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Joseph E. Bogen

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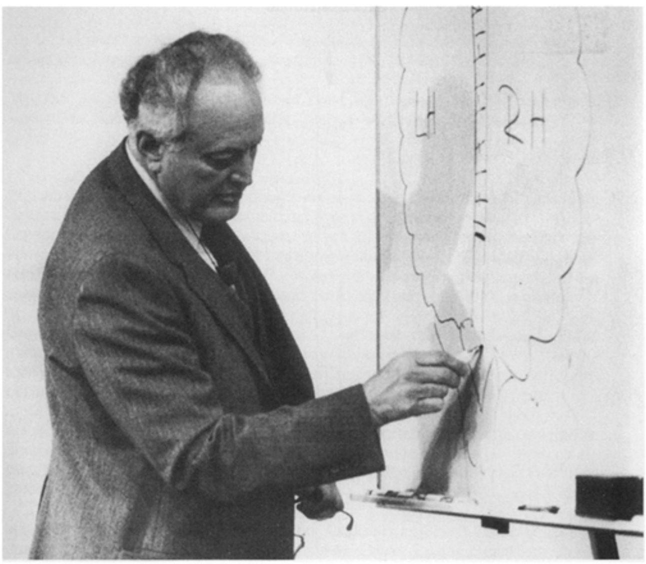
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Joseph E. Bogen

Born:

Cincinnati, Ohio
July 13, 1926

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Whittier College, A.B. (1949)
University of Southern California, M.D. (1956)

Appointments:

Assistant in Surgery, Cornell Medical School (1957)
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California Institute of Technology (1958)
Research Associate in Neurophysiology,
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California College of Medicine (1964)
Consultant in Neurosurgery,
California Institute of Technology (1968–1990)
Clinical Professor,
University of Southern California (1973–2005)
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Fellow, American College of Surgeons (1968)
President, Southern California
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President, Los Angeles Society of
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Award of Merit, California Association of
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Joseph Bogen was a neurosurgeon who pioneered "split-brain" surgery for epilepsy and studied (with Roger Sperry and Michael Gazzaniga) the first "split-brain" patients of the modern era. He was also an effective popularizer of concepts of hemisphere specialization and wrote widely about consciousness as a neurobiological phenomenon.

Editor's Note: Joseph Bogen was working on this chapter up to the time of his death on April 22, 2005. It was completed by the editor to achieve an appropriate length and consistent style; and by his daughter, Meriel Bogen Stern, who has also added a few paragraphs (printed in italics) to provide factual background at various points in the narrative.

Joseph E. Bogen

How I Got This Way

It is a special honor to be asked to contribute to this history of neuroscience in autobiography, particularly because it is the only honor I have received for scientific work. This is because unlike almost all of the other contributors, I am not a professional scientist: Save for three summers as a graduate student and 2 years as a post doc, I have never been paid to do science. Being in this illustrious company is largely attributable to my long association with Nobel laureate Roger Sperry. So these selected stories from my life hope to explain how I came to that association. The stories are true, so far as memory serves, although there are bits of digression where some moral might be served.

Earliest Years

According to my mother (*Esther Bogen Tietz*), she and I graduated from medical school together; she was then 8 months pregnant. Four weeks later on July 13, 1926 I was born on the steps of Christ Hospital in Cincinnati, Ohio. She was apparently working too hard as a medical intern to give my birth much attention. Thirty years later when I was an intern we worked well over 100 hours every week; in her time it was probably even more demanding. As far as I know, when she went to medical school my mother was the only woman. She said later that she had been tricked into going to medical school by my Uncle Emil who graduated from the same medical school 3 years earlier. She had been studying to be an artist, but he told her she would be a much better sculptor if she took the medical school courses in anatomy and physiology. Then, when she had excelled, he said she might as well finish up and get the degree.

My father, J.B. Tietz, also graduated in 1926, from law school. I remember nothing of him until I was five. A few days after I was born, my mother returned to work and I was put in the care of my grandmother. My only recollection of those days was a colorful birthday party for me, possibly when I was 4 years old. I recall, from visiting a few years later, that there was a rather long stairway down from the second floor. Some 60 years later, I spent some time with my Uncle Dave who was quite active in his early 90s, playing golf three times per week, attending chess club each Friday night, and singing in a barbershop quartet. When talking a bit about those days he confessed that one day when he was left in charge of me when

I was 3 years old, his attention was diverted and I rode my little tricycle off into space. He saw me tumbling head over heels all the way down the stairway and was certain I had sustained great injury until I quickly stood up. He told me this story on three different occasions in 4 years. There was no question of senility by any other criterion; it seems he had never totally absolved himself of this rare dereliction of duty. For the most part I was the object of unfailing affection for my four first years.

When I was 5 years old, my family, including my 1-year-old brother, Bob, moved into a newly constructed house. There was a deep wood on the other side of the country road. In the woods was a sparkling brook with clay banks, tadpoles, and crayfish. My father took us to neighboring farms where there were many animals. He once took me to Lunken airport for a flight in a "pusher," a plane with the motor mounted backward. On that trip we stopped for lunch. I do not recall what I ate but he ordered "ham and Swiss on rye," which has seemed to me ever since to have a certain manly air about it. One day my father arrived home in a panel truck full of Oh Henry candy bars; it was tangible evidence that he could win a case and take somebody's assets.

I helped my father plant trees, which probably contributed to my becoming what is nowadays called a "tree hugger." And I helped him with the dozens of rabbit hutches. The rabbits were subjects in my mother's Ph.D. research on the newly discovered Friedman test for pregnancy. I was fascinated to see some woman's urine injected into the ear vein of a rabbit, after a blood sample had been removed. And a few days later when the rabbit was anesthetized, I was just tall enough to see it supine on the kitchen table, its belly shaved and painted purple (with potassium permanganate); then it was opened up long enough to draw another blood sample and to observe the ovaries before the wound was closed and the rabbit awakened. (There would be ovulation if the urine had contained the gonadotropins of pregnancy.)

Once, when I was 6½, my father killed a chicken by holding it on a stump with one hand while lopping off its head with a hatchet. He was fairly fearsome looking with spattered blood and that hatchet in his right hand. The most memorable aspect, however, was seeing the headless chicken, running around the yard and occasionally flapping its wings while my 2-year-old brother was saying, "chicken dead, chicken dead." My mother explained how the chicken's spinal cord controlled its movements independently of its brain. I doubt that my little brother got much out of this lecture; but it stayed with me permanently.

The Separation

The influence of a physician/scientist family on my later career choice did not become very evident until I was almost 24 years old. This seems, in retrospect, the result of being angry with my family for most of the years subsequent to separation at age 7. For most of 17 years afterwards I was aimless, lazy, and difficult. For reasons I only understood later, I was put in a boarding school at age 7. It was run by Edna Mae Waterman Castle of whom I will only say that she has ever since been in my memory the living embodiment of The Wicked Witch of the East. At boarding school for grades two and four (ages 7 and 9) I was constantly in trouble, especially so when my parents forgot to visit on promised weekends. I remember waiting in vain one Saturday, staring for hours out the window at a drenching, endless rain; nobody came. Other years I boarded with various people (paid by my parents) and attended public schools where I was persistently a problem.

My mother did come on some weekends to take me places, like the zoo. On one occasion when I was in the fourth grade she sent a friend who picked me up and took me to an auditorium. I recall being high up in the back and seeing a parade of people wearing gowns. I eventually learned that it was the ceremony in which my mother received her Ph.D. in biochemistry. Sometimes she took me to the hospital where she worked. On a couple of occasions, when I was about 9 years old, she allowed me to play with a brain model, about 3 feet high, which she had constructed from glass tubing filled with various gases. (Getting a chemistry degree in those days included expertise in glass blowing.) At the base of this big glass brain there was a panel of toggle switches. Tripping one caused a red tubing to turn on, a streak of red all the way from the outer wiggly surface down through the brainstem and crossing over to descend in the short piece of spinal cord. Other toggle switches lit up other pathways: green, white, and a bluish color. Looking back now almost 70 years, the details are doubtful; mainly I was left with the idea that brains have lots of pathways, and brains are very colorful! Sometimes my mother was accompanied by my father who was almost always difficult. He was spending each summer in the BOMC, a U.S. Army program, living in tents, marching around, and firing various guns. So far as I recall, his favored expression acquired there was what he called "The Voice of Command," which he enjoyed using frequently. As the years went by these characteristics worsened. In retrospect this was related to his inability to have much of an income as a lawyer.

One Saturday that my mother came for me was sufficiently influential that it deserves a story and title of its own.

Sam's Streamlined Lime Green LaSalle

When I was promoted, in 1977, to Clinical Professor of Neurological Surgery, one of my colleagues asked,

> "About time you got a new car, isn't it?"
> "Next year, maybe, when this one is 10 years old."

"What kind?"
"Another Oldsmobile, probably."
"Why not a really good car? Surely you can afford it."

"I guess I'm just an old fogey at heart," I replied, knowing he would never understand the real reason. The real reason had to do with what happened when I was 10 years old, in 1936.

My mother came to pick me up from the place where I boarded. She said, "Don't dawdle—we're going to see Sam G's fancy new car. I hear it was very expensive."

> "Sam, the dermatologist? Skin doctors don't do surgery—they only charge for office calls. So how can Sam afford a big, expensive car?"
>
> "The old saying about dermatologists is that their patients never die and they never get well. If the doctor is affable as well as able and available, they keep on coming back. So Sam's practice is enormous; and he can see many dozens of patients a day because the essential exam he has to do takes just a few minutes."

Even then I realized that last point was the clue. My mother was a psychiatrist, which meant, in those days, that her patients rarely died and hardly ever got well; but she was certainly not rich. The difference was that in her specialty, a conscientious exam could not be done in just a few minutes. My pathologist uncle was not rich, either.

> "I thought we were going to the museum with Uncle Emil."
>
> "Emil's going to meet us at Sam's house because he can walk there from the hospital where he has been visiting some old friends."

My Uncle Emil had become a national authority on tuberculosis. Before that he had invented the first machine that would smoke a cigarette to determine separately the chemicals in the mainstream and the side stream, instead of just burning a coffin-nail in a dish. Even earlier, in 1926, he had invented the first quantitative test for urine and breath alcohol levels. His claim that one could measure drunkenness this way was laughed at in the beginning. In 1924 everybody thought it was ridiculous except for his professor Shiro Tashiro. Now of course, alcohol level is usually tested in exhaled air instead of by a blood test. Later on, he became a Clinical Professor of Infectious Diseases at UCLA. For about 20 years he ran the lab at Olive View Tuberculosis Sanitarium. His wife, Jane Skillen, became a thoracic surgeon after graduating from the University of Michigan Medical School. He married this Irish woman (in 1933) to get, he said, "hybrid vigor, that's what we need."

Anyway, so Emil, although at this time only 40 years old, was an Eminence in the circle of my mother's physician colleagues. The mantle rested easily on Emil's shoulders. He was habitually solemn and matter of fact with his fellows. This derived in part from his having been the ruler at home among 12 siblings (including 6 adoptees, orphans of a Russian pogrom). His mother was a mellow, tolerant type who spent most of her time in her garden. Her husband was one of the first professional social workers, wrangling money from the rich and doling it out to the poor. His activities kept him traveling most of the time. The resulting "Children's Republic," as it was called, was not very democratic, according to my mother's recollection.

Emil was already there at Sam's house when we arrived. And there it stood gloriously! The lime-green, streamlined beauty, a hood ornament embodying panache, chrome everywhere, pants over the rear chrome-spoked wheels, and broadly striped white wall tires.

"It's quite lovely, Sam," my mother kindly observed.

I was speechless. It was every 10-year-old boy's dream-mobile. Had the story ended there, the image of that car would have slipped into my psyche to remain there for decades as a long-term goal.

Sam turned to my uncle and asked, "Emil, you haven't said anything. What do you think?"

