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Massachusetts Institute of Technology (1961)
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Charlie Gross and his colleagues described the properties of single neurons in inferior temporal cortex of the macaque and their likely role in object and face recognition. They also pioneered in the study of other extra-striate cortical visual areas. Many of his students went on to make distinguished contributions of their own.

Charles G. Gross

I was born on February 29 in 1936. My parents, apparently anxious of my feeling deprived of an annual birthday, celebrated my birthday for 2 or 3 days in the off years and even more in the leap years.

I was technically a “red diaper baby.” My parents were active Communist Party members. In fact, however, I never heard the term *red diaper baby* or knew of my parents’ longstanding party membership until I was in graduate school, years after my father had lost his job because of his politics. Rather, my parents hid their formal communist affiliations and made every effort to bring me up with all the experiences and options of a normal American boy. Yet they managed to transfer their political worldview to me and even, for a long time, their fear of speaking or acting on that worldview. (For more about red diaper babies, see Kaplan and Shapiro 1998.)

My Parents

My father came from the Pale of Settlement in Russian Poland to Manhattan’s Lower East Side when he was a year old or perhaps two. His birthday was on the sixth Chanukah candle, but whether in 1900 or 1901 was not clear. For school he needed a Gregorian date, and his Rabbi calculated it was December 25, but as this was deemed inappropriate for a Jewish boy he was assigned December 15, 1900. Because of the uncertainty about his true birth date, when I was a kid we celebrated my father’s birthday on several December dates.

My father’s father never held a regular job after being fired in a furrier strike when my father was age 12. My father was then sent out to sell chewing gum on the street, in between household duties like carrying coal to their sixth-floor tenement. Their two-room apartment was filled with five siblings as well as various uncles and aunts and more obscure relations on their way from steerage to a new life. His family had predicted my father’s success because when the mattresses and blankets were laid out for the night, he hid his shoes in a crevice under the dining table and thus was the only person to effortlessly find his shoes in the morning chaos. Many of the transient relatives were of various anarchist and socialist persuasions, and so political arguments saturated family life.

About once a year, when I was a schoolboy my father took me to see his parents, then in the Boro Park section of Brooklyn. (We lived in the Flatbush part.) Like the halls of their apartment house, my grandparents smelled of

chicken fat, as they greeted me by rubbing their bristly faces against mine. My father spoke to them in Yiddish. This was still their only language; it sounded weird to me. I was struck by the absence of books and magazines. The only decorations on their blotched walls were my father's diplomas.

My father was a student at Townsend Harris High School, a 3-year high school attached to City College of New York. He then went on to City College itself followed by a master's degree in history at Columbia. By this time my father was an active Communist Party member. He was working on his doctoral thesis when the Party asked him to quit graduate school and go to work as a high school teacher of history and economics. (This wasn't such a bad deal: many young communists were sent to organize in factories and fields.) He spent 29 years, one less than the number required to retire on a pension, at Seward Park High School on the Lower East Side as a fulfilled, successful, and beloved teacher. In the evening, my father taught educational sociology in the City College School of Education and economics in its Business School.

My mother was a Party member too. She was born in the States, but her parents also came from the Pale. Although my mother spent about 7 years in and out of college she never got a B.A. degree, perhaps because she felt it was too bourgeois at that time in her life. In my childhood, she worked as a secretary in the public school system and was active in the American Labor Party (ALP), an organization that had supported Henry A. Wallace's third party Presidential campaign and that was accused of being a communist front.

The only political activity of my father that I knew about as a child was in the Teacher's Union. I would read about it in *The New York Teacher News*. This was not a bread-and-butter union concerned primarily with wages and working conditions. Rather it was a politically activist union particularly interested with improving the educational and social programs of the school system. It introduced Negro and Women's History week into the curriculum. It agitated successfully for more Black teachers, more schools, and more educational resources for Harlem. It strenuously advocated racial integration for the schools long before this became a national issue. It put out pamphlets to fight discrimination, racism, and prejudice (for an account of the Teacher's Union activities, see Zitron, 1968).

As I later learned, the Teachers Union was thrown out of the AFL (The American Federation of Labor, the first national alliance of unions) in 1941 as communist dominated. It then joined the more left CIO (Congress of Industrial Organizations) but was thrown out in the great purge of left-wing unions in 1950. Starting then, its leaders were gradually eliminated from the school system, and its main activity became defending academic freedom in general and, more specifically, its own members from being fired. Perhaps 200 or 300 members were fired, and a similar number resigned to avoid public exposure (Caute, 1978; Ravitch, 1983).

In 1953, when I was applying for college, my father was called before the Feinberg Commission, the New York investigatory body set up to free

the New York City school system of anyone “advocating the overthrow of the Government by force, violence, or any unlawful means . . . and any member of a society or group that taught or advocated such action.” My father, in the parlance of the day was asked to “name names.” Naming names of those seen at a Communist Party meeting decades ago was necessary to establish “good faith” to keep one’s teaching job, assuming one was not a Communist Party member in which case dismissal was immediate. He refused to cooperate but made a successful plea to be allowed to finish out the school year. Then he quietly resigned, missing a full pension by one year, but avoiding the publicity of exposure, which in similar cases often resulted in the families having to change their names and leave town.

My parents gave me my political orientation, my interest in history, and my concern for social justice.

Childhood Education

At Lake George

From before I was born, my parents spent every July and August camping on a state-owned island on Lake George in the Adirondacks (Leonbruno, 1998). Up until the time I was about age 10, I spent the summer camping with them. Then I started going to conventional camps and eventually worked as a nature or swimming camp counselor or busboy and spent only a month or less with my parents on Lake George. My Lake George experience produced a deep and lasting love of the outdoors. It probably helped lead me to the Boy Scouts and biology, both crucial for my subsequent career. After I came back from graduate school abroad, I continued to occasionally camp on Lake George with my wife and kids or members of my laboratory. Today, no matter what the season or weather, trail walking still gives me an enormous sense of pleasure.

In Elementary School

My experience in elementary school was an unmitigated disaster. I was often sent to the Principal’s office and then exiled to the kindergarten for days for being “disruptive.” My mother was constantly summoned to school and yelled at because I “talked” (the sin that followed me at least until college), did not do my work, made trouble, and generally was bad. My long-suffering mother came to school with pages of yellow-lined paper describing my reading and intellectual interests, but it was to no avail. My grades were poor, and I am sure that today I would have been classified, at best, as hyperactive and having an attention disorder. Needless to say, I was not allowed to get anywhere near the various programs for the smart kids.

In the Boy Scouts of America

Baden-Powell's imperialist Scout movement saved me. Inspired by my Lake George experience and encouraged by my parents, I joined the Cub Scouts as soon as I was old enough. There I was fiercely achievement oriented and rapidly rose through the Cub and Boy Scout "ranks," earning lots of merit badges and becoming the youngest Eagle Scout in Brooklyn at the time. Later, getting A's in high school and college and publishing papers and getting grants as a young academic felt just like getting merit badges in cooking, civics, and bird study.

My central experience as a Boy Scout was spending a month for four summers at Ten Mile River Boy Scout Camps. We slept in open lean-tos, wore uniforms, had formal flag raising and lowering ceremonies and other than waking up, cleaning our bunks, taking a dishwashing turn, and going to bed to taps there were no required scheduled activities. Most of my "troop" spent their time hanging around the bunk, reading comics or playing baseball, and maybe joining the afternoon general swim. By contrast, I ran around frantically taking classes and exams to get merit badges in every possible thing. The guys in my troop seemed unperturbed by my weird achievement intensity. They elected me to the honorific "Order of the Arrow"; and when I became an Eagle Scout they took over my turns at dishwashing, as that duty seemed to them below the dignity of such a station.

In the general swim we had "buddies," whose hands we had to raise when the waterfront director up in a white tower blew his whistle; if separated from your buddy you were "docked" (not permitted to swim) for some days. A few years later I was the waterfront director. The feeling of power standing on the white tower blowing the whistle for "buddies" was only equaled when, much later, I stood on the podium teaching physiological psychology in the same classroom where I had taken it as an undergraduate. (Actually, I was only assistant waterfront director. I strongly disliked the director but luckily he broke his leg early in the season, so that tower was mine, and my required subservience was restricted to the half-hour each day that I visited him in the infirmary. He was not the last boss that I had trouble with.)

Education at Home

There was a good public library near where we lived. Accessible by public transportation, there was a bigger one near the high school and a very big one in the center of Brooklyn. Each week I would take out the maximum number of books allowed from one or more of them. Too much of my reading, especially in elementary school, was of classic novels I could not have possibly understood, like *Madame Bovary* and *Crime and Punishment*. Once my mother and I happened to be reading *Lord Jim* at the same time. From her casual remarks I realized, but never told her, that not only did I have no

idea what the book was about but I had missed the central plot element where the captain abandoned ship. I am still deprived of many classics because I thought I had “read” them already.

My parents carefully kept me away from communist summer camps and the network of activities for children of communist and far-left parents (for a description of these camps and activities, see Mishler, 1999). Yet my father systematically transferred his politics to me, particularly by regularly going over the *New York Times* with me. In addition, leftist periodicals such as *I.F. Stone's Weekly* lay around the house, as did novels from the leftist Book Find Club, although most of his Marxist library had been removed to someone's cellar before I could read. I was told never to mention in school Paul Robeson, whose huge form had once towered over me at a concert, or indicate I knew who Sacco and Vanzetti, Spartacus, Joe Hill, or any of the left pantheon were, not even Pete Seeger. This anxious exhortation not to talk about politics, let alone act, extended well into my adult life. “Don't jeopardize your grades,” my father said, “until you get into college.” He repeated the plea when I was in college and again in graduate school: “Don't sign anything too radical until you get a job.” When I did get a job: “Wait until you get tenure.” “Wait. Wait, until you can be effective.” I tended to take his advice. As we'll see, I waited until I had tenure as a full professor before getting arrested, finally, at an anti-Vietnam war demonstration.

Education Around the City

Starting in elementary school I would go often, alone or with a friend, to the American Museum of Natural History in Manhattan. Besides the exhibits, I went to talks (I remember one on diatoms), joined clubs (like the Jr. Astronomy Club), and went on bird-watching walks they sponsored.

Another major activity was exploring the miles of used bookstores that once existed on Fourth Avenue in the City. Many of the bookstores in which the books were arranged by subject were out of our price range. We specialized in the ones where the books were shelved by acquisition or size but were only 19 cents or so apiece. I still have a few of these like *College Physiography*.

Erasmus Hall High School

I had been afflicted with a severe and long-standing stutter. I remember being pulled out of class in the sixth grade and sent to special stuttering classes for a half-hour each day and then another half-hour class for my lisp. Nothing got better so my parents took the initiative and arranged for me to go to weekly sessions for what they called an “indirect approach to my stutter.” After about a year of this a new person was assigned to me, and the first thing he said was “Do you know why you are here?” I said, “Of course. I am here for an indirect approach to my stutter.” “No,” he said, “you are here to discuss

your emotional and personal problems.” So I went to my parents and suggested a “direct approach” to my stutter might be more successful. They arranged for speech therapy to substitute for the psychotherapy. By now I was ready to start high school, and fortuitously the stutter was very useful.

My local high school, Erasmus Hall High School, was then a very mixed school of about 6000. Except for putting the students taking Latin together, and a few “honors” classes, there was no “tracking” by academic ability because that was considered to be antidemocratic. Rather, segregation by academic ability was done more covertly and efficiently. Programming the classes for 6000 students was a formidable challenge before computers. A “program committee” of the students with the highest grades as freshman carried it out. As a reward they were allowed to make their own programs, and they put themselves together in the same classes with the best teachers.

My parents went to the Erasmus authorities and successfully argued that “because of [my] stutter” I should be allowed to take Latin instead of a spoken language. Only the very best students usually took Latin as their first language, so in spite of my lousy grades and terrible disciplinary record I got tossed in with them. In that very rich soil I suddenly flowered into a highly engaged and competitive student, no longer bad (except in gym that I almost failed each year). In that adolescent memory, the top 20 or so boys and girls around me at Erasmus seem among the smartest and most intellectual group I ever knew. Actually, I enjoyed Latin, and it was a pretty good idea to take it because I never did learn to pronounce any spoken language correctly. In fact, my ear was so bad that I never lost my Brooklyn accent, which was later called an affectation in view of the years I spent among phony English accents in Cambridge, Massachusetts, and real ones in Cambridge, England.

In high school, I took all the science possible—5 years of science and 3½ of math. I was an editor of the school newspaper, the editor of the math magazine, and founder/editor of the science magazine. In spite of my bad stutter I spoke up often, especially in History, English, and Economics classes. These classes were easy and fun for me: everything fit into the all-encompassing (Marxist) framework I had absorbed at home. I did keep away from the honor math and creative writing courses, which were taken only by the math whizzes or real writers, respectively.

In the summer before my senior year, I carried out a research project in plant ecology for the Westinghouse Science Talent Search. I studied plant succession in a one-acre plot near my family’s campsite on Lake George. *Plant succession* refers to the orderly temporal progression of plant communities starting, for example, from bare rock and proceeding to the “climax” forest for that region. Each new stage changes the environment making it more adapted for the plants of the next stage. Plant succession, it seemed to me, was actually a better example of dialectical materialism in nature than many of the examples given by my heroes, the Marxist scientists J. B. S. Haldane

and J. D. Bernal, although I never breathed a word of this in my report. As a result of this project and a written examination, I was one of 40 Finalists.

As a high school senior, I applied to several Ivy League Colleges. The financial forms required for aid presented a problem. At this time, my father had just been called before the Feinberg Commission routing out subversives from the school system and, as mentioned above, because he would not inform, he knew he would lose his job that year. So what was he to put on the form: that he had a schoolteacher's salary now but had no income prospects for the future? Every potential employer in the city knew why a highly rated schoolteacher was suddenly unemployed (and unemployable, at least as a teacher).

Undergraduate at Harvard

I was offered small scholarships at several good places, and somehow my father was able to get them to bid against one another until Harvard finally gave me enough to go. Tuition was \$600 the year I was admitted. At my Yale interview the interviewer had asked whether my trench coat was a Burberry and then whether I was Jewish. At that time the Jewish quota at Yale was about 12% (Karabel, 2005). I didn't get into Yale. My Harvard class was about a third Jews.

Making Beds and Pete Seeger

The entire freshman class lived in dormitories in Harvard Yard; seven of us shared a suite. On the first night, after I was in bed, the three from North Dakota came in and sat on my bed. They had somehow figured out I was a Jew and, never having met one before, they were eager to talk about religion, one of the few subjects in which I had almost zero interest. Although all seven of us were from public schools, I had by far the longest latency to get into a Harvard uniform (grey flannels or khaki chinos, button-down shirt, tweed jacket). The ones from North Dakota took about one day whereas I took about 3 years. I never made it to the acculturation stage of Harvard mugs or stationery.

As part of my Harvard scholarship I was assigned a job making the beds and cleaning the rooms of other freshmen. One of my clients was very embarrassed by this and helped me make his bed. I later discovered that his father was a blacklisted folksinger. In the middle of the term I quit the job, claiming falsely that it interfered with my lab courses, but they raised my scholarship anyhow. Ever since, I have been a totally, completely, and proudly retired bed maker.

