

# Autobiography of Robert Mearns Yerkes

Robert M. Yerkes (1930)

## **Classics in the History of Psychology**

An internet resource developed by

[Christopher D. Green](#)

York University, Toronto, Ontario

ISSN 1492-3713

(Return to [index](#))

---

## Autobiography of Robert Mearns Yerkes

### Robert M. Yerkes (1930)

First published in Murchison, Carl. (Ed.) (1930). *History of Psychology in Autobiography* (Vol. 2, pp. 381-407).

Republished by the permission of Clark University Press, Worcester, MA.

© 1930 Clark University Press.

Posted March 2000

---

### PSYCHOBIOLOGIST

I continue to think of the surroundings into which on May 26, 1876, I came as a first-born child, as nearly ideal. It was in the midst of a beautiful agricultural country, inhabited by intelligent, self-respecting, law-abiding, prosperous folk; hills and vales, forests and streams, as scenes of the ceaseless and ever-varying activities on large farm, with its rotation of crops, dairying, and woodcraft. There were domestic animals of many kinds, and many laborers and mechanics came and went. The great city of Philadelphia was so near that our farm and dairy products were hauled to it overnight in horse-drawn wagons. This is the picture that appears when I think of my childhood. If I were choosing now, I should not change that environment.

Pleasant occupations abounded. Fishing, swimming, skating, berry and nut gathering, fetching the cows, learning to care for, saddle and harness, ride and drive horses, and, finally, to do, and in many instances to enjoy doing well, the multitude of things necessary to comfort and prosperity on a large farm in eastern Pennsylvania, late in the nineteenth century, filled my days and rendered them joyous. Dominant among the recollections of childhood are out-of-door amusements; free, unrestricted, unaided study and enjoyment of nature; the care of household and farm pets; the capture and taming of wild animals. When the household cat one day killed a pet albino rabbit, I



ROBERT MEARN'S YERKES

# **Livros Grátis**

<http://www.livrosgratis.com.br>

Milhares de livros grátis para download.

was so inconsolable that my parents had the skin mounted and thus I long kept it as a cherished possession. I was extremely fond of every sort of game, from parchesi, dominoes, checkers, and cards indoors to such rough outdoor sports as shinny, baseball, and football. Warm, after nearly fifty years, are my memories of gathering tortoise and snake eggs in new lands when first plowed, of carrying them home in my hat, preparing earth-filled boxes, "planting" them, and watching for the hatch. The young snakes usually managed to escape me, but the tortoises became treasures of entertainment. Thus happily passed the first eight years of my life.

I have delightful recollections of three of my great-grandparents, and I enjoyed and richly profited by long-continued acquaintances [p. 382] with all of my grandparents.<sup>[1]</sup> My parents, who belonged to families long resident in the vicinity of Philadelphia and devoted almost without exception to agriculture, lived to see me established familiarly and professionally. Both were ambitious, energetic, musical, religious by nature and training. My mother, a woman of rare sweetness of disposition and unusual ability, beloved of all who knew her, was the strongest influence in my early life, and I think also the wisest. My father and I were intimate merely because of blood and social relationship. We had little in common intellectually, and more often than not we disagreed in practical matters. Except for my attachment to my mother and the influence of a capable, level-headed young German then in the employ of my father, I probably should have run away from home before my fourteenth year. Lest this should appear to belittle my father, I hasten to add that I have been described as a moody, strong-willed, unsuggestible child, difficult to control. Father doubtless lacked the magic touch of sympathetic insight. In early childhood I feared him; later, I actively disliked and disapproved; and finally, in maturity, I came to pity him for characteristics which rendered his life relatively unhappy and unsuccessful.

As I write I am reminded of many incidents of family life which are illuminating. Only a few may appropriately find place in this professional sketch, and as it happens those which I have chosen refer rather to my grandfathers than to my father or self.

Grandfather Yerkes once told me, and he was then more than sixty years of age, that he did not know what it meant to feel tired! The members of his household used to say that, on arising to his day's work about four o'clock in the morning, he would loudly call the poultry to breakfast in order thoroughly to arouse the family and get things started. Father also was like that, and I should confess that in my own household I am sometimes called the slave driver.

Lack of sympathy with my father and our temperamental incompatibility very definitely turned me against his occupation and his vocational plans and desires for me. These misfortunes also robbed me of much that should be most precious in paternal companionship, training, and guidance. The following incident, taken from my relations with Grandfather Yerkes, partially explains my estrangement from Father, for his treatment of me was as direct and unsuited to [p. 383] my disposition as was that I would now describe. I had been set an irksome, arduous, farm task which I performed as I thought proper and necessary, but with maximum economy of effort and simplicity of procedure! Subsequently, I learned that Grandfather had complained to Father that the work might better have been left undone. I bitterly resented the criticism, which I considered unjust, but even more the fact that Grandfather spoke to Father instead of to me. Perhaps, had he come to me and tactfully explained why my method was unsatisfactory, I should have been the wiser and he respected instead of disliked.

Radically different are my memories of Grandfather Carrell, for he genuinely sympathized with my intellectual interests and aspirations and always was ready to encourage and aid me in my educational efforts.

During childhood I was much alone. A sister some four years my junior, to whom I became devoted, died when she was three, and I barely recovered from the same dreaded scarlet fever. Two brothers and another sister, born later, were so much younger that I looked upon them as charges rather than playmates.

Because of its far-reaching influence on my physical and intellectual development and my

vocational choice, the scarlet fever tragedy should be more fully described. A man, prematurely discharged from a Philadelphia hospital or for other reasons a carrier of infection, came to us as a farm laborer. He was friendly with us children and, from his arrival, we were much with him. When we became ill he disappeared, doubtless conscience-stricken or fearful of responsibility. Many weeks later I learned that my little sister had gone from us. Vivid is my memory of Mother's gentle, sad words as she told me of this when, for the first time, I sat up beside a favorite window in the sunshine of early spring. In my young life that loss was irreparable. No one ever took the place of my infant sister and I continue to think of her as the most beautiful and altogether lovable of children.

The family physician, during this fight with the forces of destruction, was a cousin, Dr. John Beans Carrell, whose ministrations, often bitterly resented and opposed by my feverish self, nevertheless made lasting impressions and deeply stirred my admiration and vocational hero-worship. Ever since, in my daydreams, I have imagined myself as physician, surgeon, or, in other guise, alleviator of human suffering. This is the first indication of a social-mindedness which subsequently came to pervade my life and to establish fellow service as its chief objective.[p. 384]

I am wholly unable to confirm the observation, but Dr. Carrell assures me that my disposition radically changed during my grave and prolonged illness. Before it, according to him, I had been wilful, violent-tempered, obstinate, unruly, disagreeable. Thereafter I was so greatly improved as to be fit to live with! Be this as it may, I am convinced that my illness so far conditioned my physique and interests as practically to determine vocational choice.

