PSYCHOLOGY AT THE UNIVERSITY OF MICHIGAN:

VOLUME II

BIOGRAPHICAL SKETCHES OF FACULTY MEMBERS SERVING ON THE STAFF DURING THE YEARS 1897-1945

ALFRED C. RAPHELSON

UNIVERSITY OF MICHIGAN FLINT COLLEGE

1968

PREFACE

During the collection of the material for this history, we became acquainted with a great many biographical details concerning the men who served on the Department of Psychology staff at Michigan. These details gradually pieced together what for us were the very human and personal dimensions of the picture that was "Psychology at Michigan."

There was some difficulty, however, in deciding the best way to present these portraits. Although we desired to make available what had been learned about these personalities, we did not wish to interrupt the flow of the historic narrative to such an extent that the reader would lose sight of the course of development that psychology had taken at Michigan.

It was decided, therefore, to present the history in two parts. The first section, which composes Volume One, includes a description of departmental activities as they developed through the years. Only those biographical details that were considered necessary to convey the setting for the psychological activities were included. The biographical sketches are presented in the second section, which makes up this volume. These portraits are an ancillary companion to the narrative but one, it is hoped, that can be read and enjoyed quite independently of the first part.

With three exceptions, the sections that follow have been pieced together from details obtained from the interviews and records which have made up the data for this study. The three exceptions are reprints of previously published accounts of Professor Pillsbury's life. It seemed appropriate to include them in this collection.

Alfred C. Raphelson

Flint, Michigan
July, 1968

TABLE OF CONTENTS

CHAPTER ONE

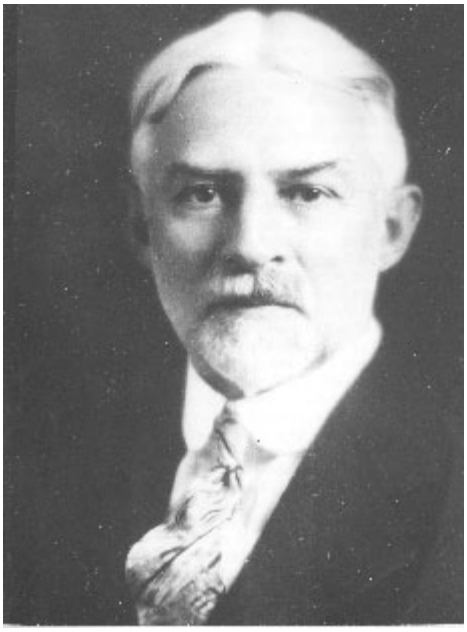
Walter Bowers Pillsbury[1]

(1872-1960)

I

When Walter Pillsbury died in 1960, older psychologists were surprised to learn that younger men on their staffs had never heard of him. For in the early days of American psychology, he was numbered among the great. He was born in Burlington, Iowa on July 21, 1872 and lived there for thirteen years before his family moved to Fullerton, Nebraska. After completing public school, Pillsbury attended the local Penn College for two years and then transferred to the University of Nebraska. At Nebraska, he came under the influence of psychologist H. K. Wolfe.

Wolfe had been to Leipzig where he was the second American to earn his doctorate under Wundt. He appears to have been an inspiring teacher who not only offered stimulating instruction, but also exerted a great personal effect on his students by general availability for conversation on all subjects. Wolfe's total involvement in teaching (psychology, philosophy, and education) and with his students left him little time for research and publication. Hence he is not well-known in the history of psychology. His major influence remained a personal one.



Walter Pillsbury, circa 1942

After graduation, Pillsbury accepted a position teaching mathematics and English at Grand Island College (Nebraska). A year later, he accepted a scholarship to Cornell University to study psychology. In September 1893, Pillsbury became Edward B. Titchener's second graduate student being preceded the year before by Margaret F. Washburn.

Titchener, Pillsbury found, was quite unlike Wolfe. The "experiment" was the keynote in all his teaching. He devoted most of his time to advanced courses and directed all the research. Titchener held aristocratic opinions on most subjects which contrasted sharply with Wolfe's extremely democratic views. Titchener believed in publication as the sole end of a scholar's endeavor. He once said that the only certain immortality was the immortality of the printed page. Pillsbury was greatly taken by his new professor's attitudes. Perhaps his temperament was one that precluded the democratic, personal level of involvement that characterized Wolfe's relationship with people and made it easy for Pillsbury to accept Titchener's value for the printed

[1] The details of Professor Pillsbury's life have been well document in his own autobiographical sketch, as well as the two obituaries written by Karl Dallenbach and Walter R. Miles. These three essays have been reprinted in this volume and will be found immediately following this article. Here we shall primarily be concerned with additional aspects of Professor Pillsbury's life which emerged in the course of this study of the departmental history.

page. Even twenty-five years after Pillsbury left Cornell, his old professor was "reinforcing" the lesson as the following letter from Titchener to Pillsbury illustrates:

> . . .I see no harm in keeping the *Attention* on the market. It is either that, you see, with a note saying that you have not been able to do more than revise – or else it is fancy second hand prices for student who can get good out of the book. My feeling is that an author is not bound to spend on revisions time that he can better use, but that he should see to it that his books (with the note aforesaid) are so far as possible available. Bradley once complained bitterly and venomously to me that Stechart had photographed his out-of-print works. I told him in reply that he was to blame. Professors held seminars and students had to have the books; if he didn't want to revise he should at any rate keep the books on sale with the statement or caution that they are not revised and did not fully represent his current views. He came around. . .[2]

For his doctoral research Pillsbury determined how images from printed pages in association with memory become words during a reading exercise. The theoretical discussion of these results became the basis of his classification of the conditions of attention which he later published in a book under the title *Attention* (1906).

In his second year, Pillsbury worked on the English translation of Oswald Kulpe's *Introduction to Philosophy*, which was later published in collaboration with Titchener. He spent a third year at Cornell as a laboratory assistant doing some teaching and studying subjects related to psychology. In 1897 both the translation and the dissertation were published. These two events in addition to the fact that he was the second doctoral student turned out by Titchener, who represented the Wundtion tradition transplanted in America, contributed greatly to Pillsbury's reputation as a rising young scholar. Several offers of an instructorship came his way including an offer to remain at Cornell. He chose Michigan and it proved to be a life-time assignment.

II

Professor Pillsbury was a rather reserved individual who held himself aloof from both colleagues and students. To those who knew him he appeared friendly and cordial. The difficulty was that it was extremely hard to break through his reserve.

It is clear that these personal characteristics were not brought about by later illness or by old age. One of his earliest assistants was J. E. Wallace Wallin who came to Michigan in 1902 desiring contact with one of Titchener's best known students. He found the thirty year old Pillsbury to be reticent, uncommunicative, fidgety, nervous and somewhat inhibited. His relationship with Wallin was kept strictly professional without any of the elements of good fellowship that were usually among young scholars. There was never any spontaneous personal contact between the two men during the entire year of Wallin's stay.

Throughout his long career at Michigan, Pillsbury remained a remote, unassertive, gentle person who exhibited the height of dignity. His white Vandyke beard epitomized the image of the

---

[2] Letter from Edward B. Titchener to Walter B. Pillsbury, May 21, 1921. In *The Pillsbury Papers*. *Attention* was Pillsbury's first published book (1906).

ideal professor. His speech, slow and characteristically Midwestern, was livened somewhat by a twinkle in his clear eyes. He not only looked the part but went to places a professor should go and knew the people a professor should know.

Yet his reticence and remoteness coupled with his position as a leading scientist and head of a major department of psychology created a great deal of ambiguity about his person. Pillsbury had identified with the autocratic role of the "head professor" characteristic of the European academic models in which he had been trained. He expected his decisions to be accepted as law. And for the most part they were. But his remoteness prevented him from establishing the kind of relationship with his staff that would allow enough communication to enable his decisions to appear to be reasonable rather than arbitrary. His smiling aloofness covered an armor which prevented any kind of argument.

This is not to say that Pillsbury was without his devoted disciples. The many testimonies received by him upon his retirement bear ample witness to the affection, loyalty and reverence many of his former students had for him. Of all the letters in that collection the following one by William Gilbert best summarized these feelings:

> . . . Among all the persons I have known, you are the one of whom I can most sincerely say, "He's a true gentleman and a real scholar." I like your poise, your mildness and your intrinsic honesty. I like the air of courtesy, refinement and distinction which you unknowingly carry with you at all times. I like your white beard and the way your eyes twinkle. I admire you for being astute regarding little matters without seeming to be so. I look up to you for having read far more widely than any other psychologist that I know. It became a habit for me to look for your name on the cards of every library book or journal I took out because I so frequently found your name on them. I became vaguely disturbed and disappointed after being around Michigan for awhile if on my way to the third floor, I did not see you in your office working away at your typewriter or leaning back in your chair reading. I was somehow proud myself because you knew your languages so well, especially your German. Perhaps I envied you in a queer unjealous sort of way. Your breadth of view and your psychological liberalism ceased to surprise me after a time. It surprised me at first, I admit, because I pictured you being pretty Titchenerian.
>
> Perhaps I should think of you as one of the leaders in the development of psychology. You have been that, I know. The picture which means more to me is that of a true gentleman and a real scholar, pecking away at his typewriter or leaning back in his chair reading an article in the original German. . .[3]

Indeed, the image many generations of Michigan students carried away from Ann Arbor was that of a distinguished-looking man with a white Vandyke, sitting hour after hour composing at his typewriter in a large-windowed office overlooking the driveway of the Natural Science Building. For Pillsbury was a prolific writer. His bibliography contains sixty-nine articles and

[3] Letter from William Gilbert to Walter B. Pillsbury, May 16, 1942, in *The Pillsbury Papers.*

eleven books – two of which went through three editions – and these eleven do not include a complete but unpublished book on *The Psychology of Economics* nor a long but unfinished novel.

And yet his writing was not particularly inspired. When Pillsbury began to prepare a textbook, he would examine the already successful texts in the field, list the topics covered and total the pages assigned to each subject. Taking these facts to be "the geography" to be covered, he would then write to meet the demands of this geography. It often seemed that as soon as he finished the last page, he would send the manuscript to the publisher with a minimum of editing and revision.

A reader who was asked to evaluate the manuscript of one of Pillsbury's texts commented:

> . . . Pillsbury's looseness of writing is extremely annoying. . . No publisher's reader could hope to indicate all the places where touching up is required. I have taken the liberty of marking. . .a dozen or fifteen spots where Pillsbury might see fit to alter if they are called to his attention. But after all that is only a small proportion of the weaker places in the construction of the book which are apparent to a half-attentive reader.
>
> My favorable reaction toward the manuscript, however, is based on the conviction that Pillsbury's lack of precision in writing is from one point of a view a definite source of strength. My instructors in psychology would, I believe, find themselves less severely taxed in teaching Pillsbury than say Woodworth. Pillsbury is successful in assembling various viewpoints and presenting them. . .in such a form to minimize their irreconcilable differences. . .[4]

Pillsbury appeared aware of his imprecision and may have also viewed it as an asset. Once an assistant came to him with a statement from his text, *Essentials of Psychology*, which could be interpreted one way according to structuralism and another if one had a behavioristic bias. The assistant pointed out the double meaning and suggested that perhaps Pillsbury might consider changing it in the next revision to a more definite commitment. Pillsbury commented, "Depending on the psychologist's view, of course, either statement would be acceptable. No, I won't change it. . . That is just the way I want to leave it."[5]

This same imprecision appeared in his social relationships as well, where, coupled with his personal aloofness, it often led to complete misunderstanding. John B. Watson, in his classic 1913 paper which inaugurated behaviorism, chastised the psychologists of that day for too narrowly defining what was acceptable data for psychology. "I shall always remember," he wrote, "the remark one distinguished psychologist made as he looked over the color apparatus designed for testing the responses of animals to monochromatic light in the attic of Johns Hopkins. It was this: and they call this psychology."[6]

---

[4] Copy of letter to R. P. Smith of MacMillan Publishing Company from an unidentified publisher's reader. April 28, 1922, in *The Pillsbury Papers*.
[5] Ford, Adelbert. *Reminiscences*, 1966, unpublished manuscript.
[6] Watson, John B. "Psychology as the Behaviorist Views It," *Psychological Review*, 20, 1913, p. 163.

Pillsbury was that "distinguished psychologist." He was out of the country when the article appeared and was very disturbed to read what Watson had interpreted his remark to mean. He had meant to show his own enthusiasm and appreciation of the work. But even the "Prince of Behaviorism" as he called Watson, had difficult fathoming the true Pillsbury.

Despite his national prominence, Pillsbury was not a great intellectual force within the department. His personality, as has been indicated, was not particularly warm and he remained distant and aloof from the students. He was the head of the department, a distinguished looking man, member of the National Academy of Sciences and the possessor of a long bibliography (even if not all the staff members were impressed with its quality). Although gentle and rather unassertive, he very much desired to function as the "head." The combination of the expression of this desire with his personal characteristics contributed to his isolation.

Although he did participate in the Journal Club meetings, Pillsbury's major contribution to the education of the graduate students was through his course in the history of psychology. It was considered an interesting course as well it should have been since he had participated in so much of the early American psychology.

Pillsbury was not a good lecturer, speaking in a somewhat halting manner. But he knew his subject matter well. He was excellent on the Greek classical Psychology and had a good command of the German literature. He would very often read sections in English to the classes, who would then discover that he had been sight-translating from the original German. It was in this course that his psychological position was presented most clearly. Although behind his back some of the staff continued to refer to him as being a pure introspectionist, he appears to have held to a kind of functionalism that had evolved from structuralism. This "functionalism-come-lately" was not easy to pinpoint in his writing. To most of his listeners Pillsbury appeared eclectic and almost anti-systematic. He described himself as follows:

> . . . I presume I am one of the men who should be regarded as belonging to no actual school. . .as a matter of fact I have long believed that a general theory has no justification except as a convenient setting for facts. From its very nature, a school must be one-sided. Its statement is either a protest against an existing school or a rallying cry for a bit of propaganda. Only within limits is either useful. I have attempted to formulate a general position on several occasions but have never pushed it very hard. . .[7]

Such a statement made today would seem proper. Contemporary psychology has moved beyond the search for a grand and sovereign theory. But we are concerned here with the 1920s, when American psychology was no more than thirty-five years old. A system or position represented the crystallization of one's acquired insight and hopes for psychology. Sovereign theories were the vogue and had great academic and personal effects on budding as well as established psychologists. But Pillsbury did not see it as his function to fill that need.

There was also another reason which prevented Pillsbury from having significant interaction with the students. The Michigan psychological orientation that he had begun was that of an experimental and physiologically-oriented department heavily oriented toward research.

---

[7] Pillsbury, Walter B., "Autobiography" in C. Murchinson (ed), *History of Psychology Through Autobiography, II*, 1932, p. 293.

Pillsbury himself, however, was very little involved in research after 1912. His publications after that date were either texts or non-experimental in nature with an occasional research article published jointly with a student who had actually collected the data.

The truth of the matter was that Pillsbury was not a good apparatus and laboratory worker. He appeared to be well aware of this inadequacy and very early in their association, he allowed Shepard to take over the actual running of the laboratory. Pillsbury never relinquished his title of "Director of the Psychological Laboratory," but the students began to turn more and more toward Shepard for the supervision of their work.

III

In the fall of 1912 and again in 1925, Pillsbury requested leaves of absence because of illness. For years after the first leave rumors persisted to the effect that he had a mild form of epilepsy. But the evidence for these attacks really being epileptic is not great. Karl Dallenbach, in his obituary of Professor Pillsbury mentioned that he was subject to petit mal convulsions throughout his life.[8] Shepard, who was closely associated with the senior man since 1906, replied to an early draft of the Dallenbach article as follows:

> . . . the doctors at the time called the attacks epileptoid, because they weren't quite sure what they were. The electroencephalographic methods. . .had not been developed then. The attacks began, as I remember it, about 1912 and ceased somewhere in the early thirties. I think the fact of some such trouble should be mentioned; but perhaps with an indication of the tentative nature of the diagnoses.[9]

During the twenties and thirties the other members of the department often heard rumors but, with rare exceptions to be described below, never observed anything that definitely confirmed them. Professor Pillsbury's lecture and conversational manner was itself nervous, abrupt and unassured in character. On occasion when Norman Maier was a student in his class, Pillsbury suddenly began speaking nonsense, waved his hand and left the room. It appeared to Maier as a strange but not abnormal event and did not make much of an impression in him. Maier, for one, always accepted the official family explanation that these attacks were due to some type of digestive allergy. What follows, then, are as many details as are available to this writer as to the nature and history of Pillsbury's affliction.

In September 1912, Pillsbury had applied for a second semester leave of absence. At that time the sabbatical system had not been established, and it was necessary for him to provide for a substitute to carry out his academic assignments. Pillsbury arranged for Shepard and Henry Adams to be in charge of the larger courses and Harry Crane, a graduate student, to handle the quiz sections that the two junior instructors would give up.

A week later Pillsbury was offered the headship of the Department of Psychology at the University of Illinois at a salary of $3,250. However, he decided to stay at Michigan when President Hutchins agreed to raise his salary from $2,700 and $3,000.

---

[8] Dallenbach, Karl, M. "Walter Bowers Pillsbury: 1872-1960," *American Journal of Psychology*, 74, 1961, p. 165-173.

[9] John F. Shepard to Karl Dallenbach, May 16, 1961, copy in *The Shepard Papers*.

On Thanksgiving Day, Adams and the philosopher Dewitt Parker had dinner at the Pillsbury home. After the meal the three men left to take a walk. Leaving the Pillsbury house at 714 South State Street, they walked to the Broadway Street hill. About the time they reached the hill, it began snowing heavily and three inches lay on the ground by the time they returned to the house. The following week, Pillsbury had his first known attack.

Pillsbury was lecturing in the amphitheater of the old building. Suddenly he felt himself falling. One of the students in the first row jumped over the rail and caught him. Pillsbury did not lose consciousness and, as he later recalled it, was very grateful to the boy. Adams learned of the attack at noon when Shepard informed him of it. They went to the Pillsbury home and found him fairly well recovered with normal consciousness. Pillsbury described in detail what had occurred and appeared as surprised as everyone else by the event.

The Pillsburys left the following month for Italy. They had planned to depart on the official leave in February but because of the attack, left on an earlier date. The Pillsburys traveled to Vienna where the professor was examined by several physicians on the medical faculty of the University. Pillsbury wrote President Hutchins that "they said they could find nothing wrong. Evidently my health is not a matter of concern."[10]

Unfortunately, not everyone was convinced of this. The following December, after Pillsbury returned to Ann Arbor, R. M. Wenley was still quite concerned by his appearance. It was budget preparation time and Wenley, who was chairman of the Department of Philosophy, had to submit a figure for Pillsbury's salary. Wenley wrote President Hutchins as follows:

> . . . Will you. . .be so kind as to let me know. . .your recollection of the arrangement you made with Pillsbury on the occasion of his Illinois call. . . I think that he is still a sick man and, needless to say, I have not broached the matter with him. He seems to me and others have spoken to me about it, to be distrait and not in condition to recollect all the details objectively.[11]

In subsequent years there were other episodes on campus but they appeared to be of minor significance. The attack that preceded his 1925 sick leave, however, was of major proportions.

It occurred on Saturday afternoon during football season. Burton Thuma had come up to the Natural Science Building to work in his third floor office. As he sat working, he heard a small child crying. Thuma went to investigate and found the nine year old Milbank Pillsbury sitting on the steps between the first and second floor crying for his father. Thuma began searching for Pillsbury. He found the professor lying on the floor in one of the lab rooms. His body was quite rigid and there was a small amount of froth on his lips. Thuma, though very frightened, called the University Health Service, but due to it being a football day, no one appeared to be there to answer the telephone. He managed to locate a janitor who called a cab. Meanwhile Pillsbury regained consciousness. Together, Thuma and the janitor helped him to his feet, placed him and the boy in the cab and sent them home.

---

[10] Walter B. Pillsbury to President Harry B. Hutchins, May 5, 1913 in *The Hutchins Papers*.
[11] R. M. Wenley to President Harry B. Hutchins, December 17, 1913 in *The Hutchins Papers*. Psychology, at this time, was still a sub-department within the Department of Philosophy.

Pillsbury's sick leave was granted for the following semester and again he went to Europe to recuperate. He informed his old professor, Titchener, about his illness, and received the following typical Titchenerian paternalistic advice:

> . . . I am very heartily sorry to know that you have been ordered off of work. If I were you I should consider very carefully two alternatives that you mention. If you go west you have the advantage of being in a familiar country which speaks a familiar language; there is the corresponding disadvantage that your tramping may be accomplished by thinking which is the very thing you want to avoid. If you go to France, you have the disadvantage of the foreign language which is always something of a strain – and of the foreign food and climate; you have on the other hand, the advantage that the little difficulties and irritations are enough to occupy your mind and to banish all thoughts of psychology. On the whole, I myself incline to France; but of course I don't know in detail what your physical condition is. At all events, make your choice carefully. If you don't get thoroughly restored now. . .you are likely to have trouble with your health henceforth forever. . .[12]

Although minor attacks did occur subsequent to this sick leave, no major episodes were recalled. Even the minor ones appear to have disappeared in the early thirties. There was talk within the department that during the 1925 leave, Pillsbury had a brain operation while in England. But no evidence of any kind has been found to support that rumor.

Those persons especially close to Pillsbury accepted the digestive allergy explanation of the attacks, for whatever his condition, it did not appear to have any appreciable effect on him. Dallenbach felt that perhaps Pillsbury's lack of assertion, modesty and retiring nature may have been due to the effect of the illness on his personality. Wallin, however, found him to be that way in 1901, eleven years before his first attack.

His senior colleagues felt that Pillsbury never did regain the condition he was in before his first attack. He continued to write but his production was mostly limited to texts and non-experimental articles of a general nature. He never again equaled the contribution he made in *Attention* (1906) and his pre-1912 publications.

IV

On most social issues, Pillsbury seemed to hold the values of a typical midwestern Republican with somewhat more vehemence than might be expected of him. His wife, however, was a life-long member of the Democratic Party and regularly contributed to the local party funds. But the party worker was warned to come to the Pillsbury home only when the professor was out. Otherwise, the usually mild-mannered Pillsbury would become furious and order the party worker out of his home.

Pillsbury was very close friends with the world-renowned Michigan geologist and explorer, William H. Hobbes. It was a common sight to see the two of them on long walks around the Ann Arbor streets and surrounding countryside. On one occasion they spent a summer

[12] Edward B. Titchener to Walter B. Pillsbury, March 23, 1925 in *The Pillsbury Papers.*

mountain climbing in Colorado. They also shared a rather conservative set of political values. Hobbes was an extremely energetic person and rather forward in promoting his particular view of the nation's interests. In the fall of 1915, he helped organize an Ann Arbor chapter of the National Security League whose main purpose was to secure legislation for universal military training and the buildup of arms.

In those days before the United States' involvement in the first World War, Hobbes along with other preparedness-minded faculty members were constantly complaining that only pacifists were being heard locally. He insisted that the other side have its day. Hobbes and his friends – presumably Pillsbury was also involved – obtained the use of Hill Auditorium for three lectures on national preparedness which were given early in 1916. The most enthusiastically received speaker was Major General Leonard who had previously antagonized President Wilson by urging that steps be taken to resist militaristic Germany.

Hobbes also kept watchful, suspicious eye on his university colleagues for anything that appeared to be pro-German sentiment. He was very annoyed by speeches that were allegedly given by some members of the Department of German and, according to campus legend, once assaulted with his cane on an Ann Arbor street one of the German professors for making such remarks. It is a matter of record that in October 1917, the red-bearded Hobbes appeared before the Board of Regents to present evidence of disloyalty against Professor Carl E. Eggert of the Department of German. Professor Eggert was then dismissed by the University.

Though not quite as energetic in his expression of his beliefs, there is at least indirect evidence that Pillsbury felt sympathetic to Hobbes' outspoken views. The following excerpts from a letter written to Pillsbury by the psychologist J. W. Baird of Clark University contained opinions which were expressed, one may be sure, to a sympathetic reader. The letter is dated October 2, 1917.

> . . . This morning papers contains the rather startling news that Professor (James McKeen) Cattell has been expelled from Columbia University for disloyalty. His offense consisted in sending letters written on Columbia University departmental stationery to our congressman urging that they refuse to support any measure which would send American soldiers to Europe. I confess that the news has not brought a tear to my eyes. You doubtless remember that Cattell's son was mixed up in some of those fool socialistic organizations last summer and is still in the toils. This raises the rather interesting question, however, as to whether a man whose treason has been demonstrated is qualified to serve on our psychological committee of which Yerkes is in charge.[13]

When the United States became actively involved in the war, the forty-five year old Pillsbury was already solidly behind the national involvement. He contributed to it by encouraging his younger associates to join the group of psychologists who were working with the armed services on testing problems. He also assumed the added academic duties of those colleagues (Shepard, Griffitts, Brown) who left for war service.

---

[13] J. W. Baird to Walter B. Pillsbury, October 2, 1917 in *The Pillsbury Papers*.

After the war Pillsbury published a book entitled *The Psychology of Nationalism and Internationalism* (1919) in which he expressed a more liberal attitude toward political movements than might be expected of him. The book was based upon observations he had made in Greece during his 1912-1913 trip to Europe. He had been intrigued by the problem of "mixed allegiance" that many naturalized Greek-Americans had returned to Greece to aid their mother country in the Balkan War. His conclusion, which he developed in the post-war book, was that the explanation seemed to be in the shift from nationalism to internationalism which was generated by their experience of emigration.

Pillsbury then went on to analyze the particular social-political conditions that characterized the post-war world with special attention to their implications for international relations. He did see the need for some world-wide or large scale organization of countries like the League of Nations. Pillsbury often referred in the book to the role of hate as a social force often leading to international complications.

In connection with his theory of internationalism there is an amusing anecdote that again illustrates Pillsbury's propensity for saying things at the wrong time. During 1922-23, he gave a series of lectures at various French universities. In order to prepare for the presentations, Pillsbury wrote four lecture on reasoning, trial and error learning applications, desire, and hate as a social force. He had them translated into French and practiced their delivery before going on tour.

At the University of Grenoble, Pillsbury was to deliver the lecture on hate as a social force. As luck would have it, that very week the French government had ordered its army into the German Ruhr to enforce its demands for war preparations. Pillsbury, whose speech had been written months before, made no special reference to the event, but did assert in general terms that if one does anything to arouse hatred of a defeated people, these people would be strengthened in their unity and ultimately in their effectiveness.

When Pillsbury concluded his address, the rector of the University, who had been especially gracious in his introductory remarks, arose and denounced him for interfering in French politics and giving comfort to the enemy. Pillsbury's French was too feeble to allow him to make an extemporaneous reply. He was very embarrassed.

V

After his retirement, Pillsbury remained active and maintained almost daily hours in his office in the Rackham Building. His dignified figure as he walked across campus never failed to attract the attention of the students now so many generations removed from him. His walks became more solitary as gradually most of his friends passed on. Once or twice during the twenty years that he lived after his retirement, a younger staff member with some interest in the history of psychology would invite him to their class to speak on structuralism some such related topic. (This writer remembers one fascinating hour during the early fifties.) But for the most part, his contacts with the department were semi-weekly visits to the main office to check his mail.

