Autobiography of Robert S. Woodworth

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Robert S. Woodworth

To begin with, it would seem appropriate for a psychologist called upon for his own story to treat his case as he would that of a problem child, by examining his antecedents and early environment with the object of revealing the causes that have made him what he is. Without attempting quite as much as that, I may at least disclose the fact that I grew up for the most part in New England and that all my ancestors for generations were New Englanders, though my genealogically minded relatives have never succeeded in tracing any of them back to the Mayflower. All the male ancestors seem to have been farmers, except for my father's father who was a school teacher, and for my father himself who was a Congregational minister and whose work took him to many churches in Connecticut, Massachusetts, Ohio, and Iowa. An ardent student of his Hebrew, Greek, and theology, he also read widely on other serious topics, and possessed a library that was awe-inspiring to me as a youngster, though I confess that I found little in it to read. Absorbed in his study and the weekly writing of his sermons, intensely and rather sternly religious, he permitted himself little relaxation with his children, except for afternoon drives about his country parish, when one or another of us was often delighted to accompany him. As I grew up during his mellowing later years, I became less and less afraid of him. He was aged fifty-five when I was born and died when I was twenty.

My mother was thirty-two when I was born, and she was my father's third wife. Her immediate

family were successful farmers who took some part in public life in Massachusetts. She was one of the early graduates of Mount Holyoke Seminary (now College), and herself soon became the "founder" or first principal of a similar seminary in Ohio for young women, now Lake Erie College. She, then, was a teacher, and I might be said to have followed in her steps, since the subjects she taught included especially mathematics and "mental philosophy." I was the oldest of her three sons.

That so important question, where I came in the family, is not so easily answered, since I had four older half-brothers and sisters, two of whom were near enough my own age to be living at home, as young man and young woman, while I was a child. I can remember squabbling with this older sister, though not on fully equal [p. 360] terms. There was a more equal rivalry with the own brother three years younger than I. For five years, from the time I was twelve, while attending high school in a Boston suburb, I spent most of the time in the family of my oldest half-sister, who, with her husband, became like a second pair of parents to me, and whose three daughters were much like younger sisters. So you would probably diagnose me as an oldest child -- Alfred Adler says it shows plainly in my "style of life." I was not free from timidity and feeling of inferiority, nor from a certain bumptousness that broke forth at long intervals.

The "Oedipus complex," as far as I can discern, was represented in my case only by resistance to adult authority. Anything like, mother-fixation does not ring true to me, thinking of my own childhood. My mother, while completely self sacrificing and devoted to her children, was not sentimental or coddling. A, far as I can re member, my attitude towards parents and older brothers and sisters was rather independent, though not exactly courageous.

To judge from my own case, recent emphasis on the "family situation," as all-important in the child's development, is overdone. My environment was the neighborhood rather than the home. In the Connecticut village where I lived from six to twelve years of age --after being born in Massachusetts and living most of the first Six year in Iowa -- and in the Boston suburb of my early 'teens, my competitions were with children outside the home more than with my sister, brothers, or nieces. The boys and girls I played with, the neighborhood bully who made me eat dirt, the men who would talk with me while doing their outdoor work, certainly deserve mention along with my own family as environmental factors. Our gang carried its playful, and sometimes only half-lawful, activities all over the village and out into the surrounding country, and the breaches of home discipline for which my mother had occasionally to snip my ears or my father to apply the birch consisted usually in my outstaying my leave when off with the gang. Always, from the age of six or seven, I had a chum, I had "a girl," I had a group of friends, whose doings loom larger in my memory than what went on within the four walls of home. So when I read case studies of children, in which the members of the family, along perhaps with the teachers, are made to appear the only actors of importance, the picture seems unreal to me, or at least atypical.[p. 361]

Fortunately, however, I have not been asked to trace my development as a human being, but only as a psychologist. My earliest aspiration, as far as I know, was to be an astronomer. Later, at about the age of fourteen, I had very serious intentions of going back to the land and becoming a farmer -- not so far "back" at that, since I lived in a rural community and was accustomed to some varieties of farm work. On graduating from the high school, I made a definite request of my parents to be allowed to attempt a career in music, of which I have always been very fond, but was easily persuaded to go on to college and delay decision on that matter. By the time I finished college, the music had dropped out of sight, except as an avocation, and I was committed to a scholarly career of some sort. Meanwhile, my parents' hope was all along that I should enter the ministry, but their pressure was very gentle, and when my own choice settled upon some form of teaching, there was no family opposition. In fact, I was so enthusiastic a student that my future seemed marked out for me.

But how and when did I come to fix upon psychology? That is a long story, and rather obscure. I remember meeting a word in my youthful reading which I pronounced "pizzicology," but I had no more idea what it meant than do many students today who elect a first course in our subject. Along through my teens, I was much of a Bible student, my interests being somewhat theoretical and quasi-theological; and I vaguely anticipated a study called philosophy, which should deal thoroughly with such matters, and in which I hoped to shine. I also remember Bacon's *Essays* as a favorite reading during those years, and I even wrote an essay or two of my own in the Baconian manner, seeking to set down wisdom in matters of the mind and of human conduct. I will quote a passage or two from Bacon to indicate my earliest models, and the sort of thing I hoped to do. One may say that already while in the preparatory school I aspired to be an armchair psychologist.

