Eleanor J. Gibson

I will abbreviate the beginning of this tale because my early life was so traditional as to make very dull telling. I did not escape from a bloody revolution or a wasting childhood disease or even a broken home. I was reared in an atmosphere of middle-class respectability among dozens of kindly, staunch Presbyterian relatives, all of English and Scotch-Irish ancestry. and all those ancestors had been long established in America. The only surprising thing to me is how I managed to break away so far from this background and emerge as an intellectual (an academic, at least), with ideas that would seem radical to many of my forebears. No one ever suggested that I was a bright child, or particularly wanted me to be bright. I began to ponder the matter when I went to high school at the age of twelve and found my peers generally older and more sophisticated than I. I wondered still more about my intellect when I grew interested in boys and found that it was essential, if I wanted any reciprocation, to conceal the fact that my grades were A's.

I grew up in Peoria, Illinois. (Quite literally, I "played in Peoria.") Grades of A were easily come by at the Peoria high school that I attended. But since I was destined, by family tradition, to go to Smith College where things wouldn't, presumably, be so easy, a couple of devoted teachers with high standards took me in hand during my junior year and put me to work. It is amazing to recall the number of hours and the effort they provided without any recompense except the hope of three pupils doing them proud on the College Board Examinations. I had two fellow pupils, another girl who was applying to Smith and a boy who was applying to Princeton. We read poetry, wrote papers, spent two afternoons a week after school doing Latin prose, and struggled with math every Saturday morning, doing all the old college board math exams for the preceding ten or fifteen years.

One of these self-sacrificing ladies (they were all ladies) was the Latin teacher, Miss Stewart. She had taught Latin and Greek to both my parents and felt privileged to turn me into a scholar, if possible, by any means she saw fit. She was notorious for cracking a ruler over the knuckles of lagging pupils, a method that, despite Skinner and his ilk, seemed to work pretty well. But she gave a really effective performance when she was occasionally driven to distraction by a bad translation. She tore her hair (red turning white), her pince-nez fell off, and she screamed, "Ye Gods, ye Gods, ye Gods!" It has just occurred to me that some of my awe of Miss Stewart has passed along, through a mysterious process of transference, to my husband, who sprinkles epithets, including "Ye Gods," over any manuscript of mine that he gets his hands on.

The efforts of the teachers were fortunately successful, and the three pupils went east to college. It was the first time any of us had lived outside the Middle West. Smith was a revelation and an instant joy to me, despite my provincialism. My roommate was nineteen, had attended an Eastern preparatory school, and had never been west of Northampton, Massachusetts. There was something comfortingly provincial about that, too. The greatest revelation was that Smith, a women's college, was a place where women were not only permitted but encouraged to be scholars, even scientists. That is said to be permissible today even at coeducational colleges like Cornell, but the atmosphere for real flourishing of an undergraduate woman who wants to be a scientist, in particular, still does not match Smith at that time.

During the first two years at Smith I wasn't much of a scholar, however. It was too important to absorb all the other wonderful and previously unattainable things: symphony concerts, mountains in the distance to be climbed, courses and books about things I had never heard of, and proms and houseparties at nearby men's colleges. But toward the end of my sophomore year, I discovered a profound interest in psychology.

I was assisted in my discovery by what now seems a rather unlikely person—Margaret Curti, from the Middle West and a confirmed dust bowl empiricist. She had taken her Ph.D. at Chicago with Harvey Carr. Her thesis, one of the first studies done on spectacle-wearing (with laterally displacing prisms), was in the tradition of the Chicago functionalists and emphasized the kind of S-R association theory that characterized Carr's theory of space perception—essentially the notion of linking, or relinking, local signs with localizing responses.

The idea as such was not so appealing to me—I doubt that I really understood it. What attracted me was the emphasis on experiment. The course was Animal Psychology, and we ran our own rats through our own mazes. The experiment was of no importance, but performing it was entrancing. I took Margaret Curti's course on Child Psychology, too. That wasn't so good; Chicago functionalism worked better for rats. But I was hooked, and I decided to major in psychology, instead of French as I had once intended.

My junior year was satisfactory, but not exciting. I had some good teachers, especially Harold Israel, who gave a year-long course in History and Systems, a hard and excellent course. He had been a student of Edwin Boring's at Harvard. He did no research at all, but he was a first-rate historian. He tried to teach us what problems a psychological theory should be able to handle, not just what any particular theory did handle—a valuable lesson. There was a course in Tests and Measurements, taught by Hannah Faterson, that I enjoyed a great deal, too, although not for the underlying theory of testing. During the second term, we learned to give Stanford–

Binets and went out to the schools to give individual tests. It wasn't exciting, and I determined not to continue in that area. But administering the test to a child was real, and I thought I would like to work with children almost as much as with rats.

At that time, the chairman of the Smith College Department of Psychology was William Sentman Taylor, a remarkable man. He had been a student of Morton Prince at Harvard, and Prince was his idol. He taught Abnormal Psychology and was interested in hypnosis. We read all about split personalities and the medium Marjorie, and we watched Mr. Taylor give rather unconvincing demonstrations of hypnosis in class. He did not hypnotize the students, of course. At a college for women that would be unthinkable, like not leaving one's office door open a few inches when a student was inside. For all the racy topics, Abnormal Psychology inspired no great interest in the students. I suppose Mr. Taylor, a very dull and prosaic man, made it dull. He had edited a book of readings in abnormal psychology that was used in his course. Each excerpt was short, ranging from a few pages down to a paragraph or even a sentence. The shortest excerpts had a proverblike quality, and evidently they were meant to, since Mr. Taylor's last publication before his death—apparently a lifetime's work—was a collection of aphorisms. Freud was seldom mentioned in the course, but when he was, the implication was clear that his ideas were unwholesome. The dynamics in what we were being taught were missing; but about the same time, they were introduced from a most unexpected quarter.

William Allen Neilson, president of the college, brought Kurt Koffka to Smith as a kind of professor-at-large. It was a time when many Europeans, especially Germans and Russians, were looking for a home in this country. Koffka brought with him a retinue of young psychologists—Molly Harrower (English), Fritz Heider (Austrian), and three émigrés from Soviet territories, Alexander Mintz, Eugenia Hanfmann, and Tamara Dembo. The latter three were known as "the Russians." Some of these people had been students of Kurt Lewin, and research began to burgeon on dynamic factors in perception and other cognitive processes, and even on emotions. None of Koffka's retinue gave a course, but naturally the psychology students were preempted as subjects, so the ideas filtered in.