Mine was not a home for formal educational regimen. Neither my parents nor any among my immediate relatives were college graduates. I can recall no thirst for knowledge in early childhood, and, although from six to twelve years I was passionately fond of being read to, I read little myself. Vocational imaginings came early, and, after transient longings for the delights of old-iron collector, huckster, locomotive engineer, preacher, I turned, as intimated above, to medicine mixing, and for a physician. many years purposed to become a physician. My motives I suspect were chiefly utilitarian, for the physician's life appealed to me as less harshly laborious, more interesting, exciting, heroic, useful, and altogether profitable than that of the farmer.

In my eighth year, when first sent to school, I was unable to read well and so shy that I went unwillingly and with intense discomfort until I had become accustomed to the routine and made acquaintances. For some seven years I attended the nearby ungraded rural public schools. I worked hard in school because I liked to succeed and stand well in the class. Ambition and social prestige evidently were primarily motivational, but usually I also liked the work itself and did it eagerly and without pressure in school or home. Subjects which induced lasting attitudes were spelling, because difficult and irksome; arithmetic and algebra, because I found them stimulating, interesting, game-like -- their problems fascinated me, whereas memorizing repelled -- and physiology and hygiene, because their objectives, information, and principles impressed me as peculiarly important.

I lacked gift of graphic expression, being then, as now, quite incapable of seeing or representing objects as does the naturally endowed artist. Musical ability, if present, I suppressed, for, despite my mother's eagerness to teach me and her urging and pleading, I never learned to sing or to play any instrument. Probably music would have been difficult for me, but I suspect that shyness and reluctance to try were the chief causes of my resistance. There are [p. 385] few things which in later years I have more deeply regretted than lack of musical education.

Probably I was prepared for high school, possibly for college except in the ancient languages, when in my fifteenth year I was sent with a cousin, Leonard Slack, to the State Normal School at West Chester, Pennsylvania. This was my first experience away from home and my educational baptism. I worked hard and achieved special commendation and promotions in mathematics. The fact reminds me that subsequently in college a professor of mathematics

suggested that I devote myself to the subject professionally. During the year at West Chester I recall being asked by my father whether I still wished to study medicine. My reply was an emphatic affirmative. Father, as I knew, hoped that I would follow agriculture, but, if I would choose a learned profession, he preferred that it be the law. Mother, on the contrary, wished me to enter the church. Almost certainly she would have become a foreign missionary had she been free to choose a career.

I think it was about this time in my educational history that an incident occurred which fixed itself permanently in my memory. Its significance is clear. An aunt, mindful of my exceptional educational opportunities, one day asked me some geographical and historical questions. When I admitted ignorance, she expressed surprise at the imperfection of my education. I well remember my mingled feelings of chagrin, resentment, and disapproval, for her conception of education struck me as unsatisfactory. Even then my interest centered in constructive, creative effort toward the extension of knowledge, instead of in achievement of scholarship through mere accumulation of facts. Thus early, my interest in research manifested itself. The incident suggests the query: Is it perhaps true that persons of exceptionally retentive memories tend to become encyclopedically learned, whereas those of relatively poor memories, among whom I undoubtedly should number myself, tend rather to become inventive, inquiring, and constructive? Whether, in such case, psychological traits are primarily conditions or results is the question in point.

So it happened that at the age of sixteen I possessed vocational orientation and determination to obtain the educational preparation desirable for the profession of medicine. Undoubtedly, our family physician, Dr. Carrell, was chiefly responsible for this choice. His personality and professional example had stirred my imagination, and [p. 386] his interest, encouragement, suggestions, and advice provided the necessary basis for definite decision. Except for the happening now to be narrated, I almost certainly would have gone to Dr. Carrell "to read medicine" and thereafter have matriculated, probably without collegiate training, in his medical alma mater, the Jefferson Medical College of Philadelphia.

But things happened otherwise and thus. When I returned to the farm from my few months at Normal School, ways and means were not discernible for the continuation of my studies. Father was struggling to pay heavy indebtedness on his farm and there were three younger children to provide for. It was then that an uncle, Dr. Edward Atkinson Krusen, who had married one of my mother's sisters and recently established himself as a homeopathic physician in Collegetown, Pennsylvania, the seat of Ursinus College, offered me opportunity to earn my way in college by doing the chores about his place. There was neither doubt nor hesitation on my part, and I rejoiced greatly in my parents' consent to the arrangement.

In the fall of 1892 I entered Ursinus Academy, and after a year's preparatory work, with concentration on the ancient languages, I was admitted to the collegiate department of the institution. I elected the chemical-biological program of study, and, in addition, did preparatory medical work in human anatomy and physiology. But it was all work and no play, for my eagerness to progress held me to my academic tasks, and my duties in the Krusen home required all my spare hours, morning, evening, and Saturday. As I look back on those happy, toilsome years, it seems as though they would have been perfect if I could have afforded and arranged to have had Saturday as holiday. Yet I was far from self-pity, and I always have considered myself fortunate in my opportunity to obtain collegiate training.

After entering Ursinus I was at home only for short visits or for a few weeks during the summer harvest season, when I worked as a paid laborer. From the small savings of my youth, which, on Dr. Carrell's advice, had been well invested, and from my current earnings in the Krusen home, I was able to pay all of my expenses in college. In addition to board and room, after my first year in his home my uncle paid me a wage of ten dollars per month. This I considered generous and just. Indeed, to Uncle Doctor, as I always called him, my debt is incalculable. He was a wise, broad-minded, generous gentleman, a beloved physician, and a staunch, dependable [p. 387] friend. Had he been my father, and, practically, from my sixteenth to my twenty-first years he stood in *loco parentis*, he could not well have done more for me.

Disinterestedly, devotedly, affectionately, he advised, guided, and encouraged me. I cannot do less than thus acknowledge my debt of gratitude and love.

A word further on personal influences. Up to the time of my entrance into college, my character, vocational leanings, educational endeavors and ambitions, had been markedly affected by six persons: my father and mother; the German farm laborer, Adolph Weise; my public school teacher, Miss Eva Roberts; and the physicians, Drs. Carrell and Krusen.

My father, I suspect, most strongly influenced me negatively. I desired to become what he was not: had he wished me to become a physician, doubtless I should have refused. My mother, on the contrary, through affection, tactful suggestion, the inculcation of the moral code, principles of character of the Christian religion and of her community, influenced me profoundly and permanently. Father's employee, Adolph Weise, was my intimate, wise friend and counselor in those years of early adolescence when I sorely needed guidance and stabilization. He read to me, talked with me of many things, aided with my lessons, and reasoned with me on endless practical matters. By sheer simplicity and convincingness of argument, this strong, clear-minded young German reasoned me away from the undesirable. Of swearing, which he abjured, although most of our farm laborers were adepts, he always said: "It is a foolish, useless, disagreeable habit. Don't form it." As I could not meet his arguments, I naturally followed his example in this and many other matters. My first public school teacher, Eva Roberts, later for many years a highly successful and esteemed teacher in Girard College, Philadelphia, deeply impressed and influenced me by her strength of character and purpose, mastery of pedagogical method, soundness of judgment, and utter justice in the treatment of pupils. I admired her almost worshipfully. There was also the all-pervasive and continuing influence of my cousin, Dr. Carrell, which certainly initially determined and confirmed my choice of medicine as a career; and, finally, that of my uncle, Dr. Krusen. To these few I owe my life and its main traits and trends. My heart goes out to them now in gratitude and affection. Would they were all here to receive such reward of appreciation as I can offer.