Although my ex-client and some other red diaper babies in my class (including one whose father was killed serving in the Abraham Lincoln Brigade) remained in the closet, I got close to three other freshmen from left backgrounds:

Emile Chi, Jim Perlstein, and Mike Tanzer. We were active in the Harvard-Radcliffe Society for Minority Rights. The only thing I remember our doing was organizing a Pete Seeger concert, actually a very daring act for the time. Somebody in the audience was taking notes—we thought it was the FBI.

Biology and Skinner

I wanted to major in history. But soon my competitive drive to get A's conflicted with political fear. I realized I probably was not smart enough to get A's and express my political opinions, if indeed that were possible. I knew it was time to find another major after I started getting back papers with comments, like "A: I'm glad to see how well your papers have progressed from the Marxist jargon that characterized your earlier papers" and writing papers arguing that Edmund Burke was really a great liberal and Victorian England was heaven on earth. Biology was the obvious alternative: it was relatively apolitical, I had lots of nature merit badges, I was a former plant ecologist, and it was a simple way to be a premed (like 70% of my freshman class had planned to be). In fact, I took a minimum of hard-core laboratory course in biology and was particularly attracted to related courses in psychology.

The most influential freshman course I took was *The Science of Human Behavior* with B. F. Skinner, the great prophet of radical behaviorism. The lectures were all from his textbook of that name. It contained no illustrations of any kind, no experiments, no data, and no references. Pavlov did get a few paragraphs, and Freud was reinterpreted into Skinner's system in parts of a few chapters. There were chapters on applying his "laws of operant behavior" to government, law, religion, economics, education, and most everything else.

There was something about the power of his charisma and the all-inclusive nature of his theory that absolutely captured me. It seemed to mesh perfectly with my materialistic view of the universe. Of course, I was far from alone. Well into the 1960s, Skinner and his disciples were the major force in psychology departments with the experimental study of learning at their core and their influence pervasive beyond the rat in a Skinner box to education ("teaching machines"), and social and clinical psychology ("behavior therapy"). Eventually Skinner's central lessons about careful experimental control, misuse of statistics, rejection of hypothetical "physiology" were generally absorbed and it was time to move on, in the "cognitive revolution," to the mental phenomena he tended to overlook or simplify such as attention, language, and consciousness. I worked in Skinner's lab that summer under his research associate's direction. I was a complete incompetent: putting the wrong pigeon in the wrong box, throwing the wrong switches, and being incapable of the simplest experimental psychology skill like drilling a hole or soldering a wire. I never saw Skinner that summer except when I mowed his lawn. For at least several years I continued to spout Skinnerian jargon, and

it was not until about 40 years later when I was forced to teach introductory psychology that I fully realized how totally inadequate an account of learning and life his formulations had provided.

I was also pretty awful in the only lab courses I could not avoid like Organic Chemistry. There I ran up an enormous bill by always shutting my drawer on expensive burettes and flasks in a rush to escape. We were marked on the quality and quantity of our yields, which I usually had to mop up from the floor or off my lab coat. I got so hot from anxiety that I would open the lab window and start ether fires that would sweep down the lab bench never endearing me to the other premeds.

I was such a “grind” that when the Sunday *New York Times* arrived I would hide it in the bottom of the closet until my day’s work was done. I resolved that as soon as I graduated my first priority each day would be to read the *Times*, no matter what. I have kept this resolution pretty well although when living in Beijing and Shanghai I had to cycle to a tourist hotel to obtain as a substitute, the *Herald Tribune*, and in places like Tibet and Cuba I had to read the *Times* on-line at Internet cafés while everybody around me was frantic on video games.

History of Science

To escape the usual large lecture courses, I took a graduate seminar in The History of Ideas on Reproduction before Harvey with I. B. Cohen, a distinguished scholar of Isaac Newton. Seven of us sat in easy chairs in his small cozy living room while he fingered his watch fob and his wife served tea and fruitcake on a tea trolley. In class, I reported on Ashley Montague’s Columbia Ph.D. thesis on “coming into being” among the Australian Arunta. The Arunta were an aborigine group that, apparently, did not understand the origin of paternity, how birth was related to sexual intercourse. Montague never saw an aborigine but had extensively reviewed a huge literature of missionaries, travelers, anthropologists, and theorists. His sources plagiarized, misquoted, trashed, ignored, interpreted, and misinterpreted one another in ways that made them sometimes seem even weirder than the Arunta. This was my first close encounter with professional scholarship, and it made a lasting impression. (Montague was born Israel Ehrenberg in East London in 1905, became for a while Montague Francis Ashley-Montagu and eventually a leading feminist and antiracist author of anthropology pop- and text books and finally, a colleague at Princeton.)

My term paper (the only written work in the course) was on Theophrastus, Aristotle’s successor as head of the Lyceum and known as the “father of botany,” largely because most of his nonbotanical works were lost. The only comment Cohen put on the paper was “A,” which was the modal length of comments I received on my papers at Harvard. I try to remember that when

all I can think of putting on a student's paper is "interesting" or "well written" and a grade. That term I also took History of Psychology with E. G. Boring, the leading historian of experimental psychology. Those two courses solidified my interest in the history of neuroscience, a subject I have continually worked on since I was in graduate school.

Neuroscience Begins

As a junior I took a summer course in Woods Hole in invertebrate zoology, with lots of graduate students, led by Ted Bullock, then the doyen of invertebrate neurophysiology. We had lectures all morning and then labs with great live material that we were supposed to design experiments on, but I could never quite figure out an experiment to do. That summer I also worked in the Woods Hole lab of Valy Menkin, a boyhood friend of my father. Menkin had worked with Walter B. Cannon at Harvard Medical School and was an iconoclastic student of inflammation. His disaffected son was around so I tried to ingratiate myself with him by saying the lab work was boring. So he told his father, and that was the end of what would have been my first scientific publication (Menkin, 1955).

A course in physiological psychology with Phil Teitelbaum (of hypothalamic feeding mechanisms fame) fixed my interest in what we now call "neuroscience." I then took a seminar with Don Griffin (the great experimental naturalist and codiscoverer of bat navigation) on *The Biological Bases of Behavior*. A paper I wrote for that course, "A Critical Review of a Theory of Bird Navigation," became my first scientific publication (Griffin and Gross, 1956). I then worked in his lab trying to measure the visual acuity of pigeons with a view to seeing whether it was adequate for current ideas on sun navigation. Sometimes I helped on his bat experiments. One day he said to me, "Gross, bring me the car battery from the next room" for use in an experiment. This Brooklyn boy answered, "What does a car battery look like?"

One of the perks of being elected to the National Academy of Sciences decades later was that I wrote the entry on Griffin for its *Biographical Memoirs*; I emphasized his poor performance in school, that he never put his name on his graduate student's papers, and that he attributed consciousness to animals rather "low" on the scale, for example, to bees. This was my first close experience with a highly original and accomplished scientist, and it profoundly affected me.

I got friendly with two biology graduate students, originally my teaching fellows, Bill Harvey and Frank Carey, students of the premier insect developmental endocrinologist Carroll Williams. I would take tea and hang around the Williams lab; there, one term I tried hard and failed to condition proboscis extension in the blowfly. Other later well known biologists were also my teaching fellows. Don Kennedy said my final in physiology was written

by a chicken with its left leg, and Tom Eisner really gave it to me for borrowing a bicycle from a senior professor's house (where I was house sitting with Bill Harvey) and getting it stolen.

The Biology Department was a lively place. Jim Watson had just arrived as a new assistant professor and was busy bad mouthing all the senior professors. Even the Nobelist George Wald, an early pioneer in molecular biology, was dismissed by him as "just a flower picker."

Life as a Harvard undergraduate was certainly not as much fun as high school or graduate school, but I did sample a few of its very many worlds and earned some merit badges giving me several options for the next stage in life. As I took no courses in graduate school (there were none), the undergraduate ones with Skinner, Boring, Teitelbaum, and Griffin formed the core of my formal education in neuroscience.

Cambridge University: Graduate School

Finding My Way

As a senior I had to decide what to do next: medical school or graduate school. My father, given his experience, thought an M.D. was a safer bet than a Ph.D. as it could give me an independent income. So I applied for medical school as well as National Institutes of Health (NIH), National Science Foundation (NSF), and Fulbright fellowships, all with success. I postponed admission to medical school (the Dean said, "come back anytime") as well as the NIH fellowship and took the Fulbright.

I had applied for a Fulbright to Great Britain because I spoke not a word of anything but English (though my stutter was gone by then except when I have to identify myself in the still-traumatic "around the circle" introductions). I couched my project in ethology because at that time ethology, the naturalistic study of animal behavior, in Great Britain was almost entirely done at Cambridge (under Bill Thorpe) or Oxford (under Niko Tinbergen), and that's where I wanted to go. I was awarded a Fulbright to study with Thorpe, at Jesus College and the Cambridge Zoology Department.

Cambridge (and Oxford)

To greatly oversimplify, Cambridge and Oxford are made up of financially independent colleges (e.g., Jesus) that admit, tutor, feed, and house undergraduates, whereas departments like Zoology give lectures, admit graduate students, have laboratories, and set examinations. Most faculty ("dons") have appointments in both colleges (to eat, drink, take snuff, and tutor) and in departments (to lecture and research). At Cambridge, unlike Oxford, the tutorial sessions were called "supervisions" and the tutors "supervisors." Cambridge and Oxford, being more or less identical institutions, had to give different names to virtually everything, for example, the spring term is the

Lent term at Cambridge and the Hilary term at Oxford; the final exam is the tripos at Cambridge, the examination at Oxford; the doctorate is the Ph.D. at Cambridge, the D.Phil. at Oxford; philosophy is called “moral science” at Cambridge; the length of the gown worn by undergraduates is much longer at Cambridge; the requirement for undergraduates to wear gowns at night when in the streets was still enforced in Cambridge but abolished in Oxford; the clothing required for sitting an examination was subfusc or formal only at Oxford; the biddie (female) cleans your room and washes your tea cups at Cambridge, but the scout (male) does it at Oxford, the end of the punt that you stand on when punting is opposite at the two places and so on.

U.S. graduates coming to study at “Oxbridge” usually came as undergraduates, that is, “read” for a “second B.A. degree” because (1) there was no formal graduate education like courses or exams but only writing a doctoral thesis, many of which were rejected, (2) U.S. bachelor degrees were rather looked down on, and (3) most important, being smoked at by your undergraduate tutor (cf. Stephen Leacock’s classic essay on Oxford dons) was considered the pinnacle of educational enlightenment, enabling its students to go out and make most of the globe red (i.e., British).

THORPE AND ETHOLOGY

So I arrived in September 1957 in the elegant eleventh-century rooms of Bill Thorpe in Jesus College, Cambridge (rooms that had once been part of a brothel and before that a nunnery). Thorpe was one of the founders of ethology, the study of species-specific behavior, and one of the few ethologists interested in animal learning, but under natural conditions in contrast to the Skinner boxes or mazes of U.S. psychology. More specifically, he had pioneered in the study of the interaction of experience and innate wiring in the development of bird song. We were not sure what to do with each other, but I wrote a few tutorial essays to ensure Thorpe that I knew the line on the ethological approach to animal behavior as opposed to that of contemporary experimental psychology. I went for another “supervision” to Richard Gregory in psychology who assigned me an essay on Hebb’s *Organization of Behavior*, but then he complained that my essay was just a book review. Although I was later viewed as a highly successful undergraduate psychology tutor at Jesus, as measured by the degree results of my tutees, I never really knew what a tutorial session was supposed to be. As a tutor, I would assign a subject, and then run to the library to read about it (as I had not taken any hard core experimental psychology courses) and return the books a few days later before my student wandered into the library for them. I did know, from my own few Cambridge tutorial sessions, that you were supposed to start the session by praising the student’s efforts before chopping him up into little pieces or at least trying to.

In my search for a home department at Cambridge I looked around the Zoology Department (where Thorpe was attached), but it seemed mostly

cutting up formalin-fixed animals. Then I applied to the combined psychology and physiology course (but my background was inadequate they said), and finally, the physiology course (where they really rejected me out of hand). The new History and Philosophy of Science program seemed attractive, but it would not start until the following year.

JESUS COLLEGE

So I wandered about sampling the social, intellectual, and political life that I had thought eluded me at Harvard. Jesus College was a big rowing college. Other colleges had portraits like of Darwin and Newton on their dining hall walls. Jesus had "Big Splash," a famous oarsman. The college had seen a succession of 7-foot Yale oarsmen, so the college servants seemed to think all Americans were rowers so they pressed me into the Jesus crew. I was put into the lowest, the seventh boat. (Even lower creatures, the ones whose glasses were on crooked and usually taped were relegated to the stationary "tubs" with holes in their oars.) It was the only organized sports I ever did in my life, other than required calisthenics in high school gym, until some postdocs turned me into a (slow) marathon runner in the 1990s. (Actually I ran only two New York Marathons because the first was such an extraordinary, high experience that the second was an anticlimax.)

In my first term at Jesus I lived in two magnificent lead-windowed rooms only a few centuries younger than Thorpe's ex-brothel. There was no heat in the bedroom (would have been unhealthy), but a little room was attached for the biddie to wash my teacups. Reputedly there were baths in some other building, but I never found them or looked hard. We ate in a huge hall lined with tables the length of the hall. Thus, to reach the bench on the far side of the tables the students would step up on the near bench and walk along the table, their gowns fluttering after them, until they found a place on the other side. We took bread from a passed silver platter and arranged it next to us between the inevitable muddy footprints. There was some meat thing served in silver chafing dishes and some pale green substance appropriately called "veg" because there was no way of distinguishing its origins as probably Brussels sprouts or peas or maybe green beans. One could order beer or hard cider in a pewter mug from one of the waiters. This was followed by huge serving bowls of a "trifle," some sweet pudding concoction of cream, cake, and gelatin. The undergraduates bolted their food wordlessly and then hurried across and along the table, if on the far side, and out the hall. Meanwhile the Fellows at High Table at the far end of the hall were just beginning their nightly banquet. Once to try to get the attention of my fellow students and start a conversation, any conversation, I waved a little silver dish that said Col. Jes. on it and yelled, "Hey look, I'm stealing the college silver" and stuck it in my pocket. Nobody blinked. I still have it, stashed among obsolete stuff like ashtrays and Japanese tea ceremony brushes. Eventually, I started getting little cards in my college mailbox from undergraduates inviting me for

(hand-ground) coffee in their rooms after dinner, the appropriate site for conversation.

One day in Hall I attempted to sit down at the Boat Club table where special delicacies like orange juice for breakfast were served. At the head of the table, The Captain of the Boats, d'-_____ (some long Norman name), explained that this was the Boat Club table, apparently not recognizing my service in the galleys. That seemed a good time to end my athletic career. I don't know how long the seventh boat waited for me that day before promoting someone from the tubs or finding another American. Anyhow it had been hard work keeping the guy in back of me from jamming his knees into my back and from "catching a crab" and being catapulted into the water. So I missed rowing in the "bumps" the first big race of the season. In fact, I never saw a boat race except on TV.

At Harvard I had been a premed grade grubber shunned, I imagined, by the literary sets, so at Cambridge I set out to write an article for every single literary magazine. I wrote an article "Science and the Control of Behavior" for *Granta* (not yet a famous magazine), film and book reviews and a special issue on science for *Cambridge Review*, "Understanding Men and Monkeys" for *Varsity*, and "Psychodrama" for *The Play's the Thing*, a theatre magazine that failed before they published me.

It was strange and liberating to be in an environment where being a communist was just another political persuasion or perhaps just another English eccentricity. There were even Communist Party officers among the senior dons. I was an officer of a club called "The Heretics" that had been founded before World War I. My principal duty was to have fancy dinners with the interesting speakers we invited.