Pillsbury seemed to occupy his time as he had most of his life – by writing at his typewriter. After his retirement he completed another book, entitled *The Psychology of Economics* and tried, unsuccessfully, to have it published. In this book, Professor Pillsbury discussed such topics as economic drives, value, interest and savings, the relationship between individual differences and economic life, property rights, the psychology of productivity,

management labor and collective bargaining. To some who knew him it may appear strange for him to have written such a book. But all his life Pillsbury was quite interested in the stock market and seems to have made a sufficient sum of money out of this interest.[14] Pillsbury corresponded with his old publishers who, though appreciative for his past successful and profitable efforts, turned him down with not too much sensitivity. The completed manuscript remains in his collected papers.

Walter Pillsbury, taken at the presentation of the first Walter B. Pillsbury Undergraduate Award, 1954

Pillsbury published twelve articles after his retirement, the last one in 1957. Five of these articles were obituary memoirs (Joseph Jastrow, C. Stone Yoakum, James McKeen Cattell, Harvey D. Carr, John Dewey) and three others were on historical topics. He also continued to work on a novel that he had started in the twenties. He had completed twelve chapters when his writing stopped about 1958. This date appears to be exact, for the last pages of the last chapter were typed in Pillsbury's inimitable way on the back of the weekly Department of Psychology Colloquium notices that he received.

Professor Pillsbury died suddenly on June 3, 1960. He had been in good health up to this date. Though almost eighty-eight years old when he died, his corneas were unusually clear and his eyes were willed to the University eye bank. His body was cremated and his ashes interred in Fullerton, Nebraska.

[14] In this connection, one of the more amusing series of letters that appear in *The Pillsbury Papers*, has to do with Pillsbury's attempts to gain either monetary or judicial redress from a party from whom he purchased some "watered" gold mind stock.

CHAPTER TWO

Walter B. Pillsbury[15]

The determination of why any decision is made in life is very difficult. It is the more difficult when it is so complicated as the choice of a life career. To answer the question forty years or so after the choice is made blurs the answer still more. One can make up many reasons ranging from lack of any good opportunity to go in any other direction to a strong natural bent plus one or several of the strong stimuli applied in the formative period. So far as I can remember the first specific assertion of the decision in my case was made at the age of fourteen, when still in the second year of high school. I had chanced up a copy of Carpenter's *Mental Physiology* in my father's library and had read it with great interest. I remember saying to my father as I finished the book that I would specialize in psychology when I grew up. How seriously the remark was meant is a question. Until well on in my college course I was supposed to be working towards the law. That also was not especially of my own initiative, so far as I can remember, but was rather acquiescence in the family opinion. I had not chosen any courses with special reference to either career, up to the last year of my college course.

The first really serious interest came from a study under Professor H. K. Wolfe at the University of Nebraska, where I was a student from 1890 to 1892. Whether the course in psychology was required or whether I chanced into it, I do not remember. I do remember vividly the interest that was aroused by the subject and by the man. Wolfe demands a large place in the history of psychology for the number of men whom he led into an interest in the subject. He was one of the early students of Wundt. He had gone to Leipzig immediately after graduation from Nebraska, and had responded to what was best in the German environment. On the completion of his work for the degree, he went back to Nebraska as Professor of Philosophy, set up a laboratory and, alone, undertook to duplicate a large part of the work he had been doing at Leipzig. He gave a course in psychology, which ran through the year, and for which he required considerable work in the laboratory. In addition, he had several courses in philosophy, and did what work was done in education. He was an indefatigable worker, but gave so many courses and spent so much energy on them that he had no time for publication.

Wolfe's influence was exerted personally as well as in the classroom. He was always available for conversation, as he usually sat in the room where the reference works were, and seemed ready to break his reading whenever a question came up. His conversation was as frequently general as technical. He and his family were interested in the Populist movement at the time and he was ready with comments on state politics, and discussion frequently drifted in that direction. He was as advanced in his interests in politics as in philosophy or religion. His viewpoint in psychology was liberal. He was more anxious that his students should think than that they should hold any particular point of view. He was a firm believer in experimentation, and made a session a week a requirement for each student in the elementary course. So far as I remember, he belonged to no special school. As texts he combined James and Ladd, and was, I presume, a fairly close follower of Wundt. That facts should stand for themselves was his dominant doctrine. One of the class exercises was to bisect a strip of paper, when held horizontally and vertically. Each student made a number of these bisections and the results were carefully kept. This exercise was continued through his whole career. The results were worked up after Wolfe's death by Guilford and published. This was, so far as I remember, Wolfe's only

[15] Reprinted from *The History of Psychology Through Autobiography, II*, 1930, pp. 265-295.

contribution after his Leipzig days. A respect for experiment, a belief in a scientific psychology, and a desire to see thinking for its own sake more general, were some of the gains from the two years with Wolfe.

My actual transition to work in psychology was due to a suggestion from Wolfe that I try for a fellowship at an eastern university. It came the year following my graduation while I was teaching mathematics and a general assortment of school subjects at Grand Island College. I was awarded a scholarship at Cornell for the year 1893-1894 and began my work in Titchener's second year. The laboratory was composed of five rooms transformed from classrooms. Titchener had no assistants and few graduate students. Miss Washburn was a fellow and a student in her second year. Titchener did not give the beginning course then, so had more leisure for advanced classes and for directing research. He was also still close to Wundt's position in general theory, so that there was little change in standpoint. Experimentation was the keynote in his teaching. Personally, Titchener was very unlike Wolfe. He held aristocratic opinions on most subjects as opposed to Wolfe's extreme democracy. He believed, while the teaching burden had compelled Wolfe to give over all writing, in publication as the end of the scholar's endeavor. He once said that the only certain immortality was the immortality of the printed page. He was an American editor of *Mind* and continually made small contributions. He had also formed his connection with Hall and the *American Journal* which he continued until Hall's death. He kept the ideal before his students.

As a means of becoming acquainted with the larger trends in philosophy and psychology, the contacts at Cornell were immediately stimulating. As a scholar it became my duty to write abstracts of literature and to review books. This insured keeping up and extending my knowledge of German, and of my more rudimentary French and Italian, and also gave a greater immediate acquaintance with the foreign work in psychology than would have been gained in any other way. An exact evaluation of just what derived from specific instruction in the four years at Cornell is very difficult to estimate after so long a time. The respect for experiment was certainly deepened; added to that was a greatly increased knowledge of what wide scholarship meant, and an esteem of its importance when added to investigation itself.

There was probably little to affect the tendency to one school or another. Titchener was still a devoted follower of Wundt, and none of the other schools had developed to the point of arousing antagonism. The most prominent whipping boy then was the old faculty psychology and the soul psychology of the religious schools. Both were even then sufficiently out of the picture to arouse no great warmth in the attack. Titchener was unwilling to take James' work quite seriously as psychology. He thought of it as literature and philosophy. He used Sully, which was new then, as a textbook in his long advanced course until he had translated Kulpe. He did not, however, oppose his school to James' in any way. It was only later, in fact after my time at Cornell, that he first labeled himself a structuralist. Outside of psychology, a course in the logic of Bradley and Bosanquet had the most influence upon my later thinking. The treatment they gave of meaning, especially the sharp distinction between meaning and image, proved fruitful. Most important was the close personal contact with Titchener. This was especially true of the last two years, when I had a room adjoining his in Cascadilla, a ramshackly school building that had been taken over by the University and transformed into an apartment for faculty and students. As an assistant during these same two years, I had much experience in the preparation of apparatus under direction and in planning courses. This proved invaluable in many different ways.

The first experimental work published was a study of the methods of localizing a point upon the skin. The idea came while acting as a subject for Miss Washburn, who was then preparing her dissertation upon tactual localization. It was noticed in her experiments that the

observer had a tendency to visualize the spot touched before an attempt was made to indicate where it was with a pencil held in the hand. We wondered how far that alone was accurate, and how far it needed to be corrected by the comparison of the two contacts in the trial and error search for the point touched. The test was made by localizing the point upon a photograph of the wrist. The results indicated that visualization was active, although it was not so accurate as the localization upon the skin. Both cooperate in the localization, although the comparison of contacts was apparently the more important factor. By a striking coincidence, Henri published a dissertation on the same subject, for which the work was done at Leipzig, just before we published. The two pieces of work were begun independently.

The choice of a topic for the dissertation was chosen in consultation. Titchener asked if I had anything I was particularly interested in, and I mentioned one or two minor problems, which were shown to be inadequate. Finally I settled on the problem of the mental processes in reading. The suggestion came from a single case of illusion. In the early morning light once I was looking for a house number, and saw on a letter box a badly scrawled MAIL. This was misread as a number, and I had started on when a second glance showed the mistake. The problem I set myself was to determine the relative importance of the sensations and of the memories in the development of the perception processes. An indication of the contributions of more subjective elements was sought in the amount of supplementation that was possible without discovering the error. Words were typewritten with certain letters omitted, others replaced by other letters and some had the letter blurred by printing an x over it. The letters were photographed and projected one at a time upon a ground-glass screen. On the projection apparatus a photographic shutter was attached so that a 1/5 second exposure could be given at will. The observers were merely asked to say what letters were seen and to describe any peculiarities that were noticed.

The experiments showed that considerable changes could be made, and have the word read, in many cases without noticing that any changes had been made in the letters. A study of the errors made possible an analysis of the procedure in building up a word under the ordinary conditions of reading. The importance of different sensory factors was studied directly from a study of the effects of changes in disturbing the reading operation. If the misprint was early in the word, the chance that it would be seen was greater than if it came in the middle. The last letters of the word also seemed to be important. The fact that omission of a letter was more readily noticed than blurring or changing one seemed to indicate that the general form of the word was an important element in determining what should be read. This was confirmed by the reports of the observers, who said that the change in the length was the first factor to impress them.

The more important part of the conclusions dealt with the laws of supplementation. This part of the work was done under the influence of Wundt's distinction between internal and external associations and between associative and apperceptive connections. It was assumed that the process of perception consisted in arousing retained elements that had been connected with the letters seen and the general form of the word. These constituted the associative connections. More stress was laid upon the factors which selected the associates. The experiments planned to bring out these forces by changing the general setting. For this purpose, in one series of experiments, a word associated with the word to be shown was called just before the word was exposed. The percentage of correct readings under these circumstances proved to be much greater than it was in the series in which no word was called. It also happened that, if the observers understood the word called in a different way from the one intended, they would at times see an entirely different word from the one given them, or at least entirely different from the one that was supposed to be shown in blurred form on the screen.

It was also noticed that chance changes in the attitude of the observer were very important in determining how the word would be read. Thus, Titchener would frequently come to the experiment from his office adjoining, where he had interrupted reading German or French. On several occasions he saw combinations that were peculiar to German in the English words that were shown. Similar confusions with the French also occurred, although these were less striking. Words that the observer had been on the point of using or that were related to his thoughts were also likely to influence the perception processes.

The theoretical interpretation of these results anticipated in some degree the conclusion of other workers later. This was before Kulpe and his students had carried on the investigations which led to the development of the notion of Aufgabe on the direction of attending and on the course of associations in recall. Several of my conclusions stated approximately the same facts in a slightly different way. The formulation in the thesis was the basis of the classification of the conditions of attention as outlined in the later book on attention. In the dissertation I divided the factors that determine the way words would be read into objective and subjective conditions. By objective was then meant the effects of the letters actually seen, of the length of the words, and the different kinds of mutilations as they affected the form of the word. Under the subjective were included the associations between the letters seen and those that were recalled, and between the form of the whole word, and the image of that word. More nearly corresponding to the subjective conditions, as the word was used later, was the series of effects of the word called, the chance antecedent conditions and the other words in the series, the occupations of the preceding hour, etc.

More generally, a discussion was given of the way the word apperception had been used by earlier writers and was used at the time by Wundt. The dissertation was entitled "The Reading of Words: A Study in Apperception," which afforded an excuse for the more theoretical discussion. It was shown that apperception had been used originally to indicate the degree of clearness in consciousness, was then changed to mean the interaction between conscious processes that was according to Herbart the cause of the clearness, and finally, Wundt had kept all of these meanings and added the suggestion of an active force or will in the popular sense. It was pointed out that so many different uses destroyed the value of the word. It was suggested that, if it was to be kept at all, it should be used to designate the fact that all elements of experience, past as well as present, were acting upon each other at any time. It should be the name for an observed interconnection and not for a force. The suggestion was made that Wundt could be interpreted to mean something of this kind and used apperception as the equivalent of will only for the sake of brevity. This use of the term and the general notion of interaction of past on present was used in the *Attention* several years later.

A year spent at Cornell after the completion of the work for the doctorate gave an opportunity to extend a knowledge of related sciences that were especially weak. Although still an assistant, I gave an independent course, and was more active in the teaching. The following year I was given a chance to go to Michigan and start a laboratory. I accepted in spite of being offered a chance to remain at Cornell as an instructor. At Michigan I was attached to the Department of Philosophy, but was given full charge of the laboratory and of the work in psychology. After a few years, Professor Wenley had me appointed Director of the Psychological Laboratory, although still attached to the Department of Philosophy. This arrangement continued until Professor Wenley's death in 1929, when a separate department was created, of which I was made chairman.

The early years at Michigan were marked by hard work, as there were large elementary classes, and, from the beginning, a few advanced students who took a disproportionately large share of the time. The latter time was gladly given. Almost from the beginning, opportunity was

taken to extend acquaintance with the work of the biological sciences. Courses in nervous anatomy, given by Professors McMurrich and Huber, were followed. Little opportunity for work in that line was afforded at Cornell in those years. I also took advantage of a chance to begin a piece of work in physiology with Dr. Lombard. We studied the changes in circulation in connection with respiration and allied processes, especially the so-called Traube-Hering wave. Two papers were published as the result of the work. It gave me an opportunity to become better acquainted with physiological methods of recording circulation and with many ingenious devices that Dr. Lombard had developed in his laboratory.

The early work on my students was influenced by this physiological excursion. Several were devoted to a study of the so-called attention waves. Many investigators had noticed that, when they attempted to watch continuously some faint light, it would appear for a time and then vanish. Numerous attempts have been made to explain this phenomenon. The work with Dr. Lombard suggested that there might be some relation between the regular recurrence of these most effective periods in perception and the Traube-Hering waves, which showed, we found, in the pulse rate and in the volume of a member. The duration of the two waves was not very different, and it seemed possible that an increase in blood pressure might have an effect upon brain function. I asked Slaughter to determine whether the two were of the same length. The average duration of the two was about the same. He did not count the number of coincidences between troughs and crests of the two curves as should have been done. Other workers tested the possibility of relating other mental operations to these rhythms. Wright found some tendency to a connection between the rate of reaction and the position on the wave; Stevens found a variation in the estimates of time with the part of the wave involved.

While all of these results seemed significant, later work by certain of Spearman's students and by Griffitts and Miss Gordon here did not altogether confirm them. The coincidences between the two curves are sufficient to be suggestive; they are not exact enough to be entirely convincing. Since the cause of the Traube-Hering waves is not itself known, it matters little whether the attention waves can be referred to them. When two responses, each of unknown cause, are referred to each other, one is little ahead in the explanation of either.

In the same general period, I attempted to relate the attention wave to the fatigue problem. The suggestion that led to the trial came from the assumption that if a wave of effectiveness was represented by the appreciation of a faint stimulus, any decrease in effectiveness would be accompanied by a briefer period of appearance. In 1903, when I had volunteered to take a group of medical students on every afternoon of the week, in addition to the usual heavy schedule, I thought a good opportunity was given to make a fatigue test. That semester, I had a teaching schedule of forty-two hours a week. I made records of the appearance and disappearance of a Masson disk, early in the morning, at noon, and in the evening. Later that year, while at Wurzburg, I took a few records from Kulpe. My records showed a progressive decrease throughout the day. Kulpe, on the contrary, showed a progressive improvement. The records were too few to be significant, but were interpreted as an indication of the difference between the morning and the evening workers. I am certainly of the morning type and Kulpe thought himself to be of the evening type.

Other attempts made to determine if there might be a correlation between the attention waves and the amount of work done were made at later times. The first was lost through an unfortunate circumstance. I was revising a translation of Kulpe's *Introduction to Philosophy* that I had made with Titchener while still at Cornell. In the revision, I used typewriting the translation as material for mental work. I wrote straight ahead and indicated the time on the manuscript each quarter-hour. Between times, at intervals, interruptions were made to take records of attention

waves. The intention was to see what the output amounted to for different periods of work, when doing work under the normal incentives. Professor Titchener was to revise it and that would furnish a measure of the accuracy as well as of the rate. Unfortunately, the manuscript was sent to the English publisher just before it went bankrupt, and by the time the firm had been reorganized twice, the manuscript was lost and the check was impossible. Ten years later I repeated the experiment by writing a book that did not require much collection of new data and keeping a record of the time as I wrote. In this instance the typewriter was provided with an electric contact that would record the end of each line and respiration was recorded during the period of actual writing. At the beginning and at the end, a record was made of blood pressure and of the attention waves. Other tests of steadiness were made that there might be as complete a picture as possible of the mental and physical changes during two hours of continuous hard work. The preliminary results were published in a preliminary form in the *Proceedings of the Eighth International Congress* at Groningen. The final results have not yet been published.

The first book published was the *L'Attention* in French. The book was written before I left for a trip abroad in 1903. It grew out of the work on the thesis, and of the experimental work on the attention waves. It was primarily a summary and interpretation of the work already done on attention. The material collected outgrew its title and extended the principles involved in attention to many fields that lay outside the title as narrowly interpreted. The book was published in French because no American publisher could see his way to publish even so slightly technical a work as that. Meantime, Titchener knew I had the book ready and when Vaschide sounded him about a book on attention for the *Bibliotheque Internationale de Psychologie*, he suggested that he get in touch with me. I sent on the manuscript and it was accepted. The time required for translation was long so that the work finally appeared in 1906. It was enlarged for an English edition in 1908.

The general purpose of the book was to trace the phenomena of selection which we ordinarily call attention to their specific occasions or causes. The attitude was entirely empirical. The first chapter was devoted to a description on the basis of earlier writers of the concrete changes in consciousness which are designated as attention. Increase in the clearness of one group of ideas or sensations was made the primary characteristic of attention. Consideration was given to the notion that this was identical with a change in intensity, but decision as to the identity of the two was left open. Analysis and synthesis were made subordinate to changes in clearness and regarded as dependent upon them.

Careful examination was made of the theories that would refer attention to motor processes in the organism, such as Ribot's, and the general conclusion was reached that the motor processes were not true causes but, at the most, were, with the mental changes, common results of deeper lying antecedent changes. The most important contribution lay in seeking the antecedent processes that could be regarded as the real conditions of attending. In general, the classification was followed that had been given to the contributions to the reading processes in the dissertation. Conditions of attention were divided into objective and subjective. The objective consisted in the characteristics of the stimulus which made it likely to enter consciousness, the intensity, duration, and extent of the stimulus. The subjective were constituted by the immediately present, and the more or less remotely past events in consciousness. They were enumerated as the idea in mind, the mood of the moment, wider educational factors, heredity, and the more general instinctive factors. The mood of the moment, which was intended to designate the general perceptive attitude, lost its intended significance when it was translated into French as *mode*. The essential point in the whole interpretation was the insistence that attending was an expression of definitely empirical factors which could be analyzed experimentally and that explanation could be given without reference to any force or faculty.

Before the English edition was prepared, Kulpe and his students had done their work on the influence of Aufgabe upon both perception and association. These I regarded as merely a more active form of what I had called "mood." In Kulpe's experiments, it was demonstrated that when an observer was asked to look for one object or for one aspect of an object, he would see that in preference to all else. In the English edition, the mood of the moment of the first edition was replaced by three separate phases: the question in mind, which arises spontaneously to the observer; the task or problem set by another as in a question; and the attitude, a more general tendency to appreciate things of a particular kind. The last was most closely related to the old mood.

In both editions great emphasis was placed upon the importance of the concrete as opposed to the abstract. This came out especially in showing that the two words, interest and effort, used popularly to designate the causes of attention, were not separate entities or forces but were merely names to designate groups of conditions and the conscious states that accompanied the different types of attention. Interest designates the pleasure that accompanies attention due to attitude, education, and the inherited characteristics of the individual. It mistakes the effects of attending for the cause, for the members of the class are more universally accompanied by pleasure, and this is obvious, while the antecedent conditions receive little notice. Similarly, attention ascribed to effort or will is really determined by social pressure, which drives the man to do what society approves rather than to what pleases. Attention of this class is always accompanied by diffuse strain sensations. These we call effort and regard as the cause of attending in spite of the fact that they follow rather than precede the clearing up of the mental state. Both are due to antecedent conditions of a concrete type, but they are characterized for the individual rather by their accompaniments than by their real antecedents or causes.

In both volumes the application of the notion that mental states were determined by events which had preceded them in consciousness was extended from receiving sensations or attention proper to the control of the reappearance of experiences, the recall through association. It was asserted that the order of presentation of old ideas was determined in part by the connections between ideas formed by the order in which they had originally been presented, but that these tendencies of recall were finally controlled by the same series of more subjective conditions, active in attention. Particularly, the purpose and the mental attitude are effective. This was suggested by the work of the thesis. There the effects of calling an associated word before showing the printed word proved itself sufficient to decide what word would be read when the word on the screen was much mutilated.

The French edition was written and not revised before I knew of Watt's work on the effect of *Aufgabe* on association. I was at Wurzburg while he was working on the problem, but did not act as subject for him, nor know specifically what he was doing. The book had been finished before that time and was sent off without further revision. A complete revision was made before the English edition was published and in that Watt's work was mentioned and his results definitely incorporated in my treatment.

The principles laid down in these earlier chapters were elaborated in the treatment of the more usually mentioned psychological processes. In the French edition, a chapter was devoted to attention in memory, will, and reasoning. In the English edition, the treatment was expanded to devote a chapter to each of these topics. New chapters were added that showed how attention was related to feeling and to the self. The former was devoted to showing the close relation between theories of feeling and attention or apperception. The chapter on the self was a reprint of a presidential address. It was appropriately included in a book on attention since it showed that self, as the term was used in philosophy and popularly, was largely involved in control of thought and

action. The real control in each of these cases was to be explained by the interaction upon each other of all the various experiences of the individual. This interaction was also the determining factor in deciding what should be attended to. The self as I thought of it as concrete as any other experience. It is a convenient term for the whole man active, and so overlaps very considerably upon the field designated attention.

The concreteness of the explanation was furthered by the attempt to relate attending to the processes of inhibition and reinforcement in the nervous system. In this the work of Exner and the early investigations and interpretations of Sherrington were followed. Both had shown that inhibition of one act by the activity of another portion of the brain was possible and both had assumed that facilitation of the action of one part of the brain might follow upon the action of another portion. It was shown that one could readily translate the conditions of attention as I had outlined them into mutual interaction of different parts of the cortex. The idea in mind would correspond to a continuance of the activity of the part affected so that it would be more readily aroused by a sensory stimulus of approximately the same character as the preceding one. Attitude or purpose would, on the nervous side, have as condition the partial activity of a group of neurons induced by the preceding stimuli or earlier activity. This would be more obviously the case where a preliminary request had been made. Education and the more remote conditions would prepare the way for attitudes by grouping the nervous elements into a complex that would be excited by incoming stimulus or idea as a unit. All of the conditions are to be looked upon as dependent directly upon neurological factors, even if the evidence for the particular nerve processes was of a general character, sometimes, at least, bordering upon speculation.

The main virtue of the approach to attention utilized in the book lay in the attempt to give an empirical explanation of each of the phases of the phenomena. Where data were available, actual experimental evidence was given for each point. Where this was lacking, observations from everyday experience were used and care was taken to emphasize the source of the evidence. Even when the specific data were lacking, the method at least avoided using the faculty method of explanation. Attention was never regarded as a specific force, but was always looked upon as the expression of earlier experiences. Keeping the possibility of nervous processes of explanation in mind also contributed to the same end. In one way it also avoided the atomism of association that had recently been condemned and avoided in another way by the Gestalt movement. I thought of the wider organization as working through and upon the associations and controlling them rather than of replacing the associations altogether by the organizations, as the Gestalt theory would.

The second large piece of theoretical work I began concerned itself with reasoning. Interest in reasoning began with a course on the modern logicians, especially Bradley and Bosanquet, with Professor Creighton at Cornell. On rereading my dissertation for this autobiography, I found that I had appended to it a note that I was at work on an application of the principles discussed in it to the reasoning process. This is the only indication that the problem was in mind, as the first articles that bore on the subject did not appear for several years. The first paper that could be regarded as connected with the subject was read at the meeting of the Psychological Association in Chicago and later published in the *Philosophical Review* for July 1904, under the title “The Psychological Nature of Causality.” It was in essence an attack on the Hume theory that cause and effect could be regarded as merely one of the laws of association and that mere frequency of succession of two events could be regarded as constituting the criterion for regarding one as the cause of the other.

As opposed to, or rather, in addition to, mere successive occurrence, I insisted that there was on the phenomenal side an ascription of force to one and of passivity to the other. This is

represented in the ascription of strain sensations to the active agent, similar to those that one feels in one's muscles when active. When two events are thought of as merely succeeding each other in time, both are regarded as passive, no effort is assigned to either, by what Lipps has called empathy. When regarded as causal, one is given the strain sensations. More important is the question as to what leads an individual to assign the strain to one and regard it as active. This is only in very small part mere frequency of succession. More depends upon the general probability of adequacy to the effect, a probability that is estimated in the light of general experiences with forces of the same type. If the cause seems adequate to the effect it is likely to be accepted with few repetitions, if it is inadequate a very large number of repetitions will not bring conviction. It was pointed out in the article that telepathy would not be accepted even with 90 percent successes, for there is no adequate mechanism for projection. Ten percent of failures with a radio receiving apparatus would be accepted as due to chance mechanical defects, for there the mechanism is known to be present and effective. De Rostand made his cock assume that he caused the rising of the sun by crowing, but this would not harmonize with the general experience of mankind. Mere frequency of occurrence does not give conviction of causation.

A more complete discussion of reasoning than had been given in the *Attention* was offered in the *Psychology of Reasoning*, published in 1910. The incentive to write the book was furnished by an invitation to give a series of lectures at Columbia the first semester of 1908-1909, while substituting for Professor Cattell. There were eight lectures in the series and they were worked over into a book of ten chapters. The position assumed was that reasoning is a process that goes on in the concrete human consciousness and that, on one side, it follows the same laws as recall or perception. As distinct from logic, which is interested only in knowing what is true, psychology studies the actual process by which conclusions are reached by a particular individual and how he knows that they are true once they are attained.

A question that presents itself is the relation to the treatment given by Dewey. Before I wrote, Dewey had published his *Studies in Logical Theory*, which I knew. The more popular and comprehensive *How We Think* was developed at about the same time as mine. They were alike in assuming that thinking is always a function that is performed only when there is some definite occasion. One never thinks unless one must. The divisions of the concrete process followed much the same lines, although they were not identical. I used the names that were given to processes in the older logic, although they were changed in their applications. Like Dewey, I insisted that reasoning always started when some purpose was thwarted. This thwarting might be an actual physical process or it might be in the way of developing a clear organization of thought. The process of removing the difficulty always takes the same course, whether the obstacle be physical or to thought alone. The first step is to understand the obstacle, which I called judgment, for it is really taken when the new can be referred to an old concept or something else that is familiar. The next step is to find some way of removing the difficulty. This, the really essential part of the reasoning operation, I called inference. Then the various suggestions that come in the process of removing the difficulty must be accepted or rejected, and when one seems to be valid it must be justified to the thinker or to his hearers. This process is called proof. These four operations are not always sharply marked off, as they are short circuited at times and at times certain ones seem not to be necessary. On the whole, they are convenient points of reference and can be regarded as constituting a complete act of thought.