In June, 1897, I was graduated from Ursinus College after four [p. 388] profitable years of strenuous intellectual work. Two Ursinus teachers profoundly influenced me. Colonel Vernon Ruby, Professor of English, more, perhaps, than anyone else, taught me the importance of careful, thorough, honest work. My ability to use my mother tongue I owe principally to him and to the subsequent practice which his precept and example encouraged. Dr. P. Calvin Mensch, biologist, I worked with as pupil, disciple, and friend. His ideals and his enthusiasm for creative endeavor became mine. Probably my debt to him is greater than to any other teacher.

Completion of work at Ursinus found me at a crossroads, for a *deus ex machina* had unexpectedly appeared and I was offered the loan of one thousand dollars for graduate work in Harvard University. Choice was between the study of medicine in Philadelphia or the unexcelled opportunities for graduate work in biology, psychology, and philosophy at Harvard. It was a momentous decision which, as now appears, determined the course of my professional career. I was just twenty-one. Readily I convinced myself that I was young to enter medical school and might better devote at least a year to special work in Harvard before completing my medical training. It was my earnest desire to work with pre-eminently able investigators and teachers.

So, in the fall of 1897 I entered Harvard, not as a graduate student, but with provisional undergraduate classification and opportunity to demonstrate preparedness for professional work. At the end of the first year I was awarded the A. B. degree and given graduate status. I might then naturally have turned to medical studies, but instead I leaned toward preparation for research in some department of biology. Encouraged by my teachers and aided by appointments to assistantship and scholarship, I decided finally to become a candidate for the degree of doctor of philosophy instead of doctor of medicine.

Again a crossroads which compelled important decision. I was keenly interested in zoölogy and also in psychology. At the suggestion of Josiah Royce, to whom I had gone for advice when I first arrived in Cambridge in 1897, and who became my teacher, friend, and colleague, I

undertook to combine these interests by devoting myself to what was then called comparative psychology. Introduced and recommended by Professor Royce, I consulted with Professor Münsterberg about opportunities in animal psychology. He was encouraging and the outcome was my transfer in 1899 from the [p. 389] laboratories of zoölogy, where I had enjoyed the rare privilege of working with E. L. Mark, G. H. Parker, C. B. Davenport, and W. E. Castle, to the laboratory of psychology, in which, during the succeeding eighteen years, as student, assistant, instructor, or professor, I conducted psychobiological research and instructional courses in comparative and genetic psychology.

From the beginning of our acquaintance, Hugo Münsterberg, with almost paternal interest and solicitude, and with rare generosity, aided me both professionally and personally, and, although I never was able to admire him as scientist, I learned to prize highly his friendship, enthusiasm for research, and scholarship. Throughout our association from 1899 until his death in 1916 our relations were intimate, and I was constantly the beneficiary because of his learning, extensive professional acquaintance and knowledge of the world, and his devotion to research. I seriously doubt whether I should have remained in Harvard more than one or two years except for his influence and encouragement. Thus I acknowledge a great debt. In 1902 I was granted the doctorate of philosophy in psychology and offered an instructorship in comparative psychology in the University, with half time for research and a salary of one thousand dollars per year. I well remember Professor Münsterberg's friendly question when he told me of the opportunity: "Can you afford to accept it, Yerkes?" "No," I replied, "but I shall, nevertheless." Thus began a period of professional service to Harvard University and science which continued until it was interrupted by the World War in 1917.

During those fifteen happy, eventful, fruitful years of research and teaching I gave my best to Harvard and received incomparably more benefits from rare associations and companionships than I could give in return. It was for me a period of intellectual and cultural growth and enlightenment, of constant stimulation to improvement and achievement, and of precious inspirational influence. For, unworthily, as it seemed to me, I was a member of a university faculty group of pre-eminently great scholars and great personalities, which at one time or another during the period in question included Josiah Royce, George Herbert Palmer, William James, Hugo Münsterberg, Francis Peabody, George Santayana, Dickinson Miller, Robert MacDougall, Edwin B. Holt, and Ralph Barton Perry. These, my colleagues in the Division, which was then inclusive of philosophy, social ethics, and psychology, were men of such personal quality, [p. 390] originality, and creativeness, as seldom are found in an academic group.

Thus, with victory for the latter, ended in 1902 the struggle between medicine and psychobiology in my vocational imaginings. My taste for scientific research, if not my ability, had long before been revealed at Ursinus when my teacher and master, Professor Mensch, himself a doctor of medicine and of philosophy, proposed for my training the investigation of a problem in physiological chemistry. I did not solve the problem, but in the attempt I learned much about myself and the attractiveness of biological research. From that time I knew positively that I wished to give my life to constructive work in the biological sciences rather than to practical service in medicine or surgery. It was then that I first resolved that making a living should, so far as practicable, be merely incidental to my life work. And so, as it turns out, it has been, these thirty years! But when I abandoned the study of medicine, lively interest in its varied problems and in the sciences basic to both medicine and surgery persisted. Although I lack a medical degree, my dominant interests classify me with the profession. Much of my work has been conducted in medical institutions; more might have been, and my friendships and companionships continue to bear witness to my natural taste and my initial vocational leaning and choice. All this merely to establish the fact that in reality my original choice of career was modified, not abandoned, and my professional interest broadened and liberalized instead of turned into unrelated channels.

A plan, whose realization after nearly thirty years has now been nearly achieved in Yale University, came to me as a stirring vision of usefulness during my graduate days in Harvard. It was the establishment and development of an institute of comparative psychobiology in which

the resources of the various natural sciences should be used effectively for the solution of varied problems of life. Naturally, psychological and physiological interests dominated in this vision. For a time it seemed that the dream might speedily come true in Harvard, but President Eliot, wise and far-sighted promoter of productive scholarship and of medical education and research, retired from his responsibilities just too soon. Instead of receiving encouragement in such seemingly impractical planning as I had been indulging in, I was gently and tactfully advised by the new administration that educational psychology offered a broader and more direct path to a professorship and to increased academic usefulness than did [p. 391] my special field of comparative psychology, and that I might well consider effecting a change. In disregarding this well-meant and wholly reasonable advice, I ran true to form. To do what I had especially prepared myself for, what I felt pre-eminently fitted for, and what, above everything else, I wished to do, seemed to me incomparably more important and desirable than a professorship at Harvard. Several of my professional colleagues agreed with me. Many times since I have had to confirm that decision or to make similar ones. I never have regretted the abiding determination to live my own professional life, irrespective of administrative and other practical considerations.