Uncle Emil paused in his deliberate way, then spoke: "It is impressive, Sam. But it's not for a doctor. A doctor should drive an Oldsmobile or maybe a Buick. But not a La Salle, Sam, not a La Salle." He turned away toward my mother's Ford and said, "We should go to the museum now, so we'll have time to see all the new exhibits."

On the Farm

The two times during childhood that I was an eager student were when I lived on my aunt's dairy farm in New York and attended a one-room school house with 12 to 15 students of different ages. My cousin Ruth and I were the only students in the fifth/sixth grade (we did 2 years in one) and then later in the eighth grade. I studied mostly what I wanted those years. And I spent a lot of time with a big globe of the world; bigger and better than any in the city schools I attended. Best of all was a large model of the solar system with all the planets on rods of appropriate lengths so that each could rotate around the sun. It was then that I decided I would be an astronomer. I spent many hours at night gazing at the sky, learning to locate many constellations and individual stars. Ever since those times in the one-room schoolhouse I have believed that having 30 students reading in the same chapter of the same book at the same time is stupid, if not institutionalized mental illness.

Memories of the farm are both mostly vague, and vaguely pleasant by now, until I make definite effort and then the rosy film fades and there returns the recollection that farming was hard work; this was most true of the fall: oat mowing, raking, bundling, wagon loading, and especially the threshing. Later in the year, for many weeks, I was up before dawn in the winter, stumbling around one or another dark pasture rounding up the cows for the morning milking. The rubber boots were easy to hose off, a complication that was repulsive at first but soon became routine, save for the rare occasion when the cow patty was wet and a mistaken step at an angle caused a slip and fall into the source of the slide. And there was slopping the hogs and feeding the chickens, all before breakfast. Nobody ever told me I was going to college; but along with having professional parents, being on the farm helped convince me.

The Looney Bin

I rejoined my parents, brother, and 5-year-old sister as I began high school in Cincinnati at the age of 13. This was because my mother's increased recompense included a large apartment on the middle of the second floor of the newest addition to the hospital. This location had an additional advantage as the women's receiving ward was on one side and the men's on the other, also on the second floor. Hence those wards were close by my mother's home and office, a help as she was still the receiving physician who screened the incoming patients for any complicating medical problems as well as providing a tentative diagnosis.

This was the Longview State Hospital for the Insane, a 5000-bed facility where, in the back wards, people were actually chained to their beds. In those days there were no psychotropic drugs. My mother, who, as I have said, had her Ph.D. in biochemistry by this time, tried whatever seemed to work, including Metrazol convulsions. A man named Meduna claimed that nobody who had epilepsy was ever schizophrenic, so the way to stop schizophrenia was to give convulsions. It was really a cockamamie idea. But he claimed success so other people were trying it because in those days there was nothing else, except hot towels and cold showers and chaining people to their beds. So she used Metrazol and then she used insulin and then when Cerletti and Bini came out with electroshock she was the first person to use it west of New York. (Kalinowsky was the first person to use it in this country.) Electroshock therapy was effective with depressed patients (and still is) but it did not do much for the schizophrenics. There was day and night a lot of screaming and not just from the back wards! There was a parking space in front of the building used mainly by visitors but also where my father's car was parked. Late one Sunday afternoon I was washing his car (I was then 14). Patients who showed improvement often had trial weekends at home before being discharged.

A car arrived, returning a patient, a commonplace event in those days, except when I looked up from washing the car I saw that the patient was undressing. She was soon totally naked at which point she threw her arms upward toward the men's receiving ward and screamed, "Come and get it boys, one at a time!" Some of the patients were sufficiently sound that they worked around the grounds as gardeners. One recollection I had was my mother saying, "These folks are less disturbed (one of her favorite words) but it would be better if you don't turn your back on any of them."

There was other bad news: First, the hospital was far from high school and the friends I made there. Second, my father's desire to keep in practice led him to buy a bullet trap, which he placed at one end of my room so that from the far end of the living room the target in the trap would be almost 50 feet away. His shooting while I was in bed did not bother me when I was reading, but I was afraid to go to sleep because of the possibility that I might get out of bed, forget for a moment, and walk into the line of fire. (This may have contributed to a lifelong problem with insomnia!) My father bought me a Mossberg .22 bolt-action rifle so I could join him. One evening while firing from a sitting position, I pulled the trigger. There was a click from the hammer falling, but the bullet did not ignite. Thinking that it had been a misfire, I ejected the cartridge. It was hang fire that exploded in midair, making a clanging noise as it dented the piano. My mother's patience was exhausted and we were forced to take the bullet trap down into the basement of the hospital and do our shooting there. Another problem with living with my family was that my father's quick anger was augmented by seeing me every day. Once when I reached for the bread instead of asking he stabbed the back of my hand with a fork and spoke some words I have forgotten. On another occasion he was furious, over what I never knew, and said, "You are going to get a good hit for that—there will be no warning and you won't see it coming."

In high school in Cincinnati, Ohio, I was an indifferent student, except for algebra, and I was a disciplinary problem. During my sophomore year, at age 14, I ran away from home on my bicycle with the ridiculous goal of pedaling to California to live with my grandmother. By nightfall I had reached a fire station some 40 miles into the Kentucky hills where the fireman kindly called my worried parents. They came at night to retrieve me; for a few days thereafter my behavior and my father's were improved.

In 1940 Joe, a loner for most of his childhood, caught the attention of schoolmates and family for winning the Cincinnati Quiz Kid contest, which qualified him and his family for a trip to Chicago where he participated in the Quiz Kid Radio Show. He remembered this episode chiefly for the disillusionment he felt when it was revealed to him that the show was "fixed." (Questions were chosen to fit the special interests of each child, which he considered dishonest.) He appeared on the show only a few times *but is included, (under the name Joey Tietz) in the book, "What Ever Happened to the Quiz Kids?" by Ruth Duskin Feldman.*

In the middle of my junior year (it was 1941) my parents decided to move to Arcadia, California. My grandmother had died and my Uncle Emil, inheritor of her property, offered it to my mother. The appeal of this offer to my parents was threefold: First my father was still a poorly paid clerk in a large law firm after 15 years with no apparent prospect of advancement. Second, my mother was, I believe, burned out as the receiving physician in the Looney Bin. A third reason for our family to move was that we had never had a proper home since 1933 when, in the midst of the Great Depression, we were dispossessed. It was only years later that I understood how the rural paradise I loved at age 6 disappeared. My parents could not make the payments. My mother abandoned her struggling practice and found a job as receiving physician in a state hospital that provided a few dollars and an apartment too cramped for us all, so my brother and I were sent into what we later called "The Exile."

I refused to move to California because I was then about to obtain a letter as a member of the swimming team and this was the most important thing in my life. I remember my father raising his fist and saying, when I had been particularly obstreperous, "You will be punished for this—you will not see it coming. You have been warned." The Japanese bombed Pearl Harbor on December 7, 1941 and it at once became impossible to buy tires, clearly needed for the long trip to California. I remember exactly where I was on that Sunday morning—at Ronald T. MacDonald's bar mitzvah. Ronald was the younger brother of my best friend, Kenneth MacDonald. Their father had married a Jewish girl agreeing that the children would have a Jewish education. The adults deserted the party to gather around a small radio to listen to President Roosevelt's Day of Infamy address. It seemed threatening to them but my own thought at the time was that maybe it would stop the move to California so I could get my letter in swimming.

My father scoured the country for old tires for the Ford and for the small house trailer that we had used for several shorter family trips. We left Cincinnati in February 1942 with 22 tires strapped to the roofs of the trailer and the car and before we got to California we used them all.

It was on this trip that I first thought (or from the first time since age six) that my father might be both heroic and quite human. The car slid into a soft shoulder some place in Arizona. There was no help to be had. My principle memory from that episode was my father, stripped to the waist with sweat pouring off of his chest as he put a board under a jack, raised the car and then with my help, pushed the car off the jack toward the pavement. This maneuver was repeated a total of six times before the car was back onto the pavement.

We eventually arrived in California and my main recollection was of endless orange groves with a powerful scent that followed for nearly a hundred miles. I had been out once before through the orange groves. We were headed East after visiting my grandmother. I know what year it was (1938) because we were parked by the side of the road in Arizona, listening to boxing on the car radio. In a previous fight Schmeling had won a decision and Hitler had proclaimed this an example not only of the inferiority of the Negro but of the superiority of the Teutonic-Aryan master race. Just before the fight started, I went back to the trailer to get a drink. By the time I got back to the car the fight was over. Joe Louis had knocked out Max Schmeling in 2 minutes of the first round—so much for the master race. I regretted for years missing this glorious moment just because I was thirsty.

So we got to California and drove for miles past the orange groves. Somewhat alarming were posters nailed to telephone poles at frequent intervals saying that anyone of Japanese ancestry must be absent from the state by the end of the month. I was soon entered into a new high school where I quickly made a reputation for myself as the best student in algebra and the worst student in almost everything else. My misbehavior was partly related to the fact that this high school did not have a swim team. During my final semester, my trigonometry teacher took me aside and explained that he admired my comprehension of the algebraic aspects but that was not enough. He said he would get me into Cal Tech if I did all of the trig assignments; otherwise, he would flunk me and I would not graduate from high school. I slaved away, day and night, often having to redo many problems because there was so much opportunity for arithmetic errors; almost all the problems required using the thick tables of logarithms.

All Those Little Colored Balls

I first saw Linus Pauling when I was 16 years old. I was a freshman at Cal Tech, I guess I was 17 by that time. We had lectures a couple times a week and he gave one of the earlier lectures. I only remember two things of that lecture, one was that he introduced us to the whole question of powers of 10. It was a new idea for me and I think almost all of those kids. That everything could be expressed in powers of 10—especially if you wanted to talk about the size of the universe or the number of atoms in a bucket of gas. And negative powers helped with molecular distances. He had numerous models of molecules on a long lecture table: He used them all to illustrate the point that if you know the angles between the chemical bonds, then you knew the structure of the molecules, and that should tell you why when you pour something yellow into something blue it turned some unexpected color instead of green, and so on. This ultimately became a religious belief for me. The function depends on its structure, and that is what you are supposed to look for. Pauling just drove that lesson over and over his whole life, so it really was not his first lecture. As Pauling gave the lecture, a flask bubbled away over a Bunsen burner. Sometime during the lecture, he dumped a bunch of stuff into the flask and the liquid turned brown. There were other things going on, but he never said anything about them or did anything with them. So I went up after the class and asked, "What is this stuff over here in the flask?" He said, "That's my tea for lunch." So he was making his tea while he was doing his lecture. I expect that he looked forward to giving this answer every year he lectured the freshman class.

I had the privilege of some social times with Dr. Pauling and further academic exposure before he went off to Stanford and became known for advocating vitamin C as a preventative for the common cold. However, one of my favorite stories about Pauling was when Glenda and I were invited to dinner by Norm Horowitz and his wife Pearl. This was before Norm became Chair of the Division of Biology at Cal Tech. At that time Norm's big job was developing methods for deciding if there was life on Mars. The Viking Lander was going to Mars where it was going to scoop up some of the surface and dump it into some chemical testing mix. The question was, what chemical reactions would be evidence of life on Mars? (Or at least evidence of water, a basic necessity of life as we understand it.) Also the dinner party was interesting because Carl Sagan was there. Anyway, Horowitz told a story about how he had an argument with a post doc about whether a particular chemical reaction could just take place in fine sand under the influence of strong sunlight.

He told the post doc, "You're going to Stanford this weekend. Why don't you look up Pauling and ask him his opinion? He may have some odd ideas but he is still the world's greatest chemist; see what he says." According to Norm, when the post doc came back on Monday, Norm asked, "What did Pauling say?"

And the post doc answered, "Well, I never got to see him. He had such a bad cold all weekend he wasn't seeing any visitors."