Back of the processes there are certain general functions which have always been regarded as characteristic of reasoning and are used in one form or other in almost every act of thinking. The first of these is meaning. It may be asserted that, while meaning is often regarded as subsidiary to mental content, it is really the first function of mental operations and may be regarded as the real datum for psychology. One knows that one means and what one means long

before he knows anything about the structural components of mental processes that are theoretically regarded as making the meaning possible. While meaning is antecedent to all else in the appreciation of the thinker, it needs to be explained in terms of the sensory components, which are more obvious to the analytically minded psychologist. There is always a definite content in mind when one means, and this content may be regarded as an element in the instrument of meaning.

We can study the nature of meaning most readily in its development. The meaning develops by means of the different connections in which a word or object is used. After it has presented itself several times in connection with another object or occasion, it comes to represent the object or occasion. At first it represents it by actually recalling it. Later, it alone may come and the other will be taken for granted on its reappearance. This is the case when a word has, by frequent use, been made to mean an object. When it represents a class the process is the same save that any one of the group may be represented by the first idea. As opposed to Bradley and Bosanquet, meaning was given an empirical psychological explanation and was left as a member of a hypothetical realm of pure thought. It was assumed to be a specific mental content as Titchener seemed to assume it his *Thought Processes*, published at about the same time. It was also assumed that meaning gains in fullness from the more remote elements of experience, that the references constituted a constellation rather than a suggestion of one element alone. The more one knows about a subject, the wider the meaning. Thought is always meaning as opposed to sensation, but the meaning is dependent upon ideational processes and also upon the action of the nervous system.

The view suggested in this book and later was that meaning and thought were synonymous. As opposed to Titchener, who found the meaning in a second idea or sensation, I held that the reference comes through the partial opening of association paths, paths which, when they open fully, recall the object that is merely meant when the partial opening occurs. The tendency to open gives rise to a specific awareness, which serves in place of the specific recall. This anticipatory awareness performs all of the functions that would be performed by complete recall. I suggested that it was the nervous correlate of what Woodworth and the Wurzburg School called the pure thought process.

Closely connected with meaning is the concept. In my view the concept was subordinate to the meaning. Any mental process that had a general meaning was a concept. What the nature of the concept might be was relatively unimportant. What it meant was essential. If the meaning was a group of specific members of the class it constitutes the class concept as interpreted by Galton; if the meaning applies to qualities, we have the concept with an implication of intention. The content might be the same in both cases. A form of concept which affects the content more than the meaning is provided by those cases in which the content of an experience is changed the better to conform to the separate experiences. In many instances of perception one sees an object as it has been transformed under the influence of numerous tests. Thus we always see square objects as square whether they fall upon the retina so as to give right-angle outlines or not. We substitute the angle that we know they must actually have for the angle which is thrown upon our retina. This resultant I called the type, and pointed out that many actual percepts are really types in this sense of the word. This was hinted at in the *Reasoning* and was developed in more detail in an article, “The Role of the Type in Simple Mental Processes” (*Phil. Rev*., 1911, 20, 498-512).

A third function peculiar to thought that received special treatment was belief. Belief is important in the reasoning operation since it serves to give temporary confirmation of the truth or adequacy of each step. Like meaning, this is purely a functional characteristic of mental life. We know that we believe, but it has no corresponding mental content. With Bain it was insisted that

belief is rather negative than positive. Disbelief can be analyzed into conflict between a statement suggested by someone else and the knowledge of the individual, whether that knowledge at the moment be explicitly or implicitly conscious. When the sum of the knowledge is definitely opposed to the statement or to a conclusion which the thinker himself has reached, it is rejected at once. More characteristic is the state that results when there is a balance between considerations that favor and those that oppose the statement. This gives rise to varied strain sensations and a constant alternation of opinion between acceptance and rejection. It is the state that we know as doubt and is highly unpleasant. When one makes a decision by permitting one series of data to dominate, or when a new point of view comes that makes one acceptable rather than the other, the strains vanish, the whole mood is quiescent. This is what we call belief, or, if put in the negative way, disbelief. In general, belief is the pleasant quiescent state which corresponds to the harmony of a decision or statement with the entire mass of knowledge.

Of the active processes, the first is judgment, which was used to designate the appreciation of the preliminary situation. This use was based first on the assumption that the proposition of formal logic was identical with judgment. Assigning this a definite place in the concrete mental operations offered some difficulty. The nearest approach to that had been made by Bradley and Bosanquet, who defined it as the process of assigning meaning to the given. They also point out that the subject is the new or unknown, the predicate the meaning which amplifies or explains the situation. In ordinary speech, the sentence is a designation of what is perceived to belong under the concept or general notion. The subject represents the name or the quality just previously applied to the object, while the predicate is the characteristic that is important at the moment and so represents the real act of judging. In the concrete thought operation, this is applied in understanding the difficulty which spurs to reasoning. When the car one is driving suddenly stops and examination of the tank shows it to contain no gasoline, the single word “gone” brings to the companion a full appreciation of the situation.

As an effective step in the thinking process, judgment prepares the way for action by determining exactly what is wrong. The mechanism is approximately the process of perception. The difference lies in the fact that the appreciation is made explicit by reference to organized earlier experience or concepts. It was emphasized that various forms of appreciation are involved in thinking. One may be interested in determining which of two objects is longer, which gives the judgment of comparison; or, in making an absolute estimate of value, the judgment of evaluation. The processes are the same psychologically. The attitude growing out of the question or the problem determines what judgment is passed. This again determines what concept is attached. The formulation of the judgment in language, the proposition or the sentence, depends upon many circumstances incidental to communication. This may affect the social situation but has no bearing upon the mental operation, and is only in part determined in character by the nature of that operation. One needs to recognize that there are these different ways of formulating the results of the judgment, and this made necessary the distinction between the different formulations.

Probably the most original part of the theory of reasoning lay in separating the process of obtaining the solution of the problem from the proof that it was adequate, and in showing that the older logician had confused them. The syllogism was always regarded as the process of reaching conclusions, and Mill assumes the same function for induction, or for his still simpler process of reasoning, from a single instance, and that a particular rather than a general. I pointed out that there were really two distinct functions, the process of finding a solution for the problem that had been set and understood in the judgment, and, finally, the operation of testing or proving that the solution was distinct. The syllogism is a process of proving only, and could never be used as a means of discovery. Nothing in the syllogism gives or could give any direction to the thought

process. Actual observation of the instances even of theoretical thinking shows that the conclusion always comes before the statement of the universal principle that is called the major premise. Induction also would have no specific value if it began with a blind accumulation of instances. In both cases one has a preliminary hypothesis which is believed to be true and which is tested either by the deductive method of the syllogism or by induction, whether by observation or experiment or both. The formal logician has apparently given all of his attention to the proof process because that is the one which is socially effective and so is most obvious. It can also be taught. The process of developing the hypothesis is complete subjective, and is so little subject to rule it ordinarily escapes observation. The logician assumed that he should be able to say something of the active thinking process, and, having developed the process of proof, which is closely related to the other, he apparently merely took it for granted, without further thought, that the two were identical.

I called the process of reaching the conclusion inference. The use of the term is not without objection, but the application made seems in harmony with the derivation of the word and with current usage. The method of reaching a conclusion does not follow any regular course. So irregular is it that the term trial and error was applied. When a problem is to be solved, one suggestion after another comes, and the attitude of the thinker seems to be one of waiting for a satisfactory idea to present itself. The act seems to be as little controlled as are the movements made by Thorndike's cats in obtaining release from a puzzle box. This suggested the trial-and-error notion. Of course, each separate suggestion springs from the stimulus of the situation and is determined by the previous connections under the influence of the attitude of the thinker. But there are so many preformed connections and the attitude changes so frequently that the suggestions cannot be predicted in advance. Also, no rules can be given that will hasten the appearance of the right solution. One can only wait and watch for the answer to present itself. There is little control.

The fact that one cannot control the appearance of the suggestions for solution does not mean that they are not subject to law. We may assume that they depend on the laws of association under the influence of context, as do other recall processes. The thinker cannot voluntarily discover the cues that will bring the solution at the suitable occasion. He seldom attains the end with a single suggestion, and often he must delay hours or days before an acceptable suggestion will make its appearance. Nearly always there are a number of ideas which succeed one another while one revolves the problem, and each is rejected in turn. Wallas, in his very interesting *Art of Thought*, began by taking exception to my interpretation of the situation, which he thought held out little hope of a solution, and devoted himself to a full analysis of the conditions under which satisfactory solutions appear. His final result, however, was not very different from mine. He found that one should not know too much, that much was to be gained from giving up and taking a walk or other forms of diversion, but the amount of actual control which he demonstrated is relatively slight.

Assuming that ideas come more or less at random, so far as the intention of the thinker is concerned, it is obvious that the process of selection is a highly important factor in the thinking process. The immediate determinant of the selection is the belief process. Each claimant to be a solution is tagged as true, possibly true, or false, or there is an alternation between acceptance and rejection or doubt. Many of the suggestions are rejected at once, because it is obvious that they do not fulfill the conditions or are not in harmony with the experience of the individual on that point. Some are accepted tentatively and given a more definite and explicit test later. Others are accepted immediately. Belief is the real determinant of reasoning. As was said above, belief is the result of a reaction between the past experience and the present environment of the individual.

The nervous correlates can only be speculated about, but any of the theories of irradiation from parts of the cortex not fully active would give a possible explanation.

Even after a solution has been accepted as true, it is essential that the retention be justified. This is done through proof, and proof is essentially nothing more than making explicit reference to the earlier experiences that are acting implicitly in determining belief. The function of the syllogism is really in connecting the conclusion that has been derived through association processes with the universal principle that is contained in the major premise. That means that the order of the syllogism does not at all correspond to the order of actual thought, in fact, exactly reverses it. The function of the syllogism seems to consist in stating in definite terms the principle that crystallizes the knowledge which implicitly controlled belief. The statement adds no knowledge. Its only function is to make obvious what previously was latent.

On the whole, this volume on *Reasoning* attempted to show that thinking was the development of the ordinary mental processes. It made more of the notion of concept and meaning than in other parts of psychology. The concept is the formulated results of earlier experiences. They fix the interpretations and serve as points of reference in simpler interpretations. The general proposition is the analog of the concept on the more complicated level, and also acts in the same way as point of reference and justification for the results of inference. These are the norms and exercise a controlling function. The active course of reasoning is determined by desires and by the necessity for the elimination of conflicts and inconsistencies. In this it is an expression of the growth of knowledge in man and in society, and of the practical and intellectual needs of the individual and of the group.

One incident of this middle period may be of general interest. Sometime between 1910 and 1912, Watson was showing me through the laboratory at Johns Hopkins. I was very much struck with the apparatus he was developing to investigate the color vision of animals by the use of spectral lights. Apparently with too-well concealed enthusiasm, I remarked, “So this is psychology,” counting upon the emphasis or attitude to make known my appreciation. While in Germany a little later, I read his article in the *Psychological Review* of 1913, in which he first stated his radical behaviorism, and was very much surprised at his interpretation. It will be recalled that he said that a psychologist to whom he had been showing his spectral apparatus had remarked, “So this is psychology,” and that this was interpreted to imply contempt for anything that was not introspection. The incident might be interpreted as evidence that even the prince of behaviorists cannot correctly interpret emotion from behavior alone, even when the behavior involves words. It was so long after the publication of the article before I saw it that I neglected to correct the impression. I may take this belated occasion to remove a possible stigma from the camp of those who do not accept behaviorism in all of its tenets.

In 1912, I published an elementary text, *The Essentials of Psychology*. As a useful text must be, it was a compilation of the more important facts of psychology. It embodied in abbreviated scope the main outlines of the material previously published in my own monographs, and was extended to include the whole field as it appeared at the time. It retained the point of view of the *Attention*, but extended it to include the nervous system, sensation, and the details of space perception. It attempted to coordinate the materials of the psychology of the day from a purely empirical standpoint and with as little reference to schools as possible. Anything in the way of a contribution was limited to methods of presentation and organization.

This was followed in 1916 by a larger book, *The Fundamentals of Psychology*, also intended primarily as a text. It gave room for a greater amount of detail in the presentation of experimental data, and could presume a greater maturity on the part of the students in the

appreciation of advanced theory. The division of the texts grew out of local needs. Almost from the beginning at Michigan, we had two courses, a longer, which extended through the year and might be accompanied by laboratory work to give a thorough survey of the field, and a briefer, introduced first at the request of the men in Education, which lasted but a semester.

During the war, work was very heavy because of the taking over of courses that had been left by members of the staff who had gone into national work of different kinds. This left little time for independent work. During this period I was interested, as was everyone else, in the social phenomena presented. The result of this interest was *The Psychology of Nationality and Internationalism*, 1919. The volume was suggested by observation on the mixed allegiance of the Greeks who had returned to their native land and whom I met in a trip to Greece in 1912-1913, during the Balkan-Turkish war. The war itself suggested the possible shift to internationalism. This offered an opportunity for a more extended treatment of instinct than I had given in the texts. There I began to insist upon the affective values which act as selecting agents as the part of the entire instinct which is probably native. It was pointed out that much of what passed for instinct in the texts was probably learned, but that the pleasure that attached to certain results, when attained, constituted the selecting agent in the chance movements. What is inherited is a tendency to continue acting until an unpleasant stimulus is removed or a pleasant one obtained, and, more important, a determination of what shall be pleasant or unpleasant to a given species. This point of view was extended in an article published in the Washburn Memorial Volume in 1929 to make the pleasure itself largely dependent upon a tendency to have one movement or group of movements persist in response to stimuli to any kind to have all movements cease under stimulations of another kind. That reduces the inheritance of pleasure to one of the inheritance of a tendency to a specific response.

Another point of psychological interest in this book was the objection raised to thinking of a community mind as a real entity. A group has no consciousness apart from the consciousness of the separate individuals who compose it. The group does show certain tendencies to respond that are different from those shown by the separate individuals who compose it. This can be explained in terms of the influence exerted by each upon the others. What may be called the social instincts must be made to bear the burden of the explanation of these reactions. Fear of the mass, on the one hand, which becomes strong enough to show itself in paralysis or in an incoordination of movements of all kinds, is the more important of these. In a weakened form, this becomes Cooley's social pressure. On the other hand is the increased strength of response and general adequacy that may show itself in a congenial group, and a confidence in which each takes courage from the others. That a group or the presence of others will, on certain occasions, decrease and on other occasions increase the efficiency leaves the final result uncertain. Nevertheless, both conditions may be observed and both are necessary to explain the social phenomena. Social phenomena must be analyzed into their component parts and not explained by mere reference to a group mind.

Of the forces which hold a group together it was asserted that a common hate was the most effective. The hatred of a group of surrounding nations increased the solidarity and so the strength of that nation, and the greater the hate the greater the unity. In a single community, dislike of an individual or of a group would develop unity in other groups more surely than would common likes and community of action towards a desirable end. Political or even religious groups need recognized evil or a rival that can be hated to be really effective. The same theory finds numerous applications in any type of society. It should be noted that Martin in his *Behavior of Crowds* developed the same theory from another point of view, and on mainly pathological evidence, at about the same time. There is no evidence from the dates of publication that either

could have borrowed from the other. I did not know of his treatment until some years after my book was published. Both seem to have been an outgrowth of observations of war propaganda.

An interesting illustration of the effectiveness of the hate propaganda, or possibly of its wide appeal, came a few years later. In 1922-1923, I spent a year in France and gave a series of lectures at a number of universities. One of the series was an adaptation of this chapter on "Hate as a Social Force." I had given it at several institutions without arousing other than favorable comment, and chanced to repeat it at Grenoble the week the French entered the Ruhr to enforce the demands for reparations. I had made no reference to French politics or to reparations. In fact, the illustrations chosen were all drawn from the War and were perfectly good pro-ally propaganda. In spite of all, when I finished, the rector of the university, who had been exceptionally gracious in his introduction, rose as I finished and denounced me in no uncertain terms for interfering in French politics and giving comfort to the enemy. I had only asserted in general terms that if one arouses the hatred of a nation, that nation will be strengthened in its unity and ultimately in its effectiveness. My French was too feeble to attempt a rejoinder. The lectures had been translated for me and I did not venture on an extemporaneous explanation. My embarrassment was great, but I did not fail to note the amusing side.

A final chapter outlined the similarities between the national and the international consciousness and attempted to trace the limits within which a society of nations might utilize the forces of nationality in gaining strength when once it had been established. This would apply whether the society was definitely organized or was a result of an unorganized good will. That offered little that was new psychologically. It merely showed how the social laws might be extended to the wider field.

The year in France in 1922-1923 was spent in learning a little more French, in preparing and giving a series of lectures, and in becoming acquainted with the smaller French universities. One quarter was spent at Toulouse, a second at Montpelier, and the third in Paris. I prepared four lectures for the general tour, of which two were published. One showed the many points at which the trial-and-error process finds application in the more truly mental operations. It first found its application by Thorndike in modern times, in explaining the motor learning of animals. I had shown that it was the main factor in the reasoning process as well as in recall through memory. In this article I showed that the process very probably is an element in the development of percepts. A percept does not correspond to the sensations that are aroused in the sense organ or in the brain through the sense organ, but are always more completely organized and consistent. It was asserted that this consistency is made possible by a series of trials which finally develop a notion that will agree with various groups of sensations by eliminating certain ones of them. Those eliminated are overlooked and forgotten, and what remains is regarded as the real object. Concepts are merely a further elaboration of the percepts and arise in the same way. They eliminate still more of the unessential or inharmonious factors and leave what agrees with all the others.

Another lecture was a summary of my theory of reasoning, brought up to date, and with more outstanding parts emphasized. Most was made of the point that the active parts of reasoning and the confirmation or proof were distinct and that the syllogism was effective only in the proof. It was not a process of thinking in the sense of reaching conclusions or solving problems. When I reached Pairs, I found that I was announced for a special course at the Sorbonne. For this I wrote a series of three lectures on the development of psychology in America. The first part on the American schools was published in the *Journal de Psychologie* in the following year. While in Paris, I made a few experiments on fatigue in confirmation of some work that had been done with Griffitts before I left America. Professor Pieron kindly offered the facilities of his laboratory and I

availed myself of it so far as time permitted. The article was published in the *Proceedings of the International Congress of Psychology* at Oxford in 1923.

While in France, and before and after, I devoted some time to the writing of *Education as the Psychologist Sees It*, intended to be a popular presentation of what psychology has to contribute to education. Possibly the central idea was developed in an article in *Popular Science Monthly*, published in 1921. In this I pointed out that many of the advantages which we take credit for as a result of education in our schools and universities are really a result of selection. That only those who have the greatest ability naturally, at least those who stand in the top 10 percent, succeed in passing through our entire educational system. These men would be the successful men, whether educated or not. The universities assume that what they do to them makes them what they are, while the chances are that nothing that can be done to them would spoil them. The only question is whether anything done to them by education really improves them over what they would be without that influence. On this we have no definite evidence.

I attempted to bring together what few facts we have that bear on the point of the effects of training. I also presented what seemed to me the more important contributions of psychology to each of the different problems of education. These chapters contain a maximum of psychology and a minimum of strictly educational application. It was written on the assumption that a brief summary of the important applications of psychology to learning and related processes might be useful. The assumption seemed justified by its success.

A book that was developed for a special purpose was the *Psychology of Language*. This was an outgrowth of a request that Professor Meader, of the Department of General Linguistics, had made that I cooperate with him and Professor Scott, at that time Head of the Department of Rhetoric, in a course on the psychology of language. This course was given for ten years or more. Finally, it was suggested that the lectures be put into book form, which was done. It was intended to bring together the phases of psychology which bear upon language in many of its aspects. In addition, Professor Meader developed some of the features of language changes and similar, more strictly philological problems which could be related to the psychological laws.

So far as anything more than the elements of psychology were involved, I tried to emphasize the great dependence of the separate parts of the language units upon the whole situation in which they developed. That eliminates all theories of the sentence that would reduce it to a form of equation, or that would make the symbols have value apart from the context in which they are found. If one goes back to the theory of the sentence, subject and predicate must be regarded as constituting different aspects of a common object or situation that attract attention successively. The successive awareness is what holds them together. They are not equal to each other; if they were, they would be identical and the statement a tautology. That means also that they are united by a common attitude, and this attitude or purpose must be present also in the mind of the hearer if they are to represent to him what was present in the speaker. While this was but one of the problems considered in the book, it illustrates the emphasis that was given to the particular situations in the explanation of each topic. The general thesis was that language is a psychological process and each step must be considered in the concrete relations if it is to be correctly understood.

In the winter of 1927-1928, I was asked by a representative of Mr. Norton if I could write a history of psychology. As I had given a course in the field for a number of years and had no plans for further work after the *Language* which was then in the hands of the printer, I consented. I also did not know that so many others were already engaged in the task. I had heard the summer before that Murphy was working at one, but, as nothing definite had been said about its

publication, I thought it might be long delayed. I agreed to have it finished by January 1929. My daughter was studying in Germany that year and was to have the summer semester in Berlin, so I went over there for the summer to work on it. I desired to know more of the Gestalt movement, also, and the chance to hear of it at first hand added an incentive. I had finished a first draft of the earlier periods before going over, and had the use of the library of the Psychological Institute and of the university for the later periods. The Institute library was very convenient and complete for more recent times. The University and State libraries could not be said to be convenient, with the restrictions of permitting books to go out only between twelve and one and then on slips left the day before, but they did have most of the books I needed. Between them, I could obtain most of the sources I required. Much could be accomplished in the two months' time free from regular duties.

The original intention was to write a brief sketch of the earlier periods, giving in each epoch only the contributions which really influenced later writers, and then to give a fuller account of the more recent men who could be said to have developed the modern psychology. As time was short and considerable difficulty appeared in extending a treatment beyond the essentials without becoming verbose and repetitious, the expansion even in the later parts was not very great. I assumed it was better to restrict what was said to what could be remembered as important rather than to enter into the variations in the treatment of a subject by different men. That offers much room for expansion, and may be useful for the specialist, but the specialist will naturally go to the sources and not limit himself to a history. Where so many men are to be covered, only their most important contributions can be touched upon. Fortunately, the men who have written at greater length on these portions have supplied any lack I may have left.

I also chose to treat a man mainly for what proved to be his main influence on the course of later thought, and to neglect those phases of his work in which he mainly improved upon the work of the men who had first written in the particular field. This is very often unfair to some of the later men whose extensions may well be more important than the first statement of the man who pre-empted the problem. Sometimes the very effectiveness in statement of persistence in repetition of one of the later men has led others to forget that he was not the originator of the particular point of view. One recent case is very striking. Most, even of psychologists, credit Watson for fathering behaviorism, while, as a matter of fact, Max Meyer had propounded all of the essentials of the doctrine a year or more before Watson adopted the position.

When it came to the later periods, when psychology really became established, the amount of material was so great that I abandoned any attempt to trace the important experiments. I was forced to restrict my treatment to the more important schools and to neglect many leading men altogether. In this later period, I contented myself with outlining the position held by what seemed to me the important schools and mentioning only the men who are closely identified with those schools. This meant omitting from consideration many men who have kept clear of theoretical controversy or who have shone only as critics of schools. This list includes a number of the more important modern writers. The classification of schools is always itself open to question. I could have increased my list indefinitely, but chose to err on the side of conservatism. In discussing the schools, occasion was taken for a certain amount of criticism. This consisted in part in pointing out the historical roots from which the different schools sprang and in part in indicating what seemed to me to be fallacies or inconsistencies. This discussion was restricted to schools in order that the treatment might be as impersonal as possible.

I made relatively little attempt to summarize the results of experimental work. In recent times this has been of such great volume that it seemed hopeless. I did point out the classical experiments and many of the classical methods in each larger topic. The neglect was not at all

because the results were regarded as unimportant, but because it seemed impossible to do all justice without making the book so large as to be unmanageable and to trespass upon the field of the larger texts. Any selection was bound to be prejudiced, in that it would emphasize those phases of the subject that seemed to me important to the neglect of the rest, and would be likely to give too much space to my friends and men who had worked in the same lines as myself. Again, any statement of results would be out of date in a very few years, while the general outlines may be counted on to persist at least for a longer time.

I may close with a statement of my own general position. I presume I am one of the men who would be regarded as belonging to no school. One of the more recent presidents of the American Psychological Association took me to task, by implication at least, for seeing something good in each new theory. He may have implied that this was not merely good nature. As a matter of fact, I have long believed that a general theory has little justification except as a convenient setting for facts. From its very nature, a school must be one-sided. Its statement is either a protest against an existing school or a rallying cry for a bit of propaganda. Only within limits is either useful. I have attempted to formulate a general position on several occasions, but have never pushed it very hard. In an early article, I pointed out that we know functions rather than structures, directly. In an address, very brief and ex tempore, at the Western Psychological Association in Leland Stanford University in the summer of 1925, I elaborated the notion and showed how I would include among the functions, functions of thought in all of its forms as well as the biological functions that were given exclusive place by the traditional school of Dewey and Angell. The same position was elaborated in a paper at the Yale Congress, and in the introduction to the third edition of *Essentials of Psychology*, published in 1930.

In all, I insisted that what one really knows directly are meanings, as was suggested in the *Psychology of Reasoning*, and that meanings are functions. This begins with sensations and perceptions. Man knew that he was warm long before he knew that he had warm spots or warm sensations. He knew that one object was farther away than another before he knew that space existed. He knew that certain phases of objects were distinct, other indistinct, long before he knew anything of attention. Attention, itself, was first needed as an aid to understanding when man appreciated that, on occasion, this clearness was due to conditions within himself and not to the medium or even to the adjustment of the sense organ. This probably occurred even before man assigned his consciousness to structural elements as sensations. One felt pleased long before he had any question as to whether affection and sensation were different structures, or whether emotions were real states of consciousness or mere back strokes from organs of internal response.

In short, the datum of knowledge is always “that we know” in each of its phases rather than any “what.” We can assert that the structures, particularly the so-called mental structures, are mere artifacts that have been developed by a psychologist to make plausible a basis for the functions. Just as it was said in the paper on “The Role of the Type” that objects are developed by a process of trial and error to explain what is immediately known, so more truly can it be said that no one directly intuits a red as sensation structure. He has developed the concept red to explain the corresponding function, and then has found it convenient to talk of an independent red element. One is immediately aware that a red-headed boy is ten feet or more away from him, and that one is afraid he will be struck by a passing car. To explain this, one may assume that there are rays of light affecting the retina, that these arouse old memories to complete the estimation of distance, and the appearance of the rapidly moving sensations of black arouse the secretions of adrenal glands and contraction of unstriped muscle, which may explain what one calls fear. All of these are constructions, interpretations of the immediate datum that one sees the boy and the car and feels fear. These functions are real and immediate, independent of any interpretation.

This attitude toward psychological facts would not eliminate any of the explanations that are ordinarily given in present day psychology, so far as they are adequate and true. Assume that what we know directly is always the "awareness that" certain events are true, and we can bring in as many additional constructions as are needed to explain how we know. We can make that more immediate even than consciousness, which, in one sense at least, is a hypothesis to explain the fact of awareness. It would certainly be that if we give it any substantial character. We could also retain as many of the concepts of sensation, Gestalten, behavioristically defined attitudes, as could justify themselves on empirical evidence. The test would be the degree to which the assumption squared with observed facts and with other hypotheses that themselves were in harmony with observations.