In glancing over Bacon's *Essays* just now, I recognize some passages which impressed me in those early days:

> "This communicating of a man's self to a friend works two contrary effects, for it redoubleth joys, and cutteth griefs in halfs; for there is no man that imparteth his joys to his friend but he enjoyeth the more, and no man that imparteth his griefs to his friend, but he grieveth the less."
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> "Let not a man force a habit on himself with a perpetual [p. 362] continuance, but with some intermission, for both the pause reinforceth the new onset, and if a man that is not perfect be ever in practice, he shall as well practice his errors as his abilities, and induce one habit of both, and there is no means to help this but by seasonable intermission."
>
> "The invention of young men is more lively than that of old, and imaginations stream into their minds better, and, as it were, more divinely. . .. Young men are fitter to invent than to judge, fitter for execution than for counsel, and fitter for new projects than for settled business; for the experience of age, in things that fall within the compass of it, directeth them, but in new things abuseth them."
>
> "Reading maketh a full man, conference a ready man, and writing an exact man."

Meanwhile my actual study in the high school and well on into college was concentrated upon the classics and mathematics, with some history, a little modern literature, and very little science. At that time in Amherst College the philosophy course, which included psychology, was deferred to senior year. It was taught by Charles E. Garman, a splendid and remarkable man, regarded by nearly all his students as the best teacher they ever had. I looked forward to this course as the consummation of all things, and managed to secure an introduction to Garman during junior year. With his usual responsiveness to student needs, he inquired as to my preparation for philosophy, and was dismayed at the little science I had studied, for how, he asked, could I grasp philosophy without some acquaintance with scientific ways of thought. He advised me to do as much reading in science as I could during the coming summer vacation, and went to the college library with me to select a list of books which I might read by myself with some profit. There is no doubt that that interview was an eye-opener to me, and a turning point in my career, for thenceforth I regarded science as the general field of my efforts. Such sciences as I could still work into my college course I elected, but I regarded the philosophy course as the main thing.

This course, which extended through our senior year with an average of over six hours a week, started with psychology, and was called psychology throughout by the students. Garman used the psychology which he introduced rather as a means than as an end, choosing dramatic topics like hypnotism to catch the student's interest and lead into philosophical and ethical problems. In September, 1890, when I entered this course, James's *Principles* had not [p. 363] yet appeared, and there was probably no book in existence that would be recognized today as a textbook in psychology -- not in English anyway. Our nearest approach to a psychological text was Carpenter's *Mental Physiology*, a book dating from about 1870, yet not so bad, as I see now on reexamination. Such topics as the modus operandi of sensation, perception, memory and imagination, aroused my interest, but the whole course, which gripped me with all force, was a continuous push towards the solution of fundamental philosophical problems. Garman insisted on our entertaining the most radical hypotheses, thinking them through, weighing the evidence, and coming to terms with each view before we passed on. Like Descartes, and in part with him, we passed through the valley of the shadow of universal doubt, and emerged with what we believed to be an indubitable positive philosophy, though I must admit that I was personally less sure of this positive philosophy than I was that somewhere in this field lay my work. Psychology and philosophy were not clearly distinguished in my mind. As a sample of the type of psychology that I then knew, let me quote a passage from one of the original pamphlets which Garman used with excellent effect in his effort to meet his students exactly on their own ground as he found it to lie from day to day.

> "Everywhere we are correcting and rearranging sense phenomena according to our code. What does not square with this we call illusion. . .. Then, again, as to the order of phenomena. Just keep a 'day-book' and record your mental pictures exactly as experienced and note the inextricable confusion. Here is a sample: Sitting in my study during a summer evening I am startled by a brilliant flash of lightning; item No. 1. Some one cries out in fear; No. 2. Doorbell rings. A book agent enters and insists on showing me a new atlas. Just as I am looking at the chart giving the ocean currents I hear a heavy clap of thunder; items 3, 4, and 5. Next the rain falls in torrents. My telephone rings and I talk with my friends who tell me that their house was struck. The railroad train whistles. Then comes a gust of wind that is a veritable hurricane. Conversation follows about the storm. Book agent presses his claims for further examination of maps and I am soon in China studying the position of Russia. Storm subsides -- other dashes of lightning -- telephone again rings -- more thunder -- other callers come. I retire and dream of China. Here are numerous items badly confused. In the morning I go to my classes. In the afternoon I take a drive and find a bridge up and a tree [p. 364] shattered. Here are a few phenomena, but there are a multitude that I have not recorded. No two days is there the same sequence, yet somehow all this confusion causes me no trouble, for from the 'day-book' I post a ledger and connect events not as they appeared but as they really happened. Then I make the lightning the antecedent, not of the coming of the book agent, but of the thunder and the riven tree. The loss of the bridge was the sequence, not of my drive, but of the storm the night before. Not in the day-book of sense, but in the ledger of common sense or judgment is there order" (Eliza Miner Garman: *Letters, lectures and addresses of Charles Edward Garman*, 1909, pp. 213-214).