Joe did not maintain a C average and then attended radar school as a recruit in the U.S. Navy. He was discharged in 1946. Between 1946 and 1951, when he was accepted to medical school, he had a series of jobs ranging from camp counselor to meat cutter. At a camp for epileptic children, Joe first thought about becoming a medical doctor. Still unsure, he went to his mother and Uncle Emil's Alma Mater, University of Cincinnati and took several courses in the sciences. During his time in Ohio, Joe became more of a devotee of traditional New Orleans style jazz and a member of the NAACP. He and several friends participated in lunch-counter-style protests. Joe would go into a club and sit down to be joined by a black friend who would be refused service. Joe would make a big scene before having eventually to leave the establishment. This activity and his other unorthodox *behaviors (during the winter instead of the usual scarf, he wore an orange bath towel around his neck) labeled him as "trouble." He applied to a few medical schools but was rejected. Back in California he took more pre med school courses at UCLA and considered a career as a jazz critic. It was also during this time that he changed his surname from Tietz to Bogen, his mother's family name.*

Medical School

None of the medical schools wanted me. It was about that time I met Ralph Gerard, a long time friend of my mother (I think because of the early ECT connection), at which time I asked him, "How much math should one have for a career in neuroscience?" He replied, "You can't have too much." So I stayed in Cincinnati and repeated the calculus, understanding it philosophically as well as mechanically for the first time. I returned to UCLA for a year, while reapplying for med school. There I took mostly math courses, all from Ray Redheffer who thought I should continue on in math. The first time I visited Ray in his office, I saw that he had a chinning bar in his doorway so I chinned myself while waiting for a prior student to leave.

> "One hand, one hand," he said, looking up.
> "Aw, you're kidding."

"No," he replied, and got up from his desk and chinned himself with each hand several times. It was the first time since being in Cal Tech that I was reminded that athletic prowess and superior mentation could impressively coexist. In addition to the math courses, I took physical chemistry. It was difficult because I was only just learning about partial differential equations. But I struggled and understood at least half of what was going on. An incidental point made by the professor was "I know I think with my big toe, because when my big toe hurts, I don't think as well." He left me with the crucial insight that everything is connected to everything else, but some connections are more meaningful than others.

After applying to more than a dozen medical schools, Joe was accepted at USC and Tulane in New Orleans. He had been to New Orleans to look at the school and sought out some of the old-timers of jazz. On the advice of Martin Grotjahn and his mother, he chose USC despite its poor standing at the time, because, as they said to him, "With your attitude, you won't last a year in the South before being jailed, or worse!"

Going to Magoun

In the winter of my first year in med school I was talking with my mother about being a psychiatrist. She said, "Well, if you're going to be a psychiatrist you need to know some neurophysiology."

"We get that in the second year in medical school."
"I don't mean textbook stuff. I mean the real stuff, on the leading edge."
"Well, where would I find that?"
"There's a man named Magoun in Long Beach. I met him at a meeting. That's where you should go."
I asked, "How do you suppose I can do that?"
"You write him a letter saying, 'I am a freshman medical student at USC. I would like to work in your laboratory this coming summer and I don't care what I get paid. I am willing to do anything you ask including sweep the floor. Sincerely yours.' That should work."

I wrote a letter just like that, word for word. Two weeks later I got a letter back and it said, "You are now a graduate research anatomist at UCLA." The letter said I could start when the school year ended and that I would be paid $300 per month. Wow!

They had a lot of money in those days. The government was just pouring money into neuroscience generally. So I found a room in Long Beach and went straight to the labs; they were mostly Quonset huts left over from WWII. There were people from all over the world. They were mostly mature scientists from Italy, France, and England. The first summer I did a variety of jobs including making microelectrodes for Italo Calma. This involved buying steel sewing needles and observing the tips under a microscope as the tips were dipped in acid. When the correct shape appeared, a nice cone with a 1 or 2 mu diameter, the needle was dipped in varnish that would nicely coat the needle with an insulating layer but was too viscous to cover the tip. Just a little would be exposed, if everything went OK. The main thing I did in the evenings was try to learn in detail the anatomy of the thalamus. I had more than one atlas and had inordinate amount of trouble figuring out that the centre mediane (unaccented in the texts) was the same as the centrum medianum, which was the same as nucleus centralis and same as the centromedian nucleus but definitely not the nucleus mediocentralis. The extent of my immersion in anatomy, to an extent I had never before experienced, was partially explained by the fact that in the same rooming house was Miss Finland, a natural platinum blond in town for the Miss World Contest. She kept smiling at me until I took her out for a hamburger and she taught me to count to five in Finnish but that was our only date.

That was the summer that Herb Jasper came out. As I understood it, his group had a different result from Magoun's; so they agreed to repeat the experiment together. This really impressed me as the right way to do science. Of course it requires two people who can afford the travel, who can get along, and who both are sufficiently established so that neither would suffer professionally whatever the result. A big feature for me was the appearance of the Penfield and Jasper book: *Epilepsy and the Functional Anatomy of the Human Brain.* Jasper generously inscribed the book and said if I were to drive from Long Beach to a place far out in the San Fernando Valley that I might be able to get Penfield to sign the book. He did. Apart from its value as a memento, the book so entranced me and has pulled me back for multiple readings so that it is fair to say that it influenced my professional thinking more than any other single book.

It was toward the end of the summer that the Man spoke the Word. A group of us went to Bob's BigBoy for a burger. The group included Magoun sitting directly across the table from me, and Dr. Jack French as well as about six others from the lab. Toward the end of lunch Magoun leaned forward and asked, "Joe, what are you going to do when you grow up?"

"I would like to do experiments like the recent one (in *J Neurophysiol*) by Schreiner and Kling with amygdalectomy making cats regressed in their behavior." I thought that they were both psychiatrists (Kling was). However, Magoun then said, "Yes, neurosurgeons do seem to understand the brain better—maybe it's because they look at it so much of the time." In retrospect, he may have been using the occasion to rib Dr. French, but it made a lasting impression on me. From that time on neurosurgery seemed the way to go.

The second summer I made an effort to understand the neurophysiology, as I was reasonably familiar with the anatomy. In fact this often required asking questions to be sure the terms in various languages were the same or not. I had some helpful discussions with Bobby Naquet and the Arduinis. I again made microelectrodes, mainly for Xenia Machne. By this time electrodes were made by heating the center of a length of capillary tubing to the right shade of red and pulling to get two tapered micropipettes, which were then filled, as I recall, with 3 molar KCl. At the end of the summer the Man spoke again, "You should spend your third summer in a different lab; how about going to Van Harreveld at Cal Tech?"

Going to Van H

Anthonie Van Harreveld was a peerless experimental tactician. Magoun had tackled a variety of problems using a specific technique, electrical stimulation and stereotaxy with refined versions of the Horsley-Clarke apparatus. By contrast, Van H pursued a specific problem, the chemistry of cerebral cortex, using a wide variety of methods, electrical as well as chemical. These various methods gave a figure for extracellular space in the neighborhood of 15–20%. However, the development of electron microscopy in the 1950s, when applied to brain tissue by the electron microscopists, had convinced them (and most brain physiologists) that the cells were packed with one another with little space left outside the cells. From his study of what happens to the brain in asphyxia, Van H came to believe that in the normal brain there is substantial space filled with water that runs from outside the cells to their insides, swelling them and making them butt against one another.

To show that his interpretation was correct, it was necessary to design experiments that would detect the extracellular water. This was far from easy, because the standard fixation techniques simply caused the water and electrolytes to disappear into the cells. With great ingenuity and persistence, Van H learned electron microscopy and after many trials arrived at the freeze substitution method that demonstrated the existence and stability of the extracellular water. It depended on freezing the tissue with liquid nitrogen so fast that the electrolyte shifts did not occur and then keeping the tissue frozen while silver ions migrated into the tissue over many days. He had restored the extracellular space to the brain after it had been missing for nearly 10 years.

Working with Van H was not easy for me; he regularly began an experiment every morning at 8 AM. More often than not, the experiment would be concluded in time for lunch, although some took all day, then lunch. Afternoon was devoted to writing up the work, and at 4 PM we stopped for tea, resuming work until going home promptly at 6 PM. As a result, his output was prodigious. With me, the main project was experiments with the spreading depression of Leão, one of which worked so beautifully that I did cartwheels and handsprings the full length of the hallway. Sadly, that experiment never worked again; it was my first exposure to the "first time effect" in scientific work, although not the last. This did not discourage me however; I found the work so fascinating that I spent much of the fall at Cal Tech missing med school classes and depending on classmates to keep me informed.

One project on the spreading depression worked quite well so I wrote it up. When Van H returned the paper, my entire opening paragraph had been deleted. I exclaimed, "If you leave that out only 5 or 6 people in the world will understand what the paper is about!" He replied, "There are only 5 or 6 people in the world who care." That was my first scientific paper (Van Harreveld and Bogen, 1956). Van H must have thought I did all right because he suggested that after my internship it might be profitable for me to return to him for a year as a postdoctoral fellow; it seemed my next 2 years after finishing medical school were set.

History of Medicine: The Wrong Thinker Part 1

Medical school was a woeful experience, an endless litany of fact whose origins were rarely explained and whose usefulness was infrequently justified. My distaste for rote learning and my questioning attitude were not shared by most of the class of 96 students. This was particularly evident on one occasion when a biochemistry lecturer claimed to be deriving the Nernst equation. The class was faithfully copying what he wrote on the board. Having only a year before taken the Pchem course for chemistry majors at UCLA, I though he was bluffing.

"Where did you get that value for k?" I asked.
The class shouted me down: "Let him finish! Just copy it."

The medical curriculum progressively became more bothersome for me, because so much was expounded without explaining its origins. Part way through the second year, four of us who lunched together agreed we needed more study of medical history, there being no course available. We met once a month to discuss readings—a sort of journal club. My Uncle Emil gave us some advice with respect to readings and arranged for some guest speakers. At his suggestion, and to emphasize the antiquity of our art, we called ourselves the Avicenna Society. Attendance grew to as many as a dozen students at some meetings. By the end of the third year, the senior faculty (such as Helen Martin, Pete Reynolds) had become aware of the Avicenna Society and decided it was something to be proud of. They invited the Society's president to give a 5-minute talk at the graduation ceremony for the class of 1955, the class a year ahead of ours. Because I was then President, I gave a talk. It emphasized that one thing that we had learned in our study of the history of medicine was that half of what was taught soon became obsolete. We were therefore particularly interested to know which half of what we were currently learning was a waste. I was not invited back.

By the third year most of the class was somewhat accustomed to me and were themselves asking an occasional question, although there was a small clique who wanted to get through each class with no interruptions. Some of us tried not to give their views much credence, partly because their leader was Pappy Spano, a recently discharged Army pilot. When we were supposed to bring a semen specimen to class for microscopic exam, Pappy set up a 30-cc graduate full of cloudy fluid and announced, "Here is enough for all, including you gals." (There were five women in the class.) During this year I was only rarely being shouted down by Pappy's clique, and I continued to ask questions. One occasion was the most memorable. Paul Starr, the chair of the department of medicine, was delivering one of his endocrinology lectures. That day his talk was about the adrenal gland. In the course of the talk he mentioned that ectopic adrenal cortical tissue does not respond to adrenocorticotropic hormone (ACTH). This struck me as odd so I raised my hand. He ignored me for a while because he knew me. But I kept waving my hand and he said, "What is it?" I said, "I don't understand why ectopic adrenocortical tissue doesn't respond to ACTH. What's the evidence for that? And after you give us the evidence it's true, what is the explanation?" And there was a kind of dead silence in the class as this man pulled himself to his full height and looked down and said, "Young man, there are so many things in medicine for which we do not yet have good explanations that you better begin right now to stop thinking and start memorizing."

The Original Betrayal

During my second year in medical school my mother suggested that I should "talk things over" once a week with a Dr. Balachandran. He was a small, dark man, a Freudian psychoanalyst recommended by Martin Grotjahn. He had the same penetrating gaze but with brown eyes rather than blue like Grotjahn's. I was expected to lie on a couch with him in a chair behind me. From time to time I would turn and look at him. He was almost always sitting quietly with his hands prayerfully in front of him, the five fingers of his left hand lightly touching the fingers of his right hand while a sixth finger of the left hand, apparently boneless, drooped sadly without a mate.