In addition to these more subjective functions of knowing and feeling in their various forms, we would, of course, have the objectively determined biological functions that were the problems of the older functionalists and of the modern behaviorists. They would be treated in the way that they treat them, so far as that is in harmony with the facts and does not deny the functions of knowing. If accepted in this double sense, functionalism would be the broadest statement of the problems of psychology, and would take least for granted. It could use two points of view, what is seen by the onlooker and what is immediately given to the actor. It would first outline the simple functions – in fact, this has been fairly adequately done – and would then relate them to antecedent experiences by analogy to the known facts of nervous anatomy and physiology, and to as many as possible of the related functions. It would not change the modern psychology; it would, however, formulate its principles in a way to remove a large part of the dogmatism that now cumbers the work of the various schools.

CHAPTER THREE

Walter Bowers Pillsbury: 1872-1960[16]

Cooperating Editor 1897-1960

Walter Bowers Pillsbury, cooperating editor of this JOURNAL for sixty-four years and one of the first generation of psychologists trained in America, died in Ann Arbor, Michigan, late in the morning on June 3, 1960. His death, due to myocardial infarction, was sudden and unexpected as he had until then been enjoying excellent health and was at the time planning a trip to Europe to attend the meetings of the International Congress at Bonn, Germany. He was within a few days of his eighty-eighth birthday, as he was born on July 21, 1872, in Burlington, Iowa. He was the oldest of seven children – four boys and three girls – born to William Henry Harrison and Eliza Crabtree (Bowers) Pillsbury. His father was a Methodist minister who was sent by his Church, because of his administrative ability, through wide areas of Iowa and Nebraska.

The young Pillsbury was a precocious lad. He was graduated from high school before his sixteenth birthday and entered Penn College, Oskaloosa, Iowa, the following fall. His predilection toward psychology was aroused during his second year in high school by a chance reading of Carpenter's *Mental Physiology*, which he found in his father's library, to which had free access. After reading this book, which he found to be most intriguing, he remarked to his father that he intended to specialize in psychology when he grew up. Until late in his college course, however, this boyhood interest was not pursued as he found himself working toward law – not on his own initiative but rather from "acquiescence to family opinion."

After his sophomore year at Penn College, Pillsbury transferred, in the fall of 1890, to the University of Nebraska. There his early enthusiasm for psychology was revived by a course that he elected under H. K. Wolfe, Wundt's third American student and a magnetic teacher who turned a large number of his Nebraska students to careers in psychology. This course, which ran throughout the year, required considerable laboratory work and aroused in Pillsbury, as he afterwards reported, "a respect for experiment, a belief in a scientific psychology, and a desire to see thinking for its own sake more general."

He graduated from Nebraska in 1892, and then taught "mathematics and a general assortment of school subjects" at Grand Island College, Grand Island, Nebraska. He remained here for but one year, as he accepted, in the spring of that year, the offer of a Sage Scholarship in Psychology at Cornell University, for which he had applied at Wolfe's suggestion. The fall of 1893 found him, therefore, in Ithaca, New York. His lifework was decided; his early ambition was to be achieved.

He found the academic atmosphere at Cornell congenial and stimulating. Titchener was beginning his second year there. Sine he, like Wolfe, was a student of Wundt's there was little change in point of view. Experimentation, as with Wolfe, was the core of Titchener's teaching. In addition, however, Pillsbury found great pressure for publication which Titchener held was the end of a scholar's endeavor.

---

[16] Taken from *The American Journal of Psychology*, founded in 1887 by G. Stanley Hall, Vol 74, No. 2, June 1961.

Psychology at Cornell was then, and for many years after, a part of the Sage School of Philosophy. As a Sage Scholar, it was Pillsbury's duty to acquaint himself with the trends in philosophy as well as in psychology and to write abstracts and book reviews for the *Philosophical Review*, a quarterly magazine that had shortly before (in 1891) been established by the Sage School. These duties assured the development of a facility in writing and, since many of the books received for review were French or German, in reading those foreign languages. In addition to these "outside" duties and his courses in philosophy and psychology, Pillsbury served as an observer in the various studies being conducted in the laboratory. The exchange of observational hours among graduate students was standard procedure in the Cornell laboratory throughout Titchener's directorship of it. This practice provided the necessary observers, trained them in introspective reporting, and frequently gave them ideas for further research. Titchener, moreover, held that as much knowledge of psychology was gained by serving in a well-conducted experiment as in an academic course.

From his service as an observer in one experiment – Miss Washburn's doctoral dissertation on the influence of visual associations on the spatial perception of the skin – Pillsbury obtained the idea for his first experimental research, which dealt with the problem of cutaneous localization with and without vision and with vision on life-sized photographs of the area (left forearm) stimulated. He started on this "minor" problem, as all research performed by students except doctoral dissertations were designated, during the second semester of his first year – experimentation was indeed, as he soon found, the core of Titchener's instruction. Pillsbury's observers were Titchener, Miss Washburn, and three graduate students; so soon did he realize the benefits of the interchange of observational hours. He pushed the study rapidly forward, but withheld publication until Miss Washburn's study, which was temporally and logically prior, had appeared.

His scholarship at Cornell was renewed for a second year and he immediately turned to work upon his dissertation and, in collaboration with Titchener, upon the translation of Kulpe's *Einleitung in die Philosophie.* The topic for his dissertation was chosen in consultation with Titchener. Pillsbury suggested several topics, which discussion revealed were unsuitable, and finally proposed an investigation of the mental processes involved in reading – an idea that came to him from the misreading of a street number on a letter box – which Titchener deemed appropriate. Pillsbury conceived of the problem as one in reading, but, as the investigation proceeded, it developed into a systematic study of apperception and attention, a turn that Titchener doubtless anticipated and directed.

For this third year at Cornell, Pillsbury was advanced to an assistantship. During this year, he completed his dissertation, his translation of Kulpe's *Einleitung*, a minor study on the projection of the retinal image, and was awarded his PhD degree. As teaching was, in those days, practically the only occupation open to a psychologist, and the demand for teachers in 1896 was small, Pillsbury found himself with a degree and no place to go. This situation, however, was soon rectified by his reappointment to the assistantship. The post-doctoral year proved highly profitable. He attended classes in physiology and related sciences in which he thought he was weak, rewrote his dissertation for publication, read the galley proofs of his translation, and devoted himself to teaching as he, though still an assistant, was permitted to give independently a course in the Department.

His dissertation and his translation, both published in the spring of 1897, attracted attention to him, and calls to positions in psychology were now not lacking. Among those received was an instructorship at the University of Michigan. Though Cornell met this offer, Pillsbury accepted Michigan's invitation because it entailed the welcome responsibility of

establishing a new laboratory. At the same time, he was invited by G. Stanley Hall to join the Cooperating Board of Editors of this JOURNAL – the first of Titchener's students so honored. Both of these affiliations were maintained by Pillsbury throughout the duration of his long life.

Psychology at Michigan was then taught in the Department of Philosophy, of which R. M. Wenley was the head. Pillsbury, however, was given full charge of the laboratory, which he quickly established, and all of the work in psychology. His advancement, slow according to present standards, was then considered to be rapid as he was promoted to an assistant professorship in 1900, to a junior professorship and the directorship of the psychological laboratory in 1905, and to a professorship in 1910. Psychology was continued in the Department of Philosophy, however, until Wenley's death in 1929. Then a separate Department of Psychology was created and Pillsbury was made chairman, the position he held until June of 1942, when he was retired at the age of seventy years to an emeritus professorship.

Pillsbury carried to Michigan his enthusiasm for research and publication gained from Titchener. His early years there were marked by hard work. During one semester, he had, as he reported in his autobiography, "a teaching schedule of forty-two hours a week." For several years after joining the staff at Michigan he further improved his knowledge of the biological sciences, begun during the post-doctoral year at Cornell, by taking courses in neural anatomy and by collaborating with W. P. Lombard, professor of physiology, on studying the influence of circulatory and respiratory changes, in particular the Traube-Hering waves, on the fluctuation of attention. Two papers published jointly with Lombard and nine papers from his laboratory resulted from this excursion into physiology.

In 1902, the beginning of the most active decade of his life, he revised his translation of Kulpe's *Einleitung*. The manuscript of the revision was, however, lost when the English firm to which it had been sent for publication went into bankruptcy shortly after receiving it. Due to the time required for the firm's reorganization and in the preparation of a new manuscript, publication was delayed until 1904. In the meantime, in 1903, shortly before he left for a trip abroad to visit the European laboratories in psychology, he wrote his first book. This book, which was on attention, grew out of his dissertation and the experimental work on attention done in his and Lombard's laboratories. It was written in English but translated into French and published by a Paris firm as no American or English firm was willing to undertake the risk involved in the publication of such a technical and highly specialized work. Because of the time required for the translation, the publication of *L'Attention* was delayed until 1906. Except for the translation, which was evidently not made by a French psychologist as it was not always clear nor correct, the book was well received. Soon offers to publish the English text were forthcoming. Pillsbury chose, however, to rewrite and greatly to enlarge it, with the consequence that the English edition of *Attention*, which appeared in 1908, was practically a new book. Of all his numerous works, it is probably the one for which he is best known.

During the first semester of the academic year 1908-1909, he gave a series of lectures on "The Psychology of Reasoning" at Columbia University, a topic of interest acquired at Cornell while taking a course on "the modern logicians" with J. E. Creighton, and sustained over the intervening years by a series of articles on reason, thought, and judgment. These lectures were expanded and published under their title in 1910 – the year that he held the presidency of the American Psychological Association, was promoted to his professorship at Michigan, joined *The Psychological Review* as Advisory Editor, and also the year marked by the translation of the English edition of his *Attention* into Spanish and its publication in Madrid.

From the vantage point of these achievements, he turned from his laboratory to the writing of textbooks and non-experimental articles. His first elementary textbook, *The Essentials of Psychology*, published in 1911, was eclectic in its point of view. Pillsbury was not a systematist, either in inclination or training, and he never pledged allegiance to any school. He moved freely, therefore, from one system to another as his interest in the topics being considered dictated. Though professedly *The Essentials* was written from a functional point of view, he made use "of the results of structural psychology wherever they throw light upon function or are interesting for themselves." The catholicity of his treatment proved to be popular. The book was widely adopted. It went through numerous printings, a revised second edition in 1920, and it was still so greatly in demand that it was profitable to publish a third edition in 1930.

The Essentials was written for a short, single semester course. It was not adequate, however, for the longer, year-long courses in elementary psychology which, by then, universities were offering in increasing numbers. Michigan very early offered two elementary courses: a semester course for students in Education and those who wished merely an acquaintanceship with psychology; and a long, two-semester course for students intending to major in psychology and for those desiring a more thorough survey than could be obtained in the short course. To meet the needs of the long course, Pillsbury wrote *The Fundamentals of Psychology*, which was published in 1916. This book is also eclectic in its point of view. As stated in the Preface, Pillsbury drew in its writing upon "the work of all schools": functionalism, structuralism, and behaviorism – the latter had just recently appeared upon the psychological scene in America. He subordinated theory to fact and expounded the latter from the point of view from which it could most favorably be treated. Since beginning students have little or no appreciation of what is involved in a shift in point of view, the lack of system was advantageous and soon this book had achieved the popularity of his first. It, too, was widely adopted and used for many years; a revision was published in 1922, and a third edition in 1934.

During World War I, work was heavy upon Pillsbury. He remained at his post and took over the load of his colleagues as they entered the Services and other work for the National defense. There was little time for independent work. He bethought himself, however, of the observations that he had made in Greece during his second trip abroad in 1912-1913, on mixed allegiance. To assist in the Balkan-Turkish War being waged at that time, many Greco-Americans returned to their mother country. Why did they leave America to fight for the country from which they had migrated? The answer seemed to Pillsbury to lie in the shift from nationality to internationalism. He developed this theme in The *Psychology of Nationality and Internationalism*, published in 1919.

Beginning with his *L'Attention* in 1906, Pillsbury published a new book in psychology at about the rate of one every three years. In addition to the six mentioned above, he published the revised edition of his *Essentials* in 1920; the revised edition of his *Fundamentals* in 1922; *Education as the Psychologist Sees It* in 1925; *The Psychology of Language* (with C. L. Meader) in 1928; *The History of Psychology* in 1929; the third revised edition of his *Essentials* in 1930; *An Elementary Psychology of the Abnormal* in 1932; the third edition of his *Fundamentals* in 1934; the second edition of his *History* in 1937; and his last work, *A Handbook on General Psychology* (with L. A. Pennington) in 1942, the year of his retirement.

The break in the three-year rhythm of new books, which occurred in 1922, was doubtless due to his trip to France, where he spent the academic year 1921-1922 as an exchange professor. He lectured one term at Toulouse, the second at Montpelier, and the third at the Sorbonne, in Paris, and, as opportunity permitted, toured the country, lecturing at the smaller universities. In this schedule, little time availed for the writing of a book. He did, nevertheless, as he states in his

autobiography, find time to begin the writing of his provocative book on *Education as the Psychologist Sees It*. In this book, he defended the theses that the results of education are actually the results of selection; that only the students with greatest native ability succeed in passing through the educational system; and that they would have been successful men or women whether they passed through it or not.

The Psychology of Language, which followed in 1928, was the outgrowth of a joint course he had given for ten years with C. L. Meader, professor of linguistics. His *History of Psychology*, undertaken to fill a real need, as no American had written a text in this field, was also an outgrowth of a course that he had taught for many years. He wrote it in Berlin during the summer of 1928, where he had gone to gain first hand knowledge of Gestalt psychology and to be with his daughter who was studying there at the time. Though his book was one of three on History that appeared in 1929, his was sufficiently successful, because it was brief and simply written, to warrant the publication of a second edition in 1937.

His last book, *A Handbook on General Psychology*, written with L. A. Pennington, one of his former students, is a strange potpourri of psychological information, as its subtitle, "A Summary of Essentials and A Dictionary of Terms," indicates. It is divided into four parts: Part I is a brief elementary textbook; Part II is a 73-page dictionary of psychological terms; Part III contains 105 brief biographical sketches of psychologists; and Part IV is a bibliography of 62 reference books in general psychology. It is difficult to see for what part of the trade it was written.

In addition to these books, Pillsbury wrote many articles on psychology and cognate subjects. During his early years, these articles were concerned chiefly with experiments; his middle years, with essays and timely topical reviews; and his latter years, with historical and necrological notes. Due to his long life, he was frequently called upon to mark the passing of his early contemporaries (Titchener, Washburn, Claparede, Jastrow, Yoakum, Cattell, Carr).

Eleven books – to them going through three editions, one through two editions, and one appearing in two foreign languages – one translation which went through two editions, and sixty-nine articles secure his place in the history of American psychology. He was one of the pioneers who founded laboratories and departments, who wrote books and edited journals, who prepared the way for the generation of psychologists that followed him. He received, in recognition of his achievements, all the honors within the power of his confreres to bestow upon him. He was elected to the presidency of the American Psychological Association in 1910; to the presidency of the Western Philosophical Association in 1907; to the vice-presidency and chairmanship of the psychological section of the American Association for the Advancement of Science in 1913; to membership in the National Academy of Science in 1925; to the National Research Council in 1921; as a foreign associate of the Societe francaise de psychologie in 1925; and he held membership in the Linguistic Society of America, The Society of Experimental Psychologists, and the Midwestern Psychological Association. In addition to the lectureships at Columbia University and in France, previously mentioned, he gave the Henry Russel lecture at the University of Michigan in 1933. In that same year his Alma mater, the University of Nebraska, conferred the LL.D. degree upon him.

Throughout his professional career, Pillsbury gave freely of his time and experience to editorial work. In addition to his long service (sixty-four years) on the editorial board of this JOURNAL – the longest editorial service on record – he served *The Psychological Review* as noted above for twenty years from 1910 to 1929. He was also an associate editor of "The Journal of Social Psychology" for thirty-one years, joining its staff at its founding in 1930 and remaining

on it until his death. Earlier, he edited the series of "Studies from the Psychological Laboratory of the University of Michigan," which ran from 1901 to 1905; and with J. W. Baird and M. F. Washburn, the commemorative volume, *Studies in Psychology*, published in 1917 in Titchener's honor by his colleagues and former students.

Pillsbury was subject to petit mal through most of his life. This affliction did not, as we have seen, affect his productivity nor his ability to work, nor did it shorten the span of his life, but it may have affected his personality. He was not the self-assertive man that one of his position, honors, and achievements might well have been. He was a gentle man – modest, retiring, and unobtrusive. Though always cordial and friendly with acquaintances, he held himself a bit aloof and allowed himself but few close and intimate friendships. Those who succeeded in breaking through his reserve, found his friendship firm and true.

He is survived by his wife, Mrs. Margaret M. (Millbank) Pillsbury; daughter Margaret Elizabeth (Mrs. Warren Baxter) and her three children and four grandchildren; and son, Walter Millbank Pillsbury.

Karl M. Dallenbach
University of Texas

CHAPTER FOUR

Walter Bowers Pillsbury

1872-1960

A Biographical Memoir by Walter R. Miles

Elected to the Academy in 1925, an outstanding scientist and writer in the field of experimental psychology, Walter Bowers Pillsbury has been widely known and respected by generations of college students and by the teachers of those students. He was one of the early prominent American psychologists to complete his graduate training in the United States under what were considered quite ideal and stimulating circumstances. During his graduate studies in America he advanced the international front of his chosen science by the translation of important German publications into English. Within a year after receiving his doctoral degree at Cornell University, he joined the faculty of the University of Michigan. Here he developed and headed a productive laboratory and department of psychology. He had been a member of the University of Michigan community for sixty-three years when he died suddenly in Ann Arbor on June 3, 1960, at the age of eighty-seven. During the last week of his life he was moving about apparently in good health, associating with friends and colleagues on the campus and at the University Club.

In 1635 Pillsbury's ancestors were living in Newport, Massachusetts, and it is recorded that one ancestor was given land in southeastern Maine after serving in the Revolutionary War. His mother's ancestors were sea captains who lived near Mt. Desert, Maine. His mother, Eliza Crabtree (Bowers) Pillsbury, was graduated from Kent High School and later taught in Rockford Seminary, Rockford College, Illinois. Walter Pillsbury's father, William Henry Harrison Pillsbury, D.D., after college graduation was a Union soldier in the Civil War. After his discharge he enrolled in Boston University and was graduated in theology. He then served as a pastor in a Methodist church in Portland, Maine, for three years. The succeeding appointment was to Burlington, Iowa, and it was here that Walter Pillsbury was born, July 21, 1872. During the early years of Walter's life his father, because of his success in building up churches, was moved quite frequently, serving a succession of congregations in county seats of southeastern Iowa.

Walter was the eldest in this family which came to include seven children, four boys and three girls. He perforce attended a succession of public schools and in spite of the handicap of discontinuity seems to have made good progress, due in part, no doubt, to the strong educational influences exerted by his parents. He learned to read early and by the time he was seven, reading had become his avocation. His father, described as an avid book buyer who was often seen studying secondhand book catalogs, accumulated a library of two or three thousand volumes. Although his shelves held a goodly number of books on theology, there were also many on philosophy, literature, popular science, and standard history. In this family there were often discussions of questions and problems which resulted in suggested readings among the books in the same library. The pastor's wife sometimes tutored her children in Latin. As she was interested in botany, she developed a herbarium and was always trying to add other specimens to it. Parental educational influence was continuous, even though residence might be transitory.

When Walter was thirteen, he had three brothers and three sisters, and his parents decided that living on a farm would be a good way to meet the needs of the family. About 1880, Pastor Pillsbury had purchased some land when an Indian reservation was abandoned near Fullerton,

Nebraska. In 1885, he decided to farm this land and with team and wagon moved his family and chattels to a small house that stood on the place. The hilltop location of this dwelling gave a wide view of almost unbroken prairie. Life was not unlike that of pioneers. The small church in Fullerton was not far away from his farming pasture and, furthermore, the town had a high school. It was not all work and no play in this country community. Hunting and fishing and other recreations were enjoyed. Two teachers constituted the faculty of the high school, and Walter, attending as a new student, seems to have impressed his teachers favorably. He took more courses than would usually be permitted, and it seems, within one school year was given two years of high school credits. When summer vacation came he started to learn the printing trade as an apprentice in the shop of a weekly newspaper. Also he occasionally served as a reporter. These interesting activities had become possible because the family had moved into town. The farming adventure had actually proved impractical before the end of the first winter. In 1886, Pastor Pillsbury was assigned to a church in Oskaloosa, Mahaska County, Iowa, and his eldest son entered high school with junior standing. The Iowa schools were at that time more formal than those in Nebraska, and Walter was allowed to take only the regular number of courses. He was graduated in the spring of 1888. That autumn he enrolled as a freshman in Penn College, which had been founded by Quakers in Oskaloosa in 1873. This institution was small, young, and vigorous. Walter, having lived nearby for two years, knew much about Penn and was enthusiastic to become a student here. He described the college faculty as ranging from high school level to men with doctor's degrees. He noted that chemistry was taught by a man with a degree from Johns Hopkins in geology, and German by a man who had earned his degree in theology in Germany. Both these men he described as competent scholars and inspiring teachers. The other faculty members might compare well with teaching fellows who now give much of the elementary work in large universities.

Freshman Pillsbury, two months past sixteen, was about two years younger than most of his classmates. However, he was tall and of medium build, he had accumulated experience and knowledge from living in different communities including Oskaloosa, and, moreover, he had probably read in as many or more books than others in his class. Inspection of the college records shows he was a leading student. Within the two years spent at Penn College he had eight term courses in Greek and Greek literature, three in German, three in Latin, and three in English. He did courses in geometry, trigonometry, algebra, analytic geometry, engineering and chemistry, all with flying colors. Almost without exception his grades were in the 90s. In conduct he was marked 100% except in the spring term of his freshman year, when he seems to have kicked over the traces so noticeably as to receive only 98%. At the end of his sophomore year, Walter joined his parents who now lived at Grand Island, Nebraska, where his father was pastor of another Methodist Church.

There was no sophomore course in psychology at Penn College. There was a course labeled Moral Science, which Walter had taken along with three terms of Greek Testament. The combination, including his other Greek and Latin studies, may have given the impression that he planned to follow his father in the Christian ministry. This inference was incorrect. He knew about psychology two years before entering college. At fourteen he had discovered in his father's library Carpenter's *Mental Physiology* and had read it with much interest. He has recorded that, on finishing this book, he told his father he intended to become a psychologist. Now, after his completion of the sophomore studies, his continuation in college was a matter of concern to his parents. They would have wished him to attend an institution founded by the Methodist Church. However, after visiting one such college and talking to members of the faculty and later talking to some other faculty members at the University of Nebraska, the parents left the choice to their son. He chose Nebraska, founded in 1869, and was admitted to the junior class.

In making this choice Pillsbury was quite lucky, for he came under the instruction of Professor H. K. Wolfe, who was the third American student to get his doctor's degree in psychology with Wundt at Leipzig. Pillsbury found Professor Wolfe an inspiring teacher, who stimulated his students to consider the attractiveness of a career in psychology. He offered his students unusual opportunities in conducting experiments and made himself available for consultation and conferences, even though he was at that time responsible for teaching university courses in education and philosophy as well as those in psychology. Pillsbury took several of Wolfe's courses and as usual did much independent reading. On graduation with his A.B. from Nebraska, he was offered an opportunity to teach mathematics and English branches in a small denominational school which was started under the name Grand Island College at Grand Island, Nebraska. He accepted and at first found the experience quite frustrating since it involved teaching courses for which he had had no proper training or background, according to his own evaluation. But he was astute and shifted some courses and students, except freshman, to other members of the faculty whose classes were small. The new college had buildings but no endowments, and at the end of the initial year the faculty was much reorganized. Pillsbury had made a sufficiently good teaching record to be requested to continue as a faculty member. However, he decided to accept a scholarship in psychology, for which he had applied at the suggestion and with the recommendation of Professor Wolfe. This offer of a Sage Scholarship in Psychology at Cornell University was a more stimulating challenge. At Cornell University for the year 1893-1894, he became Professor Edward Bradford Titchener's second graduate student – it was also Titchener's second year at Cornell. The graduate student in psychology who preceded Pillsbury was one year and four days younger than Washburn, and they found their companionship as graduate students mutually enjoyable. Washburn, well on her way in a research problem on cutaneous space perception, used Pillsbury as one of her experimental observers. She was investigating the influence of visual interpretation on the localization of a point touched on the skin of the subject's arm. From his experience serving as a subject observer, Pillsbury got a problem on which he wished to work. This was to test the accuracy of vision alone when attempting to identify the contacted skin points by reference to a life-size photograph of the subject's arm. He had his observers indicate on a picture a point that had been touched. The localization proved to be more accurately indicated on the skin than on the photograph. This experiment resulted in Pillsbury's first scientific paper, "Some Questions on the Cutaneous Sensibility," published in the *American Journal of Psychology* in 1895.

For his doctoral dissertation he formulated a problem which he first described as a study of reading. Specifically, he proposed to examine how images from the printed page become words in association with memory. He tried to determine the relative importance of a sensation and of the memory in the development of the perceptual process involved in reading. As a title for this research he chose "The Reading of Words: A Study in Apperception." This thesis contained a large amount of theoretical discussion, and some of these early formulations in Pillsbury's thesis later became bases for his classification of conditions for mental attention which were detailed in his book *L'Attention* (Paris 1906).

In his second year at Cornell, Pillsbury devoted a large portion of his time to an English translation of Oswald Kulpe's *Einleitung in die Philosophie*, which he later published in association with Professor Titchener as joint translator. Titchener, as a devoted student and follower of Wundt, sent the first doctoral thesis written under him, that of Washburn, to Wundt to be translated into German and published in *Philosophe Studien*, which Professor Wundt edited. Pillsbury's doctoral thesis was published in English, but not until that of Washburn had appeared in print, since the two studies bore a relationship to each other.

With this dissertation completed, Pillsbury had a third year at the Cornell Laboratory as an assistant and devoted this period to the study of related sciences and also to teaching some independent courses. He had received his PhD in June of 1896, and his translation of Kulpe's book and his own thesis were published in the spring of 1897. These publications did much to build his reputation as a rising scholar in the new field of experimental psychology. He received offers of academic positions. One was an instructorship at the University of Michigan. There was also a welcome invitation from Cornell, but he chose Michigan and this proved a choice for life.

As in other American institutions at the time, psychology was taught in the Department of Philosophy at the University of Michigan, and this department was under the chairmanship of Professor R. M. Wenley. However, when Pillsbury occupied his position as instructor, he was immediately given responsibility for the work in psychology. Considering the early history of this new branch of science at Michigan, this was quite an honor. And another professional recognition came to Pillsbury in his twenty-fifth year, just as he was transferring from Cornell to Michigan. President G. Stanley Hall of Clark University invited him to become a member of the Cooperating Board of Editors of the *American Journal of Psychology*, which Hall had founded in 1887. Pillsbury accepted this invitation and was active in this connection during the remainder of his life, that is, for about sixty-two years.