On graduating from college in 1891, I thus had some acquaintance with a philosophical type of psychology, and a definite slant towards that subject. Yet it was twelve years more before I was definitely committed to a career in psychology. At the outset I was advised to teach for a while, rather than to continue to "absorb." I taught mathematics and science for two years in a secondary school, and mathematics for two years more in a college. During these last two years I devoted myself assiduously to mathematical study, and, when I broke off teaching to repair to a university, it took me several months to decide whether to continue in mathematics or to swing back to psychology.

But, during these four years of teaching, I had been subjected to two important influences towards psychology. These influences were William James and G. Stanley Hall. I possessed myself of James's *Principles* soon after its publication and was much stimulated by it. Hall's conception of a university as the home of untrammeled study and research had roused my enthusiasm as a senior in college and contributed towards my choice of an academic career; and some years later I heard him lecture and was much taken by his way of saying, "We now know," or "We are just finding out." I seemed to glimpse the frontier of scientific discovery, and, on returning to my room, I inscribed a card with the motto, INVESTIGATION, and suspended it by my desk. Though my "investigations" for the time being were mathematical and not psychological, the influence was felt a year later, when, on entering Harvard, I decided to quit mathematics for psychology and philosophy, the two not being clearly distinguished in my mind, any more than they were in the organization of the university.

My first two years at Harvard were divided almost equally between philosophy and psychology, and my principal teachers were James and Royce, to each of whom I was much devoted, while each of them was kindness itself to me. With James I studied general and abnormal psychology. Münsterberg was back in Germany for these two years, and I did not come into contact with him, but his place in the laboratory was taken by Delabarre, with Lough as assistant, and from them I had my first lessons in psychological experiment. Several of the subjects on which I worked and wrote for Royce and James have continued to interest me. The perception of time was one of these, on which, however, I have never published, though I have had students working on this problem. Another subject was "Thought and Language." I was challenged by the dictum of Max Müller, one of the folk-psychologists or philological psychologists, to the effect that there was no thought without language, and that the science of thought should be based upon the science of language. My own experience did not bear this out, since I often had difficulty in finding the words required to express my meaning, and since, in geometrical thinking, which had been one of my favorite pursuits, I was sure that I thought in terms of diagrams and gestures rather than in words. In fact, to think clearly in geometry I had to get away from words. Max Müller had said that counting would be a crucial case, and had asserted that counting could not be done except in words; but I found by experiment that I could count by rhythmical groupings and could group the groups and so work up to over 100, converting the rhythmical result afterward into ordinary numbers. I have returned to this subject a number of times, as in considering the curious discrepancy between colors as seen and colors as named, and again as incidental to the work on imageless thought; and recent attempts to revive and modernize the old theory that thinking consists in speaking have always found me skeptical, mostly because my own experience convinces me that there are other modes of thought besides the verbal, and that these other modes are more direct and incisive.

This study of thought and language was begun as a term paper in Royce's course on logic. James, in his abnormal psychology -- a course in which he was at his best, and in which he became well known to his students through the visits to institutions on which he piloted us -- James set me to work on dreams. Besides consulting the literature, I recorded many of my own dreams and made certain [p. 366] experiments on the speed of continued association and revery in waking conditions, as a check on the often asserted extraordinary speed of dreams. I found the speed sufficient in waking revery to account for all the instances of rapid dreaming that had seemed so remarkable. I also was led by my readings and records to a hypothesis on the cause of dreams that I have often wished I had published, as it has a certain resemblance, along with a difference, to Freud's conceptions which were published a few years later. Ives Delage had pointed out that we do not dream of matters that fully occupy us during the day, but of something else. I thought I could see that we dreamed about matters that had been opened up but interrupted or checked during the day. Any desire or interest aroused during the day, but prevented from reaching its goal, was likely to recur in dreams and be brought to some sort of conclusion that was satisfactory in the dream, while activities that had probably taken much more time and energy during the day, but had been carried through to completion, were conspicuous by their absence from the dream. But the wishes "fulfilled" in the dream, according to my idea, were of any sort -- sometimes mere curiosity -- and the suppression of them which had occurred during the day might be the result of external interruption as well as of moral censorship.

Other problems which took hold of me during those student days and which have continued to exercise me are those of motivation and of the mind-body relation. I remember saying to Thorndike, my fellow student, whose sane positivism was a very salutary influence for a somewhat speculative individual like myself, that I was going to try and develop "motivology"; and he agreed that it was worth doing. Always searching for some fruitful attack on this problem, I was naturally much interested in the works of Freud and McDougall a little later; and I have taken one or two shots at the problem myself, but have to agree that the desired science of motives is still very embryonic. As to the mind-body relation, it was not till some years later that I reached any solution that satisfied me.

