developed a metapsychology that suggested the wellsprings of human behavior were sexual in nature. His work inspired a notable number of case studies and treatises on human sexuality and sparked a growing public interest in psychological explanations for human behavior. In the medical world his ideas, when translated into English, were immediately controversial and were widely discussed. Whether his readers agreed or disagreed with his assumptions, Freud changed the terms of the debate about human personality and made sex salient in ways never before imagined.

G. Stanley Hall, a leading figure in the development of the New Psychology, wrote extensively about sexuality and its role in human development, including the problem of sex education. Dorothy Ross, in her biography of Hall, paints a picture of Hall as a man periodically obsessed with sex.
On the one hand, he could write about the beauty and naturalness of sex in a way that was described by some of his contemporaries as reflecting a morbid obsession. On the other hand, he could be a rigid moralist, warning against masturbation and urging Christian chastity. Ross suggests that Hall's preoccupation reflected his struggles with his own sexuality.11

A major influence on Hall was the writings of Freud. Hall was one of the first American academic psychologists to extend a warm welcome to the theorizing of Freud. Ross notes that psychopathology and sex were the two topics that drew Hall to Freud's work. It was Hall who invited Freud to America for his only visit to this country in 1909. Although Freud and Hall approached the topic of sex from different viewpoints, together they offered a psychological perspective that was influential in shaping public and professional conceptions about sex. Hall's work in particular can be seen as characteristic of the New Psychologists, in that he seized the opportunity provided by the work of social hygienists and others to gain an entree for psychologists into a new field of application. For Hall, sex was a potentially new field of application for psychologists. Several of his students followed his lead into this field.

At the height of Hall's interest in Freudian psychology, he directed some of his students to investigate
Freudian concepts about childhood sexuality in Clark University's new Children's Institute (founded in 1909). Hall established in the Institute a section for the study of "Psychology, Pedagogy, and Hygiene of Sex." Although the Institute was short-lived (1909-1914) and apparently little research was ever done, it reflected Hall's intense interest in the subject.

An earlier group of Clark students had also incorporated an emphasis on sex into their graduate work. Two students interested in religion, James Leuba and Colin Scott, wrote about art and religion as substitutes for sexual fulfillment. A former Clark fellow, Sanford Bell, investigated the development of normal sexual emotions as a way of establishing sexual hygiene guidelines. Lewis Terman, a CRPS grantee in the 1920s and 1930s, supported Bell's conclusion that sexual feelings develop very early in life in his 1905 study on sexual precocity. Other than the contributions of Hall or his students, periodic pieces about sex or sex education written by psychologists appeared in psychological and popular literature from 1900 to 1920.

Despite these early contributions, no consistent program of sex research developed in American psychology until the formation of the CRPS in 1921. Given Hall's interest in sex, it seems appropriate that one of his former students, Earl Zinn, initiated the sequence of events that led to the CRPS and, subsequently, a consistent and enduring
involvement of psychologists in sex research. Zinn's proposal came at a time when larger institutional forces were moving to bring a cooperative approach to scientific research and philanthropic foundation officers were seeking to utilize such research in a wide-ranging program of social control.

Cooperative Research and Philanthropy

In the period just before and after World War I, many scientists advocated the benefits of cooperative research. As Lily Kay has pointed out, "cooperation did not just mean collaboration." Rather, cooperation in science in this era was a joint project by prominent scientists and philanthropy officers to minimize the importance of disciplinary boundaries and to facilitate the application of science to issues of human importance. Contemporary scholars have characterized this long-range project as attempts at social control, social engineering, and human engineering. Prominent among those scientists who called for cooperative science was the astronomer, George Ellery Hale. As a member of the National Academy of Sciences (NAS) and the founder of the Academy's National Research Council, Hale was in a position to promote cooperative research. After World War I, he could cite the benefits of cooperative research to scientists, business, and philanthropies. The National Research Council was organized by Hale in 1916 to facilitate scientific assistance to the government as the
nation prepared to mobilize for war. The cooperation among scientists during the war was of tangible value to the war effort. What perhaps was of more lasting value were the lessons learned about the power of organization of scientific research.

Hale and his colleagues at the NRC were able to use the successes of the NRC during the war to gain large scale funding for a permanent site for the NRC and the endowment of research fellowships for the training of young investigators. But Hale had a vision for the scientific community that went beyond a physical plant and the funding of fellowships. He wanted scientists to collaborate in order to see the "larger relationships." In a number of articles that appeared after the war, Hale and other officers of the NRC touted the benefits of cooperation in research. In an address given to the Royal Canadian Institute and published in Science early in 1920, Hale argued that "the great advance in science that appears to be within reach can not be attained without organized effort and much hard work." Hale acknowledged the fears of many scientists that organization would lead to control, but he sought to allay those fears by stating that organization must be "adapted to the needs of the individual worker, stimulating him [sic] to larger conceptions, emphasizing the value of original effort, and encouraging independence of action."
James Rowland Angell, psychologist and then current chairman of the NRC, wrote a complementary piece that appeared one month after Hale's in the intellectual weekly, The Review. Angell sought to discount the fears of central control over research ideas while promoting the value of cooperative research as a method of increasing productivity and efficiency of scientific work. Angell noted that the NRC was created to promote these objectives. "Organization is the clue," Angell wrote, to ensuring that full use is made of the "intellectual capital" of the nation's scientists. Similar articles appeared in a variety of journals and magazines that reached audiences in industry, science, and the public. It appears that there was a concerted effort on the part of NRC officials to sell their agenda for cooperation in scientific research. Concomitant with the push for cooperation was the encouragement of interdisciplinary research, for scientists from disparate but related fields to collaborate in the investigation of scientific issues that lay on the border between them. What is taken for granted by many scientists today, namely, the need for interdisciplinary research, was a novel and stimulating idea to many scientists in the 1920s and 1930s.

Hale had noted the narrowness and overspecialization of scientists in his 1915 assessment of research, National Academies and the Progress of Research. He thought that
interdisciplinary research would reduce this narrowness and allow greater progress to be made in solving difficult research problems. The experience of World War I had only confirmed this belief, as scientists from different specializations had collaborated to produce results important to the war effort. From the start, then, one of the goals of the NRC was to foster and encourage "borderlands" research projects that necessitated the cooperation of scientists from different disciplines.

This goal was reiterated in 1927 by the Princeton physicist, Karl Compton. Compton noted that the natural world is not neatly divided into chemical, physical, or biological units. He called for cooperation in the investigation of "so-called borderline fields" and suggested that the research that would prove most fruitful was in the "intervening spaces" between established fields of knowledge.

This attitude was institutionalized in the public report of the NRC in the pages of Science in 1935. The chairman of the NRC, Isaiah Bowman, noted that the NRC had historically supported the "advancement of research in the borderlands between traditional scientific fields." He went on to state that "the greatest borderland of all is that between the physical and natural sciences on the one hand and the social sciences on the other." He then predicted work in this borderland as the next great achievement of organized research efforts. The public statements of
scientists like Hale, Angell, Compton, Bowman and others should not be considered as mere position papers designed to obtain approval for their research agenda. Nor should they be considered in isolation. These men and their research agenda were linked to the efforts of philanthropists to build a scientific community for the advancement of knowledge and the amelioration of social problems.

Robert E. Kohler has characterized the relationship between scientists and foundation managers after World War I as demonstrating a shared concern for cooperation in scientific research. According to Kohler, this was a new phenomenon that reflected the re-orientation of American society toward cooperation. Ellis Hawley has called this trend a part of the "search for a modern order." Lily Kay has also noted that "the new scientific enterprise no longer extolled the virtuosity of the individual," but called for a new spirit of cooperation. This new cooperation followed a period of suspicion between the managers of the new national philanthropies and the scientists. As Kohler has pointed out, the scientists were accustomed to very few funds for their research and were also accustomed to obtaining any available funds on an individual basis. With the advent of large-scale philanthropy, there arose a new system of patronage with a concomitant rise in a new system of professional managers of the funds. In the decade following the war, these managers and their clients in science worked
out a new relationship with the primary goal being the
general advancement of knowledge.\textsuperscript{34}

Kohler has argued that the NRC was an ideal
intermediary between academic scientists and the
foundations. The NRC's structure, more a trade association
than anything else, facilitated the flow of funds from the
foundations and gave a certain assurance to the foundation
managers that the funds would be used wisely.\textsuperscript{35} Research
fellowships and cooperative research projects were two of
the methods that were used to facilitate the mutual goals of
the scientists and the foundation managers. While the
advancement of knowledge was the overarching goal, both
parties believed that scientific knowledge was the key to
the amelioration of pressing social problems. One such set
of problems concerned sex.

The development of a community of interest in sex
problems is a complex story of the interplay among
scientists from different fields: physicians, physiologists,
embryologists, anatomists, zoologists, psychologists, and
anthropologists. Among this group psychologists took
leading roles. Psychologists' roles in this enterprise
served to extend disciplinary boundaries in two ways.
Cognitively, psychologists demonstrated that they could
provide useful knowledge about sex. Organizationally,
psychologists demonstrated that they could be productive
initiators, collaborators, and managers of a large
scientific enterprise. The expansion of psychology into this new borderland was interwoven with larger social and institutional issues. The work of psychologists in sex research illustrates the desire to build a new ethos of cooperation to explore the intervening spaces of human knowledge and the desire of foundation managers and scientists to bring to bear the promise of cooperative science on social problems.

The Founding of the Committee for Research on Problems of Sex

In this section, I detail the developments that led to a formal program of sex research and I focus on the role of psychologists in these developments. The official history of the Committee for Research in Problems of Sex acknowledges the role of psychologists in the formation of this Committee, but, in my view, does not indicate how extensive and important psychologists were to this project. Using archival documents, I show that psychologists were instrumental in providing the rationale and initial impetus for sex research. I also argue that they were critical in setting the initial agenda for the research. The role of psychologists in this endeavor reflected their intellectual and social participation in the conservative cultural ideals of post-war America.

The initial impetus for a formal, programmatic approach to sex research was due to the efforts of a psychologist, Earl Zinn. Zinn, a graduate student at Clark University in
Worcester, Massachusetts, had recently completed his master's degree in psychology under G. Stanley Hall. Zinn, like Hall, was interested in human sexuality and sought to interest scientists in conducting sex research. In October of 1920, Zinn was hired by the American Social Hygiene Association (ASHA) as coordinator of questionnaires. The ASHA was one of several "propaganda" organizations that emerged during the first two decades of the twentieth century to combat prostitution and venereal disease and provide sex education to the American public. In this position Zinn spent the next nine months attempting to enlist the interest of scientists in the study of human sexuality.

Zinn was inspired in his efforts by the recent publication of two psychologists, John B. Watson of Johns Hopkins University and Karl S. Lashley of the University of Minnesota. In 1920 Watson and Lashley published the results of a lengthy questionnaire on physician's views of sex and sex education. Watson, the senior author of the survey and a past president of the American Psychological Association, was an advocate of the use of psychology in social engineering. He was also very interested in sexual behavior. Watson and Lashley's study indicated that there was widespread ignorance and disagreement among medical personnel concerning human sexuality.
Zinn was also aided in his efforts by the perception among reform-minded physicians that what was needed to address the sex problem were data that were more objective than the propaganda of the social hygiene agencies. As Thomas Salmon, medical director of the National Committee for Mental Hygiene, wrote to a fellow physician in June of 1921, "There is a little uneasiness in some groups as to whether the amount of information which is distributed [by social hygiene and other propaganda groups] regarding personal, sanitary, and social phases of sex questions is not running somewhat ahead of established facts." He went on to ask for his colleagues support for "approaching this field from the scientific standpoint." Salmon specifically noted the results of Watson and Lashley as indicative of the need for a scientific investigation of sex.

Zinn used the results of Watson and Lashley's survey and the support of physicians like Salmon to promote the scientific investigation of sex. His efforts soon led him to the National Research Council and, particularly, to Robert Yerkes. It was the involvement of Yerkes that was to prove critical for the success of Zinn's efforts.

Initial Efforts to Instigate and Organize Sex Research

When Zinn approached Robert Yerkes about the involvement of the NRC in sex research, he did so as a representative of the American Social Hygiene Association
(ASHA). Zinn's superior at ASHA was Max Exner, author of
Problems and Principles of Sex Education: A Study of 948
College Men. Published in 1915, the book was a survey of
attitudes about sex education. Exner had also developed
ties to the Bureau of Social Hygiene, a Rockefeller-funded
organization led by Katherine Bement Davis, a pioneer in
survey research on women and sexuality. When Zinn made his
initial proposal to Exner concerning a broad scientific
investigation of human sexuality, Exner approached Davis
and, after revising his proposal, Exner and Davis encouraged
Zinn to approach the NRC. Zinn's proposal, as it was
presented to George McCoy, Chairman of the Division of
Medical Sciences, focused on continence, masturbation,
intercourse, sexual aberrations, prostitution, venereal
disease, family and monogamy, divorce, birth control, and
sex education. It was a broad but vaguely defined program.
Nevertheless, Zinn's proposal offered a rationale for
engaging a set of perceived social problems with the
methodology of science.

At the NRC, Zinn found some who were interested. His
superior, Max Exner, in a plea for support to John D.
Rockefeller, Jr., wrote, "He has talked with a number of men
prominent in the National Research Council and they have
verbally indicated their interest and their approval of the
general plan." Among those so interested was Robert
Yerkes. Evidently, Zinn first approached Yerkes in late
fall, 1920. As Aberle and Corner note in their official history of the CRPS, it was fortunate that Zinn sought out Yerkes for help in enlisting the cooperation of the NRC. Once interested in the project, Yerkes devoted significant amounts of time to promoting it. He first sent Zinn to Clark Wissler, then chairman of the NRC's newly formed Division of Anthropology and Psychology. For, Yerkes later wrote, it seemed to him that this division "might properly become responsible for research in sex." Wissler and the division turned down the opportunity, noting "that while psychologists could contribute something to such an investigation, the chief line of approach lies through the medical sciences." Clearly, in Wissler's view, sex was properly a topic for medical investigation. Although, perhaps, Wissler did not want his division drawn into a potentially controversial arena of research. Yerkes, however, continued to feel that Wissler and the division officers "missed an opportunity for usefulness."

Despite the refusal of the Division of Anthropology and Psychology, Zinn and Yerkes persisted. Yerkes next sent Zinn to see the chairman of the Division of Medical Sciences, George W. McCoy. McCoy opposed the plan despite the efforts of men like Thomas Salmon, the prominent psychiatrist and medical director of the National Committee for Mental Hygiene, to marshall support from the medical community for it. With the end of McCoy's tenure as
chairman of the division and the installation of Victor Vaughan, the eminent dean of the University of Michigan medical school, Zinn and Yerkes had better success in gaining a hearing for Zinn's plan. Although at first tentative about the proposal, Vaughan, in the fall of 1921, agreed to sponsor a conference where the feasibility of the plan could be discussed by a larger group of scientists.

Zinn, along with Max Exner and Katherine Bement Davis, had been working for some time to secure funds from John D. Rockefeller, Jr., to initiate Zinn's proposal (which included a generous salary for Zinn). Finally, some time in late June, Rockefeller agreed, with the funds to be channeled through the Bureau of Social Hygiene. The initial appropriation was for $10,000 for one year, with $4000 allocated for Zinn's salary, $2000 for traveling expenses to interview scientific men, as they were called at the time, and $4000 for a conference "to map out a systematic program of research." Once funds were assured from the Bureau of Social Hygiene, Vaughan gave his support to hold a conference to explore the possibility of NRC sponsorship of a program of sex research.

The Conference on Sex Problems, October, 1921

Vaughan, with the permission of the Executive Committee of the NRC, called an exploratory conference for the end of October, 1921. Invitations were extended to a small number of "representative scientists in the related fields to
advise the Council (1) concerning the feasibility of systematic research in this field, and (2) to suggest to the Council the most efficient methods of administration of such a project." Those in attendance included two biologists (E. G. Conklin and L. R. Jones), a physiologist (Walter Cannon), two zoologists (Albert Barrows and Michael Guyer), two psychiatrists (William Alanson White and Frankwood Williams), a toxicologist (Alice Hamilton), and three psychologists (Carl Seashore, Helen Woolley, and Robert Yerkes). Vaughan was present as chairman of the Division of Medical Sciences. Jones, Seashore, and Barrows were present in their official capacities as officers of the NRC (Jones and Seashore as chairmen of the NRC's Divisions of Biology and Agriculture and Anthropology and Psychology, respectively; Barrows as the Executive Secretary of the NRC). Woolley and Hamilton were the only two women invited. Earl Zinn, representing the Bureau of Social Hygiene, also was present.

The transcript of the conference reveals that psychological and anthropological approaches to problems of human sexuality dominated conference discussion. (In the light of this, it is somewhat surprising that the first ten years of research funded by the CRPS were so dominated by the more strictly biological work of the physiologists, biologists, and zoologists.) Apparently, the psychologists had agendas for potential research drawn up even before the
conference and were able to articulate those agendas during the conference. It is worth looking more closely at those agendas.

One week before the conference, Robert Yerkes submitted a memo to Vaughan outlining a program of potential research on sex behavior. In his memo, Yerkes made clear that his first choice of subjects was primates other than man; "infrahuman" was his phrase for these subjects. His research program with the primates was to be as naturalistic as possible and it was to focus on sexual and social behavior. A secondary focus was to be on the role of hormones in sex behavior. A week later, at the conference, Yerkes provided more details about his approach and provided a rationale for it. First, he stated that three different agencies had approached him about applying his psychobiological method to sex research. But, he argued, "the problems of psychological influence [on sex behavior] are so bound up with the physiological that there is necessity for cooperation between differently equipped groups of investigators." His expressed view was that the study of infra-human primates could facilitate greater understanding of human sex problems. Thus, his method would be a viable part of a necessarily cooperative venture.

Carl Seashore, a psychologist from the University of Iowa, also presented what was apparently a prepared agenda. His agenda made a bow toward the physiological (e.g.,
endocrine studies) but the real focus was on the use of anthropological and psychological methods as tools to bring about a more eugenic society. Seashore advocated a scientific approach to the "sex life" that would result in practical benefit to society. Like many other psychologists, Seashore had found that the route to growth for psychology lay in the direction of application. Seashore's remarks illustrate John O'Donnell's contention that the impetus for psychology's growth came from practical application and that the greatest potential for growth came at the boundaries psychology shared with other professions. In his remarks to the conference, Seashore suggested a variety of methods, including biometry, observation, interviews, and psychoanalysis, for a scientific investigation of "sex impulses in normal life." These approaches, Seashore argued, could facilitate the development of a more eugenic society. Eugenics and practical applications were rationales for sex research also offered by other conferees.

William Alanson White, one of the first promoters of psychoanalysis in America, agreed with Seashore that a eugenics approach would be most useful and would have the widest popular appeal. He suggested that a physiological approach, especially one focused on glandular secretions, would not be useful or interesting to a popular audience. Eugenic concerns were also stressed by Vaughan, who
expressed fears about race degeneracy and worried that the English race in this country was going to disappear, and E. G. Conklin, who suggested that criminals and defectives be used as subjects to test methods of birth control and sterility. All these men suggested that practical applications of any potential sex research must be given priority. One gets a sense of urgency in reading this transcript; that the participants were genuinely concerned about social problems and the perceived role of sex in those problems.

The other conferees brought up a range of potential topics and methods: birth control, hormones, sexual habits among "primitives," questionnaires, anthropological field studies, laboratory investigations. Although the discussion roamed over many topics and methods, a common thread throughout the first day's discussion was the need to find a way to control the sex impulse. As Walter Cannon said, "Aren't we going to get light on what we are after, namely, means of controlling sex impulses and acts?" 60

At the end of the day's session, Yerkes warned the group that the NRC was not likely to approve any line of investigation that was overtly controversial, like birth control, or any type of propaganda program. He again urged primate studies as a way of safely investigating sex. Vaughan and Zinn reiterated the importance of scientific investigation if the NRC and foundations were going to
support sex research. Vaughan ended the day by appointing a subcommittee of Cannon, Conklin, and Woolley to review the day's deliberations, make a recommendation as to whether the NRC should undertake the oversight of sex research, and, if so, suggest possible lines of investigation.

When the conference reconvened the following day, Yerkes and Zinn presented a plan for the administration of the research on sex. They did so prior to the report of the subcommittee on whether the NRC should even undertake the oversight of such research. Their plan called for a committee of five scientists with a full-time executive secretary. All were to be paid for their services. This was the plan that was adopted. Walter Cannon presented the report of the subcommittee. The subcommittee recommended that the NRC, as a "body of investigators whose disinterested devotion to science is well recognized," take responsibility for organizing research into problems of sex. The subcommittee suggested eleven problems that might fruitfully be investigated in a scientific manner. The eleven problems, posed as questions, were:

1. Are there changes in sex glands and in other glands with internal secretions associated with seasonal sex activity in lower animals? What is the influence of sex glands on secondary sex characteristics and sex behavior?
2. To what degree is sex behavior modifiable by food, exercise, climate, drugs, diversions?
3. What are the sex habits of primitive peoples, and how are they altered in different stages of civilization?
4. Are there racial differences in sex behavior, i.e., among negroes, mongolians, Malays, American Indians, and inhabitants of the South Sea Islands? What conditions favor its control or inhibition? To what degree is it accompanied by tendencies to repression and by conflicts?

5. How does the sex impulse vary as an experience? What conditions favor its control or inhibition? To what degree is it accompanied by tendencies to repression and by conflicts?

6. To what extent among civilized people is the sex impulse regarded as natural and proper and to what extent as evil and shameful?

7. What variation is there in the frequency and intensity of the occurrence of the sex impulse at different ages and in the two sexes?

8. What are the physiological and psychological effects of masturbation?

9. What are the physiological and psychological effects of sexual intercourse?

10. What are the physiological and psychological effects of continence?

11. What is the differential birth rate among different types in the United States and what does it signify?

The subcommittee recommended that physiological, psychological and anthropological methods be used to answer these questions and that "every variety of subject that might increase our knowledge-infrahuman species, savages, primitive peoples, and normal and pathological human beings at different ages" be studied.

The full conference then approved the motion that the NRC undertake the work. After much discussion of the list of potential problems to be explored and with a general lament over lack of available men to conduct the work, the conferees agreed to let the yet-to-be-named committee use the list as suggestive rather than directive and so moved and approved it. Yerkes remarked that this type of research was by necessity a cooperative venture, requiring the efforts of workers from several fields. Several members
discussed ways in which they could adapt their present work
to include research on problems of sex and they were all
couraged to do so. With that the conference adjourned.

Clearly, the underlying theme of the conference was a
need to impose control on the powerful urges that were
subsumed under the simple label of sex. As Lily Kay has
pointed out, this was part of a general trend, inspired by
American elites, to bring order to all phases of American
life. Donna Haraway has characterized this work as a
project in "human engineering" and noted that the goals of
foundations and scientists converged to reflect "a harmony
of scientific philosophy and social space." Donald
Dewsbury has provided a portrait of Robert Yerkes'
participation in this human engineering project, positing
that Yerkes' work reflected his "commitment to human
improvement." Certainly, Yerkes saw his work as a project
of adjustment, helping individuals "fit into the social
fabric." In this sense, their research must be viewed as
serving a conservative function, that of maintaining the
status of those who were unmarked. The "sex impulse"
presented a potential threat to the stability of American
society. So conceptualized, sex and its urges fit into the
medical model of potential degeneracy and stimulated dreams
of eugenic solutions. Sex, as a problem, was a medical
problem with social implications. The initial impulse of
the conference was to involve science and scientists in the
quest to find a scientific solution under the aegis of medicine.

The Committee on Sex Problems: Setting the Agenda

On November 14, 1921, Walter Cannon, E. G. Conklin, and Robert Yerkes (also named chairman) were appointed as the first three members of the Committee on Sex Problems; Earl Zinn was appointed Executive Secretary. Their first meeting was called and held on December 12th in the offices of the NRC in Washington, DC. The Committee agreed to ask Thomas Salmon, the medical director of the National Committee on Mental Hygiene to be a member and discussed the importance of having a woman serve on the committee as well. By the next March, Salmon, the zoologist Frank Lillie, and Katharine Bement Davis had been added to the committee. It was noted that a number of prominent scientists had expressed interest in cooperating in the venture, but that there was a need for a comprehensive plan to systematically investigate sex problems. In an attempt to meet this need, each member of the committee agreed to outline a program of potential research problems. Evidently, only two committee members (Robert Yerkes and Frank Lillie) initially submitted research agendas. Aberle and Corner note that the Committee saw the possibility of dividing the work into "anatomical and physiological, anthropological, psychological, and sociological aspects of sex." At this point, however, as the committee began its work, the focus
was still on the investigation of human sex problems. (The first preliminary report of the full committee, dated March 15, 1922, contained the same list of suggested research topics as originally endorsed by the exploratory conference the previous fall.) Over the next two years that was to change to a focus on the underlying biology of sex.

The Yerkes plan

Robert Yerkes submitted a proposal for a program of sex research for the consideration of the Committee in late summer of 1922 (see Appendix A for a copy of Yerkes' research agenda). He began his proposal with an argument for the need to stay close to human problems. It was human sex problems that justified the existence of the committee. Yerkes focused on psychobiological research with primates other than man as the subjects. He argued that this approach held the most potential for the understanding of human sex problems. "Of the mammalia, the primates manifestly have peculiar significance in connection with our particular interests and responsibilities. Concentration on the human subject might be highly wasteful at this time." Primates, such as chimpanzees, orangutans, and gorillas, could be studied in captivity or in the wild as a way of gaining insight into sex behaviors as diverse as masturbation and family relations. Yerkes' inclusive list of subjects for study with primates was well planned. He wanted studies of climate and food supply, as well as
inquiries into traditional psychological topics such as memory and imagination. Yerkes' plan reflected his orientation to the whole organism, to what Dewsbury calls the historical tradition of psychobiology, its focus on the "splendid complexity" of organisms.75 Certainly, Yerkes did not take a reductionistic approach to the study of sexual behavior in primates or its potential usefulness in understanding human sexual behavior.

The other main focus of Yerkes' proposal was the organization of the Committee and its relationship with the NRC. Yerkes was dissatisfied with the administration of the Committee, even though its administrative procedures reflected his and Zinn's suggestions as to how the Committee should be organized. What Yerkes preferred was a Research Director, in charge of a full-time staff that would coordinate and conduct sex research. Yerkes argued that research could best be conducted if limited to a small number of research centers, preferably located in universities. He refused to rule out the need to invest in the individual investigator, although he wanted such subsidies carefully monitored. What is evident in Yerkes' discussion of the Committee's organization and administration is his frustration with the slow and cumbersome process of getting a comprehensive program of research underway.
The Yerkes plan was essentially to focus sex research on problems that could be investigated in a scientific manner and that would throw light on human sex problems. It also called for greater independence of the committee from the National Research Council. Although Yerkes acknowledged the importance of research into the basic biology of sex, he argued for a more holistic focus that employed primates as the closest analogy to human sexual behavior. The research agenda outlined in his plan was not as fully funded as the agenda set by Frank R. Lillie.

The Lillie plan

The appointment of Frank Lillie to the Committee on Sex Problems in January, 1921, proved to be a critical decision for the direction of the Committee. Lillie came to the Committee already well established in sex research. Lillie, with a PhD in zoology, spent most of his career investigating the biology of sex at the University of Chicago. He served as chairman of his department from 1910 to 1931 and dean of the Division of Biological Sciences from 1931-1935. By the time of his appointment to the Committee on Sex Problems he had already published important research on the role of sex hormones in the embryological development of cattle.

Aberle and Corner argued that Lillie was better prepared to state the case for an emphasis on research in the biology of sex than the other committee members were for
their fields. They reprinted his plan as one of the appendices of their official history (see Appendix B for a copy of Lillie's research agenda). Certainly, Lillie's research agenda was comprehensive and well conceived. It also held less potential for controversy than the Yerkes plan. Perhaps more importantly, it was doable with the methods and approaches then extant. Adele Clarke has recently pointed out the importance of doability in the emergence of the reproductive sciences. By doability, she means that the researchers must be able to pull together physical and financial resources and articulate methods that will work in available situations and with available resource materials. Lillie was already "networked" with others who were involved in reproductive research and knew the capabilities of those researchers and what resources were available and what was needed. Thus, he was in a better position to formulate a plan in terms that would be attractive to those who held the resources that would make the research possible, the Bureau of Social Hygiene.

His plan, as reprinted by Aberle and Corner, was a model of economy and conciseness. Beyond its economy, it was also a comprehensive research agenda. Lillie's plan encompassed all of the relevant and doable issues within his field and defined the borderlands as well. Lillie framed his agenda as basic to the other lines of investigation. That is, biology was assumed to be foundational for the
elucidation of psychological and sociological aspects of sex. The Committee and its sponsor acknowledged the vastness of the topic and the paucity of available knowledge. In the face of such large questions, Lillie’s plan made it possible to begin sex research. His conciseness framed the work and made it appear doable. His specificity indicated where fruitful lines of investigation might be initiated. It can be argued, from a comparison of the Lillie and Yerkes agendas, that Lillie’s offered what Yerkes’ did not, a concise approach that would illuminate the basis of sex and ultimately throw light on an important arena of social problems.