Life as Larry Weiskrantz's Student

MEETING LARRY

Finally, after 6 months of sampling, I trudged back to the Psychology Department to try to get into its undergraduate program. I was ushered into the office of its Professor (i.e., Chair) O. L. Zangwill. He was the son of the distinguished Anglo-Jewish writer Israel Zangwill (who coined the term "melting pot" for the United States, though he had never been there and also wrote the classic "King of the Schnorrers"). O. L. Zangwill was a founder of modern neuropsychology and one of the first to grasp the perceptual functions of the right hemisphere. He sat there in a pin stripe suit, chain smoking, and never looking me in the eye (or anybody else's I discovered later). I told him I wanted to read psychology. He asked me if I knew any psychology. I used to cram for my finals in a cubicle in Lamont Library at Harvard surrounded by psychology textbooks and suddenly the rows of titles that had surrounded me in that aroused state flashed across the screen and, tired of

being rejected by all the other Cambridge departments, I started to read off the titles. After a few rows of bookshelves, he stopped me and suggested I should become a research (i.e., graduate) student rather than an undergraduate and sent me off to a Dr. Weiskrantz.

So I went through the creaky wooden halls looking for that old German and came across a young American fellow in a tan lab coat and sunglasses (science faculty wore white lab coats in lieu of the Arts faculty's clerical gowns; and technicians and janitors wore this tan coat). It turned out he was Larry Weiskrantz. The dark glasses were because his regular pair had been broken in an accident. Larry had just been hired by Zangwill to start a new monkey research lab. I told Larry who I was, and he invited me to join him to work on "the effects of brain lesions and drugs" on monkeys. That seemed interesting and lo, then and there, I became Larry's research student and for more than the next 50 years his colleague and friend. Larry came from the Girard Orphanage in Philadelphia, studied psychology at Swarthmore, got a MSc. from Oxford and a Ph.D. from Harvard. He was Karl Pribram's graduate student. Pribram along with his students, particularly Larry and Mort Mishkin (D. O. Hebb's student at McGill), set the standards for the modern experimental study of the functions of the primate cerebral cortex in the use of surgical techniques (Pribram had been a human neurosurgeon), behavioral analysis, and anatomical reconstructions.

At about this time, at the end of my first Michaelmas (fall) term, an Indian Prince was given my rooms and I moved out into digs. I rarely returned to Jesus. Work was beginning as I was now Larry's research student and I was starting to meet my kind of English people. Thus began among the best intellectual and social years of my life, but this is not the place for the social part.

The only requirement for the Ph.D. was a dissertation and an oral defense. There were no required courses, seminars, or examinations. I did go to a variety of lecture series over the 4 years I was at Cambridge such as by Horace Barlow on cerebral cortex, Giles Brindley on the visual system, Zangwill on neuropsychology, and Richard Gregory on perception. Alas, but the great Lord Adrian, founder of modern neurophysiology, had retired the previous year, so my only contact with him was when he introduced visiting speakers or when I almost ran over the little man when I was in my girlfriend's car and he on a bicycle

PH.D. THESIS: FRONTAL CORTEX

My dissertation research was on the effects of lesions of the frontal cortex (now usually called "prefrontal cortex") in macaques. In the 1930s Carlyle Jacobsen and John Fulton had shown that frontal lesions made monkeys and apes unable to perform "delayed response." In this task the monkey sees a peanut hidden under one of two cups and after a brief delay tries to retrieve the peanut from one of the cups. This was actually the first objective evidence

for a severe and permanent cognitive deficit after experimental damage to a specific region of the brain. It also led directly to the tragedy of many tens of thousands of humans receiving frontal lobotomies. (Fulton reported at an International Congress in 1935 that two chimpanzees, Becky and Lucy, had delayed response deficits after frontal lesions and, incidentally, that Becky was no longer upset at all when she made errors on the task, unlike before the operation. The Portuguese neurologist Egas Moniz was in the audience. Hearing how unperturbed Becky was after her surgery, he rushed home to begin the frontal lobotomy craze for which he received the Nobel Prize in 1949.)

Jacobsen and Fulton thought the delayed response deficit was one of “recent memory,” but this was only one of several interpretations when I began my work. Frontal lesions also produced impairment on auditory discrimination learning. Finally, a third effect of frontal lesions was the production of locomotor hyperactivity. My research had two main questions. First, could the three symptoms be produced by different lesions of frontal cortex: could the syndrome be fractionated? The second question was what was the nature of the delayed response deficit. This was approached by studying what other tasks animals with frontal lesions could and could not perform in order to infer the underlying dysfunction.

Previously, most primate learning and cognition studies were carried out manually, particularly in the “Wisconsin General Test Apparatus.” For example in the delayed response task, the experimenter would bait one of the two cups in the monkey’s view but out of his reach, lower a screen between the cups for a few seconds, then raise the screen and let the monkey choose a cup. I set out to automate this task and other tasks such as visual discrimination learning. Not only did this eliminate interaction between the experimenter and the monkey but by using drops of water or small sugar pellets as a reward, the animal could be trained and tested for an order of magnitude more trials per day. Some of my ideas for all this had probably come from my time working in Skinner’s lab where all the experiments and data collection were automated, often ingeniously. (Similar automatic apparatus are now in widespread use for primates, but not in my time.)

Before I started this work, about the only tools I had ever used were an axe, a knife, a hammer, and, inadequately, maybe a simple screwdriver. When a chair in our house broke, my father would put it in the cellar until his “handy” friend would arrive with his toolbox to fix it. So to build my research devices I had to learn from scratch about the many kinds of screws and nails, about taps, dies, and drill bits, about power saws and drill presses. I controlled my devices with electromagnetic switching circuits identical to the ones I failed to build or understand when I worked in Skinner’s lab but now had to master. Although I was proud of learning these skills, they really gave me little intrinsic pleasure. So as soon as I had research assistants or graduate students or money to pay shop personnel, I was happy to abandon

these hard-learned skills except for wiring an occasional lamp or hanging a picture at home. However, this experience helped me later in dealing with shop technicians, and I always encouraged my students to acquire machine shop skills.

I renewed my Fulbright for a second year. At the end of that year, taking what the Harvard Medical Dean had told me literally, I wrote to Harvard Medical School and said I would like to enter the following September. Some secretary sent me the entire application for admission, either by mistake or by design as some motivational test. I took one look at that massive pile of forms on why I wanted to be a doctor and so on and pushed it off my desk into the waste basket at which point I was transformed from a premed into a psychology graduate student. I reactivated my NIH predoctoral fellowship for my two remaining years in England and lived rather royally on it.

As recounted in an oversize doctoral thesis and several papers, I found that different partial frontal lesions could produce the delayed response and auditory discrimination deficits independently (reviewed in Gross and Weiskrantz, 1964). I suggested that the delayed response deficit was due to the inability to use information near the time of its input. This view was not all that different from Jacobsen and Fulton's original interpretation, or for that matter from the contemporary one of a deficit in "working memory." The changes in locomotor activity were interpreted as due to increased reactivity to external stimulation and unrelated to the other symptoms. Today, none of these papers is ever cited except by my students, and then rarely. *Sic transit gloria.*

About 6 months after I started writing the historical introduction to my thesis, I had reached Galen in the second century (who had actually studied the effect of frontal lesions in piglets.) At that point Larry said that I "had better get on with the more empirical parts of the thesis." So my thesis never had any historical introduction at all. However, that 6 months provided the seeds for my book *Brain, Vision, Memory: Tales in the History of Neuroscience* (1998) as well as for several other papers not included in that book.

MY FIRST SCIENCE PAPER

My first science paper was with Larry and Buba Mihailovic showing that the frontal delayed response deficit could be produced reversibly by electrical stimulation of lateral frontal cortex (Weiskrantz et al., 1960). We published an expanded version to *Brain*. Buba was visiting from Belgrade and stayed with me. You could not win a political argument with him because he would always resort to the fact that when captured by the Nazis as a Communist partisan he at one point had to dig his own grave and lie in it, thereby making his political opinions inviolate. He was a chain smoker and threw his butts and empty cigarette packs on the floor because cleaning them up was "women's work." Actually his wife had been a Communist partisan too, but she stayed in Belgrade so I don't know how she dealt with this problem.

MY FIRST NATURE PAPER

One day at the Departmental tea, Professor Zangwill expressed some skepticism of the practice in the United Kingdom of awarding support for graduate work only to students who received a First Class Degree. This refers to the grade on the final (and only significant) exam at Cambridge and Oxford. In terms of frequency it might be similar to a summa cum laude in the United States, but it requires much more originality, (“cleverness” in the British sense) and less rote learning. Zangwill wondered whether all Fellows of the Royal Society had received Firsts. I turned to the graduate student sitting next to me, Liam Hudson, a recent Oxford graduate and said, “Let’s find out.” Because the class of degree of graduates of Oxford and Cambridge was readily available in any large library, as was the roster of the Royal Society, we checked one against the other and wrote a letter to *Nature* that about one quarter of the F.R.S.’s who went to Cambridge or Oxford had not received Firsts (Gross and Hudson, 1958). This, my first *Nature* paper, yielded more correspondence and more immediate press coverage than any paper I ever wrote until my paper with Liz Gould on cortical neurogenesis (Gould et al., 1999a). Although apparently upsetting to the British notions of hierarchy and cleverness, our results did not surprise me. Some F.R.S.’s had gotten in as explorers or inventors and never went to graduate school. A few were ill during exam time. At least some must have just had the drive and brilliance to circumvent the usual path to academic success, hardly a surprise to somebody from a country that had produced Benjamin Franklin and Thomas Edison.

LIFE IN LARRY’S LAB

Besides my thesis work I had several little projects. I collaborated with Larry and another graduate student, John Oxbury, on several drug studies. I presented one at a meeting in Rome and hitchhiked there by way of Athens. This was the beginning of my infatuation with travel especially on a shoestring. Among my other nonthesis nonpop publications was one on the effect of gymnecic acid on rat’s taste. Gymnecic acid is from an herb that blocks sweet taste in humans, and we claimed it did so in rats too. Among the studies that never got published was one with Alan Cowey, Larry’s third research student, on learning in planaria, including after they had been cut in half. We collected an obscure local species and chuckled that its rarity would make it hard for others to fail to repeat our results because they couldn’t get the species. However, we never got any reliable results ourselves. We continued to collaborate after graduate school, rather more successfully, and Alan became a lifelong friend.

A study of the effect of hypothalamic and amygdala lesions in rats on adulterated food also got nowhere because, unlike the control and unoperated animals, the lesioned rats just totally stopped eating and would have starved if I had not given them palatable food. After my roommate Carey McIntosh saw me operate on a rat, he couldn’t eat for a while either.

In England, one could not do surgical procedures on a monkey without a Home Office license, and one could not get such a license without experience in surgery. Larry got around that because he had been trained by Pribram in the United States, but technically Alan and I could not do surgery except with Larry. However, when Larry went on sabbatical leave in my third year, I needed a frontal cortex lesion made on a trained animal so Alan and I operated anyhow. We intended to remove the principal sulcus, a very prominent sulcus on the lateral surface. During surgery, it looked a bit “atypical” and a year later, on autopsy, it turned out that we had only removed half of the sulcus. (Alan was not responsible: this was “my” part of the brain.) So I learned two (obvious) lessons about surgery: (1) make the opening as big as you need to really see where you are and (2) if something looks “atypical” or different, you may just be lost (a lesson good for map reading too). Larry had left without putting any one of us “in charge.” Later, when I had a lab of my own I realized the value of that approach. Usually when I went away for, say, 3 months to China or someplace, I just said good-bye and in my absence everything ran more or less fine because everybody had his or her responsibilities. One year I left someone in charge, and that person was transformed into a petty dictator even complaining about the time other people came to work.

Once, in collaboration with Larry, I was supposed to film some monkeys with brain lesions reaching for food. Somehow I had held the camera upside down so I when I showed my results to Larry I had to hold the projector upside down. Nary a criticism came from Larry, but the next time he locked the door and did the filming himself.

APPRECIATION OF LARRY

I am now very embarrassed about how little I appreciated Larry while I was a graduate student. As I was leaving Cambridge I even commented how little I had learned as his graduate student: I complained that I still had only the faintest idea what the frontal cortex did. About a year later I was invited to write a review of my thesis results for a small meeting on the frontal lobes to be followed by a book. I asked Larry if he wanted to be a coauthor of my contribution (Gross and Weiskrantz, 1964) and he said, “Yes, please. After all you could not have done the work without me.” I suddenly realized that this was equally true for four of the five empirical papers from my thesis I had already submitted without even showing them to him, let alone making him a coauthor (e.g., Gross, 1963a, 1963b). Now, more than 40 years after he took me as a student I understand how supportive and tolerant he had been and how absolutely critical he was for my development as a scientist.

Even outside of the laboratory, my 4 years in Cambridge, England, were probably among the richest years of my life. I met a number of wonderful and very special people, including my first wife, Gaby Peierls. Many of these have

continued to be close friends even when years and an ocean intervened. I lived in several extraordinary households and traveled widely in Europe. I sat at the feet (or at least at the tables) of unique savants such as Sol Adler, Rudy Peierls, Joseph Needham, and Jerzy Konorski and acquired several permanent friends particularly Maggie Berkowitz (nee Angus) and Bob Young. I hope to recount these marvelous adventures in the future when there is more time and space.

As Larry's student I became a permanent if junior member of an extended family studying the behavioral functions of the cerebral cortex. That family included my seniors such as Karl Pribram (Larry's teacher), Brenda Milner (Zangwill's student), Mort Mishkin (Hebb's student and Pribram's postdoc), and H.-L. Teuber (my postdoc advisor), as well as peers such as Pat Goldman (later Goldman-Rakic) and Charlie Butter. Everyone in this group has always been amazingly friendly, supportive, and collegial to me and I assume to each other.

YOU CAN'T GO HOME AGAIN

Although this is true in profound and trivial ways I often tried to anyhow. I returned to England on sabbatical leaves twice. Both times were to Oxford rather than to Cambridge because Larry Weiskrantz and Alan Cowey had moved there, Larry as head of the Department. The first time, in 1990, Larry had arranged a Visiting Fellowship for me in Magdalen College. I was given magnificent rooms overlooking the deer park. The only problem was that, given the college's monastic tradition, my wife Greta was not allowed to stay in my room so we had to rent a flat. Conversation at dinner at high table was often very exciting, except when one would get stuck next to a local vicar. I wasn't supposed to bring my wife there either, although some fellows would just bring each other's wives as guests. Toward the end, I actually did bring in Greta a few times. They even offered her snuff when we retired to the "desert" room for brandy. England, with its toleration for nonconformity, often treated women as slightly eccentric men.

On my second sabbatical to Oxford, I was a visiting fellow of Wolfson College. This was a new college consisting only of graduate students and faculty. It had no high table at all, and we were given nice coed rooms. It may have been democratic and nonsexist but, frankly, it was rather dull after the medieval tomfoolery of Magdalen. Besides jogging and kayaking around, I spent valuable time in the Physiology Department's magnificent history library. I also visited Cambridge for the first time since I had left. The office that I had shared as a graduate student with Alan Cowey and about four others seemed tiny and dirty. This was also true of some of the houses I had lived in. Apparently, things often look smaller when you go back to them, although I was certainly about the same size, if not a bit shorter and wider.