Koffka himself did eventually offer a course, which I took my senior year. He read his lectures, which were notes for his book on Gestalt psychology, and the class seemed to me Germanic in style, just as he seemed dictatorial as a professor. He did not welcome discussion or inquiry from the students. I found him unattractive as a person and as a scholar, but I was an exception; for many students, including some of my most respected friends, were charmed by both the man and his ideas. I may have been rendered immune because I had just found the charm elsewhere.

James Gibson came to teach at Smith, fresh from a Princeton degree and only twenty-four years old. I had no contact with him until I met him at a garden party at the end of my junior year. It rained and we were happily stranded in a corner of a quadrangle where he was supposed to be shaking hands with parents of seniors as I offered them punch. He took me back to my dormitory in his ancient Model-T Ford, and next day I hurried to the class dean's office and changed the following year's schedule to include his class in Advanced Experimental Psychology. It was a wonderful course and I fell in love with experimental psychology and with the instructor.

The course was small—just nine students—and very time-consuming. Each person, with a partner, did four experiments each term, getting her own subjects wherever she could (Amherst was a favorite source) and writing up the experiments, complete with background. I made some of the best friends of my life in that course. Everyone in it went on to do graduate work. We did experiments on a wide range of problems—color constancy, aftereffects, conditioning, retroactive inhibition, adaptation to wedge prisms. One of the experiments that I did with Gertrude Raffel Schmeidler on bilateral transfer of a conditioned response (finger flexion to shock) was published (Gibson et al., 1932). It was very exciting and both of us felt we were budding scientists. We had our own laboratory in the basement of the psychology building. It functioned as a sort of club room as well as work place.

It was clear by that time that I wanted to do graduate work in psychology, but the Depression had hit. My family's finances were already strained from sending me to Smith, and there were no training programs and few fellowships in those days. I was in luck, however. Smith College did not give Ph.D.'s, but it did give a master's degree, and the Introductory Psychology course needed teaching assistants. I was hired with two of my friends, Hulda Rees and Sylvia Hazelton, to teach laboratory sections (they were given routinely as part of the course) while working for a master's degree. Hulda and I shared an attic apartment and were in seventh heaven. We were the best of friends (and still are), we felt terribly important hobnobbing with the younger faculty members, and we learned a lot. The students wrote one laboratory report a week, and we each had sixty of them to read, but even that wasn't too bad. We bogged down a bit once when an instructor in the department suddenly began to fancy that she was being poisoned by her colleagues, and we had to grade all of her papers, too. But we emerged from that episode with enhanced reputations for reliability and loyalty—very useful later.

During my first year as a graduate student, my exhilaration was compounded by my increasing intimacy with James Gibson. I took his seminar on William James, I began research for my master's thesis with him, and I drank bathtub gin and grapejuice with him and other friends. He was an ardent experimenter, among other things, and was doing his research on the aftereffects of exposure to curvature. Perversely, I was not particularly interested in perception at the time (learning was the stylish topic of the day). But I was interested in James Gibson, and we were married in September, after I had completed one year of graduate work. The wedding took place in Peoria, under the eyes of a large contingent of relatives, and we drove back to Northampton through the Adirondacks, carrying a suitcase full of books on social psychology. My husband had been assigned to teach that subject for the coming year, and I had been assigned as his assistant. Neither of us had ever had a course in social psychology, but things like that can happen in a college the size of Smith. I don't remember either of us reading any of the books on our honeymoon, but the course turned out pretty well. Taking a naive look at a field, if one has had good fundamental training in how to pursue knowledge that is desired, can pay off. My husband has always had a knack for that. The course even turned out to be popular.

I completed my master's degree that year, doing my dissertation research on retroactive inhibition (Gibson and Gibson, 1934). The experience was very instructive. I revised that thesis again and again under my husband's critical eye. Writing a thesis with one's own husband as director is definitely not to be recommended, but we survived.

It was time to go somewhere else to study, but the Depression made things difficult. I stayed on at Smith as an instructor, read widely on my own, and went to such seminars as were available. There was always Koffka's seminar, attended by his retinue, all the young faculty in the department, and the graduate students. Many of the people who came to Northampton with Koffka had left for other jobs by that time, but Fritz Heider remained, at the Clarke School for the Deaf. He and his wife Grace, also a psychologist, became our close friends. I think they were among the first psychologists in this country to study cognitive processes and linguistic development in deaf children.

Although the discussions in Koffka's seminar were on a very high level and were stimulating, I was not attracted by Gestalt psychology and yearned for what I thought of as "hard" psychology. I didn't like introspective methods. I wanted to be objective, as I thought of it then, and I wanted to work with animals and children. The time came when my husband had his first sabbatical leave coming up, and I applied to Yale, the antithesis of everything Koffka represented. I would go there alone for one term and my husband would join me for the other.

A Year at Yale

Yale was a lively place then. The Institute of Human Relations was new, and great things were to be done cooperatively with psychiatrists, anthropologists, and sociologists. Clark Hull had recently arrived. There were chimpanzees, which had come with Robert Yerkes from Orange Park, Florida. There were excellent people in neurology, a science that had been only minimally available at Smith. The idea of a big university with lots of research going on and famous people to study with was very attractive. I was not discouraged by the fact that I secured only a tiny scholarship that paid tuition and left me $25 to spend on other things.

What I did not realize when I departed for New Haven was how favored a life I had led at a women's college. It never occurred to me that a big university would not be quite as welcoming as Smith. I found this out in a hurry. After three days, I succeeded, with considerable effort, in making an appointment with Yerkes. A secretary let me in, and Yerkes invited me to sit down and asked why I had come. I answered that I had come to Yale to work with him. He stood up, walked to the door, held it open, and said, "I have no women in my laboratory." I was astonished and angry. I took my troubles to Carlyle Jacobson and Henry Nissen, who were both young professors in Yerkes' laboratory at the time. They said that was how it was and nothing could be done, but that they would find other opportunities for me to work with animals. Roswell P. Angier, who was chairman and a sort of kindly grandfather to the graduate students, just said it was only to be expected and I had better find a young faculty member to work with. Yale, as it turned out, did nothing for women at that time except tolerate a few as graduate students. The graduate school had a fine new building, with a library, refectory, living rooms, and so on, but women graduate students were not welcome. There was no place to live except for shoddy rooms and apartments that one had to locate for oneself. The institute was surrounded, for about an eight-block radius, by the most miserable slums in New Haven. They had to be negotiated on foot and alone at night because there was no place nearby to live and few students could afford a car.

