CHARLES H. JUDD

My parents were missionaries in northern India. They went to India shortly after the Sepoy Mutiny and served somewhat more than twenty years in various stations near Lucknow. During the last years of their stay both were in poor health. They returned to the United States with their family of three children—my two sisters and myself—in 1879. My father died during the following winter and my mother died in 1884, after an invalidism of four years.

The most vivid impressions of my childhood are related to the intense religious devotion of my parents. They were both evangelists and pietists. They had the most implicit faith in the literal interpretation of the Scriptures and exerted every endeavor to bring up their children in the same faith. I remember that I was disturbed in my high-school days by some books on evolution that came into my hands. I recognized the apparent conflict between the teachings which had been given in the family and the doctrines taught in the books. My curiosity was aroused, and I became greatly absorbed in books on anthropology and biology, a number of which I found in the public library.

The hope of my mother had been that I would enter the clergy, and when I started for college, I was disposed to prepare for the Methodist ministry, following my mother's wishes and the example of several generations of the family.

I have no doubt that my early intensive religious training had a large influence in directing my later interests toward a study of man and his mental life. I abandoned the plan of becoming a preacher early in my college career and have found my chief interest in scientific rather than religious solutions of the questions that arise with regard to man and his nature.

When my parents came to the United States after their long residence in India, I was six years of age. I had learned to read, as the youngest child of a family often does, through the unsystematic training given by the older members of the family. I spoke a combination of English and Hindustani which I found was a source of great amusement to my American relatives and to my playmates. I soon dropped the Hindustani and acquired the vernacular of my new environment.

I was entered very shortly after our arrival in America in the first

grade of the public schools of Binghamton, New York. My father had selected Binghamton as a residence for two reasons. First, it was near his former home in Candor, New York, and, secondly, the schools of Binghamton were reported to him to be excellent. He was often quoted to me as saying that he could leave his children very little but hoped to launch them on their careers with a liberal education.

I went through the elementary schools and the high school of Binghamton, graduating from the latter in 1890. True to his expectation, my father had left his children little in the way of a material fortune. My older sister, with a great deal of effort, kept the family together, and my younger sister and I graduated together from the high school.

Three men stand out in my memory as exerting the largest influence over my education in Binghamton. The clergyman in the Methodist church which we attended took an interest in the orphan boy who lived across the street from the parsonage. The Rev. George Murray Colville did much—I hardly know how much—to arouse my intellectual interests. I recall one conversation of the many which I had with Dr. Colville. He called me into his study and remarked on the poor record that I was making in my high-school work. He said it was the judgment of my teachers that probably I had a very low grade of intellectual ability and he suggested that I show them that they were wrong. Certainly that conversation demonstrated the wisdom of Dr. Colville in dealing with a surly adolescent boy. I began to work with a vigor that astonished everybody, myself included.

Later, it was this same Dr. Colville who made it possible for me to go to a German university and obtain my doctor's degree. He lent me the money for the trip and generously waited long years for the return of his loan.

I had many books to read from Dr. Colville's library, and he took me on several trips, one to New York and one to Boston. I have no doubt that I owe to these trips and to my contacts with Dr. Colville much more than I can remember.

Another man who influenced me very greatly in my high-school days was the science teacher of the Binghampton High School, R. W. Griffiths. His domain was on the top floor of the high-school building. Here, in what was a kind of unfinished, spacious attic, one end had been partitioned off into a science laboratory. There were

microscopes, chemical scales, physics apparatus, a three-inch telescope, and a heliostat, all closely packed together in this laboratory. Professor Griffiths presided over all the sciences. He used to allow a few of us who were especially interested to have keys to the laboratory, and mornings before school and afternoons after school we used the apparatus and added to the scientific enthusiasm which we gained from courses in Steele's *Physics* and other similar brief surveys of science. I recall that one day Professor Griffiths pointed out the difference between an expert and an amateur in their methods of handling a microscope. "Both of them seem to be careless," he said. "Both pick up the microscope in what seems to be the same easy-going way. But notice," he went on, "I pick it up by the base; you pick it up by the tube. You are ignorant."

Perhaps we did not learn much of the content of science from Professor Griffiths, but we acquired a lasting enthusiasm for Leyden jars and the things one sees in the field of a microscope and in the field of a three-inch telescope.

The third man who influenced my high-school years and, through them, my whole education was the principal of the high school, Eliot R. Payson, now professor at Rutgers College. Professor Payson taught us Latin and Greek and a great many things not written in the books. He was a teacher of unique qualities, as I learned afterwards when comparing my college preparation with that of my college classmates from other schools. Professor Payson was exacting, sometimes harsh, I should say. He was absolutely just, and he knew how to be familiar with boys without losing one grain of his dignity. He found a number of us one day engaged in disorder. He looked at us in silence for a few minutes while we stood petrified and then he calmly left us—wiser and, on the whole, I think, more moral beings. He gave us a very high grade of training in the classics. I have always thought better of his teaching than of the classics.

From high school I went in 1890 to Wesleyan University at Middletown, Connecticut. Wesleyan was, more than most colleges of that day, devoted to scientific research and literary production. On its staff were Professor Winchester in English literature, Professor Atwater in chemistry, Professor Rice in geology, Professor J. M. Van Vleck in mathematics, and a number of others whom I shall have occasion to mention as teachers who contributed directly to my special training.

The curriculum at Wesleyan in my day included a number of

required courses. We were required to take analytical geometry in the sophomore year. It was my good fortune to be in a division taught by the physicist, Professor E. B. Rosa. Later I took several courses in physics under Professor Rosa. He was one of the clearest teachers I have ever had. I never had known what mathematics really meant until day after day he taught us analytical geometry. Later, in laboratory physics, too, he used to make everything so plain that science became more than a body of facts; it became a system of thinking.

In the junior year we were required among other subjects to study physiology. Here we were under Professor H. Conn, who came to Wesleyan after being trained at Johns Hopkins in those early days when graduate work was new in American universities. Afterwards I elected laboratory courses under Professor Conn and, because I was specializing in psychology, he let me work on nervous systems.

