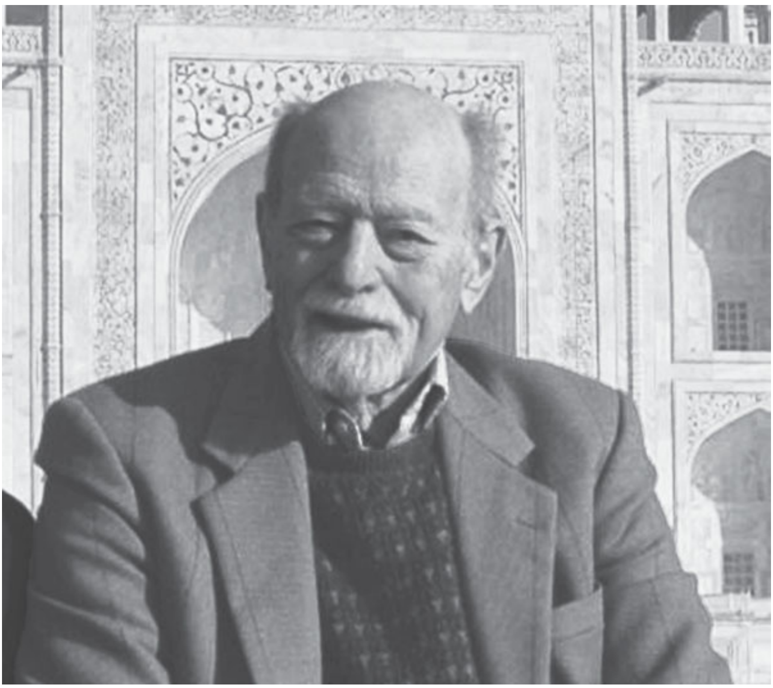

Richard Held

Born:

New York, New York,
October 10, 1922

Education:

Columbia College, B.A. (1943), B.S. (1944)
Swarthmore College, M.A. (1948)
Harvard University, Ph.D. (1952)

Appointments:

Instructor, Assistant Professor, Associate Professor, Professor and Chair, Department of Psychology, Brandeis University (1953–1963)
Member, Institute for Advanced Study, Princeton, NJ (1955–1956)
NSF Senior Research Fellow, and Visiting Professor (1962–1963)
Department of Psychology, Massachusetts Institute of Technology (MIT) (1962–1986)
Department of Brain and Cognitive Sciences, Massachusetts Institute of Technology (MIT) (1986)
New England College of Optometry (1995)

Honors and Awards (Selected):

American Academy of Arts and Sciences (1967)
Society of Experimental Psychologists (1971)
National Academy of Sciences (1973)
Honorary Degree of Ocular Science (The New England College of Optometry) (1977)
Glenn A. Fry Award (American Academy of Optometry) (1979)
Howard Crosby Warren Medal (Society of Experimental Psychologists) (1983)
Doctorat Honoris Causa, Free University of Brussels, Belgium (1984)
Kenneth Craik Award, Cambridge University, England (1985)
Galileo Award (American Foundation for Vision Awareness) (1996)

Since childhood Richard Held was intrigued by the illusions of vision and their motor consequences. He and his colleagues made extensive studies of the effects of sensory rearrangement and have modeled the adaptive processes that reduce, and sometimes eliminate, the induced errors. He pursued studies of the visual capacities of animal and human neonates so as to test the implications of plasticity for early development of spatial vision and motor control in accord with the following logic. If the adaptive process yields full and exact compensation in the mature animal, then it should be capable of compensating for any neonatal errors and may even account for development itself. Although Held pursued many sidelines, he always returned to this issue.

Richard Held

Childhood

I distinctly remember seeing the phenomenon and puzzling about it on that boat ride so long ago. It was 1928 and I was a 5-year-old and only child. My parents had taken me for a holiday ride on the excursion boat that sailed across New York Harbor from Battery Park to Coney Island. It was a bright sunny day, and once the boat was in the channel I scanned the water idly looking at birds and boats. At one point I shifted my gaze to the deck on which our chairs sat. I felt a mild shiver as I watched the deck. It was moving under me. What was happening? After a minute or two, I realized that the deck was not going anywhere, it just appeared to move: I would much later learn that this was a paradox that exemplifies the difference between perception and physics. After a time, the motion slowed and then stopped. What was this motion? Could it be confused with real motion? What was real? I asked my parents, but they couldn't or didn't clarify the mystery.

Of course at the time I didn't suspect that this incident might be the first indication of a line of interest that has remained throughout my life. What is the relation between the physical description of the world and its perception? Clearly, we all require what has been called "veridical vision" to survive. We always need to distinguish a lion from a lamb, and we continually need to grasp the sizes and distances of objects to manipulate and locomote without damage to ourselves. The issue posed by illusions, transient ones in particular, is how do their perturbations of appearance square with the need for stability and permanence of the perceived environment?

This incident says still more about me and my origins. The idea of taking the family on an excursion through New York Harbor to Coney Island and back was a very bourgeois notion. It was cheap. It was relatively safe. In the hot summer the harbor breezes were cooling. The watery scenes were mildly interesting, and the destination was a long pier leading to an amusement park and hot dog stands that provided entertainment and an economical lunch. And, indeed, my immediate family, father and mother, were quite a conventional couple. My father, whose education was limited by the early death of his own father, was an export broker whose business was a marginal success. My mother was an artist who worked for a time at fashion design. She took courses at the Metropolitan Museum of Art and often took me there for visits, which gave me an early familiarity with visual arts. She lived to be age 97 and encouraged my interest in all aspects of vision except for the idea of becoming a vision scientist or, for that matter, any kind of scientist. After all didn't scientists live on the fringes of society and how did they make a living?

Growing up my interests turned first to taking things apart—clocks, locks, and other expendable gadgets—to see how they worked. Then on to making things—electric motors and radios. I was the typical boy-scientist, boy-engineer. My favorite fiction was the Tom Swift series of the adventures of a boy inventor. I soon moved on to books on electricity and other scientific topics. My grammar school was the same one my father had attended before me, Public School 6, Manhattan, whose students walked to school through what was called the Silk Stocking district. The elderly teacher of the first grade even remembered my father. To my parent's surprise I did well in school. They hadn't expected their child to excel in school. After all, they hadn't. I remember one particular revelation. During my seventh grade, a school psychologist gave a test to each and every student in my class. A few days later I was called out for an interview with the psychologist together with a girl named June. It so happened that I was enamored of June but painfully shy about any expression or discovery of my secret. To be called out together with her was upsetting because I thought the psychologist had somehow discovered my secret and was about to reveal it to her. But this was of course my fantasy. I must have had more faith in psychologist's insight then, than I have now. It turned out that he had only chosen to interview the two of us because we had had the two highest scores in what turned out to be his intelligence test.

As I grew older, when not in school I spent a lot of time roaming Central Park with my good friend and schoolmate Alfred Halliwell. In warm weather we roller-skated on the hard-topped paths from one end to the other of the Park. In snow we did the same on Flexible Flyer sleds. We always stopped for a hot dog at the small lakeside tavern.

We talked about what we wanted to do when we grew up. Alfred wanted to build things. Last I heard of him he was an electrical contractor in Connecticut. Occasionally we were joined by Robert Primoff, a boy who was more interested in ideas than athletics. We spent a lot of time talking about science and propounding notions of truth and reality; questions of epistemology and ontology as I later learned when I took one of several courses in philosophy. In the interests of science we broke apart a large single-cell dry battery that made an epic mess resulting in the phrase, "Remember the battery!" a cautionary phrase we used when confronted with potential disaster.