I was finally moved to ask, "Do your patients ever say that the sixth finger of your left hand looks like a penis?"

"Yes, quite often." And then, after a pause, "What else comes to mind?"

After approximately 6 weeks of me complaining to him, mostly about my father, he finally spoke up for the first time and said, "Interesting that in all this time you have never mentioned your mother. What can you recall about her in your early days?" So I talked about her coming to get me and going to the zoo, and to hear Gilbert and Sullivan's *Pirates of Penzance,* and to the museum, and so forth.

"Did she ever take you home?" I remembered with a shudder a visit to the small apartment she had when I was about seven, and having a tantrum, kicking the black sofa on which I was lying, when she insisted that it was time for me to go back to the boarding school.

"How did you feel when you first went to the school?"

When it was time to enter second grade, my mother took me to a school I had never seen before. The backyard was a wonderful playground with slides and swings and, most memorable, a jungle gym with kids swinging around. I thought it was going to be grand. I was too busy playing to notice when my mother left. Then came the end of the afternoon and almost all of the children left. One of the teachers showed me a room where I was supposed to sleep, and took me in to dinner. "Where is my Mom?"

"She will come to see you on Saturday." She came on Saturday, took me home and the next day took me back to the school, after I had finally exhausted my tantrum on the black sofa. There were other visits to that small apartment in the hospital. It was on a third floor hallway ending in a balcony that ran around a large atrium. I recall that I considered jumping over the railing and falling three stories to make a big splash.

"So not only did your mother not protect you against some of your father's aggression, she actually abandoned you, or so you thought."

These sessions with the analyst were most enlightening, especially as I realized they had been arranged by my mother. They ended rather abruptly when the analyst moved on to another town.

The Fascination of Surgery

It was during my third year in medical school that I saw an operation using an artificial aortic valve. At the time there were still in the L.A. County Hospital a couple of operating rooms with little balconies for observers. You would put on a mask and cap and sit in a little balcony overlooking the operation. It was literally an operating theater.

"Hey, Bogen, they're going to do a Hufnagel on your patient."

I was a medical student on the cardiology service and had worked up a man who had aortic insufficiency from syphilis; his aortic valve would open when the heart contracted but it would not close properly so most of the blood would whoosh right back into the heart.

This patient was about to die of congestive heart failure. He had a heart, on chest x-ray, almost as big as a volleyball. The cardiologists expected a guy like that to die in a matter of days so they were willing to let the thoracic surgeons try one of these newfangled valves. The Hufnagel valve was a little cage with a ping-pong ball in it. The surgeons would open the aorta to put it in. They did not take out the old aortic valve, they would just open up the aorta above the aortic valve, put in this cage that had the ping-pong ball, sew it into place and close the chest. They had to work very fast because in those days nobody had yet learned how to stop the heart for more than a few minutes, without brain damage. After the valve was in, every time the heart would beat, if you listened with a stethoscope, instead of hearing "lub dup," it would go "lub click, click." In fact, if the patient would open his mouth you could hear the ping-pong ball going "click, click" without a stethoscope. We students watched this operation and I remember when they opened the aorta, the blood went 3 feet in the air. It was absolutely astonishing. The operation was quite successful. I met the man on the steps of the hospital a couple of weeks later. He was walking up he steps. After greetings, I asked, "Can you still hear it?" He opened his mouth, "click click."

> "Does the sound bother you?"
> "Only when it stops," he replied.

It was during medical school at the Los Angeles County Hospital that Joe met and married a nursing school graduate, Glenda Miksch. After a while she became the nurse for Esther Bogen Tietz, who had built a psychiatric practice in Hollywood.

The Wrong Thinker: Part 2

My first solo publication was a paper called *Student Concepts of Functional Disease*. Many of the clinical faculty kept talking about "functional disease" and it was never clear to me what they meant. I felt that they were actually preaching what in those days I considered dualistic garbage; but there was no easy way to call them on it. I was pretty unhappy with many of my teachers in medical school, because I thought they were mostly an intellectually limited bunch. By the beginning of the fourth year I was so unhappy with all this talk of "functional" that, when some obstetrics (OB) doc was talking about functional bleeding, I asked "Wouldn't it be better to call it dysfunctional bleeding?"

"Yes, some people do," he agreed. But Pappy's clique was growing restless again, and urged the lecturer to keep on. After hearing about functional hallucinations and functional pain, there came the last straw. A guest lecturer on gastrointestinal (GI) problems included in his differential diagnosis for blood in the stool, something he called "functional bleeding." This apparently meant that it was slight and intermittent and the usual tests were unrevealing so it could be disregarded. I jumped up and said "Now what the heck? If there's blood, there's a broken vessel and there must be a structural defect in the gut, not simply dysfunction." One student shouted, "Oh, get off it Bogen!" The professor ignored me and went on to ulcers, malignancies, and so forth.

I was so outraged that I made a large sign and posted it in the hallway. It said: A FREE BEER TO ANY MEDICAL STUDENT WHO WILL GIVE ME A WRITTEN DEFINITION OF FUNCTIONAL, AS IN FUNCTIONAL DISEASE.

I got about 3 dozen replies. Subsequently I asked them to explain their answers (passing notes back and forth during class). Where one had written, "Any illness without structural change," I asked, "How about on the chemical level like early avitaminosis?" He wrote back, "Bogen, you are a wrong thinker and a trouble maker." Many of them wrote, "Anything that's mental and not physical," an almost standard answer. One wrote "It's something that can only be corrected with psychotherapy." So I asked him, "Can people with head injury ever benefit from psychotherapy?"

"Of course," he replied. (I knew his father was a psychiatrist.)

Several said, "Well, the problem really is here and there and you are not sure what it is." Which, of course, is what they were learning from their teachers. I asked, "Does this depend on the amount of your medical knowledge?" The very best reply was "I may not be able to define that word, but when I use it, I know exactly what I mean."

The second best reply was, "Bogen, I wrote you an answer, now where's my beer?" So after class we went down to the nearby Mission Street Bar and I bought everybody a beer including people who hadn't written anything.

By this time it occurred to me to write up what I had. With my paper in hand I approached the two professors who were full time in the school and had (and continued to have for many years) a reputation for teaching. They were sitting in chairs not quite side by side but at a 45-degree angle so my approach was from one side. After I stood there for a bit they stopped talking, and Dr. Helen Martin said, "Yes?"

> "I wrote a paper and would appreciate advice."
> "Oh good, we like having students write papers. What is it about?"
> "It is about the student concept of functional disease."

Without a further word she turned away, back toward Dr. Reynolds, and they resumed their previous conversation without comment, as if I were not present.

It then occurred to me to ask for help from Dr. Peter V. Lee who had become Assistant Dean. He read it immediately and said, "This does seem to have been written in the heat of the moment. Tell you what. You put this away for a couple of weeks and then pull it out. If you think it can be moderated, including removal of the four letter expletives, I'll help you get it published."

I did as he said, and he was as good as his word, sending it to the *Journal of Medical Education* with a letter of recommendation (Bogen, 1956).

"It Ain't Over 'Til It's Over": Graduation

Spending so much time at Cal Tech contributed to the scary problem that suddenly arose the last week of school. Our med school class was to graduate on Saturday AM. When I arrived home from the lab Wednesday afternoon there was a letter from the Dean's office. It said only, "You are required to be in the Dean's office at 7 PM tonight."

"What is this? Some prank?" I thought. However, the letterhead looked all right, and the letter was signed by Assistant Dean Peter V. Lee.

When I appeared, there were five stern faced men; the Assistant Dean Peter V. Lee, the Professor of Surgery, the Professor of Pediatrics, the Professor of Pharmacology from whom I had received an A, and a fifth man whom I did not recognize and who was not introduced.

"Have a seat Joe." Dr. Lee was apparently the chair of this group. "You are here because some serious doubt has arisen with respect to the desirability of you graduating."

"Uh oh," I thought. My best friend in the faculty, Tom Brem, my advisor, had been made Acting Dean. And Dean Brem was out of town!

"Why is that?" I asked.

Dr. Lee replied, "Both Dr. Carmona and Dr. Rabdon have raised some objection to your graduation and when they did, it seems that you have made a number of enemies over the past few years and they have all jumped on the bandwagon."

"Could these be Drs. Starr, Reynolds, and Martin?"

"Among others. There was considerable opinion that you have not been a sufficiently serious student."

It was true that I had expressed disbelief from time to time in some of those professors' favorite theories. And I had been rather flippant when the pediatrics professor once gave me an oral exam. He wanted to know the relative types and dilutions of milk substitutes for various ages and afflictions of infancy.

"Well, generally for a well baby in the absence of mother's milk, half-and-half water and condensed milk. For other situations I would refer to a pediatrician." This was not what he wanted to hear.

The professor of surgery said that my lack of seriousness was evidenced by my cursory treatment of his 3-hour final exam. "A single Blue Book, when most of the class used an entire blue book for each question." There had been four questions, the first being, "distinguish between acute appendicitis and acute pelvic inflammatory disease." On the first page of the Blue Book, I headed two columns: one was "appy" and the other was "PID." Down the left margin of the page I wrote age, sex, location, duration, presentation, and other distinguishing features. I filled in the resulting table and went on to the following questions treating each of them in a similar fashion. I handed in my Blue Book in less than an hour and left. I learned later that another student had used the entire 3 hours to fill up six Blue Books and that he had received an A+ on the final exam. As the story of the surgery professor's poor opinion of me unfolded, I thought I saw a slight flicker of a smile on the pharmacologist's face. It was true that I had missed a number of classes, and there was not much I could say about that. However, I suggested that since I had already been accepted at Cornell starting in July, that I might do some pediatric makeup on my return in 1 year. The pediatric professor refused to consider this solution. It turns out that it would not have helped because he was fired within the subsequent year for harassment of the hospital staff. There was also the question of my having contributed to "class disturbances" on a number of occasions.

After about an hour of discussion, Dr. Lee said that we were through and asked if I had anything further to add. I said, "Yes. I just want to say that if I erred, it was not for lack of seriousness, but more likely that I might have been too serious." On Friday afternoon I received a phone message that I would graduate on Saturday with the rest of the class.

Sam Moore

In spite of absences, "disturbances" and misunderstandings, I graduated from med school and appeared at The New York Hospital Cornell Medical Center on June 30, 1956. Over the next few days Glenda and I moved into the assigned apartment across York Avenue from the hospital and I was put through an orientation. We new interns were outfitted in white pants and short white coats and inspected in military fashion. The inspection was done by Professor Moore who strode in after we were lined up. This was when I realized that his white coat was not only longer than ours, almost to his ankles, but also double-breasted with two rows of buttons down the front. It seemed to me reminiscent of pictures that I had seen of General Erwin Rommel, The Desert Fox. He looked us over and then left us to the Chief Resident, whose speech I have mostly forgotten except for his admonition, "You take care of the charts and we will take care of the patients," making it crystal clear that we were not yet considered doctors. We saw a lot of surgery, mainly while holding retractors, but never did any. Indeed, over the succeeding months it became clear that if I were to do any surgery, I would have to stay for at least another year—assuming I was retained. In that pyramidal system only 8 assistant residents were retained of the 16 surgical interns. The importance of my staying for a second year had to be explained in a lengthy letter to Professor Van Harreveld. To my great relief, he said that he understood and said I could start my postdoctoral year in neurophysiology a year later than originally planned. The internship was quite demanding, as we worked well over 100 hours per week. One episode lightened the grind. I had just emerged from a room into the hallway as Professor Moore was marching down the hall with the usual collection of house staff, nurses, and medical students following behind. Seeing me, he abruptly stopped, did a brisk left face and said.