I was especially fortunate at that time to secure an introduction to Dr. F. K. Hallock of Cromwell. Dr. Hallock was associated at that time with his father in conducting a private sanitarium for neurasthenic patients. Dr. Hallock had studied in Vienna and, among other acquisitions which he had brought home with him, was a microtome large enough to make sections of a cat's whole brain. He had at the time I first met him imbedded in the well of this microtome the stem of a horse's brain and he proposed to me that I make sections and bring them up to his sanitarium, and we would look them over together. I thus came into the extraordinary fortune of a course in brain anatomy the like of which few undergraduates have ever taken. Professor Conn gave Dr. Hallock's microtome space in the biological laboratory, and I cut sections and once a week made a pilgrimage to Cromwell where I received the best kind of individual training in neurology.

The core and center of my training at Wesleyan I received from Professor A. C. Armstrong. Professor Armstrong had recently come from Princeton where he had been a pupil of President McCosh. He taught required logic to sophomores, required psychology to juniors, history of philosophy to juniors and seniors, and some electives, one of which was a seminar in James's psychology.

I do not now locate with any exactness the date when Professor Armstrong's teaching led me to decide that I would devote myself to the study of psychology. He has turned others also to the same study: Dearborn, Freeman, Thorndike, and others who, like myself,

studied required psychology and later continued the study because of the interest which Professor Armstrong aroused in the subject.

I found myself in my junior year specializing in psychology. Professor Armstrong put into my hands Ladd's *Outlines of Physiological Psychology* and Sanford's *Experimental Psychology*. He encouraged me to make the study of nervous systems to which reference was made in an earlier paragraph, and he also encouraged me to take advanced laboratory courses in physics.

In my senior year I read James's two volumes in a seminar course with Professor Armstrong. In this class E. L. Thorndike also received his introduction to James. I took, in addition to the courses mentioned, Professor Armstrong's senior electives in the history of philosophy.

My associations with my first teacher of psychology have extended over so many years and have been so intimate that it is difficult for me to keep them in clear perspective. I remember that he gave me a great deal of individual attention during my undergraduate days. Wesleyan had no psychological laboratory at that time. Professor Armstrong, while protesting that he was no experimentalist, took me to the physics laboratory and tried out, with the help of apparatus that we borrowed there, some of Sanford's experiments. This statement will make clear what I mean when I say that Professor Armstrong was very generous in his care of a young undergraduate.

Later he brought me back to Wesleyan to teach in his department and he gave me the same kind of generous help while I was struggling with the problems that confront a young instructor.

Professor Armstrong taught me, too, the meaning of productivity. He had collected some interesting material on visualization, following the example of Galton's study. When I was a senior, he let me assist him in working up this material and he attached my name with his to an article which was published in the *Psychological Review*. This was my first attempt at scientific writing.

Professor Armstrong took me to the meeting of the American Psychological Association which was held at Columbia University in 1893. I saw the psychological laboratory, and more than that, I saw the galaxy of psychologists in attendance on the meeting. James was there and Dewey. Münsterberg was a newcomer and was heard on several occasions. Cattell and Farrand showed us their experiments. If I had been in any doubt as to my future calling,

that meeting would, I think, have fixed my determination to become a psychologist.

Of the contributions to my mental life which Professor Armstrong made, there are two which I think are of such importance that they should be especially mentioned. In all of his teaching, in logic, psychology, and philosophy, he insisted that thinking should be systematic and coherent. He had a device which he frequently used in criticizing James. When James, the neurologist, wrote paragraphs which were highly flavored with materialism, Professor Armstrong would point out that James No. 3 was responsible for that passage. James No. 1 was the writer who believed that attention and the will actually exist as mental forces and as dominant factors in human nature. I do not remember how many Jameses there were, but I think there were four or five. The main point made was that a student who would be clear in his thinking must not be misled by the captivating enthusiasm of James's writing into accepting uncritically positions which are inconsistent.

The second important contribution of Professor Armstrong to my training was the truth that no thinking is complete which does not take into account in a broad way the contributions of all the leaders in the field. Professor Armstrong used Sully as the textbook in his required course in psychology, but he told us about Spencer and induced as many of us as possible to read Spencer. He told us, also, about Galton and Lloyd Morgan, about Bain and Wundt. He introduced us to James and Ladd. I remember very well that he repeatedly exhorted me to read extensively.

I took courses with Professor Armstrong during my last three years in college. To the clear and carefully formulated lectures which he gave us and to his methods of conducting discussions and recitations I owe more than to the example and instruction of any other teacher.

There are two episodes in my student life at Wesleyan outside of my regular courses that I think are worth mentioning. My sisters had moved to Middletown when I entered Wesleyan, and we lived near the campus. During the long summer vacations, when the college was closed, we had that part of the city almost to ourselves. One of these summers was especially quiet for me because my sisters went on a visit. I was allowed to have a key to the library by Mr. James, the college librarian, and, under an impulse which I suppose

came from Professor Winchester's course in English literature, I read all day and long into the night Thackeray, Dickens, and Scott.

During the last summer vacation of my college career, I went to Worcester where one of my college acquaintances, John Bergstrom, was a student at Clark University. Bergstrom had offered to give me some help with German in preparation for my entrance the next year on a German university. He did more than that. He made me somewhat acquainted with the laboratory at Clark and he let me serve as subject in some of his experimental work.

On the social side I gained much at Wesleyan. My experience with the world was very limited, astonishingly limited when contrasted with the experience of the modern college youth. During the autumn of my senior year I served as manager of the football team. At that time Wesleyan was in what was known as the "big league" with Yale, Princeton, and Pennsylvania. I was injected into a group of associations which were wholly unfamiliar to me. I have no objective knowledge of the figure that I cut, but I am very well aware that I encountered situations that, to put it very mildly, severely taxed my social ingenuity.

In June, 1894, I graduated from Wesleyan and started for Leipzig to study in Wundt's laboratory. I have been asked many times by my own students, "What is the best course to follow in regard to study for the Doctor's degree? Should one go straight forward to the degree or should one teach for a few years and cultivate maturity and all that maturity implies?" My answer is not given without some reservations, but I always say that I am glad I took the risk that I did. To be sure, I was fortunate in the fact that I had a friend in Dr. Colville who lent me the money with which to make the expedition. If I had known in advance how many years of economy would be necessary to pay for my education, I am not sure that I would have had the boldness to start. Fortunately, I was so ignorant of financial matters that I went. My share of our patrimony had been more than used up during my college days and, when I started for Germany, it was an investment in advance of the only resource that I had—my profession.