I reached the end of my second year at Harvard without definite commitment to either philosophy or psychology. In philosophy, I had passed through a stage of absorption in the pantheism of India -- the "That art thou" philosophy -- but Bradley's *Appearance and Reality* had about convinced me of the relativity of all human modes of thought, so that no positive system of philosophy could claim any [p. 367] absolute validity. But still I was much interested in ethics and especially in logic -- as taught by Santayana and by Royce -- and was quite willing to continue working at them. But need for a decision arose when James secured for me the opportunity of a year in the physiological laboratory and recommended it strongly if I were going on with psychology. I consulted Royce on the matter, and he suite agreed that it might be better for me to choose psychology! And so, in 1897, I turned from philosophy, half expecting to continue some effort in it, but discovering, as time went on, that psychology was amply sufficient occupation, and that philosophy would be an undesirable distraction. The path of psychology at that time led between mountains, down through a valley that seemed to open out below into fertile country; but there were alluring trails up the mountains into which one was likely to stray with such satisfaction as to lose interest in the arable land below. Nowadays, psychology has emerged upon the plain, and the mountains are more distant and less enticing.

But the choice of psychology meant physiology in the first instance, and five of my next six years were spent in physiology, the final decision for psychology not being reached till the end of that time. The physiologists with whom I studied and taught were Bowditch and Porter of Harvard, Graham Lusk of New York, Schafer of Edinburgh, and Sherrington, then at Liverpool. My physiological studies were on the heart, stomach movements, carbohydrate metabolism, electrical conductivity of nerve, cerebral localization, and reflex action. Of my fellow students during this period, I specially remember Cannon at Harvard, and his early studies of stomach movement by aid of the X-rays, in the course of which he was led to study visceral processes as related to emotion and thus to establish an important link between physiology and psychology.

But meanwhile one of these years had been devoted strictly to psychology, and during that year at Columbia I was working with Cattell, whom I count as the chief of all my teachers in giving shape to my psychological thought and work. His emphasis on quantitative experiments of the objective type, and his interest in tests for individual differences, were powerful influences with me. During this same year I studied anthropometry and statistical methods with Boas, and gained from him and also from Farrand some appreciation of the value of anthropology to a psychologist. My experimental work dur-[p. 368]ing this year was on the control of muscular movement, a topic on which I continued to experiment at intervals for some years. Soon afterwards, while teaching physiology, I collaborated with Thorndike in studying the question of transfer of training, and this also is a subject to which I have returned again and again.

At the beginning of 1903, then, I was Sherrington's assistant at Liverpool, and much minded to make my psychology contribute to a career in brain physiology, rather than vice versa. Sherrington, to whom I owe very much, was willing that I should remain with him and develop my experimental psychology and brain physiology together. Just at this juncture, Cattell called me back to Columbia to work at experimental and physiological psychology, and careful consideration indicated that this was, after all, the line for which I was best prepared. Never was a finer chief than Cattell, alike in personal, departmental, and strictly scientific matters. So, fully twelve years from college graduation, after studying mathematics, philosophy, and physiology, I finally settled down to psychology as a member of the staff of Columbia University, and there I have steadily remained, aside from certain summers and leaves of absence. One of the latter, in 1912, I spent in Külpe's laboratory at Bonn. Though I was over forty years old at that time, I like to count Külpe among my fathers in psychology.

In returning from England in 1903, to enter the Columbia Psychological Department, I was fortunately able to bring with me a young wife, and we soon became part of a small group of congenial young couples in the University. For years we lived in the "Montrose colony" in the woods nearly forty miles up the river, with the Thorndikes, the Woodbridges, the Keppels, the Bagster-Collinses, and others later, and our four healthy, lively youngsters grew up in a group of twenty children. Though I never attempted any systematic psychological study of my children, I had my eyes psychologically open in watching them, and have certainly learned much from them. As they have grown up, I have not made any effort to steer them towards academic careers -- in spite of my own great satisfaction with such a career -- and they have tended towards the business field. For a period of years, I had considerable land to play with, and actually did get "back to the land" rather intensively, in the way of gardening, wood-chopping, and road-making, heartily enjoying this outdoor work and perhaps spending too much time on it. Always delighted with the woods, the mountains, the plains, the [p. 369] sea, I have been in later years quite an enthusiastic motorist, enjoying both the driving itself and the trips and scenery. Sometimes I have wished that I had gone into geology or anthropology, so as to have a professional excuse for faring afield. I have never succeeded in solving the problem of finding time for outdoor interests, family interests, musical interests, general reading interests, university, and departmental interests -- all of which have been very genuine interests in my case -- and still concentrating on what always remains my main interest, psychological research.

Even within the confines of psychology there is a wide field to wander in. My lectures have varied in topic from year to year. Aside from the general introductory course, in which I have taken a hand from time to time, my stand-bys have been experimental and physiological psychology. But for many years I lectured on abnormal psychology, and for another long series of years on social psychology. At one time or another I have lectured on tests, statistics, the "problems and methods" of psychology, its theory, history, and applications. Special topics on which I have repeatedly held seminars include movement, vision, memory, thinking, and my old hobby, motivation. Of late years, however, I have limited my courses to experimental psychology and to a survey of contemporary schools and debated questions.