Coming to MIT

Hans-Lukas Teuber

When I received my doctorate in September 1961 and asked Larry from whom I would learn the most on a postdoctoral fellowship, Larry said, “Teuber—he could not stop teaching every time he opened his mouth.” Hans-Lukas (“Lukas”) Teuber had just been appointed the head of a new Psychology Department at the Massachusetts Institute of Technology (MIT). A man of great erudition and charm, Teuber and his colleagues at New York University-Bellevue Medical Center had played a major role in establishing human neuropsychology as an experimental science closely linked with contemporary neurophysiology and experimental psychology. When he came to MIT, psychology was only a section of the Department of Economics and Social Science. It had no undergraduate or graduate program, and very few undergraduates took its courses. Yet it had a number of distinguished psychology faculty members such as David Green, John Swets, Ron Melzack, Roger Brown, and Michael Werheimer. Teuber brought two postdocs from New York University (NYU): Steve Chorover and Joe Altman. They and I went along with Lukas to the psychology faculty meeting as voting members, which enraged the older faculty, a rage directed at Lukas, not especially at us. After a turbulent year, the entire previous psychology faculty had departed, and we were a Department of Psychology with a graduate program and a building of our own, and Altman, Chorover, and I were assistant professors. Walle Nauta was the first senior appointment and probably the first distinguished neuroanatomist in a Psychology Department. Emilio Bizzi, a single neuron physiologist, was another unique appointment, and the philosopher Jerry Fodor was hired as a junior appointment. Our new department was well on its way to becoming the first Neuroscience Department, combining what would be called “cognitive psychology” with neurophysiology, neuroanatomy, linguistics, and computer science and would be a model for neuroscience departments around the country.

A major factor in the growth of the department was Teuber’s extraordinary success in teaching introductory psychology. He was a marvelous lecturer and gave all the lectures twice in the fall and spring term. The course soon became the most popular one in the Institute. Along with some of the junior faculty I taught a discussion section in the class. Although as a naïve and hypercritical purist I was often disturbed by the way he oversimplified and distorted the evidence, pandered to the audience, and graded very easily, I learned an enormous amount from Lukas about how to lecture.

I had come to Teuber to learn neuropsychology: to work with brain-injured human patients. He had no access to patients but arranged for me to go over to Norm Geschwind’s aphasia unit at the Boston V.A. hospital to work with Harold Goodglass. Goodglass, understandably, thought I was coming to work on his research whereas Teuber somehow thought I could

gain access to the V.A. patients for his own work. So after a few very instructive weeks watching Norm examine patients and Harold and Edith Kaplan give psychological tests and giving a few myself I went back to MIT. That was the end of my training in human neuropsychology.

We had not yet moved into our building at MIT and were in Building 20, a “temporary” three-story wooden structure built in 1943 and a legendary hotspot of creativity. Among our neighbors were Walter Rosenblith’s Communications Biophysics lab with cat auditory physiology and the early use of small computers in neurophysiology, Jerry Lettvin’s frog neurophysiology lab, and Noam Chomsky.

Rat Lesions and Hamster Curiosity

While I was waiting for the monkey colony to be built in the new building, I carried out some studies on brain lesions in rats with Steve Chorover (e.g., Gross et al., 1965). In one we studied the effects of circumscribed cortical lesions on several maze and discrimination tasks. Our results suggested that Karl Lashleys’s finding that the size of the cortical lesion, not its site, determined the size of the deficit in maze learning could be accounted for by the fact that larger lesions encroached more and more on multiple, distributed mechanisms important for different aspects of maze learning.

With an undergraduate Peter Black we measured spontaneous alternation as a function of intertrial interval and found that the hippocampal lesioned rats seemed to learn less but forgot at the same rate as the controls (Gross et al., 1968). This rather clean result got lost in the morass of contradictory results on hippocampal lesions in rats. I was bitten too often by the rats and avoided them in the future.

My first graduate student at MIT was Jerry Schneider, who had never studied psychology before. His wife had bought him a pet hamster but would not let him train it by food depriving it and using food reward, as was the wont of experimental psychologists. So he rewarded it on various learning tasks by letting it run around his apartment for a few minutes. Using this reward he replicated many of the laws of learning as part of his introduction to psychology. We brought 27 nonpet hamsters into the lab and studied the use of exploration as a reward and wrote a paper on it called “Curiosity in the hamster” (Schneider and Gross, 1965). We also carried out some learning experiments on the tree shrew, *Tupia glis*, and tried unsuccessfully to breed them. Tree shrews were of some interest because they were very visual and were thought at the time, erroneously, to be primitive primates. When I left MIT, Teuber held on to Jerry; and for his dissertation Jerry contrasted, in hamsters, the spatial functions of the superior colliculus with the pattern recognition functions of striate cortex and anticipated Ungerleider and Mishkin’s (1982) deeply influential two visual system idea (Schneider, 1967). I worked with another MIT graduate student, Michael Potegal, on the effects of caudate nucleus lesions in rats and cats.

Beginning to Study the Cortex of the Temporal Lobe of Monkeys

The macaque colony I designed at MIT for about 40 monkeys was based on Larry's in Cambridge copied in turn from Pribram's. It made life simple that this colony was exclusively for the use of my students and that we were our own veterinarians.

Discouraged by my inability to understand the frontal lobe, I decided it lay in an inaccessible limbo bearing little relationship to anatomy, physiology, and psychology. (How completely wrong I was demonstrated by Pat Goldman's [-Rakic] subsequent brilliant application of anatomy and physiology to understanding the frontal lobe.) So I decided to turn my attention to the cortex on the inferior convexity of the temporal lobe: inferotemporal cortex later known as inferior temporal (IT) cortex.

This story begins in 1938 with Klüver and Bucy's demonstration that temporal lobectomy produces an impairment in object recognition and visual learning as well as a variety of other, somewhat weird, behaviors for a monkey, such as docility, indiscriminate sexuality, and eating ordinarily inedible objects like feces and bolts. This complex of symptoms became known as the "Klüver-Bucy" syndrome. Chow, Mishkin, and Pribram then "fractionated" the syndrome by showing that the changes in visual recognition and visual learning could be produced independently by lesions of IT cortex and that the other changes, like docility and indiscriminate sexuality could be produced by lesions confined to the amygdala, a large subcortical nucleus within the temporal lobe. A number of subsequent studies, particularly from Pribram's laboratory, showed that the IT deficit in visual recognition is only visual, exists in the absence of any changes in visuo-sensory thresholds, and occurs for a great variety of visual learning tasks as long as they are sufficiently difficult. The IT deficit in visual cognition is similar to human "visual" agnosia, a term first used by Freud. The tale is told in Gross (1973).

At first, it was puzzling how an area so far from striate (or primary) visual cortex could be visual in function. By the time I began my work at MIT it was realized that the visual functions of IT cortex depended on a multisynaptic cortico-cortical input from each striate cortex. Later, it became clear that the monkey's cortical mantle between striate and IT cortex contained a multiplicity of visual areas now known as V4, TEO, and others.

My initial work on monkeys at MIT involved studying the effect of IT lesions on visual perception and learning (see below; reviewed in Gross, 1973). But then, as this was the time of the brilliant successes of Hubel and Wiesel in using single neuron recording to study visual cortical function, I had the rather obvious thought that single neuron recording might help in understanding the role of IT cortex. But I had never seen a microelectrode or turned on an oscilloscope, so I decided to seek a new postdoctoral position where I could learn the requisite techniques. When I told this to Teuber he said, "don't go" and offered to pay to set up an electrophysiology lab for me.

When I told him I wouldn't have a clue as to how to use it he suggested I collaborate with George Gerstein, a postdoc in the Communications Biophysics lab at MIT, and he had agreed to do so. George was doing single-unit studies of the auditory system of cats and knew about electrodes and oscilloscopes. Even then he was a pioneer in the development of computer analysis of single neuron activity, having been the first to use poststimulus time histograms.

George and I set out to record from IT cortex in awake monkeys during the performance of visual discrimination tasks because, as he often chanted, "the cortex dissolves in anesthesia." We decided to begin by recording surface potentials from IT cortex during visual discrimination learning on the grounds (that now seem silly) that this would help us to know what to look for with single-unit recording (Gerstein et al., 1968). At about this time, Herb Vaughn (visiting from Albert Einstein Medical School) and I carried out a study of the effect of optic tract and various cortical lesions on cortical evoked responses (Vaughn and Gross, 1969). Both studies convinced me of the futility of recording gross potentials from the cortical surface, at least in my hands.

In 1964, before we recorded from our first inferior temporal neuron, Gerstein left for the University of Pennsylvania. (This was related to a disagreement with his lab head Walter Rosenblith. They, with others, were involved in developing early laboratory computers such as the LINC-8 and the dispute involved money, power and status.) Because George was now a long-distance collaborator, I decided to radically simplify the planned experiment so that I could carry it out without him holding my hand. The simplest experiment I could think of was just to ask whether IT neurons responded to visual stimuli and to use anesthetized animals. Because Teuber had raised the "double dissociation" paradigm to a commandment, for control stimuli we used auditory stimuli and, for a control area we recorded from the superior temporal gyrus, believed to be an auditory analogue of IT cortex. Soon another postdoc who had also come to work with Teuber, Peter Schiller, joined me. He had been trained as a clinical psychologist and had then done very innovative work on visual masking. (His father was the ethnologist Paul von Schiller and his stepfather no less than Karl Lashley: name-dropping was one of the habits I acquired from Teuber.) Even for the time, our experiment was beyond simple: it was naïve and simplistic. For example, the standard visual stimuli we used were diffuse light, already known to be rather ineffective for cortical neurons. Moreover, the monkey's eyes were uncorrected and merely covered with a viscous silicone fluid to prevent drying out; the fovea and other retinal landmarks were not located. The animals were immobilized and anesthetized.

By vigorous averaging of the responses to 100 or more stimulus presentations, we managed to get IT responses to diffuse light in about a quarter of our sample; no IT cells responded to the auditory stimuli. We found the

opposite pattern in the superior temporal gyrus. For “about 30 units” in IT we did try “moving and stationary circles, edges and bars of light projected on a screen” and found no responses and therefore “an absence of evidence for receptive fields.” We interpreted these results as reflecting one or more of the following: (1) “an organization fundamentally different from that found” by Hubel and Wiesel in visual cortex of the cat; (2) failure to use sufficiently “adequate,” “optimal,” or “appropriate” stimuli; or (3) use of anesthesia (Gross et al., 1967). The unfocused eye was another possibility.

So we decided to return to our original plan of recording from awake behaving animals and, because of some of my concurrent behavioral experiments on IT lesions and attention, to study unit activity during “attention” rather than during visual learning. On Peter’s suggestion, we set up a board in front of the monkeys with little windows to which we could apply our eye or present such objects as a wiggling finger, a burning Q-tip, or a bottle brush, stimuli that elicited attention until the animals got bored. Most of the units responded vigorously to such stimuli, and we classified them as “attention units” because they fired to any stimulus that seemed to draw the animal’s attention, or, at least, any stimulus that would elicit continued fixation at the stimulus as reflected in an electrooculogram. These observations were made on several monkeys and with a number of collaborators, such as Peter Schiller, George Gerstein, and Alan Cowey, my friend from graduate school, and were published over a decade later (Gross et al., 1979). We interpreted these results as suggesting that these neurons either were involved in some attentional mechanism, had foveal receptive fields, or both.

AN ARREST IN THE LAB

My first tech at MIT did everything: brain histology, assistance at surgery, data analysis, training animals in Wisconsin Boxes and automatic boxes. Then, one Friday afternoon, federal, local, and university agents showed up to arrest for her for having sold LSD to an undercover federal agent in the lab. She kept them waiting 2 hours until she had finished an experiment. I found about the arrest only the following Monday morning from a barrage of phone calls from MIT officials as to whether we were making LSD in the lab. When, furious, I asked the other lab members why they had not told me about her arrest, they said, “We assumed you knew all about it . . . as usual.” That was my first lesson that I would be the absolutely last person to hear about nonscientific happenings in the lab (especially who was sleeping with whom). Later, she was the link to my visiting appointment at Berkeley and still later a Professor of Behavioral Sciences.

On the Harvard Faculty

Just a Visitor

In 1963, I readily accepted an invitation from the Harvard Psychology Department, presumably on Teuber’s recommendation, to teach an undergraduate

course in physiological psychology as a part-time “visiting lecturer.” This was the same course in the same lecture room that I had taken with Teitelbaum and that had really got me into the field. Thus, as I mentioned before, it was a very big thrill for me. I worked very hard preparing my three lectures a week, almost every night, most weekends, and any days I was not recording or operating. There were no adequate textbooks then, so all the readings were from primary sources. As this was before the time of photocopying services for course readings I got Harper and Row to publish a three-volume set of some of the reading for the course (Gross and Zeigler, 1969). When good textbooks started to appear such as the third edition of Morgan’s *Physiological Psychology* (1965) and Thompson’s *Foundations of Physiological Psychology* (1967), I was really annoyed because they were so similar to my lectures and figures, that I saw no point in trying to publish my own text. I continued to use my lecture notes, with yes, some updates, for the next few decades.

Two woman students came to work with me from Harvard. (They were still called Radcliffe students and given Radcliffe degrees although Radcliffe had had no classes or faculty of its own for the previous decades!). Martha DiNardo, later Neuringer, studied classical conditioning in monkeys with IT lesions for her undergraduate thesis and became a lifelong friend and, after a week with my wife and me on Lake George, she and her husband became permanent outdoor people. Rhoda Kessler came from a Brooklyn working-class background to Richard Herrnstein’s lab at Harvard. He refused to even talk to her. So I took her on as my student, and she did a thesis on the effect of caudate lesions on behavior in rats. Later, as Rhoda K. Unger she became a major feminist scholar and activist. Her account of the intense gender oppression she was subjected to at the beginning of her career is worth reading (Unger, 1998).

The situation for women in neuroscience has improved since then. Yet, although for some time women have made up a large proportion of undergraduate majors in biology and psychology and of graduate students in neuroscience, they are still markedly underrepresented at the top of the profession as, for example, in the National Academy of Sciences. Attrition occurs at many stages for several reasons (Committee on Maximizing the Potential of Women, 2007). Attrition at the postdoctoral level seems to be particularly related to the conflict between career and child care and could be markedly ameliorated by greater University financial support for child care and a change in the division of function between parents.

At this time I taught my first graduate seminar, which was one of the most exciting I ever ran. The topic was Comparative Psychology. The students were a heady mix of Harvard Skinnerians, MIT Chomskyites and physiological psychologists and included Bill Baum, Alan Neuringer, Laurel Furamoto, Larry Marks, Don Pfaff, and Whitman Richards. I really feel that my undergraduate and graduate classroom teaching was at its best in those early years and then steadily declined, hopefully at a slow rate. Perhaps only at the

beginning of my career did I have a grasp of a wide swath of the field, maybe just because the field was so much smaller.