> "Bogen!"
> Startled, I came to attention, "Sir?"
> "I read your paper in the *Journal of Medical Education* and I have just one thing to say."
> "Yes, Sir?"
> "I may not be able to define the word, but when I use it, I know exactly what I mean."

After which he did a right face and resumed his march down the hall followed by a somewhat bewildered retinue.

The Open Heart

In the spring of my year as a surgical intern at the New York Hospital, there was a minor epidemic of cardiac arrests in the operating suite. The chief of anesthesia insisted that his trainees use ether anesthesia. Nobody uses ether nowadays, but in 1957 they were still using it along with Pentothal inductions. There was a little room next to the operating room where they put the patients to sleep. Inductions with intravenous sodium Pentothal are very nice. If you push the stuff in fast people go out in an instant. But if it is pushed in slowly they go to sleep slowly, a period of time during which they feel pretty good. I was once standing by while the anesthesiologist was dribbling the stuff in and the patient said, "You know, if I could, I would shtay like thish all the time!" During the operation the intern would just be holding retractors. When the surgery concluded, the intern remained to take care of the patient, along with the anesthesiologist. And you would go with the patient to the recovery room where you would write postop orders.

One problem was that there was often a time at the end of the operation when the attending surgeon was gone, the resident surgeon was gone, and the only doctors left were you and the anesthesiologist. During surgery there would be a scrub nurse and the circulating nurse. But by the time the operation was over, one of them was gone and one was folding stuff up. So there would be at most one nurse available. And then the heart would stop.

This happened a few times and people were getting upset about it. Eventually it turned out to result from cardiac potassium shifts caused, in certain patients, by ending the ether anesthesia too quickly. But at first this was not clear. The thing learned first was that if you could keep the heart going, the problem would subside in 10 minutes or so and then everything would be all right. So the problem was to keep the heart and respiration going during that 10-minute interval.

The hospital Executive Committee issued a proclamation that if the anesthesiologist could not feel the pulse in the neck (in the carotid artery), whichever other doctor was there in the operating room (OR) was supposed to open the chest and start squeezing the heart. It was called cardiac "massage" but you would be squeezing it. If there was anybody else in the OR, like a nurse, the job of that third person was to feel for a femoral pulse just below the groin to see if you were squeezing the heart hard enough. Meanwhile the anesthesiologist would maintain respiration by inflating the lungs. So suddenly all of us naive interns were expected to open somebody's chest and squeeze the heart!

In that hospital an intern did not often handle a knife the entire year; all you did in the OR was hold retractors. They might occasionally let you use a pair of scissors to cut the sutures after they tied the knots. It was not like the County Hospital in Los Angeles, where I was a medical student. At the County, the intern is operating the first month he shows up, whether he knows anything or not. There, the saying was: watch one, do one, teach one. Here you were just supposed to watch for a year. In fact, it was almost

10 months, in April, before I did any surgery. I was helping one of the attendings who had private patients in this hospital. The patient needed an appendectomy. The surgeon drew a 2-inch line on the belly with a felt tip pen. Then he stopped and said, "Dr. Bogen, you ever do an appendectomy?"

"No, I haven't held a knife since I've been here."

"Well, you take this knife and you first cut the skin where I've drawn this line on the belly; you cut right there." And he took me step by step through the appendectomy.

You had to be on the private service for this to happen, because when you were on the clinic service, the residents wanted to do all the operating; probably the resident who was there that day thought that he should have got it instead of me. I had a bit more experience when I was helping a thoracic surgeon. All of a sudden he said, "Have you done any operating, just to start?"

> "A guy helped me through an appendectomy last week."
> "Well, why don't you make the incision? Right from here to here, see?"

So I took the scalpel, and I started in a bit gingerly, because he had indicated an incision about 8 inches long. He grunted, "Cut it, doc, don't scratch it!"

By the end of June, I actually ended up doing most of a gallbladder removal. But it was in April when they had first wanted us to open people's chests. I thought, Jeez! I'm going to be there all by myself in the OR, and how do I know whether to believe the anesthesiologist? Suppose the anesthesiologist says, "I can't feel any pulse in the neck." And then I cut open the chest. Suppose there was a pulse but the anesthesiologist just could not feel it? What a hell of a fix to be in.

One time in the follow-up clinic I removed somebody's sutures a day too soon. Henry Mannix came in screaming, "What the hell are you doing, for Christ's sake?" (He was an assistant resident in the fifth year of a seven-year program. I don't know how he got that far because he had a terrible temper and blew up at the least thing.) Thinking about cardiac massage in the OR, I figured I would feel much worse and get hell from more important people than Henry if I opened somebody's chest when I should not. At the end of the year (first of July) I became a junior resident, the second step in the long ladder. After 2 months on Orthopedics I rotated to the general surgery service and covered the emergency room (ER) every other night. It was not an ER as we think of one today. There was just one nurse who was there all the time. Her job was to take care of minor wounds or call a doctor if needed. If the patient had a limb injury she would call the junior resident on Orthopedics who was Walter Shim, from Hawaii; he had been an intern with me. Walt had followed me on to the

Orthopedic Service and he was just down the hall that night. The medical junior resident was upstairs in bed on the 22nd floor. The nurse's instructions included that if a patient came in who had a pain in the chest, call the medical junior resident. If the patient came in with pain in the belly, call the surgical junior resident (me).

So when a man came in shortly after 1 AM with a pain in the belly, she called me. By this time I could tell the difference between a rigid belly and a not rigid belly; a rigid belly meant paging the chief resident; if it was not rigid then I could take longer to try to figure out what was causing the pain. I stepped out in the hall a half hour later when the nurse suddenly appeared.

"The guy just died! He died, just like that!"

"What's that?"

"Come with me!" she said and we ran up the hall to an exam room. In it was a man, lying on the floor, naked.

"What happened?"

"He came in with severe left chest pain so I called the medical resident. She said she would come down. I brought the man into this exam room and told him to get undressed. I came back to take his temperature, and there he was, naked, on the floor, and he wasn't breathing!"

Just then the man made a gasp, a bit like a hiccup. I said, "Well, he's not totally dead. Let's get him up on the bed!" This was possibly a mistake in retrospect, but anyway we lifted him onto the bed and he gasped again. He had no pulse. He was as pale as the sheet under him. No pulse, no breathing except for those two gasps. So he wasn't totally dead yet.

Fortunately, during the summer, I had spent a little time in the library. You never have time as an intern to do anything except run around; you are lucky to ever get to sleep. But as a junior resident I had a little time and went to the library where I started reading up about cardiac arrest. I was still worried that I might be in the operating room when there was a cardiac arrest and, being a junior resident, it would be up to me and not the intern. I came across an article about cardiac arrest. It seemed ours was not the only hospital that had the problem. This was long before anybody ever thought of pushing on the chest. A group at Johns Hopkins Hospital finally figured out that if you pushed on the chest (hard enough to crack the ribs) you could get a femoral pulse from an otherwise lifeless heart. But that came about 2 years later. In 1957, the only method was to open the chest and start squeezing the heart. This approach was well known because sometimes the heart would stop in the middle of an operation with the chest already open. They would squeeze it to get a pulse, and then to get it going again they had electrical defibrillators. If the heart went into fibrillation so it was no longer contracting, they would defibrillate, "Bang!" The shock was enough to make the body bounce right off the table. So that part of it was well known. But the problem for me was: if you have never opened a chest in your whole life, how to do it? There was that one time when the surgeon said to me, "Don't scratch it, doc!" But after I cut the skin, he took over, so I had never actually opened the chest. While I was reading in the library, in one article about cardiac arrest in the OR, there came the answer!

"If you're not sure, cut through the skin and the muscle, between the ribs about the sixth intercostal space. If it doesn't bleed, keep going into the chest. If it bleeds, sew it up."

Now that is practical advice! You are not totally committed right from the start, on the advice of some trainee anesthesiologist you do not know. I have been dubious about people's opinion about anything, anyway. But here was a way to decide. So, at 2:00 in the morning, there was this naked guy going "gasp" just twice. There was a tracheostomy kit in the next room. It had a scalpel, scissors, sutures, gauze, and hemostats. We had been told the kit was there so if somebody's airway was obstructed and you could not get an endotracheal tube in, you could do a tracheostomy right there in the little operating room next to the dressing rooms. It occurred to me that it has everything I need for this. I told her, "Maybe you'd better get that trach tray and bring it in here." She brought the tray in and opened it up. I took the knife and I cut through the guy's skin; it did not bleed. So I opened up the chest.

If you want to get in there, you've got to pull the ribs apart. So I asked her, "Pull up" while I pulled down with my left hand. I shoved my other hand in there and the ribs clamped down on my wrist and I could feel his heart and it was absolutely flabby. Lifeless. So I gave it a squeeze, and I asked the nurse, "Can you feel a pulse anyplace?" She tried the wrist first.

> "No."
> "Do you know how to feel a femoral pulse? Have you ever done it?"
> "Yeah." Fortunately, she knew.
> "Yeah, I can feel a pulse."

I was squeezing about once per second. Let me tell you, if you have not been doing this all your life, your hand gets very tired very soon and the ribs are clamped on your wrist, and your hand is going numb and just about that time the medical resident arrived. She came down 22 floors, sauntered in and said, "What's going on?"

"Breathe for the guy!" So she got a bag and mask and she started breathing for the guy and I was pumping, squeezing the heart. She was having a little trouble holding up the chin but she managed to inflate the lungs and the guy's color started to pink up a little bit but I was about to the point where I could not do any more.

Just then Walter Shim appeared. I said, “Walter, get a glove!” I didn’t have a glove on, being in such a hurry, so I was barehanded in the guy’s chest. “Get a glove and relieve me ‘cause I can’t keep this up!”

“I was just looking for a Band-Aid.”

“Well, I don’t know what you were looking for but get yourself a glove!” He realized what was happening, so he found some sterile gloves.

“You ready, Walter?”

“Yeah.” So I pulled my hand out and he put his hand in and he started squeezing. I decided the thing for me to do was to start an intravenous (IV). Meanwhile the medical resident was still bagging the patient, and Walter Shim was squeezing nicely, which we could tell not only from the pulse but because the man was bleeding from the chest. Suddenly, there at the door was Henry Mannix. Henry stood there a moment and then he screamed, “Bogen, what have you done?” I thought, “Can’t he see? What is he asking this question for?” Well just about the time that he was ranting on for another few seconds or so, the chief resident, Winslow showed up. He did not shout. He just came in, took a quick look, then said, “Where’s an endotracheal tube?” The nurse pointed. He picked it up, pushed the medical resident aside and intubated the guy in about 10 seconds. Then to Shim, “Keep squeezing.” He turned to Mannix, “Get the pads” to defibrillate the heart. Walt needed to be relieved because by this time his hand was totally numb. This time, I had gloves on. I put my gloved hand back in, squeezing while the chief resident and the assistant chief hooked up this machine, and they put the paddles on the guy’s chest. Winslow said, “Pull out your hand.”

I pulled my hand out, Mannix pushed the button, “Bang!” and the guy’s entire body jumped up from the bed. But the heart remained lifeless. I reinserted my hand and kept squeezing. He was breathing on his own now, and being intubated, he had a nice clear airway so they do not need to bag him any more.

“Let’s give it another try,” Winslow said. “Bang!” Finally, the third time they shock him, the heart started up.

The chief resident says, “Alright, his heart’s beating. That’s good. Now we’re going to close up the chest. Henry and I’ll do that. And Bogen, you go and write this up in the chart.”

Walt Shim found his Band-Aid and went back down the hall, and the medical resident went back to bed.

The chief and Mannix moved the guy into the teeny operating room next to the exam room. They filled up his chest with penicillin and streptomycin, dribbled in some Pentothal to keep him from moving, and sewed up his chest while the man was starting to wake up. Charlie Bidlo was his name. He survived with, so far as we could tell, no brain damage. He had a stormy course at first but was looking fairly good toward the end of his 30-day hospital stay.