From the beginning the research activities of the staff and students have centered in the "Seminar" -- Cattell's Seminar, as it was at first. Here each candidate for the doctor's degree presented his research plans, his progress from time to time, and finally the outcome of his work, for consideration by fellow students as well as professors. Cattell's criticism could be keen as well as kindly. He was skeptical of any result that did not come out with a small "probable error," and with work which did not take account of what had been achieved by previous investigators. I remember one student whose seminar report seemed to indicate both sloppy work and poor perspective, and who disappeared altogether from our midst directly afterward. In the selection of dissertation topics, Cattell followed a plan which may have been derived, by antithesis, from that of his master, Wundt. Cattell expected the student to make the first move. The student was expected to have a problem on which he desired to experiment, and, if a workable plan of attack could be mapped out, he was told to go ahead and to depend largely on his own initiative. [p. 370] I have followed Cattell in this respect, but the plan has certain disadvantages with a large group of students, many of whom desire above all things to get away from the "conventional" and, if possible, to discover something about "personality." I have given some sort of advice and guidance to students working on a great variety of problems. Of recent years, with a larger staff to divide the field, and with the attitude taken by the University (as represented especially by Dean Woodbridge) and by Poffenberger as executive head of the department, that each professor should have his own research interests to which the student must adjust himself, the scattering of effort has mostly disappeared.

But the scattering of effort has not been entirely the fault of the students. There seem to be many interesting problems in psychology, and from time to time new ones have been added to the list of my active interests. I have mentioned the thinking process, time perception, transfer of training, motor control, and motivation, as topics which interested me in my days of apprenticeship; and these have reappeared time and again in the work of my students. The study of motor control led over into an examination of the question as to whether or not kinaesthetic images were essential as the immediate antecedents of voluntary movements. In this study I departed from the custom of our group and used the introspective method, feeling half ashamed of myself for doing so; but the agreement of different subjects seemed to justify the method, and I continued to use it for an examination of images in perception and thinking, and thus was led into the "imageless thought" controversy. This was in 1906-1908.

I recently found in my files an old memorandum with the heading, "Hammering at the images," which projected several additional ways in which an attack could be made on the false prominence of the image in psychological theory. However futile the imageless thought discussion may now appear, it played a part in relegating images to the relatively minor position that they occupy in present psychological theory. I was far from doubting the existence of images, for I have abundant auditory images myself -- of speech, of music, of noises -- and I have not the least reason to question the testimony of psychological colleagues who speak with similar certainty of visual images in their own cases. The bald statement, sometimes heard of recent years, that images do not exist, strikes me [p. 371] as simply vaporing; while the statements that they are muscular movements, or sensations with present peripheral stimuli, are hypotheses worth entertaining, but far from established and with the balance of probability against them, in my opinion. At any rate, the processes that we have called images really occur in abundance; of that there can be no doubt; but the point of the imageless thought contention was, and is, that these imaginal processes are often almost if not quite absent just when thought is proceeding actively, and that, therefore, there must be thinking processes which are nor imaginal processes. I still believe that this finding is genuine and of importance in dynamic and physiological psychology.

But the question of images in thinking is only a small part of the whole problem; and I like to believe that the series of studies of thinking that have issued from time to time from our laboratory have contributed bits towards the understanding of this fascinating performance. It is a rather elusive sort of performance, and, though introspection shows us much about it, the great need is to find objective methods for studying it in the laboratory. Promising leads are to be found in the memory experiment and in the transfer experiment. Memory can be aided by seeing relations in the material to be learned, as G. E. Müller has abundantly shown; and, consequently, it would seem, an *aided memory* experiment should afford an objective means of studying relational thinking. Again, problem solution, depending as it certainly does on the utilization of past experience, demands the *transfer* of what has been learned into the novel situation. It is partly for this reason that memory and transfer experiments have continued to appeal to me and have appeared frequently in the output of the laboratory.

Motivation has always seemed to me a field of study worthy to be placed alongside of performance. That is, we need to know not only what the individual can do and how he does it, but also what induces him to do one thing rather than another and to put so much energy into what he does. We need a study of motivation in order to understand the selectivity of behavior and its varying energy. In my books I have sought repeatedly for a formula that should bring motives right down into the midst of performance instead of leaving them to float in a transcendental sphere. The main object of such a formula, provisionally, is to free the conscience of the hardheaded experimentalist of any qualms he might otherwise feel in entering this subject. Here, again, a survey of the studies that have come [p. 372] out of our laboratory yields the comforting thought that, though I have not personally conducted many researches, I have probably played some part in an advisory capacity.

My interest in psychophysics was stimulated in the first instance by my master, Cattell. Certainly the psychophysical methods present themselves as a challenge for further inventiveness as well as for patient standardization. When I have assembled the results now available for generalization, I have been dismayed by their divergence of methods and consequent lack of comparability. At this point, psychology comes into much-to-be-desired relations with such advanced sciences as physics, chemistry, and astronomy, and should certainly be eager to do its share in bringing the subject into some kind of order. This interest also has borne some fruit in the research of the laboratory.