The Lillie plan was the one that was most fully implemented. Psychological investigations, whether of the type called for by the Yerkes plan or of the type that was the original focus of Zinn, Davis, and the exploratory conference, received significantly fewer funds than "basic" biological research. (See Table 1 for a comparison of funding for the two areas.) Two factors that contributed to this funding differential were the potential controversy that might have accompanied human sex research and the perceived lack of qualified investigators. Psychologists moved to fill this perceived void and in so doing initiated a line of research that has endured to the present.
Table 2.1 Comparison of CRPS Funding for Biological, Comparative, and Human Sex Research, 1922-1930

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<th>Fiscal Year</th>
<th>Amount</th>
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Source: Aberle and Corner, Twenty-Five Years of Sex Research, pp. 114-131.

Psychological\Psychobiological Approaches to Sex Research

Although human sexuality was perceived as within the medical purview, few physicians were prepared to do sex research. On the one hand, this resulted in the rapid expansion of American biology into basic sex research: sex physiology, endocrine functions, etc. On the other hand, this also presented an opportunity for expansion of American psychology. Just as the opportunity provided by education and advertising in the previous thirty years had resulted in disciplinary expansion, so this convergence of societal and scientific interest in controlling sex also provided an opportunity for the expansion of psychology's boundaries. Psychologists moved into the new borderland via "psychobiology." Psychobiology held multiple meanings, but in general, referred to an approach that considered the organism in its environment and was not exclusively reductionistic in its rationale or methods.
Psychobiological research encompassed both animals and humans.  

Psychobiology also held rhetorical advantage. That is, it provided cognitive linkage between psychology and medicine by its linguistic overlapping of psychology and biology. Robert Yerkes used the term in 1921 to draw attention to the potential usefulness of psychobiology to medicine. He argued for "pioneering in psychobiology as a basic medical science" and suggested that psychobiology would aid the physician by informing him of the "function and structure of the human mind." Yerkes' influence and use of the term can be traced in the project on the "Psychobiology of Sex in Man" begun under the sponsorship of the CRPS in 1924.

A close look at the human sex research conducted under the auspices of the CRPS in the 1920s reveals three main avenues of disciplinary expansion by psychologists that served as prototypes for later sex research. One avenue utilized the cooperative approach: psychologist and physician working together in the pattern established in the first two decades of the twentieth century and detailed in the previous chapter. A second avenue was the comparative approach: psychologists working in the tradition of animal research to elucidate patterns of sexual behavior that might illuminate human sexuality; in sex research this also served the purpose of further legitimation of the comparative
approach. A derivative of this approach actually formed the centerpiece of human sex research in the 1920s and illustrates the blurring of disciplinary and professional boundaries: the work of a physician trained in the comparative approach and attempting to apply that approach to human sexual adjustment. The third avenue was the individual differences approach: psychologists did sex research designed to explain differences in human abilities based upon sex.

The Cooperative Approach: Human Sex Research

A critical impetus for the formation of the CRPS and its program of sex research was the publication in 1920 of John B. Watson and Karl S. Lashley’s lengthy questionnaire on physician’s views on sex and sex education, as noted above. Watson and Lashley received a sizable grant from the U. S. Interdepartmental Social Hygiene Board (ISHB) in 1919 to assess the value of several films then in use to combat venereal disease. The Interdepartmental Social Hygiene Board had been created in 1918 by an act of Congress to fight the spread of venereal disease among soldiers by civilian women. The paucity of sex knowledge and the widely divergent views among physicians about sex revealed by Watson and Lashley’s study added to the felt need among many reform-minded physicians and scientists that a scientific investigation of sex was needed. Watson and
Lashley's method, the questionnaire, also became an important tool in much of the early human sex research.

The Bureau of Social Hygiene (BSH) provided the funds for the work of the CRPS during the 1920s. The Bureau was the creation of Rockefeller philanthropy and had as its main mission the cause of social hygiene through sex education. At the time of the inception of the CRPS, the Bureau's director, Katharine Bement Davis, had begun questionnaire research on college women's attitudes about sex and sex practices.84 The initial conference on sex problems held in the fall of 1921 gave priority to the investigation of human sexuality and recommended the use of questionnaires.

The first recipients of funds for the study of human sex practices were physicians who proposed the use of questionnaires to gather data. The physicians, Roger I. Lee of Harvard and Adolf Meyer of Johns Hopkins, sought to determine norms of sexual behavior. Lee's proposed subjects were college undergraduates; Meyer's were medical students. Adolf Meyer

Adolf Meyer received a grant from the CRPS in 1922 for a "collation of replies to a sex questionnaire to medical students and other persons, and organization of detailed observations of the sex life of selected groups over a period of time."85 Although it is probable that Meyer collected the data, he never saw fit to publish them. It may be possible to ascertain what his questionnaire might
have looked like. Meyer outlined his method of training first year medical students in a paper published in 1938. One task of new students was to conduct a self-study and study three classmates. Meyer suggested the topics this biographical study should cover. Prominent among them was "sex development and patterns." The specifics of this section of the self-study included questions concerning first interests in sex, sexual stimuli, sex education, masturbation, and relationship of sexual feelings to attitudes about having a family. The five subtopics in this section cover a wide range of sexual material and concludes by inquiring whether the student would be willing to cooperate in a confidential study with Meyer or one of his coworkers. 86 Even though we do not have any published record of Meyer's sex research, it is certainly true that he wrote more extensively about sex from the early 1920s to the late 1930s than he had before; a period that covers the time he was receiving grants from the CRPS. 87

Roger I. Lee

Roger Irving Lee was Professor of Hygiene at Harvard and assistant chief of the medical service at Massachusetts General Hospital at the time he was funded by the Committee. The Committee allocated $1000 for Lee's research in 1922-23 and $1750 in 1923-24. This was a substantial portion of the funds allocated for human research in these years. The Department of Hygiene worked closely with the athletic
program in a service capacity to the students of Harvard College. In this capacity, Lee was able to obtain the funds from the CRPS to assess the sex life of students at the college. Although Lee was the grantee, he did not do the actual research, leaving that to two colleagues at Harvard.

Lee's original proposal was to determine the range of sexual experiences among "normal" college students. The original sample of 250 men was drawn from the Graduate School of Business Administration. The men who participated were all volunteers and were asked to complete a lengthy questionnaire about their sexual practices, sex education, and beliefs about sex. A second facet of the research was a voluntary interview with one of the investigators, where the student was asked about a wide range of topics, from physical health to political beliefs.

Lee was apparently little involved in the actual research, preferring instead to recruit others to do the investigative work. He chose from Boston Psychopathic Hospital the psychoanalytically-inclined Chief of the Outpatient Department, Martin Peck, and the new Chief Psychologist of Boston Psychopathic, Frederic L. Wells. Martin Peck was a psychiatrist who had been psychoanalyzed by Otto Rank in 1921 and who became a leader in Boston psychoanalytical circles. Peck was later described by his colleagues as modest, gentlemanly, "gentle Peck, a man without enemies," and was known for great integrity in his
person and work.\textsuperscript{91} Wells had accepted the position at Boston Psychopathic Hospital after many years at the McLean Hospital. He was well known among psychologists and psychiatrists for his psychiatric research (see Chapter 1). After Lee resigned his position at Harvard in early 1924 over a squabble concerning the proposed merger of the Department of Hygiene and the athletic program, the work was then left entirely to Peck and Wells, who received one more grant of $2250 for the fiscal year 1924-25.

\textit{Martin Peck, Fred Wells, and the study of human sex problems in Boston}

The sex research of Peck and Wells was part of an interest by Thomas Salmon and the CRPS in organizing centers for the research of "psychobiology of sex in man." Thomas Salmon had forged a career in psychiatry that included an appointment with the United States Public Health Service, where he established a psychiatric screening program for the immigration authorities on Ellis Island. In 1912 he was appointed the director of special studies for the National Committee for Mental Hygiene; in 1915 he became its Medical Director, a position he held until January, 1922. Appointed to the CRPS in the same month, he was the committee's sole psychiatric member until his death in 1927.

It fell to Salmon to devise a program of research into human sexuality for the CRPS. He did so at a time when the Committee members were arguing among themselves about a reorganization of its work: whether the work should be
placed under the direction of a few individuals at a few select research centers (Lillie and Yerkes) or continue as they began (Zinn). There was also sentiment at this time for a separation from the NRC and for direct funding from the Rockefeller Foundation. Salmon's proposal reflected the desire by Lillie and Yerkes for concentration of Committee funds in a few research centers.

Salmon attempted to initiate a broad-based program of human sex psychobiology centered in Boston and using the facilities of Boston Psychopathic Hospital, Harvard Medical School, Harvard University, and the Judge Baker Foundation. In January, 1923, Salmon called a conference to discuss the feasibility of such a center. The meeting was held at the home of Roger I. Lee and resulted in the establishment of a local committee composed of Walter Cannon, C. Macfie Campbell, Lee, William McDougall, and D. A. Thom. This committee was charged with formulating a plan of organization and research for a Boston Center for Study of Human Sex Problems. Although the local committee met again to discuss the proposed center, by March 6 Earl Zinn had concluded that they were going to be unable to agree on any plan for a center. As Zinn wrote to Cannon, "There is a manifest desire to coordinate the work to the extent that this can be accomplished by occasional conferences between investigators, but the idea of centralization of all the investigations under the direction of a single individual
has not been favorably received." Zinn included in his report the research suggestions made by the various members of the local committee. They included research on the sexuality of delinquent girls, study of sex problems of preschool children, case studies on individuals referred by the courts to the Judge Baker Foundation, and the continuation of the work on sex norms of college men begun by Lee. The total dollar amount necessary to fund these projects was $20,000; the CRPS had proposed $10,000 to cover all the work of the proposed center. The result of the discrepancy between the desires of the CRPS and the interests of the local committee in Boston was the tabling of the proposed center. Instead, the CRPS voted to continue the support of Lee's investigation into the sexual norms of college men.

Once the sex research was left entirely in the hands of Martin Peck and Fred Wells, they were able to garner funds from the CRPS for one more year after Lee's resignation: $2250 for the fiscal year 1924-25. They continued their investigation into the sex life of college men and expanded their work to include inquiries into homosexual practices and the correlation of sexual attitudes with personality. Peck and Wells were both interested in dynamical theories of personality. Psychoanalytic ideas had found a more congenial home among physicians and psychologists in the Boston area than in any other American locale in the first
three decades of the twentieth century. The work of Peck and Wells reflected this dynamic orientation as they sought to explain the sexual practices and preferences of their subjects. In both their orientation and their method, then, Peck and Wells reflected the wishes of initial conferees more explicitly than any other researchers in the first decade of the CRPS. Their aim of establishing norms also reflected the desire of the initial conference members to find a way, as Cannon said, "to get light on what we are after, namely, means of controlling sex impulses and acts." The work of Peck and Wells, then, can be placed within the broader perspective of social control so sought after by scientific and industrial elites.

The subjects in Peck and Wells's research were asked to answer a questionnaire about their sexual attitudes and activities. In addition, Martin Peck interviewed those college students who volunteered to come to his office. While sensitive to the privileged social position their subjects occupied, Wells and Peck saw in them a way to investigate the development of sexual interest and practices among "normals." Following the lead of Watson and Lashley's research on medical views of sex education, Peck and Wells asked a range of questions designed to elicit information about their subjects' type and variety of sex education, the development of interest in sex, masturbatory practices, initiation and frequency of heterosexual intercourse,
incidence of venereal disease, and efforts at continence. The main foci of attention were the frequency and duration of masturbation and sexual intercourse.

Peck and Wells published their findings in a series of articles that appeared from 1923 to 1927. They placed their findings within a normative framework; that is, their subjects were compared to what was considered normal by mental health professionals. Their conclusion that about 10% of the male college population were homosexual led them to call for intelligent psychiatric management of homosexuality in this population; that is, psychiatrists must act in a preemptive way in order to prevent neuroses from developing and to promote mental hygiene. Martin Peck also made attempts at intervening in the lives of those who expressed homosexual preferences. He wrote of "curing" and of trying to help the person try to understand that his "defect" was psychobiological in nature.

There were two social issues that informed Peck and Wells's interpretation of their research findings. The first was the need to marry and have children. Given the eugenic fear of the depletion of good stock in America, as evidenced by the low birth rate among the "better class of people," the need to prevent homosexuality or cure it was a legitimate concern among many psychiatrists. The second issue, strongly related to the first, was the necessity of preempting the formation of a homosexual identity among
these sons of the privileged. Historians of sexuality have argued that in the late nineteenth century sexual acts, for the first time, became definitive of identity.98 Seen in this light, Peck's interventions were aimed at reorienting these young men into an acceptable sexual identity.99 Along with these social issues, was the professional issue so finely delineated by Elizabeth Lunbeck: sexual practice was among those practices that fell within the sphere of the "psychiatric persuasion." Psychiatrists sought to increase their domain of influence in American life by bringing private acts within that domain. Sexuality, heterosexual and homosexual, was included.100 Siobhan Somerville has recently suggested that the medical profession's redefinition of homosexuality in this period was part of a scientific racism toward difference. He argues that physicians were able to do so because they shared in the cultural authority of science and so were authorized to make such definitions.101 Peck's work can be seen as an example of the extension of psychiatric hegemony and the redefinition of sex in the service of social control.

The work of Fred Wells with Peck illustrates how psychiatrists and psychologists collaborated on issues of mutual interest. Their joint efforts were a continuation of the pattern begun at the end of the nineteenth century and continued in the twentieth century by men like Boris Sidis and Ira Van Geison at the New York Pathological Institute,
Shepherd Franz and William Alanson White at the Government Hospital for the Insane, Robert Yerkes and E. E. Southard at Boston Psychopathic Hospital, and Wells with several psychiatrists when he was still at the McLean Hospital. Thus, the collaboration of Peck and Wells was part of an interdisciplinary tradition at Boston Psychopathic Hospital and part of the pattern of cooperation between psychiatrists and psychologists.

Psychiatrists were the dominant members of such partnerships, offering clinical diagnoses, intervention, and interpretation, while psychologists provided experimental and psychometric expertise. Southard encouraged collaborative "team" work among the psychiatrists, psychologists, and social workers on his staff. Team efforts continued at Boston Psychopathic after Southard's death in 1920. The organization of work at the Hospital was detailed by one of its medical officers, W. Franklin Wood, in 1924. Included in Wood's organizational chart were psychiatrists, psychologists, social workers, occupational therapists, a dentist, a roentgenologist, and nurses. John Burnham has suggested that this type of collaboration became more common after World War I as members of the medical profession, especially psychiatrists, attempted to meet an increased demand for mental health services. The complexity of mental health and illness suggested that there were many etiologic factors. Collaboration and cooperation
also reflected the emergence of a holistic, organicist viewpoint among many scientists in the period between the wars; Cross and Albury have called this the rise of "organic analogy" and have contrasted it with the older reductionist model.\textsuperscript{104}

Peck and Wells's collaboration, then, was part of their institution's policy. Peck, as noted above, provided the authoritative interpretation of the college students' behavior while Wells provided a way to organize and make coherent data that were unusable to Peck otherwise. But Wells had his own agenda as well. As Chief Psychologist at Boston Psychopathic, Wells was strategically placed to utilize the cooperative approach to extend his discipline's boundaries. He bolstered his own credentials, furthered the cause of "clinical psychology," and articulated a meaning of human sexuality from his disciplinary perspective in the new borderland of sex research. His work fits Burnham's thesis that psychiatrists sought the help of non-medical experts when the demand for psychiatric services exceeded the supply or when non-medical professionals offered expertise outside the professional armamentarium of the psychiatrists.

Wells extended the range of psychological practice through his interpretation of the questionnaire data in three articles published from 1925 to 1927.\textsuperscript{105} In these articles Wells went beyond the role of handmaid to psychiatry to offer his own view of the role of personality
in human sexual practices. For example, Wells employed Jungian typology and correlational techniques to make the rather unorthodox observation that extroverted personalities were more likely to have been sexually active before marriage and to be more satisfied with their lives than introverted personalities. Rather than employing conventional standards derived from mores and class bias, Wells wrote from the perspective of his psychometric data. Sex instruction, he concluded, had the effect of spreading libidinal energy into healthy sexual expression rather than confining it in a neurotic manner.

Wells also noted that there were so many differences among individuals in sexual expression that conventional teaching concerning sexuality was nearly useless and was certainly misinformed. In this, he anticipated Alfred Kinsey, another CRPS grantee. Wells cited four "typical" cases, each illustrating a type of sexual adjustment. The most remarkable case was that of a highly intelligent woman of 18 who had become pregnant after a period of sexual activity that had begun at age 10. Wells described her as exhibiting the kind of adjustment that any man might wish "his own daughter to exhibit." Wells' suggested that psychological investigation of sex offered hope for a "constructive eugenics" that might result in the psychological progress of humanity.
Both Peck and Wells sought to make their disciplinary and clinical expertise useful to the understanding of human sexuality. In an era when little was understood about the psychological underpinnings of human sexual behavior and when there were few empirical results to illuminate sexual practice, Peck and Wells made a beginning toward the establishment of an empirical foundation. In doing so, they brought with them their disciplinary agendas and the moral and social agendas of the cultural mainstream. Human sexuality was a borderland, with very ill-defined borders. It is useful to see Peck and Wells's work as part of a redeployment of attitudes about human sexuality that both altered and reflected their changing era. When the CRPS ended their funding after the fiscal year 1924-25, a start had been made toward a delineation of the boundary lines of human sexuality. Peck and Wells' collaborative efforts were part of a new tradition, of which Wells was a founding member, that extended the dialogue of psychology and medicine by providing evidence that both psychiatry and psychology had unique contributions to make to the understanding of human sexuality. Their collaboration reflected the complexity of sexual behavior; no one discipline could fully explicate all the relevant factors in human sex behavior.

In terms of extending the disciplinary boundaries of psychology, Wells demonstrated that psychologists were ready
to do more than provide psychometric expertise to physicians. His papers on human sexual practices and attitudes were informed interpretations that went beyond psychometrics to cogent suggestions about the viability of sex education, the diversity of sex practices, and the role of sex in adjustment. In this, he anticipated the conclusions of Alfred Kinsey; a fact noted by Kinsey himself. Beyond the cognitive contributions of Wells is his example of vital collaboration with a physician. Wells wrote the two reports that appeared under both his and Peck’s name; he was more than a technical adjunct to the project.

Wells’s example was followed by later investigators. The oft-noted work of the gynecologist, Robert L. Dickinson, and his psychologist collaborator, Lura Beam, followed in the tradition of Peck and Wells. Dickinson provided the data and the umbrella of medical authority; Beam provided the statistical analyses and offered an interpretation of the data.107 Frances Strakosch did her dissertation research on sexuality of the women who were patients in the New York State Psychiatric Institute and Hospital.108 Carney Landis, Research Psychologist at the New York State Psychiatric Institute, published two correlational studies of psychological and medical data gathered in a medical setting; both studies were supported by monies from the CRPS.109 While it would be an oversimplification to assert
that Wells shaped the pattern of cooperation and investigation of future psychologists, it is not too much to state that his work on both levels was exemplary for those who followed. Wells has not been appropriately appreciated for his influential role in shaping clinical research and practice. Two psychologists of note who worked under his direction and acknowledged his influence were David Shakow and Gardner Murphy. Wells’s sex research extended the cooperative model in a new direction and facilitated the extension of psychological boundaries into new areas of practice and cognition. The work of Wells was important, then, in furthering an established approach of cooperation into a new borderland.

The Comparative Approach to Sex Research

Donald Dewsbury has noted that "comparative psychology has always been a small part of the total psychological endeavor." Nevertheless, comparative psychologists made important contributions to sex research and were major recipients of CRPS grants during the 1920s. Their work served to extend the boundaries of psychology in new directions. I begin this section with a definition of comparative psychology, then proceed to outline some professional difficulties encountered by comparative psychologists in the first two decades of this century, and then move to a consideration of the readiness of some
comparative psychologists to participate in the shaping of the new borderland of sex research.

My main focus in this section is the CRPS-funded sex research of Calvin Stone, Robert Yerkes, and their students. I argue that Yerkes' work as a science bureaucrat served the interests of comparative psychologists well in securing funding from the CRPS. Yerkes demonstrated leadership that signalled new possibilities and strengths for psychology in the new era of cooperation in science that began after World War I. Yerkes also directed sex research by his students that provided an ideological basis for the effort to bring human sexuality under the control of social engineering. Stone initiated a program of research that crossed disciplinary boundaries and exemplified the cooperative ethos favored by foundations and the NRC. I argue that Stone was the first important sex researcher among psychologists; the first to initiate and sustain an ongoing program of sex research. The funding he received from the Committee was critical for the sustenance of his program. His work extended the cognitive understanding about sex, while it also served to support the agenda of social control pursued by scientific and cultural elites. Lastly, this section will set the stage for the comparative approach of G. V. Hamilton in the study of the psychobiology of sex in humans.
Dewsbury has defined comparative psychology as "the area in psychology concerned with the evolution, function, immediate causation, and development of behavior in a variety of species." He notes that these were all emphases of comparative psychologists in the first years of this century. While the comparative approach never dominated American psychology, many of the central figures of the discipline did comparative research at some point in their careers. For example, men and women like G. Stanley Hall, Edward Thorndike, Margaret Floy Washburn, John Watson, Robert Yerkes, Karl Lashley, and Calvin Stone all did comparative work and also had in common the honor of being presidents of the American Psychological Association. Perhaps what attracted these leaders was what Dewsbury has identified as a cardinal attraction of comparative psychology: its broad perspective of behavior. In fact, Dewsbury has recently argued that comparative psychology offers the broadest perspective of behavior of all the fields of psychology. He wrote, "Students can locate themselves in the broader matrix of all living things and appreciate the similarities, differences, and complex patterns of interaction among living things." Despite its attractions, comparative psychology had its professional problems in the first two decades of this century.

The chief difficulty encountered by individuals desiring to pursue a career in comparative psychology in the
first two decades of this century was finding a job. O'Donnell and Dewsbury have separately detailed this
difficulty and the career responses of comparative
psychologists. Dewsbury noted that the accomplishments of
comparative psychologists were recognized as important but
peripheral to the mainstream of the discipline. One result
was that young graduates, upon whom the future of the field
depended, were unable to find jobs and left comparative
psychology for other fields, especially education.

O'Donnell has chronicled the career difficulties of
Watson and Yerkes. In their correspondence with each other,
both complained of administrative pressures to pursue
applied interests. Both Watson and Yerkes also struggled
with their professional identity, writing to each other that
they were unsure where they fit: physiology, biology, or
psychology. Yerkes retrospectively stated that in his
career during this period, he might have easily been
classified as a physiologist. Yerkes was under a great
deal of pressure to pursue educational applications at
Harvard. Although he did offer a course in educational
psychology at Harvard, Yerkes resisted becoming identified
with educational applications. He eventually found a way to
satisfy administrative demands by pragmatically accepting a
half-time position at the new Boston Psychopathic
Hospital. Watson eventually turned to work with human
infants in the few years just before his academic career
ended. Other psychologists trained or interested in the comparative approach had similar experiences. In light of these professional problems, Dewsbury has called it a "miracle" that "so much quality work was completed" in comparative psychology.\textsuperscript{118}

In spite of these difficulties, a core of comparative psychologists was strategically placed to benefit from the new trends of organized science and the influx of foundation money that occurred after World War I. Dewsbury has shown that, contrary to received histories of comparative psychology, the field actually solidified and grew in the years after World War I. Comparative psychologists, he pointed out, "secured the position of the field from disruption in a manner not accomplished by the cadre that existed prior to World War I. They secured tenured faculty positions, benefited from the development of Federal funding programs, and prospered."\textsuperscript{119} One who was in a position not only to benefit from the changes but also to shape their pattern was Robert Yerkes. His work as a science bureaucrat was important for his field and for sex research.

\textbf{Robert Yerkes in Washington}

By 1921, Robert Yerkes had become an important person with whom to deal in scientific circles. He was head of the NRC's Research Information Service and well-connected with business and social elites.\textsuperscript{120} Yerkes, who had recently begun to call himself a psychobiologist rather than a
psychologist, had enjoyed a productive and respected career in the study of animal behavior.\textsuperscript{121} Academic exigencies had led him into clinical work at the Boston Psychopathic Hospital, as noted above and detailed in the previous chapter. His wartime work on mental testing with the armed services, work which grew out of his experience at the Boston Psychopathic Hospital, has thus far been a major focus of historians who have written on his career.\textsuperscript{122} Yerkes recounted psychologists' role in the wartime mental testing enterprise in \textit{Psychological Examining in the United States Army}, published under the auspices of the National Academy of Sciences.\textsuperscript{123} However, Yerkes' real love was the study of animal behavior in an evolutionary context, with an emphasis on comparative analyses. Although his comparative research included crows, pigs, fish, turtles, and a dancing mouse, he had come to focus on primates other than man as his main research subjects.

Zinn's proposal for research on problems of sex came at a time when Yerkes was searching for an opportunity to return to comparative research. Certainly, he was interested at this time in exploring the possibility of fulfilling his "dream:" the establishment of a primate research laboratory. At the time, Yerkes sounded out Adolf Meyer, the prominent Johns Hopkins psychiatrist, about the feasibility of establishing a "Professorship of
Psychobiology" in the Johns Hopkins Medical School, with an emphasis on primate research.\textsuperscript{124}

Yerkes had stayed on at the National Research Council after the war as chairman of the Council's Research Information Service. He stayed despite his prior agreement to head a reorganized psychology department at the University of Minnesota. Yerkes later wrote that one chief reason for staying in Washington was "because the chances of furthering my plans for anthropoid research and securing financial backing for them would be better in the east than in the midwest."\textsuperscript{125} His dream of a primate research center was evidently on his mind even as he became involved in the Committee for Research on Problems of Sex. His years spent in Washington making connections and "organizing science" made him well suited to help Zinn spark interest among scientists in conducting sex research. These connections and contacts also served him well as he worked to achieve his dream of working with primates.\textsuperscript{126}

Beyond his personal dream, Yerkes as Chairman of the CRPS pushed for funding of comparative psychology in sex research. His justification of animal research, that it was the best way to understand human sexual behavior, helped garner resources for sex research by other comparative psychologists in the first years of the CRPS. (In the second half of the decade, Yerkes himself received significant funding for sex research.) Yerkes had many
connections among comparative psychologists. An important connection proved to be the biologist/psychologist Karl S. Lashley.

Lashley, a native of West Virginia, had obtained his PhD in genetics with H. S. Jennings at Johns Hopkins University in 1914. Before and after his degree he worked closely with John B. Watson. He spent the summer of 1913 with Watson doing comparative work with terns. As we have seen, he and Watson collaborated on a project to evaluate the effects of venereal disease films on soldiers. This project led to their stimulating questionnaire on sex education views of physicians, the results of which were a vital impetus for the initiation of a scientific investigation of sex. Lashley took a position at the University of Minnesota in 1917, although his service there was interrupted by the war and the work with the United States Interdepartmental Social Hygiene Board. He finally returned to Minnesota in 1920 and stayed until 1926. After the founding of the CRPS, he was asked by Yerkes to help develop a program of sex research in psychobiology. When the Committee began making grants, Lashley was one of the first recipients. He received $3500 in 1922-23 for research on "sensory motor and glandular components of sexual behavior in the rat." The Committee allocated another $2000 the following year, but Lashley declined the money. Lashley never became a major recipient of Committee funds,
but he was highly interested in sexual behavior. His 1924 article entitled, "Physiological Analysis of the Libido," was a powerful statement of the need to tie theorizing about sexual behavior and interests to demonstrable physiological functions. In the article he frequently cited the work of his former doctoral student, Calvin Perry Stone.

Calvin Perry Stone

Calvin Stone completed his doctoral work under Lashley's supervision at the University of Minnesota. The youngest son of Indiana farmers, Stone was familiar with hard work. His father died when he was five; Calvin stayed at home rather than attend the funeral. While the family was at the funeral, their home burnt down. Calvin was rescued by neighbors. Calvin's mother refused to move so the family rebuilt their house and continued farming. At an early age, Calvin went to work for other farmers in order to help his family. After completing as much school as was locally available, Calvin worked his way through Valparaiso University to earn the equivalent of a high school degree and a few college credits. He then enrolled at Indiana University to complete his undergraduate work (1914) and stayed to complete his M.A. (1916). While a student at Indiana, Calvin worked with M.E. Haggerty. Haggerty had completed his doctoral work at Harvard with a dissertation on imitation in monkeys. Upon his return to Indiana, he taught courses in comparative psychology and did comparative
research. The same pressures to do more applied work that Yerkes, Watson, and other comparative psychologists felt at their respective institutions were felt by Haggerty at Indiana and he moved toward work in educational psychology. Haggerty left Indiana in 1915 to become a Professor of Educational Psychology at the University of Minnesota.\textsuperscript{131} Haggerty apparently influenced Stone toward comparative psychology; he supervised Stone’s master’s thesis on light discrimination in dogs and persuaded him to come to the University of Minnesota to complete his doctoral degree.\textsuperscript{132} Stone eventually followed Haggerty to Minnesota, but first completed a year as Research Psychologist at the Indiana State Reformatory for Boys and two years in the military.