On the steamer I fell in with a gracious gentleman who was taking a trip up the Rhine. He adopted me as a companion, and we had a short holiday together. I arrived in Leipzig early in July. I plunged at once into my first task which was to learn German. I went to Berlitz classes; I attended church regularly; I lived in a

German pension; I committed long pages of German to memory; I read a German grammar through every two weeks. In short, I lived with the German language. I remember the suspense with which I attended my first lecture in October. The summer's devotion to German had been successful; I could understand the lecture.

This is perhaps the point at which to introduce the statement that I never became adjusted to German life or to the Germans. My profound obligations to Wundt cannot be overstated, nor can my gain from study in German institutions, but the formality of the intercourse between German students was something against which my democratic training in America made me recoil most violently, and the snobbish attitude of most of the people whom I met seemed to me intolerable. I never became anything but a rank outsider and a very lonesome one at that.

There were some advantages in the strict limitations of my resources which I always had to recognize. I could do nothing but work. When the other Americans went on vacations, I stayed in Leipzig and read. The result was that in February, 1896, my thesis was accepted, and I was admitted to examination.

The Leipzig laboratory, or *Institut,* as it was called, was located in my day in the *Altes Trierisches Institut,* a dingy old building with little rooms arranged along a long corridor. The door was unlocked at 1:00 P.M. In the middle of the afternoon Wundt would arrive with the punctuality so characteristic of all his movements and would go down the long corridor to his office There would be a stir among the readers in the library and among the workers in the experimental rooms. At four o'clock, the *Institut* would empty into the neighboring lecture-room, and everybody would go to hear Wundt. Now and then Wundt would look into the experimental rooms, but, for the most part, he left the supervision of laboratory work to Meumann, who was *Privat Docent,* and to Kiesow, who was assistant in the laboratory.

Wundt's lectures were of special interest in those days because he was changing from the form recorded in the *Menschen und Thierseele* to the form which was later published in the *Grundriss der Psychologie.* Also there were rumors abroad of the *Völkerpsychologie,* though it was not good form to speak publicly about anything that Wundt was doing until the work was completed.

Meumann was dynamic in his lectures and was busy in his work on time perception. He conducted an *Einführungscursus* which met

once a week and was designed to introduce all newcomers to the laboratory equipment and to the methods of experimental work.

Kiesow was engaged in his investigations of skin sensations. He was a great help to American students in general and to me in particular because he spoke English and also because he was willing to give advice on many matters.

There were other American students in the *Institut* beginning their work in the autumn of 1894. G. M. Stratton and Guy Tawney were the Americans with whom I was most intimately associated. It was a rule of the *Institut* that no student could begin an *Arbeit* until he had been a member of the *Institut* for a semester, We Americans, especially Tawney and I, circumvented this rule by beginning work on our investigations in our rooms. In the meantime, we could read in the library of the *Institut* and we could serve as *Versuchpersonen* in the various investigations that were going on in the *Institut*. Also, of course, we attended lectures.

The *Institut* was full of traditions of Americans and adopted Americans who had been there in earlier years: Hall, Cattell, Münsterberg, and, lately, Titchener. Münsterberg was recognized as quite heterodox; the others were regarded as more or less in good standing as Wundtians.

There was in the *Institut* very little respect for the leaders in American psychology who had received their training elsewhere than in Leipzig. Especially was there a very pronounced antipathy to James. James had done what was thought to be quite out of order; not only had he criticized Wundt but in some cases—as, for example, in discussing the *Innervationstheorie*—he had allowed his criticism to take the form of witty sarcasm. This was far too much. Not only so, but he had indulged in that remark about patient laboratory work in a land where they did not know what it means to be bored. As a result, diplomatic relations were promptly and totally suspended.

I had occasion to learn in a very pointed way the acuteness of the estrangement. I once asked why James was not translated into German along with most of the other books in the world. I found out in no uncertain terms that James was not regarded as a thinker of the first order.

In later years, after I had become in some measure an enthusiast for the teachings of Wundt, I prepared an article for the *Philosophical Review* on one aspect of the work of Wundt which is not com-

monly discussed in America, namely, his philosophical system. I had the temerity to think that my article might interest James, who, I found, thought of the Wundt school in somewhat the same way that the Leipzig *Institut* thought of the James school. I sent my article, which contained, I must admit, a great deal of interpretation as well as exposition, to James and besought him to find in it much in common with his own thinking. I received from James one of those postcard acknowledgments with which he used to favor his younger colleagues. The card contained the following seven words: "Would to God it were true. James."

The experimentation in the Leipzig laboratory during the two years that I was there was very largely devoted to problems of perception: space perception and time perception. The period of reaction-time experiments was past. Wundt's article on sensory and motor reaction-times had so far invalidated the older averages that there was an evident disposition to let the matter rest where Wundt's article had left it. There was some interest manifested in experiments on Weber's Law or, rather, in the general principle of relativity. In the main, perception was to the fore, and it was in this field that I prepared my thesis.

I was greatly impressed by the care with which Wundt read my thesis when it was handed to him. He did much of the editorial work for the *Psychologische Studien* and when he examined a thesis he also prepared it for publication. In due time I was summoned for a conference. Several changes were suggested in the text and finally we came to a point on which I had ventured an assertion without adequate evidence. Wundt asked for the evidence, and I offered as a defense of my position the remark that I thought what I had said was *a priori* probable. I received an answer that is so typical of what I was taught by Wundt throughout my contact with him that I quote it. *A priori ist garnichts wahrscheinlich.*

The spirit of empiricism which is exhibited in this remark was characteristic of all of Wundt's teaching. There are those who have accused Wundt of dogmatism. It has always seemed to me that those who make this charge overlook the enormous range of facts which Wundt had at his command. When he expressed a judgment there was back of it a wealth of material which has never been paralleled in any mind with which I have had contact.

Wundt was a prodigious worker. He followed a regular daily program and devoted all of his life to collecting materials for his

ponderous tomes, to writing, to exmining theses and dealing with candidates for the degree.