During the first year of my Harvard instructorship, opportunity appeared for a brief visit to Germany and Switzerland to study the organization and equipment of physiological and psychological institutes. This was in preparation for the planning of suitable building and facilities for comparative and other branches of psychological work in Harvard. The experience naturally was very valuable to me. It was seventeen years before I again visited Europe, and then it was to England and France that I journeyed. This neglect of international professional contacts was due to financial limitations and the demands of my research, not to lack of interest or desire. Indeed, it has proved a very serious disadvantage. As I write these words, I am on my third professional foreign tour, which includes visitation of numerous psychobiological establishments in the principal countries of Europe, and, in addition, the laboratories of the Pasteur Institute at Kindia, French Guinea.

In 1905, when I was fairly started in my career as a psychobiologist, began a partnership with Ada Watterson (Yerkes), which perfectly blended our lives and incalculably increased our professional and social usefulness. Successful marriages appear in these times to be not unworthy of record and remark. Moreover, from 1905 my professional autobiography is no longer mine alone. At this moment our partnership is publishing jointly, as the outcome of six years of continuous preparatory labor, a book on anthropoid life, *The Great Apes*.

Crowded with interesting activities were the years between 1902, the beginning of my professional life, and America's entrance into the World War. My intellectual environment was stimulating, conditions within and without were favorable to creative endeavor, incentives to service abounded. I was busy, contented, happy in my [p. 392] scientific work, my family life, and friendships. As my colleague Ernest E. Southard once remarked, professionally speaking, for years I lived on cream. To supplement my small and obviously insufficient Harvard salary, which during fifteen years of service as instructor and assistant professor averaged about two thousand dollars a year, I taught in Radcliffe College, Harvard Summer School, and the University Extension Department in Boston. It was through my teaching of elementary psychology that I first was brought into contact with Edward B. Titchener. Use of his textbooks in my courses provoked exchange of opinions, discussion, and, on my part, endless questions, for in introspective method and its results I was the novice, he the master. I treasure a folder of letters which represent much of my vital exchange with the most learned psychologist I have ever known. Whatever interest I have in introspection, competence in its use, and appreciation of its results, and whatever I know of the psychology of the self, as contrasted with objective psychology, I owe primarily to Titchener. With his aid I came to distinguish sharply between my special interest in the materials and problems of psychobiology and psychology as the science of experience. Efforts to systematize my thinking in this direction for the benefit of my students resulted in the publication of my *Introduction to Psychology*, the first and only textbook I have had the will to write. My professional debt to Titchener is equaled only by that to Münsterberg, Royce, and Holt.



In the midst of intensive work with students and colleagues in the Harvard Psychological Laboratory I found time for several profitable adventures in cooperation. These are some of them. From association as pupil and assistant with Edward L. Thorndike at the Marine Biological Laboratory at Woods Hole, I profited much. Later for some years I labored with John B. Watson for the improvement and the standardization of methods for the comparative study of vision in animals. At this time Watson was in Baltimore, I in Cambridge, and our exchanges were mostly by letter. One spring, to my great satisfaction, I was granted leave of absence from Harvard to acquire knowledge of neuro-surgical technique through association with that skillful technician and brilliant investigator, Professor Harvey Cushing. My weeks in the Hunterian Laboratory of the Johns Hopkins University, under the guidance of Cushing, provided stimulating, enlightening, and revealing experience, whose effects were permanent. In addition to technical training and new [p. 393] professional insight, I carried from the laboratories of comparative surgery an enduring friendship. That I have made no noteworthy contributions to neurology or psychobiology by way of surgical techniques is the fault of circumstances beyond my control. Yet another important season was that spent with my former pupil, Gilbert V. Hamilton, in his ideally situated private laboratory at Santa Barbara, California. And with another pupil, Daniel W. La Rue, who, like Hamilton, returned with interest what little I had been able to give as teacher, I planned, used in courses of instruction, and finally published *An Outline of the Study of the Self*.

Much more than an episode in my almost too full professional life of the young century was opportunity, on recommendation of Ernest E. Southard, Professor of Neuropathology in the Harvard Medical School, and Scientific Director of the Psychopathic Department of the Boston State Hospital, to serve as psychologist in the Hospital. This was my introduction to research in psychopathology. During five years I gave half of my time to the direction of psychological service and research in the Hospital. It was here I discovered certain urgent needs of psychiatry for improved techniques of psychobiological examining and measurement, and here also, with the aid of graduate students and assistants, I developed the point-scale method of measuring aspects of intellectual activity and the multiple-choice method for the study of ideational behavior. Naturally, both practical and theoretical relations of psychobiology to medicine, and more particularly to psychopathology and psychiatry, commanded my attention and I thought and talked much about ways of rendering these subjects more helpful to one another.

I have mentioned Ernest Southard as my master in psychopathology. He was that and much more, for, even after a decade of separation from his influence, his brilliant originality, vision, versatility, and tireless industry, continue to stir my imagination and to spur me to more fruitful effort. His was a remarkable intellect, backed by exceptional training and vision, which neurology and psychiatry could ill afford to lose either early or late.[2]

Those were particularly stirring years, for when I accepted hospital duties I gave up no portion of my teaching burden or program of research in the Cambridge laboratories. Doubtless, it was fortunate for my health that in its fifth year this dual life was abruptly ended by the World War. The internal values of my concentrated [p. 394] practical experience in psychopathology it would be difficult to overestimate. The external results are scant because I published relatively little.

Throughout my Harvard connection several graduate students each year shared my labors and enthusiasm for discovery and invention. I then considered the university the logical and altogether fitting home for research, and I now even more strongly hold that conviction after some thirty years of varied professional experience, both within and without American universities. Stable in my professional life and not over-eager for increased income or rank, the current ran smoothly and it seemed that I might continue at Harvard until the end of the chapter. It had been relatively easy to refuse numerous opportunities to migrate. Then out of the war-clouded sky came an attention-compelling, insistent call to reorganize psychological work and take direction of the laboratory in the University of Minnesota. At first I declined thoughtfully and reluctantly, with the urgent advice of Professors Royce and Münsterberg. But when, a year later, the offer was made even more alluring, I hesitated and was lost to my university birthplace and home. It was a difficult decision, opposed I recall by such disinterested

friends and advisers as Professors Royce and Herbert W. Rand, but supported by such as ex-President Eliot and Professors Münsterberg and Taussig.