I Move to Harvard

Harvard offered me an assistant professorship in 1965. Everybody advised me strongly against leaving MIT and taking it: you'll never get tenure, they said (as I very well knew from the department's past behavior). Teuber was devastated and offered to go the MIT Dean at once to try and get me tenure there. With no hesitancy I decided to take the Harvard offer. The reasons I gave myself were that my life at MIT under Teuber's protection was not the real world. For example, he had already gotten me onto, successively, two NIH study sections, sent me to represent him at fancy meetings, bought anything I wanted while I had a little NIH grant of my own (for the façade of independence) and what was perhaps most valuable, immeasurably so, he gave me copious suggestions, corrections, and rewrites for the multiple drafts of my papers from my thesis and after. Exactly why I was in a hurry for the real world, and why Harvard in a twisted way was that, I am not sure. The call to Harvard, at least to its graduates, is irrationally powerful. When I decided to leave, Teuber not only would not talk to me but also turned and went in the opposite direction when he saw me. Since, subsequently, I was satisfied with my research and teaching at Harvard, it probably was not an error to have left MIT to go to Harvard, however strange the reasons seem now. As had been the case with my graduate advisor Larry, I greatly underestimated how much I had learned from Lukas. I have written several appreciations of him and his building of the first neuroscience department in the world (e.g., Gross, 1994, 1999).

As I was designing my new monkey quarters for Harvard, two papers came out that claimed memory could be transferred from one rat to another by injections of brain RNA extracted from the first rat and injected into the second. I thought if this were true, maybe I didn't need monkey cages to study memory. So Frank Carey, my teaching assistant friend from undergraduate days, and I tried to repeat this memory transfer and failed (Gross and Carey, 1965). After an attempt, a few years later, to test an idea of Karl Pribram's with an undergraduate Phil Schwartzkroin, class of 1968 and Alan Cowey (Schwartzkroin et al., 1969), I finally realized, a bit belatedly, that spending time trying to falsify an intrinsically absurd idea was a waste of time.

Studies on the Temporal Lobe

RECORDING WITH ROCHA-MIRANDA AND BENDER

At Harvard I continued my IT single-unit work now with Carlos Eduardo Rocha-Miranda and David Bender. Carlos was a Brazilian aristocrat who

had worked with Madame Denise Fessard and Wade Marshall and really wanted to work with me on developmental visual physiology in opossums not on IT cortex. He later did it with brilliant success and became Brazil's leading visual neurophysiologist. Dave Bender was the son of a famous Harvard Dean (cf. Karabel, 2005) and had taken my undergraduate Harvard course in physiological psychology. When he came in for a recommendation and I discovered he was an engineering major, I said "You're hired, start work this afternoon." Carlos stayed 3 years and then sent some of his former students to collaborate with me. Dave worked with me for about a dozen years as undergraduate, technician, graduate student, postdoc, and research associate. He recently retired as Professor of Physiology at SUNY Buffalo. The three of us worked and argued vehemently for days and nights for those 3 years about everything from where to put a bolt in the relay rack to what it all meant. It was the heyday of Kuhn's "scientific revolutions" and from the beginning we thought that, for better or worse, we were doing "nonparadigmatic" science.

We were not sure how to test the "some attentional mechanism" hypothesis about IT neurons, so we decided instead to test the "foveal receptive field" idea by trying once more to plot receptive fields in an immobilized animal. This time we used nitrous oxide and oxygen for anesthesia, and we set out to teach ourselves how to use an ophthalmoscope, a retinoscope, find the fovea, use contact lenses, measure expired CO₂, etc., etc. The full story of this "learning experience" is some place between a stand-up comedy routine and a morality tale about letting total ignoramuses unfettered into a lab with expensive equipment and monkeys. A few examples. We couldn't find instructions on using a retinoscope that we could understand. When we asked ophthalmologists about the multiple images we were seeing instead of a single one, they were frightened off, thinking we scientists knew something that they didn't. (First-year medical students quickly learn to suppress the irrelevant images, I discovered later.) Finally we found a "Flight Surgeon's Manual" that started: "aim the beam at the center of the patient's chest, then follow the buttons up"), and we could follow its instructions. One Sunday we accidentally lost a monkey by attaching the air input to the output valve of our new respirator. (At first we thought this taught us a lesson about working on Sundays, but it never stuck.) At the start we used a number of monkeys before we found a single IT cell that we held long enough to see if it responded to visual stimuli. One very important lesson we did learn is never, never argue with your collaborators when sleep deprived. (Pity the poor undergraduates who tried to complain about something as I was staggering home after an all-night-plus recording session.)

Eventually, we got all the many pieces working at once, and lo and behold, IT cells responded to visual stimuli but only in certain parts of the visual field and that part had to include the fovea (which we had eventually learned to find with an ophthalmoscope and then, with a prism, project back

onto the screen). That is, we had found that IT cells do have visual receptive fields, and unlike those previously described in other visual areas these receptive fields were not retinotopically organized and always included the fovea. Their large size was unusual, and it was unique that the receptive fields sometimes extended into the ipsilateral half-visual field. Although light slits and dark bars would sometimes elicit a response from IT cells, we soon realized that more complex stimuli including colored pictures and three-dimensional objects were almost always better in driving IT cells. A few cells responded best to faces and a very few to hands (Gross et al., 1969, 1972). A “hand” cell was found before the first “face cell.” Here is an early description of that finding (Gross et al., 1972):

One day . . . having failed to drive a unit with any light stimulus, we waved a hand at the stimulus screen and elicited a very vigorous response from the previously unresponsive neuron. We then spent the next 12 hr testing various paper cutouts in an attempt to find the trigger feature for this unit. When the entire set of stimuli used were ranked according to the strength of the response that they produced, we could not find a simple physical dimension that correlated with this rank order. However, the rank order did correlate with similarity (for us) to the shadow of a monkey hand. (pp. 103–104).

There was no mention of the “hand unit” in the draft of our 1969 *Science* article when I asked Teuber to read it, in part because of how helpful he had been with my previous papers and in part to help “make up” for abandoning him for Harvard. He knew about the “hand cell” and urged us to put it in the article and we did.

WHY WE FOUND FACE AND HAND CELLS

The stimuli we soon began to commonly use to elicit responses from IT cells, namely, brushes, faces, hands, feathers, pieces of fur, and other objects, were far from the usual visual stimuli of the time like bars and slits. Why did we use them, and more important why were we primed to notice responses to such stimuli? There were several factors that probably lowered our threshold to use such stimuli and to find cells responding to them.

First, a few years earlier I had been the guest of the Polish scientist Jerzy Konorski who was unusual in being both very smart and very knowledgeable about human clinical neurology and visual physiology and animal learning and the cognitive effects of lesions in monkeys. Integrating data from these fields he had postulated the existence of “gnostic neurons” such as ones selective for faces, facial expressions, body parts, simple objects, or scenes and had suggested they would be found in inferior temporal cortex (Gross, 1968; Konorski, 1967).

Second, we had begun these IT studies at MIT in the department of neuropsychologist Lukas Teuber who would often tell stories about prosopagnosia (agnosia for faces) after temporal lesions.

Third, our first lab at MIT in Building 20 was down the hall from Jerry Lettvin's lab. He was working on bug detectors in the frog (Lettvin et al., 1959) and had invented the term "grandmother cell" (Gross, 2002). It was Horace Barlow (1953) who first used the term *bug detectors*, and I had heard him lecture on the subject when I was a student in England.

Finally, we were in the same institution, if across the river, from Hubel and Wiesel who had just published on hypercomplex cells in V2 of the cat and had suggested that cells with even more complex properties would be found beyond V2 (Hubel and Wiesel, 1965).

Thus it is not surprising that we found face and hand cells in this environment! What is surprising is that for some time our findings on the unusual receptive field properties of IT cells and our finding of face- and hand-selective cells seemed to have little or no impact on the field. Although we published in such high profile places as *Science* and the *Journal of Neurophysiology* there were no attempts to replicate and extend (or deny or even comment in print on) any of our results until 12 years after our initial paper. One of the reasons for the skepticism or sheer disbelief in our results may have been because of our somewhat sparse use of quantitative methods, objective data collection, and mechanical stimulus presentation. Another reason may have been our use of even more unconventional stimuli than hands and faces, such as a toilet brush, a picture of which we had published (Gross et al., 1977). The editor had demanded we remove the figure with the toilet brush so we just eliminated a different figure and renumbered the remaining ones. I am still not sure why oval-shaped toilet brushes were often good stimuli for IT cells. One suggestion was that it was because all the experimenters had beards. One of the very first groups to finally test and replicate some of our basic findings successfully used a toilet brush too (Richmond and Wurtz, 1982).

Whatever the skepticism about our claims, it did not seem to interfere with our ability to get published or to obtain grant support or jobs. When replications of "hand" and "face" first appeared they were by two Brits, Edmond Rolls and Dave Perrett who were considered "a bit flakey" themselves by many in the field, perhaps further delaying general acceptance of our findings (Perrett et al., 1982).

Are the face and hand cells found in IT cortex examples of the "grandmother cells" of Lettvin (in Barlow, 1995), cells that respond only to a specific visual concept, such as your own grandmother "however displayed, whether animate or stuffed, seen from behind, upside down, or on a diagonal, or offered by caricature, photograph or abstraction"? Are they examples of the "gnostic" cells of Konorski (1967), neurons that represent "unitary perceptions"? The available evidence provides an overwhelming "no" to both possibilities.

IT cells that respond only to a specific object, such as the face of one individual, and continue to do so across various transformations have never been seen. Rather IT face cells respond in varying degrees to a set of faces and never solely to one. Different IT cells show a different pattern of responses to a set of faces. Thus the coding of faces (and presumably other objects) appears to be done by the pattern of firing over a set of cells, that is, by what has been termed “coarse coding,” “ensemble coding,” “population coding,” or “cross-fiber pattern coding.” This absence of one cell–one visual concept is true for both natural stimuli such as faces as well as for arbitrary stimuli that evoke responses of IT cells after explicit training (Gross, 1992, 2002).

However, something closer to true grandmother cells may exist elsewhere than monkey IT cortex. Quiroga et al. (2005) reported cells that certainly seem to fit the criterion for a grandmother cell in the medial temporal lobe of human patients. For example, one such cell in the hippocampus fired only to a variety of images of one individual (known to the patient) including in various costumes and views and even to her name in letters and not at all to images or names of a number of other individuals also known to the patient.

BEHAVIORAL EFFECTS OF IT CORTEX LESIONS ESPECIALLY WITH ALAN COWEY

Starting at MIT and continuing at Harvard, we carried out a number of experiments on the behavioral effects of IT lesions parallel to the single-unit recording experiments. One series was carried out with Alan Cowey, who came from Cambridge to Harvard for a year, and Harvard graduate student Rick Manning. We compared the effects of lesions of Area TE (or anterior IT cortex) with those of a more posterior region area we called “foveal prestriate cortex” (a combination of Area TEO and what is now known to be the central representation of visual area V4). We thought the results indicated that the more anterior lesion affected primarily visual memory and the posterior lesions impaired visual perception (e.g., Cowey and Gross, 1970; Manning, 1971; Manning et al., 1971; reviewed in Gross, 1973). Although this idea, deriving from Mort Mishkin (Iwai and Mishkin, 1969), is still widely accepted, it is only a first preliminary step in understanding the role of the temporal lobe in visual recognition.

Earlier experiments, largely from Pribram’s lab, had failed to find effects of IT lesions on visual acuity, visual perimetry, or critical flicker frequency. My very long time collaborator, Dave Bender (1973) extended these findings of no sensory losses after IT lesions by finding negative effects on backward masking and on detection of a brief stimulus.

One of the major subcortical outputs of IT cortex is to the caudo-ventral putamen. To see whether it was part of a circuit involved in visual pattern recognition Al Buerger, a postdoc, Carlos, and I studied the effects of its destruction on visual and auditory learning and a delayed response task. Its damage

only impaired visual learning, supporting the idea that the caudo-lateral putamen is part of a circuit specific for visual pattern learning (Buerger et al., 1974).

Marlene Oscar-Berman, a postdoc in my lab and Simon Heywood, a graduate student from Oxford, showed that animals with IT and prestriate lesions during visual discrimination tasks look longer at one stimulus and switch less frequently between stimuli, perhaps because they have trouble recognizing it (Oscar-Berman et al., 1971).

The Psychological Round Table (PRT)

After a few years at Harvard I was invited to become a member of the Psychological Round Table (PRT). This was a secret, self-perpetuating club who seemingly thought themselves the best and the brightest psychologists under age 40. It had been founded in 1936 as a Young Turk rebellion against the elite Society of Experimental Psychology founded by Titchner in 1904. When I was elected, PRT was all male and consisted almost entirely of experimental psychologists, mostly from Harvard, Yale, Princeton, Penn, McMaster, and a few other Eastern schools. There was no program distributed; you had to be prepared to give your talk (“revelation”) at any moment. The discussion was superficially “lively” but usually more jocular than serious. The gavel was a brass penis and testicles. The big event was the Saturday night lecture that was, to be generous, rather crude pornography, slides of women in various states of undress. Later, slides of various varieties of sexual activity became more common.

In the late 1960s I stopped going on the grounds it was sexist and anti-democratic but never really communicated my views to the membership. I returned in 1974 with Naomi Weisstein, a distinguished perception student and militant feminist, to raise these issues. By this time there were woman members, the officers were still self-perpetuating but known, and there was a greater range of departments represented. The Saturday night porno lecture continued often with woman speakers and male genitalia. Members were still expected to be quiet about the organization.

Many PRT members were really upset when they heard I was planning to write an article on it (Gross, 1977). They felt I was trying to destroy “the most important intellectual event of the year” for them. PRT presumably helped me get my job at Princeton because at that time, I think all the tenured members of the Princeton Department under age 40 were members. In 1994 I was elected to the grown-up version of PRT, namely the Society of Experimental Psychologists. Now it was all right to put membership on your resume, because, I guess, when you’re old it’s acceptable to announce you’re a member of an “elite” club. I have gone only to the two meetings that were held in Princeton. There were no porno lectures.

Antiwar Activities and the Harvard Strike

My time teaching at Harvard coincided with rising protests against the Vietnam War. Although my anguish and anger over the war filled a not small part of my consciousness, my antiwar activities were pretty trivial compared to those of many around me. I was active in the Boston Area Faculty Group on Public Issues (BAFGOPI), which organized teach-ins, marches, and demonstrations and raised money and signatures for advertisements against the war. Our leader was Salvador Luria who was the most marvelous combination of a Jewish and Italian comic besides having won a Nobel Prize. Although I went to many antiwar events and had members of various student antiwar groups in my lab, I knew little about what was brewing among student leaders.

On about noon, April 9, 1969, a group of students led by SDS (Students for a Democratic Society) “occupied” University Hall, central home of the Harvard deans, and they and their staff left or were pushed out. Many students milled around the occupied building or entered it out of support. By 4:15 Franklin Ford, the Dean of the faculty, over a loud speaker had announced “anyone failing [to depart] will be subject to criminal trespass.” When I had entered in my professorial tweeds, someone rushed over and yelled, “Get this faculty member out: he will report our names.” Somebody with more clout, apparently, said, “Don’t worry. He can stay. He’s just a ‘CP-liberal.’” And so, after all these years being semiclosed as a clandestine red diaper I was suddenly a “Communist Party-liberal,” the SDS leadership being so far left that the difference between the “old left” CP and a liberal was insignificant.

There were about 400–500 students and a few faculty members in the Hall. Assembled in a large, packed room, the group voted nonviolence toward the police, decided to leave the doors open, and set up various rules and committees to get food, keep things clean, and other housekeeping on the general assumption that, as at Columbia University the previous year, the occupation would go on for days. Finally, the meeting was over, and most of the crowd left for their rooms, planning, as I did, to come back the next day to the occupied building, now “Che Guevera Hall.” I drove home to work on my next day’s lecture. About 150, including some teaching fellows and one faculty member, stayed overnight.