Despite my original shock, I soon began to find Yale almost as interesting as I had expected. It consisted of small empires, each with its czar. Yerkes had the most impressive empire, or at least the most noticeable one, because of the chimpanzees. They had outdoor cages atop one of the buildings and, in fair weather, could be heard from every quarter of the institute. Arnold Gesell had a large institute of his own, with many subordinate faculty members—Helen Thompson, Louise Ames, Frances Ilg, and others whose names I have forgotten. Raymond Dodge had recently retired and was ill with

Parkinson's disease, but he appeared at his laboratory occasionally. Walter Miles had a large laboratory full of fascinating gadgets, but few followers. The dominant figure at that time was Clark Hull. His laboratory was large and he had many students. It was a lively place, although Hull himself was rather an awesome figure until one came to know him. There were younger faculty, too—Donald Marquis, Neal Miller, Richard Wendt, Carlyle Jacobson, Henry Nissen, Leonard Doob—excellent people, all very active and approachable. Marquis and Miller were both on leave the year I was there, but I became well acquainted with the others. John and Lillian Wolfe and Hobart and Molly Mowrer were at the institute as postdoctoral fellows, and my husband and I became friends with them as well.

Outside the department of psychology, in the Institute of Human Relations, there were Mark May, John Dollard, and some psychiatrists who participated with psychologists in an interdisciplinary seminar—but not one that was open to women students. Having been turned away from that endeavor, I made the best of necessity and explored what the medical school had to offer. I attended John Fulton's seminar and took a course in neuroembryology with H. S. Burr, both impressive and invaluable. I was permitted to watch and assist in minor ways when there was experimental neurosurgery on cats and monkeys, and I got involved in some research with a young neuroanatomist. The anatomists and physiologists were far more tolerant of women students than the psychiatrists, although they were not included in the institute. In the end, not much came of the interdisciplinary human-relations venture. Interdisciplinary cooperation apparently cannot be commanded. It can happen when a couple of people get together voluntarily with an idea (as Miller and Dollard did), but referring to some important scholars collected under one roof as an "institute" seems to be of little avail.

Graduate students as a rule educate one another. Yale was no exception. All the new graduate students (there were ten of us) had to attend a "proseminar." We moved rapidly from one area of psychology to another, the areas determined primarily by the interests of the senior professors. Each czar had his fortnight. But he himself didn't necessarily have to appear more than once. (I don't remember seeing Gesell more than once, for instance.) He could, and frequently did, send emissaries. We got little out of these meetings from the leaders, but the group of students became closely knit, and we learned a lot by arguing among ourselves, despite the vaudeville style of the seminar. Irvin Child, Helen and Vincent Nowlis, and Austin Riesen were part of the group, respected friends then and now. Older graduate students (although not older in years than I) included Carl Hovland, John Finan, John McGarvey, Adella and Dick Youtz, and Elliot Rodnick. Some of us had a supper club in the Youtz's tiny apartment at 80 Howe Street. The conversations with these friends were very influential in my final decision to work with Clark Hull.

When I approached Hull, he was somewhat standoffish, but not uncivil. He said that he already had a number of students working with him (as indeed he did), that he had large-scale plans mapped out for his own work, and that he was interested only in graduate students who fit somehow into those plans. By that time, he had published most of his justly famous series of articles on the conditioned reflex and its role in various animal learning situations, such as the maze, the discrimination box, and problem solving. He had introduced the concepts of the goal gradient and the habit–family hierarchy, and he had argued in the first and strongest of all these papers for the functional nature of the conditioned response and its properties of extinction, generalization, and spontaneous recovery. He was at the height of his interest in developing what he called "miniature systems" that applied principles of the conditioned response to more complex learning situations, using a rigorous deductive method to generate testable predictions from carefully stated postulates.

He lent me some of his more recent "notebooks" to see if I could find an idea for a thesis that he would approve. He wrote in these notebooks every Sunday, jotting down ideas for experiments, thoughts for future papers, goals to be achieved, and sometimes his personal feelings about people and events of academic interest (see Hull, 1952). They were fascinating reading, but I found nothing there that I wanted to work on. When I returned them, I said that I had previously worked on verbal learning and that I thought I could stay within his general plan by attempting to apply some conditioning principles—generalization and differentiation—to various phenomena of verbal learning and forgetting. The idea was "around" at the time—I had discussed it with Elliot Rodnick and with Carl Hovland—but it had not been exploited. He asked me to show, in a preliminary way, how I would do it and demonstrate that some productive experiments would be generated. I did this (actually as part of my "prelim" examination) and he acceded, with the stipulation that I would construct a miniature system, with axioms and derivations in the kind of logical format that he had himself worked out.

My time at Yale was limited to just one year, for both marital and financial reasons, so I took every examination going (including the two foreign languages required then) and strove to make the most of every moment. I left in September, with some regret, to go back to Smith to teach again and to work on my dissertation. Since I taught full-time, I spent two years on the dissertation, traveling to New Haven as often as possible to discuss progress with Hull. We gradually grew well acquainted and I became truly fond of him. He looked like my father, who even had a limp rather like Hull's, but I don't believe my fondness sprang from some Freudian depths. He was interested in what I was doing, read everything I sent him carefully before I came for discussions with him, and often talked freely about his own goals and worries (especially his fear that his health would not permit him to complete his "grand plan").

The dissertation was completed by the May 1st deadline in 1938. It included a long, theoretical paper—my "miniature system" linking the concepts of generalization and differentiation to paired associate learning, forgetting, and various transfer phenomena (see Gibson, 1940). There were also four experiments, testing some of the theoretical predictions (Gibson, 1939, 1941, 1942). Pavlov would probably not have recognized my definitions of generalization and differentiation. I thought of generalization as confusability, and of differentiation as a process leading to hitherto unachieved discrimination. I did not use the concept of extinction as inhibition that could be compounded, a possibility that was explored by others. I felt wary of it, as I did of the concept of reinforcement as Hull used it, though I didn't and couldn't make that clear at the time. I was not an S-R psychologist at heart, I suppose, for all my attraction to animal research. I didn't believe, then or now, that external reinforcement, in Hull's sense, was applicable to perceptual learning. I thought that differentiation, once achieved, was not extinguishable, even though a subject was commanded to learn new responses to whatever had been differentiated. However, it wasn't possible to say all this in so many words at the time.