I had a somewhat unusual opportunity to come into personal contact with him after I received my degree. His *Grundriss der Psychologie* had just come from the press. I made application to the publisher for permission to translate it into English. Wundt was known to be adverse to translations. Long years before an early edition of the *Grundzüge* had been translated into French with the unfortunate result that French references to Wundt's work were for the most part based on this early work. The publisher of the *Grundriss* secured Wundt's consent to the translation of his new book on condition that it was to be printed in Germany in small editions so that it could be revised whenever Wundt so directed. The publisher gave me permission to prepare the translation. Wundt insisted that he should see the proof. So it came about that on Thursday afternoons during the spring of 1896 I had conferences with Wundt lasting from fifteen minutes to half an hour. He made many suggestions and listened to such explanations as I had to offer of my translations of particular words and phrases. I brought him some American reviews of the *Grundriss* and listened with much interest to his comments.

At that time G. Stanley Hall was conducting in the *American Journal of Psychology* a section of short reviews. When Wundt read Hall's review of the *Grundriss,* he pointed out where, in his judgment, Hall had missed the chief contributions made by the book.

My two years in Leipzig were profitable not only because of my contacts within the *Institut* but also because of the opportunity which I had to take work in departments other than psychology. It was the custom of members of the *Institut* to take as one of the two minors required for the examination lectures in comparative anatomy with Leuckart. Leuckart was one of the most fluent and picturesque lecturers whom I have ever heard. He had a collection of specimens which were so striking that his lectures would have been illuminating if they had not been what they were in content and finished form. I had an experience in the first course which I took with him that made me aware of some of the shortcomings of the lecture method. I found at the end of the semester that my notes were nearly worthless. Everything seemed so obvious and easy to remember when Leuckart was talking that I had failed to take

usable notes. This made it necessary for me to repeat the course, which I did with great profit.

My other minor was the history of pedagogy with Volkelt. I was not at that time especially interested in education. I was availing myself of the generally recognized privilege of taking one easy minor. I am fully convinced that the only reason that Volkelt passed me on the final examination was that he saw from the record that I had passed the two preceding examinations.

I took a course with Fleschig. He was, I am quite sure, one of the worst lecturers who ever gave a course to university students. He paid no attention whatsoever to his class and at times it was quite impossible to hear what he was saying to himself. He had, however, casts and slides which members of the class examined with profit after the lecturer had retired.

I found in Leipzig an uninterrupted opportunity to read. I have sometimes thought that students ought to be shut up in convents and kept from ready communication with their friends and relatives. It is not strictly true that I did nothing but read and work in the laboratory, but it was nearly so. The isolation of a student in a foreign land, especially a land which seems to him to be very foreign, will be understood only by one who has had an experience similar to that which I had in Leipzig. While isolation is emotionally depressing, it is intellectually stimulating in the sense that it keeps one hard at work so that one's stay may be short. I covered much ground in my reading and profited greatly by doing so. I read extensively in Wundt's writings. I became acquainted with Stumpf and Ebbinghaus. I read, also, some of the older works, especially those of Johannes Müller, of Ernst Heinrich Weber, and of Gustav Fechner.

My Leipzig training gave me a number of points of view which have been influential in all my later work. In the first place, I adopted without reservations Wundtian voluntarism. I have often thought that if Wundt had repeated in the later editions of his great work on physiological psychology the clear statement which he made earlier of his doctrine of innervation, he might have saved the world from the discussions of so-called "behaviorism" under which our science has been compelled to suffer in recent years. Wundt recognizes fully in all his writings and teachings the essentially active character of all mental life. He gives to the motor processes in the nervous organization of the individual a prominence which is

beyond anything that the new behaviorists have ever been able to comprehend. The reason why some of the recent behaviorists flourish is that they have reduced psychology to a few simple formulas which can be carried in the mind without serious mental effort. They have simplified the mental universe as one might simplify the celestial world by mistaking the stars of the first and second magnitudes for the real cosmos.

Wundt never over-simplified. He recognized the complexity of mental life and he recognized the fact that mental life is a process and not a collection of items. His teaching was functional and synthetic, never atomistic and structural.

I accepted, also, and accept today, the doctrine of creative synthesis. Wundt never attempted to explain the higher mental processes by piling up great collections of lower mental processes. He believed in organic fusions. He believed that when mental processes become complex there appear new forms of experience which no lower stages of mental life exhibit or remotely resemble.

In the third place, I learned from Wundt what he calls the historical method of psychology. His contribution of a new method and a new body of material in his *Völkerpsychologie* does not seem to have exerted in America an influence at all comparable to the influence which was exerted by his earlier work in physiological and experimental psychology.

The explanation of American neglect of his monumental work seems to me to be that the physiological psychology preceded James and preceded the development of an independent American psychology. Wundtian social psychology came at a stage of the maturity of American psychology in which American workers were far less open to foreign impressions than they had been in the eighties and early nineties.

For my own part, I became absorbed in social psychology. It is my firm conviction that the great strides which the future will see in psychology will be not in the field of individual psychology but in the field of social psychology. The importance of language has appeared in every serious effort that has ever been made to study human nature. Wundt's two volumes on *Die Sprache* will, I believe, come to be thought of as his most important single contribution to psychology.

I have often commented on the fact that James seems to have had very little interest in social psychology and very little interest

in the psychology of language. James evidently suffered in this respect from one of the blindnesses of human nature. American psychology has followed James rather than Wundt. In my judgment, this has been a grave misfortune and one that the future will have to remedy.

My personal interest in social psychology was intensified by a course of lectures which I attended under the historian Lamprecht. During the spring semester, after I had taken my examination and while I was translating the *Grundriss,* I took the opportunity to attend lectures in history. I learned, under Professor Marks, about the Franco-Prussian War and gained an impression of the attitude of Germans toward Bismarck. Lamprecht was lecturing on *Culturgeschichte des Deutschlands* and, as readers of his writings will readily understand, it was for me a course in social psychology.

No American who has taken a Doctor's examination in Germany is likely to forget the experience. The dress-suit worn in mid-afternoon, the high hat, and white gloves add to the excitement of the occasion. Wundt examined me first, and I remember with gratitude the first question which he asked me. He asked me in what part of the United States I lived. I have often thought that probably that was the only question I could have answered with assurance at that moment. I learned in the course of the ordeal that an examination is an opportunity to tell what one knows, not an effort to discover one's failings. I have tried in my later years as an examiner to emulate the example of those who conducted my examinations for the Doctor's degree.