Stuyvesant High School

During my eighth grade it was time to apply to high school, the next 4-year step in our educational system. The best high schools selected applicants on the basis of admission tests. Stuyvesant High School was known as the science high school and remains to the present as one of several in New York City. I applied and was admitted. I enjoyed it immensely, especially science and mathematics and shopwork that was still considered important for budding scientists and engineers. Stuyvesant was then housed in an old and grubby building at an address called in New Yorkese *Fifteent Street and Foist Avenoo* in downtown Manhattan. The campus was the street. Stuyvesant had an excellent faculty and was the springboard for many students who went on to study science and engineering at MIT and other universities A surprising number of them became faculty members at MIT.

Stuyvesant High School was not merely the selective school of science. It had a very diversified student body and a politically alert faculty that was the source of a social and political education. The roll call at Stuyvesant read like a delegation from the United Nations. A few of the more exotic names of classmates that I remember included Pasquale Pasquale, Lazlo Szabo, John Bruzza, and Hannibal Castiglia who, incidentally, was the nephew of the notorious Mafia boss, Frank Costello, to mention a few. Many were the children of immigrants, and they traveled to high school from all over the city. I rode the now-extinct Second Avenue Elevated train back and forth from 92nd Street to 14th Street twice a day. The income range of the students' families was almost as broad as that of the entire country. I became a close friend of a thin fellow named Joe Hurley who wore ill-fitting shabby clothes. We often had lunch together either at the White Tower, where a hamburger cost a nickel, or at Nedicks where you got a hot dog and an orange drink for 10 cents. He couldn't afford more. On the other hand there were a few well-heeled students whose fine clothes betrayed their origins. These years were the mid and late 1930s and the country was just emerging from the Great Depression. The school had some racial variety, but the samples were relatively small. Confronted with this diversity, one could not help but expand social horizons and knowledge of different ways of life.

The faculty of Stuyvesant was almost as diverse as the student body. Half of them were Ph.D.s and were addressed as doctor. There was Dr. Kaplan who was rumored to have designed submarines in Russia but had a hard time controlling his class, Dr. Schur who taught us biology and took a deep interest in his students, a teacher of French language, Miss Popo, a short stocky woman in frilly dresses who dyed her hair deep red and reminded me of a small Pekinese dog. Dr. Myers taught us physics and coached the swimming team on which I got my letter. We used the pool in the local public bathhouse for practice. Economics proved to be an interesting course of thinly disguised Karl Marx. Actually, leftist politics seemed to be the rule among the interested faculty and students, a fact that became apparent years later when during the reign of Joseph McCarthy many of New York City's teachers were persecuted by that demagogue. This prewar period was perhaps the heyday of leftist politics in the States. The first World War had not brought a satisfactory peace, and the Depression had disillusioned many people who then looked for political change.

Seeing and feeling the injustices in society of this period, I sympathized with the left in an abstract way, but I was not about to leave school or go on other radical adventures such as joining the Spanish Loyalist army as did some of my radicalized contemporaries. Instead I was drawn further to science. I thought much of the ills of society stemmed from irrational thinking. The antidote was logical thinking and science was its epitome. It took a long time before I saw the naivete of my "thinking."

Columbia College

During my senior year at Stuyvesant High School, I applied for admission to Columbia College that my parents and I thought offered the best education in Greater New York. We were told that admission was difficult for a student from New York City because there was said to be a quota on local applicants: one that was a thinly disguised reflection of anti-Semitism. Perhaps foolishly I applied nowhere else, including other Ivy League schools. I simply did not want to leave New York City. Fortunately I was accepted in the Columbia class of 1943 and looked forward to attending in September 1939. In retrospect I recognize that my senior year in high school was a high water mark in my sense of feeling on top of things. I thought I knew more and had more opportunities than I would ever again believe. But that confidence and trust in progress was soon shattered by events. The very month I began to attend college (September 1939), the Germans invaded Poland and World War II began.

For my family and friends the implications of those events in Europe were profound. In a sense we were prepared to hate Germany. As a small child the Germans, opponents in the last war, were still the enemy in play and games. My father was a veteran of the War. The hated Spanish Nationals were supported by the fascist countries of Europe. Information about cruel treatment of Jews had been leaking out from Germany for some time. Much as war seemed an outrageous mistake to the rational mind, this particular war seemed justified, and one needed to contemplate joining the defense forces. Although the threat of war hung over our heads throughout my college career, we students managed to continue our education and other activities that go with college ages. As a freshman I tried out for the rowing crew but gave that up after realizing how filthy the Harlem barge canal was. Any cut that got a drop of its water was infected by nasty microbes—a specialty of the Harlem and East rivers. I then tried the wrestling team, and although I once defeated my good friend Eddie Marwell I was not a general success at that sport. I joined a fraternity against my better judgment and rarely attended its meetings. Then there were the Barnard girls ensconced across Broadway. Various social events brought us together with them, and many long-term relationships were started. I am still reminded occasionally of my own connection of the time: Edith Schmidt, a sweet young woman from Texas.

In those days any student who was proficient in science and math was assumed to be on the road to becoming an engineer. After all that was where the jobs and careers were. Pure science was as remote a profession as the study of Etruscan epigraphy. Columbia offered a 5-year combination AB-BS program in addition to the regular 4-year engineering option. That arrangement suited me fine, and the addition of the extra liberal arts courses played an important role in the development of my career.

Meyer Schapiro

Why did I opt for liberal arts as well as engineering? Because I wasn't sure that engineering was for me. I imagined myself in some occupation that engaged more of my interests in the arts, and what later I recognized as the field of what may be called "perceptual science." I looked into architecture as an alternative. That profession seemed to combine art with building and planning, both of which interested me. But I was told by knowledgeable older friends that that view of the profession was more of an ideal than a reality. Moreover, the school of architecture at Columbia was dominated by the Beaux Arts approach which seemed neither innovative nor interesting. Then in the course of my junior year I had what I might call an epiphany.

I encountered Meyer Schapiro by taking a couple of courses with him in art. He was Professor of Art History and Criticism, a polymath who in brilliant lectures brought all sorts of information from historical, scientific, iconographic, and other sources into his discussions of works of art. He transported his audience to exotic places when they sat in a dimmed seminar room watching slides and raptly listening to him lecture. He assigned homework in the following way: Go forth, find an object or work of art you like, and write about it. That was all. But of course we students would try to emulate the teacher and probe as deeply as we could. On one of the assignments I chose to examine the *Starry Night* of Vincent van Gogh hanging on a wall in the Museum of Modern Art. That painting had always struck me as strangely exciting. As you will recall, the scene portrays a bright sun surrounding a crescent moon with stars surrounding them and a great vortex in the sky with no obvious identity as an astronomical object. From the earth below a church steeple points to the sun-moon. In the process of reading Vincent's letters to Theo, his brother, I discovered that he attributed the appearance of the moon as a crescent to occlusion by the earth's shadow that is, of course, incorrect because that is a description of a partial eclipse of the moon by the earth. I discovered that there was no such eclipse at the time. And considering that Vincent always painted natural scenes, however he may have transformed them, one might conclude that some kind of subconcious process was influencing this portrayal. Being interested in symbols at the time, I proposed that this was a cryptic portrayal of the holy trinity: sun:father, moon:son, and vortex:holy ghost; church steeple proclaiming: BEHOLD!