Work and Family

Smith College had no nepotism rules and continued to employ both my husband and me as full-time members of its faculty. Full-time was really full-time, however. One taught three classes a term (sometimes only two courses, with one repeated) and had numerous advisees, committee duties, and so on. The atmosphere was that of a college rather than a university, but the faculty was excellent, a group of scholars carefully picked by the president, William Allen Neilson, certainly one of the great college presidents of all time. He was a Shakespearean scholar, so the arts were rather heavily emphasized. The faculty was about half men and half women, a balance that Mr. Neilson liked, but he saw to it that there were distinguished women on the faculty and that they were promoted at the same rate as the men. There were Mary Ellen Chase, Eleanor Duckett, Grace Hazard Conkling, Gladys Anslow (a physicist), and a number of excellent women in music, art, and philosophy.

A number of my own forebears had come from Northampton, including Eleanor Strong, whose first name was given to me, along with some of her silver spoons and a silhouette portrait. I felt at home there and loved the place, as I still do.

My husband and I had a son in 1940, I was finally promoted to assistant professor, we acquired a lovely, very old house with five fireplaces, and life was exceedingly busy. Trying to care for an infant and a house, teach a rather heavy load, and perform all the incidental duties of supervising honors theses, new laboratory assistants, and many advisees took all the time there was. My thesis was rewritten and published, but my only chance for research was via an occasional master's thesis or honors thesis, not actually performed by me. I did in that way get several more of the theoretical predictions in my thesis put to experimental test, but I couldn't do it myself. It seems to me that one can do two jobs at a time, but not three. The teaching and the family I could manage, but not the research, too. I am afraid that restriction may still hold, despite modern reorganization of family life, with the father taking more responsibility. It does not have to be frustrating, though, when there are plenty of rewards and when the period does not last too long.

War Years

During these years the Depression had been desperate, but Smith and its faculty came out of it unscathed and probably the better morally for attempts at forming a teachers' union and assisting local factory workers. It was the threat of war in Europe, and possibly our own involvement in it, that began to change the calm academic life. We had a number of refugees from European countries at Smith, the last one to arrive in the psychology department being Annalies Argelander, a German developmental psychologist. Her husband, Jerzy Rose, a physiologist, accompanied her. They arrived in the nick of time, having had to leave all their personal belongings, even books, in Poland. On Pearl Harbor Day, my husband was away at a meeting of the Psychological Round Table (a kind of Young Turks experimentalist group that excluded women), but he arrived home already wondering what he would do in the coming months.

Early in 1942, he joined a group of psychologists in the Training Command of the Army Air Force. They were to construct tests for selecting personnel for aircrews, and it was thought that a psychologist specializing in perception could help. He left for Washington, D.C., less than halfway through the second semester. I took over some of his experimental duties, Fritz Heider took over others, and the social psychology course was carried on by Richard Sollenberger, a social psychologist and good friend at nearby Mt. Holyoke College. But Sollenberger joined the Training Command too before the term ended, and I found myself grading examinations for social psychology again, little better prepared than I had been some years before.

When the Training Command was finally located in Fort Worth, Texas, I left in July with our son, then two years old, to join my husband. There were many psychologist friends there with their wives—the Geldards, the Kemps, the Sollenbergers, the Ghisellis, the Hennemans, the Deemers—and others came and went constantly. For a short time, I rather enjoyed the experience of socializing with the women and doing a not-very-useful job for the Army Emergency Relief. Our daughter Jean was born in Fort Worth in 1943, and a few weeks later, my husband was moved to Santa Ana, California, to an air base near Los Angeles. I followed again, after a while, and lived in a succession of temporary quarters in various areas of Orange County, a community so reactionary that the local newspaper featured editorials inveighing against free public education.

We stayed in California until April 1946. Again there were some good friends, especially Bob Gagné and his wife Pat. The young noncommissioned officer psychologists who worked with my husband and Bob were rather like a group of graduate students. They did research on aircraft recognition and space perception, and they made training films. The entire time was a kind of latent period for me; I discussed their research with them sometimes, but there was no way that I could be truly involved in it. As so many women have done, and still do, I wondered whether I would ever be able to make it back. But the rather empty life I led there taught me something useful. If I had ever had any doubts about the desirability of an academic career and the joy of research, as opposed to a life of feminine socializing, community service, and women's clubs, they were thoroughly dispelled. The boredom of it became awful. Teaching and research were glorious to contemplate.

Postwar Years

Although I did not have tenure, Smith College took me back, with my husband, and things returned to a new equilibrium. I taught better for the sure knowledge that I was doing what I wanted to do. Northampton was beautiful, a fine place for the children to grow up, and it was good to see the old friends again. I taught a heavy load, served on innumerable committees, and spent all the time I could with the children. It all worked pretty smoothly, because we located a young Japanese girl, fresh from an internment camp in Colorado, who came to live with us and help with the children. Sadako ("Sadie" as she wanted to be called because it sounded American) was wonderful and became a kind of foster daughter to us. She stayed with us for three years, until she went off to nursing school and we exchanged Northampton for Ithaca.

Although Northampton seemed like paradise after four years of uncertainty and life in unlikely places, it became clear within a year or two that Smith was no longer the right place for my husband. The students were still first-rate, and he had certainly never been a male chauvinist, but there was no emphasis on research or even much reward for doing it. The era of government-supported research arrived immediately following the war, and he quickly got a navy contract (as they were generally called at first) to work on problems of perception, such as gradients of surface texture and motion, that were leading him to a new theory of space perception. The problem was that there were few graduate students to work with him and, except for me, almost no one to talk to about his new ideas. Koffka was still alive, but he was interested only in his own views. Fritz Heider was there for a while, but he moved away to Kansas. My husband became a bit restless. Various places made tentative gestures, but none of them seemed an improvement. I asked him one day where, of all universities, he would most like to go. His answer was "Cornell," and like a miracle, a letter came the next day from Robert MacLeod, the new chairman of the psychology department at Cornell. Of course we went, but there was no job for me. However, it was not a repetition of the war years, since I would have the time and the opportunity to do research and to be part of the community of psychologists in a big university again. I was given the title (without pay) of Research Associate at Cornell.