I was in my fortieth year when, in the spring of 1917, I accepted the Minnesota appointment. Barely had I made this new arrangement than America's entrance into the War upset all of my plans. For two years after resigning my appointments in Harvard and in the Boston Psychopathic Hospital I held my western academic post and during that time made necessary recommendations for staff reorganization, planned the establishment of a department of psychology, and arranged for the transfer of the laboratory to a new site and building. It was a profitable experience, although in the end I resigned my post without having at any time been resident in Minneapolis. For this circumstance the War was wholly responsible. The members of my staff in Minnesota who, after my resignation, carried on effectively included, in addition to Herbert Woodrow, who was originally on the ground, Richard M. Elliott, William S. Foster, Mabel Fernald, and Karl S. Lashley. A better-trained, more able, and altogether competent group of young psychologists was not to be found.[p. 395]

Thus, with America's declaration of war ended one of the most important periods of my professional career -- measured by twenty years as student, teacher, and investigator in Harvard University. It is appropriate to note here the distinctive characteristics of my research interests and results during this period.

My first scientific paper was published from the Laboratory of Comparative Zoölogy of Harvard in 1899, when I was twenty-three years of age. It was the outcome of suggestions received from my teacher, Charles B. Davenport, and of observations made under his direction. The title of this maiden publication in psychobiology, *Reaction of Entomostraca to Stimulation by Light*, indicates one of my major fields of interest, namely, organic receptivity, its nature, conditions, and relations to behavioral expression and to experience. There followed several papers on phases of receptivity and response in invertebrates. All show the helpful influence of my biological teachers, Messrs. Mark, Parker, and Davenport, and all are classifiable under the physiology of the nervous system, although even then it would have been fairer to my interest and point of view to place them in psychobiology.

Shortly my interest extended to include organic adaptivity, which then was almost universally designated as habit formation, and from 1905 to 1912 I published several reports of investigations on adaptivity and receptivity in such relatively lowly vertebrates as the amphibians and reptiles. Other aspects of physiological process which at this time suggested to me important neurological problems were temporal relations of response, inhibition, and facilitation. A little later I became profoundly interested in problems of instinct versus individual acquisition, and several of my investigations and those conducted under my direction were concerned with the essential characteristics and relations of maturational or so-called hereditary modes of response and their neuromuscular mechanisms.

I still consider solution of the assemblage of problems suggested by these phrases of the utmost theoretical and practical importance. Many times my work on the mechanisms and behavioral expressions of inheritance and acquisition has been interrupted, once by the loss of my colony of dancing mice, and again by the World War, which found me with apparatus ready for continuation of work with mice. Investigation of the behavior of wildness and savageness in rats, well begun with the cooperation of Professor William E. Castle, I abandoned because conditions of experimentation were not favorable to reliable results.[p. 396]

Especially conspicuous in my research has been interest in methods and efforts to advance comparative psychobiology by invention, adaptation, and improvement thereof. My work, I suspect, has been characterized rather by ingenuity and originality than by technical skill and mechanical gift. Theoretically, method conditions progress; practically, it has always seemed to me more important than observation. My investigations, I think, entirely support this conviction, for the greater part of my life has been devoted to methodological work in the biological sciences.

I have mentioned my abiding interest in the problems of organic receptivity, adaptivity, and instinct. Always my research has been more nearly physiological than psychological, for I have dealt with problems of behavior, not with experience. Therefore my constant use of the descriptive term psychobiology. That either my interests or methods of work, my descriptions or interpretations, have become consistently more or less objective during the past thirty years I am not aware. Certainly there have been fluctuations of opinion, and gradually the conviction has strengthened that open-mindedness, willingness to envisage all problems and all trustworthy results, and to consider and test the value of all types of method, are prime essentials for the advancement of knowledge. With extreme objectivism, as voiced during the early years of my career by such eminent biologists as Loeb, Beer, Bethe, and von Uexküll, I have never been able to sympathize unreservedly because it impressed me as dangerous in its restrictions and negations. On similar grounds I have rejected the more recent objectivism, or as he calls it, behaviorism, of Watson, for it is characterized by the same logical and practical defects which appear in the historical types of psychological objectivism. More forcibly than ever, after thirty years of earnest thought and persistent study of problems of organic behavior and experience, it strikes me as wholly indefensible, and extremely unprofitable, to deny the possibility of scientifically investigating phenomena of experience in their relations to other vital happenings.

That my own interest has always centered in problems of organic structure and function in no degree prejudices me against the study of consciousness and mind. Instead, I consider the problem of the nature and relations of consciousness as at once the most fascinating and the most important in biology, and it is my earnest hope that I may live to help in some measure toward its solution. That my path is not obviously directed toward this end needs neither explanation nor apology. My course in research is pragmatic.[p. 397]

The scope of my research was broadened in 1913 by the addition of psychopathology, for it was in that year I accepted appointment in the Boston Psychopathic Hospital. Naturally, I undertook work in psychotechnology which promised to be helpful to psychiatry, but at the same time I formulated and, with my peculiar equipment as comparative psychobiologist, attempted to solve certain problems relative to the nature and causation of psychobiological disturbances and defects. Unwittingly I was thus prepared for the military opportunities and demands which were shortly to confront me. Had I planned my adventure in practical mental measurement with full knowledge of what awaited me in the World War I could not have arranged things better. My work at the Hospital was abruptly terminated by the War, but, even without it, removal to the University of Minnesota would have caused a break. Much of my work in psychopathology continues as I then left it, unfinished.

It was thus the presidential proclamation of April, 1917, found me. At the moment a group of experimental psychologists was meeting informally at Harvard University. Naturally, we asked ourselves what professional service American psychologists might hope to render in the military emergency. Discussion revealed eagerness, coupled with optimism and assurance that some, at least, of our techniques could be made serviceable.

Because I happened to be President of the American Psychological Association, it became my privilege and duty to take the initiative in organizing our group and in attempting to discover ways in which we might be useful. It is indicative of my lifelong professional leaning and affiliations that I promptly established relations with the Medical Department of the Army and that the major service for which I was personally responsible throughout the War, the psychological examining of recruits, should have been conducted in that arm of the service.

The story of psychological service has elsewhere been told officially and completely, if not in detail.<sup>[3]</sup> It is appropriate here to consider its principal relations to my professional life.[p. 398]

For nearly two years I lived in military psychology, with scarcely a thought of the psychobiological problems which previously had occupied me. The novel opportunity which my profession created for itself in the American military establishment called for constructive planning, combined with methodological resourcefulness and skill. For these demands, as

contrasted with many which more usually come to the academician and investigator, it shortly appeared that I possessed unusual qualifications.

During my term of military service I wrote little for publication. There was no time. But my official correspondence was both extensive and profoundly important for my intellectual and technical growth and the development of facility in verbal expression. It was necessarily descriptive, expository, argumentative, for my chief task, aside from making clear what we planned and proposed, was to convince military and civil officials that what we desired to undertake possessed practical value. Often it seemed that my foremost duty and obligation -- one for which I usually felt myself peculiarly unsuited -- was to vanquish seemingly insuperable difficulties by overcoming the passive resistance of ignorance and the active opposition of jealousy, misinformation, and honest disagreement.