A day after I handed in the assignment, Schapiro asked me to come to his office where he showed me an illuminated manuscript with a sentence describing a scene from the Apocalypse reading: "In the sky was a woman clothed with the sun with the moon under her feet and stars surrounding them." My interpretation of the *Starry Night* was off but not too far. Schapiro published this discovery in a footnote to an article in the magazine *View*, attributing it to me. Apart from the excitement of recognition by this greatly admired teacher, the event had the following significances for me. It showed me that with motivation, effort, and devotion, one could discover the underlying truths in the world and its artifacts: what I later learned to call research. And, just as important, I COULD DO IT. But would I have the opportunity if I continued in engineering? I doubted it. What should I do? For the moment, the question had to be shelved. The war in Europe was escalating. Everyone knew that we would have to enter it. It was not the time for me to quit engineering before I at least had the degree. Moreover, being in engineering training gave deferment from the military draft that by then had been instituted. The military believed that in the long run a trained engineer would be more valuable to the service than immediate induction into the defense forces. Service in the military at the time was at least a relatively egalitarian affair—every young man was subject to the draft—unlike the situation in the current fiasco. In any event, imminent and actual military service was to be my major concern for the next few years.

War

Not many weeks after I received my engineering degree, I applied for an officer's commission in the United States Naval Reserve. I had discovered that holders of such degrees were eligible to apply directly, and I preferred the watery road as an officer to being drafted into the Army as a private. After a thorough physical examination and a cursory intelligence test I was told that a decision would take several months. In the meantime I learned that the draft was threatening to take me for the Army and would not wait for the Navy's decision. Soon after I was formally drafted and spent 2 months in the Army before the award of the Navy commission as Ensign finally came through. Fortunately I was able to transfer to the Navy, although as I handed back my Army equipment the quartermaster announced that I was going from the frying pan into the fire. I was assigned to what was called Indoctrination School on the campus of Princeton University for 60 days. Students in the school were taught how to act like officers, distinguish port from starboard, and learn a few nautical skills that would be useful on board ship. On completion we were derisively called "60-day wonders." After Princeton I opted for further training in tactical radar, then a very hush-hush new technology for target identification, fighter direction, and coastal navigation. I got so interested in it that I was asked to stay on after course completion to help out at the radar training school in Hollywood Beach, Florida. Because winter was on its way, I had no objection to remaining there. Most of my teaching was as a coach going through the motions drilling on simulated operations. However, at one point I got my first experience lecturing in front of a class of seasoned officers. I had my trepidations. But I was unprepared for the panic response I felt soon after I began to address the group. Being in Florida it was a hot day—the large windows were open—and as I glanced out one of them to apparent freedom, I had a terribly strong impulse to jump out the window. It took a while for me to overcome this reaction to addressing groups of people. After several months in Florida I was sent to Saint Simons Island in Georgia for further radar training in fighter direction. A few weeks later I was ready for sea duty as a tactical radar officer on an aircraft carrier.

My orders directed me first to San Francisco by slow stages on a Boeing DC3 across the country. Then on an even slower unescorted freighter to Honolulu where I was to pick up a berth on one of the carriers that was in from the western battle zones.

After some months of cruising the ocean aboard the escort carrier *CVE Kadashan Bay* the long-awaited new weapon was dropped while we were anchored at Eniwetok atoll in the mid-Pacific ready for the final push. But the war ended, and for me further duty consisted of having my ship converted to a troop carrier and, as such, visiting several exotic ports of the Pacific Ocean including Saipan, Okinawa, Shanghai, and finally the Panama Canal and its locale.

In retrospect the war for me was quite an adventure, but apart from shipboard duties I was not entirely idle. Even before joining the Navy I had been reading in the literature of perception. Ernst Mach's book on *The Analysis of Sensation* particularly intrigued me. Then early during my time on shipboard I received a letter from Meyer Schapiro in which he said he was sending me under separate cover a thick monograph written by Wolfgang Koehler and Hans Wallach. Knowing of my interests in vision he thought I would be fascinated by its contents. Koehler, was the eminent Gestalt psychologist who had left Nazi Germany on principle and was living and teaching in the States. The monograph was titled *Figural Aftereffects: An Investigation of Visual Processes*. Essentially it showed how the study of what can be called an "induced visual illusion" can support a theory of brain function via a linking assumption. I pored over that monograph with increasing enthusiasm. Here was a subject I could really dig into with pleasure. In detail, the monograph describes a long series of experiments illustrating how an observer's prolonged gaze (inspection) at a particular figure will alter the appearance of a second (test) figure presented after the first is removed. The second figure, or parts of it, will appear displaced in space for some time after it has appeared. These aftereffects might well be called "illusory" because they distort what is normally seen. They obviously belong to the large category of illusions, among which is that of motion with which I had been intrigued as a child. The fascinating aspect of his account is the theory that Koehler propounded to explain how the brain managed to produce these illusory aftereffects and how they related to the normal state of spatial vision. Finding that the aftereffect could be produced when one eye has inspected only the first figure and the other eye only the second figure implied that the neural processing was going on at the level of the cerebral cortex where information from one eye is first combined with that from the other. The theory then proposed that the shape of perceived forms was represented by the distribution of field potentials in cortex resulting from excitation originating at the retina. That distribution was in turn influenced by the distribution of electrotonus (a form of electroionic resistence) in the cortex left as a residue from previous potentials. This was the notion that Koehler would try to prove experimentally as I discuss.

Wolfgang Koehler and Swarthmore College

On learning of my enthusiastic reaction to the monograph, Schapiro offered to introduce me to Koehler when I returned to the States. And, true to his word, he did exactly that. Very soon after I returned to the States, still wearing brass buttons and epaulettes, I called Schapiro to announce my return and then made an appointment to meet Koehler at Swarthmore College. As I recall we met at his house just off the Swarthmore campus. At some point his wife, a pleasant Swedish woman he called Flicka, joined us for a time in part, I suspect, to check me out. I mention her presence because much later on, after I had become quite familiar with both, I learned that there was something about that visit that they found very amusing. Although they were too polite to tell me directly, I gleaned enough information to reconstruct the cause of their amusement. As I imagine seeing myself through their eyes: one day there arrived into the laid-back academic environment this 6-foot-tall 24-year-old Lieutenant junior grade in formal naval uniform who proceeded to engage Kohler with a formality matching his attire. They had probably not experienced anything so military since they left Germany. But apart from their amusement, Koehler did decide that I, with my scientific and engineering training, would be useful to have available as his assistant, and he set in motion my engagement at Swarthmore. Now it was his intent to confirm the existence of the electric fields in human brains that he had hypothesized, and I was to be his collaborator in the endeavor.

We set out to do so with only the crudest equipment; an old electroencephalograph with crude electrodes to be pasted on the scalp of the observer or patient. To cut out external sources of ambient radiation we built a cage of wire mesh within which the observer sat. We volunteered Koehler's services as observer because I was busy with running the experiment. Because we pasted fairly large electrodes on the scalp, it was desirable to cut off the hair at their sites. Because Koehler had a healthy head of hair, albeit silvery gray, I had to serve as barber. I got so skilled at it that I wondered if I ought to change my profession. In any event, Koehler appeared in public with two holes in his hair which he pointed to almost proudly. Once hooked up for recording we saw periodic fluctuations of potential and the alpha rhythm but nothing very clearly related to the visual patterns that the observer watched. Koehler concluded that the potentials he sought would be of small magnitude and slowly changing with the movements of a visible pattern that moved slowly across a screen. To record such potential changes was a challenge at that time. We needed an amplifier that could handle near DC levels with minimal drift. We needed nonpolarizable electrodes that would eliminate the drift caused by polarization at the electrode–skin surface. We obtained both, the latter as a result of my learning how to manufacture a silver-silver chloride interface at the scalp electrodes. Still, even with all the equipment we had developed over at least a year's work, we had not found the potentials that Koehler believed should be present. We were disappointed, but Koehler was indefatigible and would simply say, "we have not yet found them."