At intervals my old mathematical interest has re-asserted itself, and I have spent happy days endeavoring to work out some useful statistical device or in making statistical computations and graphs. With the accomplished mathematicians who are now marching in the psychological procession, I have naturally fallen far behind the band, but without losing the thrill of it.

At various times, from 1904 on, I have tried my hand at the devising and perfecting of tests, the chief work of this sort being the joint product of Wells and myself, the *Association Tests* of 1911. The "Psychoneurotic Inventory," or "Personal Data Sheet," was another effort. There have been many student researches in the field of tests that I have supervised more or less closely. Of late, in the division of labor within the Department, I have ceased to concern myself actively with tests, though I will admit that I still have in the back of my mind one or two schemes for tests that I should like to work out.

With all this sad array of scattered interests, I hope I shall receive credit for not dabbling to any appreciable extent in animal psychology, which is, in fact, a branch of psychology in whose general significance I most heartily believe and in which I should have liked to be myself a worker. The same can be said of child psychology. I have done what I could, as opportunity offered, to push forward these lines of research.

The story would not be complete without reference to activities that have taken me outside the University -- and the University, it [p. 373] should be said, has been generous in lending its men to worthy scientific or public enterprises. The first such enterprise in which I took part was the World's Fair at St. Louis in 1904. Having provided for the assembling of representatives of many different races, the Fair also made provision for anthropometric and psychometric study of these samples, and I had direct charge of this work, with Frank G. Bruner for my chief assistant. We examined about eleven hundred individuals, making the standard physical measurements of the anthropologist, and also testing muscular strength, speed and accuracy, vision and hearing, and intelligence as well as we could with formboards and other simple performance tests that we devised. When the Fair was over, we promptly worked over our data, and reported some of the results at scientific meetings. Bruner published the results of the auditory tests as his dissertation, and I gave a general summary of our results and their bearing on the question of racial differences in mental traits. Further than that, the results have never been published, not from any doubt on our part as to their value, but partly because of the unlimited number of fascinating correlations which still remained to work out, partly because of the expense of publication, and partly, I am afraid, from a certain inertia or indifference to publication on my part. Once I have worked out the results, and perhaps reported them at a meeting, I feel satisfied.

There are a number of other studies which I have brought to some sort of conclusion but never published except in the reports of meetings where I have presented them. One such paper, read in 1905, demonstrated to my own satisfaction that vision during eye-movements was just about what would be expected from the retinal stimulation received and thus afforded no ground for assuming any special inhibitory effect of the eye-movement upon visual sensation; but I did not publish this paper, because I found that most of my confreres needed no elaborate convincing of this proposition. Another paper developed a statistical method of measuring rank order correlation which had certain advantages over the method in use; but, as it had also certain disadvantages and was not received with any show of enthusiasm, I let it drop. In other instances, I do not have so good an excuse for letting my work go unpublished.

When the War reached America, my strong inclination was to respond to the call for psychologists in the Army testing service, but conditions seemed to demand that I be the one to stay at home and [p. 374] carry on some semblance of psychological instruction in the University. The American Psychological Association entrusted me with the duty of seeking a test for emotional stability. The experience of other armies had shown that liability to "shell shock" or war neurosis was a handicap almost as serious as low intelligence. After considering other possible emotion tests, I concluded that the best immediate lead lay in the early symptoms of neurotic tendency which the neurologists and psychiatrists were finding in the case histories of neurotic subjects. Collecting hundreds of such symptoms from reported case histories, I threw them into the form of a questionnaire which could be applied to a group of subjects at a time, the single questions to be answered Yes or No. I tried this questionnaire on normal groups, and eliminated questions, or so-called Symptoms, which were reported so frequently by the normal subjects that they could scarcely have any diagnostic value. The abridged questionnaire was tried on a thousand recruits in one of the camps, and on small groups of diagnosed abnormal subjects, and the results worked up again and submitted to a conference assembled by the Surgeon General to advise him as to the military use of the questionnaire. The decision was to give the device a trial as part of the psychological examining procedure in one of the camps. Soon afterwards, the War came to a close, leaving the question unsettled as to whether or not the questionnaire would really assist in discovering the recruits who were specially susceptible to psychoneurosis. The idea was to use the quantitative score of unfavorable responses as a first indicator, to be followed up by individual examination at the psychiatrists' hands. At all stages of this work on the "Personal Data Sheet," I had valuable collaboration -- that of Poffenberger in preparing the first draft, before he went into the Army, and that of Boring in securing the results from the Army samples. Hollingworth used the questionnaire on "shell shock" cases invalided home, with interesting results. Since the War, quite a number of psychologists have used the questionnaire or modified forms of it, and, though the results have never been striking, it still seems to have possibilities of usefulness.

Since the War, I have had the honor of participating in the activities of the National Research Council and also of the Social Science Research Council, and it has certainly been a liberal education to come thus into contact with leaders in the sister sciences, and with problems which call for the cooperation of workers from dif-[p. 375]ferent disciplines. From the year that I spent at the National Research Council, I remember with special satisfaction my association with the committees on the "psychology of the highway" and on "child development," both of which undertakings are still going on, as, indeed, there is every reason why they should. It came rather as a surprise to the psychologists, when the social science group invited our Association to participate in their Research Council; but I have found this group entirely hospitable to psychology and hopeful of advantage to social science from association with psychology. There is no doubt in my mind that psychology is properly both a biological and a social science, and the logical meeting place of those two groups of sciences.