Life as a Research Associate

Freedom to do research is one thing, but more is required—a laboratory, some support for equipment, and a source of subjects at the very least, plus of course some good ideas. Cornell did not give me the opportunity to seek my own outside support at first. In some desperation, I accepted an offer from Howard Liddell to work on his project at what was known as the "Behavior Farm," a laboratory in the country about three miles from the university. It was literally a farm. The laboratory was surrounded by fields and pastures, and the subjects were sheep and goats. Liddell's project was to investigate experimental neuroses by establishing conditioned reflexes based on shock in these animals. I was to photograph leg movements and monitor breathing and heart rate. In short, I was to become a "sheep shocker." Liddell had been at this project for some time, and I had often heard him speak at meetings. He was an exceptionally entertaining speaker, and I began my job without too many qualms, prepared to learn about experimental neuroses.

To my dismay, I found that Liddell himself almost never went to his laboratory and that the research was run by the farm manager, his brother-in-law Ulric Moore. Moore was a very pleasant man, an expert with apparatus, and a lover of gadgetry, but he had no training as a psychologist. The research had no clear aim except to produce neuroses in the animals. There were several show cases, always brought out as demonstrations for guests. One was Brown Billy, a mature goat who was a real performer. He lifted his foreleg and rolled his eyes for the visitors from the Rockefeller Foundation and was rewarded with cigarettes. We made miles of records of heart rate, breathing, and movements, but no one appeared ever to read the records. The animals obviously did not like to be shocked, but so far as I could see they were no more neurotic than I was, and even if they were, could one generalize from such a procedure to a human neurosis?

Although that aspect of the work seemed, frankly, humbug to me, I was interested in the theory of conditioning. Did a shock to an animal's foreleg produce a flexion that was copied, like a conditioned reflex, when a buzzer or some neutral stimulus had preceded it long enough and then was presented alone? Did the flexion come to anticipate the shock (the classic expectation), and why should it if the animal got the shock anyhow? What if the animal escaped the shock by anticipating it with leg flexion? Did the pattern of behavior resemble that of conditioning with inevitable shock? These questions were of popular interest at the time, relating to the dual-factor theory of reinforcement introduced by Hobart Mowrer. I trained kids (young goats) in a situation permitting free locomotion with varied conditions of shock (inevitable, avoidable, and random) and recorded their behavior in detail. I did come out with a two-factor theory, though not one very similar to Mowrer's. The shock had two functions: first, to reinforce an emotional state that instigated some kind of defensive response (locomotion backward always came first); and second, to suppress that response as it was found ineffectual. Inevitable shock did not increase the probability of recurrence of the same response, as a simple Pavlovian view would have demanded, but produced variable reactions, a kind of continuous trial and error (Gibson, 1952).

Perhaps that research had some implications for a theory of neurosis, but I did not think so, and I began to concentrate on something that interested me more: ethological observations of maternal–infant interaction in goats. A newborn kid is a very interesting animal. It gets up and walks almost at once, looking curiously at the world around it. I was interested in olfactory bonds in the maternal–infant relation, and so on one occasion I was taking the kids from the mother, before she could lick them, and bathing them in a detergent. I had bathed the first kid as its twin began to appear and wondered what to do with it, in a hurry, while I dealt with the newcomer. Moore was watching. He said, "Put it up on that pedestal"—a camera stand about five feet from the floor that we used frequently when filming behavior. The platform was only large enough to hold a camera, but the little animal stood there motionless, watching the scene. After all, he had evolved from ancestral cliff dwellers and retained some of their genes.

My second year at the Behavior Farm was devoted to this work, and with Moore's help, I made a nice motion picture of maternal–infant behavior in goats. I was engaged in a controlled rearing experiment (pairs of twins separated—some reared with the mother, some reared under other conditions of isolation or human companionship)—when my faith in the possibility of working in that laboratory was shattered. One of my groups was given away during a weekend absence of mine. The experiment was ruined. I looked for a new job, but working with the kids was instructive and rekindled my interest in development.

About this time, our old friend Bob Gagné joined Arthur Melton at an air force laboratory that had ample funds for supporting outside research. They were interested in perceptual learning, and so was I. My husband and I were the beneficiaries of a very generous grant from them, with my own projects to focus on perceptual learning, especially distance perception. Nothing could have suited me better. I began (at Bob's suggestion) by reviewing and putting together all the experimental literature on perceptual learning. It extended back to 1858 (A. W. Volkmann's experiment on the effect of practice on the two-point limen on the skin), and no one had ever got it together before. I could see a brand-new field for research and theory. The experiments I did at this time were fairly traditional psychophysics, involving special kinds of practice, mostly done outdoors in a very large field. I had some good graduate-student assistants, and the work was fun. We collected data all summer, while the weather was clement. The subject situation was any experimenter's answer to a prayer. Subjects arrived every day from nearby Samson Air Base via bus, attended by a sergeant. They were new recruits, since Samson did not train flying personnel. The trip to Ithaca was part of their indoctrination, and they were glad to get away from the routine of shots and tests. By the third summer, we had a whole series of experiments lined up for them and kept them busy all day, but they enjoyed it.

The "field experiments" were very straightforward investigations of the effect of training on all kinds of distance judgments—absolute, relative, and fractionation—over quite large distances (Gibson and Bergman, 1954; Gibson et al., 1955; Purdy and Gibson, 1955). Theoretically they were not terribly exciting, but I have always felt rather proud of them. A lot of important muddles got straightened out, such as the confounding of perceptual learning with response bias and the danger of generalizing from photographs to real space. It also became pretty obvious that young human adults are very skilled, without training, at making relative judgments of distance, even over a large area, and that perceptual constancy for stretches of distance over the ground is good. At the end of the three years this research occupied, I felt that a new field for learning—perceptual learning—had been staked out.

Meanwhile, the urge to theorize, acquired from all my mentors—my husband, Hull, and even Koffka—was still strong. My husband and I spent many hours arguing about perceptual learning, what it really was and how it happened. Generalization and differentiation—concepts exploited in my thesis—were prime candidates for describing the process. We thought that perceptual learning was a change in what was perceived, not in the association of a response with a stimulus, and that the change was best described as increasing differentiation or decreasing generalization. It could be described as a narrowing of the class of things or displays responded to in some predesignated way. I did an experiment with a set of scribbles, originally all very confusable, that we designed to serve as an illustration of our ideas. The experiment was developmental, too, with subjects of three age groups, and showed that the kind of change we hypothesized occurred developmentally, as well as with practice. The result was a paper, "Perceptual Learning: Differentiation or Enrichment?" (Gibson and Gibson, 1955), one of only five that we have written together. The results of our collaboration were apt to be good, but the arguments always got too heated.