One evening he called me at the laboratory. "Held," he greeted me. I should say that by this time we had worked together in close contact for more than a year. Previously, he had always addressed me as Mr. Held. Calling me *Held* was a big step in informality and intimacy. It presaged an important message. I listened with bated breath. "Held," he continued, "what do you do if you have an ordinary cell (like a flashlight battery) whose voltage is insufficient for a job?" My immediate response was, "Why you get several cells and wire them in series of course." And with that response came a premonition that brought gooseflesh to my skin. "Yes," continued Koehler, "and that is exactly what we shall do on our next trip to Princeton (where we were working). We shall recruit four willing Princeton undergraduates, wire them in series, have them all stare at a spot in the screen as we pass an edge across it, and we shall record the summed potentials from their heads." The audacity of this bizarre proposal shook me. But that was Koehler. He would not be stopped. He often spoke admiringly of "bold" proposals, and surely this one was an exemplar. Accordingly, a few days later at Princeton four undergraduates appeared. I performed the necessary tonsorial modifications, attached electrodes, and connected the necessary wiring. When all was set, Koehler rotated the projector that cast an image on the screen they viewed. The needle, on the paper recording the potential changes swung over and back. We repeated the procedure again and again with the same result. Finally we had seen a substantial potential shift related to the passage of the image moving across the retinae of our four subjects. Soon after we managed to find these potential changes recording from one head only (Koehler and Held, 1949). Koehler had found his Holy Grail. He would continue working on these potentials, and not long after he gained the help of a new assistant

(Donald O'Connell) because by this time I was approaching the end of my 2½-year stay at Swarthmore College (Koehler et al., 1952).

The Wild West

During the years that I spent at Swarthmore, I attended several courses. Those of Hans Wallach were most instructive, perhaps even more in style than substance although the latter was not lacking. He lectured in a laid-back manner with long pauses to ponder and raise questions so that the students were challenged to respond thoughtfully. I spent some time with Solomon Asch and even participated in his early group influence experiments, although the field of social psychology was not my cup of tea. I got to know the zoologist Robert Enders. Through his contacts I became one of several students who received summer fellowships to do field observations of animal behavior at the Jackson Hole Wildlife Station in Moran, Wyoming, observing animal behavior. Like a cattle ranch in the old West, The Station was composed of a bunkhouse and a couple of utility cabins supervised by a local couple whose wife served as cook. Originally I proposed to study the behavior of packrats, which were supposed to be plentiful in the area. There were signs of the animals, but they turned out to be nocturnal, and I was not prepared for night observation. The small herd of bison were much more accessible, and I tracked their activities, under the supervision of Margaret Altmann, long enough to gather sufficient information for a short paper (unpublished) on their grazing patterns. Margaret was an interesting character who gained fame through her book on the red deer of German forests. Her project was to observe the elk of the area. However, the elk had migrated to higher ground, and although Margaret had brought her own horse for transportation, the elk were hard to locate. Because there was a corral in nearby Moran, Margaret took it on herself to teach several of us to ride. Once we gained sufficient skill, she took us out on trips seeking the elk. They culminated in a pack trip for several days in the wilderness. Here I can't forebear mentioning what Abe Maslow might have called a "peak experience." By late summer my jeans were well worn, I wore boots and chaps and a wide brimmed hat, and I had grown a straggly beard. I even carried a small pistol to scare off the bears. One day I was riding down a dirt road when an open car pulled up and the driver called out to me, "Hey buddy, you want to join us for the roundup?" He had taken me for a cowhand. It was the peak realization of my fantasy life. There were lots of fun and games that summer with Margaret Altmann, Howard Schneiderman, Trudy Enders, and others, and the animal work was a good background for appreciating the increasing influence of the ethologists who were just beginning to come into prominence in this country.

During my last year at Swarthmore I began a research program of my own that served me well, as I explain, when I went on for further training. I had become interested in what I later learned was the classic prism adaptation problem. Look through a transparent prism and you see the world displaced and distorted. If you wear such prisms as eyeglasses you will see a slow reduction of all the distortions and displacements. If you reach for an object seen through the prism you will initially misreach, but with repeated efforts slowly regain accuracy. It is this ability to return to accurate performance despite the transform, called "adaptation," that has intrigued experimenters at least since Helmholtz wrote about it in the nineteenth century. These experiments returned me to my earlier question on how perceptual and motor stability is maintained despite perturbations of the system.

Harvard Psychology Department

At the time, for me the practical question was should I continue my education in experimental psychology or possibly go to medical school where I might study the real brain as well as gain the security the profession offered. I decided that I was already too old to begin medical school and instead applied to the Harvard Psychology Department. Koehler had close friends at Harvard and approved my choice. Before I left Swarthmore I asked Koehler, "How does one make one's way in this field?" He answered shortly, "Make discoveries." This answer has had many ramifications for me over the years, but at the time it encouraged me to continue in the field that I did at the Harvard Department of Psychology. Soon after leaving Swarthmore I joined my graduate class in studies and social activities. In the beginning I was quite excited at the prospects and gung ho to go.

It was only much later that I realized how radical a change in culture was my shift from Swarthmore to Harvard. Of the faculty I knew, those of Swarthmore contrasted strongly with those of Harvard. In gross terms, the Swarthmoreans tended to be European in the image of Koehler: a subtle-thinking, historically and esthetically oriented group. The Harvard psychologists were all American educated. The ethos was "entrepreneurial intellectuality" to coin a phrase. The general aim was to show how smart one could be. Prizes were awarded to the chosen, as for example, appointments to the Society of Fellows. With a few exceptions, the students seemed to be more interested in succeeding than in the substance of what they were doing and planned to do. Who would write the first book, be elected to an honorific society, be called to take a professorship in a prestigious university? These were the Harvard-defined goals of academia.

A few years before I joined the Harvard Department of Psychology it had split into two parts: the experimental psychologists on one hand and the Department of Social Relations in which the more social and clinical aspects of psychology were joined with sociology and social anthropology.

E. G. Boring was the putative head of the experimental group. He was a person with whom it was difficult to communicate—he seemed constantly uneasy in one's presence. He had a generous policy of taking graduate students to lunch one at a time at the Faculty Club. At my lunch occasion I remember that he asked me about my experience with Wolfgang Koehler and talked about him extensively. How whatever Koehler did seemed to turn to gold. He was clearly quite envious of him. Gossip would have it that after Koehler delivered the William James lectures Harvard wanted to appoint him permanently in the Psychology Department, but Koehler couldn't stand Boring. Although Boring was not renowned for his work in experimental science, he did write the most scholarly books on the history of sensation and perception.