At dawn Over 500 helmeted and face-shielded police came in swinging. About 50 students required medical attention, some for serious injuries. The student body was outraged, and an overwhelming majority eventually supported a total strike demanding the end of ROTC, stopping Harvard expansion into poor neighborhoods, establishing a Black Studies Department, and not punishing the building occupiers (collectively known as “the eight demands”). Much of rest of the term was spent in interminable meetings, from small faction planning groups to an estimated 10,000 assembled in Harvard stadium. The faculty was split into a “conservative” and a “liberal” caucus. Stephen J. Gould, then another assistant professor, and

I were in a minuscule “radical” caucus led by Hilary Putnam, perhaps the leading philosopher of the day. We essentially supported the SDS positions. As a group we even walked out of the Commencement exercises. We were so inconsequential that I could not even find us mentioned in the several detailed histories of the strike I examined recently (e.g., the even-handed Eichel et al., 1970 and the conservative Rosenblatt, 1997).

There was a great flowering of strike art and rhetoric. One poster summed it up:

STRIKE FOR THE EIGHT
 DEMANDS STRIKE BE
 CAUSE YOU HATE COPS
 STRIKE BECAUSE YOUR
 ROOMMATE WAS CLUBBED
 STRIKE TO STOP EXPANSION
 STRIKE TO SEIZE CONTROL
 OF YOUR LIFE STRIKE TO
 BECOME MORE HUMAN STR
 IKE TO RETURN PAIN HALL
 SCHOLARSHIPS STRIKE BE
 CAUSE THERE’S NO POETRY
 IN YOUR LECTURES
 STRIKE BECAUSE CLASSES
 ARE A BORE STRIKE FOR
 POWER STRIKE TO SMASH THE
 CORPORATION STRIKE TO MAKE
 YOURSELF FREE STRIKE TO
 ABOLISH ROTC STRIKE BECAUSE
 THEY ARE TRYING TO SQUEEZE
 THE LIFE OUT OF YOU STRIKE

-poster by striking students at Harvard Graduate School of Design.

During the summer, when no one was around to protest, about 15 SDS leaders were expelled or otherwise punished, and the one faculty member arrested in the building was fired. In the longer run, the University mostly met all the “eight demands.”

A moment of activist glory came a year later on May 5, 1970, when I cochaired a mass meeting to protest the U.S. invasion of Cambodia. Over 3000 convened in Harvard’s four large lecture halls linked electronically. The meeting voted to strike against the Southeast Asia war, the oppression of political dissidents, particularly the Black Panthers, and the multiple involvements of universities in the war. The *Harvard Crimson* said, “The chairmen generally succeeded in keeping the heated meeting in order.” My

picture was in the upper left of the front page; on the lower right was the now iconic woman with outstretched arms over the dead body at the Kent State shootings by the National Guard the previous day.

Getting Fired from Harvard

In 1970, as expected, I did not receive tenure at Harvard. The previous year my title had changed from assistant professor to lecturer so maybe I was at risk of becoming a graduate student again. My years at Harvard had been very good. I had superb students and plenty of space. Originally, I was given space for a monkey colony and a few rooms and offices. But as a senior appointment in physiological psychology was never made, I expanded until I occupied the entire eighth floor of William James Hall except for the shops, which we were the principal users of anyhow. I taught what I wanted when I wanted. I had no committee assignments or administrative duties. Not only did I not feel obliged to suck up to the senior faculty but I had little contact with them. They never came on my floor, and there were no social occasions.

The only time I saw the senior faculty was at the monthly departmental meeting, and those were always entertaining. B. F. Skinner was constantly feuding there with S. S. Stevens, the founder of modern psychophysical scaling. Both spoke only the hermetic jargon each had created so no communication was possible. George Miller, a past-president of the American Psychological Association who had among many other accomplishments introduced computers to psychology, cofounded cognitive psychology, and started experimental psycholinguistics, had been a Harvard graduate student so the older faculty continued to treat him as one. (Later, he became my colleague at Princeton and was a close friend and mentor of my high school age son.) Georg Von Bekesy who received a Nobel Prize for his work on audition couldn't even come to the meeting because he did not have a teaching appointment. He would have liked a professorship, but I doubt if he cared about the meeting. The chairman when I arrived was E. B. Newman who wasn't even deemed worthy of a professorship but was a lecturer. One day he turned to another assistant professor (who was an ordained rabbi) and asked, "What do you people do with sour cream? Pour it over your head?" That this remark continues to rattle around in my head suggests I am still sensitive to anti-Semitism.

I did have some trouble with Richard Herrnstein, who was the chairman for most of my sojourn at Harvard. He was a Skinnerian who had written an inflammatory racist article in the *Atlantic Monthly* and later expanded it into a book, *The Bell Curve*, with Charles Murray. He was on the floor below me, and the waste plumbing from my monkey colony occasionally leaked into his lab and some of my rats escaped to his office. Off Harvard grounds

he did secret research for the Army, teaching pigeons to recognize Viet Cong and radio their location back to airbases. Whereas I thought his racist writing was well within his academic privilege, I made it clear that I thought his secret research put him in the category of a war criminal. (This was at the height of the Vietnam War.) Luckily, my secretary, Maureen Ashby, was a tony Cantabrigian M.A. that intimidated him, so she dealt easily with him and I never had to.

Berkeley Interlude

Although I had been appointed Professor of Psychology at Princeton in 1970 I did not arrive until 1971. I spent the intervening time as the guest of Walter Freeman in the Department of Physiology and Anatomy in Berkeley (and with the help of an NIH fellowship). He was the son of the Freeman that brought us outpatient frontal lobotomies with an ice pick; he was a great guy as well as an early prophet of computational neuroscience. My first contact with him had been when he had called me for help with my first tech at MIT (the one arrested in the lab) who was now working for him. She had been arrested again, this time for possession of marijuana and had used her one phone call to tell him she would not be in to get the cat ready for the day's experiment. He needed my help for her bail because all his cash was tied up in bail for anti-Vietnam War demonstrators. (In the end, a third phone call got her mother to bail her out.)

There were many things surprising and wonderful about Berkeley. It was really puzzling how Berkeley could be invariably rated as one of the two or three greatest research universities in the country (or world) when, as far as I could see, the faculty spent hours daily eating fabulous lunches at outdoor cafes. Maybe it was a Heisenberg uncertainty problem: they were only sitting around in the sun drinking Chardonnay because they were entertaining me.

Politics in Berkeley, at least to a visitor like me, was like Nirvana. The day we arrived there was a massive demonstration at city hall demanding 24/7 free child care. It was less weird when I discovered the City already provided more child care than virtually any U.S. municipality does even today. The City Council was dominated by the left with nary a Democratic let alone Republican member. When we had visitors the first tourist stop was the COOP, the first food store we had ever seen whose primary purpose didn't seem to be to steal your money and make you obese or otherwise ill. It is gone now, but stores across the country now try to imitate it. I did my first and last door-to-door political canvassing for a major party candidate, Ron Dellums, the long-time Congressman from Berkeley and now mayor of Oakland. Life in Berkeley deserves another memoir, especially if I have to go back for data. When I left, my friends gave me a little sachet of dirt labeled "holy soil." I still miss the place (and the time).

At Princeton University

Getting Arrested

One of the first things I did at Princeton, or, at least, one of the first memorable things, was to get arrested at an anti-Vietnam War event. It was for trespassing on the grounds of the Institute for Defense Analysis (IDA), a federally funded think tank on campus that carried out war-related secret research. That day 225 demonstrators including seven Princeton faculty were arrested and charged with “interfering and molesting.” As we filed into the Trenton courthouse, one cop, on seeing me said to the cop standing next to him, “Hey, there’s an old one.” And I was in my mid-thirties! We were released after paying \$100 bail. Most of us eventually paid \$100 in fines; a few, not me, instead went to jail for 10 days.

Although my father asked a bit nervously what my colleagues thought of my arrest, he seemed more proud than anxious and stopped asking me to wait. I am embarrassed to say, that when later I obtained my FBI files under the Freedom of Information Act, the arrest in Princeton was the major item. I guess I had taken my father’s advice.

The afternoon after the arrest, Mother’s Day, I chaired a mass (for Princeton) “Mother’s Day Rally/Teach In for Peace.” In preparing this memoir, I found my opening statement:

Today is mother’s day and this meeting is dedicated to the millions of mothers who have been murdered and maimed by this horrible war, who have seen their children and husbands destroyed, burned and tortured, who have experienced the devilish ingenuity of American technology—the bombs that flatten the area of football fields and the plastic flechetes that defy x-rays, designed by our own IDA and finally to the mothers who for countless generations will bear misshapen monsters due to the genetic effects of the defoliants sprayed by American planes.

There doesn’t seem to be many rallies against the Iraq War in Princeton.

Another Monkey Colony

At Princeton, for the third time, I set up a monkey colony. As at MIT and Harvard, for about the first 15 years at Princeton, we handled all aspects of the colony ourselves and very rarely called for veterinarian help. Then the University hired a “consulting veterinarian” who stopped in occasionally and eventually took over supervising the cleaning of the colony. More recently a full-time University vet was hired and then an “animal facilities manager.” These steps were due to increasing regulation of primate facilities by the federal government and more recently the stricter Association for Assessment

and Accreditation of Laboratory Animal Care (AAALAC). The animals and experimenters may or may not be better off now, but life was certainly much simpler in the old days.

Life In My Lab

Perhaps the greatest satisfaction in my scientific life has been the success of so many of the people who spent time in my lab at MIT, Harvard, and Princeton. Of course, to what extent their success was due to entirely to their own efforts and to what extent due to the environment we created is impossible to assess. What is clear is that my success depended on their efforts.

LAB TECHS

The group I am really most proud of, and perhaps most unjustifiably, are the women who worked as lab techs. Many had not been biology or psychology majors. After a few years in my lab, most of them went to graduate school and are now accomplished figures in neuroscience research. This cohort includes Charmane Eastman, Rush University, Laura Frishman, University of Houston, Christine Curcio, University of Washington, Vicky Ingalls, Marist College, and Susan Volman, NIH. I doubt if my record of techs into full professors has often been surpassed, even today. A few went into medicine and are on medical school faculties such as Carolyn Wells, Yale, and Lynn Seaford, Washington University. I would not be writing this memoir except for their intelligence, competence, and loyalty. The only explanation I have for this high yield is that I was so incompetent, bumbling, and all thumbs in the lab that they thought if I could get by in neuroscience, they certainly could. It might be noted that during most of this period there were initially almost no women and then very few tenured women at the institutions I was at and only a very few senior women in all of neuroscience.

GRADUATE STUDENTS

I have had only a small number of graduate students, and never more than two or three at a time. Fortunately, the universities I was at often attracted good graduate students, even when, as was the case at Harvard, the only neuroscientist on the faculty (outside of the medical school in far away Boston) was one unknown starting assistant professor (me). I did spend a lot of time and energy recruiting and selecting graduate students. They usually spent 2 or 3 days visiting my lab, including hanging out during long boring experiments. They often stayed at my house and visited twice, before and after they were admitted. Several had worked in my lab as undergraduates or technicians. Thus by the time the prospective students and I had to make our decisions to commit to each other, we had more of an idea of what we were in for than is usually the case. For whatever reasons, virtually all of my Ph.D.'s have been professionally successful and continue to do neuroscience. So far, they include two members

of the National Academy of Sciences, two members of the American Academy of Arts and Sciences, two National Academy of Sciences Troland Awardees and one former Howard Hughes Professor. (The weaker graduate students I may have driven out by my sarcasm or something.)

At least by the time I was at Princeton certain customs were established. On their arrival, I would tell the new student what was going on in the lab and provide a copy of the last grant. In the next few weeks, they would usually choose to work with an advanced graduate student or postdoc on an ongoing project. Starting their own project before they knew how to do anything usually did not work. The senior and junior students would then publish together and then either continue collaborating or, more often the formerly new student would find his or her own project, later to be joined by a new incoming student.

We had weekly lab meetings for at least 90 minutes over lunch (during one decade everybody ate quarts of yogurt) that took priority over experiments, surgery, life, everything. There we would take up proposed experiments, recent results, paper drafts, new directions, papers, and grants that I was given to review, critiques of a recent speaker and if there was nothing else, some recently published papers. Frank critiques, often rather brutal, were the tradition, especially with regard to new experimental proposals and interpretation of results. Meetings were often heated, which, at least I thought, was really healthy. (Sometimes visitors were shocked by what they perceived to be sibling rivalry, hostility, and excessive competition.) Usually I would work with a student alone in preparation for the meeting and then again after the meeting to pick up the pieces, if needed. Administrative things like ordering animals and equipment, scheduling surgery were dealt with and the tech often had “who left the mess?” business. During a dull experiment I once complained to a student that the *Iliad* was a bloodthirsty bore. He replied, that, on the contrary, the story of Achilles was a key to understanding a certain arrogant and difficult colleague. So we spent the next lab meeting on the *Iliad*, and he convinced me of his point.

There were separate sessions for practicing talks, especially because sessions for job and other long talks often went on at great length. For some students a single session with little comment was enough, whereas some otherwise equally good students would require a dozen sessions to mold a job or colloquium talk into shape.

Someone leaving the lab, getting his or her degree, or my birthday were occasions for spit roasting a goat, pig, or lamb in my backyard. This was an all-day affair with much basting and drinking. Over the years we had lab expeditions to the White Mountains (when we were in Boston), canoeing on the Delaware, camping on Lake George, hiking from my house in Woodstock, N.Y., and, lately, often to the local authentic Szechwan restaurants.

I really understand nothing about being a student in my lab, but some hints come from their comments:

“When I came to Charlie’s lab it was like another, very different world unlike anything I had previously experienced or heard about.”

“Charlie does not just work with you; he moves in with you.”

Sometimes the students would come in to carp. Once a male in jeans, sandals, and work shirt came in to complain about a female graduate student’s outfit of heavy makeup, high heels, long red fingernails, etc. being inappropriate for a monkey lab. I told him, perhaps too vehemently, (1) that in my day no one in his outfit and without their tweed jacket etc. would be allowed in the lab, (2) that it was none of his business what anybody wore, and (3) to get out of my office. I think I did complain about students walking around the lab and in the monkey room barefoot. Now, under current rules, my normal clothing prevents me from even entering the animal quarters.

POSTDOCS, VISITORS, AND UNDERGRADUATES

About half the people who got doctorates from my lab stayed as postdocs (and then sometimes research associate or lecturer) for 1 to 15 years, about 4 years being the mode. I would joke that this was because they had learned so little in the first 4 or 5 years, they would try again. In fact, it was a great deal for them because by this time they were usually working and publishing independently, and I was doing the fund-raising, housekeeping, supplying them with tech help or eager undergraduates, teaching and dealing with the world. Several people came back to the lab after a few years of graduate student or postdoc experience elsewhere (3 days in one case), usually claiming they had more freedom in my lab. I also had several great postdocs who had not been my graduate students.

I was really lucky to have super faculty visitors who stayed from 1 to 3 years and usually returned for shorter periods such as Alan Cowey, Carlos Rocha-Miranda, Ricardo Gattass, and Charlie Butter. Besides collaborating with me on experiments they played a very major role in training, keeping me and the students sane, and helping run the place.

A number of undergraduates researched in my lab and some first authored papers. Of course, many students in my undergraduate classes became successful at whatever, including neuroscience, but that has little to do with me and more with the good students that MIT, Harvard, and Princeton had gathered.

AUTHORSHIP OF STUDENT PAPERS

The basis of assigning authorship was never explicit, but in practice was someplace between that of my undergraduate advisor Don Griffin and what is common today. Griffin never put his name on any of his students’ research papers, and they were only coauthors when they helped with Griffin’s own research.