I did work with my husband for two years after this, however, on a navy contract for which he was investigating the perception of motion. We did some nice experiments and I was initiated into the concept of detection of invariants (Gibson and Gibson, 1957), an idea that gradually came to fit very well in a theory of perceptual development.

A new colleague, Richard Walk, came to Cornell about this time and was assigned to teach the learning course. He was interested in perception, too, and we soon planned some collaborative experiments on perceptual learning in early life. The experiments were done with rats and involved rearing groups of them from birth with exposure to various cut-out shapes hung on the walls of their dwelling cages. When they were three months old, we trained them on an appropriate discrimination to see if there was transfer from the "early experience" of viewing the cut-out shapes. Aha! Early experience worked in our first experiment (Gibson and Walk, 1956), and the National Science Foundation gave us a generous grant to delve into the problem. Those experiments did not pay off very well. Sad to say, we never got results again that looked as convincing as the first experiment. I now think it is simply the case that one doesn't have to learn to see triangles and circles. Differentiating them was easy, even for rats that had been reared in the dark (Gibson et al., 1959). The work constituted a great learning experience for me, however, and I began thinking far more seriously about what must go on in perceptual development. Shapes do not get etched on the brain, nor do we learn to see them because somebody reinforces us for it. My urge to work on perceptual development grew very strong.

One of Walk's and my experiments produced some unexpected, serendipitous results. We were engaged in rearing a number of rats in the dark, and we decided to do something with them in addition to giving them the lengthy and boring discrimination training after bringing them out. A replication of Lashley and Russell's (1934) experiment on depth discrimination following dark rearing seemed a good idea, if we could find a way of testing the animals before they had experience in the light (as they necessarily had, with the jumping-stand method used in that earlier experiment). Walk had worked in the army with trainees learning to do parachute jumping from a high platform, and I had a long-standing aversion to cliffs, dating from a visit to the Grant Canyon with a small child. We decided to build a simulated cliff and see whether the animals would step off, even when they had never seen a drop-off or walked over one. My kid had stayed on a high camera platform, so something of the sort seemed a possibility. Thomas

Tighe (our research assistant at the time) and I hastily put together a contraption consisting of a sheet of glass held up by rods, with a piece of wallpaper under one side of it and nothing under the other side except the floor many feet below.

A few rats left over from other experiments got the first try. We simply put them on the glass and watched them. They walked around nonchalantly, apparently not caring what was under them or even looking to see. We had to make them look somehow. We put a board about three inches wide across the division between the surface with flooring and the unlined glass, and put the rats on the board. Would they descend randomly to either side?

What ensued was better than we had dared expect. All the rats descended on the side with textured paper under the glass. We quickly inserted some paper under the other side and tried them again. This time they went either way. We built some proper apparatus after that, with carefully controlled lighting and so on, to be ready for our dark-reared animals. It worked beautifully. They behaved like the light-reared animals. Rats (hooded ones), at least, didn't have to learn to see depth at an edge to avoid stepping over it (Walk et al., 1957). Of course, other animals might. The National Science Foundation was good to us again, and we proceeded to investigate various aspects of the problem and compared the behavior of many young animals, including human infants (Gibson and Walk, 1960b; Walk and Gibson, 1961). We couldn't very well rear the infants in the dark, and we had to wait until they could locomote on their own to use avoidance of the edge as our indicator of depth discrimination, but infants of crawling age did avoid the "deep" side. They may have learned something in the months before they could crawl; but whatever it was, it could not have been externally reinforced, since the parents never reported that the babies had fallen from a height.

An invitation to spend a year at the Institute for Advanced Study in Princeton seemed to present the perfect opportunity to go away and think hard about perceptual learning and development, perhaps to write a book about it. I got there full of determination. But first there were many things to finish and to write up. I wrote a few chapters, but they did not satisfy me, and I spent a lot of time reading. It is easy to collect material and summarize, but thinking is hard. I did some chapters for other books—one for Paul Mussen's *Handbook of Research Methods in Child Development* (with Vivian Olum) on methods of studying perception in children (Gibson and Olum, 1960a) and another on perceptual development for an NSSE yearbook (Gibson, 1963a). Somewhat later I did an *Annual Review* chapter on perceptual learning (Gibson, 1963b). These all helped to formulate the field I was trying to think about, but I still needed time to let my ideas mature.

Something happened then that postponed the book for a long while, but provided the maturing time. Two Cornell friends, Alfred Baldwin and Harry Levin, both professors in the Department of Human Development, came to visit me at Princeton with the proposal that I join them in a kind of interdepartmental consortium to generate some theories about the reading process and do research on it. We were assured of support from the U.S. Office of Education, and the research could be basic, not necessarily oriented toward instructional programs.

The idea of working on reading had never crossed my mind, and I resisted persuasion at first with the argument that I had only recently got in clear focus the area of psychology that was going to be mine: how perception develops in children, what perceptual learning is, and how it comes about. Eventually the counterargument of my friends began to make a lot of sense. The point was that perceptual development and learning are absolutely basic to acquisition of reading skill, and that they could be investigated with profit in the setting of a task that was anything but artificial and was, in fact, of great relevance to society. It was a good argument and it succeeded. The next fall the consortium convened and arranged a schedule of meetings. The group included Harry Levin and Alfred Baldwin (then from the Department of Child Development), Charles Hockett from Linguistics, myself and my husband from Psychology (although I was still only a research associate), and a number of younger people—Rose-Marie Weber, John Watson, Anne Pick, Harry Osser, and others. The senior people had each prepared a fairly detailed research proposal, so the meetings began with presentations to the group of what we hoped to do and there was lively discussion. We brought in people from outside as well. The cross-fertilization between psychology and linguistics was very useful, at least for the psychologists. Most of us were only superficially acquainted with linguistics, but we studied it eagerly. "Psycholinguistics" was young then, and applying it to the reading process was quite new. It was a splendid breeding ground for new ideas.