Fortunately, I flourished amidst difficulties and discouragements, and the service which my group rendered finally yielded abundant satisfaction. It has been characterized by those who observed it from above the battle as uniquely significant alike for military progress and for the development of psychology and its technologies. Assuredly it was highly beneficial to me to be carried by force of circumstance from the comfortably sheltered provincialism of a great university into the swirling current of world conflict. As never otherwise could have happened, I was brought into active give-and-take contact with men of varied interests, abilities, and points of view, at a time when every man rose superior to himself; with national and international problems, plans, and programs; with organizations, methods of administration, and ideals which are foreign to academic experience. Necessity made me at home in this novel situation and I was able to present [p. 399] and maintain the needs, claims, and merits of my profession as determinedly, and I think also as effectively, as I could have done in my customary environment. As obligations and opportunities multiplied, so also my knowledge, insights, faith, and will to succeed, and when suddenly the great conflict ended I was so completely engrossed in helping to increase the efficiency of the military organization of my country that for a time I felt like a person without a calling.

If ever I have spoken or written as though the contribution of military psychology in Army or Navy was largely mine, I would beg here to correct the impression. Mine, as it happened, was the responsibility for initiative and leadership, but scores of my colleagues enthusiastically and loyally gave their best. To mention names would be invidious and in bad taste, because the honor roll is too long to be reproduced entire. I could have accomplished little indeed without the whole-hearted, generous, and efficient constructive work of my fellows. The reward of growth, self-revelation, and confidence in my ability to serve mankind which came to me by reason of my share in the great conflict is more than adequate compensation for the arduous labors of the most trying years of my life.

As never otherwise could have happened, military opportunities, demands, and achievements gave American psychology forward and directed impetus. Owing primarily to an endless succession of difficulties, resultant delays, and finally the termination of the War just when our service was fully organized, our methods perfected, and authority granted for the extension of our work throughout the Army, the strictly scientific as contrasted with the practical returns of our labors, although by no means unimportant, proved meager in comparison with what we had planned for and legitimately expected. It will be long, however, before our profession entirely escapes from the directive influence of psychotechnological military developments or forgets that almost incredibly extensive and precious gift of professional service, which to the laity and the military profession was the more impressive because wholly unexpected and unsolicited.

When discharged from the Army shortly after the Armistice, I found myself faced with choice between continuation of work in Washington in connection with the National Research Council, through which much of our psychological military service had been organized and rendered, or reporting for duty in the University of Minnesota. For two reasons, chiefly, I hesitated and then decided to resign my academic post: I wished to complete and superintend the [p. 400] publication of the official report of our psychological work during the War, and, picking up the threads of my psychological past, to endeavor to find financial support for systematic utilization

of the anthropoid apes in biological research. The latter interest, as one of the most important in my professional career, here demands brief historical comment.

In the course of comparative studies of receptivity and adaptivity which I conducted or directed in Harvard University, and especially because of the work of my student, M. E. Haggerty, on imitative tendency in monkeys, and varied observations of my own on marmosets, monkeys, and orangutans, I had become convinced that, for certain major groups of psychobiological problem to whose solution I hoped to dedicate my life, the primates, and, more particularly, the great apes, promised to be supremely and perhaps also uniquely serviceable. My conviction found expression in a plan of action which I formulated for publication as early as 1916.<sup>[4]</sup> Following the publication of this plan several offers of assistance came to me, but no one of them could be safely accepted because I was not financially independent and thus able to give my time to the project without compensation. From 1917 to 1919 my efforts to finance suitable laboratories were necessarily in abeyance, but my dream recurred with increased vividness and compelling power when the war clouds vanished. So it happened that I was ready and eager to serve the National Research Council as chairman of one of its divisions, in part because the connection enabled me to remain in Washington where conditions seemed peculiarly favorable for the promotion of my pet project.

When I originally decided to stay in Washington instead of going to the University of Minnesota, I supposed that it would be for only one or two years, for I was optimistic that within that period I should succeed in arranging to go forward with my research. But it was not so. Disappointments succeeded one another as in the Army, and the period stretched to five years before I escaped to more congenial activities. In the meantime my personal research was almost wholly in abeyance and my only noteworthy service to my particular branch of science was the organization and facilitation of research in problems having to do with aspects of sex and human migrations. This work was done primarily through the agency of committees. I initiated and for more than two years served as Chairman of the Com-[p. 401]mittee on Scientific Problems of Human Migration of the National Research Council,<sup>[5]</sup> and simultaneously gave much of my time to the Chairmanship of the Council's Committee for Research in Problems of Sex. During my association with these committees we were able to secure, through the National Research Council for the support of our programs of research, sums aggregating eight hundred thousand dollars. That our promotional endeavors were fruitful is convincingly established by the content of scores of reports which have been published by cooperating investigators. Although it was far enough from my primary interest and desire, I nevertheless took great satisfaction in this promotional work, and I even dared to hope that the committee method as we developed it might become so well established as to continue in use. In this, the migrations organization proved disappointing, whereas that for the study of problems of sex has continued with increasing usefulness to the date of writing.

As I reflect on my experiences I realize that personal relations during my sojourn in Washington were far too significant professionally to be ignored. My period of military service was slightly less than two years. The National Research Council elected me to membership in 1917 and for several years I served that organization in various capacities. Among the many delightful and professionally invaluable acquaintances and friendships which came to me during seven years' residence in Washington, I mention the following because of their pre-eminently great influence on my professional career: with George E. Hale, astronomer, the boldly imaginative and constructive genius of American science; with John C. Merriam, paleontologist, wise, far-sighted organizer and director of research; with Raymond Dodge, physiological psychologist, gifted in methodological inventiveness, friendship, and loyalty; with Clarence E. McClung, zoologist, socially minded, devoted investigator and leader in the organization of research; with Victor C. Vaughan, bacteriologist-physician, beloved and widely influential teacher, investigator, friend; with William H. Welch, pathologist, fount of wisdom, adviser of unnumbered thousands of medical students, colleagues, and friends.<sup>[p. 402]</sup>

As, earlier in life, it was my good fortune in Harvard University to be intimately associated with men of genius in scholarship and in the art of living, so somewhat later I enjoyed in Washington the incomparable advantages of working with men such as I have named, of wider and different

experience, more thorough scholarship, more varied insights, and better intellects than my own. One's professional achievements may not be understood if such aspects of social environment as these are overlooked.