What seems common today is that every paper that comes out of a Principal Investigator's lab or was supported by his or her funds gets the P.I.'s name on it. About 20% of the papers from my lab did not have my name on them, usually because they were from a student's thesis, by a postdoc, or because I had little involvement.

GRANTS

I always enjoyed writing grants. It was a very intense and very social cooperative effort with all members of the lab involved. The main support for the lab came from a single NIH grant started in 1964 and renewed about every 5 years through 2004. We would usually start working on the renewal 3 months before it was due. I managed to expand the scope so that in its last year it was supporting, among other things, research on parietal and premotor cortex, adult neurogenesis, functional magnetic resonance imaging (fMRI) on monkey temporal lobe, and, as usual, some neuroscience history. Sometimes there were ancillary grants from NSF, NIH, or private sources, sometimes with postdocs as the PI. The role of my students working on my grants is reflected in the fact that their own early grants looked just like mine even when very different in content (just as when visiting their labs I saw reflections of mine).

The first Study Section I sat on was exclusively for psychology fellowships. A small group of us, particularly Colwyn Trevarthan, would delight in awarding people who looked promising and had no relevant background at all, especially none in psychology. Some are now successful neuroscientists. The second Study Section I sat on was for research grants and more serious. When Roger Sperry joined, I hoped I would find out how the brain worked. But it turned out his ratings were simply inversely proportional to the budget and the only equipment he thought worth buying was a dissecting microscope. By my third Study Section I was already a tenured professor and could not understand many of the applications. So thereafter I usually refused to serve on Study Sections as it was a bit depressing to realize how out of it I seemed to be (or less likely, how misguided the field was).

Research in My Princeton Lab

When I was at MIT and Harvard I was willingly to be the advisor for undergraduate or graduate research on a wide range of topics as long as the student was really enthusiastic and the project seemed feasible. Thus, besides monkeys, we worked with iguanas, owls, rats, hamsters, tree shrews, and cats. At Princeton I became more focused (or narrow) and would usually only support experimental research on brain mechanisms of vision or memory.

Major exceptions were when I collaborated with other faculty particularly Marc Bornstein on babies and Liz Gould on adult neurogenesis and in advising undergraduates. All Princeton undergraduates have to do an experimental

or “library” thesis. Given that Princeton is inundated by jocks, I supervised (too) many library theses by women on exercise-induced amenorrhea and by men on traumatic amnesia.

Recording from the Temporal Lobe

How does visual information reach IT cortex? In collaboration with Mort Mishkin, we showed that the visual responses of IT cells depend on input from striate cortex over a route that includes the corpus callosum and anterior commissure (Gross et al., 1977; Rocha-Miranda et al., 1975). By contrast, lesions of the inferior pulvinar, which might have provided an alternative pathway for visual information to IT cortex did not eliminate IT responses to light. We also continued our studies in awake monkeys trained in visual discrimination and on Konorski’s “recent memory” task. We found that the activity of about half the IT cells sampled reflected the animal’s recent experience; the IT cells were coding short-term memories (Gross et al., 1979).

BORDERS OF IT CORTEX

The first new graduate student to work on IT neurophysiology at Princeton was Bob Desimone, who came in 1974 and then stayed, eventually as a postdoc until 1980. A few days after he arrived he came into my office and told me the various ways the running of the lab could be improved. Eventually that skill made him the Scientific Director of the National Institute of Mental Health and then the Director of the McGovern Neuroscience Institute at MIT. He showed his dedication to science equally early. He was learning to perfuse a monkey over a sink and had forgotten to take out its contact lenses, which then went down the drain. As Dave Bender, who had been teaching him later reported to me, “Desimone will do. He took apart the whole plumbing of the sink until he found the contact lenses, then put it back together.”

Previously, we had sampled from only a limited portion of IT cortex. Desimone developed methods for repeatedly recording from the same animal when immobilized and anesthetized that enabled him to sample the visual properties of neurons throughout the temporal cortex. He found that the basic properties of IT neurons, as described above, were similar throughout cytoarchitectonic area TE: receptive field size, inclusion of the fovea, laterality were all similar. However when moving dorsal, ventral, or anterior to Area TE the cells were no longer only visual but were polysensory: they were always visual but sometimes also responded to auditory and/or somesthetic stimuli (Desimone and Gross, 1979).

SUPERIOR TEMPORAL POLYSENSORY AREA AND BIOLOGICAL MOTION

Charlie Bruce (postdoc, 1977–1979), Bob and I then studied the area dorsal to Area TE, which we termed the “superior temporal polysensory” area or STP.

Neurons in STP had several intriguing visual properties. Like IT neurons they were not retinotopically organized but had larger receptive fields. Like in IT cortex, some responded best to faces. Most were sensitive to some type of motion including complex movements such as in depth or radially symmetric about the center of gaze or more extraordinary movements, as in this description from the original paper (Bruce et al., 1981).

a person walking within the visual field was more effective than any other stimulus tested . . . the pattern of movement generated by walking and not the person per se was crucial . . . a person seated in a moving chair or a person walking with the lower part of the body shielded elicited little or no response. Inanimate moving objects also elicited little or no response. The angle subtended by the person . . . and the persons . . . size and clothing were also irrelevant. Half of these units responded preferentially to particular directions of walking. (p. 374)

This was the first published description of neurons sensitive to “biological motion.” For some reason we never explored this phenomenon much further, devoted only a few sentences to it, and didn’t even mention it in the paper’s abstract. Dave Perrett and his colleagues, at St. Andrews, soon replicated and greatly extended these observations (e.g., Perrett et al., 1989). Much later, in a collaborative effort with Lucia Vaina of Boston University and Harvard Medical School, we had evidence for a similar superior temporal area in humans involved in biological motion (Vaina and Gross, 2004).

Another unusual property of many superior temporal polysensory (STP) neurons was their multisensory responses: responding to sounds and/or touch as well as visual stimuli. Later Earl Miller (graduate student, 1985–1990), and Carol Colby (postdoc, 1983–1989) had evidence that the visual and auditory responses were correlated. Bruce, Desimone, and I found that the properties of STP depended on striate cortex and the superior colliculus, unlike IT cortex, which is totally dependent on its striate input (Bruce et al., 1986.)

Perhaps the reason why we never further studied the biological motion and polysensory properties of STP is that we already had enough “unbelievable” results on face and hand cells. In retrospect, it was a mistake not to have done so.

MORE FACE CELLS AND STIMULUS INVARIANCE

As I mentioned above, most of our early evidence for face and hand cells might be thought of (especially by hard-core visual physiologists) as rather informal. Finally, in 1984, Desimone, Bruce, and Tom Albright (graduate student and postdoc, 1979–1987), and I submitted a more quantitative description of face and hand cells to the *Journal of Neuroscience* (Desimone

et al., 1984). The editor, Max Cowan, rejected it on the grounds that these phenomena had been reported, and this paper was nothing new. I wrote back telling him to just view the paper as a replication of previous results, which would be useful because no one had believed them. He accepted the paper by return mail without comment. That and an earlier paper with Eric Schwartz, then of NYU, Bob, and Tom also showed that IT cells showed invariant responses to shape over changes in size, contrast, or retinal location (Schwartz et al., 1983). The cells acted like perceiving organisms!

INFANT MONKEYS

Are monkeys born with face cells or do they develop them with experience? Hilary Rodman (graduate student, 1981–1986; postdoc etc., 1986–1989; 1992–1995) and I received a grant to study this. We were going to raise monkeys from birth without their seeing faces and then determine if they had face cells. We had solved all the technical problems but could not bring ourselves to actually deprive infant monkeys of seeing faces. So instead with Jim Skelly (graduate student, 1985–1990, later renamed Seamus O’Scalaidhe) we studied normally raised infants and found face cells as early as 6 weeks of life that was as early as we could record in awake monkeys (e.g., Rodman et al., 1991, 1993).

One of the difficulties of working with monkeys was that it was very upsetting to me and most lab members when a monkey died or had to be “sacrificed” at the end of an experiment to locate its electrode tracks or lesions. It was particularly traumatic when an infant monkey died.

I also worked with C. Y. Li on face cells in infant monkeys in Princeton and in his lab in Shanghai. Going to work each day in a Shanghai lab as I did on one sabbatical or teaching in Peking University on another added a dimension to being in China besides that of the usual tourist. Overall, over seven visits I traveled around China for a total of many months by boat, bus, truck, four-wheel drive, plane, bicycle and worst of all, horseback, including to Tibet, Inner Mongolia, Xinjiang, the borders with Vietnam and Burma, visited innumerable temples and monasteries, climbed several holy mountains, and ate a lot of really great street food.

LEARNING AND CIRCUIT PROPERTIES OF IT CELLS

Paul Gochin came as a postdoc (1987–1995) from my old collaborator George Gerstein, thereby reviving our joint efforts. With Earl Miller, a graduate student (1985–1990), we studied the circuit properties of IT neurons and their ensemble coding (e.g., Gochin et al., 1991, 1994). Paul also wrote a number of modeling papers on IT cortex (e.g., Gochin, 1996). Paul and Earl carried out several innovative studies on the attention and habituation properties of IT cells (Miller et al., 1991, 1993). Earl later expanded this work into an important series of studies when he went to work with Bob Desimone at NIH. Earl was an unusual graduate student because he never

stayed as a postdoc. Instead, he went to work with Bob Desimone who had moved to NIH, but maybe that was a little like staying in the lab.

IT AND HIPPOCAMPAL NEURONAL ACTIVITY DURING SHORT-TERM MEMORY

Mike Colombo (Rutgers graduate student; Princeton postdoc 1989–1992) compared the responses of IT and hippocampal neurons during visual and auditory short-term memory tasks and examined the role of activity in the delay period as a possible “mnemonic trace” (Colombo and Gross, 1994). Later, with Tom Fernandez a class of 1992 undergraduate, and Kotuku Nakamura, a postdoc from Japan, we found that the posterior hippocampus tended to be more involved in spatial processing and the anterior hippocampus in directing movements to points in space (Colombo et al., 1998).

ON AREA MT

Area MT, the middle temporal area, is an extrastriate cortical visual area that was known to be particularly sensitive to the direction of stimulus motion. As a graduate student and a postdoc, Tom Albright made major contributions to understanding its organization and functions. First he demonstrated that MT was organized into cortical columns sensitive to the axis or direction of movement (Albright et al., 1984). This was the first demonstration of cortical columns in a visual area outside of striate cortex. Then he showed that there were two types of MT cells, one sensitive to movement of contours and one to movement of an entire pattern (Albright, 1984; Rodman and Albright, 1989). His demonstration of pattern motion selectivity in MT was actually prior to that of Adelson and Movshon (Movshon et al., 1985), but theirs was so much more elegant that Tom’s earlier observations were lost, which never seemed to bother him. In work begun at Princeton, Albright (1992) showed that MT cells were sensitive not only to luminance contrast borders but also to borders defined by motion contrast and by texture contrast, that is, their motion sensitivity was form invariant.

Hilary Rodman, Tom, and I found that after the inactivation or removal of striate cortex, the majority of MT neurons were still sensitive to the direction of stimulus motion (Rodman et al., 1989, 1990). We showed that this residual motion sensitivity depended on input from the superior colliculus. Thus the superior colliculus may be responsible for the sensitivity to direction of visual stimulus motion that survives striate lesions in humans and monkeys, that is, for blindsight.

Behavioral Effects of Temporal Cortex Lesions

INTERHEMISPHERIC TRANSFER

Why is a rose a rose wherever its image falls over the central retina? Lynn Seacord (undergraduate class of 1975, then tech, 1975–1977), Mort Mishkin

and I found an absence of interhemispheric transfer of visual pattern information after IT lesions. We suggested that the similar response properties of IT neurons in both halves of the visual field were the basis of perceptual equivalence for patterns in the left and right visual fields. By extension, we suggested, the large receptive fields of IT cells provide for perceptual equivalence across retinal translation within as well as between each visual half field (Gross and Mishkin, 1977; Seacord et al., 1979).

DISCRIMINATION OF ROTATED FIGURES

On very easy visual tasks animals with IT lesions may be essentially normal, whereas they usually find very difficult visual tasks virtually impossible to learn. There is one interesting exception: Animals with IT lesions can learn to discriminate normally between two identical objects rotated 60 degrees or more from each other. Ed Holmes (graduate student, 1974–1980) and I suggested this was because the control animals have normal shape constancy, view the rotated stimuli as being the same things and therefore hard to tell apart, whereas the animals with IT lesions have impaired shape constancy, see the objects as different, and therefore can tell them apart more easily, thus eliminating the difference between the groups (e.g., Gross, 1978; Holmes and Gross, 1984).

EFFECTS OF SUPERIOR COLLICULUS LESIONS ON ORIENTATION

Although the superior colliculus had been implicated in visual orientation and localization, there was little direct evidence of such functions in primates. Working with Diane MacKinnon (undergraduate, 1971–1973), Dave Bender, and Visiting Professor Charlie Butter, we obtained such evidence (Butter et al., 1978; MacKinnon et al., 1976).

EFFECTS OF SUPERIOR TEMPORAL ASSOCIATION CORTEX LESIONS ON AUDITORY LEARNING

Mike Colombo, working with Hilary Rodman and me, showed that lesions of superior temporal association cortex (Area TA) impaired short-term auditory memory thereby supporting the view that this area plays a role in audition that is homologous to that of IT cortex for vision (e.g., Colombo et al., 1996).

Mapping Retinotopic Organization

PULVINAR NUCLEUS

Although Dave Bender, who holds the record as my longest continuous collaborator, continued to work with me on IT at Princeton, he also began his own studies of the pulvinar, a large subcortical structure of obscure function but major connections to the superior colliculus, striate cortex, and extra-striate visual cortex, including IT cortex. He carried out the first electrophysiological

mapping of the pulvinar and found a complete representation of the contralateral hemifield in the rostral inferior pulvinar and evidence for two adjacent retinotopically organized areas (e.g., Bender, 1981).

EXTRASTRIATE VISUAL CORTEX

When we came to Princeton very little was known about the organization of the visual cortex beyond striate cortex. This changed in part as the result of two visitors from the Federal University of Rio de Janeiro sent by my old friend Carlos Eduardo Rocha-Miranda. Ricardo Gattass and Aglai de Sousa stayed initially for 3 years, and then Ricardo returned several times for shorter visits. Under Ricardo's leadership we mapped with multiunit electrodes the topographic organization of a number of extra-striate visual areas, usually for the first time, including Areas MT, V2, V3, V4, TEO, and PO (e.g., Gattass, and Gross, 1981a, 1981b; Gattass and Sousa, 1985; Gattass et al., 1988). These mapping studies became the basis for study of these areas in many other laboratories. This project and some associated anatomical connective studies involved, in addition to Ricardo and Aglai, Carol Colby, Ellen Covey, postdoc (1980–1981), Carl Olson, Princeton Assistant Professor, Sue Fenstermaker, graduate student (1981–1986), Julia Fleming (1978–1979) a physician from Australia, and Julie Sandell (1975–1979) an undergraduate (who wins the prize for the undergraduate in the lab with the most papers in prestigious journals).