Psychology became an academic subject in Europe in the period 1872-1880, and in America in the late eighties and early nineties. It was often called mental philosophy. At Michigan, psychology proper had begun with John Dewey, who was an assistant professor of philosophy there from 1884 to 1888 and again from 1889 to 1894. Dewey had written a textbook entitled *Psychology*, published by Harper and Brothers in 1886. Under the encouragement of Dewey, in 1890 James H. Tufts, with a PhD from Freiburg, who was an instructor of philosophy, gave a course at Michigan physiological psychology and conducted it as a laboratory course. Later George H. Mead added some experimental work and also gave what was called an advanced course. These early courses in psychology at Michigan captured the interest of some outstanding students who later became important figures in this field. One was James Roland Angell. Michigan thus had an interesting and rather recent tradition in psychology on which Pillsbury, with his rich years of experience at Cornell, could build effectively. He worked hard, teaching large elementary classes, developing demonstrations and experimental apparatus, reviving interest, reassembling and procuring the makings of the psychology laboratory, and training some advanced students. His interest, scholarship, and effectiveness in psychology were recognized by his colleagues in philosophy and in 1901, the word Director of the Psychological Laboratory were added to his title in recognition of the separateness of the two disciplines. Almost from the beginning of Pillsbury's work at Michigan, he resumed a pattern he had adopted as a boy. This was to pursue a reading course outside his prescribed school work schedule, with the aim of broadening his education and scholarship. To this end he followed courses that two Michigan professors were giving in anatomy and, particularly, made a study of the nervous system. Also he engaged in some cooperative research with Professor W. P. Lombard, physiologist at Michigan, who was one of the founders of the American Psychological Association. They made studies of respiration and pulse rate under a variety of conditions and published two papers based on these investigations. These extra-departmental activities were not untaken because of slack duties in connection with teaching and laboratory work in psychology during the first few years. Although these departmental duties were heavy and were well executed, academic promotion under the wing of Philosophy was slow. Pillsbury was a gentle and friendly man and did not overexert himself to become divorced from philosophy. Finally he was promoted to full professorship in 1910. He was elected a member of the American Physiological Society in 1905 at its eighteenth meeting, held in Ann Arbor.

During the period when Pillsbury was an instructor at Michigan he first met socially an undergraduate woman who would become his wife. She had previously attended a normal college course at Potsdam, New York, and at Michigan was majoring in English. She was, or became during their acquaintance, president of her sorority, and was elected to Phi Beta Kappa. Her home had been in New York City, and later she lived in Rye and Port Chester, New York. Her name was Margaret M. Milbank. They were married on June 16, 1905. Mrs. Pillsbury became closely associated with her husband's work, particularly in the final preparation of his scientific manuscripts.

As a scientific scholar Pillsbury deserved to be rated unusually proficient. He had a special sense or intuition of how to follow a scientific path and not be led into byways, and he could ingeniously make his scientific efforts do double duty by providing in themselves data for study of attention waves, fatigue, and other complex problems of human behavior.

As an example of his being his own experimental subject while getting his schedule of professional work done, one semester when he was teaching a schedule of forty-two hours a week and had also volunteered to take a group of medical students every afternoon of the week, he fixed up the delicate Masson disk test to examine how faint a visual stimulus he could discriminate under a standard condition morning, noon, and evening. His records showed a progressive decrease of sensory efficiency throughout the day. These results prompted him to determine the correlation between the "attention waves" and the amount of work he accomplished while engaged in revising his translation of Kulpe's *Introduction to Philosophy*. He did his work on the typewriter and inserted a mark on every sheet every quarter-hour, and at certain other intervals made interruptions to record the "attention waves." The manuscript, which had served as the vehicle for this series of self-experiments, was unfortunately placed in the hands of an English publisher shortly before the firm became bankrupt and in the confusion of it reorganization the manuscript was lost and the data thus disappeared. Pillsbury, in an extended review of his own professional life, tells of other instances in which he arranged to make studies of his progress in composition on the typewriter as he produced a book or shorter manuscript. He inserted marks representation of time intervals, making use of electrical contacts in the typewriter and he took blood pressure readings and "attention wave" records at different intervals. In this way he would study mental and physical changes during a period of two hours of consecutive work. Results of such studies are noted in his autobiography. He was so thoroughly interested and occupied in his research work and writing that he lived and flourished under philosopher chairmanship more successfully than did some of his colleagues in other institutions. In the academic year 1896-1897, Robert Mark Wenley (A.M., Glasgow, 1884; PhD, Glasgow, 1895; LLD, Glasgow, 1901; Sc.D., Edinburgh, 1891) was appointed Professor of Philosophy and head of the Department of Philosophy at Michigan. Pillsbury joined the department the succeeding year. Professor Wenley continued as departmental head until his death in 1929, after which a department of psychology was created as a separate entity, with Pillsbury as chairman. By that time Michigan had already granted twenty-three PhDs in psychology. Chairman Pillsbury's bibliography numbered some forty scientific publications, including textbooks covering different aspects of the new and rapidly developing field of experimental psychology.

Pillsbury's first book developed from his doctoral thesis. The problem that profoundly interested him at this time was the analysis, definition, and identification of predisposing conditions resulting in the psychological phenomenon called attention. He endeavored to seek out the antecedent processes which could be verified as real conditions for attending. These were divided into objective and subjective factors. He enumerated some of these as the idea in mind, the mood of the moment, background educational factors, and instinctive and hereditary factors.

"The mood of the moment" seemed to him of central significance. Earlier psychology has posited a number of "faculties" often thought of as forces or entities such as attention, reasoning, etc.

For Pillsbury, "the essential point in the whole interpretation was the insistence that attending was an expression of definitely empirical factors which could be analyzed experimentally and that explanation could be given without reference to any force of faculty." No American or English publisher was found who would take the risk of issuing such a specialized scientific study in psychology. It was accepted by the French publisher Doin, was translated, and appeared in 1906, and found many interested readers. An English version was brought out in 1908 by Swan Sonnenschein and Company, Limited, London, in a series called "Library of Philosophy" edited by Professor Max Muirhead. It was dedicated to Professor E. B. Titchener. The manuscript had been read by Professor James Angell. The English edition could make use of suggestions offered by reviewers of the previous version, and new material was added. There were twenty chapters, a rather extensive bibliography for each chapter, an index of names including some 130, and an excellent index of topics. This 350-page octavo book contained no illustrations or tabular matter but offered a great many clearly stated results of psychological experimentation, with critical evaluations and citations of analogies from wide ranges of human behavior and experience. The mutual interaction of the different parts of the brain was stressed. Attention thus represented a focal point in a large complex, one-word title of this excellent book which was propitious for the growth of its author's scientific reputation.

At a meeting of the American Psychological Association in Chicago in 1901, Pillsbury read a paper entitled "The Psychological Nature of Causality." This paper, which was an attack on Hume's theory that cause and effect could be regarded as one of the laws of association, was the beginning of a large investigation into the theoretical nature of reasoning. Pillsbury could not accept the idea that mere frequency of succession of two events could be taken to mean that one was the cause of the other. He held that one event had about it the characteristics of activity and that the other represented some passivity. In this paper and in these considerations, he was initiating his second large piece of theoretical work, which would engage much of his interest for several years and result in the production of his second volume of experimental studies published under the title of *The Psychology of Reasoning* in 1910. In the preface we learn that "this little volume" is based upon eight lectures given during Pillsbury's tenure of the non-resident lectureship in psychology at Columbia University in January and February, 1909. He describes this undertaking as an effort to determine the ways in which reasoning has grown out of the simpler mental operations, and to discuss the uses that have been made of these materials in reasoning.

Before Pillsbury published *The Psychology of Reasoning*, he was familiar with Dewey's book *Logical Theory*. Dewey's *How We Think* and Pillsbury's *Psychology of Reasoning* appeared in the same year. There is a similarity of thought in these books. Both hold that thinking results only from something in the way of a definite occasion. Usually reasoning starts when some purpose is thwarted. The first step is to identify the obstacle and to see it in terms of some concept or at least something that is familiar. The next is to formulate some way of displacing or avoiding the difficulty. Various suggestions present themselves. These may involve immediate or remote implements. If a suggestion has the quality of seeming valid, then the reasoner must justify it to himself and perhaps to his hearers. In so doing he claims to have produced proof. These four operations may be drawn out and make distinctive or they may be short-circuited and become almost indistinguishable. Pillsbury worked out his treatment of reasoning in great detail, from many different angles, and with many different illustrations. He took pains to show that thinking was the development of the ordinary mental processes and not something resting on a pedestal composed on a mysterious substance.

Professor Pillsbury was a gentle and rather shy personality. It seems probable that he found more pleasure in teaching through his writing than in lecturing to classes. He was or became a devoted, conscientious, and industrious author of textbooks dedicated to the advancement of the understanding of psychology. His general text, *The Essentials of Psychology*, appeared in June of 1911. On page one, the reader found a surprising definition: "we measure the intelligence of an animal by its accomplishments. Mind is known from man's activity. Psychology may be most satisfactorily defined as the science of human behavior." This definition was a breakthrough for psychology. It seems to bring the subject out of the fog and into sunlight. Not a few were surprised, including J. B. Watson, who commented: "I was greatly surprised some time ago when I opened Pillsbury's book and saw psychology defined as the science of behavior." This brief explanation was to become popular. Watson's book *Behavior: An Introduction to Comparative Psychology* appeared in 1914. Pillsbury, in the third edition of his *Essentials of Psychology*, which appeared in 1930, continued to make use of his earlier definition. In the introduction he writes "We define psychology as the science of behavior and of the knowing functions of man. This definitely asserts that psychology studies the behavior of animals other than man, and at the same time makes it explicit that we intend to consider the problems connected with man's perceiving, remembering, and thinking as well as his mere objectives of behavior." The reader was told: "Our problem is to understand behavior and to investigate the laws of human experience as the immediate antecedent and conditions of behavior." His fourth book, entitled *The Fundamentals of Psychology*, appeared in 1916. In the preface he wrote:

> My own theory inclines towards a functionalism. The book is more concerned with what consciousness does than with what it is. As opposed to the extreme behaviorism, however, I am not concerned alone with understanding the movements of the organism and the function of the movements, but also with understanding knowledge and the way in which it develops. It is my belief that the content of the science is the same whatever the point of view from which the subject be approached, and that this content is essential and changes slowly and then through growth. The theories are less important and likely to change from decade to decade.

In both the *Essentials* and the *Fundamentals* the fore part of the book features up-to-date material on the nervous system. This impressed upon the student the close relationship existing between physiology and psychology. The *Fundamentals* was much revised in the edition of 1922.

Professor Pillsbury's books characteristically contained up-to-date discussions, diagrams, and first-class illustrations. The references, indices, and lists of questions were all planned with much care to aid the student and further his interest and advancement in psychology. Pillsbury made minimal use of secretarial assistance, and thus details of his writing, which he did on his typewriter, were looked after personally. His interests were broad; he had methodological and meticulous ways of working. Throughout his professional life he was not disturbed by shifting from one university post to another; he had steady drive; and he constantly received ample evidence of the success and usefulness of his publications, which led him to issue a procession of psychology texts.

During his professional career Professor Pillsbury attended several international congresses and many less formal meetings of psychologists. For instance, he was a welcome visitor at the Fourth Annual Meeting of the Western Psychological Association at Stanford University, August 8-9, 1924, and contributed a paper entitled "A New Type of Functionalism."

He expressed the opinion that a definition of the purpose of psychology should formulate as closely as possible the actual ends and attainments of the majority of men and women who work in this field. It seemed to him that psychologists might agree on the existence of mental functions as such and study each function as a fact, leaving all theory as to ultimates until facts are exhausted, and even then might state the theory as a hypothesis or probably formulation of observed facts rather than a major premise. His criticism of published statements on functionalism by both Dewey and Angell made the meeting a lively and memorable occasion. Perusal of Pillsbury's professional bibliography, especially the first half, reveals his continuing interest in an effort at defining and portraying psychology as a science distinctive from its philosophical background, with which he was so familiar. He was interested and well prepared, during many years, to write on the theme "The Present State of Psychological Science in America," as he did in the *Scandinavian Scientific Review* in 1925. His familiarity with the beginning and trend of the new psychology prepared him well for writing a very readable history of the subject.

Pillsbury's manner of delivery as a lecturer or public speaker might sometimes be described as rather apologetic and lacking in self-assurance. Still, he would deliver his lecture or paper with evident interest in the subject, and to those who might remain to talk with him individually he revealed a warm, friendly, and enthusiastic personality. He had a strong interest in people and their ideas; he was objective in his discussions and tolerant of points of view other than his own. Pros and cons could be worked over in much detail and with vigor, but the discussion was such as to give pleasure to those who engaged in such debates with him. His writing was characteristically clear, factual, and concise.

It falls to the lot of senior scientists to be called upon to prepare necrologies and memoirs for scientific colleagues and acquaintances whose lives may end before their own. Pillsbury, as his bibliography will show, was very generous in this respect, and among such manuscripts which he prepared were those for James McKeen Cattell and John Dewey, both for the National Academy of Sciences.

In general, the Pillsburys as a family had good health. It is true that the Reverend Mr. Pillsbury, Walter's father, died in 1893 at the age of fifty-three. However, this was due to blood poisoning. Walter was twenty-one and was just entering Cornell as a graduate student when his father died. His mother, Eliza Crabtree, lived to see her eldest son well established and widely recognized in his professional field; she passed away in November 1919, at the age of seventy-seven. As the eldest of the seven children, he partly supported his mother and also helped his six siblings as they attended and were graduated from the University of Nebraska. His mother made her home at Lincoln, Nebraska, after her husband's death. One sister, Susan, became a Latin teacher in a Detroit private school for girls. Hannah became Dean of Women at the University of Montana, and later married and took up ranching in Wyoming. She died in 1955. Margaret, the youngest of the seven, married Dr. F. E. Denny, who took his PhD at the University of Chicago and was Plant Physiologist, U.S. Department of Agriculture. Edward, after serving as a soldier in the Spanish-American War, took his medical degree at the University of Michigan, practiced successfully in Frankenmuth, Michigan, for forty-eight years, and died in 1951 at the age of seventy-four. William went into business selling automobiles. Paul devoted himself to farming, first in Nebraska, and later had a business in grain wholesaling and crop insurance in California. On a slender and uncertain income, but with strong intellectual interests and consecrated efforts, the Methodist minister and his wife served well their day and generation. All of these seven children except Susan, the Latinist, had married. Five of the six couples had children. Walter's family consisted of a daughter, Margaret Elizabeth, and a son, Walter Milbank, both of whom survived him.

Professor Pillsbury purchased his first automobile in 1925. He learned to drive but preferred that Mrs. Pillsbury do the driving and thus have the car at her disposal while he was at the University. Usually he was driven to his office and called for before lunch; after lunch and a nap at home, he was taken again to his office. He worked in the Psychology Department at experiments, lecturing, consultation, or writing until about 3:30 when he would close up shop and walk over to the University Club. Here he met many warm friends. He would play bottle pool of bridge or engage in other forms of entertainment until Mrs. Pillsbury called for him in the family car. After dinner at home, he would usually spend the evening reading. He was strong physically and not one to be pampered; he looked after his own coal furnace and did other chores at home; when the weather was good he often walked to and from the University or used a bus. It was a regular routine for him to spend one or two hours on some afternoon during the week on a walk with a special friend. Two of the colleagues with whom he often enjoyed such occasions were Professor Arthur Cross of English History and Professor William Hobbs, a geologist. He also played golf occasionally until his retirement. He was good at skating, which he had learned as a boy in Iowa. Almost any kind of outdoor activity or sport interested him, and he was especially fond of camping, canoeing, and swimming. During his early married life, he and his wife frequently canoed on the Huron River near Ann Arbor. They were members of a canoeing club that used a lake twenty miles away, and they spent one or more summers canoeing in the Algonquin Park area of Canada, and also in the Rockies of Colorado. In some summers Pillsbury went to Maine. He went to the University of Michigan's geological camp in the Jackson Hole country of Wyoming. In 1930 he went elk hunting in Wyoming from Hannah's ranch. In 1934, with Professor Hobbs, he climbed several 12,000-foot peaks in Colorado, looking for glacial cirques. He and Professor Hobbs had spent a considerable portion of the summer of 1915 in studying the only U.S. volcano, Lassen Peak, shortly after it had erupted. Spending summers in Europe, with long walking trips in Switzerland and elsewhere, was typical of Pillsbury's earlier professional life. In 1922-1923, when he was a visiting lecturer at the Sorbonne for a semester, he also lectured a few times at nine other French universities. But he also did much traveling and walking in France, Germany, and Switzerland. His family was with him during that year abroad. He was in Europe the summers of 1925, 1926, and 1927. In the latter year he attended the International Psychological Congress at Groningen. Again his family was with him, and his daughter, who had been graduated from Bryn Mawr that year and had won a European fellowship to study physical chemistry in Germany, was his walking companion in the Swiss mountains.

It was in the Michigan Scientific Club that Professor Pillsbury found his deepest and most abiding fellowship outside his family. He was a member of this organization for half a century. His devotion to it was deep and enduring. The senior member of the group, he seemed younger in body, mind, and spirit than many of his juniors. He was the club's Nestor, without whose participation no gathering of the group seemed complete. His many honors brought no change in his innate modesty, which made his personality so attractive to these intimate associates. The Scientific Club gave a banquet for him in honor of the occasion of his retirement from active duty at the University, and on this occasion he asked to be excused from making any formal response to the eulogies pronounced by his friends and colleagues.

Although Walter Pillsbury earned a Reserve Army Commission from his ROTC at Nebraska with General Pershing as his Professor of Military Science and Tactics, he never served on active duty in the U.S. Army. However, he did serve on active duty as an active, tireless soldier for Science. And we may note he willed his eyes to an eye bank at the suggestion of his oculist, who found that his corneas were unusually clear.

CHAPTER FIVE

John Frederick Shepard

1881-1965

I

John F. Shepard was born on January 30, 1881 in Greenfield, Illinois where he spent his early childhood and obtained his public education. He entered St. Lawrence College in Canton, New York in 1897 receiving his bachelor's degree four years later. In the fall of 1902, Shepard began his graduate study in psychology at the University of Chicago. Toward the end of his first year, he heard that Michigan was looking for a graduate student who would assist Pillsbury. Shepard applied, was accepted, and transferred to Ann Arbor in the fall of 1903. The assistantship was to grow into a life-long assignment.

In 1906, Shepard completed his dissertation and received Michigan's first doctorate in psychology. The same year he received an appointment as an instructor. Another graduate student at that time was a young lady from Marshall, Michigan, Florence Berenice Barnes, who was interested in abnormal psychology. Miss Barnes had earned her master's degree in that area in 1902. Shepard and Miss Barnes were married on August 19, 1909.

John F. Shepard, 1922

Shepard became Pillsbury's junior colleague in 1906 and for half a dozen years these two men made up the entire psychology staff. They differed in many personal ways. The sharpest differences were in the very areas of personality that prevented students from being drawn to the senior professor. Shepard was very willing to spend time with students and went out of his way to encourage a degree of intimacy. He shared with them his opinions, knowledge, and anecdotes on any and all topics. Shepard was a man of extremely liberal convictions on social and political issues. This liberalism tended to attract to him many of the young social scientists with whom he shared this social idealism.

It was as a teacher, however, that Shepard had this greatest effect upon students. This is not to say that all reacted favorably to him. But none were ever neutral about him and his type of psychology.

Those men who were to consider themselves to be disciples found him to be an extremely systematic, original, critical-minded thinker. More than one former student has expressed the opinion that if Shepard had published the material he presented in his courses, he would have founded a school of psychology. Shepard taught well-organized courses with a great deal of

confidence. He knew the literature so much better than the students that they stood in awe of his grasp of psychological subjects. These students soon came to believe that what Shepard said about maze learning, reasoning, perception, etc., was correct and what others said was incorrect.

Shepard's approach to psychology was very operational and not merely verbal manipulation. He could and would pick out the many neglected controls in the experiments that dominated the literature and convinced his listeners that he was one experimenter who knew what precautions to take. For Shepard was very much at home in the laboratory and accumulated vast amounts of data from rat runs in his various mazes. But nothing much ever came from them.

Shepard, then, was a dominant, driving, intelligent person who was interested in research and in communicating to the students what for him were very valid theories of learning and perception. His traits were the kind, however, which do not universally lead to a band of devoted disciples. They often caused reactions in some people which were less than positive. Some of his colleagues did not find it easy to work with him. To some he appeared as a dogmatic, stubborn person who had difficulty admitting that anyone other than himself could conceivably know anything about psychology.

Even his strong points, curiously enough, had the effect on his students of often stifling creativity and productivity. Shepard appeared as such a severe and cogent critic in his classes that his students seemed to hesitate doing anything on their own for fear of receiving in turn the unsparing criticism that they observed Shepard directing toward the work of others. And then again, Shepard worked so hard and so long in his laboratory and accumulated huge amounts of data which were never published. Some of his students had visions of themselves working just as hard and as long and getting no further than he did.

For the tragedy of the man was that he did not publish any of his life work. Shepard always placed the blame for this failure on his heavy involvement in the campus buildings program.

Perhaps the years with the building program did leave little time for writing. But there were twenty-five years between the end of that involvement and his retirement. But still the publications did not come. Instead he and his student assistants continued to run animals and collect data. He was always in the laboratory working long hours. There was always something else to try – some other variable to control in order to obtain the answers he wanted to the problems he set.

Individuals' reasons for not publishing are always complex and different. For some, publishing is too public an exposure to be endured. This was probably not Professor Shepard's problem. It may have been, simply, that his teaching actually became a report of his research. A glance at his notes for his advanced systematic course clearly reveals this fact. If he had published this work, there would have been nothing new to present to his classes.

Shepard used this material with great effect. The students learned a great deal by going through with him the successive research steps. It generated much enthusiasm and respect and created a large group of student-disciples. However, it was not the way to develop a national reputation nor to receive reactions to ideas from professional colleagues.

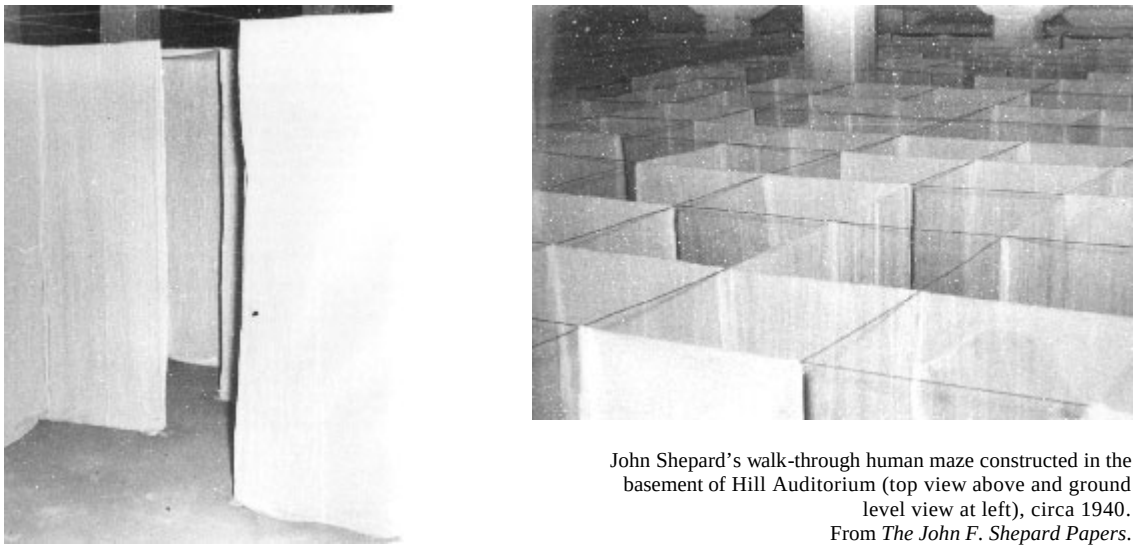
In the early twenties, Burton Thuma went to Stanford University after having taken some courses with Shepard. He took along the "Shepard point of view," the truth of which he had been convinced. Thuma was shocked to learn that Calvin Stone and the other Stanford psychologists

were not certain who Shepard was. Norman Maier had the same experience at the University of Chicago.[17]

Wherever Shepard's students went, they would talk about him and his work and what it would do to psychological theorizing when it was published. It was always "work that was about to appear."

Shepard did take a sabbatical leave during the spring semester in 1929. It was his first such leave and hopes were high that he would now complete some written reports. Shepard took a trip to England, ran through the actual Hampton Court maze, the order of which he previously learned, and published nothing.

If Shepard's failure to publish caused disappointment and embarrassment to his students, it must be said that it had a profoundly more negative effect on the man himself. Toward the end of his life, in his retirement, he worked steadily on his data and attempted a comprehensive report on his life of research. One monograph resulted which was published at his own expense. But the larger work never did go well. Professor Shepard appeared to sense the outcome and was quite concerned about it. In 1960, he wrote E. G. Boring that his "only real source of anxiety now is the realization that much of my life would be lost if I don't get my maze results published."[18]



John Shepard's walk-through human maze constructed in the basement of Hill Auditorium (top view above and ground level view at left), circa 1940.
From *The John F. Shepard Papers*.

II

The only systematic record of Shepard's research and theorizing that is available remains the digests of the lecture notes that he had generations of graduate students prepare as a requirement for his advanced systematic course. One exceptionally well-prepared set of these

[17] Maier did succeed in bringing Shepard to the University of Chicago to give a lecture on his maze work. Shepard gave his usual well-organized performance, which was well-received by the Chicago audience.
[18] Letter from John F. Shepard to Edwin G. Boring, September 3, 1960; copy in *The Shepard Papers*.

notes has been duplicated and distributed to limited numbers of former students and libraries.[19] Some insight into the nature of Shepard's theorizing, however, may be obtained from a consideration of one of his better known studies.

In 1913, Shepard with H. M. Fogelsanger as co-author published an article entitled "Studies in Association and Inhibition."[20] This study, which Shepard had begun about 1909, is of special interest because it emphasized the same kind of stimulus patterning effect that Max Wertheimer, Wolfgang Kohler and Kurt Koffka were observing simultaneously in Germany and were eventually to develop into Gestalt Theory. Kohler, as a matter of fact, considered the Shepard-Fogelsanger work the only instance of early American research that fit in with the Gestalt point of view.

In their experiment, Shepard and Fogelsanger were arguing from the implications of Dewey's article, "The Reflex Arc Concept in Psychology."[21] This classic is typically cited as marking the beginning of Functionalism, as indeed it was. In it, Dewey attacked the analytic British psychology that attempted to dissect experience and place its parts in convenient pigeon holes. Dewey, interested in emphasizing the complexities of mental life, made two points. First he maintained that psychology must be interested in total coordinations which are not properly reduced to a sum of consistent stimulus and reflex arcs. He believed that the reflex must be thought of as an indivisible coordination between a stimulus and a response.

Second, the reflex conceived of as such a coordination should be viewed as an instrument for affecting successful adaptive behavior in relation to the conditions of the individual and his environment.

It is a matter of history that it was Dewey's second point stressing the adaptive function of behavior that impressed James R. Angell, Harvey A. Carr and others leading to their formulation of Functionalism. His first point can be seen, with the wisdom of fifty years of hindsight, to be an early anticipation of the basic principle of Gestalt psychology. Though both points were essential to Dewey's argument, only his stress on the adaptive consequences of behavior impressed his American contemporaries. Shepard, however, was impressed with Dewey's stress on the indivisibility of a stimulus-response connection and translated the principle into an experimental implication.

At about that time (1905), two German psychologists, G. E. Muller and A. Pilzecker, had developed a theory of association and inhibition which was based upon a psychological elementalism and cited the following type of evidence to support their views: using a paired-associate learning situation, subjects were presented with stimulus "A" to which they learned to respond with syllable "C." Later in the training, stimulus "A" was presented again with the response changed to syllable "D." Their results, when a test was given in which stimulus "A" was presented alone, revealed greater inhibition of the responses when compared with other stimuli that had been trained to a single response.