In describing my Leipzig training, I must not fail to comment on the highly instructive experience which I had in translating the *Grundriss.* I secured all the recent good translations in psychology and philosophy that I could find and I used these as my guides. I made a complete translation of the *Grundriss* and when this first translation was completed, I destroyed it and did the work again. The work was profitable from many points of view. I gained much by studying Wundt's compact style. I learned an English psychological vocabulary which my sojourn in the midst of German experimentation had not given me. I became conscious of problems of expression that I had never thought of before. I may confess that some years later, when I revised the translation for a new edition, I found that I had been guilty of some glaring Germanisms in my first rendering.

During the summer of 1896 I came back to America and, with the opening of college in September, I began work in Professor Armstrong's department at Wesleyan as Instructor in Philosophy. My salary for the first year was $900, and I was glad to receive that much.

I taught the required sophomore course in logic to two divisions, an elective course in experimental psychology to advanced students who had completed Professor Armstrong's course in required psychology, and a course in the philosophical systems of Locke, Berkeley, and Hume.

Much of the work in the two advanced courses was conducted as lectures. I took the advice of some of my elders on the faculty and wrote out all my lectures. I left the carefully prepared lectures in my room, however, and in this way gained experience in the informal delivery of what I had to say. While this somewhat laborious preparation for my courses kept me very fully occupied, I am sure that I profited greatly by this type of literary drill.

I found among my colleagues on the faculty a number of men who, like myself, were receiving their first initiation into college teaching. We all lived in the dormitories and a number of us boarded together. The frank interchange of experiences between the members of this group contributed to my training quite as much as had the example of my teachers.

I remained at Wesleyan during two academic years. Encouraged by Professor Armstrong and by the spirit of scholarly productivity which pervaded the institution, I prepared a number of papers for the psychological journals and secured places on the programs of the American Psychological Association for the presentation of my materials. My chief interest was in problems of visual space perception.

In 1898 I accepted an appointment in the School of Pedagogy of New York University. This appointment gave me the title of professor and a salary which, though it was not large, made it possible to marry. I was married in my old home, Binghamton, New York, to Ella LeCompte.

I found on arriving at the School of Pedagogy that there was an aspect of the science of the human mind about which I knew nothing. I was probably as ill prepared to teach teachers as any young specialist in the theory of space perception and the history of psychology could be. I recall very well that I had on one occasion been lec-

turing enthusiastically on Weber's Law to a class of New York City teachers who were seeking increases in their salaries by listening to me, when I was interrupted by one of my gray-haired auditors with this question: "Professor, will you tell us how we can use this principle to improve our teaching of children?" I remember that question better than I do my answer. I made up my mind that I would have to begin to learn something about schools. I used my mornings for a number of months visiting schools.

I also tried, during my stay in New York, to enlarge the range of my experimentation. My first effort was to get some device by which I could make a record of pupils' language reactions. I used all the different kinds of phonograph diaphragms I could find and took records of voice vibrations on smoked paper. My experimentation convinced me that I was not securing records of the voice so much as records of the diaphragms, and I turned to some of the simpler motor processes. I secured some illuminating records of the motor processes involved in handwriting and some very interesting records of emotional reactions.

I conceived the idea, while carrying on this work, that a book should be written on the significance of motor processes, and I prepared a manuscript. In later years I condensed this manuscript into my paper on "Movement and Consciousness" in the *Yale Psychological Studies*. I submitted the book to two publishers and they declined it on the ground that it was too technical and too remote from public interest to justify its publication.

The experience of having a manuscript rejected is one which I am convinced every young writer should pass through. I learned that a manuscript must be prepared for readers, not for the satisfaction of the author. It is hardly fair to say that I learned this lesson fully at that time, but I certainly made some progress in the direction of a comprehension of that idea.

The last year of my stay in New York University was spent in intense and unremitting attention to university politics. It was the conviction of several of my colleagues and myself that something ought to be done in order to raise the standards in our division of the University. We expressed our views in season and out of season; we voted our views in the faculty; we appealed to the administration; and, by every means in our power, agitated reform. Our activities very naturally disturbed the administration, and on one morning early in May I found in my mail a polite request that I resign. I

wrote a fervid document stating why I was resigning and found myself without a position and without any immediate prospects of securing one.

At this juncture one or two of my friends who were in a position to do so offered me openings in the commercial world. I made up my mind, however, that I was much less well fitted for commercial enterprises than I was for teaching and so I refused the offers and began a canvass of the academic institutions of the country. The University of Cincinnati took me in, first for the summer session and afterwards for the regular work of the academic year. I was certainly grateful for the shelter from economic desolation which the University of Cincinnati gave me.

I had also learned a valuable lesson about institutions. I found that New York University went on its way very much more easily than did I and my colleagues who had been bent on sudden reform. I have adopted since this first experience the methods of gradual reform whenever I have felt it necessary to change the practices of institutions with which I have been connected.

Before I went to Cincinnati I spent two months of arduous labor repairing a deficiency in my education which I had recognized as existing ever since I attempted to pass a minor examination in the history of education with Volkelt in Leipzig. I was to take a professorship in Cincinnati which had the pretentious title, Professor of Psychology and Pedagogy. Among the courses which I was expected to give was one in the history of education. So I read history of education all day for two months and learned much that was entirely new to me.

When I reached Cincinnati I found that the University had recently passed through a period of reorganization. The new faculty was very little welcome in Cincinnati society, and the internal organization of the institution was far from peaceful or settled. The president, who had been a biologist, believed in strict natural selection and had inaugurated a system of election of studies by students which was the most extreme in its freedom that has ever been tried anywhere in the world. The result was a degree of specialization on the part of some students and a scattering of energy on the part of others which I have never seen paralleled anywhere. At the beginning of each quarter there was a complete reshuffling of electives, guided by no one. In the midst of this chaos the faculty counted for nothing in the formation of policies. There were no faculty meet-

ings except those which gathered now and then informally, privately, and always at some distance from the administrative offices.

I made a number of friends among the school people of the city and vicinity, during my brief stay of a year in Cincinnati, who have continued, to my great satisfaction, to be my friends ever since. I did not do much scientific work in Cincinnati because I was only a few paces ahead of my class in most of the work which I conducted. I was fully occupied attending educational meetings and teaching. I did some reading, but, for the most part, the year was scientifically a failure. I look upon it as a period of devotion to economic rescue work.

At the Christmas meeting of the American Psychological Association I met Professors Duncan and Sneath, and they invited me to come to Yale. I did not let them leave the room in which the invitation was issued before telling them that I would accept. It meant a considerable reduction in salary and a reduction in rank to leave Cincinnati for the position offered at Yale, but the compensation which I saw was in opportunity for scientific work.