BRAZIL

My relationship with Carlos Rocha-Miranda, Ricardo Gattass, their families, and their Brazil was long, warm, and involved many visits to Brazil. On my first 6-week visit, the young Rio scientists found me so different from the stiff English visitors that they had known that they took me out dancing virtually every night. Other visits involved a lecture tour of about five Universities scattered around northern Brazil requiring armed guards when going out, getting marooned on an island in the mouth of the Amazon, eating large rats, and, unknowingly until too late river dolphins, touring the Amazon out of Manaus with Charlie Bruce and Harriet Freeman, living in a zoo in Belem, and visiting the Iguazu falls on the border of Argentina with Tom Albright.

Ricardo had a connection with Pope John Paul II through his mentor Carlos Chagas, the President of the Pontifical Academy of Sciences, so we organized a small symposium in 1984 in the Vatican Gardens (Chagas et al., 1985). There was so much wine and food at lunch that most of us fell asleep in the afternoons with the striking exceptions of the aged John Eccles and Albert Szent-Gyorgyi, who were amazingly indefatigable.

Blindsight and Attention

Monkeys and humans have the ability to detect and localize visual stimuli in the absence of striate cortex. This phenomenon in humans is called

“blindsight” because it occurs in the absence of any conscious awareness of the stimulus. Tirin Moore when he was a graduate student (1990–1995) and Hilary Rodman, then a postdoc, showed that the vision that survives striate lesions in monkeys has the same characteristics as human blindsight, that is, monkeys show “blindsight” (e.g., Gross et al., 2003; Moore et al. 1995, 1998). Cowey and Stoerig (1995) came to the same conclusion in a different experiment at about the same time. Maz Fallah (graduate student, 1996–2001), Alan Repp undergraduate class of 1994 and Paul Azzopardi (a visitor from Cowey’s lab in Oxford) also worked with us on blindsight (e.g., Azzopardi et al., 2003).

The visually guided behavior of monkeys who received their striate lesions in infancy was much better than that of the animals that received their lesions as adults (Moore et al., 1996). Unlike the adult monkeys they probably had normal sensation of the visual stimuli and not merely blindsight. This seems to parallel the case of humans who sustained their striate lesions early in life.

When he finished his dissertation on blindsight, Tirin went off to Peter Schiller’s lab at MIT as a postdoc and then came back to my lab for another 3 years (1999–2003). In that period Tirin showed the close relationship between the circuits that process shape and those that control eye movements (e.g., Moore, 1999). With graduate student Maz Fallah he found that microstimulation of eye movement areas alters circuits that modulate visual attention (Moore and Fallah, 2001). Then with Katy Armstrong, another Princeton graduate student, he demonstrated that such microstimulation actually modulated the activity of neurons in Area V4 (Moore and Armstrong, 2003). These studies were the first demonstrations of specific relationship between mechanisms of eye movements and mechanisms of shape recognition.

Body-Part-Centered Receptive Fields in Premotor Cortex

Michael Graziano started in the lab as an undergraduate helping Hilary Rodman. Then in his senior year (1989) he set out to record from the claustrum, a mysterious structure but supposedly a visual one. He continued in the summer and came down on weekends from MIT where he had just gone as a graduate student. Because we thought we were recording from a visual structure, we used visual stimuli and sometimes got visual responses. We accidentally discovered that touching the animal often also gave responses, and the somatosensory receptive fields formed a map of the body. In the face and arm portions of this map, neurons were bimodal, responding to visual and tactile stimuli. The visual receptive fields of these bimodal cells were attached to the body and extended out into space, usually about 10 cm. Most extraordinary, if the arm or head was moved, the visual receptive fields stayed attached to the somatosensory receptive fields and moved with the arm or head. Thus, these visual fields were in a body-part-centered coordinate system, the first that had ever been reported. Upon sectioning the brain later

we found that we had been recording not from the claustrum but from the adjacent ventrolateral putamen, which had been known to have a somatotopic organization, but its visual properties had never been noticed (Graziano and Gross, 1993).

Graziano left MIT graduate school after 2 years and returned to Princeton as a graduate student (1991–1996) and postdoc (1996–2001) where we continued these studies. Rizzolatti and his colleagues had earlier found visuo-tactile receptive fields in the ventral premotor cortex or PMv (Area F4 in their terminology) as we had in the putamen (e.g., Fogassi et al., 1992). We repeated their results and found that the bimodal receptive fields in PMv would also move with the hand or arm, like those in the putamen (e.g., Graziano and Gross, 1996; Graziano et al., 1997). At that time we interpreted the body-part-centered bimodal RFs in the putamen and PMv cortex as playing a role in sensory-motor integration. Some of these experiments were carried out with undergraduate Greg Yap, class of 1995, and new graduate student Xin-Tien Hu (1994–2000).

With Xin-Tien we found that a subset of bimodal visual-tactile PMv neurons would keep track of stimuli near the head or arms even in the dark: They had mnemonic properties (Graziano et al., 1997a). That study put us into *Glamour* magazine with the head “Kissing in the Dark.” Xin-Tien’s thesis was on spatial properties of parietal neurons. Later he returned to China, and I took several fantastic trips with him there (e.g., across a landslide to Leaping Tiger Gorge, around the Buddhist holy mountains at Yadin, to a three day Tibetan horse race festival in Litang, to Shitoucheng, an ancient village carved into cliffs over the Yangzi and reachable only by many hours in a four wheel drive and then a footpath and to Lugu Lake with its “walking marriages”). Graziano, undergraduate Lina Reiss class of 1997, and I found a representation of auditory space in PMv: neurons with trimodal visual, somesthetic, and auditory receptive fields that would only respond to auditory stimuli and visual stimuli near the head (Graziano et al., 1999).

When Graziano, Moore, and Charlotte Taylor, a graduate student (1999–2004), stimulated PMv, they produced integrated complex movements (Graziano et al., 2002). About this time, Graziano became an assistant professor at Princeton and continued working on this phenomenon in his own laboratory.

History of Neuroscience

My then wife, Greta Berman, gave me a copy of a new biography of Charles Darwin, because it had a deservedly enthusiastic jacket blurb by our friend Stephen J. Gould. The book talked about a controversy and a “lobe” of the brain I had never heard of, the “hippocampus minor” controversy. The story turned out to be a splendid “case history in the social construction of neuro-anatomy” (Gross, 1993a, 1993b). The leading Victorian anti-Darwin scientist

was Sir Richard Owen, and his main argument against evolution was that the human brain was unique, particularly in having a hippocampus minor. Thomas Huxley, Darwin's ferocious defender, set out to discredit Owen, not merely by demonstrating the hippocampus minor in a variety of primates but by painting Owen as a fraud and charlatan. The tale illustrates the political and social matrix of brain study and the extraordinary persistence of ideas in biology. The hippocampus minor is now known as the "calcar avis" and is actually only a slight indentation into the lateral ventricle caused by the calcarine fissure. This was a return to my long-standing interest, from high school on, in the social context of science.

The success of my papers on the hippocampus minor spurred me to write more than a dozen additional history of neuroscience papers, several of them deriving from the unfinished introduction to my thesis. Some of them were collected in *Brain, Memory, Vision: Tales in the History of Neuroscience* (Gross, 1998) and another volume is almost finished, to be entitled *From the Paleolithic to the Internet: More Tales in the History of Neuroscience*. Aristotle, Galen, trephining, the evil eye, Leonardo, Swedenborg, psychosurgery in Renaissance painting, Rembrandt, Alhazen, Claude Bernard, phrenology, the discovery of motor cortex, and adult neurogenesis are some of the historical topics I have written on.

Collaborating with Other Princeton Faculty

Bornstein, Babies, and Color

Marc Bornstein was a faculty colleague who did important work on color and babies. We wrote a really neat paper "On Left and Right in Science and Art" for the journal *Leonardo*. It ranged over physics, chemistry, brain laterality, anthropology, art criticism, and stage craft and had pictures by Pouissant, D'Arcy Thompson, Cajal, native Americans, ancient Greeks, our colleague Julian Jaynes, and, of course, Leonardo. We made predictions about dyslexia and explained Leonardo's mirror writing (Gross and Bornstein, 1978). The essay inspired several collaborative findings on human infants: that infants confuse lateral mirror images as do many other animals and that vertical symmetry is very special for infants (Bornstein et al., 1978, 1981). With undergraduate Julie Sandell, Marc and I demonstrated that monkeys divide the spectrum into the same four-color categories that human infants and adults do (Sandell et al., 1979).

fMRI Imaging of Monkey Cortex

In monkeys, face selective cells are found throughout inferior temporal cortex but are concentrated in the vicinity of the superior temporal sulcus including in STP. In humans, faces especially activate one limited region on

the ventral surface of the temporal lobe and another in the dorsal and anterior superior temporal sulcus, perhaps corresponding to Area STP. To relate face mechanisms in humans and monkeys, my faculty colleague Sabine Kastner, Marc Pinsk (graduate student, 2001–2005 and currently postdoc), and I, studied fMRI in awake monkeys as they looked at face, body parts, and other objects. We found two areas of activation by faces and one by body parts. These seemed to be similar to the areas activated in humans by the same stimuli (e.g., Gross, 2008; Pinsk et al., 2005).

Adult Neurogenesis and Elizabeth Gould

From the time of Ramon y Cajal a central dogma of neuroscience has been that no new neurons are added to the central nervous system of adult mammals. This dogma was challenged in the 1960s by Joe Altman (who was in Teuber's department at MIT when I was). Altman reported new neurons in the hippocampus, olfactory bulb, and cortex of adult rats, guinea pigs, and cats. Few believed him; he failed to get tenure at MIT and eventually turned to more conventional subjects. (This history is reviewed in Gross, 2000.) Then in the early 1990s Elizabeth Gould and her colleagues at Rockefeller University confirmed Altman's results on neurogenesis in the hippocampus of adult rats (e.g., Cameron et al., 1993; Gould et al., 1992). Many others then began to report similar results in rats. Gould went on to show adult neurogenesis in the hippocampus of tree shrews and marmosets, a New World monkey, and how experiential factors such as stress, hormones, and learning could modulate adult neurogenesis in the hippocampus (e.g., Gould et al., 1997, 1998a, 1999a, 1999c). Gould then moved to the Princeton psychology department and we began to collaborate. We demonstrated hippocampal neurogenesis in the adult macaque (Gould et al., 1999c). We also reported neurogenesis in the frontal, temporal, and parietal cortex of adult macaques and noted that the new cortical neurons, like new neurons, in the adult rat and adult macaque hippocampus tended to have a transitory existence (Gould et al., 1999c, 2001), perhaps related to a role in learning (e.g., Gould et al., 1999c; Gross, 2000, Leuner et al., 2006). Since our first report of adult neurogenesis in the cortex there have been a number of confirmations as well as some failures to do so, and thus the issue is still unsettled, and the controversy unpleasantly fierce (see review by Gould, 2007).

More recently, in collaboration with Gould's postdoc Ben Leuner and her graduate student Genia Kozorovitskiy, Gould and I showed a decline in hippocampal neurogenesis with aging in the marmoset, the first such report for a primate (Leuner et al., 2007). In another marmoset study with Kozorovitskiy, Leuner, and Gould, experience in an enriched environment produced structural and biochemical changes in the brain, the first such demonstration in primates (Kozorovitskiy et al., 2005).

Marriage and Family

I decided not to scatter bits of my family life in among my experiments and graduate students. They were much too important for that and deserve much more space than is possible here. So until I publish my real (i.e., personal) memoir a few sentences will have to suffice. In 1961 I married Gaby Peierls, an Oxford graduate whose father was Professor Sir Rudolph Peierls, Professor of Theoretical Physics at Oxford. He was born in Germany and was one of the leading theoretical physicists of his day. Her mother was born in the Soviet Union and was an exaggerated fusion of a Jewish and a Russian mother and features in many of the biographies of physicists of her generation. Both came from totally assimilated Jewish backgrounds but that made no difference to Hitler or Stalin. Gaby was successively a stock analyst, economics graduate student at MIT, ran the local New Jersey American Civil Liberties Union (ACLU) office, went to University of Pennsylvania law school, and worked until very recently as an advocate for children, abused women, and recent immigrants. She was a wonderful wife and mother to our four children and crucial to every aspect of my academic life. She is retired now and lives near our eldest daughter, Melanie.

Melanie went to Barnard College and Robert Wood Johnson Medical School and is Assistant Professor of Internal Medicine at the University of Gainesville and has two boys, Sam and Noah, and a Physics Professor husband Steve Hagen. My youngest daughter Rowena lives in Princeton and managed until recently a "Ten Thousand Villages Store" that sells fair-trade products from peasant cooperatives worldwide and lectured on fair trade in local businesses and churches. I had another daughter Monica who died in an accident at the age of 2 on Mt. Washington and a son, Derek who died of cancer at age 27. He went to Simons Rock and Oberlin as an undergraduate and the University of Rochester as a linguistics graduate student and worked in the computer/linguistics/ publishing world.

I was with Iris Fodor for about 8 years. We knew each other since MIT days. Iris is a red diaper baby, Professor of Psychology at NYU, and a psychotherapist. She has a house in Woodstock, N.Y., where we spent a lot of time with our five kids. One summer we went around Italy with the four youngest. Iris and I spent 6 amazing weeks in India together. As a result we got seriously into photography at the same time, and she now teaches photography to Tibetan and Peruvian children when she is not teaching psychology or seeing patients.

I was married to Greta Berman for 14 years. Greta was another red diaper baby. Greta went to Antioch, got her doctorate at Columbia, and teaches art history at the Juilliard School. We had a good life together in Princeton, Manhattan, and Woodstock, N.Y., and traveled widely. Greta often told of following me to the end of the earth, and it was true (Tibet, Papua New Guinea.). She was a great comfort when my son Derek died.

Greta was also very supportive of my photography efforts. She dismissed my early efforts as “like post cards or National Geographic pictures” and acted as a muse and informal curator for my first one-person show.

The Twenty-First Century

Students and Teaching

In the new century I stopped taking new graduate students and postdocs but continued to collaborate with colleagues. Until recently, my undergraduate and graduate teaching was mostly physiological psychology, now called “cognitive neuroscience” with an occasional more specialized course such as the history of neuroscience. A few years ago I introduced a course for psychology graduate students in “Responsible Conduct of Research,” which deals with such matters as authorship, mentoring, peer review, conflict of interest, and the use of animals and humans in experiments. I have also started to teach courses in Neuroethics for undergraduate and graduate students. They deal with the ethical implication of developments in neuroscience such as brain imaging and drugs that change mood and performance as well as more traditional questions such as when does life begin and end and the involvement of scientists in the military. In the past I taught in Brazil, China, and Vietnam and, more recently, in Uganda and in Cuba. I hope to do more of this outreach teaching in the future.

Inferior Temporal Cortex and Processing the Facial Image

My contribution to understanding IT cortex was largely confined to the sensory properties of its neurons. Since then their cognitive properties, particularly in attention and memory, have been extensively explored by my former students such as Desimone, Miller, and Albright and by many others. Furthermore, the processing of objects and faces by human IT cortex is now being widely studied by imaging techniques. Models of IT cortex are coming close to the anatomical, physiological, and perceptual facts. Disturbances in temporal lobe face processing have been implicated in a variety of human disorders such as autism. I continue to be astonished by the growth and vitality of the inferior temporal-face industry (Gross, 2005, 2008).

Overview

This memoir comes at a major change in my life: I have closed my laboratory and have full time for teaching, writing, travel, and photography. Until now I have been very fortunate indeed. I have had wonderful companions in research, good students in the classroom, more-than-deserved recognition from the field, generous and continuous financial support, and institutional affiliations that were never onerous. May the future be as rewarding.

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