S. Smith Stevens, known to everyone as Smitty, was Director of the Psychoacoustic Laboratory, which was established during the war to serve the military. He was another person with some difficulties communicating with people. He was quite friendly to me and claimed that I put him on a research track that he followed for many years after our encounter. As he reports the incident, he had been lecturing on scaling sensation, and I had quizzically asked if it might be possible to directly assign numbers to the qualities of sensation. For example, if you told a listener that he would hear two sounds with loudnesses 0 and 100, would all the intermediate loudnesses be assigned a series of numbers that varied in some rational order? He urged me to work on this problem of scaling sensation on the grounds that the results would be printed and reprinted in the handbooks until time immemorial. But I was too young at the time to be concerned about that issue.

Fred Skinner was to me somewhat of an enigma. He was a cultivated and talented individual, facts that seemed incompatible with his simplistic theory of behavior. He claimed that most behaviors were products of reinforcement schedules. You could do anything with appropriate reinforcement. He was truly the heir of J. B. Watson, who famously said "Give me the child and I shall make the man." Like so many believers in simplistic theories, whose downfalls seem to result from life's complexities—witness the Frenchman LaMettrie who wrote *L'Homme Machine* and then died of overeating—Watson found his downfall having illicit relations with young female students. Skinner managed to avoid such a fate. Then there was a younger group including George Miller and Jerome Bruner of computational and cognitive fame respectively and Edwin Newman who administrated.

Georg von Bekesy

My Ph.D. thesis committee was composed of Boring, Stevens, and Newman; but the person who really advised me knowlegeably was Georg von Bekesy, although he took no part in formal supervision. It was some time after I had begun working as a graduate student in the basement of Memorial Hall that I first saw Bekesy. As I stood in the hall one day a stooped-shouldered bald-headed man of medium height shuffled by paying no attention to either his surround or the people in it. Although his substantial reputation had preceded him, this peremptory passage left me with an initial impression of a shy and unimpressive man. Little did I suspect his depth of sensitivity and intelligence until considerably later when I had gotten to know him. Bekesy had been persuaded to join the Psychoacoustic Laboratory at Harvard, leaving behind positions he had held for many years in Sweden and before that in Hungary, his native land. He had worked as an engineer and troubleshooter for the Hungarian telephone company, which was a very important job because at that time Hungary was the telephone hub for central Europe. Telephone signaling was of poor quality, and Bekesy determined to find out why. After solving some of the purely physical problems, including the earphone to ear coupling, he realized that remaining problems of signal quality were in the ear itself. Undaunted, he proceeded to study the fate of signals in that organ. In an extraordinary research program he solved the classic problem of how the cochlea of the inner ear works and how lateral inhibition among neurons accounts for the high degree of pitch resolution that the ear displays. For that work he won the Nobel Prize.

How did I get to know Bekesy? In the laboratory it was customary to have a research meeting almost every week. Usually, one of our colleagues presented his or her work or an outsider was invited to do so. Bekesy often attended these meetings. My turn came, and with much trepidation I presented my far-out ideas. They were not greeted with much enthusiasm. But after the meeting Bekesy came up to me and said, "You presented too much." He added, "When you speak to a group like that you should present only what they already know for the first forty minutes, then say one new thing." I've tried to follow that advice. Although Bekesy prized his privacy, he usually left open the door to his laboratory. I passed by it often, and every time I did so I noticed a small sculpture or other icons on his laboratory bench. They seemed to change regularly. When I had become friendlier with him I often stepped into his laboratory for a better view. Occasionally he made comments on a particular piece. One I remember was a rather obtrusive and ugly wooden head peeking over onto his bench. He referred to it as the Lab Director. I began to realize that Bekesy must have a substantial collection of exquisite antique objects which was indeed the case. He had collected a set of Oriental and other antiquities that were the envy of the best museums in the world. He bequeathed the collection to the Nobel organization, which I learned disseminated it among several Swedish museums.

Independent Research

I mentioned earlier that I had begun to develop a research program of my own while still at Swarthmore College. One might have thought that I would follow my mentor and continue along lines that he had laid out and in which I had aided and abetted him during the 2½ years I spent at Swarthmore. But as I began to learn something about contemporary neuroscience—after all I had begun knowing nothing—I increasingly realized that however ingenious were Koehler's ideas, they were far removed from current thinking among those who were practicing neuroscientists. The idea that field potentials played an appreciable role in brain function was diametrically opposed to the prevailing nerve cell doctrine in which the brain consists of vast numbers of neurons playing different roles and much information was transmitted by impulses traveling down axons at relatively high speed. Thus though Koehler's work with my assistance was not taken seriously by most neuroscientists of the day, Koehler commanded respect for his long and productive history that could hardly be true for a newcomer to the field. Besides, I had already become interested in other researchable questions.

To trace the origin of my research I return to the waterfall illusion and the questions it raises. It is one of a large class of aftereffects that have intrigued observers and kept scientists busy for several centuries. The underlying issue these illusions raise is: what does it mean to claim that one sees the world correctly—We recognize that if our perceptions were habitually false we would be in deep trouble. How do we generally keep our perception of the world correct? Or is that the question to ask? Two general types of answer have been traditionally proposed. The first is to propose that the habitual state of adaptation is fairly uniform and keeps perception stable. For example, prolonged motion in one or another direction is relatively rare, and when it does occur, we learn to anticipate and counter its consequences. The second is the claim that perception has its own rules—sometimes referred to as Gestalt properties. That such rules lead to correct perceptions of the world could conceivably result from some sort of evolutionary selection of rules that are in accord with the properties of the environment. But there is a third possibility, namely, that the concept of correctness is misleading. That an illusion simply reflects an extreme case of the operation of the process of perception. When those processes are understood, so will the illusions. As Johannes Purkinje wrote in the early nineteenth century: "Illusions are the truths of perception."

When I became a graduate student at Harvard and sought a research position I soon discovered that such positions were possible if one worked in the domain of hearing, not vision. That situation resulted from the fact that the major source of research funding was the PsychoAcoustic Laboratory headed by S. Smith Stevens. At that point I had sufficient confidence in my theory of adaptation to what I called "visual rearrangement" that I was ready to apply it to audition as well. Consequently, I developed an auditory analog of prism rearrangement. In effect, I rotated the ears of experimental subjects by a small angle around the head. This was done by interposing microphones connected to hearing aid amplifiers over the natural ears. The positions of the substitute ears and their separation determine the interaural differences in times of arrival of sound at the two ears. And those differences determine the perceived positions of sound sources. The outcome of prolonged wearing of this device was indeed a shift in the direction of heard sound sources. Most interesting was the doubling of the apparent source of sound after the wearer had spent hours wearing the device and walking around Harvard Square to the amusement of passersby. This research became incorporated into my Ph.D. thesis, demonstrating adaptation of auditory localization and published in the *American Journal of Psychology* (Held, 1955).

After completion and acceptance of my thesis, I applied and was awarded a National Science Foundation postdoctoral research fellowship that kept me at Harvard for another year and a half. During that time I explored the process of adaptation to the edge colors caused by dispersion of wavelengths of white light seen through a wedge prism. I came close to discovering the startling aftereffect found by Celeste McCullough a few years later; but her work, which demonstrated the orientational selectivity of edge colors, followed the very significant discovery of orientation selective cells in visual cortex by Hubel and Wiesel, which gave her the idea of testing for the orientational selectivity (Held, 1980).