At the end of the year at Cincinnati I moved to New Haven. My main stated duty was to teach three divisions of the introductory course in psychology. The course had been one of the most generally elected snaps in the curriculum. The students, quite unaware of any change in the administration of the course, had elected it with full confidence in tradition. Most of those who were interested in athletics and other absorbing student activities were registered in my course during the first year. The registration in the course was less by somewhat more than fifty per cent the second year.

Not only were my classes at first disappointing, but I found that the department to which I belonged was divided by serious internal discord, and, to make matters worse, the university administration was totally out of sympathy with much that was going on in the department. All this I learned when it was much too late to withdraw the enthusiastic acceptance which I had given to the invitation to become an instructor in psychology at Yale.

It took some years to correct the difficulties within and without the department. In the meantime, I found an opportunity for scientific work, the like of which I had never seen. The Yale laboratory was well equipped and above all was supplied with a workshop and a mechanic that made possible the construction of apparatus which in turn opened up limitless possibilities of experimentation.

A number of well-trained graduate students came to work in the laboratory, and experimentation went on at a gratifying pace. There were three lines of experimental work which received most of the attention of the group. First, and foremost, it was possible to greatly extend the studies in perception in which I had always been interested. A camera for kinetoscopic photographing of eye-movements was constructed and used for various lines of investigation. Secondly, a number of pieces of work were undertaken which dealt with motor processes. Thirdly, experiments in learning were launched. Several of these I carried on myself and found that they led me into the discussion of transfer of training which was at that time a very live topic.

In addition to experimentation in the laboratory, I found opportunity to give an advanced elective course in social psychology. From time to time I also gave courses in psychology for teachers.

I prepared and published a number of books during the seven years that I was at Yale. The first was entitled *Genetic Psychology for Teachers*. In this I attempted to show how learning to read is a process of social inheritance. I did the same for number and handwriting. I was able to combine with this discussion of the social aspects of education some results of experimentation. I became very keenly aware of the paucity of experimentally determined principles in education.

My second effort at book-making was in the field of general psychology. I prepared a general textbook for college classes and some laboratory manuals. These had a moderately wide use but never succeeded as have a number of the well-known American textbooks in general psychology.

The reports of laboratory work which were published in the *Monograph Supplements* of the *Psychological Review* were much more successful than the textbooks. In fact, I am of the opinion at this remote date that some of my early efforts to formulate systematic treatises in psychology were premature. In educational psychology, especially, there was not enough material at hand to justify a textbook.

In this connection I recall a remark made by James once when a group of us were watching the sun set behind the mountains in the Adirondacks. James had come for a short visit to the camp and was, as usual, the center of the group. He had recently published his *Talks to Teachers on Psychology*. In the course of the evening

I asked him what he thought of educational psychology. "Educational psychology," he answered, "I think there are about six weeks of it."

I am convinced that my own work would have been more profitable and probably more useful if I had devoted all of my time to experimentation. I often advise my younger colleagues to postpone the writing of textbooks and to produce as much first-hand experimental material as possible before they yield to the temptation to write general summaries.

Some day in the future I plan to gather up the work which has been done in various educational laboratories in a treatise on educational psychology. I shall interest myself in my old age in comparing this more mature work with the books I published in 1903 and 1907.

I was drawn into other lines of endeavor at Yale. There was some demand in the state for courses for teachers. Yale organized a summer school. The second year of this enterprise I was put in charge. The attendance was small and the experiment was discontinued. I had, however, become acquainted with the State Superintendent of Schools, Charles D. Hine, and through him I was brought into intimate relations with the school system of the state. I inspected high schools for the State Board of Education and participated in the state's program for the supervision of schools and the training of teachers. President Hadley favored my participation in this kind of work because it showed the state that the officers of Yale were ready to render public service whenever possible.

In 1907, I gained my promotion to a full professorship and was made Director of the Psychological Laboratory. I found that I had unlimited freedom for scientific work and that I was in command of material equipment which was superior to anything I had known anywhere else. The only limitation from which the position suffered was that the number of graduate students was small. Yale was essentially a college. Graduate work at that date was far outdistanced by undergraduate. When the invitation to move to Chicago came, there were two considerations which led me to accept, namely, the prospects of more graduate students and a greater enthusiasm, as I thought, for the extension of investigation into practical fields.

Before I enter upon a discussion of my experiences in my present position, it may be proper to state some of the scientific views which my Yale experience had tended to fix in my mind. With regard to

a number of these scientific views, I am sure that it must be said I brought them to Yale from Wesleyan or Leipzig, but they certainly gained in maturity during the seven years at New Haven.

I was convinced by the results of my experimental work in the Yale laboratory that the higher mental processes are not of the same pattern as lower mental processes. I had observed the growing tendency to seek the explanation of human mental life through experiments with animals, and I had become convinced that this effort to explain the complex phenomena of mental life as mere summations of the elements that appear in lower forms of behavior is fundamentally wrong.

In my presidential address before the American Psychological Association in 1909 I discussed at length the view which I still hold that evolution has produced in human life a group of unique complex facts which cannot be adequately explained by resolving them into their elements. Human mental life is a unique product of organization. Through evolution certain complexes have been produced which are new and potent causes in the world; among these is human consciousness.

I have sometimes regretted that my duties in other lines have kept me from further vigorous participation in some of the recent discussions that have been going on in psychology. I find myself so much in harmony with the conclusions of my colleague, C. Judson Herrick, in those chapters in his volume on *Brains of Rats and Men* in which he attacks the non-introspectionists that I am impatient with the slow assimilation into current psychological literature of his cogent arguments. I am also encouraged by the recent paper by Shepherd Ivory Franz on *The Evolution of an Idea: How the Brain Works*. These writers seem to me to reinforce very powerfully the conclusion that issued from all the earlier experimental work, namely, the conclusion that all the higher neural and mental processes are the products of organization.

The experimental work which I did in the Yale laboratory led me, also, into the field of learning. I became very much interested in an examination of the processes by which training transfers. My experiments made it perfectly clear that wherever conditions are favorable to generalization there is transfer. The nature of generalization is such that no simple formula like that of the presence of identical elements is remotely adequate. Generalization is a type of organized mental reaction; it depends on creative synthesis.