Of all the organized groups that I have learned to love, none is dearer than the American Psychological Association, whose annual meetings I have attended with but few exceptions since 1898. Outsiders sitting in our meetings sometimes get the impression of mutual hostility within our group, but I am sure that is a false impression. My own impression is one of fundamental solidarity, along with the freedom of discussion that comes from direct handling of the subject-matter. If I were listing the honors that have fallen to me, I should place first that of being elected President of the Association.

Of my books, the earliest was a monographic analysis of the literature on movement and the perception of movement. There was a small book on personal (not mental) hygiene, emanating from my years as a physiologist. Next in time came a much more extensive piece of work, the collaboration with Ladd in the revision of his *Elements of Physiological Psychology*. In the revision, Ladd took care of the more philosophical parts of the work, and I was responsible for nerve anatomy and physiology and for experimental psychology. *Dynamic Psychology* was a reaction to Titchener, Watson, and McDougall, and sought for a position that should be independent and yet have room for all genuine psychological efforts. It sought also to show that the study of motivation had a proper place in psychology, no matter how positivistic the science should be. More recently I have written and rewritten an elementary textbook, and, like many other textbook writers in our science, have tried to make this a scientific contribution, by clarifying my own ideas, keeping abreast of developments, and interrelating the several topics. A survey of the contemporary schools now completes the list, but I am laboring hopefully, with many interruptions, at a general book on [p. 376] experimental psychology, which should soon be finished, since it was started fifteen years ago! My object is here to digest the literature on as many topics as possible in experimental psychology, weighing the evidence and effecting some measure of synthesis of the established findings.

Though my ideal all along has been "investigation," and though I have been busy all along with research in an advisory capacity, I have done comparatively little investigation on my own account. Probably my bent is more towards weighing evidence and "seeing straight" than toward active enterprise. I should have liked to be a discoverer, so that anyone asking, "What did Woodworth do?" would be promptly answered, "Why, he was the man who found out" this or that. It is likely that many other psychologists have the same feeling of disillusionment. It seems as if real discoveries, on a par with those in some of the other sciences, simply were not made in psychology. As I diagnose the situation, we started thirty or forty or fifty years ago with a background of philosophical problems. These have gradually disappeared from our view, because they were not genuine psychological problems, and we are left with what seems to be a multitude of rather disconnected problems, none of them appearing as very fundamental. We are, then, passing through the stage of becoming acquainted with our subject-matter in detail and for its own sake, and there is no telling when or where discoveries of really fundamental significance may be made -- probably where we least expect them.

My bogey men -- the men who most irritated me, and from whose domination I was most anxious to keep free -- were those who assumed to prescribe in advance what type of results a psychologist must find, and within what limits he must remain. Münsterberg was such a one, with his assertion that a scientific psychology could never envisage real life. Titchener was such a one, in insisting that all the genuine findings of psychology must consist of sensations. Watson was such a one, when he announced that introspection must not be employed, and that only motor (and glandular) activities must be discovered. I always rebelled at any such epistemological table of commandments.

The desirable principles, so it seems to me, are those that free the investigator rather than those that restrict him. My thinking on the mind-body problem has been guided, latterly at least, by some such desire for freedom. I must have entered my first psychology [p. 377] course, as an undergraduate, with some half-formed spiritualistic conception, for I remember the shocked resistance with which I first encountered the notion that thought was in any degree dependent on the brain. Carpenter's evidence regarding the effect on thinking of fever, old age, and blows on the head, reminding me as they did of some experiences of my own, converted me to his interactionist view, which seemed at the time very radical and gave me a feeling of daring freedom from my older theological views. Reading Lotze in the years immediately following, and Paulsen and Höffding while a student at Harvard, about shifted me over to the parallelist position. I was impressed by Höffding's words in a book which James placed in our hands:

> "What we in our inner experience become conscious of as thought, feeling and resolution, is thus represented in the material world by certain material processes of the brain, which as such are subjects to the law of the conservation of energy . . .. It is as though the same thing were said in two languages . . .. Here this hypothesis interests us as the most natural determination of the relation between physiology and psychology. These two sciences deal with the same matter seen from two different sides, and there can no more be dispute between them, than between the observer of the convex and the concave side of a curve (to make use of a simile employed by Fechner). Every phenomenon of consciousness gives occasion for a twofold inquiry. Now the psychical, now the physical, side of the phenomenon is most accessible to us." [H. Höffding: *Outlines of psychology*. (Trans. by Mary E. Lowndes.) London, 1893, pp. 65, 69.]

James, in his lecture, joined battle with this view, supporting interaction. When I asked him, after class, if he did not think that parallelism was a good guide for the investigator, leading him to the full cultivation of his own specialty, James replied that precisely that was what parallelism was not good for, since the line of scientific progress led, the rather, by way of tracing the interaction of mental and physical.