Pretty soon the whole hospital knew about this. In that hospital they had a busy public relations department. The New York Hospital folks liked to think that theirs was the best hospital. They would never admit that the Massachusetts General might be better. And one thing that helped make them good was money. Of course, the better the hospital, the more the rich people want to go, and once the rich people go, you hit them up for money. Some really wealthy people in New York were patients. (When I was an intern, one morning my partner and I started IVs on Senator Jack Kennedy, Ella Fitzgerald, and the Foreign Minister of Saudi Arabia along with a dozen other patients before morning rounds.) So they had the Paines and the Whitneys and other rich donors. Every year they put out a brochure about the hospital. The brochure told what wonderful things the hospital had done that year. That year, the brochure was going to be about Charlie Bidlo, because opening somebody's chest in the ER had never been done before in the entire country.

Professional journalists that worked for the publication came around interviewing everybody. I remember one asked, "Tell me just what happened." I started my story and I got to the point where I said to the nurse, "Maybe you better get that trach tray and ..."

She interrupted, "Doctors don't talk that way. They say, 'Get the emergency tray, stat.' Isn't that what you said?"

"No, that's not what I said. I said 'maybe you ought to get that trach tray.' "

"Well, that's not the way it's going to be in the story." That was my first direct contact with the journalistic mind, with media truth.

Meanwhile, there was an epidemic of house staff opening chests. Every time somebody would die, whoever was nearby would open the chest. The hospital Executive Committee had a meeting and issued policy. The next day the chief resident, Winslow, who had closed up Charlie Bidlo's chest, said, "When we get together for rounds in the morning, we will itemize."

We would typically make rounds about 7 AM, after the interns had started all the IVs. So if you were a junior resident, you did not have to get there at 5:30 AM, you only had to get there about a quarter to 7:00. The chief got everybody together.

"Now, we're going to make rounds. Gonna be a little bit different. Of course I want to hear what you know about the patients, and we're going to discuss what to do. And one other thing. For each patient, I'm going to tell you whether you open his chest if the person arrests or not. We're not going to have any terminal cancer patients with their chests opened up because they have finally been relieved by the Lord of their burden."

He went through all three dozen patients. As we walked the rooms, we would come along to somebody and the intern would give a report. The chief would ask a few questions and give a few orders. Then we would step out in the hall and he would say, "Opening this one is OK." Or, "Not this one." We went through the whole ward like that. This guy was quite clear as to who was going to be salvaged, if possible, and who was not. They were looking for excuses to let house staff go anyway, as it was a pyramidal system. There had been 16 of us surgical interns and only half went on to become junior residents. The other 8 went somewhere; they mostly disappeared. One guy I do know about went into psychiatry. Halfway through the surgical internship he said, "I'm going into psychiatry." I said, "Why?" He says, "Because I am sick and tired of being sick and tired." That was shortly after an intern named Maury Hanson threw up blood and fainted while in the operating room. Elton Cahow, who had been my intern partner, went on to be an intern in medicine. He had decided he wanted another whole year as an intern! Talk about masochism! But he wanted to be really good in medicine, because he had great academic ambitions. In fact, many years later I remember reading in an American College of Surgeons Bulletin a notice that Dr. Elton Cahow had just been promoted to Professor and Chairman of the Department of Surgery at Yale.

After a month in the hospital, Charlie Bidlo went home. A bit later he came to visit my wife and me for lunch, which was nice. I next went to the urology service, learning how to catheterize bladders. A few days after I arrived, the urology resident turned to me, "Well, Bogen, it looks like your reputation is made in this hospital."

I said, "Yeah, until the next time I screw up." What I could have told him, but did not, was a conversation in an elevator just before I came on the urology service. I was still on the general surgery service and was in an elevator with Joe Harbison, who had been an intern with me and, like me, was now a junior resident. The elevator stopped at the fifth floor to let some people on. They came on in order of seniority: first was Frank Glenn, the Lewis Atterbury Stimson Professor of Surgery and Chairman of the Department of Surgery and Surgeon-in-Chief of The New York Hospital Cornell Medical Center. After him came an assistant professor, then behind him the chief resident and a junior resident and then an intern, three medical students, and a couple of nurses, filling up the elevator. The elevator went up four flights to where they were going to get off. As the door opened, Professor Frank Glenn, turned to my friend, put his arm across his shoulders and said to him, "Joe, you know, opening that man's heart down in the ER was a wonderful thing. We're proud of you." And then he went out the door, followed by his retinue. Joe Harbison waited for the door to close before laughing, shook his head, and said "The Man sure knows his troops, doesn't he?" It was as delicious as whipped cream on a hot fudge sundae.

I sent a copy of the New York Hospital Annual Report to my mother back in Los Angeles. I thought she would call me on the telephone and compliment me on my heroic deed. She did not call. About a week later I got a little package from home. In the package was one of those miniature gold painted cups you can get at trinket shops. It has a little plate on the base you can have engraved. The entire thing was about 8 inches high, a little golden cup on a stand and a plate at the bottom. It was engraved, "World's Greatest Doctor."

A Little Something to Treasure

While on the urology service I was paged: "an old man named Lowey on the 18th floor has a bladder stone and needs a Foley." I picked up a catheter tray and went up the elevator. After knocking, I entered to see an elderly man doubled over in pain and moaning. When the bladder spasms receded a bit I asked, "Are you Mr. Lowey?" In the middle of his misery he straightened up and answered, "I am zuh(sic) Herr Professor Doktor Otto Loewi!," following which his spasms worsened and he was again rendered speechless and groaning. I thought, "My goodness, I thought he was dead! Here I am with a legendary man, right here, and I can help."

"Well, Professor, I am going to fix you up."

After 1000 cc had been drained, I clamped the tube to avoid a sudden drop in blood pressure and waited about 15 minutes (it turned out he had over 2000 cc in his bladder). While we worked he was rather talkative, feeling great, relieved, and grateful. On this and a subsequent session he was full of advice, having learned of my scientific ambitions. He particularly enjoyed telling the story of how his Nobel winning experiment came to him in a dream. "It's in my book, young man." As I was pretty much stuck at the hospital, my wife obtained a copy of *From the Workshop of Discoveries.* Meanwhile I held Professor Loewi's hand, literally and metaphorically, as he went to the OR where he had a spinal anesthetic. His urologist inserted a big endoscope large enough to insert the tools with which he broke up the stone and laboriously removed it piece by piece, taking about an hour. After accompanying Professor Loewi through his procedure I spent more time in his postop care during which he lectured me further. He signed my copy of his little 62-page book, above his signature in a shaky hand he wrote,

> "Facts without Theory is chaos,
> Theory without facts is phantasy."

Paul Starr and Bronson Ray

When I was rotating as an assistant resident, there was a famous neurosurgeon named Bronson Ray. So Sam Moore, who was the professor in charge of all the residents' things, said, "Well you need to spend a few months with Bronson Ray. If you want to go on to neurosurgery, before you leave the New York Hospital you should spend some time with Dr. Ray." So I did. Ray was doing about two hypophysectomies a week. Taking out the pituitary was the best treatment at that time for metastatic breast cancer and prostate cancer. And the Memorial Hospital for Cancer was right across the street so they had these cases by the dozens. They would ship them across the street and Ray would take out the pituitary. He did so many that the neurosurgery residents were bored with it so the general surgery residents like me rotating through did all the assisting for the pituitary.

It was worth watching because he had it down to a system. Because all the operations were almost always alike, it was the same operation over and over again, like watching a guy conduct a symphony and every bar was familiar. The whole thing moved smoothly. A great contrast was when I helped him with some other cases like putting a tantalum plate in somebody's head. He said, "Which way is up? How do you do this?" It was pretty funny. Anyway, I said, "Dr. Ray I suppose I should read a little bit about the pituitary while I'm helping with all these surgeries." He said, "Well, you go to the medical library and read anything written by ..." (Whatever the man's name was, I forget, an endocrinologist at Yale.) This was a man who had invented a clever system that my mother employed at one time. You would put a piece of endocrine tissue in the anterior chamber of the eye of a rabbit and you could watch it. You could actually observe what happened when certain things happened. For example, you put a little adrenal cortex in there and you give ACTH and so on and so forth.

So I read the paper. He said, "We had this disagreement with a group in Los Angeles about the actions of hormones and they said that the reason we didn't get the same results that they got, when we used our special technique, was because ectopic adreno-cortical tissue would not respond to ACTH." And it dawned on me that Paul Starr, who had taught us all in medical school that ectopic adrenal tissue does not respond to ACTH, had taught us as a fact something that he had dreamed up to explain why he did not have the same results as Yale. I know this recollection is correct because we had a yearbook when we graduated from medical school, and some guy did a bunch of cartoons for the yearbook and one of them was a cartoon of Paul Starr saying "Stop thinking and start memorizing." He remembered it the same way I did. So you could imagine how mad I was when I got to New York and I was in the library reading about how there are doctors in Los Angeles who have hypothesized that ectopic adrenocortical tissue does not respond to ACTH.

Part of the background to this was when I was a sophomore in medical school I had been particularly impressed with the physiology professor, a man named Doug Drury. He was a very solemn guy. Everybody was supposed to have a little elective in the second half of the sophomore year so I went to Dr. Drury because I liked him better than the other people. He said, "All right, I'll give you a little project. We do a lot of work with insulin and carbohydrate metabolism, and we have rabbits with pancreatectomy and what we could use would be some rabbits with hypophysectomy.

Take out the pituitary. So why don't you figure out how to do that. You go to Al White and he'll show you where the rabbits are and will help you get some instruments to work with. And I tell you what. If you take out the pituitary of a rabbit and keep it alive for three days, I'll give you an 'A.' Goodbye." So I went to Al White and asked, "Will you show me?" And he said, "Yeah I can show you where the rabbits are but I don't know how to do the surgery." They gave me a little broom closet to work in, a little teeny room but it was enough of a room with a table and I anesthetized the rabbits.

It is really easy to kill rabbits. Anesthetizing cats is simple stuff relatively. But with rabbits it is different. Especially with the ether. If you give them a little too much ether, they are gone. So that was the first hurdle. But how to do the surgery was the hurdle before that. I had to figure out what I was going to do before I even got the first rabbit. So I looked in the library and I found a book called *Experimental Surgery* by a man named Markowitz. I looked in the book and sure enough there is a description of how to do a hypophysectomy on a dog. I thought, well it cannot be that different. So it was very helpful. You do it through the roof of the mouth and you have to drill away all the palate. It turns out, when you get through the base of the skull up through the palate (you have to make a hole in the base of the skull to get to the pituitary), that in a rabbit there is an enormous plexus of veins immediately under the bone. The first thing you get is this big upwelling of blood. You cannot see anything but blood. And I did not have suction and I had to mop it up with cotton. So I had a few rabbits that bled to death.

Finally after a few weeks, ignoring a few other classes and spending my time in the broom closet, I figured out how to get control of the bleeding and get through the bone. Then I scooped out the pituitary. No problem there. Then I close up the rabbit. But when I came around the next day, the rabbit was dead. This goes on week after week. One dead rabbit after the other. I got the surgery figured out. They were not bleeding to death anymore but they were all dying the next day. I figured there was something missing. Well what was missing, of course, was the ACTH, and I did not have any cortisone. Then I happened to stumble on an article about hypophysectomies. The way you do it with a rat is that you turn it upside down and run it in a jig under a drill press and it goes zip, so they were doing them by the hundreds. What they found out when doing hypophysectomies on rats is that they eat salt veraciously. I thought OK. So what I did was get a big syringe full of saline, and I stuck it in the belly, and the rabbits lived. I went around to Drury and said, "I got one!" He came around and looked and said, "It's alive all right. When did you do this surgery?" I said, "Four days ago." He said, "Really... where did you get the cortisone?" I said, "I didn't get any cortisone." He said, "You can't get ACTH. It's not available." I said, "No, I just gave it a lot of salt." He said, "Well, you got an A."