When Stone did finally enroll at the University of Minnesota in 1919, he was married and the father of two children. He did take a course in educational psychology with Haggerty, but pursued research with Karl Lashley.\textsuperscript{133} His thesis topic was "An Experimental Analysis of the Congenital Sexual Behavior of the Male Albino Rat."\textsuperscript{134} Stone minored in anatomy and worked as instructor in the Department of Anatomy after completing his degree. He stated in an autobiographical sketch written late in his career that the basic medical sciences had held a strong appeal for him as a student.\textsuperscript{135} Thus, early on in Stone’s career, he showed the disposition for interdisciplinary
work. He was an ideal candidate for the kind of work that the CRPS had in mind when it began dispensing funds.

Karl Lashley described Calvin Stone in 1922 as "quiet and unassuming, slow in speech, and outwardly unemotional. I have always found him absolutely reliable and conscientious in detail." Lashley was recommending Stone to Lewis Terman for a position at Stanford University. Stone was hired and left for Stanford in the summer of 1922; he was to remain there for the rest of his career. The remarkable program in sex research that he began there was the first of its kind by a psychologist.

In the fall of 1922, Lashley asked the CRPS for permission to divert a portion of his $3500 grant to Calvin Stone. He stated that Stone’s dissertation research had been part of the proposal that the Committee had funded in his, Lashley’s, work. The Committee declined to divert any of Lashley’s funds, but did make a special appropriation of $250 to Stone. It could be argued from this that Stone’s sex research was funded from the beginning by the CRPS. At any rate, Stone became a regular recipient of CRPS funds, receiving $18,650 in the years from 1922-23 to 1940-41. He never received any large grants (the largest grant was $3000 in 1935-36), but was consistently granted funds, for Stone was funded by the Committee for all but four years in that time span. He was able to turn the money to good use; he was a diligent researcher who produced a steady stream of
research publications. His work was dependent, to some degree, on external funding, as evidenced by his plea to Stanford University's president for replacement funds when the CRPS ended his funding in 1941. Beyond his own work, he used Committee funds to support the work of his graduate students. Stone and his students published their work in psychological and physiological journals. His work illustrates the overall plan of the Committee to foster sex research at the boundaries of relevant disciplines.

Stone's first publication on sex behavior of the rat is considered a classic in its area. The article was derived from his dissertation and was entitled, "The Congenital Sexual Behavior of the Young Male Albino Rat." The research reflected the ongoing debate about instincts then occurring in psychological literature. A young Chinese student of Edward C. Tolman, Zing Yang Kuo, had sparked the debate by arguing that the concept of instincts was unnecessary in psychology; that environment was the critical determining factor in human development and behavior. Although Kuo later moderated his position by acknowledging that it was too difficult to disentangle the effects of environment and heredity from each other, Stone's paper was part of the early response to Kuo's more extreme position. Stone concluded that the copulatory act in young male rats did not depend on prior learning or environmental experience. Stone's article also reflected his interest in
development; he noted that sexual behavior was not initiated until the young rat was physiologically mature. That is, the presence of female rat in heat did not elicit premature, undeveloped attempts at copulation by young rats. From these observations Stone directed a criticism against Freudian ideas of infantile sexuality, noting that his research did not support the Freudian view. But Stone did have an eye toward human applications in his article. He compared his findings to the work of Havelock Ellis on the role of external stimulation in sexual arousal. This first publication by Stone demonstrated his experimental rigor and careful analysis of findings. It is also an adumbration of some of the themes he would develop over the next twenty years and illustrates the close connection between comparative research and human application that was so desired by the CRPS.

Stone followed this first article with a comprehensive review of extant literature on the role of neural and hormonal factors in male sexual behavior. This was the area that was being opened up by physiologists and anatomists at the University of Chicago, Cornell, the University of California, and elsewhere. Stone stated that psychologists should address these issues experimentally and incorporate the findings into the psychology of sex. In effect what Stone did in this article was set his own research agenda. Most of his publications
over the next few years addressed the neural and hormonal control of sexual behavior.

Stone's first research report to acknowledge the financial support of the CRPS appeared in 1923. It was an extension of the work begun in his dissertation. Stone sought to determine the factors necessary for the initial activation of sexual behavior in young male rats. The study demonstrated that copulatory ability was not prevented when most of the animal's exteroceptive receptors were destroyed. Stone continued this tack of ablating various parts of his animal subjects' neural and sensory capacities to test what was necessary for sexual behavior. Stone concluded from his cortical ablation studies that sexual behavior was probably controlled subcortically in the species under consideration. Castration was also employed to test for hormonal influences on sexual behavior in the rat. Stone argued that his work had importance in the light of claims of physical and sexual rejuvenation of human males when injected with testicular substances. Again, comparative work was pursued not only for its intrinsic interest and value, but was also justified as having value for the understanding of human sexual behavior. Stone also conducted research on the effects of diet on initial copulatory ability, female reproductive behavior, and maternal behavior of rats. This last study was conducted with his graduate student, Mary Sturman-Hulbe and
illustrates Stone’s support of his students with CRPS funds.\textsuperscript{150}

During Stone’s first decade of CRPS funding, he supported five graduate students’ research projects. George T. Avery pioneered the use of guinea pigs in sex behavior research. Avery detailed both male and female’s copulatory behavior. He closely followed Stone’s method and, in general, his results accorded with Stone’s.\textsuperscript{151} As noted above, Mary Sturman-Hulbe’s project on maternal behavior in rats was supported by Stone’s funds from the CRPS. Her study was intended to provide an analogous situation of wild rat behavior in the laboratory by varying external stimuli such as light, wind, and heat. William Dollard Commins continued Stone’s work on the effects of castration, with support from CRPS funds.\textsuperscript{152} Stone, who remained interested in human psychology, supported Lois Doe-Kuhlmann’s research on premature pubertal development.\textsuperscript{153}

Perhaps the most important of Stone’s graduate students was Clarence Ray Carpenter. Carpenter came to Stanford from William McDougall at Duke in 1929 and completed his PhD in 1932. His work at Stanford was on the effect of castration on pigeons.\textsuperscript{154} Stone supported Carpenter’s work at Stanford with CRPS funds; this connection proved vital in tying Carpenter into the funding pipeline. After completion of his dissertation, he became a National Research Fellow under Robert Yerkes at the Yale Laboratories of Comparative
Psychobiology. From this position Carpenter ventured into field research with primates. Dewsbury argues that, in this era, Carpenter's work "set a new standard for completeness, accuracy, and detail" and became "the major driving force behind primate field biology."\(^{155}\) Donna Haraway has offered another perspective on Carpenter. Haraway explains Carpenter's research, both with pigeons and primates, as premised on the view of "sexual activity as the unifying locus of the individual organism and of organic society." Sex and mind, she argues, "were keys to scientific control of life."\(^{156}\) Haraway perceives Carpenter's work, and that of his colleagues and professors, as part of the effort to extend scientific control to all of life; a project she argues was at the center of scientific and foundation leaders' plans in the interwar period.

Stone's research was certainly part of a "community of interest" in sex. The reports of his work and that of his students appeared in psychological, embryological, and physiological journals, alongside the reports of anatomists, physiologists, and zoologists supported in their sex research by the CRPS. Sex was a borderland open to any qualified investigator, regardless of disciplinary affiliation. Diana E. Long has argued that these individuals identified themselves as part of a "community of interest" in sex and that traditional disciplinary identifications were not of primary importance.\(^{157}\) Within
this community, Stone and Yerkes (whose work is described below) provided leadership in extending the range of phenomena suitable for psychologic investigation. To fully elucidate how comparative psychology functioned in this research community, we must examine the work of Robert Yerkes and his students.

Robert Yerkes, his students, and sex research at Yale

In 1924 Robert Yerkes left his position at the National Research Council for a professorship in the new Institute of Psychology at Yale University. The institute was the brainchild of Yerkes, James Rowland Angell, Yale's president, and Beardsley Ruml, director of the Laura Spelman Rockefeller Memorial (LSRM). Ruml was a University of Chicago PhD in psychology, where Angell had been his advisor. Ruml had then been Angell's assistant when Angell was the president of the Carnegie Corporation. The LSRM was established in 1918 by John D. Rockefeller, Sr., in honor of his deceased wife. Its initial support was mostly for projects to benefit women and children. Ruml became its director in 1922 and immediately set out to direct the Memorial toward the support of scientific research, particularly in the social sciences. Under Ruml's leadership the goals of the LSRM meshed with those of other Rockefeller philanthropic agencies. Those goals centered around the advancement of knowledge and the use of that knowledge in the service of social engineering.
Franz Samelson has termed the general effect of Ruml and the LSRM as facilitating the turn of psychology toward "practical problems of behavior and adjustment." Though one could make the argument that psychology had always had an eye on practical applications, Samelson is right that foundation support, or the hope of it, did influence many psychologists to tack toward the practical. The Institute of Psychology at Yale was a specific example of the goals of Ruml and the LSRM. Angell wrote to Ruml in early 1924 that there is unquestionably great need at the present moment for the development of psychology-using the term in the broadest sense-to contribute light and guidance for the solution of many problems of our present social order. I think it is a conservative statement to say that we are at the very threshold of the most important advances in our understanding of human activities and our ability, particularly through educational methods, to control human life for the betterment of all future generations.

The way to do this was to establish a center where competent researchers could work together. In 1950 Yerkes recalled that the opportunity at Yale had been great for a true cooperative venture. He argued for a professional school of psychology to be part of the Institute, where psychologists could be trained as scientists and practitioners. The new institute, then, was to be a realization of Ruml, Angell, and Yerkes' aspirations to advance knowledge in a manner that would ameliorate human problems. The press release announcing the Institute proclaimed, that it would be "devoted to the study of fundamental problems of human behavior, and to the training of personnel for the pursuit
of new studies in the field of teaching, research, and of practical application." 163

Yerkes wanted to call the new center The Institute of Psycho-Biology at Yale University. 164 Even before the plans had been finalized, Yerkes was arguing for a focus on the "higher mammals, especially the apes." 165 Upon the establishment of the Institute of Psychology, Yerkes accepted an appointment as Professor of Psychology. He was not obligated to any undergraduate teaching, indeed, he was, as he later stated, "free to devote myself chiefly to research with primates other than man." 166 This was, finally, his opportunity to pursue his longtime desire to work with primates and to fulfill his dream of a primate research station.

Before the end of his first year at Yale, Yerkes outlined his proposal for primate research. 167 The initial emphases would be on ideational and sex behavior. (Yerkes had worked on the former problem with G. V. Hamilton on the McCormick estate in California in the mid-1910s.) Yerkes also wanted to train someone for the anticipated anthropoid research station; at this time, Havana was his choice for the location of the station. He had in mind Harold Clyde Bingham, a recent Johns Hopkins PhD and professor at nearby Wesleyan University. Bingham did join Yerkes at the Institute of Psychology later in 1925. He was the first of
several competent researchers to work with Yerkes in this decade.

Chauncey McKinley "Red" Louttit also became a research associate with Yerkes at the Institute of Psychology in 1925. Unlike Bingham, Louttit did not yet have his PhD; he did not earn it until 1928. Louttit worked with guinea pigs, with special emphasis on sexual behavior. Bingham's focus was on primates. He was responsible for the care of the chimpanzees, as well as conducting research with them. His research resulted in several publications that focused on sex development and behavior. Two years later, in 1927, Otto L. Tinklepaugh came to the Institute to work with Yerkes. Tinklepaugh's research was with monkeys and chimpanzees. These three men all received funding for their research from the CRPS, through its grants to Yerkes. Yerkes received $23,000 from the CRPS in the years from 1925-1928; he also received funds from the Rockefeller Foundation for anthropoid research in this period. Curiously, Yerkes withdrew his request for funds from the CRPS for the fiscal year 1928-29 and stated that he was temporarily abandoning sex research "because of the impossibility of satisfactory arrangements." No other explanation was forthcoming; Yerkes did not receive any more funds from the Committee until the fiscal year, 1931-32.¹⁶⁸
Ape sexual development: H. C. Bingham

Harold C. Bingham's monograph on sex development in apes appeared in 1928. The document is a detailed and fascinating account of Bingham's observations of sexual behavior in primates. The four chimpanzees, two females and two males, that made up the colony at New Haven were the primary subjects for Bingham's observations. He attempted to determine the genesis of sexual behavior among these animals. Bingham found that non-sexual social behavior was an important precondition for later sexual behavior. He found evidence of homosexual, heterosexual, and masturbatory behavior among the chimpanzees.

As remarkable and interesting in itself as Bingham's research was, the reader is constantly aware of the human factor in the work. The human analogue was of obvious importance to Bingham. He partially justified his work as an attempt to circumvent the problem of lack of direct observation of sexual behavior among humans. At the conclusion of the report, Bingham suggested that sex research with primates may "help materially in dealing with the sexual problems of man." It would be unfair to suggest that Bingham's work was pointed only at human problems. It had merit and interest in itself. But given the stated purpose of the CRPS and of Yerkes, that primate research was to facilitate the understanding of human sexual behavior, and by extension, offer a means of controlling the
sex impulse, Bingham’s project is perhaps best understood as a contribution to the agenda of social engineering.

The sexual behavior of guinea pigs: C. M. Loultit

"Red" Loultit’s project on sex behavior of the guinea pig was more ambitious, in some ways, than Bingham’s work with the chimpanzees. Loultit first sought to determine the ontogenesis of sex behavior. Second, he sought to modify sexual behavior and determine whether the changed behavior would be inherited (the offspring all died). Third, he compared the sex behavior of litter mates and non-litter mates in search of the incest barrier. Fourth, Loultit attempted to develop methods for measuring motivated behavior in his subjects. Loultit found evidence of the incest barrier; litter mates were less likely to mate than non-litter mates. His work on the heritability of experimentally modified sex behavior, which was the original problem set to him by Yerkes when Loultit came to the Institute, was more problematic. Yerkes reported that Loultit could not confidently claim that such modifications were heritable.

In light of the original aims of the conference that led to the formation of the CRPS, Loultit’s research, done at the suggestion of Yerkes, fits with the eugenic concerns of the original conferees. In a fragmenting society where old sexual norms and mores were failing to regulate sex behavior, perhaps it was possible to experimentally modify
sexuality and have it passed along to the next generation. The engineering ideal underlay this work.

**Monkey sexual behavior: O. L. Tinklepaugh**

Otto Leif Tinklepaugh did not come to work with the Yerkes group until 1927. Tinklepaugh was a 1927 University of California PhD whose dissertation had been on problem solving in monkeys. He brought four rhesus monkeys (three females, one male) with him to Yale. His work at Yale included comparative research on memory in monkeys, chimpanzees, and human subjects, visual perception in monkeys and chimpanzees, and handedness in monkeys and chimpanzees. Tinklepaugh's sex research was with his rhesus monkeys. He investigated masochism, sexual and social activity, and parturition, maternal behavior, and infant behavior. This last set of studies was conducted with the Johns Hopkins embryologist, Carl Hartman. Yerkes, with an eye toward practical justification of funding, asserted that Hartman and Tinklepaugh's work was of great value to gynecologists and obstetricians. In the second report of their research on maternal and infant behavior, Hartman and Tinklepaugh provided a rationale for their research in terms of the heredity-environment debate. Freud, on the side of innate sexuality, and Watson, on the side of environmental influences, were invoked as context for Hartman and Tinklepaugh's research. In their conclusion, they questioned the role of instinctive or innate behavior and
offered an interpretation that supported a mechanistic explanation. Tinklepaugh also made a very practical contribution to the care of the primates at Yale. He devised a way to prepare their food so that it would require more chewing, with the result of healthier teeth and improved digestion.

Organizing and directing sex research: Robert Yerkes

Yerkes did not do a great deal of experimental work in the first few years at the Institute of Psychology. He taught a seminar in Comparative and Genetic Psychology during the academic years 1925-26 and 1928-29 and supervised the work of his research associates. Most of his time, however, was spent assembling a massive bibliography of literature on anthropoids. This was published in 1929 as The Great Apes. His research efforts were devoted mainly to his work with a captive gorilla, Congo. Each winter, for three successive winters, Yerkes traveled to Florida to work with Congo. Yerkes conducted a variety of psychological tests on Congo: tests of memory, imitation, insight, and adaptability. He also reported on her mood, sexual behavior, motor skills, and playfulness. Yerkes later recalled his experience with Congo as greatly enhancing his understanding of mind and personality. His travels to Florida also resulted in the idea of building the proposed primate research station there. In 1929, Yerkes finally was given the necessary backing to build his longtime dream in
the form of a $500,000 grant from the Rockefeller Foundation. The Yale Laboratories of Comparative Psychobiology were established just south of Jacksonville, Florida in Orange Park.

In 1929 the Institute of Psychology was closed as part of a reorganization of research at Yale. The Rockefeller Foundation poured a massive amount of money into the new Institute of Human Relations there. Yerkes got his own Laboratory of Comparative Psychobiology in the Physiology Division of the Medical School and he was able to establish a primate research station in Orange Park, Florida. Yerkes remained at Yale for the rest of his career. At Yale and at Orange Park, Yerkes, his junior associates and peers, utilized primates to find ways to solve psychological and social problems. What began in Washington in the early 1920s as an attempt to find solutions to the sex problem, a field in which few scientists dared to work, was an expression of a broader attempt to order life through science. Yerkes was a principal architect and planner of the evolution of sex research and of the re-organization of science. Dewsbury has characterized Yerkes as "above all, a planner." His planfulness was of vital assistance as he pioneered a new role for psychologists as science bureaucrats; individuals who could plan large-scale projects, find funds to support those projects, and
cultivate a research group of competent men and women to investigate a coherent body of research topics.

To those who had supported the experiment in the advancement of knowledge of which the Institute of Psychology was a part, the work of Yerkes and his colleagues was a success. Beardsley Ruml, speaking at the dedication of the new Social Science Research Building at the University of Chicago in 1929, claimed that the work of the LSRM in places like the University of Chicago and Yale's Institute of Psychology had demonstrated that existing divisions of knowledge were anachronistic and that the route to the use of science for human betterment lay in the interdependence of research. Yerkes and his group at Yale, and Calvin Stone and his students at Stanford, were part of this new era of science. This was one effect of the reorganization of science begun after World War I by George Hale and the National Research Council and financed, in large part, by the Rockefeller Foundation as an attempt to minimalize the importance of disciplinary boundaries in the service of placing all human behavior under the rational, ordered gaze of science: in short, social control was the aim. From a broad perspective, this was an attempt to save the Enlightenment project of privileging science as truth in the face of the fragmentation and alienation of contemporary society. Lily Kay has argued this was the case in molecular
biology and Donna Haraway has perceived it in primate studies.182

From a disciplinary perspective, Stone and Yerkes extended the boundaries of psychology cognitively and organizationally. They and their students opened a new field for psychological research. Stone initiated the first programmatic approach to sex research by a psychologist. He and his students used sex research to address questions that were considered important by his fellow psychologists: questions of instinct, cerebral control of behavior, and the role of experience and maturation in the development of behavior. Yerkes was more important in his role as science bureaucrat. It is tempting to characterize Yerkes as foresighted when he agreed to assist Earl Zinn, when the latter approached him for help in launching a scientific investigation of sex in 1920. Whether he was foresighted or not, Yerkes was persistent once embarked upon the venture. His organizational skills with the CRPS served the Committee, his fellow psychologists, and his own agenda well. Yerkes helped bring greater legitimacy to his discipline through his bureaucratic efforts. This was new ground for psychologists at the time. The research of the Yerkes group was important as well. Bingham, Louttit, and Tinklepaugh had little to guide them when they began their work with Yerkes at Yale. Each of them made some contribution of technique or method that facilitated later
developments in the field of comparative psychology. The cognitive contributions of these men fit well under the ideological umbrella of the CRPS and its philanthropic sponsor.

The Comparative Approach to Sex and Marriage

The comparative approach also found a place in the attempt to explicate the role of sex in marital adjustments and maladjustments. In order to contextualize the comparative approach to the role of sex in marital adjustment, it is necessary to return to the point where the CRPS ended the funding of Martin Peck and Fred Wells. The work of Peck and Wells was part of a proposed center for the study of human sex problems in Boston. When the CRPS ended its funding, it did so in part because Peck and Wells were unable to devote enough time to their research. Both had strong clinical commitments that had to be met before any sex research could be done. An additional complaint by the CRPS was the lack of research sophistication on the part of Martin Peck. In correspondence with Yerkes and Cannon, Earl Zinn, the Committee's Executive Secretary, described the situation in Boston as "rather hopeless from the standpoint of research" and agreed with them that the work could best be done on a full-time basis.  

The failure of Salmon and the Committee to establish in Boston a research center for the "psychobiology of sex in
man" was illustrative of the difficulty of doing human sex research. There were problems on both the personnel and programmatic sides. Although all involved agreed that the need to understand human sexuality was great, cooperation among potential researchers was obstructed by failure to agree on a specific research agenda and the resistance to centralized direction. Additionally, the failure of Meyer to produce published results and the focus of the Lee group at Harvard on homosexuality were not solid returns on CRPS investment. Salmon and the Committee sought to move human sex research into the heart of the borderland: marriage and sexuality. Their special interest was the role of sex in marital adjustment or maladjustment.

The initiation of research on sexual factors in adjustment or maladjustment in marriage was problematic for the Committee. Although this kind of research had been envisioned by the CRPS from the beginning, problems arose in finding a director for the project and in gaining approval from the NRC to sponsor it. The problem of a suitable director was solved in the appointment of Gilbert van Tassel Hamilton. Hamilton was one of the more interesting figures of this era of sex research. He was a psychiatrist who was also trained as a comparative psychologist. In this section, I detail how the proposed Center for the Study of Problems of Marriage came about. I then relate how Hamilton was chosen as its director and how his training in
comparative psychology informed his research on sex and marriage. I then argue that Hamilton was an example of the blurring of disciplinary and professional boundary-lines.

The heart of the matter: Sexuality and marriage

Despite the disappointing failure to establish a sex research center in Boston, Salmon persisted in his quest to establish a center for human sex research. Even before it became obvious that the work begun under Roger Lee’s direction was not going to prove productive in illuminating human sexuality, Salmon was planning a much more ambitious research project, a study of sexual adjustment in marriage. The idea for a center for marital studies grew out of a meeting held in New York City in November, 1923.

Five men met in the home of the neurologist and psychoanalyst L. Pierce Clark on the first day of November, 1923, to formulate a plan for conducting human sex research that would meet the strict criteria of the CRPS. The five: George Kirby, Bernard Glueck, Salmon, Earl Zinn, and Clark, all shared a commitment to psychodynamic formulations of human behavior. Their immediate concern was to address a proposal made by the noted New York psychoanalyst, Abram Kardiner, for a study of factors leading to marital unhappiness. Salmon expressed concern that Kardiner’s proposal could not be formulated in great enough specificity to be studied scientifically; failing such specificity,
Salmon argued that the experimentalists on the CRPS would not approve it.

Bernard Glueck, at Salmon's behest, offered an alternative proposal: to test Freudian formulations objectively and quantitatively. Glueck, former associate of William A. White at St. Elizabeth's Hospital in Washington, DC, and current head of the psychiatric clinic at Sing Sing prison, suggested drawing subjects from divorce court cases, prisons, juvenile homes, and from referrals by "reputable psychiatrists." Topics to be studied in the proposed tests included masturbation, homosexuality, the effect of "abnormal home situations" on sexual development, sexual offenses, genesis of sexual malfunctioning, frequency of sexual factors in personality dysfunction, and "study of behavior in a day nursery, with special reference to sex reactions."186 This curious admixture of potential subjects and topics represented the normal, the near-normal, and the psychopathic. As a research agenda it did not have much to recommend it and it was declined by the full CRPS. Nevertheless, the idea of studying sex and marriage was a compelling one and it was pursued by Zinn, Salmon, and Katharine Davis.

Out of this conference was born the project that was central to the Committee's human sex research agenda in the 1920s: the Center for the Study of Problems of Marriage. Praised by some and labeled a fiasco by one of its earliest
supporters, Robert Yerkes, this project came to symbolize the perils and promise of the borderland of sex research.\textsuperscript{187}

Although the full Committee turned down the Glueck proposal, Salmon was encouraged by the Committee to pursue the possibility of sex and marriage research based upon divorce court referrals. The court that seemed most promising was Judge Hoffman's Court of Domestic Relations in Cincinnati; comparable courts in New York were ruled out because of unfavorable political and religious sentiment. The model for this research approach was the success of William Healy and Augusta Bronner's work with juvenile offenders referred by the courts, first in Chicago and then in Boston at the Judge Baker Foundation.\textsuperscript{188}

While Salmon pursued this possibility, Zinn was working on the possibility of establishing a clinic in New York City to which couples who were having marital difficulties could be referred for study and assistance. Their efforts were redirected by the appearance of a potential new benefactor, Max Rosenberg of San Francisco. Rosenberg demonstrated great interest in the idea of marital research pitched to him by Zinn, Yerkes, and the sociologist, William Ogburn. Ogburn, proponent of the theory of cultural lag (the idea that social institutions and understanding lag behind scientific progress and technological change), was strongly interested in the sociology of the family.\textsuperscript{189} More importantly for the planned research center, he was a
personal friend of Rosenberg's and had great influence with him. Rosenberg initially agreed to fund up to one-half of the proposed center. Ogburn then influenced Salmon and Zinn to include happily married couples in the proposed study in order to act as a control for the study of those maladjusted in marriage. The result was a change in plans from a clinic to a center for the study of marriage. The center was to be located in New York City.

The justification of the center in terms of the Committee's brief was that the factors that lead to marital adjustment or maladjustment were the "important sex problems." The elucidation of such factors would provide a clarification of problems in human sexuality and facilitate the formulation of a concise research program that could be funded by the Committee. The key to its success, it was argued, lay in the right selection of personnel.

Once it was decided that the Committee would support a center rather than a clinic, the decision as to who would be the director became the critical issue. The first candidate to come to the Committee's attention was the British physician and sexologist, Havelock Ellis. Ellis had built a substantial reputation through his writings about sex. But Ellis was just one of several names placed before the CRPS for consideration for the directorship.

Katharine Davis and Earl Zinn compiled a list of prospective directors. One method for building the list was
to ask prominent psychologists and physicians to name individuals they thought qualified to do the work. Ellis almost invariably was the first named. Although invited by the Committee in 1924 to come to America to discuss the possibility of directing the research, Ellis fell ill and was unable to come. At some point between March and July, Ellis was removed from consideration. The final candidate list placed the prospects in three tiers. Those in the first tier were all psychiatrists and, with one exception, all psychoanalytic in approach. This first tier included the noteworthy William Alanson White of St. Elizabeth's in Washington, DC, White's former assistant, Bernard Glueck, George Kirby, George Amsden, Charles Lambert, and G. V. Hamilton. The second and third tiers were also dominated by men who were psychodynamic in their orientation. These lists indicate that human sexuality was considered, first and foremost, a medical concern. But they also indicate that a psychological approach was necessary to the explication of human sexual behavior. That is evident even in the one exception to the psychodynamic rule: the paradoxical G. V. Hamilton. Although not committed to a psychoanalytical approach, Hamilton was invested in a psychological approach to the understanding of human behavior. A former student, and long-time friend, of Robert Yerkes, by midsummer, 1924, he was offered the directorship.
The background of G. V. Hamilton

G. V. Hamilton was a native Ohioan, born in the town of Frazeyburg in 1877. His undergraduate work was completed at Ohio Wesleyan in 1898 and he received his M.D. from Jefferson Medical College, Philadelphia, in 1901. Hamilton's initial work as a psychiatrist was in a custodial state hospital in Pennsylvania. In 1905 he resigned his position there and took a position as resident physician at the forward-looking McLean Hospital in Waverly, Massachusetts, where he remained until 1907.