I was left with a question in my mind rather than a conclusion. Parallelism seemed to have the neater logic, but to suffer from an atmosphere of unreality. As my work in the next few years took me into both psychology and physiology, the problem remained a very genuine one for me. I came to see, to my own satisfaction at [p. 378] least, that the parallelism that we know is really a parallelism of sciences. It is not a parallelism of different processes, but one of different scientific descriptions of the same process. By 1908, I had reached a view of the matter that still appears sound. The parallelism is not necessarily between the psychical and the physical, but may and does occur whenever different sciences set themselves to the description of the same natural process. The different sciences will employ different techniques, and, in particular, one science will go into finer detail than the other, even as one map goes into more detail than another map of the same country. While the detailed map certainly includes much that does not appear in the comprehensive map of a larger area, it has to leave to the latter the presentation of the broad geographical relationships. Thus the same real object can be given description at two (or more) different "levels" or magnifications. The same parallelism appears between gross and microscopic anatomy, between organ physiology and cellular physiology, between geology and the physics and chemistry of the minute processes that enter into the broad geological processes.

Let me take a more concrete example. Cellular physiology reveals something of the "fundamental" or "underlying" processes that go on in the heart muscle; but, if we want to understand the heart as a pump, we must study its action in another, less minute way. Both approaches are needed, and neither makes the other superfluous.

In the same way, the psychologist describing a conditioned reflex in terms of stimulus and response, and the physiologist describing it, as far as possible, in terms of nerve currents, etc., are describing the same identical process, the physiologist in more detail, the psychologist with more breadth. The chemist would demand still finer analysis than the physiologist gives, and the sociologist might wish to include the conditioning of the individual in a still broader view than is taken by the psychologist. Parallelism, then, is not necessarily psychophysical, but occurs whenever a more detailed and a more comprehensive description of the same thing or process are undertaken.

It may be urged that the difference between a brain process and a conscious process or experience of the subject is a more radical difference than those we have brought forward. It may seem that the psychical and the physical are so absolutely unlike that they cannot possibly be the same identical process differently described.[p. 379] I used to think so, but have concluded that I had no real reason for thinking so. Without arguing the case here, I will merely point to the undoubted fact that the experience of hearing a tone, for example, belongs to a whole process, while a physiological description would certainly go into detail. If a subject under suitable acoustic stimulation reports hearing a tone, and a physiologist inspecting his brain reports certain detailed processes as occurring, there is no longer any doubt in my mind that these detailed processes are parts of the identical total process which the subject himself reports.

I should be inclined to urge that this "levels of description" theory is not quite the same as the two-language or double-aspect theory with which it started. The theory to which I have become attached (1) is not limited to the psychophysical situation, (2) is not limited to just two aspects, and (3) purports to give some account of what the difference is between the several aspects, viz., a difference in degree of breadth (or of detail) of observation and description.

The value of the theory, to me, is that it keeps all of psychology, introspective and behavioristic, within the bounds of natural science. The hearing of a tone is no less an event in the stream of natural events than the movement of the arm, or than the physiological processes into which either of these total processes may be analyzed. The theory has a similar value in saving for psychology (and for the social sciences as well) a place to fill in the general framework of natural science. Once and again I have heard it predicted that psychology would pass away in proportion as physiology developed. The finer analysis of physiology would make the coarser psychological descriptions superfluous. Psychology, it seemed, had no ultimates of its own, and could be completely resolved into either physiology or thin air. The analogy with cellular and organ physiology comes to our rescue here, showing that the finer analysis cannot do the work of the coarser.

As to "ultimates," I look at the matter this way. No matter how completely you describe the cells composing the heart and their activities, you need to take account of the structure of the whole heart in order to tell how the organ behaves. The total structure is an ultimate in describing the action of the organ. Let me add a geometrical illustration. A ring composed of two concentric circles seems merely a derived form, and all its properties can be deduced from the properties of the circle. Yes -- granted the definition of the ring as [p. 380] composed of two concentric circles with specified radii. This structure of the ring must be given; it is an ultimate, not to be deduced from the geometry of the circle. For psychology, the ultimates are not the electron and proton, but the individual and the fundamental types of activity determined by the organization of the individual and by the situations in which he is placed.

Such general considerations cannot be expected to serve the investigator as a guide, but they may free him from inhibitions and from a sense of futility. If I were advising a young investigator, from the standpoint of my forty years of psychology, I might point out this or that promising topic for study, but I should be more likely to tell him that the whole field was still new and open before him. He need not despise what has already been done, for it affords a much better first-hand acquaintance with the field for investigation than was available forty years ago. But many incisive discoveries remain to be made. For getting on the trail of what will prove to be important and fundamental, there is no sure rule to be given; but the experience of investigators in many fields does seem to show that persistent following up of what is queer and out of line with accepted beliefs often leads to significant discoveries. What we seem to need in psychology is surprises; and by following up a small surprise one may find a greater one beyond. Or one may not -- such are the chances of the game.