Brandeis University

At some point during my stay at Harvard I began to wonder about the future. By that time Doris Bernays, a student at Radcliffe College and I had married and had settled in the area. But where would I find a job? Once again Meyer Schapiro supplied an answer. He suggested that the new university, named after the celebrated jurist Louis Brandeis, and supported by the Jewish community, was looking for young faculty, and he would recommend me to the appropriate people. I made an appointment to interview Abraham Maslow who was chairman of the Psychology Department. We found each other congenial. When Meyer Schapiro and Wolfgang Koehler supported my application I was quickly appointed as an Instructor in 1953 with a salary of $4,000 per year. I can't say that I was overcome by the magnitude of this salary. Quite the contrary, I had already been offered more to work at one of the local government-supported laboratories. But I wanted to set up my own laboratory and to teach subjects I enjoyed. The transition to Brandeis was easy. The only practical change would be the 20-minute commute to Waltham.

At that time Brandeis was in its fourth or fifth year of existence. It would grow greatly during the next decades, giving new faculty the opportunity to shape many aspects of the growing entity. Already the faculty was an unusual group of people for an academic entity. The political left was well represented by people who wrote for the *Partisan Review* and its offspring, *Dissent*. They included literary luminaries of the left such as Irving Howe, Bernie Rosenberg, and Philip Rahv among others. The political sociologists included Lewis Coser and Herbert Marcuse. It was an intellectually stimulating place.

From the top down the faculty of Psychology were a mixed group of theorists. They were a likeable bunch, but as scientific colleagues I didn't find them challenging that ultimately was a reason for my departure from Brandeis. In 1953 when I joined the Brandeis Faculty there were five of us in the Department of Psychology. The Chair was Abraham Maslow, a congenial man who led with a light touch. Having himself chosen the faculty, he was quite supportive of us as well as generously laudatory. At the time he was propounding his theory of self-actualization—a sort of peptalk exhorting people to develop their assets wherever they might lead. His ideas must have been in accord with the Zeitgeist because they caught on among various strata of people ranging from rebellious young men like Abby Hoffman, who at the time was a student at Brandeis, to Business School professors seeking to energize their students. Maslow became an icon for diverse people eager for new ideas. I must confess that as much as I liked him, I couldn't take his ideas seriously. I can't resist mentioning a bizarre experiment he got some students to perform. Conjuring up the idea that female breast size had something to do with maternal instinct, he had students measuring the diameters of the breast aureolae of their female classmates as well as taking a verbal test of their maternal predilections. There were no scientific review boards at the time. I never learned whether a correlation was found.

Then there was Jim Klee, a huge man from the Midwest who had gained his degree in one of the departments of psychology whose faculty we, in the more enlightened departments, called "dustbowl empiricists." He had rebelled against that ideology, as had Maslow, and was developing a new theory of behavior that I could not comprehend. He was otherwise distinguished by having had built the largest chair I have ever seen and placed it in the lecture hall. Then there was Ricardo Morant, who was my contemporary and also an experimental psychologist, although he seemed to prefer theorizing to experimenting. His family came from Catalonia, and he was very proud of that origin. He obtained his degree from Clark University and had the earmarks of a student of Heinz Werner and Seymour Wapner, who were the experimental types at Clark University among a friendly group of theorists and clinicians.

Last, but not least, was the lone female, Eugenia Hanfman, a rather distinguished person who taught and served as head of the counseling services of the University. As a young emigree from Leningrad, she had studied with Kurt Lewin in Germany and then migrated to the States with her old mother and brother, a Professor of Fine Arts at Harvard specializing in ancient artifacts. Known as Genia she was admired by all for her sense of humor and sound judgment. Occasionally I would drive her home from Brandeis. On one occasion she told me that she was going on half-time during the next semester as a partial sabbatical. I immediately asked her what she would do with the other half. She sort of sighed and said, "Why must I do something during that other half—it isn't necessary to fill up time. You Americans!!!!" She had given me a new perspective but not one I could adopt.

Brandeis proved a good environment for young faculty including myself who were ambitious to develop their careers. The teaching load was reasonable—a class or two per semester. Over time I taught Statistics, History and Theory, Comparative Psychology, and Experimental Laboratory. The course I was best prepared to teach (perception) was already taught by Morant, and he kept doing so. That was just as well for me because I then had to learn more and having to teach was the best way. I particularly enjoyed teaching inductive statistics whose logic to me always had the intriguing sensation of getting something from nothing.

Princeton Institute

During my second year at Brandeis I received an invitation to spend a year at the Institute for Advanced Study at Princeton. Koehler was to be there and must have proposed that I join him. It was a memorable year during which I met and exchanged ideas with many interesting fellows of the Institute. Early on, I was interviewed by the Director, J. Robert Oppenheimer hero of the development of the atom bomb, who had recently had the devastating experience of being denied his security clearance for alleged disloyalty. I was ushered into his office and seated across from him. While loading his ever-present pipe he asked me what I was interested in doing in my field. I said I wanted to apply mathematics to deal with certain perceptual puzzles. He puffed his pipe and then said, "You must beware the Pythagorean Mystique." I did not respond in like manner although I would have liked to. I spent a good bit of time with the art historian Leopold Ettlinger whose colleague Ernst Gombrich visited and took us to attend his lecture at the Smithsonian. The material was incorporated in his book *Art and Illusion* that I reviewed (Held, 1960). Alexander Koyre and Irwin Panofsky were two other very stimulating presences with whom I had some contact at the Princestitute, as Lukas Teuber dubbed it.

During my year at the Institute I did a lot of thinking about aftereffects and staring at stationary patterns of lines. The outcome of these experiments was a broadening of my concept of figural aftereffects and an anticipation of the extensive use of the analysis of patterns by spatial frequency and phase (Held, 1962). I wrote a grant proposal and submitted it to the National Science Foundation. The award was a great stimulus to developing an active laboratory. I was eager to do experiments and to engage graduate students who were coming in small numbers to our department at Brandeis. With grant funding we could build apparatus and even pay small stipends to student research assistants. My first graduate student was Alan Hein who, 50-plus years later, is currently a colleague at MIT. Joe Bossom and others soon followed. During my 8 years at Brandeis I had about a dozen students working in my laboratory including graduate and undergraduates. Much of our work dealt with prism rearrangement experiments: displacing, extending, and rotating the visual field and measuring the adaptive results of exposing the wearers of these optical devices to the environment. They resulted in a series of presentations at meetings and publications mostly coauthored with my graduate students (Held and Bossom, 1961; Held and Hein, 1958; Held and Rekosh, 1963; Held and Schlank, 1959; Mikaelian and Held, 1964; summarized in Held, 1965). We also introduced the disarrangement experiment in which a continuously variable prism was used and demonstrated degradation of the accuracy of reaching (Held and Freedman, 1963). At that time we began animal experiments based on conclusions drawn from the rearrangement experiments. They had shown that active movement in space was important for adaptation of the moving body or part of body that when prolonged could lead to full and exact compensation for the initial errors introduced by rearrangement. But if the adaptive process can yield full and exact return of correct function, then it should be capable of compensating for any neonatal errors and may even account for initial development, a proposal that got us into the nature–nurture arena.

Alan Hein and I set out to test our theory of the early development of visual–motor coordination in kittens based on the results of our rearrangement experiments. Alan built a small breeding colony of cats to supply us with newborn kittens and proceeded to demonstrate the importance of self-produced movements in development in accord with our theorizing. This research involved what became known as the “kitten carousel” in which one kitten actively moved itself while pulling a coupled but passive mate so as to equate their purely visual exposure. The former developed its visual guidance of behavior while the latter remained deficient. This experiment caught the attention of the field and “Heldenhein” became a household word among experimental psychologists (Held and Hein, 1963).