Another general principle which was to my mind fully established by the experiments in the Yale laboratory is discussed in my paper entitled "Movement and Consciousness" published in the *Monograph Supplement of the Psychological Review,* No. 29.

I reviewed in that paper the views of a number of writers, especially those of Dewey and Münsterberg, and attempted to show that, while the content of mental life is derived from impressions, the forms into which these impressions are organized are conditioned by the motor processes.

I am convinced that the theory which was originally formulated and called the innervation theory was an important anticipation of the contribution which James made to psychology in his statement that emotions and certain other aspects of mental life are conditioned by bodily movements rather than by sensory impressions.

I look forward to the time when a true behavioristic theory will be accepted in psychology rather than the pseudo-behaviorism, which is nothing but non-introspectionalism, that has been the boast of some members of the present generation of popular writers on psychology. There is much productive laboratory work to be done on human behavior, especially the higher forms of behavior, such as language. When this experimental work is done there will be very little ground for the shallow dogmatism of the self-styled behaviorists.

In June, 1909, I left Yale to enter upon the duties of the post which I now occupy. I came to Chicago as the administrative officer in charge of the School of Education of the University of Chicago. My personal opportunities for research have been much less than they were at Yale. On the other hand, it has been possible, with a much larger staff and with the cooperation of a large body of graduate students, to carry on investigations on a scale incomparably larger than would have been possible anywhere else.

During the twenty-two years that I have been in Chicago I have been able to carry on four lines of scientific work in the special field of educational psychology. First, I have shared in the analysis of reading to which the Department of Education has given much attention. Secondly, I made an effort in 1915 to formulate and discuss the major psychological problems which arise in high-school education. Thirdly, I have done some experimental work dealing with number consciousness. Fourthly, I have taken the first step in the direction of formulating the social psychology on the basis of which I believe all sound education must ultimately rest.

Before I attempt to outline somewhat more fully the results of these four lines of work, I pause to comment on the state of general psychology as I see it from the point of view of my specialty. It seems to me that psychology suffers from a lack of fundamental unity. Many of those who started out, as I did, with interest in the general science of psychology have become, as I must confess I am myself, engrossed in a special branch or phase of the science. The result is that very few workers are left in psychology who have escaped the dangers of narrow specialization.

There are, for example, the investigators of the neural and mental reactions of animals. It is hardly to be expected that these specialists will be concerned except in a remote way with the psychology of the higher mental processes. There are those who deal almost exclusively in tests and in the statistical manipulation of the results of tests. The findings of this group are so spectacular that it can hardly be expected that its members will follow the road of long, arduous labor in the laboratory. The clinical psychologist has had such éclat during recent years that he is justified in his own eyes in going his way without giving attention to normal psychology. Educational psychology has been in such demand that its devotees have often thought of their branch of the science as the only branch worth reading about.

I believe the disintegration of psychology will bring with it serious consequences. One such consequence which it seems to me is already evident is that there is much hasty and speculative generalization. The animal psychologist holds that all mental life can be explained in terms of his special findings. The maker of tests speculates without any inhibitions on the nature of intelligence. The educational psychologist is quite competent, in his own mind, to discuss all the social institutions of modern times and to guide conduct, private and public.

Another consequence of the decentralized state of psychology appears in the fact that students in other fields, finding no adequate psychological principles to guide their thinking, begin to construct their own psychology. This they do in many cases without utilizing the methods of analysis and investigation that psychology has already devised. The most striking example of this is to be seen in the fact that much of the social psychology is being written by sociologists or by writers who are quite willing to speculate rather than develop a body of verifiable principles.

Psychology is paying the price of its popularity and of its intense human appeal. It will, it seems to me, have to undertake in the near future much fundamental research. It will have to be cured of the idea that a hundred loosely related facts can be welded into a body of scientific truth by averaging discordant tendencies and covering up most of the facts through statistical juggling. It is my belief that all of us who are at work in special fields suffer from the lack of well-founded general doctrines.

The general observations made in the last few paragraphs will explain the reason why I have attempted in my own work to hold to experimental and analytical methods. In the investigations which have been made in reading by our department, we have found it highly profitable to make detailed studies of individual performances and of the reactions of individuals to a great variety of special conditions. We have used reading tests, but we have thought of the results of these tests merely as starting-points. We believe that it is inherent in the nature of a test to reveal a present condition rather than to uncover a fundamental cause. A test may show, for example, that a certain individual is a poor reader, but the test does not tell what is the cause of the deficiency. The underlying cause of the present condition can be discovered only by painstaking analysis.

One of the methods which has proved highly productive in the analysis of reading processes is the method of photographing the eyes of a subject while he is reading. The apparatus which has been used by a number of members of our department was originally designed by W. F. Dearborn when he was on the faculty of the School of Education. The apparatus has been enlarged and reconstructed by a number of users. The films which are secured by means of this apparatus can be translated only with the most arduous labor. It requires from three to five hours to decipher an ordinary record. I mention this fact in order to make clear what I believe to be one of the strongest reasons for the infrequency with which graduate students select thesis subjects requiring the type of experimental technique which we have found to be so highly productive.

Difficult as the technique is, it has produced results of far-reaching importance. Huey called attention in 1908 to the difference between oral and silent reading. It was not, however, until that difference had been established by numerous photographic records that teachers accepted the distinction and began to modify their teaching so as to give special training in silent reading.

Investigations later than those which made clear the difference between oral and silent reading have shown that the reading of mature individuals is of several different patterns according as conditions of reading change. Reading undertaken for the purpose of preparing to answer questions is very different from reading for one's own satisfaction. Reading of an arithmetic problem is different in character from reading of ordinary prose.

I have pointed out in some detail the results which have been secured by the application of laboratory methods to the study of one educational problem in order to show that there is a very promising field for future work open to anyone who is willing to adopt experimental methods in the study of mental processes.

In 1915, I prepared a psychology of high-school subjects. There was very little empirical material at hand on which to base such a work. In 1927, I completely rewrote the book and found it possible to incorporate much new material of an empirical type.

The psychology of high-school subjects is to my mind one of the most promising fields for further work. The opportunities to which this statement refers may perhaps be described more adequately by the general statement that very little is definitely known regarding the character and genesis of any of the higher mental processes. We know much about space perception, but we are unable to give any adequate account of what goes on in the mind of a student who is trying to follow the reasoning of a geometry textbook. The higher mental processes are, of course, the processes which it is most important to call forth in the mind of a pupil. At the present time we are ignorant regarding methods of teaching in higher institutions very largely because we are in the dark about the nature of abstract thought and logical reasoning.