In the spring of 1924, seven years after I left Harvard to enter the Army, I was enabled to return to my professional career by appointment to a professorship in the Institute of Psychology of Yale University. This research position I accepted with the understanding that I should be free to devote myself to comparative psychobiology and to promote, as might prove practicable, achievement of facilities for the scientific utilization of anthropoid subjects. The agreement was for a term of five years. Although it did not provide immediately precisely the type of establishment and equipment which I had long desired and labored to bring into existence, it did supply an institutional connection which, largely because of the sympathetic interest and professional knowledge of President James R. Angell, promised to be incomparably useful.

Turning immediately from my administrative and promotional activities in the National Research Council, I devoted the summer of 1924 to anthropoid research in Havana, Cuba, where, thanks to the generosity of Señora Rosalia Abreu, and with the cooperation of the Carnegie Institution of Washington, I was able to observe a large colony of primates. On returning from Cuba, I took up residence in New Haven.

Progress has been rapid in several lines of endeavor during the five years which I have spent in Yale University. Signally important for the realization of my plans are the following achievements: (1) The establishment in New Haven of a special laboratory for psychobiological study of primates; (2) completion of an inclusive survey of the naturalistic and experimental literature of anthropoid life, preparation of an informational catalogue, abstracts, and indices, and the publication of the source book for investigators previously mentioned as *The Great Apes*; (3) supplementation of the New Haven primate laboratory by establishment near Jacksonville, Florida, of a subtropical anthropoid station in which subjects may be bred and observed; (4) perfecting of arrangements for systematic naturalistic study of the chimpanzee and gorilla in Africa; (5) preparation and publication of a program of psychobiological research with anthropoid subjects; and, finally, (6) formulation of plans for a department of comparative psychobiology in Yale University which shall include the existing primate laboratory and be conducted in conjunction with, and as the academic headquarters of, the Florida station.

Throughout this period of continuous intense activity I have endeavored to prepare the way for effective use of anthropoid apes and other primates in the solution of assemblages of problems which include the psychobiological, physiological, psychopathological, anthropological, and sociological. Always the ape has been thought of as means to an end: namely, the solution of important problems which may not readily be approached initially by aid of human subjects. Despite considerable contributions of fact, this section of my professional life may best be characterized as one of systematic preparation for work which doubtless will engage many investigators over an indefinite period.

In 1929, after fifteen years of persistent effort, the provision for anthropoid research which I first proposed and urgently recommended in 1916 finally was achieved. Above I have referred to this consummation of my efforts as the establishment of special primate or anthropoid laboratories and station. Not even the difficulties and discouragements of psychological military service equaled those which at one time or another confronted me in my attempts to secure suitable provision for study of the anthropoid apes. Visionary, impracticable, promising slight returns, too difficult of realization, impossible, are some of the unfavorable characterizations offered as objections to investment in the plan. To have succeeded after so long a period of endeavor is heartening indeed. It renews and redoubles my faith in both plans and objectives and my desire to press forward.

As I write these words (September, 1929) I am on an extended tour of those foreign laboratories whose research equipment, personnel, and publications bear obviously important relations to the psychobiological work which I have projected. I have visited several institutions



and conferred with many colleagues in Europe and am now homeward bound from the African laboratories of the Pasteur Institute at Kindia, French Guinea, which some eight years ago were established for utilization of the chimpanzee and other African primates in the investigation of problems of disease. Few experiences [p. 404] are more inspiring than discovery or rediscovery of the fact that scientific interest, activity, and sympathetic appreciation recognize no geographical, national, or racial limitations.

My immediate work and my plans for the future find appropriate setting in the recently established Institute of Human Relations of Yale University, in which the former Institute of Psychology has been incorporated, and in the Human Welfare Center of which the Institute is an important part.<sup>[6]</sup> I firmly believe today, as ever, that comparative method and infrahuman organisms may and will be made to contribute increasingly and importantly to the solution of a multitude of pressing human problems. I believe also in the logic and fitness of establishing laboratories of comparative psychobiology in conjunction with those of physiology in a great center for research in social biology, and as supplementary to the appropriate special establishments for human psychology, psychotechnology, and the various social sciences.

Such value as this account of my professional life may have for the reader, aside from the satisfaction of his legitimate curiosity, is more likely to come from analysis and revelation of character, motives, and methods, than from simple record of achievements or failures. This assumption is my excuse for concluding with an attempt at revelation and appraisal which, if not complete and adequate, is at least honest.

Physically handicapped from my seventh year by scarlet fever, I have had to conserve my strength and act circumspectly in order to work continuously and efficiently. Probably this explains why intellectual and especially professional satisfactions have come to dominate over physical pleasures. Endowed with a mentality in many respects ordinary, I have always had the advantage of a few wholly extraordinary abilities. Love of work and the power to tap new reservoirs of energy seem to have been paternal heritages which the circumstances of my life greatly strengthened. From childhood I have been able to work easily, effectively, and joyously, even when associates whom I considered my superiors physically and intellectually faltered or failed. This I attribute more largely to exceptional planfulness, persistence, sustained interest, and abiding faith in the values [p. 405] of my objectives, than to unusual intellectual gifts or acquisitions. My love of planning and a degree of prophetic insight therein, which sometimes seems to approach genius, have, I suspect, more than compensated in my professional life for relatively poor memory, a degree of inaptitude for the acquisition of languages which, to the amusement of my family, I often refer to as linguistic idiocy, and almost complete lack of power of artistic expression either graphically or vocally.

As I view my life in retrospect, its professional achievements, and especially its originality, constructivity, and fruitfulness, which many of my colleagues characterize as exceptional, are attributable primarily to the habit of planning with care, foresight, and acquired skill whatever I propose to undertake, to steady unflagging interest and constancy of purpose, and, finally, to persistence which is slow alike to yield to discouragement or to admit failure.

At the age of fifty-three, and though deriving from long-lived stock, I cannot say, as did my paternal grandfather in his sixties, that I have never known fatigue. Instead, it is what I most often have had to work against. Reputed among my intimate friends and my family to be a hard worker, I have never been able to accept the fact, for during most of my years of intense professional activity I have worked not more than eight in each twenty-four hours. It is true, however, that during hours of application my concentration usually is intense and my efficiency relatively high. That the chief if not the only secret of my professional progress is hard work finds illustrative support in my ability to use my native tongue. Not infrequently, when I speak to professional friends of my joy in writing, they voice either surprise or envy. I think I enjoy composition almost as much as I do inventing, planning, or perfecting apparatus and methods or the act of observation, but I cannot discover in my present measure of ability unusual native or inborn gift. To me it seems instead the product of ceaseless practice from youth to the present moment. It is said that I have published much, perhaps it might be said too much, but

nevertheless of what I have written during the last thirty years I estimate that barely one-tenth has been published. Letter writing has, I am sure, immensely increased my facility in expression. If relieved of the irksomeness of making a multiplicity of symbols, I usually would rather write to a friend than eat my dinner!