While at the McLean, Hamilton enrolled for graduate work in psychology at Harvard. Two psychologists influenced Hamilton while he was at the McLean Hospital: Shepherd Franz and Robert Yerkes. With Franz, Hamilton investigated the effects of exercise upon depressed patients and was exposed to the methods and apparatus of experimental psychology. More importantly for the future was Hamilton's work with Robert Yerkes at Harvard University. It was this relationship that sparked Hamilton's interest in the comparative approach to psychological and psychopathological problems. For example, Hamilton's study, done under Yerkes' direction, of reaction tendencies in dogs was published in 1907. In the next few years, Hamilton's published articles were all in the comparative field, including one on sexual behavior in monkeys and baboons. Hamilton credited Yerkes with directing him toward the "objective" method in
psychopathology. Hamilton wrote that he used this method, which apparently consisted of listening closely to patients' self-reports, in psychiatric treatment to determine how his patients were maladjusted. This method Hamilton contrasted with psychoanalysis and its inferential demands.\textsuperscript{198}

In 1906, Hamilton became involved in the psychiatric care of the wealthy financier, Stanley McCormick. When McCormick was discharged from the hospital, he employed Hamilton as his private physician. In 1908, Hamilton and his family traveled with McCormick to the latter's estate in Montecito, California. Hamilton was to remain there until 1917. In that year, Hamilton left the employ of the McCormick's in something other than an amiable parting. Whatever happened between Hamilton and the McCormicks, it was bad enough so that Stanley McCormick's widow, when she endowed research into schizophrenia at the Worcester State Hospital, specified that no funds were to be used for psychological approaches.\textsuperscript{199}

While Hamilton was in residence on the McCormick estate, he invited Robert Yerkes and family to come and spend a working vacation there. In 1915, Yerkes took a half-year sabbatical from Harvard and spent the bulk of the time on the estate doing primate research with two monkeys and an orangutan. Yerkes called the situation an "investigator's paradise."\textsuperscript{200} Hamilton and Yerkes worked together with the primates, using the multiple-choice method
devised by Hamilton and modified by Yerkes. Yerkes stated that he was interested in the animals' problem-solving abilities, while Hamilton was interested in the animals' adjustment to the variations in difficulty or reward. For Hamilton, this was an approach to understanding psychopathological reactions.²⁰¹

Hamilton pursued a variety of activities between 1917 and 1924, the year he became the director of the Center for the Study of Sex Problems in Marital Relations. He served in the Army during World War I and worked at the Santa Barbara Clinic in California between 1922 and 1924. In 1922, he submitted a request for funding from the CRPS for a proposal to study "Problems of Sexual Behavior in Cattle, Swine, and Primates."²⁰² The proposal was not funded. Beginning in early 1921, Hamilton spent a year in his native Ohio collecting psychiatric data, the report of which was published in 1925 as An Introduction to Objective Psychopathology.²⁰³ Hamilton's writing was heavily preoccupied with sexual themes. Hamilton saw sex everywhere, especially what he perceived as sexual deviancy (masturbation, exhibitionism, homosexuality, "skinny-dipping," marital infidelity, "frigidity," and adolescent sexuality). Harry Stack Sullivan, in an amusing and damning review, cited it favorably for the potential suggested in it by the comparative material.²⁰⁴
Hamilton was able to sell the Committee on his plans for the Center based, at least in part, on his proposed use of the comparative method. Hamilton was offered and accepted the directorship on July 27, 1924 and agreed to be ready to work on October 1 of the same year. Hamilton's research on sex and marriage in New York City

The new center experienced problems from the start. A grant of $16,000 was made to the Center by the Bureau of Social Hygiene for the first year of operations. But the Executive Committee of the NRC objected to the CRPS's involvement in the project on the basis of its perceived social science nature and the potential for controversy that the work held. An initial compromise formulated by Katharine Davis placed the CRPS in an advisory capacity to the Center, though the NRC insisted that even that capacity be kept secret. John D. Rockefeller, Jr., vetoed Davis' plan and sought to have Hamilton's work transferred to the Social Science Research Council. Robert Yerkes strongly protested this proposal, with the result that Davis' plan was reinstated. At the time that Hamilton was prepared to have the manuscript report of his work published, he was initially told that the book could be published with the full acknowledgement of the support of the Committee. But the NRC again intervened to prevent any association of the CRPS with the report; the result was that Hamilton had great difficulty in finding a publisher for A Research in
Marriage. He finally arranged with the specialty house of Arthur and Charles Boni to publish it.\textsuperscript{208}

Hamilton arrived in New York City to begin his research in early October, 1924. He brought with him from California a research proposal for the new center. The perspective for the research was that of psychobiology: "the individual will be viewed as a functioning organism whose responses as a whole are to be attributed to theoretical knowable and controllable exogenous and endogenous stimulations."\textsuperscript{209} Hamilton made a very specific set of proposals for the solicitation of subjects and the collection of data. In the first phase, he essentially sought to use interview and questionnaire methods that would be standard from one subject to the next. Phase 2 would allow for "clinical analysis," where the interviewer would probe for the causes of the sexual maladjustment. These latter data, Hamilton asserted, would be regarded as supplementary.\textsuperscript{210}

Hamilton began interviewing subjects in late 1924. His subjects came from referrals by fellow psychiatrists and physicians. Hamilton eventually collected data from 200 subjects, 100 married men and an equal number of married women. These individuals were mostly college graduates under the age of 40.\textsuperscript{211} Hamilton found that his subjects engaged in a wide variety of sexual behavior, socially sanctioned and otherwise. Alfred Kinsey noted in his landmark 1948 study that Hamilton's findings generally
accorded with his own. What is of greater interest here is the relationship that Hamilton perceived between comparative psychology and his marital research.

In 1927, Hamilton published an article on the relationship between comparative psychology and psychopathology. In it, he urged psychopathologists to draw from the findings of child psychology and comparative psychology. On the other hand, Hamilton urged the use of the comparative method to explore the development of maladaptive behavior. He wrote, "The ideal procedure to follow when one seeks to explain a given set of phenomena is, in principle, the same whether one is studying a rat, a chimpanzee, or a psychoneurotic patient." Hamilton argued from the perspective of one trained in medicine and in comparative psychology. For him, the methods of each were complementary and equal in cognitive authority.

Hamilton applied this perspective to his sex research. He raised the question of the adaptive value of sexual intercourse, i.e., did it have value beyond procreation and continuation of the species? The context for his question was the lack of orgasm among half of his female subjects at the Center for the Study of Sex Problems in Marital Relations. Comparative studies, Hamilton argued, could illuminate the human experience or lack of experience of sexual pleasure. Hamilton also raised the question of modifying sexual behavior. He suggested that one way to
explore such a possibility would be through exposing young monkeys and apes to "the counterparts of both the planned and unplanned influences to which our human children and adolescents are commonly subjected." Hamilton extended the question of behavior modification to the question of conditioning individuals to avoid sexual interaction with appropriate members of the opposite sex. Conditioning experiments could be used on animals, especially primates, to "throw much needed light" on this question. The incest barrier and the Oedipus complex were also worthy topics, Hamilton suggested, to try to elucidate through the comparative approach.

There are some interesting parallels between Hamilton's suggestions and the investigations of the Yerkes group at the Yale Institute of Psychology, mentioned above, in this period. In particular, the research of Bingham and Louttit fits nicely with the program of potentially worthy topics suggested by Hamilton. The direction of influence between Hamilton and the Yerkes group is difficult to determine and would benefit from careful historical scholarship. Hamilton and Yerkes were close; Yerkes later wrote that much of his insight into primate behavior was gained from Hamilton. What does appear certain is that Yerkes and Hamilton were of the same mind on the relevance of comparative psychology for the understanding and control of human sexual behavior.
The Center for the Study of Sex Problems in Marital Relations closed in 1928. Hamilton returned to Santa Barbara and clinical practice. He remained there until his death in 1943. Hamilton contributed a chapter to Problems of Ageing, edited by E. V. Cowdry, on "Changes in Personality and Psychosexual Phenomena with Age." The chapter reflected his comparative interests and showed evidence of a marked influence of psychoanalytic concepts. He reiterated his findings from his years as director of the Center and continued his preoccupation with masturbation. He suggested that masturbation among the elderly was a sign of regression.

The responsibility for research into the psychobiology of sex in humans was shifted to the Social Science Research Council, where the focus was shifted to studies of the family. After the Center closed, Yerkes called the whole attempt to study sex and marriage a "fiasco." Such an ambitious project was not to be tried again until Alfred Kinsey launched his massive study of human sexuality approximately ten years after Hamilton ended his. Kinsey was also a CRPS grantee, and, like Hamilton, his work sparked controversy within the NRC and among the public.

Hamilton was a borderland figure working borderland territory. In some ways he was the ideal person to conduct sex research. He was a physician and sex had long been considered a medical topic. He was trained in the methods
of comparative psychology by one of the century's greatest comparative psychologists, Robert Yerkes. Hamilton was active in comparative research with animals for nearly a decade after his training with Yerkes. In that sense he was more of a comparative psychologist than some individuals who, after being trained in the field, left for careers in education or other applications.221 It is clear that Hamilton's work with humans was informed by comparative methods and attitudes. His championship of the comparative method in sex research also served to further the validity of psychological approaches to sex studies.

Finally, there is the question of influence. How influential was Hamilton? I see his influence in the work of Robert Yerkes. Yerkes' 1913 ("Comparative Psychology and Medicine") and 1921 ("The Relations of Psychology to Medicine") statements on the need of incorporating psychology, particularly comparative psychobiology, into medicine bear the marks of Hamilton. Yerkes' used Hamilton's multiple-choice method in animal research and suggested its use in human psychopathology. The research of Yerkes' junior associates at Yale's Institute of Psychology also reflects Hamilton's suggestions made at the same time. Lewis Terman acknowledged the importance of Hamilton's work in preparing the way for the reception of his 1938 book on marital happiness.222 Alfred Kinsey, of Indiana University, was also influenced by the research of Hamilton, as noted
above. Cornelia Christenson, in her biography of Kinsey, states that Kinsey was aware of Hamilton's work as early as 1929. She cites Kinsey's use of Hamilton's material in a 1935 lecture to a campus discussion club and notes that he incorporated the material into his lecture notes for the pioneering "Marriage Course" at Indiana University in 1938. Kinsey, as noted above, credits Hamilton's research as indicating the same general variety of sexual behavior as his own research had shown, though he did fault Hamilton's methodology. (Wardell Pomeroy, Kinsey's collaborator, conducted animal research prior to his work with Kinsey and is listed by Dewsbury as worthy of note in the American comparative psychology tradition.) My conclusion concerning Hamilton is that he was an important figure for sex research and for the extension of psychology's boundaries into the new territory. His training in medicine and comparative psychology served to enhance the status of psychology and to extend the range of material thought suitable for psychological investigation.

At the same time that Hamilton, Yerkes, and Stone were utilizing the comparative approach to sex research, Lewis Terman applied for a grant from the CRPS to fund his research into gender differences in non-intellectual traits. Terman's work will be discussed next.
Individual Differences and Sex Research

In 1925, Lewis Terman applied to the CRPS for $1500 to conduct exploratory trials of methods to develop a "test which will afford a serviceable index of an individual's masculinity or femininity in the non-intellectual traits." With the assistance of Robert Yerkes, Terman was granted the funds, with $22,900 more to follow in the next four years. Terman eventually received $62,000 from the Committee, by far the most given to any psychologist. In this section I am concerned with Terman's work of the late 1920s that was funded by the CRPS, in order to show that Terman extended the use of the recently developed techniques of determining individual differences in a new direction: sex research. In doing so, he ratified socially and culturally defined notions of masculinity and femininity.

Like Calvin Stone, Terman was a native of Indiana and a son of farmers. Terman earned his bachelor and master's degrees at Indiana University, where he worked with the psychologists, William L. Bryan and Ernest H. Lindley. Terman then earned his doctorate at Clark University in 1905. While at Clark, Terman published a study on sexual precocity, mentioned earlier. Terman became an assistant professor of education at Stanford University in 1910 and then professor of psychology and head of the psychology department in 1922. At Stanford, Terman revised the Binet test of intelligence and was a leader in the development of
the field of intelligence testing. In the 1920s, Terman became identified with a strong hereditarian view of intelligence and other traits. Terman was president of the APA in 1923, an acknowledgement, not only of his own work, but of the prominence of psychological technology.

Terman, in his Presidential address to the APA in 1923, justified the use of mental tests as a legitimate psychological method. Mental tests, Terman argued, had transformed psychology from the "science of trivialities into the science of human engineering." A well-constructed mental test was just as valid an experimental method as laboratory investigations employing elaborate apparatus, he asserted. For Terman, mental tests were the route to gain practical, experimental knowledge about human traits and abilities. Masculinity and femininity were two traits that Terman wanted to explore.

The grant Terman sought and received from the CRPS was to study differences in these traits. The study of sex differences had received a great deal of attention from psychologists in the two decades before Terman's request. Terman in his proposal to the CRPS noted that research had shown that sex differences in intelligence were small, and potentially unimportant. However, there was some question whether other traits could be typified as either masculine or feminine. "Emotion, interests, character, and personality" were the traits that Terman mentioned in his
letter to Earl Zinn. Terman stated that he was inclined to believe that there were sex differences in these traits, but he wanted to establish those differences empirically. Terman believed that if he could make masculinity or femininity measurable by a test, then the test would have great utility in assisting individuals to make the correct occupational choice, to choose the right friends, to choose the best mate, and would make for maximum marital happiness.  

With the grant in hand, Terman began to collect his data. He hired Catharine Cox, a former graduate student and future wife of Walter Miles, to assist him. Terman and Miles published an extensive review of the extant literature on sex differences in the association of ideas. They concluded from the review that some differences did exist, but that it was impossible to tell if these differences were innate or due to social custom. Terman and Miles promised more definitive results from the masculinity-femininity test then under construction. The final results of the project by Terman and his group were published in two volumes in 1936 and 1938.  

Terman and his colleagues' conclusions, as Henry Minton has pointed out, accorded with the social and cultural norms and expectations of the period. Women with advanced degrees scored lower on femininity than housewives and non-professional working women. Men of academic achievement
scored lower on measures of masculinity than men whose interests were mechanical and athletic. Among homosexual males, there was a positive correlation between measures of masculinity-femininity and sex role in homosexual intercourse. Homosexual men who preferred the "male role" in sexual intercourse were more masculine than those homosexual men who preferred the "female role." In general, Terman and colleagues seemed to believe that mentally healthy and happily married individuals were characterized by masculine men and feminine women.

Terman and his group at Stanford extended the use of a well-established psychological topic and method, individual differences and mental testing, to a new area: the role of sex differences in personality traits of masculinity-femininity. Although Terman had high hopes for his project, it probably had less lasting impact than his longitudinal studies of genius or his work on the Stanford-Binet intelligence test. Terman’s project in this area was well-supported by CRPS funds. Like the work of Wells, Stone, Yerkes, and Hamilton, it was a project that fit within the agenda of social control and social engineering that was a prominent feature of science and science managers in the interwar period.

Conclusion

New opportunities for the extension of psychological knowledge and practice were made possible in the 1920s by
social concerns about human sexual behavior. I have characterized this opportunity as the opening of a new borderland of sex research. Psychologists used this opportunity to find a niche in the new borderland of the scientific investigation of sex. Sexuality had traditionally been under the professional hegemony of medicine in America, although in the years from 1900 to 1920 other voices had been increasingly heard on the topic of sex.

Under the aegis of the National Research Council’s Division of Medical Sciences, a massive, well-funded effort to bring the perspective and methods of science to bear upon sex began in 1921. Oversight to the project was given to a group formed for the purpose, the Committee for Research in Problems of Sex. The original goal was to gain control of human sexual behavior through scientific understanding. Looking at the sex project from a broad perspective, it was part of the attempt financed by philanthropies during the interwar years to control human society through scientifically informed social engineering. As such, it was one of many like-minded projects begun in this period which have been delineated by others.

I have shown in this chapter that psychologists were vitally involved in this borderland from its inception to its end. A psychologist, Earl Zinn, made the original proposal to investigate sex scientifically. The
psychobiologist, Robert Yerkes, was critical to shepherding the idea through the labyrinth of the National Research Council. Yerkes set a precedent for psychologists through his role as a science bureaucrat; he demonstrated leadership, persistence, and offered a workable plan for psychobiological approaches to sex research.

I have also shown in this chapter that psychologists extended the range of their professional authority by providing psychological knowledge about sex. This expansion of the range of their discipline was along a shared boundary with medicine. The research methods and locales developed by psychologists in the years leading up to 1920 were utilized by them to gain a space in the new borderland during the years from 1920-1930. Psychologists used established research strategies and locales in new ways as the borderland of sex research opened up. First, I showed that the pattern of collaboration between a physician (psychiatrist) and psychologist already established in the years 1904-1920 was continued by Fred Wells at Boston Psychopathic Hospital from 1921-1926. He and his psychiatrist colleague collaborated to explicate norms of sexual behavior. Wells used this opportunity to move beyond the role of psychometrician into that of interpreter of human personality and its place in human sexual behavior. I called this the collaborative approach.
In the comparative approach, I demonstrated that animal research strategies were employed by psychologists to throw light on basic underlying mechanisms of sexual behavior. Comparative psychology, while never the mainstream of American psychology, offered insights about human sexual behavior by looking at its analogue in animals. Within this approach, Calvin Stone established the first consistent program of sex research. He and his students worked on questions of the role of heredity and learning, cerebral and hormonal influences, and motivation through their sex research. The programmatic nature of Stone's work, the involvement and support of graduate students in that program, and the interdisciplinary nature of the resultant publications were important in extending psychology's disciplinary boundaries.

The work of Robert Yerkes and his group at Yale's Institute of Psychology was also done from the comparative approach. The Institute represented an investment of the Laura Spelman Rockefeller Memorial in the advancement of knowledge and, through that advancement, the amelioration of social problems. Yerkes' group of investigators apparently had an eye to the human application of their comparative research. Their work on motivation, sexual development, and sexual behavior was amenable to human engineering and eugenic concerns. Yerkes and his colleagues at Yale were like a "pilot plant" for human engineering, to use Donna
Haraway’s phrase. But they were also a pilot plant for the extension of psychology’s organizational boundaries. The comparative work at the Institute of Psychology was a test project for the utilization of the comparative approach in a self-consciously structured teamwork setting. Although one may argue that teamwork was not the priority of the project’s participants, Yerkes did use his group’s research at the Institute of Psychology to justify the institutionalization of his work in the new Institute of Human Relations at Yale in the 1930s.

Thirdly, I showed that comparative approaches informed the attempts of the CRPS to understand sex factors in marital adjustment. G. V. Hamilton, a physician trained in comparative psychology, used the comparative method to understand human sexual behavior in the marital research center funded initially through the CRPS. I demonstrated that his work informed the sex research of his contemporaries, as well as of later, more prominent, researchers like Alfred Kinsey.

Fourth, I demonstrated that the widespread interest in individual differences, with its attendant focus on psychometrics, found expression in sex research as an attempt to explain gender differences in non-intellectual traits. Lewis Terman, at Stanford University, a pioneer in mental testing, pioneered a new application of mental tests for individual differences: the role of sex differences in
personality traits of masculinity-femininity. He extrapolated his results to discuss marital happiness, homosexuality, and psychological adjustment.

The boundaries of psychology were different at the end of the 1920s than they were at the decade's beginning. Psychologists who were leaders of their discipline used the opportunity created by social concerns and the response of philanthropic foundations to those concerns to extend the range of psychological knowledge, expertise, and organizational patterns. It is an important part of the story, never fully told, of American psychology.

Notes

1. Robert M. Yerkes, The Scientific Way [manuscript autobiography], p. 229. Yerkes Papers, Manuscripts and Archives, Sterling Library, Yale University, New Haven, CT.


9. Ellis was prolific. A good example of his writing about sex is, Sex in Relation to Society, vol. 6 of Studies in the Psychology of Sex (New York: Davis, 1910).


22. ibid., p. 150.


25. I am indebted to Donald Dewsbury for pointing this out to me.


33. Kohler, Partners, ch. 1.

35. For the context of the development of NRC and its similarity to a trade association, see Hawley, Search for a Modern Order.


37. ibid., ch. 2.


41. The National Committee on Mental Hygiene was formed in 1909 from the efforts of Clifford Beers, a former mental patient. Beers's moving account of his experience, A Mind that Found Itself, brought him sufficient support from physicians, psychiatrists, and other concerned citizens to initiate a movement to reform the care of mental patients and to work toward prevention of mental illness. On Beers, see Norman Dain, Clifford W. Beers: Advocate for the Insane (Pittsburgh: University of Pittsburgh Press, 1980).

42. Thomas Salmon to Henry Christian, June 8, 1921. National Academy of Science\National Research Council\Division of Medical Studies\Committee for Research in Problems of Sex\Beginning of Program. National Academy of Science, Washington, DC.

44. See Bullough, "Katherine Bement Davis, Sex Research," p. 82. Earl Zinn, "Outline presented by Mr. Zinn to Dr. George McCoy," no date, but before July 1, 1921, as this was when McCoy's tenure as Chairman of the Division of Medical Sciences ended. NAS:NRC:Division of Medical Sciences:CRPS:Beginning of Program (National Academy of Sciences, Washington, DC).

45. Max Exner to John D. Rockefeller, Jr., 7 June 1921, NAS:NRC:Division of Medical Sciences:CRPS:Beginning of Program (National Academy of Sciences, Washington, DC).

46. Yerkes told the initial NRC conference called to discuss the feasibility of sex research sponsorship that Zinn first approached him in late 1920, see below, note 31. Cf., Yerkes, "Scientific Way," p. 228.

47. Aberle and Corner, *Twenty-Five Years*, p. 10.

48. ibid., p. 228.

49. Clark Wissler to Earl Zinn, 11 May, 1921, National Academy of Sciences (NAS), National Research Council (NRC), Committee for Research in Problems of Sex (CRPS), Administration, Initiation of Program (National Academy of Sciences, Washington, DC).


51. Salmon, the chairman of the National Committee on Mental Hygiene, wrote a circular letter to enlist support for Zinn's proposal. He cited the lack, even among physicians, of accurate knowledge of sex. Interestingly, along with the letter, he included a copy of Watson and Lashley's, "The Opinion of Doctors Regarding Venereal Disease," *Mental Hygiene* 4 (1919): 769-847, as evidence in support of the need for a scientific inquiry into human sexuality. Thomas W. Salmon to medical group (circular), no date, but before July 1, 1921, National Academy of Sciences, National Research Council, Beginning of Program (National Academy of Sciences, Washington, DC).

52. "Request for an Appropriation of $10,000 to the Bureau of Social Hygiene to be used in Promoting the Working Out of a Plan for Research in the Field of Sex," June 1921, National Academy of Sciences, National Research Council, Committee for Research in Problems of Sex, Beginning of
Program (National Academy of Sciences, Washington, DC).

53. Earl Zinn to A. L. Kroeber, 7 October 1921.
NAS:NRC:Division of Medical Sciences:CRPS:Beginning of Program (National Academy of Sciences, Washington, DC).


55. "Memorandum for Dr. Vaughan Concerning Program of Research on Sex Behavior," Robert Yerkes to Dr. Victor Vaughan, 22 October, 1921, National Academy of Sciences, National Research Council, Committee for Research in Problems of Sex, Conferences (National Academy of Sciences, Washington, DC).


57. Seashore recounted his experiences in diverse applications of psychology at the University of Iowa in, Pioneering in Psychology (Iowa City, IA: University of Iowa Press, 1942).

58. O’Donnell, Origins of Behaviorism, esp. ch. 11.


61. ibid., p. 22.

62. ibid., pp. 22-23.

63. ibid., p. 23.


65. Haraway, Primate Visions, p. 76.


69. See Steven J. Cross and W. R. Albury, "Walter B. Cannon, L. J. Henderson, and the Organic Analogy," *Osiris*, 2nd series, 3 (1987): 165-192. Cross and Albury argue that the interwar period was characterized by a change in the analogy that served as source and guide for researcher and society alike. This change was from a medical analogy of degeneracy in society to an organic analogy of the need for cooperation to maintain societal stability.

70. I cite Aberle and Corner here, *Twenty-Five years of Sex Research*, p. 17. My own search of the archives also yielded only these two initial plans. Bernard Glueck, at the behest of Thomas Salmon, outlined a potential program for human sex research in November, 1923. Whether this program was officially submitted to the full Committee is unclear from the archival record.


72. "Preliminary report of the Committee for Research on Sex Problems to the Division of Medical Sciences of the National Research Council, March 15, 1922." National Academy of Sciences\National Research Council\Division of Medical Sciences\Committee for Research on Problems of Sex: Formulation of Program, National Academy of Sciences, Washington, DC.

73. "Minutes of meeting of Committee on Sex Problems, December 12, 1921." National Academy of Sciences, National Research Council, Committee on Research in Problems of Sex, Beginnings. National Academy of Sciences, Washington, DC. One clear indication of the human focus was the interest on the part of the Committee in securing information from universities where physical education was a part of the curriculum.


81. That Watson and Lashley's publication was highly influential in the initiation of sex research by the NRC is supported by a document in the NAS archives: "Preliminary Report of the Committee for Research on Sex Problems to The Division of Medical Sciences of the National Research Council, March 15, 1922." NAS: NRC: Division of Medical Sciences: CRPS: Beginnings. National Academy of Sciences, Washington, DC. The anonymous author(s) lists Watson and Lashley's article as critical in establishing the need for scientific research in sex. See the Salmon quote below from the same archives.

82. The amount of the grant was $6600. This was a substantial sum for research of any kind in this period. It was larger than any grant made for psychological research into sexual questions until Lewis Terman received $12,000

83. On the ISHB, see Allan Brandt, *No Magic Bullet*, pp. 88-91. Many of these women were interned in detention camps until they were declared non-infectious.


85. Aberle and Corner, *Twenty-five Years*, p. 123.


87. See, especially, volume 4 of his *Collected Papers*. Meyer became a member of the Committee in December 1928 and did not resign until April, 1945.

88. There is little biographical information available on Lee. His autobiography, *The Happy Life of a Doctor* (Boston: Little, Brown, 1956), is a rambling, unfocused collection of anecdotes that mostly stresses how many important and distinguished individuals he knew in his lifetime. Oddly, it is also an apologia for being fat.


Hall Clark University to Psychoanalysis," ibid. pp. 29-62; references to Wells are on pp. 31, 54-57.


94. See the reviews by Wells of recent work in psychodynamic theory that appeared in Psychological Bulletin from 1912-1915.


99. Peck had problems in attempting to help those students who were homosexual. He had arranged for various departments of Harvard to send men who admitted any homosexual preferences or actions to him as part of his study. As it turned out, he got few referrals, owing to the "over-conservatism" of the Harvard administration. See, Martin Peck to Earl Zinn, Nov. 29, 1924. NAS:NRC:Div. of Med. Sciences:CRPS:Grantees. National Academy of Sciences, Washington, DC.


107. Robert L. Dickinson and Lura Beam, A Thousand Marriages (Baltimore: Williams and Wilkins, 1931) and idem, The Single Woman (Baltimore: Williams and Wilkins, 1934). Dickinson received funds from the Bureau of Social Hygiene in 1925, 1929, 1930, and 1931 for his Committee on Maternal Health. Lura Beam also wrote a biography of her lover and former Executive Secretary of the Committee on Maternal Health, Louise Stevens Bryant, Request from a Life (New York: Author, 1963). Bryant had also been trained as a psychologist under Lightner Witmer at the University of Pennsylvania.


117. ibid., pp. 129, 140-155. Yerkes rather disingenuously characterized his course in educational psychology as due to the "compulsion of this growing interest in the study of man as natural object." It is probably fair to say that the real compulsion was the pressure from Harvard's President Lowell, who made it clear to Yerkes that his future success at Harvard lay in application.


119. ibid., pp. 54-55.

120. For example, Yerkes in 1923 spearheaded the formation of the Directors of Industrial Research, a loose, informal association of business executives, who met once a month to discuss research possibilities and plan strategies for furthering business interests. This is yet another example of how Yerkes participated in that overlapping group of men who were influential in shaping the direction of American science and business. On Yerkes and the Directors of Industrial Research, see Olivier Zunz, "Producers, Brokers, and Users of Knowledge: The Institutional Matrix," in Modernist Impulses in the Human Sciences, 1870-1930, ed., Dorothy Ross (Baltimore: Johns Hopkins University Press, 1994), pp. 290-307.

121. Robert M. Yerkes, "The Relations of Psychology to Medicine," Science 53 (1921): 106-111. Donald A. Dewsbury argues that Yerkes' first published self-designation as a psychobiologist appeared in this article. See his, "'Psychobiology,'" American Psychologist 46 (1991): 198-205, for a discussion of the tradition of psychobiology as the investigation of the "dynamics of whole organisms viewed not reductionistically but in all of their splendid complexity" (p. 198). Yerkes' work was an early example of this tradition.


124. Dewsbury, in "Man with a Dream," notes that Yerkes' dream of a primate research station dated from 1900. The proposal for psychobiology at Johns Hopkins is in a document entitled, "Memorandum on Psychobiological Research, Prepared with special reference to the Johns Hopkins University." The correspondence with Meyer and others is dated late 1920 and early 1921.


126. In an unpublished biography of the Yerkes family, Yerkes' wife, Ada, wrote that in the summer of 1919 Robert had discussed his plans for the anthropoid station with a number of important and influential scientists and science administrators, including, Thomas Hunt Morgan, Edwin Conklin, John C. Merriam, and James Rowland Angell. In 1920, Robert was offered a site for the station on Marco Island off the west coast of Florida by the newspaper magnate, E. W. Scripps, but for unspecified reasons turned it down. She also notes that during the years 1921-24, Robert devoted more and more thought and effort to his "plans for a research laboratory for the study of behavior, using the anthropoid apes as subjects, especially the chimpanzees." Ada Watterston Yerkes, "Family Matters: A Record, Part 3," unpublished manuscript, 1940-1957, pp. 11, 13, 16.