---

[19] Shepard, John F. *Lecture Notes from a Course in Systematic Psychology*. Prepared by Seymour Wapner. 2 vol., 1939-1940. Mimeographed copies available through the Department of Psychology, University of Michigan, Ann Arbor.

[20] Shepard, John F. and Fogelsanger, H. M., "Studies in Association and Inhibition." *Psychological Review*, 1913, 20, 290-311.

[21] Dewey, John. "The Reflex Arc Concept in Psychology," *Psychological Review*, 1896, 3, 357-370.

Muller and Pilzecker interpreted their results in terms of transition associationism, offering their special version in the form of a "drainage theory." The analogy, they argued, may be made to a water reservoir, where when water is removed by opening one connection, it drains energy from other connections. The nervous system, during learning, acts in the same manner. If one connection, "A-C" is set up as a path of lowered resistance between "A" (sensory) and "C" (motor response) and then another association "A-D" is set up, then when "A" is presented, there will be a division of energy between "C" and "D." If the energy goes from "A" to "C," it inhibits the other connection "A-D." If the energy is divided between the two connections, the responses "C" and "D" will inhibit each other because of the fact that neither will receive amounts sufficient to cause a response.

Shepard believed that Dewey's position as put forth in the reflex arc article offered an alternative interpretation of Muller and Pilzecker's inhibition effect. Dewey had made the point that a stimulus-response connection must be thought of as an indivisible unit or pattern, and not as separate elements having one or more connections with other elements. Shepard interpreted this to mean that what was learned was a pattern, not molecular-connections, and conceived of the following experiment which could decide between the "drainage theory" and patterning learning.

Supposed that instead of having subjects learn that stimulus "A" leads to response "C" and "D," the learning task is reversed by having the subjects learn that stimulus "C" leads to response "A" and stimulus "D" also leads to response "A." Then, if stimulus "C" and "D" were presented together there should be no interference with the evocation of response "A" according to Muller and Pilzecker's drainage theory. As a matter of fact, since both stimuli have connections which lead to the same response, the drainage theory should predict an enhancement effects, i.e., "A" should occur more rapidly when preceded by both stimuli than when preceded by either one singly. If, on the other hand, inhibition was obtained, it would be difficult for the drainage theory to handle the results. No connection would have been formed which interfered with either of the singly-established associations.

In his study with Fogelsanger, Shepard ran three subjects through seven series of paired learning tests in which they learned variations of the paradigm described above. The test was to expose both of the key stimuli ("C" and "D") simultaneously. If the "drainage theory" was correct, the two connections established during the acquisition phase leading to the same response, should reinforce each other and lead to quicker reaction times than either stimulus by itself. However, the results showed that the double-stimuli presentations had longer reaction times than the single-stimulus presentation. Shepard and Fogelsanger conclude:

> It seems impossible to understand these results except by the assumption of a form of inhibition of one association by another, which cannot be brought under the ideas of drainage or division of energy. This means that an association cannot be explained as a mere path of lowered resistance, which is the usual statement. An association involves other processes, which prevent any other stimulus from using the same neurons at the same time, which check it if there is any variation or incompleteness in the recalling process, and which block any other association that is

> tending to operate at the same time, even though both lead to the same result.[22]

Muller attempted to re-interpret the Shepard-Fogelsanger results in line with this theory by assigning the individual associations to separate cortical centers. Kohler, however, severely criticized Muller's attempt and claimed that only Gestalt theory could satisfactorily handle the Michigan results. The inhibition, he claimed, was merely the result of conflicting organizations.[23]

Shepard's theory of behavior appears to have been an extrapolation of the implications of this early experiment. He viewed learning as an acquisition of responses to a patterning of stimuli. To this extent, his views fit in well with what the Gestaltists were saying. Shepard, in turn, was enthusiastic about this new system when he first began to hear about it in the mid-twenties. It must be remembered, however, that in those years not very much was known about Gestalt psychology. To be enthusiastic about what the system might be saying, therefore, was not the same thing as identifying oneself with it.

As a matter of fact, Shepard never called himself a Gestaltist and in later years appeared to have serious doubts about it. What seemed to trouble him was not its basic thesis concerning the nature of organization but rather its ancillary principles which stressed the innateness of organization, the Gestalt principles of organization, isomorphism, etc. Shepard placed his stress on the role of experience in the development of organization.

A better-known example of what this writer feels to be the Shepard theory of stimulus patterning may be seen in Clark Hull's concept of afferent neural interaction. Hull's stimulus-response reinforcement behavior theory had often been criticized for its neglect of the stimulus complexities involved in behavior. These criticisms were most often made by the Gestaltists. Hull's answer to his critics was to postulate a process, whereby any incoming stimulus interacts with all stimulation active "inside" the organism at the time the stimulus is presented as well as all other stimuli that have been learned that are on a stimulus generalization continuum with the stimulus.

It is of some significance that when Hull presented his concept of afferent neural interaction in his 1943 book, *Principles of Behavior*, he cites the Shepard and Fogelsanger results as support.[24] Hull was a student of Shepard's at Michigan from 1914 to 1916 and was certain to have been familiar with Shepard's handling of stimulus patterning. It is this writer's belief that if Hull had developed further the implications of afferent neural interaction, he would have been presenting a theory of perception that would have been very close to Shepard's theory of stimulus patterning.

The fact that Hull's Gestalt critics failed to be mollified by the implications of this concept reflects the essential difference that existed between Shepard and the Gestalt view of perception and the role of learning in behavior.

Shepard's view as expressed in his course in advanced systematic psychology, however, did come close to the Gestalt position on many topics so that his students were prepared for or at least inclined by their Michigan experience toward what the Gestaltists were saying. Several of

---

[22] Shepard and Fogelsanger, ibid, p. 310.
[23] Kohler, Wolfgang, *Gestalt Psychology*, New York, London, 1929, p. 315-316. See also Koffka, Kurt, *Principles of Gestalt Psychology*, New York, 1935, pp. 602-603.
[24] Hull, Clark L. *Principles of Behavior*, New York, 1943, p. 219.

his students (Maier, Colby, Woodburne) were encouraged by him to go to Berlin to learn first hand about the new Gestalt ideas.

III

As indicated earlier, during 1912-1915 Shepard played a pivotal role in the construction of the Natural Science Building.[25] He acted as liaison between the architect and contractor on one hand and the University and faculty on the other. In 1916, when the state legislature appropriated money for a new general library, the University again called on him to play the same role.

Shepard was appointed superintendent of construction and worked closely with Albert Kahn, the architect, and William Warner Bishop, the librarian. In May 1916, Bishop and Shepard toured Albany, Worcester, Boston and other eastern cities inspecting various libraries. When the First World War broke out, construction ceased and Shepard went to work for the Army in Washington.[26]

When he returned from his war work, Shepard rejoined the library project even to the extent of aiding in the interior furnishings. When the library was dedicated on January 7, 1920, Shepard's contributions were generally acknowledged. In his dedication address, Librarian Bishop paid the following tribute to Shepard:

> For four years Professor Shepard and the library have worked together on the library, first its plans, later the details of construction and lastly the furnishing and equipment. Professor Shepard's well-known skill in experimenting, his experience in constructing the Natural Science Building, his acquaintance with the architect force pointed him out as the logical man to do this. But what devotion he has shown to it, what sacrifice of his own leisure and comfort, what even greater sacrifice of his time from work in his laboratory no one can know save perhaps Mrs. Shepard and the present speaker. The debt which the University library owes Dr. Shepard is impossible to comprehend. He has met emergencies with skill, difficulties with determination, drudgery with patience and has reached the Scriptural heights of suffering fools gladly. I have but one regret in recalling the four years of constant association. I am uncomfortably conscious of what precious hours he has freely taken from the pursuit of his own research to make research in the library possible for others.[27]

If Bishop felt somewhat guilty about the years Shepard lost from the laboratory, he must have experienced relief by subsequent developments. President Hutchins retired in 1920 and was succeeded by Marvin Leroy Burton. The University under President Burton's leadership undertook a large scale building program.

---

[25] See Volume One, Chapter 2.
[26] See Volume One, Chapter 2.
[27] Speech at the Dedication of the General Library by William Warner Bishop, University Librarian, January 7, 1920, in *Michigan Alumnus*, 1920, p. 260.

In the winter of 1920-1921, the state legislature appropriated five and a half million dollars for building purposes. The direction of the program was placed in charge of the so-called "Committee of Five" which included President Burton, Secretary of the University Shirley Smith, Regent W. L. Clements, architect Albert Kahn (ex officio), and Shepard who was chosen to act as Supervisor of Plans. His duties were to see that the new buildings would adequately meet their educational purposes. The committee's operating procedure was to appoint a subcommittee of faculty for each new building to initiate the ideas that would define the function of the building. All contracts with the architect and contractors, however, were to go through Shepard, who was an ex officio member of each subcommittee.

Under this program the University constructed the University High School, East Engineering Building, Angell Hall, East Physics Buildings, as well as the completion of University Hospital and the Dental School. From 1921 to 1925, Shepard was released from most of his teaching responsibilities. He taught only one course a semester – advanced systematic in the fall and advanced comparative in the spring.

By 1923 the building program was near completion. The University High School and East Engineering were finished in 1923 and Angell Hall was ready for occupancy by the end of 1924. In anticipation of the program's completion, the Committee of Five planned to break up its organization in time for Shepard to resume his teaching and departmental responsibilities by fall of 1924. But for the next two years he continued to be involved in the completing operations. He contributed to the preparation of a chronological report of all the building operations along with a record of their step by step progress. He also was involved in a University committee that met with the city authorities to work out a cooperative plan to beautify the campus and its surroundings.

But despite the apparent end of the building program, Shepard's return to his academic responsibilities in September 1925 came as a surprise to him. President Burton died on February 18, 1925 and Clarence Little was appointed to the presidency nine months later. During the interim, Alfred Lloyd, an old colleague of Shepard's in the Department of Philosophy was acting president. Shepard was suddenly dropped from his position and "allowed" to resume his professorial duties.

The reasons for Shepard's sudden release from the building program were never clear. His position was an important one that involved making many crucial decisions that affected various departments. It is difficult to believe that he could have performed as efficiently as he did without stepping on a good many toes. Shepard was not an especially diplomatic person and was quite direct in saying what he believed to be right. It is probable that among those persons who came to power after President Burton's death, there were those who were not favorably inclined toward what Shepard had been doing. As a result, the fall semester of 1925 found him resuming his teaching and research duties.

Shepard appeared happy to be back at his professorial work. It is hard, however, to imagine that he accepted his release without some degree of ambivalence. He must have certainly enjoyed his administrative assignment. No man with Shepard's dominant and self-assured nature would have meekly accepted an assignment that took him away from his own work for at least eight years unless there was a great deal of personal gratification involved.

But Shepard did feel his first obligation was to this profession, and his friends on other campuses would not let him forget it. Karl Dallenbach wrote him in December 1925 that he was glad to hear. . .

> . . .that you [are] now giving your full time to psychology. I always thought it a shame that you have been distracted to your work in the supervisor's office. I welcome you back to the fold. I don't think in your heart you were ever very far away. . .[28]

Shepard replied:

> I am thankful that I have little to do on building since September so that most of my time goes to psychology. I am literally [?], up trying to catch up with the reading I haven't been doing during the last few years. But I'm having the best time of my life doing it.[29]

IV

No biographical sketch of Professor Shepard would be complete without some mention of his political and social philosophy. Shepard brought to these topics, views that were held and put forth with the same dominance, confidence and determination that characterized his approach to psychology.

The most conservative way to describe Shepard's social-political philosophy is to say that it was slightly to the left of socialism. He was a great admirer of Norman Thomas and a great champion of society's underdogs. For many years, he had been a strong supporter of the Soviet Union and had influenced many of the graduate students during the twenties and thirties with his discussions and advocacy of the many social experiments that were being conducted in Russia.

This writer had many talks with Professor Shepard during the years after his retirement. Often these conversations were on political topics. Shepard would say that though his vocation was psychology, his avocation was American history. He believed wholeheartedly in the principles of freedom, liberty and equality upon which this country was based. But the economic system which allowed the opportunity for these values to be experienced at the country's inception no longer was appropriate to our times. In the days of the open frontier, the free-enterprise system fostered the pursuit of liberty and freedom. But the frontier had since closed. Since freedom and liberty depended on equal opportunity, steps must be taken to ensure that the condition exists. Thus, in almost classic Marxism form, Shepard felt that in order to keep our country directed toward the values upon which it was founded the society must be altered toward a socialistic state. Given the situation that exists today, only socialism can assure the realization of the truly American dream.

Shepard held this position most tenaciously and continued to support all movements and events that were consistent with it regardless of the circumstances. When the Russian government, for example, signed the non-aggression pact with Germany in 1939, many of the young "liberals" in the department were quite disappointed and disillusioned with the Soviet "experiment." But Shepard defended Russia's action as a necessary political expedient and presented a position in defense of the Soviet Union that was too radical for most of the students.

In 1942, Shepard was president of the Civil Rights Federation of Detroit which distributed pamphlets entitled "Smash Detroit's Fifth Column." The document was a detailed

---

[28] Letter from Karl Dallenbach to John F. Shepard, December 1, 1925, in *The Shepard Papers*.
[29] Letter from John F. Shepard to Karl Dallenbach, December 10, 1925, copy in *The Shepard Papers.*

attack on rightist organizations operating in Michigan as well as elsewhere. Chapters with quoted excerpts from their publications were devoted to Father Coughlin, Gerald L. K. Smith, The Ku Klux Klan, the National Workers League and the Silver Shirts. Gerald L. K. Smith was so disturbed by the pamphlet that he telephoned and wrote President Ruthven complaining about Shepard in the following manner:

> Professor Shepard always seems available whenever the Communist Party line wants to put over something in Detroit. . . It is indeed unfortunate that a professor at the University of Michigan allows himself to be used by such ruthless propagandists. . .
>
> Please understand that I am not asking you to take any steps or any action that would in any way menace academic freedom in this nation where freedom is becoming altogether too scarce. I would rather be attacked by Professor Shepard than to destroy the liberty which makes it possible for him to attack me.
>
> The real purpose of my call and the real purpose of this letter is to point out that in attempting to market this book, its promoters are making capital of the fact that Shepard is associated with the University of Michigan. I think you should stand ready to release a statement to the press or take whatever steps may be necessary to make it clear that Professor Shepard does not speak for the University of Michigan.[30]

It was generally agreed that Shepard did not talk politics around the department to any large extent. It was true that he occasionally had discussions with his graduate students. But Professor Henry Adams could not remember ever hearing him declare himself on political issues nor did he ever hear of any criticism of Shepard on that issue. Pillsbury, who was quite conservative politically, did not appear to be concerned about Shepard's views.

Shepard did use his well-attended one-credit course in the psychology of religion as a platform for his socialistic philosophy. When this writer attended that course in the late forties, he heard little of religion per se and much of the inevitability of the socialistic revolution that was coming after the phase of the "phony revolution" that was fascism had passed. He also ran for Mayor of Ann Arbor in 1948 on Henry Wallace's Progressive Party ticket.

Left-wing student organizations almost always approached Shepard to be their faculty advisor. He very frequently agreed and often was used by some of the young people for their own personal rather than idealistic motives. Shepard appeared well aware of what was going on, but felt that the ideals expressed were the important thing and would, if kept active, eventually dominate as a true value rather than a means to some immature, personal achievement.

---

[30] Letter from Gerald L. K. Smith to President Alexander Ruthven, June 18, 1942, copy in *The Kraus Papers*. No action appears to have been taken against Professor Shepard. In his defense, it must be said that although he was identified in the pamphlet as "Professor John Shepard," no mention was made of his University of Michigan affiliation. It should be noted that a copy of the Smith letter and pamphlet has remained in Professor Shepard's file in *The Kraus Papers.*

Shepard had already retired when the McCarthy-era of Communist "witch hunting" reached Michigan. In the spring of 1954, Congressman Kit Clardy of Michigan, a member of the House Subcommittee on Un-American Activities, decided to hold a one-man hearing in Lansing. He notified the University that three faculty members would be called before him and questioned about their one-time communist associations. He also announced to the press that he had anticipated calling a fourth faculty member but had learned that this person had recently retired and was no longer in a position to influence students. Rep. Clardy felt, therefore, that there was no longer any necessity to investigate him.

The reference was obviously to Shepard. When asked about Clardy's statements, Shepard expressed, in rather strong language, a wish to have been called to the hearing in order to state in no uncertain terms, his feelings about the activities of the Un-American Activities Committee.

V

Professor Shepard was almost sixty-five when the department was reorganized under the chairmanship of Donald G. Marquis. No one is quite certain about how he took being passed over for the chairmanship. He never discussed his feelings with anyone nor did he ever in even the smallest way complain about what had happened. It is a safe guess, however, to say that he was very disappointed. It is reasonably certain that he expected to receive the appointment. It had been his department for many years despite Pillsbury's title and he, along with others both in and outside of the University, believed that his contributions to the University would be rewarded with the chairmanship.

As indicated in the earlier discussion of the chairmanship selection, it appeared that there were very real reasons for the College administration to select a chairman from outside the department.[31]

Psychology at Michigan had failed to keep abreast of new developments in the field. A departmental redirection was needed. Personal factors possibly played some role in the decision. Professor Shepard's politics, his years in the building program, his membership on the Literary College Executive Committee from 1936-1939 may have resulted in his acquiring some influential antagonists in the University. It is difficult to see how such a dominant, confidently dogmatic personality like Professor Shepard could have occupied such positions without stepping on at least a few important sets of toes.

Shepard never discussed his disappointment. But the older staff members sensed it from his behavior. He simply began to withdraw. He would agree quite complacently to the changes that were proposed to him which in effect reduced his sphere of influence. His permission was always requested, but the requests were put in such a manner that he could not refuse. In the end, he simply withdrew.

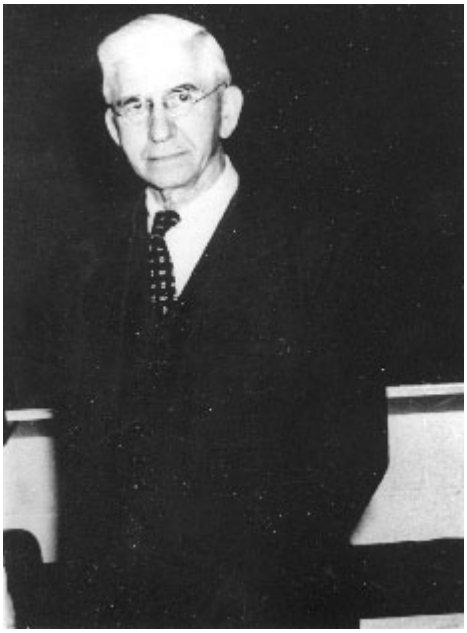
Shepard retired in 1951 and the task of writing up his life of research became almost an obsession with him. It was constantly on his mind and was interjected into almost every conversation he had with former colleagues and students.

In 1959, he published a monograph entitled, "An Unexpected Cue in Maze Learning." He considered this monograph the only portion of the main study that was detachable. Shepard contacted Norman Munn, then editor of the *Psychological Monograph*

[31] See Volume One, Chapter Six.

> . . . frankly, I do not expect many readers to be concerned about this study until after the next monograph is out. However, I think there would be a few: some oldsters who still remember a little about the maze; and a few others curious enough to start reading.
>
> Lastly, I do not want you to bother anyone else with the matter. If, after you have read the manuscript, you have any doubts as to the advisability of publishing it, for any reason, please return the manuscript to me. I shall lithoprint it and let the Michigan department distribute it. It would be of use to the department in more than one way.[32]

Munn turned the monograph down since he believed that Shepard's contribution on the floor cues had been known for a long time and all the details included in the monograph appeared to be anticlimactic.[33] Shepard had the work lithoprinted at his own expense. Shepard was contracted by a publishing firm in Berlin to write a chapter summarizing the research on maze learning for a tentative publication, *Handbook of Zoology*. He accepted the assignment since it would fit in well with his plans for his own major opus. Shepard worked over a year on the chapter and completed a thirty-seven page review. He submitted it to the firm in English, and they sent back the German translation for him to correct. There appeared to have been some misunderstanding at that point, and Shepard withdrew his chapter. It was never published.



John F. Shepard, taken at the presentation of the first Walter B. Pillsbury Undergraduate Award, 1954

Professor Shepard continued writing and would occasionally request departmental secretarial services to type some completed text. But the years were catching up with him. In 1963, he had a slight stroke which affected his speech. Old friends who visited him began to detect the inevitable speech repetitions and perseverations. A year later Professor Shepard entered a nursing home. On November 2, 1965, at the age of eighty-four John F. Shepard died. His last draft of research writings were gathered up and examined by some former students. It was regretfully decided that there was nothing which might be salvaged for publication.

[32] Letter from John F. Shepard to Norman L. Munn; March 2, 1959, in *The Shepard Papers*.
[33] Letter from Norman L. Munn to John F. Shepard; March 10, 1959, in *The Shepard Papers*.

CHAPTER SIX

Henry Foster Adams

(b. 1882)

Henry F. Adams was born on November 11, 1882, in Oak Park, Illinois. His father was a Methodist minister and his mother a physician. The family moved to Ann Arbor when Henry was two years old remained there until his sister, Anna, graduated from the University in 1890. They then moved to Clifton Springs, New York, which became Henry's home until he went to college. A small hundred-fifty bed mental institution was located in Clifton Springs and young Henry worked there doing the kind of things a young man could do – running the elevator, acting as an orderly, etc. After attending the Casanovia Seminary, he went on to Wesleyan University for his undergraduate training.

At Wesleyan, Adams learned his psychology from Raymond Dodge, who had trained at Wurzburg under Benno Erdmann. Dodge's type of psychology was eclectic and followed no particular school. Adams learned his Wundt, Ebbinghaus and Titchener and performed the laboratory experiments that were usual in those days. Upon graduating in 1905, he decided to continue his training in psychology.

Henry F. Adams

The elder Adams, however, felt that the twenty-three year old youth was not yet mature enough to decide on a profession and suggested that he work for two years before beginning his graduate training. He asked Adams what he would like to do during that period, and Adams answered that he did like to write and would enjoy any experience along that line. A patient at the institution was the wife of an editor with a New York newspaper and through him, Adams secured a position on the paper. He kept the position for a year and enjoyed the work. But he still wanted to go on to graduate school.

The agreement Adams had with his father was for two years of work experience and there was still another year to go. Since he also enjoyed working with his hands, his brother obtained a position for him in Chicago as an assistant foreman with the Chicago Screw Company.

Adams held this position for nine months. In April 1907, he entered the University of Chicago to study psychology. While there, he had courses with James H. Tufts, George A. Mead, G. Moore and James R. Angell and became well acquainted with other graduate students and junior staff members like John B. Watson, Joseph Hayes, Walter Hunter, and Harvey Carr. He became especially friendly with Carr, who later directed Adams' dissertation.

During the spring of 1910, Adams took a semester's leave to accept a position at the University of Kansas, replacing an instructor who had become ill. When he returned to Chicago,

Angell, who was the departmental chairman, gave him an assistantship but also informed him that it was time for him to take his degree and get on with his career. Adams completed his dissertation with Carr doing a detailed study of the autokenetic effect. This research is still referred to whenever research on this phenomenon is reviewed.[34] Adams remained at Chicago for another year before he heard through Angell of the opening at Michigan.

Michigan, it appeared, needed someone to assist Shepard in the laboratory, and since Adams had considerable experience working with Watson and Carr as their laboratory assistant the match appeared ideal. Adams accepted the Michigan position at an annual salary of $1,000.

Adams arrived for the 1911-1912 academic year. During his first year, he handled quiz sections for the introductory courses and assisted Shepard in the laboratory. The next year, both Pillsbury and Shepard inquired if he would prefer to do an independent course. Adams indicated he would like that very much but wondered what was available. Pillsbury was offering the introductory, abnormal, history, and language courses and Shepard was in charge of the laboratory as well as the work in comparative, systematic, and psychophysical methods. Neither man appeared willing to give up any of their offerings. They were covering all the traditional subjects for which Adams had been trained.

Adams, however, had had the earlier experience in journalism and manufacturing. Drawing upon these experiences, he proposed two new courses, one in the psychology of advertising and another in salesmanship. In later years, he also developed advanced courses in each as well as courses in management.

In this somewhat indirect manner, applied psychology gained an early start in the psychology curriculum. Adams devoted the rest of his career at Michigan to advancing this specialty. There is no question that he would have preferred, in those early days, to have been "allowed" to offer more experimentally oriented courses. But none were available to him. The only way for him to enjoy an alternative to handling quiz sections for the introductory course was to develop a new area. His previous experience provided him with nothing other than the applied subjects.

This decision to move into the applied field was to be a decision determiner of the relationship that Adams would have with the rest of the department during his remaining years in Ann Arbor. The department continued in its tradition of experimental-physiological psychology and showed very little interest or sympathy for the direction to which Adams had turned. As he worked to develop his specialty, Adams received almost no encouragement from the department. Occasionally another staff member (for example, Ford, Greene, Griffitts) would show some interest in his work or join him in offering a supplementary course. But for the most part, he worked alone. His solitary efforts led to his personal as well as intellectual isolation from the mainstream of departmental activities.

It must be admitted that there was considerable antipathy between Shepard and Adams. It was, however, a quiet kind of feud that did not in any way disrupt the department. Adams had little desire for having graduate students work with him and was more interested in undergraduate rather than graduate courses. So the feelings of the two men never affected the main departmental program.

---

[34] See, for example, Royce, J. R., Cannon, A. B., Aftonas, M., Lehman, R. S., and Blumenthal, A.; "The Autokenetic Effect: A Critical Review," *Psychological Bulletin*, 65, 1966, p. 253.

The antagonism between them appeared to have become established early. One member recalled that is was obviously present when he arrived in 1915. Shepard did not think too highly of Adams' professional competency and of course did not believe that applied psychology was very good psychology. It seems, in retrospect, somewhat ironic that Adams would have had his specialty held against him, for it appeared obvious that Pillsbury and Shepard left Adams very few alternatives by their refusal to turn over to him some of their assignments. It is true, however, that Adams' personality tended to be somewhat cynical and jocular, and these traits were certain to antagonize the rather somber and domineering Shepard.

Shepard did not bother to conceal his opinions of Adams and would talk about him to the graduate students. Since these students were rather evenly split between Pillsbury and Shepard, a good many came to take as their own Shepard's negative attitude toward Adams. In later years – after both Shepard and Adams had retired – many of these men came to believe that Shepard had overdrawn the picture of Adams he presented, and that Adams' isolation had not been quite fair.

For his part, Adams appeared rather stoical about what had happened to him. He never quite knew where he stood with the departmental members. He believed that some of them liked him as a person but did not care for him as a professional. Consequently, he went his own way, contributed very little to the department and did not concern himself with its opinions.

Around 1925, space began to be a problem in the Natural Science Building and psychology was offered some additional room in the Pharmacology Building. Adams moved into the new space, thus isolating himself physically from the rest of the department. About ten years later, new space was offered to the department in the West Medical Building. Again, Adams was moved. He remained in these rooms until his retirement in 1952. He was joined there for several years by Edward B. Greene. Several times during the forties, Adams requested space in the Natural Science Building but was never allowed to return.

Adams did not allow his isolation to keep him from having an active professional career. He published many articles as well as one book on his specialty. In many little ways, he did contribute to the instructional program and research activities.