Another then-contemporary graduate student, Burton White, wished to work with human infants. He found a source of infants being reared in somewhat impoverished environments in a state-supported institution. He then showed that increased opportunity to engage their environment speeded up their development of sensorimotor coordination in accord with the ideas we had developed (White et al., 1964)

Although later work was more sophisticated, the enthusiasm for research of that early group at Brandeis was never exceeded. It was during that period that my wife and I grew a family. In a period of 4 years, Lucas, Julia, and Andrew were born in succession. I rose in the professorial ranks from instructor to tenured professor within a few years. In my 7th year Maslow, the Department Chair, went on leave and I was asked to fill his position on a temporary basis. I did so but had enough of administration after a year. Moreover, Hans-Lukas Teuber, an acquaintance I had met through Koehler, was about to become the Head of the Psychology Section at MIT and had indicated an interest in having me accept an appointment. I needed to make a choice. Luckily I was eligible for a sabbatical and took the opportunity to spend the year at MIT so as to test the waters. My experiences at Brandeis had been very favorable, but MIT offered a much greater challenge and opportunity to be among people who were closer to my interests than those available at Brandeis. In addition I did not relish the thought of becoming Chair of the Brandeis department. Although I liked many of the individuals, I had no desire to take on the problems of governance of a group, much of whose interests and work was remote from mine.

MIT and Hans-Lukas Teuber

When I moved to MIT in 1963 the section had just been housed in a refurbished three-story loft building numbered E-10 that was Spartan in its furnishings but adequate. There were about a dozen faculty, several of whom had antedated Hans-Lukas Teuber's appointment as Head of the section. Within a year or two all of the latter had left leaving open several faculty slots soon filled with new appointments. A few postdocs, and a scattering of venturesome graduate students filled out the ranks including Stuart Sutherland, a larger-than-life swashbuckling experimentalist visiting from England. After a few years the Section had grown in size, funding, and reputation to the stage where it was ready to become a Department with all its rights and privileges.

Before coming to MIT Teuber had spent years studying the sensory capabilities of brain-injured patients. At that time access to the real brain was quite limited compared to the present situation, yet Teuber and his colleagues had done creditable work with the tools available. Perhaps more important, his exposure had persuaded him that the future of our field lay in the direction of what later came to be called "neuroscience." Consequently, he conceived of his new department as one combining the best of system neuroscience with experimental and cognitive psychologies. In so doing he anticipated the wave of the future. Accordingly, Teuber recruited faculty as diverse as Walle Nauta, the distinguished neuroanatomist, and Jerry Fodor, the young philosopher-psycholinguist, so that the Department represented a diverse spectrum of disciplines all within the potential rubric of brain and cognitive sciences, which many years later became the name of the Department. In later years several of the graduates of the Department were hired (Whitman Richards, Gerald Schneider, and Ann Graybiel), a form of inbreeding that didn't seem to hurt the Department at all. This diversity of faculty interests was good and bad for a small Department. The good part was the strong intellectual interaction among its members. The bad part was that it put us at a disadvantage when we were compared with other departments in our field. We did not have the strength in numbers in any one specialty possessed by one or another more monolithic department.

I should mention here another pioneering group in the development of neuroscience and one which originated the name. The Neurosciences Research Program was founded and run by Frank Schmitt, an MIT Professor of Biology who was the prophet of neuroscience. The Program ran seminars and gatherings of groups of scientists, including myself, chosen by Schmitt and his advisors to come together to exchange their knowledge in the interests of making progress in understanding brain and nervous system. George Adelman and Theodore Melnechuk edited their publications, which were circulated widely. The group, fondly known as The Schmitt Circus, deserves much credit in furthering the development of neuroscience.

Department Head

Under Teuber's benevolent leadership, the Department flourished as did its members for the most part. In addition to his administrative duties and professional obligations he managed to teach the elementary course to great acclaim and to continue his research and supervision of students. But catastrophe struck in 1977. While swimming on holiday in the Virgin Islands, he was swept out to sea by a tidal current and disappeared. In the wake of his loss I became Head of Department, first temporary then full term. My inclination was to preserve and enhance what had been a good thing: a group of about a dozen congenial faculty with a graduate program awarding the Ph.D. and a minor for undergraduates. For several years this policy succeeded in increasing the faculty with appointments, particularly in the computational area with David Marr and Tommaso Poggio. But with the rapid growth of neuroscience, we began to realize that we needed more strength in the biological areas and more space to accommodate the expansion.

It was at this point that the top administrators had a brainstorm. Under pressure from the Whitaker family, which had made a large grant for a new building to accommodate a proposed medical school at MIT, they saw a means to achieve several goals with one action. Whitaker College, in collaboration with Harvard Medical School, had set up a quasi-medical program assembled under a medical director (Irving London). It then had a motley crew of faculty, assembled from various corners of the Institute, that sparsely occupied the new building. Their proposal was to split our department into wet and dry science sections and move the wets into Whitaker College, thereby raising the number and quality of its faculty and filling the new Whitaker building. The adverse effect of breaking up our department was hardly considered despite our protests. It has been only in recent years that this fissure has been remedied by the recombination and expansion of the faculty of Brain and Cognitive Sciences and the availability of its own building. We hope that some of the original elan of the Department will be re-created.

After leaving Brandeis with our fairly extrensive laboratory it was necessary to re-create it at MIT. With the initial help of Alan Hein, who joined the faculty in the following year, we did just that. And throughout the succeeding years, including my administrative service as Department Head and beyond, I maintained my research laboratory and managed to obtain continuous funding for it from grants and contracts until I relinquished the leadership of the laboratory. Of course I had excellent help throughout, although anyone who runs a large laboratory knows that "help" is an inadequate word for the kind of support that research associates can provide. In that vein a most significant human addition to our laboratory was the employment of Joseph Bauer. After having done a fair number of studies of kitten development (Hein and Held, 1967; Hein et al., 1970), we wished to proceed to study development in a primate. For that purpose I contacted Harry Harlow, who was doing extensive research with infant monkeys and asked him if he could recommend to us a source of help in developing that capability. He quickly recommended Joe who at the time was one of his graduate students who had not yet found a thesis problem and perhaps needed a change of milieu in which to do so. It was a fateful decision for all of us. Joe not only set up a successful breeding colony of stumptailed monkeys, he managed to test them with devices he made (Bauer and Held, 1975; Held and Bauer, 1967), and in a short time became usefully involved in most of our ongoing laboratory activity to the great benefit of people in the laboratory. After 20-plus years, he had done the work of many thesis projects without receiving the award, but by then I don't think it mattered to him.

Together with student involvement we continued with rearrangements and related experiments in the succeeding years. We kept up a barrage of oral presentations at professional meetings and of follow-up publications on the research and thinking we had done (Efstathiou et al., 1967; Graybiel and Held, 1970; Hardt et al., 1971; Held et al., 1966). One product of the diversity of disciplines within our department was the realization among a group of us that the visual system had two modes of functioning, the "what" and the "where" (Held, 1968). Evidence came from neuronal as well as behavior study (Held, 1970). A few years later Mortimer Mishkin's group identified this distinction with the anatomical difference in function between dorsal and ventral projections from striate cortex. Another product of this distinction was our discovery that the latter mode of vision remained even when the former had been destroyed by blinding lesions of the visual cortex (Poeppel et al., 1973). Lawrence Weiskrantz subsequently made a career of exploring this phenomenon under the name "blindsight." Still another direction our research took at this time was the exploration of adaptation of combined color and edge channels with the collaboration of Stefanie Shattuck (Held and Shattuck, 1971; Shattuck and Held; 1975; summarized in Held, 1980).