127. Aberle and Corner, *Twenty-Five Years*, p. 17. I was not able to find any document by Lashley that corresponded with this statement in my search of the NRC archives. It does not appear likely that the program Yerkes submitted (see Appendix A) was prepared by Lashley.

128. ibid., p. 121.


131. Dewsbury, "Triumph and Tribulation."


134. ibid.


136. Kart Lashley to Lewis Terman, 28 February 1922. Microfilm, Calvin Stone family.


139. Dewsbury, Comparative Psychology in the Twentieth Century, p. 96.


141. For a summary of this debate, see Dewsbury, Comparative Psychology in the Twentieth Century, pp. 90-93. Also, see Nancy Innis, "Tolman and Tryon: Early Research on the Inheritance of the Ability to Learn," American Psychologist 47 (1992): 190-197, for an interesting result of this debate in the work of Edward C. Tolman.


143. See Dewsbury, Comparative Psychology in the Twentieth Century, p. 90.


154. C. R. Carpenter, "Psychobiological Studies of Social Behavior in Aves. I. The Effect of Complete and Incomplete Gonadectomy on the Primary Sexual Activity of the Male

155. Dewsbury, Comparative Psychology in the Twentieth Century, p. 103.

156. Haraway, Primate Visions, pp. 84-111. Citations on p. 86.


163. "Definite Announcement by Yale of Important Development in Psychological Research," folder 827, LSRM, Rockefeller Foundation Archives, RAC.

164. Robert M. Yerkes, "Preliminary Formulation of Plan for Institute of Psycho-Biology at Yale University," Spring, 1924, folder 827, LSRM, Rockefeller Foundation Archives, RAC.
165. ibid.


170. ibid., p. 159.


174. Yerkes, "1928-29 of the Institute of Psychology."


177. Robert M. Yerkes, The Dancing Mouse and The Mind of a Gorilla (New York: Arno Press, 1973). His studies were originally reported as a series of monographs in Genetic
Psychology Monographs 1927 and Comparative Psychology Monographs 1928. He received CRPS funding for at least one of the trips.


181. Beardsley Ruml, "Recent Trends in Social Science," box 2, folder 12, RG 3.1, Rockefeller Foundation Archives, RAC.

182. See Kay, Molecular Vision of Life and Haraway, Primate Visions.


184. Salmon and the Committee also investigated the possibility of establishing a center for human sex research at Yale under the direction of Arnold Gesell. The Committee tabled Gesell's proposal on September 4, 1923. NAS:NRC:Med. Sciences:CRPS:Projects:Research Centers:Psychobiology of Sex in Man:Proposed. National Academy of Sciences, Washington, DC.

185. On Clark, Glueck, and Kirby's early commitment to psychoanalysis, see Burnham, Psychoanalysis and American Medicine; on Zinn, who became an analyst after leaving the CRPS, see Shakow, "Contributions of Worcester State Hospital," pp. 37-39.


187. Robert Yerkes to Earl Zinn, August 22, 1928. The context is Yerkes warning to Zinn to proceed carefully in the matter of joint sponsorship of family studies with the Social Science Research Council, "in order to avoid a repetition of the sort of fiasco that resulted from our attempt to arrange for study of problems of marital relation." NAS:NRC:Div. Med. Sciences:CRPS:Projects:Research
Centers: Marital Research. National Academy of Sciences, Washington, DC.

188. For example, see William Healy and Augusta Bronner, *Judge Baker Foundation Case Studies* (Boston: Judge Baker Foundation, 1923). The team approach of a physician and psychologist working together to produce viable results was also modeled by Healy and Bronner.


190. Ogburn became a member of the Committee the next year, although Rosenberg never contributed to the center.

191. This section was based on a memorandum from Earl Zinn to Katharine Davis, "Memorandum to Dr. K. B. Davis. Subject: Center for the Study of the Problems of Marriage, 28 February 1924." NAS:NRC:Div.Med.Sciences:CRPS;Projects:Research Centers:Marital Research. National Academy of Sciences, Washington, DC.


194. Aberle and Corner, *Twenty-Five Years*, p. 16.


196. G. V. Hamilton, "An Experimental Study of an Unusual Type of Reaction in a Dog," *Journal of Comparative Neurology and Psychology* 17 (1907): 329-341.

198. idem, An Introduction to Objective Psychopathology (St. Louis: C. V. Mosby, 1925).


203. Hamilton, Objective Psychopathology.


209. G. V. Hamilton, "Proposed program for the Institute for Research on Human Sex Problems, October 1924."
Centers:Marital Research. National Academy of Sciences,
Washington, DC.

210. ibid.

    C. Boni, 1929).

212. Alfred C. Kinsey, Wardell B. Pomeroy, and Clyde E.
    Martin, *Sexual Behavior in the Human Male* (Philadelphia:

213. G. V. Hamilton, "Comparative Psychology and
    200-211.

214. ibid., p. 207, emphasis added.

215. ibid., p. 209.


217. G. V. Hamilton, "Changes in Personality and
    Psychosexual Phenomena with Age," in *Problems of Ageing*,
    ed., E. V. Cowdry (Baltimore: Williams and Wilkins, 1942),
    pp. 810-832.

218. Cf., his comments in *Objective Psychopathology*,
    Psychopathologists seem to have overlooked the possibility
    that such pathological responses as those of the
    masturbated spinster may have a scientific knowability and
    manageable physiological determination fundamentally like
    that to which we ascribe the abrupt movements of withdrawal
    and the reported pain of a patient whose abscess is being

219. Robert Yerkes to Earl Zinn, 10 July 1928.

220. Robert Yerkes to Earl Zinn, 22 August 1928.

221. See Dewsbury, "Triumph and Tribulation."

222. Lewis Terman to Earl Zinn, "Report of Progress on Sex


224. Lewis Terman to Earl Zinn, "Proposed program for sex research for year 1925-1926, Beginning December 1, 1925. National Academy of Sciences, Washington, DC.


228. ibid., p. 106.

229. ibid., p. 108.

230. Terman, "Proposed program for sex research for year 1925-1926."


233. Terman and Miles, Sex and Personality. They report the data on homosexuals on pp. 236-258.
Scientists commenting on the tremendous gain which has accrued to us during the last decades of specialization, are calling attention to the fact that many of our most vital problems lie between the sciences and cannot be even perceived without going beyond the confines of a specialty.

(H. Flanders Dunbar, 1935)

The present time is an age of integration in which the functional, as contrasted with the purely logical, aspects of science are coming to the fore. There is an insistent urge to utilize all scientific knowledge obtainable for the public welfare in the innumerable ways now open for the application of knowledge in industry, in governmental administration, in public welfare and in other portions of our social order. Such problems do not accommodate themselves to the limits of our traditional fields of learning. Integration of knowledge from varied sources, brought to bear upon a single social purpose or end, is characteristic of the present scientific situation.

(Frank R. Lillie, 1936)

In all societies, the social sciences have taught us, boundaries are integral to social structure.

(Ludmilla Jordanova, 1986)

In this chapter I trace the role of psychologists in the development of a new borderland of experimental investigation and clinical practice: psychosomatic medicine. Psychosomatic medicine emerged as a distinct field of interdisciplinary research and practice in the late 1930s. It was then concerned with the "interrelationships between
emotional life and bodily processes both normal and pathological.\textsuperscript{4} These interrelationships were thought to be at the crux of such diverse illnesses as colitis, peptic ulcers, and asthma. Standard histories of psychosomatic medicine focus almost entirely on the role of physicians, especially psychoanalytically oriented physicians, in its development.\textsuperscript{5} This received view traces the evolution of psychoanalytic theories of psychological influences on bodily processes from Freud’s concept of the symbolic conversion of psychic distress into somatic expression to the physiological representationalist views of Franz Alexander and Flanders Dunbar.\textsuperscript{6} Important psychoanalysts who are made to fit in this unproblematic lineage include Sandor Ferenczi, George Groddeck, Felix Deutsch, Alfred Adler, and Smith Ely Jelliffe.\textsuperscript{7}

The received view leaves a gap in the historical understanding of this interdisciplinary field. In this chapter, I attempt to narrow the historical gap by focusing on the contribution of psychologists to the fashioning of psychosomatic medicine. I wish to show that psychologists made critical contributions in two ways. The first was through research in experimental psychopathology. I argue that psychologists provided the experimental underpinning for the emergence of psychosomatic medicine as a recognized and coherent field of practice and theory. Secondly, psychologists provided leadership in the shaping of the
field. I wish to show that psychologists utilized lessons gained in the formation and growth of sex research to implement a framework that allowed psychologists, psychiatrists, and psychoanalysts to collaborate on problems of mutual interest. Through these two broad and critical contributions to the making of a new borderland of science, psychologists extended the range of psychological practice along the boundary shared with medicine.

Introduction

In 1939, a new scientific and professional journal, Psychosomatic Medicine, was launched under the auspices of the National Research Council (NRC). One historian of psychosomatic medicine observed that the new journal was "an event of singular importance for the development of psychosomatic conceptions and medicine." The journal served to give the nascent field a coherent, research oriented identity instead of a loose, anecdotally-based set of convictions about the importance of psychological factors in health or disease.

From the beginning, psychosomatic medicine was multidisciplinary in nature. The story of its emergence is complex: it involved psychologists, psychiatrists, psychoanalysts, and medical practitioners of various orientations. It also reflected a philosophical reorientation in the life sciences toward organicism and away from reductionism; this was especially true in the
explanation of the relations between mind and body. Philanthropic interests were also represented in the development of this new borderland. Philanthropists' concern with social order led them to reach into medicine, especially psychiatry in the 1930s, and the related life sciences, especially psychology, for answers and techniques to facilitate their agendas.

I must emphasize that psychosomatic medicine was a new field of interdisciplinary investigation. It did not exist as a distinct and recognizable field prior to the last half of the 1930s. As was recognized at the time, psychosomatic medicine was a classic example of borderland science; it was fashioned from the contributions of psychoanalysis, psychobiology, and experimental psychopathology. Psychologists had stakes in all these. For example, psychologists' research in psychopathology was often informed by psychoanalytic concepts. This body of research provided a point of contact between psychologists and physicians and became a bridge for the entry of psychologists into the "borderland" of research that led to the formation of the field of psychosomatic medicine.³

The converging interests of psychologists, physicians, philanthropies, and the National Research Council resulted in 1937 in the formation of the Committee on Problems of Neurotic Behavior, a "Borderlands" committee under the auspices of the National Research Council. I focus on the
role of psychologists in the formation of this committee and show how they attempted to emulate the successful program of sex research begun in the 1920s, though with considerably less success. The archival record of this Committee is a fascinating account of the attempt to fashion a new field of scientific inquiry. An important outcome of this Committee was the founding of the journal, *Psychosomatic Medicine*. This chapter, then, is an account of how psychologists came to play such an important part in the fashioning of a borderland science.

**The NRC and Borderlands Science**

As shown in the previous chapter, cooperation in science became a watchword among American scientific leaders and the managers of philanthropic foundations in the years after World War I. One clear example of this ethos of cooperation was the Committee for Research in Problems of Sex (CRPS), but there were also other such committees staffed by scientists from different disciplines working together on projects of mutual interest. Frequently, these interdisciplinary committees were formed to address some issue of social concern, as in the CRPS and the Committee on Problems of Human Migration. Both of these committees were generously funded by philanthropic foundations.¹⁰

By the mid-1930s, the NRC was actively promoting interdisciplinary research, now termed "borderlands" science in its annual summaries. An interdivisional committee was
formed in 1935 to foster research in overlapping areas of physics, chemistry, and geology. The success of this committee led Frank Lillie, then chairman of the NRC, to suggest that a similar approach might prove productive for those borderlands "between biology, psychology and anthropology and the medical sciences."11 In January, 1937, the NRC formed the Interdivisional Committee on Borderland Problems in the Life Sciences. It was this Committee that sponsored the series of conferences and events that led to the formation of the Committee on Problems of Neurotic Behavior (CPNB). The work of the CPNB in turn was critical for the establishment of the new field of psychosomatic medicine. I return to this below.

Foundations and the Support of Borderlands Science

The interest of the NRC in promoting cooperative, multidisciplinary research was supported by philanthropic foundations, especially the Carnegie and Rockefeller Foundations. Glenn Bugos has shown that in the 1930s the terms "borderlands" and "cooperative", like the word "interdisciplinary" in contemporary science, were the "passwords to the coffers of the patrons of science."12 In the 1930s, the Rockefeller Foundation refined its policy of promoting the advancement of knowledge through cooperative science. In this section I briefly discuss the Rockefeller Foundation's expansion of support for psychiatry and related sciences and its support of institutionalized
interdisciplinary efforts. A particular example presented is Yale’s Institute of Human Relations.

In January 1931, Alan Gregg became director of the Rockefeller Foundation’s Division of Medical Sciences. Gregg was educated at Harvard, where he became interested in psychology through the work of William James. He also became interested in psychoanalysis at this time through his contact with Sigmund Freud, Sandor Ferenczi, and Carl Jung at James Jackson Putnam’s vacation retreat after the three psychoanalysts had spoken at Clark University. These two early influences were nurtured over the years and proved important for psychology and psychosomatic medicine.

Gregg earned his MD from Harvard Medical School and after World War I took a position with the Rockefeller Foundation in Brazil, doing medical work on hookworm disease. Gregg eventually became the assistant to Dr. Richard Pearce, the director of the Rockefeller Foundation’s Division of Medical Education. When Pearce suddenly died in early 1930, Gregg became the Director of the renamed Division of Medical Sciences.

Alan Gregg played an important role in the shaping of American psychiatry and allied sciences through his careful administration of financial support during the 1930s and 1940s. He was a pivotal figure as well in the Rockefeller Foundation’s support and direction of Yale’s Institute of Human Relations. I address first Gregg’s direction of
funding for psychiatry and then discuss the relation of that support to psychologists working in psychiatric settings. These two foci are important to my argument concerning the role of experimental psychopathology to the development of psychosomatic medicine, as I shall make clear below. 

Alan Gregg and the support of psychiatry and allied fields

During the tenure of Alan Gregg as the Director of Division of Medical Sciences (1931-1951), some sixteen million dollars were given by the Rockefeller Foundation to support research in psychiatry, psychoanalysis, and the neurosciences. This support represented the majority of the Foundation's support for medical sciences in those years. Why were these fields, especially psychiatry, so well-supported? The answer partly lies in the perception of psychiatry by Foundation officers and trustees and many members of the medical profession. Despite many professional gains made by psychiatry from the turn of the century to the late 1920s, the specialty still had the stigma of medical shabbiness to many who cared to look its way. As the author of the 1933 Rockefeller Foundation proposal for support of psychiatry wrote in answering the question of why so much support should be given the field, "Because it is the most backward, the most needed, and the most probably fruitful field in medicine." Efforts began at the Rockefeller Foundation under Richard Pearce's direction to fund projects that would benefit psychiatry.
Under Gregg’s direction, the pace increased considerably. Gregg was determined to use the Division’s resources to take psychiatry out of its backwardness. He had a vision to vitalize psychiatry through building up the sciences that he saw as underlying the field, as well as providing support for more traditional clinical psychiatry. Gregg wrote in 1933, "Explicit recognition should be given to two considerations: advances in the sciences fundamental to the field are most likely to bring the greatest aid to understanding, and the mind is not presumptively separate from the body."16

The sciences that Gregg perceived as fundamental to psychiatry were neurology, neurophysiology, neuroanatomy, psychology, psychobiology, endocrinology, and genetics.17 Gregg focused especially on psychology as fundamental to psychiatry, once commenting that, "As physiology is to the practice of medicine, psychology is—or should be—to the practice of psychiatry."18 From 1933 to 1941, the Rockefeller Foundation gave nearly nine and one-half million dollars to support projects that fell under Gregg’s plan.19

Within this time span, Gregg also developed an alliance with Warren Weaver and the Division of Natural Sciences to develop a Science of Man that included a generous place for psychiatry and psychology under the rubric of "psychobiology".20 Weaver and Gregg’s partnership lasted until 1937, gradually becoming an alliance in word only. It
does show, however, that the Rockefeller Foundation was
strongly committed to a broad, interdisciplinary approach to
attacking questions of human behavior and that psychiatry
and psychology figured prominently in those plans.21

The projects supported under Gregg’s plan of attack
varied widely across psychiatry, neurology, and allied
sciences such as psychology. Gregg also directed funds
toward the support of psychoanalysis. Franz Alexander and
the Institute for Psychoanalysis in Chicago were generously
supported for research in psychosomatic illnesses, as was
the work of Stanley Cobb at Massachusetts General Hospital.
Apart from Alexander at Chicago, several institutions where
work important to the development of psychosomatic medicine
was conducted were supported. These included Worcester
State Hospital, Columbia University College of Physicians
and Surgeons, Cornell University, Johns Hopkins School of
Medicine, Harvard University, Brown University, Yale Unive-
rsity, and Yale University’s Institute of Human Relations.22
(See Table 3.1 for complete listing and amounts of financial
aid.)

The Institute of Human Relations: A case study in
cooperative science

While Gregg’s principal focus was on psychiatry and
allied sciences, he also became involved in the allocation
of funds for Yale University’s Institute of Human Relations
(IHR). The IHR became an important center of research
activity on topics relevant to experimental psychopathology.
Table 3.1 Rockefeller Foundation Support of Psychiatry and Related Fields—Relevant to Development of Psychosomatic Medicine, 1934-1941

<table>
<thead>
<tr>
<th>Institution</th>
<th>Support</th>
</tr>
</thead>
<tbody>
<tr>
<td>Institute for Psychoanalysis Chicago, IL</td>
<td>$220,000.00</td>
</tr>
<tr>
<td>Yale University-Psychiatry New Haven, CT</td>
<td>300,000.00</td>
</tr>
<tr>
<td>Yale University-Institute of Human Relations New Haven, CT</td>
<td>700,000.00</td>
</tr>
<tr>
<td>Worcester State Hospital Worcester, MA</td>
<td>136,500.00</td>
</tr>
<tr>
<td>Cornell University (H. S. Liddell) Ithaca, NY</td>
<td>65,700.00</td>
</tr>
<tr>
<td>Columbia University</td>
<td>99,000.00</td>
</tr>
<tr>
<td>College of Physicians and Surgeons New York, NY</td>
<td></td>
</tr>
<tr>
<td>Johns Hopkins University (W. H. Gantt) Baltimore, MD</td>
<td>107,650.00</td>
</tr>
<tr>
<td>Harvard University (H. Murray) Cambridge, MA</td>
<td>60,000.00</td>
</tr>
</tbody>
</table>

Total $1,688,850.00

Source: Alan Gregg, "A Record of Support During the Years 1931-1941 of Psychiatry, Neurology, and Closely Related Subjects, Appended to Speech 'What is Psychiatry?' 3 December 1941," folder 19, box 2, series 906, RG 3, Rockefeller Foundation Archives, RAC.

Psychologists at the Institute who conducted research in this field included Neal Miller, John Dollard, Robert Sears, Carlyle Jacobsen, Walter Miles, and Catherine Miles. One significant focus of their psychopathology research centered around the experimental testing of psychoanalytical concepts. Their work was truly cooperative and represented the closest approximation of the founding intentions of the IHR.
The Institute of Human Relations grew out of the Institute of Psychology at Yale. Its central idea was to bring together all the fields relevant to "the study of human behavior in its individual and group relations," with the hope to "place on a rational and sound basis the understanding of individual human behavior." The fields to be included were psychology, mental hygiene and psychiatry, physiology, pathology, anatomy, anthropology, sociology, law, and theology. One rationale offered was that by bringing researchers together from so many different fields "contacts and cooperation so essential to the successful integration of the subject matter" would be accomplished. The Rockefeller Foundation in 1929 provided generous funding for the proposed Institute of Human Behavior.

In the new Institute of Human Relations (behavior was dropped from the name in favor of the more inclusive "relations"), psychiatry, psychobiology, and psychology were generously funded. Psychiatry, under the direction of Eugene Kahn, included the psychologists Walter and Catherine Cox Miles. Walter Miles established the Laboratory of Physiological Psychology within the Department of Psychiatry. Robert Yerkes' Laboratories of Comparative Psychobiology were made part of the Department of Physiology. Clark Hull's Habit Laboratory was in the Department of Psychology. In each of these three areas
important work was done that contributed to the development of experimental psychopathology and the incorporation of that approach to the understanding of psychosomatic disease.

Through its support of psychiatry, psychoanalysis, and psychology, the Rockefeller Foundation played a critical role in the research that eventually led to the development of psychosomatic medicine and the provision of institutional resources to conduct that research. The Rockefeller Foundation's support was part of their long-range plan of social control through understanding. The advancement of knowledge was key to this plan. Beginning in the 1920s with such projects as Yale's Institute of Psychology and continuing through the 1930s with projects like Yale's Institute of Human Relations, the Rockefeller Foundation sought to implement its objectives with little regard for disciplinary or professional boundaries. The Rockefeller Foundation's provision of funds worked to stimulate interdisciplinary and multidisciplinary approaches to the solution of concrete human problems. Perhaps without such provision, psychologists, psychiatrists, psychoanalysts, and animal researchers would not have cooperated on common projects or even have been open to each other's ideas. Psychologists used the opportunity to work in close settings with psychiatrists and psychoanalysts, to extend the range of their disciplinary practices, and to extend their cultural authority, just as they had done with the
opportunity provided by the massive funding of sex research in the 1920s. In some ways, the work of psychologists along the interface with medicine in the first three decades of this century was preparatory for the events of the 1930s and the establishment of psychosomatic medicine.

**Psychoanalysis and Experimental Psychopathology**

In this section, I focus on the role of psychoanalytic ideas in psychological research. I do so because psychoanalysis provided an impetus for much of the research on experimental psychopathology conducted by psychologists in the 1930s. This body of work formed one of the pathways to the development of psychosomatic medicine at the end of the decade. Psychoanalysis also provided a point of contact between psychologists and physicians who were working on questions of psychosomatic relationships in health and disease. First, I discuss in general terms the reception of psychoanalysis by psychologists in the first three decades of this century. Then, I discuss the research in experimental psychopathology conducted by psychologists during the 1930s and early 1940s. I wish to show that psychoanalysis was influential in shaping this research, especially when problems in human lives were a focus of experimental or clinical investigation. The confluence of experimental psychopathology and psychoanalysis facilitated communicated between psychologists and psychoanalytically
oriented physicians, making it possible for them to jointly
develop the new borderland of psychosomatic medicine.

_Psychoanalysis and Psychology: General Considerations_

The reception of psychoanalysis in America in the first
twenty years of this century was marked by both hostility
and acceptance. John Burnham has thoroughly documented both
in his _Psychoanalysis and American Medicine, 1894-1918_. Though Burnham focuses on the reception of psychoanalysis by
physicians, particularly psychiatrists and neurologists, he
does note that some American psychologists, primarily
William James and G. Stanley Hall, also made important
responses to Freud's ideas (or the Americanized version of
those ideas). Nathan Hale in his _Freud and the Americans_
also charts the beginnings of psychoanalysis in America over
a similar period of time. Hale, like Burnham, focuses on
psychoanalysis and medicine, but does provide a broad
perspective on cultural influences on the reception of
psychoanalytical ideas and is more inclusive of
psychologists involvement with psychoanalysis. David Shakow
and David Rapaport provide the most comprehensive view of
the influence of Freudian ideas on American psychology.
They are concerned primarily with showing the positive
influence of Freud on academic psychology.

Shakow and Rapaport suggest that the obstacles to
understanding psychoanalysis by psychologists were many. For
example, there was no good reliable source for a clear,
systematic formulation of psychoanalytic concepts; there were internal inconsistencies in psychoanalytic theory; Freud's ideas concerned both clinical application and theory, with no clear delineation between them; psychoanalytic theory was difficult to prove or disprove; and, perhaps, most irritating, psychoanalysts claimed that only those who had been analyzed could understand the theory and the practice of psychoanalysis. As a result of obstacles like these, Shakow and Rapaport claimed, psychologists trained to privilege exact methods and to formulate testable hypotheses only had partial understanding of psychoanalytic concepts and this partial understanding usually led to rejection of psychoanalysis or modification of psychoanalytic ideas into terms more amenable to mainstream American psychology.

More recently, Gail Hornstein has characterized the relationship of psychoanalysis and American psychology as problematic. She argues that psychoanalysis represented a threat to academic psychology and that during the period under consideration in this chapter, American academic psychologists retreated into positivism. This, Hornstein argues, was followed in the 1940s and 1950s by a strategy of co-optation or incorporation of psychoanalytic concepts through experimentation. Her conclusion is puzzling, given that a number of experimental trials of psychoanalytic concepts were undertaken in the 1930s, as I will show below.
Central to all these accounts of psychoanalysis in America is the prominence given by it to the power of the irrational in human lives. The notion of powerful, biological impulses arising in the unconscious of humans and motivating human behavior in unpredictable ways was difficult for many psychologists trained in more exact sciences to accept. Popular reception of psychoanalysis, or at least popularized versions of psychoanalysis, began among the more literary and artistic elements in society. But by the 1920s, psychoanalysis had become something of a fad, according to John Burnham. Barry Richards also locates the influences of Freud on American culture as part of a response to alienation and isolation due to the disruptive powers of urbanization and industrialization. Shakow and Rapaport argue that these popular responses to psychoanalysis resulted in pressure on academic psychologists to address the issues raised by psychoanalysis, both in the college classroom and in their experimental work.

Many psychologists did respond, especially in college classes, to the challenge of psychoanalysis. For example, Lewis Terman in his 1932 autobiography, acknowledged the importance of Freudian theory by stating that psychoanalysis was "one of the two most important contributions to modern psychology, mental tests being the other." Neal Miller stated that while he was a student of Terman's at Stanford,
Terman pointed out to him the importance of Freudian ideas. Calvin Stone taught a course in Freudian psychology at Stanford. His notes for the course have been preserved and show a sympathetic understanding of Freud's ideas and his influence on American psychology. Stone also spent a morning in intense conversation with Freud's British disciple, Ernest Jones, during his long visit to Europe in the spring of 1932. Stone's notes from the meeting show that he was deeply impressed by Jones and by Freud's concepts, although he expressed reservation about the mixture of theory and therapy in analytic writings.

Beyond the classroom, psychologists also responded to the challenge of psychoanalysis in their research. In 1930, Flugel suggested that, "In view of the vast benefits that psychology would be likely to derive, if psychoanalysis could be made amenable to experimental technique, the attempt seems emphatically to be worth making." The decade of the 1930s saw a number of psychologists make the attempt.

**Psychoanalytic Influences on Experimental Psychopathology**

My focus in this section is on the response to psychoanalytic concepts by psychologists in the 1930s who worked in settings where human problems, especially mental and emotional problems, were addressed. The settings for my argument include both psychiatric and academic laboratories or clinics. My argument centers around the notion that
psychoanalysis became a focus of interest, positive or negative, for the psychological investigation of human mental and emotional problems in this time period. The term that has been used for this research is experimental psychopathology. While both human and animal subjects were employed in this research, the focus was on human problems. It is my contention that experimental psychopathology formed one of the pathways to psychosomatic medicine and that the interest in psychoanalytic concepts facilitated the interaction of psychologists and psychoanalytically-oriented psychiatrists that led to its formation. It is also worth noting that the psychologists involved in this work were not marginal figures in the history of psychology; most of them fashioned a place for themselves by making contributions that have been perceived as important in psychology's development. In this section, I locate these individuals and consider their experimental research on psychoanalytic concepts.

Worcester State Hospital

David Shakow, the late "father" of modern clinical psychology, discussed the involvement of psychologists in experimental psychopathology at Worcester State Hospital, Worcester, MA, in two major papers. In the first, he described the collaborative research of psychologists in the schizophrenia program at the hospital. In the second
paper, Shakow discussed the role of psychoanalysis in the work of psychologists at the hospital.  

The Schizophrenia Research Service at Worcester State Hospital was established in 1927 through a grant from Katherine D. McCormick. Mrs. McCormick's late husband, Stanley, had suffered from schizophrenia and had been treated by G. V. Hamilton for nearly ten years at the family estate in Santa Barbara, California. (This was the same estate where Robert Yerkes had conducted research with the orangutan, Julius.) Katherine McCormick evidently was so dissatisfied with Hamilton's treatment that she forbade the use of her grant for psychological research. After consultation with the noted physiologist, Walter Cannon, Mrs. McCormick asked the endocrinologist, Roy Hoskins, to draw up a plan for an organic approach to the cure of schizophrenia. Hoskins did so and was asked to direct the research team at Worcester. As described by Shakow, the Research Service at Worcester State Hospital was truly interdisciplinary. Endocrinologists, internists, psychiatrists, psychologists, and statisticians collaborated on a broad program of research aimed at understanding the complexities of the disease.