Adams was the first to suggest that the students in the introductory courses should be allowed to substitute six hours as an experimental subject in research work in lieu of writing one of three required term papers. In addition to originating the course work in advertising and selling, he gave the first departmental course in personality. Adams also started the procedure of allowing undergraduate students to register for two or three hours of original laboratory work under the supervision of a staff member.

His own research activities seemed to involve problems that were of personal interest rather than being of any theoretical importance. One of his students taking the undergraduate course in research was given as a problem the investigation of accuracy in putting in golf. In one half of the trials, the subjects established a line between the ball and the cup, adjusted his putter at right angles to this line and stroked the ball while still looking at the ball. In the other half of the trials, the subjects adjusted the putter, then looked at the cup and shot. It was found that more accuracy was obtained by the second procedure.

At one time Adams was interested in Spearman's theory of the "g" and "s" factor theory of intelligence and could not believe that vanishing tetrads in two or more different tests proved that both measured intelligence. He tried to find a test of skill that would permit some kind of correlation between tests on players. He selected golf and obtained the hole by hole scores of

different players on the same course and intercorrelated them. The table of the correlations yielded vanishing tetrads and so fulfilled the requirements of Spearman's theory.

Adams used the data from this golf experiment to determine whether the relationship between the average number of strokes per hold and the distance covered on a hole was linear or curvilinear. It turned out to be a linear relationship. He had hopes of being able to develop a "golf quotient" analogous to the "intelligence quotient," but the lines for the different players had different points of origin and so there was no common zero point. These results were published.

Another study required subjects to draw a line on a sheet of paper that indicated the direction of some well-known campus or town building from the point at which the subject was seated. The average directional error was 7.5 degrees.

Once he had the students in his personality course work with volunteers who, in groups of ten males or females, agreed to rate themselves and the other members of the group on a set of personality traits. The results were published in an article entitled "How We Judge Ourselves and Our Fellow Man" and printed in a book edited by Albert E. Wiggam entitled, *Exploring Your Mind With The Psychologists* (1928).

In this somewhat isolated but active manner, Adams spent forty-two years in the department. During that time he had only four doctoral students. These four were students who the rest of the staff did not seem to know what to do with. One was the white Russian aristocratic émigré, Skitsky, who did a thesis (1940) which appeared to be too abstract to be comprehended by anyone in the department. Another was Tooi Xoomsai (1941) from Bangkok, Thailand. Phyllis E. D. Swann was interested in stuttering and since no one on the staff cared for the topic, Adams drew the assignment as her chairman. However, the year (1932) that she was finishing her work coincided with his sabbatical leave which was spent in Europe. Pillsbury replaced him as chairman. Herman H. Long, one of the department's few black students became Adams' last doctoral student in 1949 when he completed a dissertation on the individual differences among American minority groups.

Adams became an assistant professor in 1918, an associate professor in 1920, and received his promotion to professor in 1927. His service in the department terminated after forty-two years when he retired in 1952.

CHAPTER SEVEN

Sven Froeberg[35]

(1880-1966)

Sven Froeberg was born on February 26, 1880 and attended undergraduate school at Bethany College (Kansas) graduating in 1903. He received his graduate training at Columbia University under James McKeen Cattell, Robert S. Woodsworth and Edward L. Thorndike, completing his doctorate in 1908. He had been teaching at Upsala College in East Orange, New Jersey when in 1915, he received the offer to come to Michigan as an instructor. Froeberg remained at Michigan only two years. During this time, he instructed in the quiz sections of the introductory courses, supervised laboratory work, and introduced a course in the methods of mental and social measurement.

In this latter course, Froeberg covered such topics as variable measurement, measurement of change, correlation, reliability, tabular, frequency, and graphic methods. He never felt it was a very successful course. It did not attract many students which may have been due, he believed, to the mathematics involved, the scarcity of material relevant to the subject, and the short time he had to organize the work.

Froeberg was described by his colleagues as a pleasant but rather quiet and unostentatious person who carried out his work effectively if not too dramatically. While at Michigan he carried out two minor studies, one on the effect of varying the interval between presentations in paired associate learning and the other on the motor effects of smoking.

Dr. Froeberg found Ann Arbor to be a pleasant place in which to work. He had come as an instructor after holding a professorial rank at Upsala College and consequently found the salary somewhat less than he could appreciate. So when an offer of an assistant professorship at Pennsylvania State College was made to him, he accepted it and left Michigan.

Subsequent years found him holding positions at the University of Utah (1920-1921) and Gustavus Adolphus in Minnesota where he remained for twenty-five years. After his retirement in 1945, Froeberg lived in Delano, Florida until his death on October 11, 1966.

[35] The personal details contained in this biographical sketch were contained in a letter to the writer from Professor Froeberg which was written a day before he died.

CHAPTER EIGHT

Charles Hurlbut Griffitts

(b. 1889)

Charles H. Griffitts was born on January 5, 1889 in Ozowkie, Kansas. He attended Campbell Academy (1906-1909) and College (1909-1913). After graduation, Griffitts began graduate work at the University of Kansas. While at Kansas, Griffitts studied under Floyd C. Dockeray, an early Shepard student, who often spoke highly of Michigan psychology. Upon receiving his master's degree in 1914, Griffitts accepted an instructorship at Parks College in Parksville, Missouri where he remained until 1916. That year, he accepted an assistantship at Michigan and began an association that lasted until his retirement in 1958.

In 1917, Griffitts was made an instructor to replace Sven Froeberg and to aid in the courses made vacant by Shepard's leave of absence to do war work. The following year he accepted a commission as first lieutenant in the Medical Research Laboratory of the United Air Service. At the war's end, the work wound up quickly and Griffitts returns to his instructorship in Ann Arbor. In 1919, he became the department's sixth PhD when he completed his dissertation, "Individual Differences in Imagery." Griffitts carried out this research under Pillsbury's supervision.

The twenties were the most productive years of Griffitts' professional career. Of the thirteen publications that comprise his bibliography, eight were written during this decade. The most significant item was a book, *The Fundamentals of Vocational Psychology,* which was brought out by the MacMillan Company in 1924.

Griffitts was never one of the more impressive teachers on the departmental staff. Though intelligent, he was never very effective in the classroom, and students – undergraduates and graduates alike – found it difficult to relate to him. His major interest was in the area of individual differences. During his army service, he had learned the galvanic skin response (G.S.R.) instrumentation and utilized this technique in his work. He conducted a great deal of research on the effects of fatigue on G.S.R., physical capacity, and strength. These studies lead to his interest in industrial and vocational psychology.

Griffitts had a related interest in the problem of instincts and drives and was particularly concerned with the possibility of separating out what was innate from what was acquired. In the thirties, he attempted to collect data on this problem by working with tropical fish. He would separate their eggs and raise groups under different environmental conditions. The results, however, were never published.

The most significant contribution made by Griffitts to Michigan psychology was his initiating role in the establishment of the Psychological Clinic. The Institute of Human Adjustment had been created in 1936 by the Mary A. Rackham Fund as a center devoted to all phases of rehabilitation – to teach the deaf to read lips, to teach the aged man vocations and avocations, to care for the indigent and expectant mothers, to rehabilitate the injured, etc. The Institute was administered by the Horace Rackham School of Graduate Studies, then under the deanship of Clarence S. Yoakum.

Dean Yoakum had been trained as a psychologist and had played a significant role in the development of the Army's psychological testing in the First World War. After the war, he had come to Michigan as a psychologist in the School of Business Administration. In subsequent years he entered the University administration where he directed the program of testing and evaluation of students. Yoakum became dean of the Graduate School in 1934.

Griffitts and Dean Yoakum were good friends and shared an interest in measuremental problems. Griffitts informally approached the dean as to the possibility of establishing a psychological clinic. Professor John H. Muyskens of the Department of Speech had already established a speech clinic as part of the Institute of Human Adjustment. Griffitts' idea was to start a clinic that would supplement the Speech Clinic by providing a diagnostic center as an adjunct service. It could also provide services to the community for dealing with vocational counseling, reading difficulties, the adjustment of superior children, the causes of abnormal behavior, the determination of capabilities and intelligence of children who were considered for adoption and problems of personality adjustment within the family.

Dean Yoakum approved the idea and authorized Griffitts to begin organizing the service while he convinced the Rackham Fund trustees of the soundness of the plan. Griffitts set up the clinic on the fourth floor of the Natural Science Building in late 1936. Wilma Donahue and Lela Bechtel (Kleemier) were his first clinical assistants. They began their work by seeing some of the children who were being treated at the Speech Clinic.

In January, 1937, the Trustees authorized the purchase of a house at 1027 East Huron Street and the next month, Griffitts and his staff moved in. The Psychological Clinic was now officially established. Griffitts became the clinic's first director and served in that capacity until 1944. Although he still taught courses in the department, part of his official appointment was now with the clinic. During this period, he worked out a comprehensive program of clinical study and counseling covering thousands of school children in Ann Arbor, Port Huron, Ypsilanti, and other Michigan towns.

The departmental staff did not work very closely with Griffitts on this project nor was there a particularly warm feeling toward the clinic. First, the department's experimental-physiological orientation was not sympathetic to clinical work. Second, there appeared to be some resentment toward Griffitts for having established the clinic without going through the departmental structure.

One of the main purposes of the clinic, however, was to provide a training facility for graduate students who were interested in clinical work. Students did come to work there through the direction of Dr. George Meyer who was offering courses in measurement. In the fall of 1939, Griffitts put together a master's program in clinical psychology which was approved by the department. Some of the early students who worked in the clinic were Roger Heyns, Irvin Berg, Stanley E. Wimberly, A. Dudley Roberts, William F. Holmes, Steward Armitage, Woodrow W. Morris, Robert W. Kleemier, and William Gilbert.

The circumstances of Griffitts resignation from the directorship were not happy. The creation of the clinic had been, as has been indicated, a very casual event due to a conversation between two friends. The relationship between the clinic, the Graduate School and the Institute had remained informal. The same was true with the Speech Clinic. Without any notification to either of the clinic directors, in February 1939, Dean Yoakum appointed Mr. Clark Tibbetts, an instructor in the Department of Sociology as director of the Institute. Griffitts and Muyskens were to deal with Tibbetts on all administrative matters and were not to bring them to Dean Yoakum.

Both directors were hurt and quite angered by the reorganization. Muyskens resigned the following fall but Griffitts remained as director for another five years.

Griffitts never became fully reconciled with the arrangement, however, and refused to deal directly with Tibbetts. Whenever matters had to be discussed, he would send Mrs. Donahue over to Tibbetts' office to act as his representative. The situation ultimately became intolerable for everyone and in October 1944, Griffitts resigned as director. Since Mrs. Donahue had been carrying on a considerable amount of the administrative work, she stayed on as ranking staff member without title.

Griffitts remained a somewhat isolated figure during his remaining years in the department. He had been promoted to assistant professor in 1922, associate professor in 1925, and professor in 1936. With this last promotion and the beginning of his association with the clinic, his research activities seemed to have ceased completely. His last publication occurred in 1936.

Upon his return to full-time teaching, he again undertook courses in vocational psychology, instincts, feelings and emotions, individual differences, and race psychology and mental measurement. In his last years, he offered the introductory statistics courses. Professor Griffitts retired in 1958.

CHAPTER NINE

Carl R. Brown

(b. 1892)

Carl Brown was born in Kansas on May 2, 1892, and received his early education in that state. He entered the University of Kansas in the fall of 1907. His academic plans were to earn a degree in German, and he began his work by electing as many hours as he could in that subject. He was advised to take some work in psychology. Brown followed the suggestion but the course made no impression on him whatsoever except to take its place as the dullest class experience he ever encountered.

During his senior year (1911) Brown was against advised toward psychology. This time he elected the course in experimental psychology offered by Floyd C. Dockeray. Dockeray had just arrived from Michigan and was currently collecting his data for his doctoral thesis. He was studying the effects of physical fatigue on mental efficiency and had devised a very cumbersome, frightful-looking apparatus which allowed the subject to learn nonsense syllables while hand-carrying weights. Brown participated as a subject and improved Dockeray's apparatus by building a harness that allowed the weights to be carried without the use of hands. In those days, Brown was an enthusiastic clarinet player and while he enjoyed being in the experiment, he did not want to harm his hands in any way.

The laboratory course thrilled Brown. He enjoyed the experimental approach to the problems and carried out some research on his own on lie detection utilizing a galvanometric apparatus. After the first semester, the course was repeated and Dockeray invited Brown to become his assistant. Brown accepted and, in his work with Dockeray, began to hear about psychology at Michigan – which for Dockeray meant John F. Shepard.[36]

Upon graduation, Brown was undecided as to what he should do. He was offered a fellowship and invited to begin graduate work in psychology at Kansas. Brown accepted the offer and the $200 a year stipend. He took several courses in math, did some work in sound localization, and earned a master's degree at the end of the year. The Department of Psychology offered him an extension of his assistantship. He had also been taking math and that department also offered him one. But psychology won out and Brown stayed on doing some work with dogs in a Thorndike-like puzzle box situation.

During his second graduate year, Brown met two young men from Phillips University, a small college located in Enid, Oklahoma. It had been founded about five years before by the Disciples of Christ Church for the single purpose of turning out preachers. Brown's new friends were on the faculty, one being chancellor and the other a professor of Hebrew. They offered him a position as an instructor in math. Since he had no other offer for the following year nor was he certain as to what he wanted to do, Brown accepted.

---

[36] It is interesting to note that in 1910-1911 when Dockeray was at Michigan, Shepard was already the dominant figure in the department. Brown reports that Dockeray used to talk about Shepard all the time – Shepard's maze, his courses, his personal intimacy with students. He never talked about Pillsbury.

It did not turn out to be a good experience for him. Brown was not successful as a math teacher. His classes did not go well and there were extra-curricular demands placed upon him which were difficult for him to accept. First, he was assigned the duty of directing the band. Though Brown loved music, he did not enjoy being responsible for the organization and proficiency of an ensemble. Second, Brown was not a fundamentalist in religion and that was what Phillips University stood for. He never went to church and that irritated the administration. He also smoked and that make it furious.

He had made friends with the football coach and used to scrimmage with the team that never won a game during Brown's entire two year stay. After practice the two friends would walk along the country lanes into town and enjoy the "clandestine" pleasures of a smoke. Eventually, they were discovered and falsely accused of offering cigarettes to students. The two men were fired.

Brown, not knowing what to do, returned to Kansas. Dockeray suggested that he go on to Michigan and study for a doctorate in psychology. Dockeray wrote Shepard and Pillsbury, and Brown was accepted and offered a $400 assistantship.

Brown arrived in Ann Arbor in the fall of 1915 and immediately went to the Natural Science Building. The building had just been made ready to receive its departments, and Brown's first meeting with Shepard found the professor and his assistant, Nellie Perkins, in the new laboratory busily unpacking equipment. For his first two years, Brown works as Shepard's assistant but was also able to continue the research he had begun at Kansas on sound localization. He also handled quiz sections in the one semester elementary course.

His course work was varied and took him to other departments. Brown elected mathematical statistics in the math department, a genetics course, as well as all of Shepard's offerings.

Brown was awarded a university fellowship for the 1916-1917 academic year but when the First World War broke out, he found himself involved in the psychological testing program. Either Pillsbury or Shepard – Brown was never sure which one – recommended him to the group of psychologists working under R. M. Yerkes who were setting up the Army Intelligence testing program.

In late September 1917, Brown received orders to report to Camp Devens, Massachusetts. The head of the section was William S. Foster who was on the psychology staff at the University of Minnesota. Other psychologists who joined Brown were H. B. English, J. E. Anderson, R. S. Roberts, and J. W. Bare. The group tested the whole division using what came to be the Army Alpha test. Their task was made difficult by the large number of illiterate soldiers they had to process. Brown began experimenting with a test composed of pictures with directions given in pantomime. This procedure eventually evolved into the Army Beta for non-verbal intelligence testing. But Brown was transferred before he went very far with the test.

In January 1918, he was sent to Fort Sill, Oklahoma for officers' training school. But the second morning in the school, he hurt his back and entered the hospital for a month's stay. He was sent back to duty too soon and re-injured himself. The camp surgeon then ordered his discharge from the service.

Brown returned to Washington and worked as a civilian on the testing program until 1919. Yerkes was head of the office and his staff included W. V. Bingham, C. S. Yoakum, and L. L.

Terman. When the war ended, the group worked up the statistical analysis of all the data that had been collected in the testing program. It was hard work and required long hours which were not without their disputes.

One of Terman's assistants was A. C. Otis. A disagreement occurred between Brown and Otis, who was acting as chief statistician, over the best statistic to use to express relationships. Otis was arguing for a line of relation based upon deviation from the mean, and Brown objected to that procedure as not making any sense. Terman listened to the arguments, then sided with Brown whom he made chief statistician replacing Otis.

The work was finished by June of 1919 and again Brown was undecided as to what he should do. Yoakum had returned to Carnegie Tech and asked Brown to join him. Brown went there for the summer of 1919 but then returned to Ann Arbor. Foster had also invited him to come to Minnesota but he had had enough of testing and wanted to return to experimental work.

When Brown returned to Michigan in the fall of 1919, he had found that Charles G. Griffitts, who had joined the department a year after him, had returned earlier and had been given a position as an instructor on the staff. Brown resumed as a graduate assistant and did not receive an instructorship until 1921. He continued to take courses, to assist in the quiz sections in the introductory courses, and occasionally to handle advanced work in the experimental and sensory areas.

In 1921-1922 he began offering a course in biometric methods in which he presented the various statistical procedures he had utilized in his work in the army testing program. In subsequent years, he introduced courses in advanced experimental psychology (1924-25) and the physiological psychology of the senses (1925-1926), and he participated in the supervision of the graduates students' original investigations in experimental psychology. Those assignments reflected the high esteem in which Brown was held by the senior staff, for he did not complete his own dissertation until 1927.

There is no doubt that Brown was held in the highest respect by the departmental staff. His colleagues describe him in the following terms: "a genius"; "he had a better brain than anyone in the department"; "I had exceedingly great respect for him"; "as a pure intellect, he had no superior in the department"; "a keen, analytical mind."

But Carl Brown was also a perfectionist and his severe, analytical mind would accept nothing less than that in any "product" that was brought him, whether it was an idea, an examination answer, a dissertation, or a piece of equipment. And it did not matter whether it was someone else's product or his own, it would still receive the same exacting analysis. Brown's effect on others as well as himself, therefore, was at the same time stimulating and devastating.

As far as personality is concerned, Brown can best be described as a very reserved person. Anyone who was only superficially acquainted with him might disagree with this description and, instead, describe him as antisocial. But they would be very wrong. It is true that he was not very comfortable with most people. He was relatively shy, and conversed very softly in a manner that often came across as a mumble. His analytic ability came into his informal conversations to the extent that he would listen carefully to what people would say and react to the literal meaning of their speech, rather than accepting their words as efforts aimed only at sociability. He would very often, therefore, laugh at the "wrong" times or react to conversational illogics that the speaker had never intended for a second thought.

Behind this reserve, however, Brown had a warmth, gentleness, and sense of humor that made him a favorite of generations of graduate students who took the time to become well-acquainted with him. He loved to spend time talking to students. A simple question that might be answered by other staff members in two minutes could easily turn into a two hour discussion. The student would ask the question and wait for an answer. Brown's immediate response would be silence. Sometimes that silence would last for three or four minutes. After awhile, Brown would begin answering the question by relating to other issues, all of which would also be handled in a comprehensive manner. One looked forward to these occasions and very often planned one's time to allow one or two of these sessions to occur as often as possible. And no one enjoyed them more than Brown.

It took patience and effort, however, to reach this level of intimacy with Brown. For the most part, the students and staff were somewhat awed and frightened by him. They desired to take their ideas to him because he was a very good critic. If there was anything wrong with their work, he would certainly find it. As a matter of fact, he would usually tear their ideas completely apart. For some of his colleagues, this was a good experience, for if any part remained intact after Brown's examination, it was certain to be valid. Norman Maier, for example, profited considerably from this kind of interaction. Others, however, appeared to have been devastated by the experience. Burton Thuma seems to have had almost every idea he offered destroyed by Brown and developed a rather low opinion of his own ability from these experiences.

Probably the most devastating effect that Brown's perfectionism had, however, was on himself. No idea, no experimental results, or no piece of equipment that he developed was ever quite right or quite good enough. There was always something more to be done. He would work on and on to complete that "one more part." But there was always "just one more part," and so nothing was ever finished or published.

Yet, until 1950, Brown continued to have a profound effect on the graduate students who were fortunate and patient enough to become well-acquainted with him. His theoretical orientation was close to a Gestalt school but he was not a Gestaltist. He specialized in the sensory and perceptual process but only to elaborate an all-encompassing form of field theory which included S-R psychology, Lewinian force fields and Gestalt perception as special cases. Underlying his general theory, Brown held to a field approach of brain functioning to which he anchored his general psychological theory.

Brown's theorizing, then, had a general neuropsychological orientation. He had argued for several decades in a manner that was surprisingly similar to the point of view presented by Donald O. Hebb in 1949 in his important book, *The Organization of Behavior*. Although their specifics were very different, the general point of view concerning the manner in which behavior must be viewed and related to the central nervous system was very similar. For years, Brown's ideas had provided the uniquely novel and stimulating effect on Michigan graduate students that Hebb's book began to have on psychologists across the nation in the early years of the fifties. What Brown, then, had to say to the Michigan students who came to Ann Arbor in those years, was no longer new and exciting. After thirty years, American psychology had finally caught up to him.

Carl Brown never received the academic recognition from the departmental and college administrations that he so richly deserved. The obvious explanation is that although he was always busy with research, very little was ever completed and nothing very much was ever published. The few publications he had were on apparatus. Consequently, his promotions were few and far between. He was made assistant professor in 1927 when he completed his dissertation.

His next and last promotion came years later in 1946. He was still an associate professor when he retired in 1962.

CHAPTER TEN

Forrest Lee Dimmick

(1893-1968)

Forrest L. Dimmick received his college education at Cornell University where he earned a bachelor's degree in 1915 and a doctorate in 1920. After graduating, he accepted an instructorship at Northwestern University. But Dimmick was an easterner and to him Evanston was too far from the center of psychological activity. E. B. Titchener informed him about an opening at Michigan. Dimmick wrote immediately to Pillsbury of his interest in it. Michigan, he felt, was closer to the desired eastern situation. On the strength of Titchener's recommendation, Pillsbury offered Dimmick an instructorship at a salary of $2,000.

Dimmick wanted the position but was being paid the same amount at Northwestern and did not feel he could justify the move without an increase. He wrote Pillsbury that a salary of $2,400 would be acceptable and that since his wife was also a Cornell-trained PhD, Michigan stood to gain by his appointment in that it would be possible for the department to acquire a teaching team. Pillsbury replied that since he had three other positions to fill, he would be able to "rob Peter to pay Paul," offered him $2,200 and said nothing about Mrs. Dimmick. Dimmick accepted the offer and arrived in Ann Arbor in the fall of 1921.

While at Northwestern University, Dimmick had established himself as a good research man and laboratory instructor. At Michigan, he continued his research activities. Dimmick was a thoroughly trained Titchenerian and carried out work on sensory phenomena from an introspectionist point of view. He also proved to be an interesting and stimulating teacher and contributed a great deal to the departmental discussions.

After three years, Dimmick was advanced to assistant professor but the following year (1925-1926), the pull of the east was too strong for him to resist. So when an offer came from Hobart College at Geneva, New York, he resigned and left Ann Arbor. Dimmick remained at Hobart College until 1927 when he became head of the Vision Branch of the United States Naval Medical Research Laboratory at New London, Connecticut. He retired in 1963 but remained in New London where he continued his work as a consultant in visual research. Dr. Dimmick died on January 21, 1968.

CHAPTER ELEVEN

Adelbert Ford

(b. 1890)

Adelbert Ford was born on April 23, 1890, in Michigan where he spent almost his entire youth. He completed his undergraduate as well as his graduate work (A.M., 1923; PhD, 1926) at the University of Michigan.

Ford first appeared in Professor Pillsbury's office in 1916. He had completed two years of college and had been teaching school in Houghton, Michigan. He had become interested in psychology and had decided to enter the University to complete his degree program with a major in that subject. He informed Pillsbury of his decision and sought his advice as to the best curriculum to follow. The senior professor was anything but encouraging and pointed out to the young man many of the unpractical aspects of such a career. Ford, however, was persistent so Pillsbury accepted him into the program.

The curriculum laid out for Ford consisted of a year each of experimental psychology, general biology, and college mathematics with the remaining hours heavily devoted to philosophy. Pillsbury also informed the twenty-six year old Ford that he expected him to earn an all A record. Ford had completed his junior year in Ann Arbor when the First World War began. He enlisted in the Navy and after two years of service returned to complete his baccalaureate work in 1920.

After graduation, Ford accepted a teaching position at Drake University. He was not happy at Drake. Since the administration gave him no money for laboratory equipment or supplies, he was forced to provide his own funds and time to construct what he felt were the necessary pieces of apparatus to run an experimental program. While at Drake, he constructed a complex six-channel kymographic recording device that took direct readings from a pneumograph, plethysograph, time intervals as well as three other input sources. He was very happy, therefore, when in March of 1921, Pillsbury wrote to offer him a pre-doctoral instructorship at Michigan. Ford accepted and arrived in Ann Arbor, bringing along his own shop and experimental equipment.

Pillsbury again laid out the course work that Ford was to follow in his doctoral program. Again it was heavily oriented toward experimental psychology, anatomy, and physiology. The latter two areas brought him in contact with the famous anatomist, Carl Huber. Ford completed his doctorate in 1926 with a dissertation entitled, "Attention-Automization: An Investigation of the Transition Nature of Mind." Pillsbury was his chairman, but the senior professor was not very interested in laboratory research at this time and seldom concerned himself with what Ford was doing. Ford, who did not seem to be on especially good terms with Shepard, worked alone on his high frequency electrical apparatus and took the few comments that Pillsbury offered concerning his experiment for what they were worth.

Around 1927, Pillsbury decided to reduce his own teaching responsibilities in order to allow himself more time for writing. Ford was given complete charge of the one semester introductory course which included Wednesday lecture to all the sections. Pillsbury was still listed as being in charge of the course but only came to give the first and last lectures. Several

other instructors assisted Ford in handling the quiz sections. Ford continued to be in charge of this course until he left Michigan in 1931.

Ford also offered a course in the central nervous system and mental process which he shared with Shepard and an occasional lecture by Huber. He had an interest in the applied area and occasionally offered courses in salesmanship and the mathematics of applied psychology.

In 1931, Ford accepted a position as head of the department of psychology at Lehigh University. Psychology had just been separated from philosophy, and Ford was offered the opportunity to build a new department. He continued in this position until he retired in 1955 with only time away from the department during the Second World War when he served as a research psychologist at the University of California Division for War Research. Upon his retirement, Ford accepted a position with the United States Navy Electronic Laboratory in San Diego. In 1955, he resigned but remained in San Diego to start a private consulting practice which continues to occupy his time to this writing.

CHAPTER TWELVE

John Duncan Finlayson

(1886-1950)

John D. Finlayson served on the staff of the Department of Psychology for only one year. Yet his career can probably be characterized as the most varied and interesting of any of the Pillsbury-era staff members. He was born in Thessalon, Ontario on May 16, 1886, and he came to Michigan for the first part of his college education. In 1907, Finlayson entered Alma College but remained only a year before transferring to the University of Michigan where he earned his A.B. degree in 1911.