After some years our laboratory had gained sufficient notoriety to attract postdoctoral researchers and visiting faculty. Some also contributed to our research. Johannes Dichgans, Laurence Young, and Thomas Brandt initiated our studies of vection: visual motion that induces feelings of bodily movement (Dichgans et al., 1972; Held et al., 1975). Several graduate students then took up the vection research (Finke and Held, 1978; Finke et al., 1984; Merker and Held, 1981; Wolfe and Held, 1979).

By the mid-1970s our work on rearrangement had also begun to attract criticism, in good part well meant but some simply following the tendency of competitors to attempt to destroy what they hadn't produced themselves. This discouraging criticism seemed to peak at the time of growing interest in the very early development of primate vision following the discoveries of control of neuronal development by conditions of rearing by David Hubel, Torsten Wiesel, and others. Together these factors made for a change in our research directions. For us the challenge became to develop methods of testing the vision of human infants as soon after birth as possible so as to study the human parallels to the early-rearing research with animals. Consequently we turned the direction of our efforts to that set of problems while phasing out the rearrangement work. Our first effort succeeded in showing that infant vision exhibits an oblique effect at a few months of age. In other words their acuity is less for oblique edges compared with verticals as imaged on the retinae (Leehey et al., 1975).

It was at this point that we had the good fortune to add Jane Gwiazda, a recent psychology Ph.D. from Northeastern University, to our laboratory as a postdoctoral fellow. She quickly developed the skills needed to measure the early visual capacities of human infants and, with the help of Anne Moskowitz, Sarah Brill, and Indra Mohindra, pioneered in obtaining previously unknown measurements of refraction (Mohindra et al., 1978) and visual acuity (Gwiazda et al., 1978). Our discovery of the high incidence of astigmatism in young infants was greeted by castigation from some ophthalmologists who had themselves failed to observe it. Later we discovered it had been found by an obscure Italian ophthalmologist who had published it in an obscure journal many years before. Over the years Jane's talents enabled a progressive increase in her leadership of the infant research of the laboratory.

During those years a continued stream of infants was tested repeatedly over time to obtain various other measurements of vision, each of which provided a student with a program of research leading to a doctoral degree or postdoctoral achievement. Thus Eileen Birch worked on stereoacuity (Birch et al., 1982; Held et al., 1980), Shinsuke Shimojo on vernier acuity (Shimojo et al., 1984), and Janice Naegele on optokinetic nystagmus (Naegele and Held, 1982). We also did studies of the consequences of early pathology in conjunction with Samuel Jacobson, an ophthalmologist then stationed at the Eye and Ear Infirmary (Jacobson et al., 1981).

From the beginning of this research with infants, at each experimental session we had our subjects refracted by a participating optometrist from the New England College of Optometry situated just across the Charles River in Boston. As I recall, it was my friend Herschel Liebowitz who originally suggested that we incorporate measures of refraction in our research. At first the refracting was done by Indra Mohindra, who first found the high incidence of myopia. She was followed by Mitchel Scheiman for a time and then by Frank Thorn who was not only skilled in optometric measurements but knowledgeable in all aspects of vision science. He became a fixture in the laboratory and continues to be so. As a result of collecting time sequences of refraction measurements as our subjects aged over the years, we began to see in some the onset of myopia as they reached school age. That meant that we possessed what was to our knowledge the first-ever collected set of measurements of the developmental course of myopia over the preceding years (Gwiazda et al., 1993). With these potentially valuable data in hand we turned the laboratory's attention to further collecting refraction and other ocular measurements in an effort to better understand the etiology of myopia, a serious health problem (Gwiazda et al., 1995, 2000; Thorn et al., 2005). By this time several investigators had shown with animal models that the development of myopia is influenced by early conditions of vision and we sought the human parallels.

New England College of Optometry

In 1986 after 9 years as Head of Department I stepped down, and Emilio Bizzi was appointed. Apart from relief of responsibility, the other change I noticed after a time was my reduced power over decision making. The latter became obvious when the time came for advancement of faculty I favored. Partly as a result of appointments that I had previously pushed through, the Department ethos had moved strongly toward favoring computational research as the promising direction of effort. Two candidates, Jeremy Wolfe, my former student, and Jane Gwiazda, by now a junior faculty member, that I favored were turned down for advancement despite excellent records essentially because they were not computationalists. When this bias was shared even by mathematically innocent faculty it seemed to me time to recall Oppenheimer's advice to me—see page 30. The failure to advance Jane opened the possibility that she would leave, and our collaboration would end just at the time that we seemed to have a real handle on the myopia problem. But a new development saved the day. Over the years we had developed a good relationship with members of the New England College of Optometry across the river. At about this time they had developed a desire to expand their efforts in research. And what better could they do than acquire a laboratory which was doing ground-breaking research in a field of central importance to their mission? The College made us an offer we could not refuse—appropriate appointments for three of us and plenty of space to accommodate our needs.

We moved the laboratory in September 1995, and it has remained there ever since under the able leadership of Jane Gwiazda. I continued my participation in this research and also retained an office at MIT convenient for participating in nonadministrative departmental activities.

Back to the Future

After 10-plus years working with Jane and colleagues at the College of Optometry, we have made a series of interesting findings, but a fundamental understanding of the myopigenesis process has so far eluded us and, incidentally, everyone else in this field. The basic genetics need to be worked out to obtain an explanation of the modulation effected by early environmental interaction. I have returned in spirit and actions to MIT to spend my time in the newly unified Department of Brain and Cognitive Sciences, aggregated in an impressive new building. The Department Chair, Mriganka Sur, kindly offered me the use of an office to be shared with Alan Hein.

However, my very congenial young colleague and friend, Pawan Sinha, made me an offer I couldn't refuse to occupy an office in his laboratory suite among his very capable laboratory group. He also invited me to collaborate in Project Prakash, a remarkable combination of medical endeavor to restore vision in curably blind patients and of testing procedures to understand the recovered sight of the previously sightless. The work is being carried out in India, and not long ago I had the fascinating experience of visiting there as can be proved by an examination of the background of my photograph taken by a close colleague. I participated with Pawan and colleagues in the examination of several newly sighted young patients. Among other observations that have been made on these patients are those that constitute a test of the 300-year-old Molyneux question:

> Suppose a man born blind, and now adult, and taught by his touch to distinguish between a cube and a sphere of the same metal, . . . Suppose then the cube and sphere placed on a table, and the blind man be made to see: query, whether by his sight, before he touched them he could now distinguish and tell which is the globe, which the cube?

Currently we are preparing reports of the outcomes of these tests.

Apart from participating in the activities of the laboratory, here I sit at MIT once again reviewing the extensive body of rearrangement experiments. I now view them as having revealed only the tip of the iceberg of sensorimotor functions. We require a new and broader conception of the nature of adaptation and stability of coordination. I hope to make a contribution in that direction.

Postscript

Writing this biography has been an interesting exercise, reviving many memories of people and actions past. I view my career as a long ride with many ups and downs. I have always told my students that doing science had better be fun because you won't earn enough to buy it. I must have followed my own advice because I wouldn't have had it otherwise. Moreover I don't believe in retirement for retirement's sake and look forward to continued enjoyment in research. I hope I have done justice to the many individuals with whom I have been in contact who have enriched my work and life. If not, I regret the oversight.

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