So when I got to New York with Dr. Ray doing all these hypophysectomies, I took a particular interest as I had done a lot of hypophysectomies myself.

Bidlo Lesson

The 2 years at Cornell were enriching in many ways, but probably the most important was the experience with Charlie Bidlo. If, after pondering a problem for a time, eventually coming on an illuminating conclusion, one has the fortitude to act on that conclusion, in the face of prevailing orthodoxy and if one then has the good fortune to obtain a happy result, this strengthens one's confidence when faced in the future with other weighty issues.

Adventures with Professor William Sweet of Harvard

Halfway through my second year at Cornell, and hoping to enter a neurosurgery residency after spending my post doc year with Van H at Cal Tech, I began to apply. I asked Professor Sam Moore for recommendations. He was very encouraging, saying that my application to the Mass General in Boston was the only one necessary. "You are a cinch to get in there," he said. When I was invited to Boston for an interview, Dr. Moore arranged the time for me to meet with Dr. Sweet. Soon after returning to New York, Dr. Sweet sent me a letter asking me to return because Dr. White, his superior, had been out of town. I went to Boston again. Then I waited for some reply (follow-up letter, pro or con), but none came. As summer approached I wrote Dr. Sweet asking if he wanted me in Boston again, especially as I was leaving for California in July. He did not respond. A year later, while I was at Cal Tech I was still without a residency appointment. I again wrote to Dr. Sweet. He did not reply.

Dr. Finley Russell, a former student of Van Harreveld was working in the lab and suggested a meeting with Professor Vogel of Loma Linda University Medical School. I had never heard of Philip Vogel, but I knew that the main person at the Loma Linda program was Cyril B. Courville, author of some 500 papers and about 20 books. He was a world-famous man, and I remember him as an excellent lecturer. Finley Russell, Professor Vogel, and I met in the cafeteria. I was struck at that meeting that Professor Vogel was finishing off a mustard-laden hot dog in his left hand while spooning chocolate ice cream with his right hand. I assumed that he was testing me in some way, but having gotten to know him I (now) believe he (just) wanted to eat before the hot dog got cold, and the ice cream melted. The unorthodoxy of it was apparently, for Phil, unimportant. I bargained for only 4 more years, as I already had 2 years general surgery and a year in neurophysiology. He said, "Second year residents now spend a year with Courville in Neurosurgery. If you started in the second year, Courville would then have two residents. Then there would be no one for him the next year, so you have to do 5 years, but you can do anything you want the last year so long as you take call and attend rounds and conferences." At this point Finley said, "You can be a research fellow in my lab, do what you want and I would pay you $500 per month." Wow! That was more than any first-year resident got in those days, plus freedom to do more neurophysiology of my own. It was settled.

About 6 months later while sitting in Russell's lab with Professor Berry Campbell, I got a letter from Dr. Sweet. He had an opening and was offering it to me. Here I had just committed to 5 years with Vogel and now he writes! Probably one of his people got severely ill or got drafted—something changed his mind. I wrote a vitriolic letter dripping with anger and resentfulness. Fortunately, I remembered the lesson from medical school and put this letter away for several days. After all, Dr. Sweet could wait a little for an answer. I tore up the first letter and wrote to him that I had very much wanted to join him and Dr. White, but I was by now committed and asked for his understanding.

A year later I submitted a paper with Berry Campbell to the Surgical Forum, a prestigious and rather exclusive venue. It was accepted, so I went to the American College of Surgeons meeting in San Francisco. When it was my turn to present, I approached the podium adjacent to which was a table seating the members of the program committee. The chairman, Professor William Sweet, gave me a friendly smile and a complimentary introduction. Two years later, when I presented our first split-brain patient to the Surgical Forum, Dr. Sweet was in the audience. He rose to say that Dr. Van Wagenen had once opened up the head of a patient and was at that moment called to an emergency so the resident closed the head. Sweet said, "So this patient had a mock operation without callosotomy and was seizure free thereafter. Your patient may be the same, the callosotomy being irrelevant." I knew he was wrong. I was familiar with the papers of Akelaitis and knew about Van Wagenen's patients. The patient had been only seizure free for a few weeks and was later reoperated by Van Wagenen. I managed to control my outrage and quickly left the podium trying not to alienate someone who might be on the Board 2 years hence. After I got home I sent Dr. Sweet a photocopy of the relevant papers. He did not reply.

In January of 1958 while Joe and Glenda were still in New York City their first child, Glen David was born. By the time they were back in Los Angeles, their second child Meriel was on the way, and Glen David had begun to show symptoms of what was later diagnosed as Tay Sachs disease. Soon thereafter, Joe's mother went into end-stage kidney failure, and it was left to him to continue psychotherapy for a few of her patients while finding referrals for each. Esther Bogen died in early 1960, just as Joe was beginning his residency at The White Memorial Hospital and continuing *his interest in research. Glen David died at the City of Hope Hospital in 1961, just after the birth of a third child, Mira, who like her sister, was healthy.*

A Little Work with Finley Russell

Fin specialized in the physiology of venoms, especially snake bites. The hallway the full length of his lab was lined with glass enclosures, each holding a different species of rattlesnake or, in a few cases, scorpions. So, after I had the dorsal root, ventral root reflex preparation working well, Fin wanted to see the effects of the various venoms, so I ended up working with him after all.

There was another way that we worked together. The laboratory was on the ground of the L.A. County General Hospital, and Fin frequently consulted for patients with snake bites, and he sometimes asked me to assist in the treatment. The cases that were particularly demanding were when the patient was allergic to the horse serum. For a few species of snake there were antivenoms produced in rabbits, but for most species of snake there was only horse serum. Because the antivenom was essential, the treatment commonly resulted in the side effects of the antivenom, including any allergic responses. When a snake hobbyist in San Diego was bitten by a South Pacific Sea Snake, he was flown to the County Hospital. The treatment featured the delivery of small doses of antivenom in an intravenous line in one arm, while introducing small amounts of adrenaline sufficient to maintain his blood pressure in the other arm. This went on for several days during which there was continuous newspaper coverage: "PACIFIC SEA SNAKE!" The hospital public relations department notified us that we were each to be awarded a County Certificate of Merit. The ceremony consisted of having us line up on either side of County Supervisor Debs while photographs were taken of him handing us the certificates.

Berry Campbell

It turned out when I got to Fin Russell's lab that I would be working mainly with Berry Campbell. Berry, who had been Professor of Anatomy at Minnesota, was made part of the Neurosurgery Department because Dr. Vogel had a big grant to study multiple sclerosis (MS). It turned out that Berry had pioneered experimental allergic encephalomyelitis as a candidate model for studying MS in guinea pigs, rabbits, and so forth. Berry helped me in many ways, above all in dissuading me slowly of the Sherringtonian views in which I had been indoctrinated. He introduced me to the work of Hendricks and George Eliot Coghill, whose views seemed dramatically opposite to Sherrington's. Berry had done a post doc with Gasser at the Rockefeller Institute. He went there to learn what he believed to be the coming thing, electrical recording from the nervous system. He had not yet learned that what was generally accepted could easily become dogma. Berry had been trained as a mammalogist but was aware that in neurophysiology it was widely thought that neurons were uniformly unimodal. So he was delighted to observe that the responses he was recorded from superior colliculus were influenced by various ambient sounds not just visual stimuli. Enthused, he took his findings of polymodal activity to Gasser, who insisted that they must be erroneous and would certainly bring disrepute to the Institute if they were ever to be published.

Most importantly, Berry encouraged me to do hemispherectomies in cats. This followed from what I might call his Basic Idea. The Basic Idea was that when the animal has only one brain, anything one does in the way of ablation, stimulation, or recording will provide useful information, because the outcomes will not be compensated or modulated by the other brain.

A Little About the Cats

During this time Joe began to operate on "The Cats." He would bring the newly operated cats home, and Glenda acted as their "recovery nurse." On one occasion when a family member asked Joe "How's the work going?" his only reply was, "Well, the damned cat died again." Over 30 years later, when his daughter Mira was in veterinary school, he admitted, "Well, in those days, I sure killed my share of cats!"

They look like ordinary cats unless you know what to look for. If they get frightened, for example, they circle and they always circle toward the side of the empty half of the head. I remember we had one at home for years. In fact, this cat we had at home raised a litter of kittens. It just seemed like the most normal cat in the world, except for one time when I had the lawn mower out. I started up the lawn mower and the cat started going around like a top. The other way to tell if there is something different about them, and the only reliable way, is to look for hemianopia, the field defect. Because if you do a hemispherectomy in an adult or in a youthful cat, not an infant cat, it will have the field defect for the rest of its life.

I trained a cat to come to a whistle. We had a fairly big front yard at our house in the suburbs; there was a fence between the neighbor and us and it was only a couple of inches wide, just boards. Whenever I would come into the yard I would blow a whistle and the cat would come to me along the narrow fence. When it would come I would give it some kind of treat. People would say, "Why do you want your cat to think it's a dog?" I would say, "What do you mean?" And they would say, "Well, you whistle for a dog and for a cat you say, here kitty, kitty." I would say, "If I said kitty, kitty, people would feel it was instinctive. This way it's obvious that the cat has learned." Then they would say, "Well you trained a cat, big deal."

And I would say, "But you don't understand, this cat has only half a cerebrum, half the brain is out."

I remember one fellow said, "Yeah, it did look kind of funny to me." I said, "Well in that case, which half is out?" Unless you look for the hemianopia, the cats really look normal for the most part. The monkeys are the same. There were some hemispherectomized monkeys in Pittsburgh. Well, more precisely they were hemicerebrectomized. The way I did the cats was I took a spatula and put it right down the middle. When the guys in Pittsburgh did the monkeys, they did the same thing. They did one thalamus along with the rest of it. They are the ones who called it hemicerebrectomy. They had those monkeys for at least 10 years and they published papers. You would think that because they published so many papers over the years that people would know about this, yet no one seems to care. Patton did most of the writing along with Cooper and Kostkoff, who was the neurosurgeon. In Patton's final review he said that it was still a challenge to find something that a normal monkey can do that the hemi'd monkeys cannot do just as well. They are slower, but they learn everything. People just do not seem to want to know that.

After a couple of years with Berry, we applied for a National Institutes of Health (NIH) grant. We proposed a farm full of hemispherectomized cats followed by a program based on Berry's Basic Idea. We had a site visit that ended up in a rather unpleasant discussion in which the visitors wanted us to adopt some quantitative measurement—"based on movies of the cat reactions?!" That seemed as good to them as it seemed make-workish ridiculous to me. When I laughed it became clear that I was contemptuous of their proposal.

"We could spend the first three years just working on the measurement method before doing any testing of ideas." That seemed the ultimate impolitic and I swore that I would never apply for a grant again. Subsequent to 1963, whatever I spent for travel to meetings, slides, poster, reprints, and so forth all came out of my earning as a neurosurgeon.

Two Professors

Vogel, as far as I know, never raised his voice, ever. And he was never unreasonable, as far as I know. He was unlike Kenneth Abbott. He got Abbott to come to White Memorial Hospital. Abbott would have been in line to be professor at Ohio State but he did not know the guy ahead of him was going to die. And Phil appealed to his Seventh-Day Adventist loyalty. So he got him out here and he was a typical product of the screaming school of neurosurgeons. I was helping him do a cervical laminectomy on an old lady in a somewhat sitting position. He stopped for a moment while he was waiting for something. And he asked the anesthesiologist, "How's she doing?" The guy answered, "She's doing all right." He said, "What I mean is, what's the blood pressure?" The guy said, "It's about 80 over 40." And he said, "It's what! How long?" The guys said, "I don't know, five or ten minutes or so. It's coming up a little bit." Abbott screamed, "How could you do this to me!" He really took it personally. It is true. When you have got an old lady you do not want the blood pressure to get down there. Fortunately when they are anesthetized, the brain is somewhat protected but still in all he was a screamer.