**Personnel.** Shakow joined the group in 1928 as Chief Psychologist and Director of Psychological Research. The experimental psychologist, Paul Huston, joined in 1930 and Saul Rosenzweig became a member of the research team in
1934. A number of other psychologists served on the staff of Worcester during the 1930s, including four women who were to become well-known after they left the hospital: Tamara Dembo, Eugenia Hanfmann, Marie Rickers-Ovsiankina, and Anne Roe.

According to Shakow, psychoanalytic concepts were widely discussed among the personnel at the hospital and were influential in shaping research. Shakow described himself as being influenced by Fred Wells, who was one of the first American psychologists to be sympathetic to Freudian concepts. Raymond R. Willoughby, a psychologist at nearby Clark University, was also influential in directing the staff’s attention to psychoanalytical concepts during this period. Other personnel at the hospital who were psychoanalytically-oriented included Earl Zinn and the psychologists, J. McVicker Hunt and Saul Rosenzweig. Zinn, formerly Executive Secretary of the NRC’s Committee for Research in Problems of Sex (see previous chapter), became an analyst after leaving the Committee and joined the Research Service at Worcester in 1933. He analyzed Shakow and several other staff members, as well as conducting psychoanalytically-oriented research on schizophrenia. He left Worcester for Yale’s Institute of Human Relations in late 1935. Saul Rosenzweig came to Worcester already committed to an empirical approach to investigating psychoanalytic concepts. Rosenzweig became interested in
psychoanalysis very early; his undergraduate honors thesis was on psychoanalysis and philosophy.\textsuperscript{43} I return to his initial work at Harvard University below. Hunt later worked at the New York State Psychiatric Institute and at Brown University. In all three locations, psychoanalysis was an important influence on his research. This group at Worcester State Hospital produced an impressive body of research during the 1930s, much of it guided or influenced by psychoanalytic concepts.

Research. The initial focus of the research group at Worcester was on schizophrenia. Funds from the McCormick grant supported research in endocrinology and other organic approaches. The psychological and psychiatric research was funded by regular hospital monies until the Rockefeller Foundation initiated a ten-year program of support in 1934 (see Table 3.1). Despite differences in funding sources, there was a common attack made on describing and understanding schizophrenia. Shakow characterized the multidisciplinary research as initially concurrent and collaborative, progressing to integrated, cross-disciplinary work.\textsuperscript{44} Theoretical viewpoints were diverse; included among the psychological staff were behavioral, gestalt, hormic, and psychoanalytical viewpoints. Research was also undertaken with non-schizophrenic subjects, usually with the hope that results would shed light on the patient population
of the Hospital. I consider here the psychoanalytical approach taken by Shakow and Rosenzweig.

From 1930 to 1932, Shakow, Huston, and the psychoanalytical psychiatrist Milton Erikson attempted to experimentally investigate the induction of psychological conflict in normal subjects. Their approach allowed them to consider the psychoanalytical concepts of repression, abreaction, and complexes. A stated goal of their research was to determine the applicability of their method to the psychoanalytic session. Although Shakow et al. were unable to draw final conclusions concerning these concepts and methods, their work does indicate an early interest in the employment of experimental methods to test psychoanalytical concepts. Shakow also suggested the usefulness of this interdisciplinary approach and the incorporation of psychoanalytical concepts in his contribution to the 1934 National Research Council-supported volume on mental disorders.

Saul Rosenzweig’s doctoral dissertation was an investigation of the Freudian concept of repression. After coming to Worcester State Hospital from the Harvard Psychological Clinic in 1934, Rosenzweig developed an ongoing program of research centered around the testing of psychoanalytic concepts. In a review paper published in 1937, he suggested that recent research efforts indicated that psychologists were finding ways to narrow the gap
between the clinical methods and subject matter of psychoanalysis and the experimental methods of psychologists.48 Certainly, Rosenzweig was among the leaders in this attempt. While he investigated a variety of research topics, his central concern was with the effects of frustration.

Rosenzweig was an articulate spokesman for the incorporation and testing of psychoanalytic ideas by psychologists engaged in experimental psychopathology. For example, Rosenzweig chaired a symposium at the annual meeting of the American Psychological Association (APA) in 1938 on "Frustration as an Experimental Problem." Rosenzweig argued for the usefulness of frustration as a heuristic for research into both normal and abnormal behavior.49 The symposium illustrated Rosenzweig's argument: the participants, all from different universities, each reported on the use of frustration in their area of research. George Haslerud reported on a comparative approach to the understanding of frustration from his work with chimpanzees, while Quin Curtis linked frustration to physiological changes and compared his results with those of H. S. Liddell of Cornell. O. H. Mowrer, from Yale's Institute of Human Relations, reflected on the sociological implications of frustration and psychodynamics and Roger Barker reported on the effects of frustration on cognition in children.
Beyond its heuristic value, Rosenzweig and his like-minded colleagues envisioned their work on frustration as part of the rapprochement between psychoanalysis and academic psychology. These psychologists believed that their adherence to the methodological demands of experimental psychology would profit psychoanalysis by lending precision and definition to its sometimes vague formulations. On the other hand, academic psychology, which was often charged with irrelevance in its research, would benefit from the experimental investigation of psychoanalytic concepts because real problems from everyday lives were confronted in the subject matter of psychoanalysis.

Yale University's Institute of Human Relations

An impressive group of psychologists was gathered together at the Institute of Human Relations during the 1930s. Some were prominent when they joined the Institute, others became so as a result of their work at the Institute. Their research at the Institute employed both animal and human subjects, but the guiding principle was the need to address human problems.

Experimental psychopathology and psychoanalysis were interwoven in the research conducted in several of the Institute's laboratories. Treatment issues were also the focus in two laboratories. I will briefly discuss each of
the four groups of psychologists at Yale, although there was considerable interaction and overlap among them.

**Laboratory of General Experimental Psychology.**

Psychologists at the Laboratory of General Experimental Psychology under Raymond Dodge's leadership were concerned with standard research topics in academic psychology. They were also involved in research and treatment of problems of human psychopathology. As early as 1930, Dodge and other members of the Laboratory were cooperating with psychiatry in research and treatment.\(^5^0\) Ernest Hilgard, later a leader in American psychology, worked in Dodge's laboratory with a patient who suffered from hysterical blindness. Hilgard, along with others, treated a patient with hysterical disabilities by using conditioning techniques. He also was involved in the treatment of a patient with hysterical analgesia; shock was paired with a skin prick and over the course of a week the analgesia disappeared.\(^5^1\) Hilgard was very sympathetic to psychoanalysis. His presidential address to the American Psychological Association (1949) was the first such address to be largely concerned with Freudian themes, according to Shakow and Rapaport.\(^5^2\) Robert R. Sears, as a graduate student in 1932, was involved with an experimental investigation of a female hysterical patient suffering from sensory and motor disabilities.\(^5^3\) Sears, whose work in Dodge's laboratory led to a long-time interest in unconscious processes, took a job at the University of
Illinois in 1932, where he began to test psychodynamic concepts of personality and abnormal behavior. He returned to Yale in 1936 and became involved in the group working on the translation of psychoanalytic concepts into learning theory formulations.\textsuperscript{54}

**Laboratory of Comparative Psychobiology.** When the Institute of Human Relations opened, Robert Yerkes was placed in the Department of Physiology. His Laboratory of Comparative Psychobiology became a center of primate research in this country. Psychobiology for Yerkes was the study of the whole organism.\textsuperscript{55} Although not identical with the concept of psychobiology espoused by the psychiatrist, Adolf Meyer, Yerkes use of the term bears a family resemblance.\textsuperscript{56} At this same time, Alan Gregg and Warren Weaver of the Rockefeller Foundation were using the term "psychobiology" as a descriptor for the multi-disciplinary approach to the formation of a proposed Science of Man.\textsuperscript{57} Although the term was used differently by various individuals, it reflected a movement in the medical, life, and social sciences toward an organismic or holistic approach to understanding the complexity of living organisms. Yerkes' promotion of the whole-organism approach to the understanding of psychopathology fit within this larger movement in the life sciences.

Yerkes had a longstanding interest in psychopathology, having spent four years (half-time) at Boston Psychopathic
Hospital (1913-1917). His friendship with psychiatrist G. V. Hamilton and their mutual interest in the use of comparative methods in psychopathology research also reflected his involvement with the field. When the proposal was made to establish the Yale Institute of Psychology in the early 1920s, the use of primate research to throw light on psychiatric problems was one of the central emphases. 58 Yerkes and his wife Ada reiterated the potential usefulness of the comparative approach to psychopathology in Problems of Mental Disorder (1934). 59 The Yerkes suggested seven avenues in comparative work that could shed light on problems of human mental disorder. They argued that the psychobiological approach with primates other than man would be of greater assistance to the understanding of mental pathology than actually using human subjects.

After preparation of the chapter in Problems of Mental Disorder, Robert Yerkes reported that he planned to develop a program of research in comparative psychopathology, "the distinctive feature of which should be utilization of the chimpanzee for experimental inquiries in which human subjects may not be used." 60 Studies in psychopathology conducted under the auspices of the Laboratory over the next few years included work on drug addiction, frustration, and experimental neuroses. The last was a long-term project of Glenn Finch, who began the study in 1936 and completed it in 1941. 61 Carlyle Jacobsen, whose major research focus was
neurophysiology, developed a strong interest in psychopathology, especially neuropathology, while at Yerkes' laboratory.

Jacobsen, a University of Minnesota PhD (1928) under Karl S. Lashley, became a research associate at the Yale Laboratories of Comparative Psychobiology in 1930. While at Yale, Jacobsen became involved in research with the noted neurophysiologist, John F. Fulton, chairman of the Department of Physiology. Together, they developed an interdisciplinary program of research that had profound psychiatric applications. Jacobsen had full use of the psychological equipment from the Laboratory of Comparative Psychobiology and Fulton provided the first-rate surgical facilities of his Laboratory of Primate Physiology. While in this collaborative enterprise, Jacobsen conducted the surgical separation of the frontal lobes of two chimpanzees, Becky and Lucy, that resulted in such highly significant behavioral changes that Jacobsen remarked that it was as though Becky had joined a "happiness cult." The publication of this work and the public presentation of similar results contributed to the development of psychosurgery as a therapeutic intervention with psychiatric patients.

Jacobsen's work, with its implications for psychiatry, was part of the effort to place the investigation of human psychopathology on an experimental basis. His research
fit within the framework of the experimental neurosis model formulated by H. S. Liddell (Cornell) and W. H. Gantt (Johns Hopkins). In 1937, Jacobsen left the Yale laboratories for Cornell, where he could work more closely with Liddell.

Laboratory of Physiological Psychology. Walter Miles and his wife, Catherine Cox Miles, were brought to the Institute of Human Relations in 1932, where Walter Miles established the Laboratory of Physiological Psychology in the Department of Psychiatry and Mental Hygiene. The stated purpose of bringing Walter and Catherine Miles to the Institute was for "bringing the fields of psychology, psychiatry, and physiology into a closer relationship in teaching, research, and clinical work." In order to accomplish this end, the Laboratory was given an unusually large share of the Rockefeller Foundation's allocation for psychiatry. The Miles's did clinical work and research in both normal and psychopathological processes in their laboratory. Their 1934 report stated that psychological assessments had been made on a variety of individuals referred to them. These assessments included mental tests as well as measures of psychophysiological functions. Walter Miles sought to develop psychological assessment procedures that would be useful for diagnosis in psychiatric cases. Work of both an interventionist nature and assessment was also conducted with inpatients at the New Haven Hospital. Opportunities for training in clinical
psychology were available and Walter Miles taught a seminar for medical students "for the purpose of making psychology a part of the medical curriculum."

Research topics listed in the Laboratory's annual report of activity for 1933-1934 included studies on morphine withdrawal, drug addiction, and effects of training on lobectomy patients. Walter Miles and his graduate research assistant, Neal Miller, conducted a study of the Freudian concept of regression. This was one of the first studies of psychoanalytic concepts to come out of the Institute.

Miller, later noted for his research in motivation and learning, was a research assistant with Walter Miles from 1932 to 1935. With Miles's encouragement, Miller went to Vienna on Social Science Research Council Fellowship for a year, where he underwent a didactic psychoanalysis. Miller then returned to the Institute where he played a critical role in reformulating and testing psychoanalytic concepts as part of Clark Hull's research group in the Habit Laboratory.

Habit Laboratory. Clark Hull came to Yale from the University of Wisconsin in 1929 and began his systematization of learning principles while at the Institute. Soon after he came to Yale, Hull embarked on a project to utilize the Pavlovian conditioned reflex to build a deductive system of explanatory principles to encompass all behavior. Warren Weaver, director of the Rockefeller
Foundation’s Division of Natural Sciences, described Hull in 1934 as the American Pavlov, who "uses Yale sophomores instead of dogs."  

What is often overlooked in narratives of Hull is his interest in the applicability of his research to human problems. For example, in the mid-1930s, he and Walter Miles began a study on frustration and the psychoses. Among the Habit Laboratory research projects listed in a 1934 report were, "experimental production of psychoneurotic symptoms, using conditioned reflex technique on rats, dogs, monkeys, chimpanzees, children, and adults; and influence of punishment, frustration, success in bullying, etc., in youth on later character of animals from rats to chimpanzees." In the fall of 1935, Hull decided to initiate a program to correlate the tenets of psychoanalysis with conditioned reflex theory. A potential avenue of influence on Hull in this direction came from the psychoanalysts, Thomas French and Lawrence Kubie. French, the associate director of Chicago’s Institute for Psychoanalysis, had published a lengthy paper in 1933 that suggested a number of possible links between psychoanalysis and Pavlov’s conditioned reflex theory. Kubie, in 1934, published a discussion of the possible integration of conditioned reflexes into psychoanalytic therapy techniques. Regardless of influence, Hull provided leadership among psychologists at
Yale in the attempt to reformulate psychoanalysis into testable hypotheses from 1935 to the end of the decade.

Testing psychoanalysis at the Institute of Human Relations. In the fall of 1935, Hull began holding an informal seminar every Wednesday evening where an interdisciplinary group discussed possible relations between conditioned reflex theory and psychodynamic formulations. As Jill Morawski has pointed out, Hull’s efforts were pointed toward bringing the irrational under the control of orderly, systematic science and were part of his larger project for a unified science. Although Clark Hull initiated the efforts to incorporate psychoanalysis into learning theory, it was the younger personnel of the Institute who did the work of testing psychoanalytic concepts.

When Mark May became the director of the Institute of Human Relations in 1935, he decided that the fulfillment of the Institute’s goal of integrated, interdisciplinary research lay with the younger members of the staff. Clark Hull shared this view, telling Alan Gregg in 1935 that the younger members of the Institute’s staff were interested and excited about the exchange of ideas and the possibilities of cooperative research, whereas the older men were not.

The Wednesday evening seminars were open to the entire staff of the Institute. The discussions were led by Hull
and some of the more junior members, such as John Dollard (Dollard had undergone a didactic psychoanalysis with Hanns Sachs of the Berlin Psychoanalytic Institute). Seminar discussions during the first year centered around possible links between learning theory (Pavlov, Thorndike, and Watson) and the clinical approaches of Freud, Adler, and other psychoanalytic theorists. Hull was especially interested in reformulating psychoanalytic tenets in the terms of his emerging behavior system. The sessions were well attended and served to stimulate serious consideration of psychoanalysis. After the first year, the seminar was oriented toward Hull's program for a unified social science. At this point, a new discussion group led by the younger staff members began meeting on Monday nights.

Upon Miller's return from Vienna, he and the sociologist John Dollard formed the Monday Night Group. This group was a fluid, interdisciplinary mix of junior faculty and graduate students. Like Hull's Wednesday evening seminars, the Monday Night Group addressed psychoanalytic concepts and the possibility of testing them empirically. Among this group were Miller, Dollard, Robert R. Sears, O. H. Mowrer, Leonard Doob, and Carl Hovland. Earl Zinn, having moved from Worcester, was also a regular participant, as was Erik Erikson. These individuals sought to integrate methods from psychology, sociology, and anthropology in order to develop a coherent attack on real
human problems. Out of their discussions came several important publications that attempted to incorporate psychoanalysis into learning and motivational theory. Psychoanalysis was attractive to them precisely because it dealt with human suffering. As John Dollard put it, they wanted to study actual cases.83

One approach to "actual cases" came through the Freudian notion that aggression is caused by frustration. The attempt to state this idea in a testable form led to extensive collaborative work by Dollard, Miller, Mowrer, Sears, and Doob. They attempted to utilize the Freudian notion that humans seek gratification of biological urges such as sex, thirst, or hunger. In this process, humans are bound to be frustrated by the nature of socialization, in that social forces present obstacles to the satisfaction of such desires. Humans, like other organisms, react to this frustration with aggression.

The early results of their collaboration came in 1939 with the publication of Frustration and Aggression.84 The authors acknowledged the formative influence of Freud by stating that he "more than any other scientist has influenced the formulation of our basic hypothesis."85 Dollard et al.'s basic postulate was that "aggression is always a consequence of frustration."86 The book was an elaboration of this basic theme, with the authors drawing on their own empirical investigations of psychoanalytic
concepts, as well as that of others. One conclusion drawn by the authors was that most adult aggression was the result of unresolved frustrations of childhood. In difficult times like the Depression era, Dollard et al. suggested, the frustration might lead to individual psychological problems, as well as to social problems such as crime, dysfunctional families, and labor unrest. Here were many potential "actual cases!"

The research of the Monday Night Group reflected the impetus of their benefactors, the Rockefeller Foundation, and their own felt need for relevance to real life problems. Psychoanalysis provided a heuristic while the experimental tradition of psychology gave them a rational methodology; life offered the problems to be addressed.

The participants in the Monday Night Group tested various psychoanalytical concepts beyond those presented in *Frustration and Aggression*. For example, Neal Miller sought to place the Freudian concept of displacement within the framework of stimulus-response generalization. He first presented this work in 1939, but the full expression of his attempt was not published until 1960. Miller, in his work on approach and avoidance of conflict, interpreted some of his results as supporting Freud's suggestion that we are often drawn to things we fear. O. H. Mowrer conducted experiments on anxiety and worked to relate Freudian notions of anxiety to the experimental induction of neuroses. He
rooted his explanations in the Hullian concept of drive reduction. Robert Sears sought to empirically test Freud’s concepts of repression and projection.90 Sears and C. I. Hovland collaborated on studies of aggression and frustration that served as empirical underpinning for the volume on *Frustration and Aggression*.91

The research of the psychologists at Yale clearly illustrates the convergence of the experimental tradition in psychology and the theoretical tradition of psychoanalysis. This convergence occurred in the context of concern about problems of human mental functioning. As in the work at Worcester State Hospital and the research described below, this convergence of theory and research provided a point of contact between two groups that was to be instrumental in the fashioning of psychosomatic medicine.

New York State Psychiatric Institute

When Carney Landis was hired as the head of the new Department of Research Psychology at the New York State Psychiatric Institute, the Institute already had a long tradition of innovation in research and management.92 Under the Institute’s first director, Ira Van Gieson, psychological research was conducted by Boris Sidis (see Chapter 1). Adolf Meyer then served as director from 1901 to 1910. Meyer reorganized and standardized the Institute’s assessment and record-keeping methods. More importantly, he brought a pluralistic perspective to the clinical work.
Meyer encouraged the use of many approaches to conceptualizing and treating the Institute's cases. By the end of Meyer's tenure, these included psychoanalytic approaches. When Meyer left to become director of the Phipps Psychiatric Clinic at Johns Hopkins, August Hoch became director of the Institute. Hoch was oriented to a psychoanalytic approach to psychiatry and under his directorship the Institute moved more in that direction. Hoch brought the psychologist Fred Wells to the Institute in 1911. Wells stayed only a brief time before returning to Boston. In the period from 1911 to 1930 when Landis was hired, the Institute became a center in America for the utilization of psychoanalysis in the treatment of patients and for the development of psychoanalytic theory.  

Landis was an ardent experimentalist, deeply committed to the laboratory approach to empirical investigation. He earned his PhD under Karl Lashley's direction at the University of Minnesota in 1924. Prior to his appointment as associate research scientist at New York State Psychiatric Institute, he had been a National Research Council Fellow and an assistant professor at Wesleyan University. By the time he came to the Institute, Landis had already established himself as an important investigator of the psychophysiology of emotion; a field to which he continued to make contributions.
Landis remained at the Psychiatric Institute until his death in 1962. During his years at the Institute, Landis sought to place the understanding of human psychopathology on a solid experimental basis. He recruited a number of younger psychologists to work with him in these investigations. Among this younger group were Joseph McVicker Hunt, William A. Hunt, Zygmunt Piotrowski, and Joseph Zubin.

Landis and his group's research ranged widely from the psychophysiology of emotion to psychosexuality to schizophrenia and included the experimental testing of psychoanalytic concepts. William Hunt, a long-time colleague of Landis', reported that Landis was initially very open to psychoanalysis as a necessary complement to the overemphasis of the organic approach that dominated much of psychiatry. Landis even underwent a didactic analysis himself, the results of which he reported in very negative terms in 1940. Landis' main complaints about psychoanalysis were its lack of experimental rigor and the perceived excesses of some of its practitioners. Despite his disgruntlement, Landis and the Institute's director, Clarence O. Cheney, developed plans to test the therapeutic effectiveness of psychoanalysis in an as objective manner as possible. Landis also hired the clinical psychologist, Zygmunt Piotrowski, who utilized the Rorschach Projective Technique, which is based on psychoanalytic theory, as both
a diagnostic and prognostic assessment device in the study of personality and psychoses. Although the psychologists at the Psychiatric Institute were not as involved in psychoanalysis as their colleagues at Yale or Worcester, they did not ignore the possible importance of psychodynamic factors in human psychopathology.

**Harvard University**

In a letter written to Alan Gregg in the spring of 1934, Stanley Cobb, the noted neurologist, suggested ways for integrating psychoanalysis into the university curriculum. Cobb mentioned Henry Murray and the Harvard Psychological Clinic as a good place to support psychoanalytic work in an academic setting. Murray, Cobb said, was doing the kind of objective work that lent balance to psychoanalytic theorizing. Murray was deeply involved in the Boston psychoanalytic scene, even hosting the weekly Friday meetings of the Boston Psychoanalytic Society at the Clinic. Gregg and the Rockefeller Foundation did, in fact, come to support the Harvard Psychological Clinic. This support fit with Gregg's overall plan to assist the development of psychoanalysis in this country.

The Harvard Psychological Clinic was established in 1926 through the agency of Morton Prince. Prince, 72 years old, had enjoyed a long career centered around abnormal psychology. He was considered an authority on dissociative states. Soon after the establishment of the
Clinic, Prince hired Henry A. Murray as his assistant. Murray was trained as a physician and held a PhD in biochemistry. However, he had developed an intense interest in psychoanalysis after being analyzed by Carl Jung. Murray saw his position at the Clinic as an opportunity to conduct research that would be informed by psychoanalysis.

Upon Prince's retirement in 1928, Murray became Director of the Psychological Clinic. He was opposed in this move by the chairman of the Department of Psychology, Edwin G. Boring. The friction initiated at this point between Murray and the Department of Psychology continued for many years, becoming especially intense after Karl Lashley joined the psychology department in the mid-1930s. Despite Boring and Lashley's opposition, Murray was able to attract capable students and assistants to the Clinic.

The chief focus of the Clinic's research was the exploration of psychoanalytic concepts in the formation and expression of personality. The first full expression of this body of research did not come until 1938, when *Explorations in Personality* was published. But, the research that led to the various contributions in this volume had begun much earlier. For example, Murray reported that in 1933 research was underway on various psychoanalytic concepts, such as Saul Rosenzweig's investigation of frustration and projection of blame. Rosenzweig had done his doctoral dissertation on repression while at the Clinic.
(see above). Under Murray's leadership, the Harvard Psychological Clinic was vitally involved in the experimental investigation of psychopathology and personality; psychoanalytic concepts served as guides in much of this research.

Murray's psychodynamic orientation was well-represented in *Explorations in Personality*. Murray provided the rationale for the research included in the volume, but gave considerable leeway to his students and junior colleagues in presenting their experimental approaches to personality research. Contributors included Rosenzweig, Erik Erikson, Samuel Beck, and Robert White. Nearly every facet of human personality and psychodynamic formulations of those facets was covered. A wide range of personality assessment devices was used to elicit or uncover complexes and defenses. The influence of this volume on studies of personality and motivation was immense. Murray provided leadership in the utilization of psychoanalytic techniques in the investigation of personality. Through his position at the Harvard Psychological Clinic, he was able to cultivate and encourage younger psychologists to explore the possibilities of research informed both by psychology and psychoanalysis.

*Brown University*

Brown University did not emerge as a center of research in experimental psychopathology until after 1936, when Walter Hunter became chairman of the department of
psychology. Hunter had been president of the American Psychological Association in 1930 and was chairman of the NRC's Division of Anthropology and Psychology from 1936 to 1938. His leadership of this division proved critical for the merging of experimental psychopathology and psychoanalytic interests in psychosomatic illness, as detailed below. At Brown, Hunter was able to attract a competent group of psychologists who pursued a wide range of research interests. Hunter described the research interests of the department as "oriented toward psychology and medicine."108

According to Neil Bartlett, who was a graduate student in the late 1930s and early 1940s at Brown, Freud and psychoanalysis were much discussed. The attitude among the psychologists at Brown toward Freud, according to Bartlett, was that "somehow science must seize on his [Freud's] ideas, straighten them out, and turn the magic of science and the laboratory on it and then we will truly have a theory and a practice to worship."109 The psychologists at Brown who were interested in experimental psychopathology and/or psychoanalysis included Joseph McVicker Hunt, Harold Schlosberg, and Raymond R. Willoughby. Hunt came to Brown via the New York State Psychiatric Institute and Worcester State Hospital, while Willoughby came from Clark University and Worcester State Hospital. Although psychological research and interest in psychopathology and psychoanalysis
emerged later at Brown University than the other institutions noted above, it was part of the same interest in these topics that developed earlier elsewhere in the 1930s.

Under Hunter's leadership, Hunt, Schlosberg, and Willoughby were able to pursue research in experimental psychopathology. The psychology department had links with the nearby Butler Hospital and the Bradley Home which provided research possibilities on clinical problems. Joseph Hunt continued his work on schizophrenia, while beginning a research program in the induction of experimental neuroses.\textsuperscript{110} Hunt focused on the reactions of rats placed in conflict situations, but he also devised ways to test psychoanalytic concepts. In one study Hunt compared the adult feeding behavior of two groups of rats, one of which had been allowed unlimited food as infants while the other group had been fed irregularly. As adults the irregularly fed rats tended to hoard their food, while the unlimited feeding group did not. Hunt interpreted these findings as support for the psychoanalytic assertion that early life experience is a determinant of adult behavior.\textsuperscript{111} Along with his colleague, Harold Schlosberg, Hunt attempted to experimentally induce neuroses in rats.\textsuperscript{112} In this his work was related to the experimental neurosis research of H. S. Liddell and W. H. Gantt and that of the psychologist, Norman R. F. Maier.\textsuperscript{113}
Raymond R. Willoughby brought his interest in psychoanalytically informed research in personality and social psychology to Brown. Willoughby was a graduate student of Lewis Terman's at Stanford University (PhD, 1926). While on the West Coast, he was psychoanalyzed. This experience led to an intense interest in the theory and practice of psychoanalysis that lasted until his death in 1944. Willoughby returned to Clark University upon completion of his doctoral degree, where he worked as assistant editor of *Psychological Abstracts* under the editorship of Walter Hunter. While with Hunter at Clark University in Worcester, Willoughby also worked at Worcester State Hospital. As noted above, Willoughby provided didactic psychoanalysis to several State Hospital staff members. He also had a small psychoanalytic practice in Worcester. When Hunter accepted the chairmanship of the department of psychology at Brown in 1936, Willoughby moved with Hunter to Brown in order to continue in his role as assistant editor. At Brown, Willoughby was engaged in training others to provide psychotherapy as well as conducting research. In 1939, he began investigating the development of neurasthenic behavior in mice by using the technique of varying the difficulty of the situation. In collaboration with Joseph Hunt, Willoughby published a study comparing deprivation and frustration as possible sources of hoarding behavior in rats. They concluded that, in their
study, deprivation provided a more powerful explanation than frustration. Willoughby remained a vital part of the department at Brown until his death in 1944.

Psychologists at Yale’s Institute of Human Relations, Worcester State Hospital, Harvard Psychological Clinic, Brown University, and the New York State Psychiatric Institute were influential in sparking debate and research focused on psychoanalysis. The work of these groups (except New York State Psychiatric Institute) also reflected the aims of their chief benefactor, the Rockefeller Foundation. Human problems were addressed, including problems of psychological function. The research of these psychologists illustrate that psychoanalytic concepts were a focus when psychologists attempted to deal with psychopathology. It was this pull toward finding solutions to human suffering that was one of the influences that drew psychologists and psychoanalysts together in the fashioning of psychosomatic medicine.