Aside from the improving influence of practice in writing, I attribute my power of verbal expression to systematic use of the diction-[p. 406]ary early and late, with resultant growth of vocabulary and increase in the precision of use of words. As a boy of twelve I carried in my pocket a handy English dictionary which I consulted on opportunity during the day's farm work. Often in later years I have wondered whose suggestion led me to this method of self-improvement.

Were I required to single out the one characteristic which, above all others, has influenced my professional career it would have to be planfulness. Whenever I have had to compete with my fellows I have succeeded, if at all, by prophetic planning rather than by greater activity or longer effort. The purity of my joy in creative effort -- it may as appropriately be called play as work -- probably is due chiefly to self-determination, for more often than not I have followed freely and consistently my judgments, plans, preferences, and desires, instead of another's. Whether it be a merit or a shortcoming, I am not a good follower. It cramps my dominant trait, planfulness, and reduces me to a species of intellectual slavery. The low levels in my career are due to inhibition of initiation through limitation of self-determination, and, correlatively, the high levels to large freedom for planning and achievement.

Looking backward over thirty years of diligent labor and abundant intellectual, social, and material rewards, I am impelled to view all as preparation for the future. It is as if I were now on the threshold of a great undertaking which from the first was dimly envisaged and later planned for with increasing definiteness and assurance. Whether in this characterization of my past and prophecy for my future I am substantially correct, time will reveal. As ever, I am optimistic and determined. The prospect is alluring, for, as never before, and in a measure beyond my hopes, it promises the fulfilment of my persistent dream for the progress of comparative psychobiology and the enhancement of its values to mankind through the wise utilization of anthropoid apes and other primates as subjects of experimental inquiry.

My professional self and the program of research which has become identified with that self are parts of a movement which will dominate the twentieth century, the socializing of biology. In this great movement, as in the problems which must be solved and the practical services rendered for its facilitation, I am single-mindedly and intensely interested. As a rule remote or inclusive objectives are hidden or obscured by a multiplicity of immediate demands and responsibilities. Therefore, I have presumed to point a goal toward [p. 407] which all mankind is struggling and to claim it as my own. It should not be difficult to merge the self with such a goal or to lose one's life completely in its quest.

It is ungracious to preach to one's professional colleagues. Here they should stop. Only those whose careers are in prospect may safely continue! The wisdom which has come to me from vicissitudes and achievements finds expression thus: to recognize and accept one's limitations cheerfully, bravely, but also intelligently; to choose as vocation, and to render service through, work for which one is well fitted by nature and acquisition, and, in so doing, to utilize one's special abilities to the utmost. This is the best recipe I have discovered for social usefulness and for personal happiness.

I have done scant justice to my creditors in this brief human document. What throughout I have referred to as such actually is not mine. More truly and largely it belongs to those whose work throughout the ages prepared the way for my constructive efforts and to those also who have labored for and with me as teachers, pupils, assistants, colleagues. In contemplation of my debts, I stand humble and reluctant to use the personal pronoun, for the professional strivings and achievements which I have recorded are ours and thine even more than mine. This is my inadequate acknowledgment to those who have gone before and to those who have personally companioned, guided, enlightened, and inspired me.



---

## Notes

[1] Yerkes and Carr (paternal); Carrell and Addis (maternal).

[2] Doctor Southard died February 8, 1920.

[3] Report of the Psychology Committee of the National Research Council. *Psychol. Rev.*, 1919, 26, 83-149. Psychology in relation to the war. *Psychol. Rev.*, 1918, 25, 85-115. The measurement and utilization of brain power in the army. *Science* 1919, 44, 221-226, 251-259.

Yerkes, R. M., and Yoakum, C. S. Army mental tests. New York: Holt, 1920. Pp. 303.

Cobb, M. V., and Yerkes, R. M. Intellectual and educational status of the medical profession as represented in the United States Army. *Bull. Nat. Res. Council*, 1921, 1, 457-532.

The personnel system of the United States Army. Vol. 1. History of the personnel system; Vol. 2. The personnel manual. Published by the War Department, Washington, D. C., 1919.

Psychological examining in the United States Army. *Mem. Nat. Acad. Sci.*, 1921, 15 (Official report).

[4] See "Provision for the study of monkeys and apes," *Science*, 1916, 43, 231-234.

[5] Yerkes, R. M. The work of the Committee on Scientific Problems of Human Migration. *Reprint and Circular Series of the National Research Council*, 1924, No. 58.

Wissler, C. Final report of the Committee on Scientific Problems of Human Migration. *Reprint and Circular Series of the National Research Council*, 1929. No. 87.

[6] Since this was written two years ago. the plan of organization has been altered. My work is now administratively a section of the Department of Physiology of the School of Medicine, Yale University, and I am in charge of the Laboratories of Comparative Psychobiology.

---

# Livros Grátis

( <http://www.livrosgratis.com.br> )

Milhares de Livros para Download:

[Baixar livros de Administração](#)

[Baixar livros de Agronomia](#)

[Baixar livros de Arquitetura](#)

[Baixar livros de Artes](#)

[Baixar livros de Astronomia](#)

[Baixar livros de Biologia Geral](#)

[Baixar livros de Ciência da Computação](#)

[Baixar livros de Ciência da Informação](#)

[Baixar livros de Ciência Política](#)

[Baixar livros de Ciências da Saúde](#)

[Baixar livros de Comunicação](#)

[Baixar livros do Conselho Nacional de Educação - CNE](#)

[Baixar livros de Defesa civil](#)

[Baixar livros de Direito](#)

[Baixar livros de Direitos humanos](#)

[Baixar livros de Economia](#)

[Baixar livros de Economia Doméstica](#)

[Baixar livros de Educação](#)

[Baixar livros de Educação - Trânsito](#)

[Baixar livros de Educação Física](#)

[Baixar livros de Engenharia Aeroespacial](#)

[Baixar livros de Farmácia](#)

[Baixar livros de Filosofia](#)

[Baixar livros de Física](#)

[Baixar livros de Geociências](#)

[Baixar livros de Geografia](#)

[Baixar livros de História](#)

[Baixar livros de Línguas](#)

[Baixar livros de Literatura](#)  
[Baixar livros de Literatura de Cordel](#)  
[Baixar livros de Literatura Infantil](#)  
[Baixar livros de Matemática](#)  
[Baixar livros de Medicina](#)  
[Baixar livros de Medicina Veterinária](#)  
[Baixar livros de Meio Ambiente](#)  
[Baixar livros de Meteorologia](#)  
[Baixar Monografias e TCC](#)  
[Baixar livros Multidisciplinar](#)  
[Baixar livros de Música](#)  
[Baixar livros de Psicologia](#)  
[Baixar livros de Química](#)  
[Baixar livros de Saúde Coletiva](#)  
[Baixar livros de Serviço Social](#)  
[Baixar livros de Sociologia](#)  
[Baixar livros de Teologia](#)  
[Baixar livros de Trabalho](#)  
[Baixar livros de Turismo](#)