Anne Pick and Harry Osser worked with me. We started out, as I had planned, on a study of perceptual development, choosing material appropriate for reading (Gibson et al., 1962a). The material was a set of graphic forms that were similar to real letters (we thought) in that they incorporated the same contrastive features that I thought were used to distinguish Roman capital letters. Later we spent a lot of time trying to find out just exactly what those features were, but in the beginning the choice was altogether intuitive. We planned ways of transforming our original set of forms that would be relevant or irrelevant for developing perceptual skill in visual discriminations required in reading. One relevant means was the obvious right–left reversal; one that we considered irrelevant was a transformation accomplished by photographing the original form at a slant. Preparing the material and getting data from five age groups consumed a great deal of time, but before the year was over, we began a new project not preplanned but spawned from our growing interest in psycholinguistics.

It seemed pretty obvious that a good reader does not read letter by letter. Were words the ultimate units, as many educators have thought? Surely there is something that transfers to new words, and for an able reader, it did not appear to be a matter of decoding letters to sound, one at a time. We all became very interested in writing systems and in the nature of English orthography. That system, so despised by some as being unpredictable and even whimsical, surely had some order in it, even though the language contains spellings like "tough" and "wrought." We began to look for rules in the English monosyllable, getting help from a paper by Benjamin Whorf (1940) and a book that was just appearing by Charles Fries (Fries, 1962). Whorf had a formula for a spoken monosyllable, which appeared to have some applications to its spelling, especially when combined with Fries' analysis of consonant clusters in English spelling that are in so many cases constrained as to position in the syllable (for example, WR only occurs in initial position and GHT in final).

I came up with the idea that units might be constructed by predictable spelling-to-sound correspondences that were constrained by position in a syllable or word. We set out to test this notion by constructing a list of monosyllables that were not words, but could be, in that they were orthographically legal combinations. They began with a constrained consonant or consonant cluster, followed by a vowel or vowel cluster, and finished with another constrained consonant cluster (GLURCK, CLERFT, etc.). One could then exchange positions of the consonant clusters and—Voilà!—an illegal monosyllable would be created (RKUGL, FTERCL, etc.). These "pseudowords," as we called them, were presented to skilled readers (college students) with brief tachistoscopic exposures, the legal and illegal combinations randomly ordered. The experiment worked as expected: The legal combinations were far easier to read (Gibson et al., 1962b). Other methods of presentation were tried (obtaining thresholds, presenting choices for selection of the preexposed letter string, etc.), with totally replicable results. Arguments arose about the basis for the facilitation, with the pronounceable property of the legal combination surfacing as a favored theory. The subjects presumably pronounced the letter strings to themselves and then remembered better the ones that rolled easily off the tongue. Thus the "phonemic recoding" notion of word recognition emerged. I decided to probe it in the most direct way I could devise: to do the experiment with "pronounceable" and "unpronounceable" pseudowords with congenitally deaf subjects (Gibson, et al., 1970).

My assistants, Albert Yonas and Arthur Shurcliff, and I journeyed to Gallaudet College in Washington, carrying our equipment on our laps in a Lear jet. The college provided the utmost cooperation, and we tested a number of deaf subjects. Interpreters (signers) were provided, and I paid our way, since the college refused money, by giving a colloquium that was simultaneously translated into sign language. The experience and the experiment were interesting in more ways than one. The deaf students differentiated between the pronounceable and unpronounceable items quite as well as hearing subjects. If they had shown only a mild facilitation, one might have concluded that they were doing a little phonemic recoding, despite the fact that none of them spoke comprehensibly; but the whole effect was there. A puzzle thus remained: What did explain "pronounceability"? We analyzed our data carefully for the possible effects of sequential probability of letter sequencing, but in the counts we had available for use in a regression analysis, it did not appear to be playing a role. Our conclusion was that the legal constraints in the spelling patterns of English—beyond mere sequential frequency—provided a structure that could be learned even without hearing the pronunciation.

For many years, with the help of a number of graduate students, I tried to find out how children accomplished this learning. We found that teaching it to kindergarten and first-grade children by any kind of deliberate instructional intervention was remarkably ineffective. And yet, four separate experiments by graduate students of mine showed that the average child from a middle-class neighborhood knew a lot about it in third grade. I believe now that it is learned by a process of abstraction or induction, much as a child learns speech. Of course, we don't understand a lot about how a child learns speech, either, but what little we know applies pretty well to learning the structure of the English writing system. That children make a considerable beginning by themselves has been demonstrated (Lavine, 1977).

The parallel just drawn seems to apply to structural rules that are analogous to syntax in language. But there is the semantic aspect as well, and once again, I do not think meanings of written words are learned entirely by "coding" or association with their spoken counterparts. Children infer the meanings of some words from context as soon as they begin to read. The process continues and develops for many years as the learner begins to understand that English spelling is not simply phonetic, but is morphophonemic. There has been very little work on the latter aspect of learning the system (Chomsky, 1970; Gibson and Guinet, 1971), but it is a good guess that learning about morphology and roots in spelling only begins after third grade. When does a child learn that "mishap" is not pronounced like "bishop" because it contains two morphemes that must be treated as such? Or that one can predict the vowel spelling in many nouns by knowing their adjectival counterparts (factor, factorial; manager, managerial) and countless other generally unanalyzed relationships?

My life as a research associate lasted for sixteen years and culminated in an unforgettable year at the Center for Advanced Study in the Behavioral Sciences. Lee Cronbach, Richard Atkinson, myself, and a couple of others were supposed to provide a "cutting edge" to promote the application of sound, scientific, psychological principles to education. We did hold a conference on reading, and we had some discussions, both closed and open, but we were all busy on our own writing. I got back to my book on *Principles of Perceptual Learning and Development*, starting absolutely fresh and much the wiser for the experience of the years that intervened since I began it at Princeton. I went home in August with the book nearly half-completed and the whole plan in mind.

Professorial Dignity

During the following year, the big break really came. My husband won a Career Professorship from the National Institute of Mental Health, which meant that the university no longer had to pay him. The nepotism rules apparently had something to do with finance. But I believe the climate of thinking was changing, too, and when my friend Harry Levin made a strong effort on my behalf, the rule (if it was ever actually on the books) gave way and I became the (at that time) only female full professor on the faculty of the Arts College at Cornell. (I was not the first one, however; my friend Patricia Smith preceded me.) Of course, I was delighted to be appointed Professor of Psychology—only half-time, but still, a Professor! I could teach an undergraduate course, serve as a graduate student's thesis director and sign my own name, and enjoy other wonderful privileges and duties that a professorship bestows. I finished my book in the ensuing couple of years, considerably aided by teaching a course in Perceptual Learning and Development. The research continued, half-time, still pretty much focused on reading, but the book represented better the breadth of my interests.