John D. Finlayson

The next year (1911-1912), he enrolled at Columbia University where an interest in the ministry developed which he satisfied by taking work at the Union Theological Seminary. He then transferred to the Auburn Theological Seminary where he received his B.D. degree in 1914. Finlayson was ordained in the ministry of the Presbyterian Church that same year. Following his ordination, Finlayson spent a year touring Europe where he found time to study at the universities in Berlin and Gottingen. He returned to America in order to enter Harvard University where he received his doctorate in theology in 1916.

Upon graduating, Finlayson accepted a pastorage in Ypsilanti, Michigan. However, after one year in the position, the United States entered the First World War. He took a leave of absence in order to accept a YMCA regional directorship working with the American Expeditionary Force. Finlayson was given charge of the educational work for the American army in southern and central France. When the war ended, he organized and directed the educational work for the United States Army of Occupation in Germany. Finlayson concluded this work in 1919 and returned to the United States with a well-established reputation as an educational innovator and worker.

Finlayson did not return to Ypsilanti but accepted a position as professor of psychology and philosophy at Dubuque College, Iowa. He remained there, however, for only one year, as he was again called upon by the army to organize its entire general educational program. Finlayson spent almost all of the year 1920-1921 at the army centers in Camp Dodge, Iowa and Fort Lewis, Washington.

Sometime during his educational work, Finlayson appears to have made the acquaintance of Marvin Leroy Burton who had recently become president of the University of Michigan. Presumably at the president's recommendation, Finlayson was offered an instructorship in the Department of Psychology for the year 1921-1922. His major teaching assignment was in the quiz sections of the introductory psychology course, although he also offered a specialty course in

the psychology of religion. Finlayson was also interested in attaining an advanced degree in psychology so was also enrolled in several graduate courses. The staff members found him to be an interesting person and although he was an ordained minister, they did not find him to be too "pious" nor in any way to attempt to affect the beliefs of anyone else in the department.

After a year at Michigan, Finlayson accepted a position as president of Fairmont College, a small private college in Wichita, Kansas. He was expressly recommended for the position by President Burton. At that time, Fairmont College was in financial difficulty and was struggling to survive. Soon after his arrival in Wichita, Finlayson evolved a plan for making the college a municipal university. He cautiously began to mention his idea to small groups of leading citizens, but drew nothing but skeptical responses. Finlayson persisted and talked the "Municipal University" plan in season and out. He would approach his friends, talk to larger and larger groups and attempted to interest the Wichita business and civic leaders in his proposal.

The Chamber of Commerce finally was won over after three years of persistent efforts. A special election was held and the proposal was defeated by a narrow margin. Another election was held and it carried. Finlayson was inaugurated in 1926 as the first president of the newly named University of Wichita. He remained in Wichita only that year, however, for he received and accepted the offer of the chancellorship of the University of Tulsa.

Finlayson moved to Tulsa in the fall of 1927 and remained as chancellor for seven years. In 1934, he was elected president of the Beacon Life Insurance Company and resigned from the chancellorship. He was with the Beacon Life Insurance Company only one year when he accepted the district managership of the Massachusetts Mutual Life Insurance Company which had its district office in Ann Arbor, Michigan. So after fourteen years, Finlayson was located again in Ann Arbor. He remained in this position for the remainder of his career. Dr. Finlayson died in Ann Arbor on June 4, 1950.

CHAPTER THIRTEEN

Martha Guernsey Colby

(1899-1952)

Martha Guernsey Colby was born on February 22, 1899, in Montpelier, Idaho. The family moved to Ogden, Utah where Colby attended public school, graduating at the age of 15 in 1914. The following fall she entered the University of Utah and stayed to complete her freshman year. The near year she transferred to Ann Arbor. After completing her sophomore year, Colby withdrew again to accept a position in Ogden as a primary grade teacher and public school music supervisor. She returned to Ann Arbor the next year (1917-1918) and remained to complete her A.B. (1919), A.M. (1920), and PhD (1922) degrees.

Colby was very closely attached to Professor Pillsbury and during the twenties and thirties was certainly the most devoted disciple the senior professor had. Her dissertation, "A Study of Liminal Intensity and the Application of Weber's Law to Tones of Different Pitch" was directed by Pillsbury. During this time, he was personally very seldom involved in research and, not being very handy with apparatus, had nothing to do with equipment. He made an exception, however, in Colby's case and actually helped construct the apparatus she used in her data collection. The research won Colby the Sales Dissertation Prize.

Colby appears to have had a very great amount of respect for Pillsbury and was very defensive about him whenever any criticism of the senior man was made. Her feelings for him seem to have centered around his characteristic culture and dignity rather than his intellectual traits. She accepted very little of his psychology.

In the spring of 1927, Colby was awarded a Social Science Research Council fellowship to study abroad. She intended to stay in Berlin in order to study with the Gestaltists but the impersonality of the German professors annoyed her. She then went to Vienna and spent the year with Karl and Charlotte Buhler working on the problems of rhythm, melody, and space perception in children.

On several other occasions, Colby traveled to Europe to study. In 1929-1930, she received a Laura Spellman Rockefeller Traveling fellowship to continue her work in Vienna. The next year found her in Munich and Berlin for a semester at each place. In 1936 she again returned to Vienna on a semester sabbatical leave.

Colby (nee Guernsey) married Walter F. Colby on May 11, 1929. Walter Colby, a professor in the Department of Physics, had been involved in the direction of her dissertation and evidently the experience had been so satisfactory for both of them that a friendship resulted that led to their marriage seven years later. Professor Colby was a very distinguished physicist who had been on the Michigan faculty since 1907. He was nineteen years older than his wife. From the time of their marriage, Martha Colby served in the department on a part-time basis.

During the 1920s, her major teaching assignment was in the introductory courses although she did take over the course in genetic psychology after Dr. Dimmick left. She developed the latter course into a regular two semester offering. In later years, she introduced courses in the psychology of music, psychology of social work, and the psychology of social

service workers. Her bibliography was modest and contained items which tended to be in the area of genetic psychology and the psychology of music. She began a book on the former topic and made some progress on it but never completed it. She also spent almost four years working on the speech problems of an aphasic patient whom she helped make a remarkable recovery. Colby received promotions to assistant professor in 1929 and associate professor in 1937.

During the years following the Second World War, a series of events occurred which ultimately led to Colby's resignation from the department. Walter Colby had been intimately involved in atomic energy research during the war years. In the spring of 1948, he was called to Washington to work in the Atomic Energy Commission on the important organizational task for the commission's involvement in developing the nation's atomic energy research. Walter Colby was then sixty-eight years old, and both he and his wife were not happy with the prospect of his living alone in Washington. As Martha Colby put it in a letter to Dr. Marquis,

> . . . the tasks he faces seem to be very difficult and delicate and fatiguing. I do not inquire into their nature, but see the very real evidences of tension and strain and the long hours work. The release for my husband, as for myself, is in our evenings at the piano or with our books around the fireside or our simple supers shared in peace and quiet. I'm afraid it is just these very simple things which he will miss and they do not make a very impressive evidence to present to University officials.[37]

Colby remained in Ann Arbor during the spring and summer of 1948, and then received a leave of absence without pay for the fall. She agreed to teach the spring term of 1949 and it was arranged for her to offer a full assignment (genetic psychology, psychology of music, advanced genetic psychology). The department did not find it possible to arrange a schedule that allowed her a workable commuting arrangement between Ann Arbor and Washington so that from the Colbys' point of view it was not a very satisfactory situation.

In April 1949, Colby submitted her resignation but it was withdrawn four days later. But as the fall term drew near and the prospects of another separation grew imminent, the decision to resign grew firm. On December 19, 1949, Colby submitted her final resignation and concluded as follows. . .

> . . . Many people in the A.E.C. have come to see me, without any knowledge of this to Walter. They would like him to stay on indefinitely, or at least until summer. They are convinced, quite rightly, that he will not stay here longer alone. In his heart, he prefers the quiet laboratory of Professor Randall in Ann Arbor; in his mind, he feels there are two "hard jobs" to finish up for his country and his commission. So you see, he too, is torn. We shall have quite a job, each to comfort the decision of each other.[38]

Colby's resignation became effective at the end of the fall term, 1949-1950. Professor Walter Colby's work with the Atomic Energy Commission was completed by the summer of

---

[37] Letter from Martha G. Colby to Dr. Donald G. Marquis, January 2, 1948 in the Martha G. Colby File, Department of Psychology.

[38] Letter from Martha G. Colby to Dr. Donald G. Marquis, December 19, 1949, in the Martha G. Colby File, Department of Psychology.

1952. As a “last service” to the commission, the Colbys were sent to Europe on an assignment which would require the physicist to inspect some physics laboratories in Europe. The trip would also provide them with their first post-war opportunity to tour the Europe they both loved so much.

While being driven through the mountains of Greece, their driver swerved to avoid hitting a goat and the car went over an embankment. Walter Colby was severely injured but recovered. Dr. Martha Guernsey Colby was killed.

CHAPTER FOURTEEN

Theodore C. Schneirla

(b. 1902)

Theodore C. Schneirla was born on July 23, 1902. He completed all of his higher education at the University of Michigan, receiving his A.B. in 1924, his M.S. in 1925, and his Sc.D. in 1928. Schneirla's dissertation, "The Maze Learning and Orientation of Ants" was directed by Professor Shepard and represented the culmination of the experimental work on ants that was begun by Shepard twenty years earlier. Schneirla remained at Michigan another year and then accepted a National Research Council fellowship.

In 1931, Pillsbury offered him an instructorship at Michigan but Schneirla turned it down in order to accept an assistant professorship at New York University. He remained officially in that position until 1945. During 1932-34, he accepted a Guggenheim Fellowship in order to conduct research in South America on the behavior of army ants. He also was associated on a part-time basis with the American Museum of Natural History. Schneirla joined the museum's Department of Animal Behavior on a full-time basis in 1949 and two years later became curator of the museum in which capacity he continues to serve at this writing.

CHAPTER FIFTEEN

Norman Raymond Frederick Maier

(b. 1900)

Norman Maier was born on November 27, 1900 and attended public school in Detroit. He began his higher education at Detroit Junior College transferring to Ann Arbor for his junior and senior years and graduating in 1923. Maier wanted to attend law school and had plans to enter Harvard University when he received an offer to teach high school in the town of Nitro, West Virginia. Maier, who had worked his way through college by barbering, discussed his two prospects with some fellow workers and they convinced him that he had lived in the ivory tower long enough. He was advised to go out to see what the world was really like. So Maier accepted the teaching position.

Norman R. F. Maier

Nitro, West Virginia was a small company town that belonged to a local firm that manufactured nitroglycerine. The year before Maier arrived, the company had given control of the school to a local board. He was hired to teach physics. But the board had no difficulty seeing a relationship between the molecules and atoms that Maier discussed in class and biological cells. He was accused of teaching Darwinism and discharged.

Maier returned to Ann Arbor in time to enroll for the summer session in 1924. He had taken only one psychology course while at junior college and that had not made an impression on him. That summer he elected courses in philosophy and psychology. These experiences were followed by Shepard's comparative psychology course which converted him to psychology. Maier's pre-law and philosophical background predisposed him toward an interest in logic and psychology of higher thought processes. Shepard's course greatly impressed him. He found Shepard to be a very stimulating, original and critical teacher. His two years working toward his master's degree were spent running rats for Shepard and taking courses in the department.

One of his courses was Shepard's advanced systematic psychology. Shepard's presentation came very close to the Gestalt position on many topics. At that time, not much was known about the new German school but Shepard was enthusiastic about it. Kohler visited Ann Arbor in 1925 when he was touring the United States and attended Shepard's seminar. Maier was impressed with what he heard. When the opportunity arose for him to spend a year in Berlin studying with the Gestaltists, he jumped at the chance. Shepard encouraged him to go. Maier always has considered that year in Berlin (1926-1927) to have been the most stimulating and exciting single year of his life.

He was among the first group of Americans to study with the Gestaltists. Others in the group were Karl Zener, J. F. Brown, P. T. Young, and C. R. Griffith. Max Wertheimer was considered the high priest of the circle. The students would take classes, run experiments, and just interact with each other. Maier found Kohler, Wertheimer and especially Lewin to be very stimulating. These men were taking psychological concepts that were familiar to everyone and giving them entirely different meanings.

Not all the Americans were equally satisfied with their experience in Berlin. Zener became disgruntled and unhappy at the slow pace of his research on the problem that Kohler had given him. He considered the problem to be too simple to be worthy of his efforts. Young felt that the only gain from his time at the Institute was from the good music he heard and his practice in using the German language. Martha Guernsey Colby spent two weeks waiting to see Kohler who tried to discourage her from working with children. She decided to go to Vienna and work with Karl and Charlotte Buhler. Griffith was the most disillusioned one of the group and had nothing good to say about Berlin, the Institute, or Kohler. The main objection seemed to be that the Americans were given only the simple aspects of the Gestalt research on which to work. All the interesting projects were under the personal supervision of Wertheimer and what was called by the Americans a closed circle of German Gestalt "Dozents."

Maier, however, did not wait to be invited. He carried out experiments on his own without waiting for directions. An example of this was the way he broken into the Lewinian group. Lewin, Zeigarnik, and others held a conference every Saturday morning in which the special Lewinian form of field theory was being worked out. Maier showed up uninvited to one of the meetings. Lewin looked at him and said, "You're not one of my students." Maier said that he was and had been listening to all the things that were being discussed and wanted to sit in. Lewin laughed and gave his permission.

Maier's German was very poor, and they laughed at him but he could talk and understand and he profited immensely from the experience. The discussions were often very heated with someone always running to the blackboard to diagram the discussion. Maier was the only American in the group although he was able at a later time to convince J. F. Brown to attend.

In Berlin all the Americans were accused of being behaviorists. During the discussions, they were always asked how the behaviorists would explain a point or some phenomenon. The Americans would invent some explanation and Lewin would often exclaim that that was what he would say; therefore, it could not be behaviorism.

Maier was doing some human reasoning experiments and had to wait for some subjects to be provided. The German students asked him to demonstrate what psychologists in America do with rats. Since he had spent two years running maze experiments for Shepard, Maier was happy to be able to oblige them. He obtained three rats – the first to ever be used in the Institute for research purposes – and set up some simple maze experiments for the demonstrations. One whole afternoon Maier demonstrated "American animal psychology."

After the demonstrations were over, Maier still was without human subjects but did have the rats. He began arranging a series of problems for them that were animal analogs of the human reasoning experiments that he had been doing. All the problems had the common feature of the animal being given separate experiences which it had to integrate in order to find the correct path to food. Though the Institute staff found it difficult to believe, Maier's rats demonstrated that they could solve the problems. And that was how "reasoning in rats" originated.

When Maier was ready to return to America in the summer of 1927, he had two completed studies, either one of which could have served as a dissertation. He applied to Ann Arbor for permission to take his degree in Germany so that he could stay another year. But he did not hear about it until he reached Michigan and, of course, it was granted.

Upon his return, Maier was offered an instructorship and spent the year handling elementary quiz sections. In the spring, he submitted the animal data as his thesis, “Reasoning in White Rats” and his doctorate was awarded in June 1928.

During the year 1928-1929, Maier held an assistant professorship at Long Island University and was not very satisfied with the position. That spring Pillsbury wrote him about an opening at the University of Oklahoma but Maier applied for and was awarded a National Research Council Fellowship for two years of study and research at the University of Chicago.

The experience at Chicago was extremely rewarding. It was during this period that Maier acquired his knowledge of neurological techniques which enabled him to carry out his research on the effects of cortical destruction on reasoning and learning. Karl Lashley was at Chicago and was in the process of moving away from strict behaviorism toward a more Gestalt-like position. Maier and Lashley had long discussions on Gestalt psychology. Certainly Maier, fresh from Berlin and full of enthusiasm for the new system, must have had an effect on Lashley.

In 1931, Pillsbury wrote Maier to invite him to return to Michigan as an instructor. The position had been offered to his friend Ted Schneirla who turned it down. During the negotiations, Shepard offered Maier the comparative course, which the younger man would have very much enjoyed doing. But after he arrived, the occasion to teach the course never materialized.

Maier had, or course, been an outstanding contributor to Michigan psychology ever since his arrival. His research and writings had been more widely quoted than any other work carried out in the department. He was made an assistant professor in 1935, an associate professor in 1939, and professor in 1945.

CHAPTER SIXTEEN

Edward Barrows Greene

(b. 1895)

Edward B. Greene was born on October 25, 1895 in Andover, Massachusetts. He attended Phillips Academy in Andover, graduating in 1914 and Amherst University taking his A.B. degree in 1918. Greene then served two years in the army. In 1921, he earned a diploma from the New York School of Social Work and then entered the graduate program at Columbia University. After receiving his master's degree (1922), Greene held a series of positions from which he acquired experience in the areas of testing and personnel selections. While employed in these positions, Greene continued his graduate work at Columbia. In 1927, he accepted an instructorship at Michigan and completed his dissertation during his first year in Ann Arbor.

During his first years at Michigan, Greene assisted in the elementary course and took over the offerings in mental measurement. He also introduced a new course in 1929-1930 called "Practicum in Clinical Psychology." This course was the first strictly clinical offering to become a part of the curriculum. The School of Education had been giving such work for several years but the Department of Psychology had never previously strayed from its tradition of experimental-physiological psychology.

Greene, who appeared to his colleagues as somewhat aggressive, was never completely accepted by the departmental "in-group." He was always somewhat uncertain as to his future within the department and, as a matter of fact, stayed in the department for almost fifteen years without receiving tenure. There are several factors which were relevant to this situation.

First, Greene was brought in from outside of the Department. Only Adams shared that distinction with him. The other eight members of the department had all been trained under Pillsbury and Shepard. Adams and Greene, then, were isolated by their intellectual and psychological training, and almost symptomatically, came to be isolated physically.

By 1925, psychology had outgrown the assigned space in the Natural Science Building. It was given additional room in the Pharmacology Building in the twenties and on the third floor of West Medical Building in the thirties. Adams was moved into the Pharmacology space and when the West Medical Building space became available, Adams moved again and Greene joined him. So the two "outsiders" were eventually physically removed from the rest of the psychology staff.

Second, Greene was engaged to offer the mental measurement work in the department which was, in principle, opposed to such a specialty. Though they appeared to admit that it had to be represented in the department (Greene *was* hired), the staff members were not inclined to be especially enthusiastic about the subject or its advocate.

Finally, Greene himself appeared to be too aggressive a personality to overcome the first two obstacles to his easy acceptance into the department. He was perceived by the staff to always want more and more influence in the department. Greene would continually ask for different courses in what appeared to be a constant attempt to reach and influence more and more students. He never acted satisfied with what was given him.

Greene remained in the department for almost fifteen years. Although he received a promotion to assistant professor, he was denied tenure. At the beginning of the Second World War, he received a leave to work in Washington, D.C. as a personnel consultant with the Federal Social Security Board. While away, he attempted several times to determine what his future with the University might be. In May 1942, Greene wrote Pillsbury concerning his status. He was on a leave from a non-tenured position and wondered if he should plan to return or use his new contacts to find another location. Pillsbury's reply reveals a great deal about the chairman and the department during this period:

> . . . as to your general problem, I think the whole matter rests on the interpretation of the attitude of your colleagues. That seems to be more hostile than the facts of your work and your general intention toward them deserves. You have always shown great interest in the social integration of the department. Almost more than any other member. But in spite of it you seem not to be taken at your true worth.
>
> The criticisms that they make are that you are always pushing for new courses and I supposed are critical of their work. I do not know that I ever heard that second statement made definitely but it seems to be implied in the reaction to you. You were always keen to have charge of 31. Whether you still want control now that you have one section I have never heard you or anyone else say, but it seems to be implied in their attitude.
>
> The other statement made rather freely is that you are always applying for new courses, given them for a time, then become tired of them and ask for others. All this might be a merit rather than a matter of criticism but seems to have aroused the other attitude. . . I have gone into these relatively unessential matters because in a department system it is what your colleagues think not what you are that will determine the future. It should be said that not all the members of the department are opposed. Shepard, for instance, suggested that you be advanced in salary this spring, but was voted down by the other members of the committee. One or two others have not expressed themselves. My own attitude will not count as I retire this summer. . . It has been suggested that someone (not yet chosen) be made Executive Secretary and the department run on for the war period as it is. That means that the present attitude will control the future.
>
> Your book and your standing in the association might well give you a better place. If they do, I think you would be wise to take it. . .[39]

Greene took Pillsbury's advice and accepted a position with the War Manpower Commission in Michigan in 1943. He remained in Detroit as Chief of Personnel of the office of the United States Employment Service until 1948. From 1951 until his retirement in 1961, Greene

[39] Letter from Walter B. Pillsbury to Edward B. Greene, May 11, 1942 in *The Pillsbury Papers*.

worked for the Chrysler Motor Corporation. Upon his retirement, Greene entered private practice in the Detroit area.

CHAPTER SEVENTEEN

Burton Doan Thuma

(b. 1902)

Burton D. Thuma, who was born on June 21, 1902, in Cincinnati, Ohio, began college as a mechanical engineering student at the University of Cincinnati. It had been a family decision for him to enter engineering and young Thuma did not find the program of interest to him. At the end of his first year, he convinced his parents that he should try another field and be allowed to go away to school. Thuma applied to the Wharton School of Commerce and Finance and went to Philadelphia for an interview. The admissions officer reviewed all the engineering and mathematics courses on his transcript and turned down his application.

It was then late in the summer so Thuma went to the family's summer cottage on Lake Erie uncertain as to his plans for the fall. He met a University of Michigan student who convinced him to apply to Ann Arbor. Thuma visited the campus, applied in person and was accepted.

Burton D. Thuma

During the next several years, Thuma elected courses in many of the areas that had been omitted from his engineering program – particularly history, philosophy, and psychology. In the fall of 1922, he enrolled in the general psychology course offered by Pillsbury. He was impressed with the heavy emphasis placed on the nervous system which he saw as the underlying mechanism of behavior. In terms of his previous academic work, Thuma could see it as the engineering aspects of behavior and it appealed to him.

Thuma enrolled in the two-semester laboratory course where the lectures were given by Pillsbury and Shepard had charge of the laboratory work. Martha Guernsey was the laboratory assistant. Thuma enjoyed the course and did very well in it. Shepard impressed him with his rigorous, critical, knowledgeable approach to the material. He was soon converted to Shepard's version of psychology.

Thuma graduated in June 1923 and was given as a present, a summer tip to Europe. Pillsbury was also abroad that summer. Miss Guernsey gave Thuma a letter of introduction to the senior professor and urged Thuma to visit him and request his support for the young man's application for graduate work. Thuma, quite apprehensively, approached the very distinguished-looking Pillsbury, and received his consent to enter the program.

During the year it took him to complete his master's degree (1923-1924), Thuma elected courses in philosophy and neurology (with Carl Huber and Elizabeth Crosby) as well as psychology. He enrolled in Shepard's advanced systematic and comparative courses and became interested in the work that Shepard was doing in maze learning. He also began to absorb

Shepard's systematic approach toward learning and perception. Shepard taught with such confidence and thoroughness that the students for the most part were convinced that he was right and rest of the field was wrong.

In 1924, Thuma received a year fellowship to Stanford University. Calvin Stone, Truman L. Kelley, Dean Fainsworth and Lewis Terman were then on staff at Stanford. Stone was in need of an assistant to run rats and since Thuma had had that kind of experience at Michigan, he was given the assignment. Thuma had taken along Shepard's theoretical position, the truth of which he had no doubts. He was somewhat shocked to learn that the Stanford staff was not convinced as to the validity of the theories and was not even certain who Shepard was. The Michigan professor had not published his experiments or his views, and the Californians, hearing about them only from Thuma, were not inclined to share his enthusiasm.

Stone asked Thuma to remain at Stanford when his year was over, but he had become interested in neurology and Stanford did not have much in that field. Michigan had Huber and Crosby, so Thuma returned to Ann Arbor and enrolled in a combined psychology-neurology curriculum. His work with Huber and Crosby culminated in a joint article on the lateral geniculate body of the cat.

Thuma debated the choice of a career in either neurology or psychology and finally decided on the latter field. He received an assistantship with Shepard which he held until his appointment as an instructor in 1928. He received his doctorate in June 1931 working under Shepard. The thesis was entitled "A Contribution to the Study of Auditory Sensitivity in the White Rat." He was promoted to an assistant professor in 1931, associate professor in 1938, and professor in 1948.

Thuma was in complete charge of the general elementary course from 1932 until 1935. After that date, the enrollment was too large to be handled in a one-lecture unit with several recitation sections. The students were divided into several lecture units with their own recitation sections. Thuma continued to handle several of these units. He also introduced a third type of introductory course which was more experimentally oriented than the one-semester course but less so than the two-semester sequence that Shepard offered. From time to time, he offered courses in comparative psychology, comparative neurology (with Huber and Crosby), laboratory practices, space perception, vision and perception.

His wide range of interests and versatility as a teacher are indicated by a series of specialty courses that he occasionally offered – psychology of law, psychology approaches to art, and the sensory basis of knowledge. During the early forties, Thuma was also involved in an experimental honors program that the Literary College undertook.

Thuma was very much at home with the technical and apparatus aspects of experimental psychology. Brown, Ford, and Thuma put together a manual for the laboratory course. Brown and Ford designed the apparatus for each exercise and the three of them spent long hours in the departmental shop constructing multiple sets of all the equipment.

There is a classic departmental story associated with this project. Brown and Thuma wanted very badly to have a lathe for the shop. They felt it would provide great flexibility in what they could produce. But lathes were expensive and the departmental budget was small. In the early thirties, the practice was that if the departmental current account was not exhausted by the end of the fiscal year, the surplus could be carried into the next term. It was possible, therefore, to accumulate over several years a large enough account to purchase an expensive item. The staff

agreed informally to be thrifty for a few years in order to save up enough funds to purchase a lathe. Brown made a trip to South Bend, Indiana and located an old but serviceable lathe that the department could obtain for a reasonable price.

No one, however, seemed to have impressed upon Pillsbury what the agreement had been. When the end of the fiscal term arrived, the dean sent around his annual request to departmental chairmen to return all unused and/or unallocated funds. This was a routine request by the dean which was routinely ignored by all department heads. But Pillsbury, who was probably experiencing a surplus for the first time in his career, turned in all the painfully accumulated funds and left immediately for Europe. When Brown and Thuma learned about what Pillsbury had done, they were enraged and it appears that if the senior had not left town, there may have been mayhem in the department.

In 1937, a lathe was finally obtained by Brown and Thuma's decision to purchase it from profits of their laboratory manual.

In later years, Thuma became very active in departmental and college administrative operations. After Pillsbury's retirement in 1942, he was made executive secretary of the department as well as acting chairman of its executive committee. During the Second World War, the University appointed him its coordinator of the dissemination of information concerning its war efforts as it related to faculty and students. In 1944, he entered the United States Navy returning to the University in 1946.

These departmental and University appointments reflect a recognition of Thuma's skills in organizational and administrative operations. His success in these activities stood in contrast to what in his mind was his lack of professional creativity in psychology. Rather reluctantly, he came to the conclusion that he could make a greater contribution to the growing excellence of the University by concentrating on what he could do well. When the opportunity arose for him to move into the Office of the Dean as an assistant dean, he took it. He continued to demonstrate his administrative skills in that office when he served as acting dean in 1955. His most recent contribution to the University had been his patient shepherding of the newly established Residential College through the difficult phases of it planning, approval, trial period and ultimate operation. Thuma retired form university service in 1967.