I hope to find time and opportunity in the future to follow the line of inquiry which the preceding remark has indicated as promising. In the meantime, there is one subject of instruction in the elementary school which has long seemed to me to call for attention; that is arithmetic. McLellan and Dewey wrote on the psychology of arithmetic in 1895, but, like many of the early writers in educational psychology, they speculated rather than reported experiments. While I was at Yale, I began some experiments on number which were interrupted by the move to Chicago. In the years immediately preceding 1926, I found opportunity to continue these experiments, and in 1926 I prepared and published a mono-

graph entitled *Psychological Analysis of the Fundamentals of Arithmetic.* I am gratified to be able to add that one of my colleagues, Professor Buswell, and one of the former students of our department, Professor Brownell, have undertaken experimental work in arithmetic and have already gone far enough materially to amplify my results.

The urgency of the problem which confronts the schools in their teaching of arithmetic can be made clear by pointing out the fact that there are more failures in arithmetic in the intermediate grades than in any other subject. Apparently there has been no adequate understanding on the part of teachers of the ends at which they should aim or of the stages through which the pupil passes in his efforts to master arithmetic.

The fact that psychology has been slow in coming to the rescue of the schools in this matter is to be accounted for in part at least by the difficulty of devising any laboratory methods of analyzing number ideas. Most of a pupil's use of number is so subjective that it is difficult to secure records by means of which analysis can be made. In this respect, number consciousness is very much like reasoning and all the higher forms of mental activity. It has been possible, however, to devise some very productive methods of investigation in spite of the subjective character of these processes, and I believe that it will be possible in the future to complete the study in this and the other fields to which reference has been made.

The last constructive piece of work which I have been able to complete is in a field in which I am more interested than in any other. In 1926, I published a book for which I had been collecting the material ever since my Leipzig days. It is entitled *Psychology of Social Institutions.*

The chief purpose of this book is to lay the foundation for a systematic treatise on educational psychology which I hope to prepare. The thesis which I defend throughout the book is that collective intellectual effort has brought into existence certain products, such as tools, number, language, and government, which products could never have been created by the individual. These products of cooperative effort are effective causes of new mental activities, which they initiate and condition. Thus when language is once produced, it makes possible a type of thought activity which could never have appeared before there was a language.

The profound significance for all the social sciences and for edu-

cational systems of a valid explanation of social institutions is, I believe, gradually making itself felt. The inadequacy of individual psychology as a basis for the scientific understanding of society has been pointed out vigorously by both psychologists and representatives of the special social sciences. Certainly, many of the modern conceptions of education are outgrowths of an individualistic philosophy which has no justification in the experience of the race.

In this account of the psychological investigations and writings which I have been able to complete at Chicago, no reference has been made to the other lines of work which I have carried on. I have had certain administrative responsibilities and I have usually taught a full program, which, in the University of Chicago, amounts to eight hours per week. My administrative relations have brought me into contact with educational associations and various national committees not engaged in the discussion of psychological problems.

It is difficult to say how far these non-psychological activities have had positive influence on my psychological thinking. It is easily possible to show that, negatively, they have greatly limited the time and energy which I could devote to laboratory work.

It is my judgment that there is a very intimate relation between my practical undertakings and my scientific interests. I am satisfied that far more genuine research in educational psychology has been completed by my associates, for whom I have been able to arrange working conditions, than could have been accomplished by my individual efforts. So far as my personal work is concerned, I am disposed to believe that it has probably been about as extensive as it would have been under any conditions. Research seems to me to be a periodic affair, not a continuous process. I find that now and then I can concentrate on a problem and secure results. Between research periods there are periods when teaching helps to formulate one's ideas and when contacts which seem somewhat foreign to research lead to the acquisition of wholly new and very productive ideas.

Administratively, I favor for persons of my temperament at least a program of varied activities. It is my observation that most of my colleagues, as well as myself, need periods of release from each kind of duty which they perform in order that they may come back to this duty with new points of view and with new enthusiasm. In arranging work for the staff with which I am associated, I have tried to provide men with time for research when they reach the point

where they can profitably devote all their energy to some problem which is well advanced toward solution. It is my belief that it is wasteful administrative policy to subsidize for research men who do not begin work until they are released from other duties.

Furthermore, I have no sympathy whatsoever with those people who complain that they are not given opportunity for research. I believe that the world has a place for any man who will demonstrate that he can do research work. The demonstration is, however, the first step.

I have sometimes been upbraided by my friends in psychology for not continuing the kind of work which I was doing at Yale. Certainly I believe in fundamental research and I am sure that institutions should make such fundamental research possible by creating attractive positions for men. I am equally clear in my belief that fundamental research in psychology can be carried on best by workers who are concerned, as I am, with the applications of such research to human affairs. I am disposed to think that much depends on an individual's temperament when he is choosing a field of scientific work. For one who is interested in social psychology, it seems to me quite unthinkable that there should be a lack of interest in the educational and social movements of the present day. It may be that there is a phase or branch of fundamental psychological research which is legitimately distinguished from what I have called social psychology. Personally, I think social phenomena are so much more illuminating as manifestations of what active human intelligence tends to do than are any purely individual exhibitions of intelligence that I cannot believe that individual psychology when detached from social psychology is a fundamental science.

The editors of this series of biographical sketches have suggested that some comments be included as to the lines of work which may properly be recommended to students who are beginning their work. I feel sure that such recommendations as I have to make are clear from my account of my own experience. I should say to students: Select teachers who have broad interests; read extensively; become interested in analytical methods; be sure to include laboratory work as an important part of training; and, above all, add to analysis and laboratory methods, the methods of study which Wundt has called historical, the methods of social psychology.

As to the most promising fields for research, I believe they are to be found in the intensive study of the higher mental processes. I

believe that the distinguishing fact in human life is a form of intelligence which is indeed the product of evolution from below but is so much more complex than any forms of habit or animal intelligence that categories totally different from those of biology must be developed in science for the classification and understanding of human behaviors. If psychology is to assume its proper function as the fundamental social science, it must, I believe, discover and describe clearly the nature and causal effectiveness of the higher forms of mental activity.