Convergence of Pathways to Psychosomatic Medicine

The various pathways that led to the fashioning of the new field of psychosomatic medicine began to converge in 1936. Interest in psychological aspects of physical disorders was rising among psychoanalytically-oriented physicians, as well as among those interested in constitutional medicine. According to Donna Haraway and Garland Allen, there was a widespread reorientation among
many in the life and medical sciences toward organicist/holist concepts. These concepts also contributed to the development of psychosomatic medicine through their influence on researchers and theoreticians such as Flanders Dunbar and Franz Alexander. The growth of experimental psychopathology, especially as it was intertwined with interest in psychoanalysis was a major pathway. The expression of these interests became linked with the agendas of the National Research Council and the philanthropic support of the Rockefeller Foundation and the Josiah Macy, Jr. Foundation. In this section I show how these various pathways merged to provide an institutional framework for psychosomatic medicine. I focus first on the role that psychologists played in these events, especially in providing leadership through the National Research Council. I then discuss the contributions of physicians that served as a pathway to psychosomatic medicine.

The Organizational Leadership of Psychologists

Psychologists provided organizational leadership in the shaping of psychosomatic medicine. I wish now to show that psychologists utilized lessons gained in the formation and growth of sex research to implement a framework that allowed psychologists, psychiatrists, and psychoanalysts to collaborate on problems of mutual interest.

In the annual report for 1935-1936 of the activities of the National Research Council, the NRC’s chairman, Frank R.
Lillie, suggested that an interdivisional committee in the biological sciences might serve the purpose of guiding research in the borderlands between biology, psychology, anthropology, and the medical sciences. He cited as precedent the example of the interdivisional committee of the Divisions of Geology and Geography, Chemistry and Chemical Technology, and Physical Sciences that had been formed for the purpose of research in geology through the methods of physics and chemistry. Following the publication of Lillie's report in *Science*, Lillie's successor, Ludvig Hektoen, sent a circular letter to the chairmen of the NRC's Division of Medical Sciences (Esmond R. Long), Division of Biology and Agriculture (R. E. Coker), and the Division of Anthropology and Psychology (Walter S. Hunter), inquiring as to their interest in pursuing Lillie's suggestion. Their reply in the affirmative led to a joint meeting of the three divisions in January of 1937.

The conferees from the Division of Anthropology and Psychology were led by Walter Hunter and Walter Miles. R. E. Coker led the representatives from the Division of Biology and Agriculture, while Esmond R. Long and Philip Bard led the group from the Division of Medical Sciences. The conference participants discussed potential research topics that might fall within the borders shared by the three divisions. Walter Hunter suggested the experimental study of the functional nervous disorders. He mentioned the
research at Worcester State Hospital (see above) as an example of coordinated research in this area and suggested that the experimental production of neuroses might be appropriate to all three divisions. Hunter’s idea was that the research being done in disparate places could be brought together under the umbrella of the NRC, which might result in increased opportunities for funding. 121 Esmond Long agreed that this type of work was "the research of the hour," especially in the sense that the philanthropic foundations were interested in supporting it. 122 The conferees then discussed the relevance of the work of Pavlov and cited examples of similar work in this country, notably in the laboratories at Yale’s Institute of Human Relations. Specific mention was made of work on experimental neuroses in the anthropoid station under Robert Yerkes’ direction. 123 The conference ended with an agreement that members from each of the divisions represented would draw up a list of possible lines of investigation that would fall into the borderlands shared by the three divisions.

A varied list of topics was drawn up and the first topic selected for attention was experimental neurosis and psychoanalysis. 124 Walter S. Hunter, chairman of the Division of Anthropology and Psychology, called a meeting of individuals already working in the field to be held on April 17 and 18, 1937 in Washington, DC. 125
The Conference on Experimental Neuroses and Allied Problems

The twenty-six participants at the conference included twelve psychologists. All the centers of research in experimental psychopathology were represented at the conference. The psychoanalysts, Franz Alexander and Thomas French, were present, as were leading psychiatrists from other institutions. The physiologists W. Horsley Gantt and Howard S. Liddell, both of whom had a long history of research in the experimental production of neuroses were present, as were two representatives of the Josiah Macy, Jr., Foundation, Lawrence K. Frank and Frank Fremont-Smith.

Walter Hunter, acting as chairman of the conference, began by noting the recent emphasis of the NRC on borderland problems. He cited the success of the NRC’s Committee for Research in Problems of Sex and stated his hope that a similar committee might grow out of the conference, one able to develop and direct a program of research over time. Hunter had learned from the experience of the psychologists with the CRPS and was hoping to provide leadership in a similar manner. Hunter stated that the title of the conference was meant to suggest multiple approaches to the so-called functional disorders; that the approach of Pavlov was not the only, or even the best, method of investigating neurotic behavior.

The first day of the conference was reserved for the presentation of papers relevant to the conference theme.
Extensive discussion on the problems of human neuroses was sparked by the papers and the conferees discussed ways to address those problems. On the second day of the conference, the participants met in small groups to formulate a potential program of research. All of the subgroups recommended a multidisciplinary program of research as the best way to find a solution to these problems. Specific suggestions were made for research in psychosomatic problems and for the further experimental testing of psychoanalytic concepts. The conferees generally agreed that a journal was needed to publish the results of their research and a proposal was circulated by Saul Rosenzweig for the establishment of a *Journal of Experimental Psychopathology*. After discussion of the proposals, it was decided to refer the matter of a research committee under the NRC's direction to the Executive Committees of the NRC divisions represented at the conference.

The conference was critical for the development of psychosomatic medicine. It drew together, for the first time, individuals who were working on diverse, but related, topics. As these researchers discussed their work, they found that they had much in common. The conference allowed the participants to initiate a dialogue about their research and served to break down some of the barriers to communication that prevented cooperation. The conference
also provided a model on which to organize a new borderland of science.

Robert Yerkes was the guest speaker at the evening program of the conference. Yerkes reviewed the history of the Committee for Research in Problems of Sex (CRPS). He discussed the difficulties and the successes of that committee's work. He also spoke of different types of research committees, their strengths and weaknesses. Yerkes recommended the formation of a committee like the CRPS if the conferees and the NRC decided to tackle a comprehensive program of research on neuroses. Walter Hunter and Walter Miles had just such a committee in mind.

The Formation of the Committee on Problems of Neurotic Behavior

The Executive Committees of the Divisions of Anthropology and Psychology, Biology and Agriculture, and Medical Sciences met the week after the conference to discuss the next course of action. Walter Hunter summarized the conference and asked Walter Miles to discuss it further. Miles noted the interest in experimental neuroses among the conference participants but suggested that the lack of coherent identity among interested researchers was hindering the development of the field. There were no journals, no professional society, no conferences, and no reviews, Miles stated. The Executive Committees responded by recommending that a permanent committee be set up within the Division of Anthropology and
Psychology to encourage and supervise the development of this borderland field of investigation. The committee was to be called the Committee on Problems of Neurotic Behavior (CPNB). The NRC agreed to provide funds for administrative purposes; the raising of funds for the work of the CPNB was left to its members.

The personnel of the new committee was in place by the middle of May, 1937. Walter Miles of Yale's Institute of Human Relations was its chairman. The other members were Thomas French, Institute for Psychoanalysis, Chicago, Howard Liddell, Cornell University, William Malamud, psychiatrist at University of Iowa, and Roy Hoskins, endocrinologist at Worcester State Hospital. Walter Miles wrote Walter Hunter that the first objective of the Committee was to secure funds for the promotion of the Committee's work. Miles called the Committee's first meeting for June; its agenda would be a canvass of the field. At their first meeting, the Committee members all agreed that the field of experimental investigation of the factors relevant to "human neuroses" was a "perfect specimen of borderline problems since, among other formal disciplines, it concerns physiology, endocrinology, reflexology, psychology, psychoanalysis, and psychiatry." The Committee members stated that they desired to promote and steer investigations, if they could arrange funding from one of the philanthropic foundations. In order to enhance their
chances of finding an adequate source of financial support, the Committee agreed to make a formal statement of the field, its possibilities and needs, and to authorize Walter Miles and Walter Hunter to approach the foundations for support.¹³³

Miles and Hunter found it difficult to secure adequate financial support for the work of the CPNB over the next year. The Committee developed a cogent statement of potential and needs of the field in the fall of 1937.¹³⁴ Miles and Hunter then began to approach the philanthropic foundations. They first approached Alan Gregg of the Rockefeller Foundation. Gregg’s Medical Division was already supporting most of the participants in the conference held the previous spring, but Gregg sidestepped the requests of the CPNB.¹³⁵ Miles next approached the Macy Foundation, but initially only received a promise of interest in financing any conferences the CPNB might hold in the future. The Markle Foundation was next, but Miles reported that there was only a promise to read the Committee’s appraisal of the field and statement of purpose.

Miles then reported that further discussion with Lawrence Frank and Frank Fremont-Smith of the Macy Foundation had revealed an interest on the Foundation’s part in supporting a scientific journal. Lawrence Frank, of the Macy Foundation, had been Beardsley Ruml’s lieutenant at the old Laura Spelman Rockefeller Memorial and a key player in
the promotion of cooperative research from the 1920s on. Frank had indicated to Walter Hunter, in a letter sent a few days after the April conference, that the Macy Foundation might be interested in supporting cooperative research in the borderlands represented by the conference participants. Frank told Hunter in that correspondence that the Macy Foundation was primarily interested in medicine and health, particularly in psychosomatic medicine. The link between psychosomatic disorders and experimental neuroses had been the reason for Frank and Fremont-Smith’s attendance at the conference. For some reason, Hunter had not pursued Frank’s invitation to submit a proposal for support. Now that Walter Miles had approached them with a request, Frank and Fremont-Smith suggested that the CPNB merge their interests with those of Helen Flanders Dunbar, a Macy grantee for several years. The decision of Miles and Hunter to accept Frank and Fremont-Smith’s suggestion was to prove momentous for the establishment of the field of psychosomatic medicine and the role of psychologists in it.

Physicians’ Contributions that Led Toward Psychosomatic Medicine

Flanders Dunbar: A holistic approach to medicine

Helen Flanders Dunbar was the central figure in the promotion and establishment of psychosomatic medicine in America. Dunbar was widely educated; she graduated from Bryn Mawr with majors in mathematics, pre-medicine, and psychology. She then earned a PhD in philosophy, completed
a degree in theology from Union Theological Seminary, and completed the requirements for her MD. Her psychology mentor was James Leuba, whose focus was the psychology of religion. Dunbar traveled to Europe in 1929, where she worked with the psychoanalytic internist, Felix Deutsch, a pioneer in psychosomatic medicine. Dunbar also worked as an assistant with Carl Jung at the Burgholzli Clinic in Zurich.

When Dunbar returned to America, she was prepared to integrate the viewpoints gained from her diverse training. The result of the integration was a holistic, organismic approach to the understanding of human functioning that bore a strong relationship to the whole-organism psychobiology of Adolf Meyer and William Alanson White. Dunbar's view of disease states that emerged in this time emphasized disease as disequilibrium within the person and within the environment. Emotions were perceived to play a vital role in maintaining or disrupting the person's equilibrium.

Upon receipt of her medical degree, Dunbar accepted a position at the Columbia-Presbyterian Medical Center in New York City. From this base she was able to shape the development of psychosomatic medicine. Robert Powell has pointed out that an indigenous strain of psychoanalysis, influenced by European psychoanalysis but with its own unique characteristics, developed in American psychiatry. Adolf Meyer, William Alanson White, and Smith Ely Jelliffe
were the chief proponents of this strain of psychoanalysis.\textsuperscript{142} It was this strain, interwoven with the whole-organism approach of psychobiology, that influenced Dunbar in her work at Medical Center. Dunbar was fortunate in her location; many of the medical faculty and staff at the Presbyterian Medical Center were sympathetic to or adherents of psychoanalysis and the role of psychological factors in disease.\textsuperscript{143}

George Draper was one medical colleague of Dunbar's who was committed to the investigation of the role of psychological factors in health and disease.\textsuperscript{144} Draper established the Constitutional Clinic at Presbyterian Hospital in 1916 and was its director until his retirement in 1946. Constitutional medicine, Draper stated, studied "that aggregate of hereditarial characters, influenced more or less by environment, which determines the individual's reaction, successful or unsuccessful, to the stress of environment."\textsuperscript{145} Draper developed his model of constitutional medicine over the years, eventually including four panels of personality: morphology, physiology, psychology, immunity. These four panels were collectively and individually influenced by both heredity and environment.\textsuperscript{146} Draper placed his views within the organismic/holistic movement in medicine and the life sciences, acknowledging his debt to Walter Cannon, Ivan Pavlov, Charles Sherrington, and George Coghill.\textsuperscript{147}
Research from the Constitutional Clinic, including the reports of psychologists who worked there, reflected Draper's organismal views. The first report by psychologists came in 1922. Sandor Naccarati and R. L. Lewy-Guinzburg published a study of possible relations of endocrinological factors on intelligence.\(^{148}\) This was followed in the next year by a study of the relation of body type to intelligence, especially motor aspects of intelligence.\(^{149}\) Edna Heidbreder also published a report on morphology and intelligence based on her work at Draper's clinic.\(^{150}\)

Psychoanalytical concepts were also utilized in the research of the Constitutional Clinic, as evidenced by two reports published in 1930 by the psychologist, Cecil Murray. Murray presented case studies of patients who were suffering from disorders of the digestive tract. He interpreted their case histories in the light of both constitutional theory and psychoanalysis. Murray concluded that the bodily dysfunction was due to the inability to find an acceptable solution to psychological conflict. Murray suggested that these difficulties belonged to the psychological panel of the patients' constitution.\(^{151}\) The perspective and research of Draper and his colleagues and students, then, was compatible and supportive of the views of Dunbar.

In 1931, not long after her return from Europe, Dunbar received a grant from the Josiah Macy, Jr. Foundation to
survey the extant literature on the relation of emotion to disease.\textsuperscript{152} The Macy Foundation had been established in 1930 to fund medical research and health. Its directors used the fund to promote holistic studies in medicine. Thus, Dunbar fit well within the agenda of the foundation.

The massive volume that resulted from Dunbar's survey, \textit{Emotions and Bodily Changes}, was a principal catalyst for the establishment of psychosomatic medicine as a new field within medicine.\textsuperscript{153} Dunbar reviewed and listed every relevant study published from 1910 to 1933. She drew from endocrinology, anatomy, physiology, clinical medicine, psychoanalysis, experimental psychology, biochemistry, neurology, and psychiatry, although this list is not exclusive. She stated that her "aim was to bring together all the fragments of knowledge we possess, relevant to the problem of psychosomatic interrelationships," in order to facilitate further research in the emerging field and to serve as a guide for the young scientist.\textsuperscript{154} Her aim was true. The volume did provide an impetus for further research by serving as a bibliographic guide to potential areas of fruitful research. It also served to show what had already been discovered about the relationship of mind and body in health and disease. Dunbar had contributed to this body of work, along with other physicians, most of whom were psychoanalytically or psychobiologically oriented. The 1930s were important years for the publication of theory and
research on psychological factors in disease states. A review of a few of the papers that appeared in this decade follows.

Other psychoanalytic contributions in the 1930s

John Whitehorn and Gregory Zilboorg, in a review of current psychiatric research in 1933, noted a growing trend among psychiatrists to consider multiple etiologic factors in mental disorders and an increasing concern about psychiatric factors in illness. One example that might have caught Whitehorn and Zilboorg's attention was an article by Franz Alexander. In 1931, Alexander addressed the Harvey Society on the necessity of considering psychological and physiological factors in disease processes, mental or physical. He argued that psychoanalysis provided the tools or techniques necessary for a full understanding of the psychological factors in disease. Alexander followed up his argument on the value of psychoanalysis for medicine in a paper on "Functional Disturbances of Psychogenic Nature." Alexander acknowledged the achievements of the reductionist approach in modern medicine, but also noted that many physicians recognized the importance of psychological factors in the health of their patients. What psychoanalysis offered, according to Alexander, was precision concerning those psychological factors. This precision, Alexander asserted, came from well defined, carefully observed facts. Alexander
offered as an example his work on psychological contributions to peptic ulcer. The route to peptic ulcer lay in the identification of being fed with being loved, an identification which occurs in infancy. The emotional association that occurs at this time is the baseline for the connection in adulthood between unmet dependency needs and peptic ulcer. Alexander drew upon the work of Walter Cannon on the effects on the body of emotions to argue that the physiologic linkage occurred through the action of the sympathetic nervous system.  

In 1934, Alexander organized a symposium at the annual meeting of the American Psychoanalytic Association, on psychosomatic disease. He presented the research of his group at the Institute for Psychoanalysis. The same year, Flanders Dunbar organized a symposium on psychological factors in various disease states that was presented at the annual meeting of the American Psychiatric Association. The symposiasts suggested that psychological factors were involved in hyperthyroidism (2 papers), toxic goiter, diseases of the gastrointestinal tract, and coronary heart disease. Dunbar also presented her synthesis on the relationships between physical and mental factors in illness at the symposium.

One of the most influential papers of the decade on psychosomatic disease was offered by Erich Wittkower. His survey of research on emotional factors in bodily functions
was exhaustive in its scope. The pace of psychoanalytically-oriented psychosomatic research quickened as the decade wore on.

**Psychosomatic Medicine**

As more and more research was published, a problem that was recognized by the researchers and theorists involved was the lack of formal identity of the field. There was no professional organization, no journal, no meaningful interaction between the researchers. Psychoanalytic and psychiatric journals accepted the articles of the investigators, but they were often lost in the mix with other psychiatric or psychoanalytic pieces. Not only was there a lack of identity among psychoanalytic investigators in the field, there was little opportunity for communication with the work being done in experimental psychopathology and experimental neuroses, research that was obviously related to the investigations of Alexander, Dunbar, and their colleagues.

The initiation of efforts to coordinate these approaches began with the National Research Council and was carried by the leadership of psychologists on the NRC's Committee on Problems of Neurotic Behavior, as noted above. Here the interests of the Macy Foundation and the interests of the CPNB converged. It was this convergence of interests that led to the establishment of a formal identity for
psychosomatic investigations. Psychologists were included in this identity.

Walter Miles in his meeting with Lawrence Frank and Frank Fremont-Smith of the Macy Foundation asked the two men if they would be interested in supporting a journal for research on neurotic behavior. Frank and Fremont-Smith agreed that it was a project that the Macy Foundation could support and mentioned that they had been working on such an idea with Flanders Dunbar. Frank and Fremont-Smith offered to support Miles and Hunter's proposal with the one stipulation that Dunbar's views be given precedence and that she would be the managing editor of the proposed journal. Dunbar would have to come under the umbrella of the National Research Council and its designated committee, the Committee on Problems of Neurotic Behavior. There was just one problem: Miles and Hunter had already encouraged Saul Rosenzweig in his efforts to start a journal for the field of experimental psychopathology.

Rosenzweig had circulated his proposal for a new journal at the Conference on Experimental Neuroses held in April, 1937. He had been encouraged in his efforts by Thomas French, the psychoanalyst, and by Walter Miles. In his proposal, Rosenzweig noted that the number of papers relevant to the proposed journal appeared to be increasing, but that they were dispersed throughout very disparate journals. Rosenzweig's journal would include research on
experimental neuroses, the experimental testing of psychoanalytic concepts, relevant neurological research, and psychosomatic problems. His proposed journal title was the *Journal of Experimental Psychopathology*.\(^{164}\) Rosenzweig was evidently further encouraged by Miles in the fall of 1937.\(^{165}\)

Once the officers of the Macy Foundation made their offer, contingent upon the inclusion of Dunbar and following her direction, Miles had to back-pedal with Rosenzweig. Miles suggested to Rosenzweig that publication had never been top priority for the CPNB (despite Miles' statements at the April, 1937 conference). At any rate, Miles went on, the Dunbar-led journal would serve the interests of those interested in experimental psychopathology better than any present journal. Miles urged Rosenzweig to understand that it would be foolish to throw away this chance to get major funding for a Committee project. Miles hinted that more money would follow if this project worked out and he did not want to jeopardize it.\(^{166}\)

Miles' letter to Rosenzweig prompted an exchange of letters among Rosenzweig and Miles and Walter Hunter. Hunter attempted to persuade Rosenzweig to postpone his plans in the interest of the work of the CPNB.\(^{167}\) Rosenzweig reminded Hunter that he had been working on the idea of a journal for a long time and he was worried that there would be no place for experimental psychopathology in a Dunbar-led journal.\(^{168}\) In a long letter to Miles, Rosenzweig expressed
his concern that the proposal of the Macy Foundation would not be of sufficient breadth to include work in experimental psychopathology and experimental neuroses, whereas he planned the *Journal of Experimental Psychopathology* to be inclusive of those areas and psychosomatic research. Further, Rosenzweig reminded Miles that he (Miles) had been very encouraging of the Rosenzweig journal. Due, in part, to such encouragement, Rosenzweig had arranged commitments from forty individuals to serve on a board of editors. Rosenzweig was reluctant to drop his plans without much assurance that his concerns would be addressed in the Dunbar-led journal. In August, Rosenzweig wrote Walter Hunter to say that he had dropped plans for the *Journal of Experimental Psychopathology*. By March, plans for the new journal had advanced considerably. Flanders Dunbar had agreed to the arrangement of working under the supervision of the CPNB and the Macy Foundation had agreed to provide a substantial sum to help start the new journal. A meeting of the CPNB was called for March, where the plans were laid before the entire Committee for their approval. Walter Miles stated that "here is a field that is bordering on several other disciplines," including experimental psychopathology and experimental neuroses. The Committee members all agreed that this was an opportunity worth pursuing and turned to the matter of a name for the journal and the problem of
editorial boards and numbers to appear in a year. The members agreed to name Saul Rosenzweig to the editorial board, in light of his efforts to start a similar journal. Lawrence Frank from the Macy Foundation assured them of support for five years, though in decreasing amounts as the journal supported itself through subscriptions. Flanders Dunbar joined the Committee after lunch and presented her ideas on how to manage the journal. She assured them that the journal would focus on experimental studies, rather than descriptive clinical material. Studies that included physiological and psychological aspects would be encouraged, as would animal studies. The Committee then turned to the problem of an editorial board. Those to be invited to serve on the board included Franz Alexander, Clark Hull, H. S. Liddell, and Stanley Cobb. After discussion of relevant research that might be included in the first volume, the Committee adjourned.

The Macy Foundation granted the Committee on Problems of Neurotic Behavior six thousand dollars for the launching of the new journal, to be called, *Psychosomatic Medicine*. The Committee arranged to have a circular letter sent out to potential subscribers. Walter Miles penned the letter to be sent to psychologists, while William Malamud wrote the letter for psychiatrists and neurologists. Both emphasized the interdisciplinary nature of the new journal and that it was an official undertaking of the National Research
The first issue, although delayed, appeared early in 1939. Psychosomatic medicine had begun its official life as a recognized interdisciplinary field of research and theory. It had grown out of psychoanalysis, psychobiology, and the work of psychologists in experimental psychopathology. I turn next to a consideration of the publication record of the first five volumes of *Psychosomatic Medicine*, in order to determine how involved psychologists were in this new field that they had so much to do with creating.

**The New Field of Psychosomatic Medicine**

One aspect of the development of a new field of scientific practice is the establishment of a new scientific journal that caters to the specific research interests of the relevant groups. Thomas Camfield has demonstrated the importance of journals to the professionalization of psychology from 1887-1917 and Michael Sokal has shown the importance placed on journals by the psychologist-entrepreneurs of the Psychological Corporation in the 1920s. *Psychosomatic Medicine* played an important role in facilitating an identity for those investigators from disparate fields whose work centered around neurotic behavior and its consequences for problems in living.

The 1937 Conference on Experimental Neuroses and Allied Problems had brought together many of these investigators for the first time. A recognition of commonality began
there and grew, in part, through the pages of *Psychosomatic Medicine*. Of course, there was more to these events than the overlap of intellectual or cognitive interests. The establishment of the journal also reflected the philanthropic foundations' agenda of social engineering fostered through multidisciplinary, cooperative research. And, on another level, the effort to draw together the relevant work and workers was a sincere desire to establish leadership in managing science by the two psychologists, Walter Miles and Walter Hunter. Miles and Hunter rather self-consciously sought to imitate the success of Robert Yerkes and the Committee for Research in Problems of Sex.

A survey of the first five years of *Psychosomatic Medicine* reveals that psychologists were well represented in its pages over this time. Flanders Dunbar was the first managing editor, as per the agreement between the CPNB and the Macy Foundation. The Editorial Board was dominated by psychoanalytically-oriented physicians: Franz Alexander, Dana Atchley, Stanley Cobb, and Hallowell Davis. H. S. Liddell represented the Pavlovian approach to experimental neuroses and Clark Hull represented academic psychology. The editorial Advisory Board also had one psychologist as a member, Saul Rosenzweig. By the fifth year, Clark Hull had resigned from the Editorial Board, replaced by the endocrinologist, Roy Hoskins.
A content analysis of the first five volumes reveals that 167 articles were published in *Psychosomatic Medicine*. This count includes experimental investigations, case studies, and reviews of research, but it does not include book reviews. Of the 167 articles, psychologists were authors or co-authors of 30 articles. This amounts to 17.9 per cent of the total. In volume one, psychologists were responsible for seven of the thirty six articles. In volume two, there were no articles by psychologists. Volumes three and four both contained thirty three articles, with psychologists accounting for six articles in the third volume and eight articles in volume four. In volume five, psychologists were responsible for nine of the thirty-nine articles published.

Psychologists contributed work in each genre represented in the first five volumes: experimental report, case study, and research review. Psychologists contributed, in whole or in part, six experimental reports and one research review to the first volume. This is notable since only fourteen experimental reports were published in the first volume. The same pattern continued over the next four volumes, except for volume two. Psychologists' contributions were more likely to be experimental studies than any other type: in volume three, one-half of the papers by psychologists were experimental; in volume four, three-fourths were experimental; psychologists' contributed five
experimental reports out of the total of nine in volume five. Thus, psychologists continued the role they had filled prior to the initiation of *Psychosomatic Medicine*. Psychologists had established their role as that of the experimenters through their work at Yale, Worcester State Hospital, New York State Psychiatric Institute, Harvard, and Brown. The insights into psychoanalytic tenets and/or psychopathological states via experimental investigation gained psychologists a credible voice in the new borderland that their work had helped create. I believe that it was the experimental work of psychologists that gave credibility to the new borderland in the eyes of the medical and scientific community.

**Conclusion**

Standard histories of psychosomatic medicine ignore the role played by psychologists in the fashioning of the field. I have attempted to show in this chapter that psychologists made important contributions both in leadership and experimentation. Psychosomatic medicine emerged as a new and distinct field of inquiry and practice in the late 1930s. It was a borderland science formed from the contributions of medicine, psychoanalysis, and psychology. The larger social context also was a force in the shaping of psychosomatic medicine. The agendas of philanthropic foundations, especially the Rockefeller Foundation, centered around the use of cooperative science in the service of
social or human engineering. Disciplinary boundaries were irrelevant to the managers of the foundations; they sought to encourage work in areas where interdisciplinary efforts were required. The National Research Council also encouraged interdisciplinary work through its "borderlands" policy in the 1930s. From the perspective of the foundations and the NRC, the borderlands problem of "psychoneuroses," including psychosomatic disease, represented a society out of balance. Work in this borderland presented an opportunity to address human needs through cooperative science and, in doing so, to facilitate equilibrium in society.

Psychologists brought to this borderland a history of experimental research. In particular, the practice of psychologists in mental health settings, government hospitals, psychopathic hospitals, and asylums had been slowly growing since Shepherd Franz had first taken a position at the McLean Hospital in Waverly, Massachusetts in 1904. Frederic Wells was a nurturer and carrier of this tradition right up through the time covered by this history. From small beginnings, psychologists' participation in the work of psychopathology had grown by the 1930s from a few isolated instances to a noticeable percentage of the psychological profession. With growth, psychologists were no longer found only in traditional mental health settings. Mainstream academic centers, such as Harvard, Yale, and
Brown, became sites of intense psychological research on human psychopathology.

I have shown in this chapter that where psychologists pursued research on psychopathology, they were often influenced by concepts derived from psychoanalysis. This was true at Harvard, where Henry Murray developed a cadre of individuals devoted to research informed and guided by psychodynamic principles. Among this group was Saul Rosenzweig, who left for Worcester State Hospital, where he worked for many years on the experimental investigation of psychoanalytic tenets.

Yale's Institute of Human Relations was also a center for the experimental consideration of psychoanalysis. Clark Hull initiated discussions of the possibility of merging psychoanalysis and conditioned reflex theory. His junior colleagues followed his suggestions and produced an impressive body of work that utilized the experimental approach to testing psychoanalysis. Also within the Institute, Walter Miles and his wife, Catherine Cox Miles, provided psychological services for psychiatry that involved many of the things that are considered clinical psychology today. Their research interests included psychopathological conditions. Robert Yerkes' Laboratory of Comparative Psychobiology also was involved in comparative research on psychopathology. Workers such as Carlyle
Jacobsen and Glenn Finch conducted research that had direct implications for disordered human functioning.