There were other "breaks" and honors in those years, too. I was elected to the prestigious Society of Experimental Psychologists (the only woman); I was elected president of the Eastern Psychological Association (a marvelous occasion, because EPA provided a free suite that must have been decorated for Near Eastern oil barons and their retinues); I went on a two-week, back-breaking lecture tour for Sigma Xi; and I won the Century psychology prize for my book.

The last of these honors was especially precious to me. The Century series included many of the "greats" in my life—Hull, Tolman, Hilgard, Marquis, and many others. To be part of it and to win its prize were more than I had dared hope. Psychological themes and paradigms had changed greatly since these mentors wrote, as another Century winner—one by my colleague entitled *Cognitive Psychology* (Neisser, 1967)—attested. But I had thought and worked very hard on my book, and it seemed to me to represent something new—the putting together of a field I considered very important and the search for suitable principles to organize it and provide hypotheses for investigating it. I had long ago abandoned the old S-R concepts, since they seemed to be of little use and to force one into double-talk when perception was the matter of interest. Perceptual learning I conceived of not as a change in a response, but as a change in what was perceived. The description of this change during learning or in development I found was best viewed as differentiation, not enrichment (Gibson and Gibson, 1955) by the addition or association of anything—a response, or another "stimulus" (cf. S-S learning). Indeed, I found that the concept of a stimulus was not useful if one was truly concerned with how the infant comes to extract the necessary information from a real world that he must cope with in an adaptive fashion. He does not perceive stimuli; he perceives people and places and objects and events, and he acts in relation to them. Calling them stimuli simply prevents a proper analysis of the information that the environment affords him.

The information-processing revolution was with us at the time I published the book, and I tried my hand at a few experiments in this tradition, using reaction time for deducing what was going on "inside." But I found myself discontented with that approach, which made assumptions that I could not accept about the "construction" of the world and which seemed even more bent on contriving artificial situations for research than had the S-R psychologists. It also seemed to me to bypass everything we had learned about evolution and adaptation —ideas that cannot be disregarded when one contemplates development.

The recent years are hard to describe, since one cannot look back at them in a suitably contemplative fashion. I continued with research on the reading process (not the instructional process), placing most emphasis on learning to read, so I worked mostly with young children. When I won my first (and probably only) sabbatical leave, I applied for a Guggenheim fellowship, and my long-time colleague and friend (chairman and dean, too, but I play down those relations, as does he) and I set out to write a book on reading. It seemed to us badly needed, since the last book with a wide coverage of the psychology of reading had been published more than half a century earlier (Huey, 1908). That book had been good, and we borrowed part of its title, *The Psychology of Reading* (Gibson and Levin, 1975). It seemed to us that teachers, parents, and other interested persons did not need instructional programs nearly as badly as they needed an understanding of the reading process and the kind of learning that went into becoming a skilled reader. Needless to say, the book's approach emphasized description of the information that the reader had to extract, the rulelike structure that characterized it, and the role of the learner as a person with motivation to find out and to abstract order. The latter aspects seemed crucial to us, and we suspected that what happened in schools frequently discouraged rather than fostered them. The psychology department at MIT offered us a home for a semester of intensive writing. The welcome and assistance we encountered there cannot be exaggerated.

When that book was completed, a little change seemed in order; one gets a bit stale on a topic after 650 pages. I had always wanted an infant laboratory for my students (I had not had one since the work on the visual cliff), and the time was propitious for setting it up. It has been in full swing now for something over five years, with development of the perception of invariants in infants as its principal theme for investigation (Gibson et al., 1978). Several theses and a number of experiments have come out of it so far; and there my story runs out.

Epilogue

This is an epilogue in a sense, but not an epitaph, because there is so much more that I want to do. But some sort of ending, perhaps a moral, is called for. One moral comes to mind at once, cliché though it be: Nothing succeeds like success. After sixteen years of second-class citizenship, the honors, once the ice was broken and I had attained the dignity of a professorship, came one upon another. My old college, Smith, gave me an honorary degree. Cornell gave me an endowed chair, the Susan Linn Sage Professorship. That was a first, since no woman had held one before in the university's history. Named for a woman it may have been, but the chair had been occupied for nearly a century by men. My old graduate school, Yale, gave me the Wilbur Cross Medal. I was elected to the National Academy of Sciences, the National Academy of Education, and the American Academy of Arts and Sciences. I was awarded the G. Stanley Hall Medal by Division 7 of the APA and the Howard Crosby Warren Medal by the Society of Experimental Psychologists. I was elected president of Division 3 of the APA and awarded an honorary membership by the British Psychological Society. I have served and am now serving on some truly prestigious committees. Do I wonder, now and then, whether I am serving as the token woman? Yes, I sometimes do.

Of course, I don't refuse for that reason. Better to have a token woman in these things than none at all. I think I can truly say that I have never felt any real bitterness about my inferior status during those sixteen years that I did research and paid the university a large overhead on my grants and contracts. I do lift my eyebrows, however, when people tell me how lucky I was to have all that time to do nothing but research. The people who tell me that, of course, are never women.

What does a woman need to succeed in a profession that seems to have evolved chiefly for men? She needs all the obvious things like education and drive, of course. Some women have forgone the privilege and joy of a family in order to achieve academic success. I saw many of them at Smith and at Mt. Holyoke. I am glad I did not do that. The family may introduce some obstacles, especially if one puts them—husband and children—ahead of oneself. But it is certainly worth it, and it seems to me that it works out in the end, provided one's husband is tolerant of one's ambitions, encouraging, and recognizes one's worth. Mine has always been such, and I am glad I have this chance to say so publicly. Helpful colleagues and first-rate graduate students are very important, too. Here one has to be lucky, as I have been. Most of all, one has to want the kind of life that teaching, research, and scientific fellowship offer. I cannot imagine any other kind of life being so satisfying. Sometimes I have felt that I had two lives and that one was temporarily being short-changed, but I believe each is the richer for the other.

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