Similar work and results also came from New York State Psychiatric Institute and Brown University. The extensive work of psychologists in these settings served to give experimental psychology credibility in the eyes of medical professionals who were interested in psychological factors in disease states. That is, experimental psychopathology was part of the fashioning of psychosomatic medicine. The result was that the range of psychological practice was extended along the boundary shared with medicine.

Psychologists also provided the leadership that brought the various fashioners of psychosomatic medicine together. Walter Hunter was chairman of the NRC's Division of Anthropology and Psychology from 1936-1938. His familiarity with experimental psychopathology and the work of psychologists in testing psychoanalytic principles made him a knowledgeable leader who could see ways to bring individuals from disparate fields together. His chairmanship of the 1937 Conference on Experimental Neuroses was a remarkable demonstration of interdisciplinary leadership. Hunter's work on the NRC's Committee on Problems of Neurotic Behavior (CPNB) that was formed to encourage the development of the ideas presented at the conference was important to its limited success.
The chairman of the CPNB, Walter Miles, and Hunter worked hard to fashion the CPNB after the Committee for Research in Problems of Sex (CRPS). Hunter and Miles envisioned a grand research program that would draw together the researchers in all the fields relevant to the understanding and amelioration of neurotic behavior. Although they were not able to build a research program on the scale of the CRPS, their work did serve to provide a framework for the fashioning of psychosomatic medicine.

Psychologists have played an important part in this field for many years. Their experimental work grew out of the tradition begun by Shepherd Franz and Fred Wells at the turn of the century, a tradition of cooperation between psychiatrists and psychologists in psychiatric research. The experience of psychologists in initiating and managing a research enterprise in problems of sex in the 1920s also served psychologists well in the formation of a new borderland of science. Psychologists drew on the past experiences of their discipline to expand its boundaries in a new direction.

The research of psychologists on psychological factors in health and disease has proven Saul Rosenzweig's niece wrong. She once introduced her uncle as the "kind of doctor that don't make people better."¹⁷⁷
Notes


7. This evolution of psychoanalytic thinking is most clearly presented in Kaplan and Kaplan, "Historical Survey of Psychosomatic Medicine."
8. Lipowski, "What Does the Word 'Psychosomatic' Really Mean?" p. 156.


10. Other committees formed by the NRC to encourage or coordinate cooperative approaches to research included the Committee on Radiation and the Committee on the Effects of Radiation Upon Living Organisms.


15. Alan Gregg, "Notes for Rockefeller Foundation agenda meeting, 4 April 1933," folder 19, box 2, series 906, RG 3, Rockefeller Foundation Archives, RAC.

16. ibid., p. 72.

17. ibid., p. 72-73.

18. Alan Gregg, "What is Psychiatry? 3 December 1941," folder 19, box 2, series 906, RG 3, Rockefeller Foundation Archives, RAC.

19. Alan Gregg, "A Record of Support During the Years 1931-1941 of Psychiatry, Neurology, and Closely Related Subjects, Appended to Speech 'What is Psychiatry?' 3 December 1941," folder 19, box 2, series 906, RG 3, Rockefeller Foundation Archives, RAC.
20. "Interim report to Trustees meeting, 13 December 1933," folder 19, box 2, series 906, RG 3, Rockefeller Foundation Archives, RAC.


22. Alan Gregg, "A Record of Support During the Years 1931-1941 of Psychiatry, Neurology, and Closely Related Subjects, Appended to Speech 'What is Psychiatry?' 3 December 1941," folder 19, box 2, series 906, RG 3, Rockefeller Foundation Archives, RAC.


24. ibid., p. 9.

25. "Yale University, Institute of Human Behavior, Appropriation, 3 January 1929," folder 804, box 67, series 200, RG 1.1, Rockefeller Foundation Archives, RAC.


especially chapter 3.


35. Calvin Stone, "Notes for Course in Freudian Psychology," Calvin Stone material, microfilm, Courtesy of Calvin Stone Family. Only one page of these notes is dated; a page near the end of the notes is dated 20 January 1938.


42. ibid.

43. Saul Rosenzweig, Freud, Jung, and Hall the King-Maker: The Historic Expedition to America (1909) (Seattle, WA: Hogrefe and Huber, 1992), pp. 4-5.


51. Raymond Dodge, "Report on Collaborative Investigations involving the School of Medicine which have been carried on in that part of the Psychological Laboratory for which Doctor Dodge is Responsible, 12 May 1933," folder 807, box 67, series 200, RG 1.1, Rockefeller Foundation Archives, RAC.


53. Raymond Dodge, "Report on Collaborative Investigations involving the School of Medicine which have been carried on in that part of the Psychological Laboratory for which Doctor Dodge is Responsible, 12 May 1933," folder 807, box 67, series 200, RG 1.1, Rockefeller Foundation Archives, RAC.


57. See Kohler, Partners in Science, chapter 10.

58. James Angell to Lawrence Frank, "Memorandum on proposed Institute of Human Behavior, 3 November 1928," folder 826, LSRM, Rockefeller Foundation Archives, RAC.


60. Robert M. Yerkes, "Fourth Annual Report of the Laboratories of Comparative Psychobiology, July 1, 1932-June 30, 1933," Yerkes Papers, Yerkes Regional Research Primate Laboratory, Atlanta, GA.


64. On Jacobsen's role in this, see Pressman, "Sufficient Promise."


68. Cf. Alan Gregg's comment on this in "Alan Gregg, Memo of Interviews re Institute of Human Relations, 19 October 1937," folder 810, box 67, series 200, RG 1.1, Rockefeller Foundation Archives, RAC.

69. Walter Miles, "Progress in the Laboratory of Physiological Psychology, 1934," folder 817, box 68, series 200, RG 1.1, Rockefeller Foundation Archives, RAC.


73. "Office diaries of Warren Weaver, 23 & 24 January 1934, re Institute of Human Relations," folder 807, box 67, series 200, RG 1.1, Rockefeller Foundation Archives, RAC.

74. "Research Program of the Habit Laboratory," folder 807, box 67, series 200, RG 1.1, Rockefeller Foundation Archives, RAC. The list of research topics is not dated, but it can be inferred that it stems from the mid-1930s by the publication dates of completed research.

75. "Progress in Psychology, 1934," folder 817, box 68, series 200, RG 1.1, Rockefeller Foundation Archives, RAC.


80. This was repeated often by May in statements to officials of the Rockefeller Foundation. For one example, see Alan Gregg, "Notes on Interviews, 7-11 October 1935, with Personnel at Institute of Human Relations," folder 808, box 67, series 200, RG 1.1, Rockefeller Foundation Archives, RAC.

81. ibid.

82. See Mark May, "A Retrospective View of the Institute of Human Relations at Yale," Behavior Science Notes 6 (1971): 141-172. May notes that several members of the Institute had undergone a didactic psychoanalysis.

83. Alan Gregg, "Memos of Interviews re Institute of Human Relations, 19 October 1937," folder 810, box 67, series 200, RG 1.1, Rockefeller Foundation Archives, RAC.


85. ibid., p. ix.

86. ibid., p. 1.


88. Miller's published work on conflict is extensive. A useful summary of his early work on the subject is, "Experimental Studies of Conflict," in Personality and the


100. Stanley Cobb to Alan Gregg, 24 April 1934, folder 1095, box 91, series 200A, RG 1.1, Rockefeller Foundation Archives, RAC.


104. For example, Morton Prince, *The Dissociation of a Personality* (New York: Longmans, Green, 1906).

105. In a letter to Alan Gregg, written in the spring of 1936, Boring suggests that Murray's record has not been a good one and that it is doubtful that he will be granted tenure. This can be seen as an effort to undermine Murray's standing with the Rockefeller Foundation, upon whom Murray and the Clinic depended for financial support. See Edwin Boring to Alan Gregg, 11 May 1936, folder 1096, box 91, series 200A, RG 1.1, Rockefeller Foundation Archives, RAC.


107. Henry Murray, "Researches conducted at the Harvard Psychological Clinic during the year 1933," folder 1095, box 91, series 200A, RG 1.1, Rockefeller Foundation Archives, RAC.
108. Walter Hunter to Alan Gregg, 1 February 1939, folder 1, box 1, series 244A, RG 1.1, Rockefeller Foundation Archives, RAC.


110. For Hunt's work on schizophrenia while he was at the Worcester State Hospital, see J. McV. Hunt, "Psychological Loss in Paretics and Schizophrenics," *American Journal of Psychology* 47 (1935): 458-463.


121. ibid., p. 2.

122. ibid., p. 3.

123. ibid., p. 3. Presumably, it was the work of Glenn Finch that was being discussed, although Carlyle Jacobsen's research was also thought to be relevant to experimental neuroses.


128. ibid.


Washington, DC.


133. ibid., p. 5.


139. On Deutsch, see Lipowski, "What Does the Word 'Psychosomatic' Really Mean?" p. 156. Also, see Don R. Lipsitt, "Anorexia Nervosa, Felix Deutsch, and the Associative Anamnesis: A Psychosomatic Kaleidoscope," Psychosomatic Medicine 51 (1989): 597-607. A brief biography may be found in Glenn W. Flagg, "Felix Deutsch,


141. Powell, "Rise and Fall of a Medical Philosophy," pp. 135-137.


143. For another view of psychoanalysis at the Medical Center and the attached New York State Psychiatric Institute, see Schulman, Remaking the Organization.


146. Draper presented his views in a number of publications from the mid-1920s to 1946. An especially clear presentation is, George Draper, "Man as a Complete Organism-In Health and Disease," New York State Journal of Medicine 34 (1934): 1052-1063.

147. ibid., pp. 1054-1056.


153. See note 1.

154. ibid., p. viii.


161. Dunbar, "Physical Mental Relationships."


166. ibid.


170. Saul Rosenzweig to Walter Hunter, 2 August 1938, NAS:NRC:A&P:Committee on Problems of Neurotic Behavior:Proposed Journal. National Academy of Sciences, Washington, DC. I was not able to find any correspondence on this matter dated between March an August of 1939. It is not clear what prompted Rosenzweig to drop his plans.


EPILOGUE

Although this account ends in 1942, the first full year of American military involvement in World War II, the interactions between psychologists and physicians in the borderlands of science continued after the war. The magnitude of those interactions place an account of them well beyond the scope of this monograph. In this epilogue, I will briefly describe the possible ties between the events of my narrative and subsequent developments in clinical psychology and health psychology.

Links to Post-World War II Clinical Psychology

The tradition of psychological research in medical/psychiatric settings begun by Shepherd Franz and Frederic L. Wells in the first decade of the twentieth century had consequences that extended to the rapid growth of clinical psychology in the years just after World War II. Certainly, there were other developments in "clinical" psychology in the first forty years of the century that were of equal or greater significance to the post-War boom in clinical work.

Franz and Wells were able to demonstrate that psychologists could make important empirical and theoretical contributions to the understanding of mental disorders.
Their work served to give psychology credibility in the eyes of the psychiatric community. Perhaps, more importantly, Franz and Wells also helped give psychological work in psychiatric settings credibility in the eyes of their fellow psychologists and, most importantly of all, in the eyes of psychology graduate students.

Franz and Wells, and Robert Yerkes in the mid-1910s, made it possible for interested graduate students to obtain experience with the psychiatric population. For example, two graduate students who went on to highly visible careers in mainstream psychology worked with Franz at the Government Hospital for the Insane during the 1910s, E. G. Boring and Edward Strong. Gardner Murphy, later a president of the American Psychological Association (APA), worked with Wells in the late 1910s. It could be argued that Yerkes, at Boston Psychopathic Hospital, began the practice of providing internships in clinical psychology. The point is that Franz, Wells, and to a lesser degree, Yerkes, created opportunities for the expansion of psychology through their placement of graduate students in psychiatric settings.

These opportunities expanded in the 1920s and 1930s as more and more psychologists were drawn into clinical work. Again, Frederic Wells provided leadership in this expansion. One of his student assistants at the Boston Psychopathic Hospital in the 1920s was David Shakow, who has been called the "father" of modern clinical psychology. Shakow claimed
that his experience with Wells made him the first resident intern in psychology and described Wells as influential in shaping his thought and practice of psychology.¹ Shakow, in turn, provided leadership in the 1930s for a new generation of psychologists through his work and writing while at the Worcester State Hospital.

In the 1930s and into the 1940s, two successive groups of applied psychologists gave an organizational identity to individuals who were working in clinical/psychiatric settings. From 1919 to 1937, the Clinical Section of the American Psychological Association was operative and made the first attempts at certifying psychologists for applied work. In 1937 a new organization was formed that included the membership of the Clinical Section of the APA. This organization was called the American Association for Applied Psychology (AAAP). The AAAP lasted until 1945 when it was dissolved and its members became part of the Division of Clinical and Abnormal Psychology (Division 12) of the reorganized APA.² The numerical growth of these organizations in these years and its attendant strength in the reorganized APA indicate that clinical psychology was becoming a force in American psychology.

Clinical work in psychiatric settings also grew as more students sought out the experience; some remained only briefly in psychiatric work, while others made it their life work. Formal opportunities for students to work in
internships began at Worcester State Hospital. David Shakow stated in 1946 that over one hundred students had gone through internship training at Worcester State Hospital since the program began around 1930. Seymour Sarason, a noted figure in post-World War II psychology, gained experience at Worcester in the early 1940s. In his autobiography, Sarason called the clinical psychology unit at Worcester the best place in the country to receive clinical training. Sarason asserted that the work at Worcester, Yale's Institute of Human Relations, and the Harvard Psychological Clinic, "the beginning foundations for the emergence of what in post-World War II became clinical psychology." The clinical research and practice in these institutions, Sarason argued, shaped the direction of the post-War development of clinical psychology.

The experience of psychologists during World War II also helped shape the direction of clinical psychology after the war. For example, Walter Hunter and the Committee on Problems of Neurotic Behavior (see chapter 3) issued a report on the potential usefulness to the war effort of a combined effort of psychologists and psychiatrists. The cooperation that the two groups did show during the war helped prepare the way for greater interaction once the war ended. Psychologists' involvement in the war effort also turned many of them toward applied work. The experience of Wilse B. Webb is an example of this turn. Webb was a
graduate student at the University of Iowa when the war began. Webb reported that he enlisted in the Air Force under a special program for psychology graduate students that allowed him to apply his expertise in one of three Psychological Units. One of his applications was the development of methods to assess and improve data processing skills. The experience of Webb and many other psychologists during World War II served to provide a more open attitude among psychologists toward applied work after the war ended. 

And there was a great deal of applied, especially clinical, work to be done when World War II was over. Ernest Hilgard reported in 1945 that over half of all psychologists who responded to an APA survey indicated clinical interests. The supply of psychologists was not equal to the demand. As military personnel returned from war-fronts, either to home or to military hospitals, it became apparent that a great many of them needed mental health care. There were simply not enough psychologists to meet that need. One response to the undersupply of trained psychologists was made by the Veterans Administration and the National Institute of Mental Health. Both allocated large sums to the graduate training of clinical psychologists. As a result of the available funds and the inherent interest of the field, clinical psychology became the most rapidly growing field within American psychology.
By the end of the 1940s, there were over forty graduate programs in clinical psychology. Almost all of them followed a model of training devised under the leadership of David Shakow.  

The work and leadership of David Shakow and others as they forged the field of modern clinical psychology did not spring new from the earth. Rather, the field was built on the efforts of men and women such as those who were part of the narrative in chapters one and two of this study. The work of psychologists in those borderlands of science had borne fruit.

**Psychosomatic Medicine and Health Psychology**

In December, 1942, the new American Society for Research in Psychosomatic Problems held its inaugural meeting. Three hundred people attended the meeting. Among the participants were the psychologists, Walter Miles and S. S. Stevens. As it was reported in *Psychosomatic Medicine*, the new society was formed because there was "a need for a common meeting ground for representatives of diverse scientific fields interested in psychosomatic problems who, heretofore, have been able to meet only in separate sections of their several societies or in specially arranged conferences."  

*Psychosomatic Medicine* soon became the official organ of the Society. Psychologists have been frequent contributors to its pages. A survey of the 1992 (Volume 54) contents of the journal revealed that
psychologists were first authors of forty six per cent of the articles. Psychologists have also remained a vital part of the Society (renamed the American Psychosomatic Society); Karen Matthews, a prominent health psychologist, served as Society president in 1991 and other psychologists have served in various official capacities.

It can be argued that Health Psychology as a Division (38, started in 1978) of the American Psychological Association emerged, at least in part, from psychologists' participation in the American Psychosomatic Society. Health Psychology has become a popular course for undergraduates in American colleges and universities, with an attendant proliferation of textbooks. A close reading of the historical introductions of several of the popular Health Psychology textbooks was made in order to ascertain attributions of founders and important antecedents of the field. As Jill Morawski has pointed out, textbooks serve to define the boundaries of a discipline (or sub-discipline); though they also leave much to be desired in terms of accuracy. All of the textbooks that I surveyed acknowledged that the modern field of health psychology emerged out of psychosomatic medicine. Half of them, however, did not even mention Franz Alexander or Flanders Dunbar as important contributing figures or the role of psychoanalytic concepts in shaping psychosomatic medicine. The textbooks that did mention Alexander and Dunbar
acknowledged the contributions of psychoanalysis to the founding of psychosomatic medicine. Not one mention was made in any of the textbooks about the role that psychologists had played in the founding of psychosomatic medicine, nor was any mention made of the contributions of psychologists to the field in the years between 1939 and 1978. It is as though health psychology, after a long dormancy, suddenly sprang out of the soil of psychosomatic medicine. It seems remarkable that no one has ever thought to inquire about the efforts of psychologists in experimental psychopathology and the consequences of those efforts. Nor has the leadership role of psychologists in the shaping of psychosomatic medicine ever been acknowledged.

I began this study with a statement that the boundaries of psychology were extended through opportunities for application offered and taken by psychologists. I would argue that such expansion continued to be the case after the time period covered in this study. Psychologists took advantage of the opportunities that emerged from their past experiences to move in the new directions that emerged as the social and intellectual ecosystem of which they were a part shifted and changed. The opportunism demonstrated by psychologists in the cases here presented continued and continues to provide great adaptability for psychology as a science-based profession in a dynamic ecology of knowledge.
Notes


APPENDIX A
ROBERT YERKES’ PROPOSED SEX RESEARCH PROGRAM

General Research Program: Committee for Research on Problems of Sex.
Program of Research with Primates other than Man.
Proposed by Robert Yerkes

[Note: I use Yerkes’ outline format and punctuation.]

General Research Program.
(1) That we accept as the primary goal of our research the provision of that knowledge of sex life which is essential for wise and effective educational procedure
(2) That to this end research be organized and promoted to supply
   (a) norms, indices and other standards of judgement relative to phenomena of sex
   (b) range, deviations, extremes, mutations, and their significance.
   (c) measures of control: physical, physiological, psychological, educational
(3) That because of the extensity of our research province, the several varieties of scientific interest and types of method be arranged in order of preference as follows:
   (a) physiological, chemical, etc.
   (b) psychological, psychopathological, sociological, educational, etc.
   (c) general biological, genetical, etc.
   (d) anatomical or morphological in the restricted sense.
(4) That as objects of research, organisms be preferred in general as follows:
   (a) the primates
   (b) other mammals
   (c) other vertebrates
   (d) plants
(5) That interest be concentrated on the study of the sex impulse, but not to the exclusion of work on other important and related problems, functional or structural.

Program of Research with Primates other than Man
Any one of the anthropoid apes or of the monkeys would be reasonably favorable as subject for this work, provided good conditions for the study of the animal in the wild as well as in captivity could be commanded.

Outline of study of the Chimpanzee, Orang outan, or Gorilla.

In Nature.
1. Sex behavior of male
   Before puberty, in family group
   Pubertal modifications of behavior
   After puberty
   During senescence
   Under each of the above the following phenomena should be studied as carefully as possible:
   - sex interest, play, masturbation,
   - copulation, individual peculiarities
   - relations to males, females, offspring, family group, species, other animals, man.

2. Sex behavior of female.
   Outline same as for male with the addition of
   - nest building
   - care of young

3. The life cycle.
   Ontogeny of behavior in each sex with particular reference to development and expressions of "sex impulse."
   - Detailed and connected account of sex phenomena during such important periods of life as infancy, childhood, adolescence, maturity, etc.
   - Limits sex impulse in life of individual.
   - Life span of generation

4. The family.
   - Constitution and type of family
   - Relation of male and female
   - Relation of family to species
   - Inter-relations of family groups
   - Significance of social association
     - Reproductive
     - Educational
     - Economic

In Confinement.
1. Sex behavior of male
   Accurate and detailed description of sex impulse compared with and checked by naturalistic findings.
Principal expressions of sex desire and impulse
   Norms, periodicity, physiological and psychological variations and manifestations
   in mated individuals with sex relations
   in isolated individuals without sex relations
Relations of sex impulse and activity
   Climatic factors
   temperature, humidity, chemical constituents of air, variability, etc.
   Food supply
   particular foods, diet, quantity of food, time of feeding, frequency of feeding, quantity and quality of water, etc.
Glands of internal secretion
   Relations of sex impulse to
   pineal
   pituitary
   thymus
   thyroid
   adrenal, etc.
Hormones
Nervous system
   sympathetic system
   reflex centers
Practice--facilitation
Habit--original sex-behavior pattern and modifications
Psycho-physiological factors
   interest, exercise, employment, diversions, play
Sex attraction
   specific stimuli
   beauty
Memory and imagination
Ideation
Sympathy
Altruistic behavior

2. Sex behavior of female
   Same outline as above with logical variations

3. The family
   Behavior of father, mother, offspring
   Group relations--evidences of intelligence
   temperament
   racial progress
   variations of behavior and their
significance
The sex impulse in relation to other phenomena
Evidences of altruism—sympathy, etc., in family group
General relations of successive generations
The ideal (an ape shaped to specifications).
Problem to breed an organism which shall exhibit certain behavior patterns, degrees of modifiability, and social relations. It is probable that the ideal ape would be defined chiefly in terms of intelligence, temperament, social relations and organic balance.

Experimental breeding could be conducted while studies of sex behavior were being made in Nature and in Captivity. Presumably selection, training, and artificial crossing, if not other methods of securing the desired type of organism would be employed.

It is clear that the production of either individual or social group in conformity with specification would demand norms for essential traits, knowledge of range and variation, of pathological conditions and of measures of control.

This experiment in species production to specification obviously need not be limited to individual traits, or rather it should be inclusive of social traits, so that such social units as the family or the band also might be created.¹

Note
APPENDIX B
FRANK LILLIE'S PROPOSED SEX RESEARCH PROGRAM

A Classification of Subjects in the Biology of Sex
Frank R. Lillie

I. The Genetics of Sex.
   A. The inheritance of sex and of sex-linked characters.
   B. Cytological (sex chromosomes).

II. Determination of Sex.
   A. Genetic or zygotic factors in the determination of sex. Problem for investigation: If sex is determined by two kinds of spermatozoa, is any process of selection possible by which one or the other kind may predominate?
   B. Environmental factors in the determination of sex. Problems: (1) Does the time of conception in the estrous cycle of the female influence the sex ratio? How? (2) May zygotic factors be overbalanced by environmental conditions? e.g., Are females ever transformed into males? At what time in the life cycle, etc.? (3) Do conditions of nutrition play any part in the determination of the sex ratio?
   C. The interpretation of sex ratios with reference sex determining factors.

III. Sex Development: Differentiation of Sex.
   A. Descriptive, normal (including hermaphroditism).
      1. Embryonic—fairly well known.
      2. Juvenile
      3. Adult
      4. Senescent
      We need here especially a complete histological picture of gonads, etc., at all ages. The knowledge on this subject is singularly incomplete.
   B. The problem of sex hormones. The influence of the homologous and heterologous hormones at all stages of the life history on anatomy, physiology, psyche, and physiological age.
      1. Histological.
         (a) The embryonic history of interstitial cells. Important for
understanding embryonic sex differentiation.
(b) The interstitial cells of the normal and experimentally modified ovary and testes—the data for the ovary are especially obscure and conflicting.
(c) The seasonal cycle of interstitial cells in various vertebrates.

2. Experimental.
   (a) The influence of the homologous and heterologous hormones in embryonic life. To what extent are sex characters reversible?
   (b) Sex modification in utero by other hormones or by antibodies.
   (c) Sexual modification after birth by hormones; and development: Castration, homologous and heterologous grafting, sex gland extracts, etc., including structure, function, psyche.
   (d) Effect of similar experiments (as in c) on age and rejuvenation.
   (e) To what extent are sex hormones species-specific?

3. Chemical. It will be the work of the biologist to test the nature and limits of hormone action, as in 1 and 2 above, and to discover proper criteria and indicators. It will be the province of the chemist to endeavor to identify, isolate and ultimately synthesize the sex hormones. The possibility of an ultimate ready control of sex characters and behavior within the limits discovered to be possible by the biologist must depend on the chemist working in close cooperation with the biologist.

C. The study of other factors, e.g., other internal secretions on the development of sex.

IV. The Problem of Sex-relations.
So far as these problems are human, they will be included in the physiological, psychological, and sociological divisions of the work of the Committee. But the biological aspect of this subject, so extensive and controllable, offers a field of great promise.

V. Sex Functions.
This heading trespasses on the physiological division of the work of the Committee, but no line can be drawn between the biological and physiological. It is impossible to carry out the program under III without attacking certain parts
of this field. The subjects closest to the biological field are the following:

A. Variation of sex glands under experimental conditions.
   1. Transplantation
   2. Vasectomy
   3. X-rays and other radiations
   4. Antibody injections
   5. Vital staining
   6. General physiology of the sex glands

B. Sterility:
   1. Incompatability of gametes
   2. Other causes
   3. Experimentally produced sterility

VI. Systematics of Sex in Animals and Plants.
The comparative anatomy, neurology, physiology and pathology of primary and secondary sex organs and sex characters in animals and plants. The evolution of sex, hermaphroditism, normal and accidental, in its various forms, sex dimorphism, organs of sex recognition, stimulation and realization (special sense organs, songs, ornamental characters, sex odors, organs of attack, etc.), brooding and placental structures, adaptations and habits, etc.

Note
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BIOGRAPHICAL SKETCH

Wade Edward Pickren was born on a farm near Alma, Georgia, on the night of November 29, 1952. He was the tenth child and fifth son of Bill and Merle Pickren. When Wade was nine months old, the family returned to central Florida. Wade spent his formative years in rural Florida, where he acquired a great love for the out-of-doors and for wildlife.

As Wade grew older, he developed an intense love of reading. He found in reading a way to satisfy his curiosity about the world. Wade also came to love sports, especially basketball, as he developed. He learned to play basketball from his two oldest brothers, who were All-State basketball players.

Wade graduated from Lyman High School in Longwood, Florida, in 1970. Not long after his graduation, he became a member of a commune. This experience he still counts as one of the most significant experiences of his life. During his tenure in the commune Wade also attended college for a year. He dropped out after that year and indulged his penchant for travel.
In the 1970s, Wade was drawn into service in Christian ministry. During this period, he sought to further his education in order to better understand the individuals he was seeking to help. Wade returned to college and graduated summa cum laude from the University of Central Florida.

Wade entered graduate school in 1988 in the Clinical and Health Psychology program at the University of Florida. He earned his Master of Science degree from that department in 1990. A course in the history of psychology with Donald Dewsbury sparked his interest to such a degree that he transferred to the Department of Psychology in 1991, in order to work with Dr. Dewsbury toward a degree in the history of psychology. This move he counts as the best academic decision he has ever made.

Wade plans to pursue an academic career upon completion of his doctoral work. He found that he loved classroom teaching in 1990, when he began teaching as an adjunct at Santa Fe Community College in Gainesville, Florida. Wade believes that he can combine his love of teaching and his love of historical research in such a way as to enhance both.

Wade has an intense love of music. His musical tastes range from Amadeus Mozart to Dwight Yoakum. Wade’s favorite musical artist is the Irish singer-songwriter, Van Morrison. Wade owns the complete catalog of Mr. Morrison’s recordings,
including several discs not available for sale in the United States.
I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.

Donald Dewsbury, Chairman
Professor of Psychology

I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.

Wilse Webb
Graduate Research Professor of Psychology

I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.

Elizabeth Capaldi
Professor of Psychology

I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.

Richard Griggs
Professor of Psychology
I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.

Frederick Gregory  
Professor of History

I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.

Toby Appel  
Associate Professor of History

This dissertation was submitted to the Graduate Faculty of the Department of Psychology in the College of Liberal Arts and Sciences and to the Graduate School and was accepted as partial fulfillment of the requirements for the degree of Doctor of Philosophy.

May 1995  
Dean, Graduate School