Everybody was scared of him. I was of scared of him in the beginning. But by the time I got to be chief resident, I was no longer afraid of him. I came into the office one time and there was a screen and the secretary was in front of the screen. And I did not know that Abbott was right behind the screen and heard my conversation with the secretary. What I said to her was, "Here is this list of all the procedures that I have participated in for the last six months. Dr. Abbott wanted us to keep a list so I kept a list of all the stuff that I've done and here it is." And he stepped out from behind the screen and said, "And make sure everybody else does that too!" And I thought to myself that he was totally unreasonable. It was not my job to make sure everybody does what they were supposed to. He was not going to tell me what my obligations were. That was really an epiphany. In a moment of enlightenment, I realized that I could not depend on this guy to decide what I was obligated to do. I needed to decide for myself because he did not know what he was doing. He was insatiable.

When I was attending, there was a new resident named Harris. Harris was still afraid of him even though he was the chief resident and had been there for 5 years. Abbott got Parkinsonism at an early age, in his 50s. It was very embarrassing, because he would shake. And he could not stand being a shaky surgeon. He was very upset. I remember when we would pass by somebody who was comatose, an obviously kind of hopeless brain case, but still on life support, he would say, "Boy, I hope they don't do that with me because that's a waste." Anyway, with that background, he had a heart attack and was in the intensive care unit at the Glendale Adventist Hospital. I know that for a fact, but the rest is second hand from a nurse who told me. She said he was in there on a monitor going "beep, beep" with the heart. Then it stopped. She went over and sure enough it was not the beeper that was wrong, his heart had stopped. She ran out in the hall to see if there were any doctors and there was Harris the chief resident. She said, "Quick, he just had an arrest, come in!" So Harris went in there and by this time they were not opening hearts the way I did many years before. By this time everybody was doing the external thing, leaning on the sternum and breaking ribs and stuff. So Harris starts pumping on Abbott and he woke up. He woke up! And he looked up at Harris and he said, "You! STOP THAT!" So Harris stopped. And that was the last story about Abbott.

Now Vogel, he never yelled at anybody. The only time in all the years I knew him, which was many years, he only spoke harshly to me once.

What happened was that I was in the operating room by myself with an assistant. We were starting to close and a nurse comes in and asks, "Is this patient supposed to go to intensive care after recovery room?" I said, "If you will look on the printed orders, you will see that anybody who has had a craniotomy is supposed to go to intensive care from the recovery room and if there is no intensive care there should be a private duty nurse." We did not have a special neurointensive care at that time. So she went out. The next day I was helping Vogel in the same room doing a case, a craniotomy again. The same nurse came in and asked the same question. "Is this patient supposed to go to intensive care?" I looked up and said, "It's just like yesterday. Look on the printed orders!" So she went out. And he turned to me and said, "Don't ever do that again. I won't tolerate it. It's not necessary to yell at people." So I never yelled at anybody again at the White Memorial Hospital.

Getting to Know Roger

The first time I saw Roger Sperry was when he gave a lecture at Cal Tech, which I attended as the guest of my mother, who had been working with Professor Van Harreveld on adrenaline-like substances in the blood and their variation in patients being treated with electrosleep therapy. Sperry appeared largely at the insistence of the geneticist Norm Horowitz who had been impressed, at a meeting, by Sperry's insistence that behavior was rooted in genetically determined neuronal circuits. Very few psychologists were saying such things in 1952. At this talk to a select group of faculty (Fig. 1), Sperry lucidly described and dramatically illustrated the discriminative ability of cats with various alterations of visual cortex. I was bowled over by his talk. I believe this dramatic talk led directly to his being made the first Hixon Professor of Psychobiology, a post he held for the next 3 decades.

I next saw Roger in the summer of 1955 when I was a research assistant to Van H, whose lab and office were just down the hall from Roger's. Ronnie Meyers was finishing up his joint M.D./Ph.D. program that he had started with Roger when both were in Chicago operating and testing split-brain cats. I spent considerable time with Ronnie discussing our mutual desire to emulate Penfield by doing research with humans, including our wondering what one would see with split-brain humans.

I was intrigued by the experiments on cortical spreading depression that I was doing with Van H, so much so that I often returned to the lab after summer ended, even though the school year had started and I was supposed to be in med school. But there was nothing else at Cal Tech or in med school that compared with Ronnie Meyers split-brain cats. The split-brain cats made an impression on everybody who saw them and many who only read about them. Those cats made a particularly profound

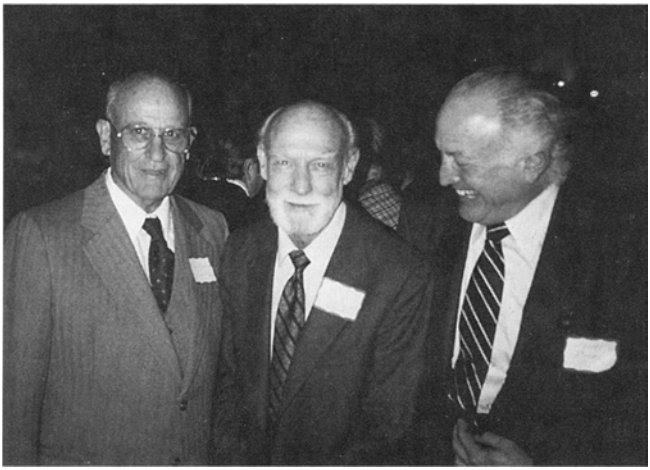

Fig. 1. From Left to Right, Philip Vogel, Roger Sperry, Joseph Bogen.

impression on me because I had been struggling over two strongly held beliefs: (1) each of us has another mind, that is, goal-directed mentation of which we are unaware and (2) all mentation needs to be explained physiologically. And here was a replicable demonstration of two minds, functioning in some ways independently, and in the same head; a duality of mind with an anatomical basis. It was for me personally the most influential scientific experiment that I have ever seen or heard of, before or since. It set the course of my life. It rarely left my thoughts. Subsequent experience with hemispherectomy patients has convinced me that each of them, having only one brain, has an unconscious in the Freudian sense—so this compounds the problem! In any case, if psychoanalytic theory, or any psychodynamic theory, is to take account of the split-brain, it has a long way to go (Bogen, 2000a).

After escaping from medical school and after 2 years in general surgery, I returned to Van Harreveld, this time as a post doc in neurophysiology. During that time Roger and I became better acquainted. Most of the time when I would pass by Roger's office, the door would be open. Sometimes he would be reading or doodling on a pad. Sometimes he would be sitting back with his feet on the desk, apparently staring off into space. Then, one day, he was gone—into the lab. Not long after, we had a third floor seminar. At one of our regular third floor seminars Attardi presented her work with Sperry on optic nerve regeneration. The slides were sections of goldfish brain, stained a bluish-black except for the regenerating fibers that were bright pink! The pink regenerating fibers were snaking their way through the neuromatous jumble of the optic chiasm. Around the front of the optic lobe they went, passing over the proximal tectum and then diving abruptly into their intended targets. It was spectacular! Unfortunately, when this work was published, the pictures were reproduced in black and white. That was 5 years after appearance of the Abstract in *Anatomical Record* in 1958. Such a long delay was not unusual for Roger. He often kept papers on his desk for a long time, for several reasons. One was that he liked to have some idea of how the follow-up experiments were developing before finalizing the discussion of the earlier paper.

Roger did not always delay. One day when I was visiting the lab I asked him about the Gordon paper on lateralized olfaction in split-brain patients. He said, "We have to send this olfactory paper in immediately." "Why?" I asked. "Because I have just refereed for *Neuropsychologia* a paper with a similar experiment in rats. People know that with human subjects, we can do in a few weeks what would take many months in rats. If we delay, people might think that I got the idea when refereeing the rat paper." Roger seemed to think of everything. I idolized him and hung on his every word, of which there were not very many. I thought him *the* experimental physiologist of our time.

In 1960 I was working at the County Hospital. I took him an essay on epilepsy entitled, "A Rationale For Splitting the Human Brain." His laconic comments included, "Maybe you should change the title." Also, "Look up those papers by Akelaitis." When I did, it appeared that the callosal surgery by Van Wagenen 20 years before had actually turned out better than was then (about 1960) the prevailing medical opinion (Bogen, 1997a). This led eventually to a nearly 30-year joint effort. I like to think of it as a collaboration, although in fact our teacher-student relationship persisted throughout. While I was at the County Hospital, one of my projects involved some behavioral experiments with rats, with results I could not understand. It seemed to me that if anybody could help it would be Roger. I took my data up to the Institute. After some technical comments he mumbled, "If you keep working with that you might come up with something dramatic." Roger Sperry's facility for "coming up with something dramatic," time after time, in a variety of contexts, was not simply because he kept in mind the value of a decisive, counterintuitive result. Nor was it only because he was an expert experimentalist. Nor only because he was at the same time a creative and highly disciplined presenter. Essential was his being among the deepest, the most profound, neurothinkers of our time.

On one occasion, after members of an NIH site visit team had left, I asked Roger what he had said to influence their decision. "Three of the five were psychologists," he replied. "I said that this was the only psychology program at Cal Tech and if it were not supported there wouldn't be any." This was not simply a ploy. Roger sometimes dryly alluded to being surrounded by molecularists plugging away without any interest in what he called "the big problems." He meant by this both problems of society and problems identified by psychologists, requiring physiological answers.

Roger's emphasis on psyche and consciousness was long present in his thoughts. This emphasis became progressively more evident in his writing. He felt his first paper to assert forcefully what he called the "Central Issue" was the "Platt piece," that is, his chapter in *New Views of the Nature of Man* in 1965. In this chapter he asserted that the Central Issue is the nature of consciousness and that a correct model of brain function could not be constructed "without including consciousness in the causal sequence."

Even by 1970, Roger had become widely recognized for the previously mentioned views and was attracting much philosophic attention, both pro and con. It was then, in 1970, that Oliver Zangwill, Professor of Psychology at Cambridge, the premier psychologist in England and possibly in the English-speaking world, came to Cal Tech for the entire month of August, at Sperry's invitation. Oliver was bent on seeing the split-brain patients for himself, and Roger wanted Oliver's reaction to his efforts to bring science into the humanities, and vice versa. After several weeks of socializing with Oliver, I was emboldened to ask him, "What are you telling Roger?" "I'm a bit concerned," he confided, "that if he goes on in this vein it is likely to diminish the impact of his many marvelous achievements." "How did he react to that?" I asked. "Very little," was the stiff-lipped reply.

Oliver Zangwill's prediction was fulfilled by the time Sperry was honored with a party at Cal Tech in 1982 for having brought to the Biology Department its fourth Nobel Award. Those who had not known him early on assumed that "he's gone religious like so many old folks." By 1990, even those Cal Tech professors who had been his friends for nearly 40 years had given up trying to defend or even to understand "the philosophy of his later years" as one of them put it. Contributing to the unhappiness at Cal Tech was Roger's habitual obliquity. Hardly helpful were his cryptic comments to senior professors, twice in my presence on the inability of quantum mechanics to save a world terribly threatened by overpopulation. "You'll never solve the really big problems of this world with quantum mechanics." When he said this in his quiet but deprecating manner to Norm Horowitz (we were the two dinner guests of Roger and Norma), Norm became incensed.

"What does *that* mean?" Norm sputtered.

More About the Cats (and Monkeys too)

During the last 50 years, a large number and variety of experiments have made it clear that the corpus callosum can transfer high-level information from one hemisphere to the other. Moreover, we now know that the hemispheres are not so much "major" and "minor" as they are complementary and that each hemisphere is capable of thinking on its own, in its own way. Much of this information has come from cutting the corpus callosum, that is, the split-brain operation.

Split-brain experiments started with the problem of interocular transfer. That is, if one learns with one eye how to solve a problem, then with that eye covered and using the other eye, one readily solves the problem without further learning. This is called "interocular transfer of learning." Of course, the learning is not in the eye and then transferred to the other eye, but that is the way it is usually described. The fact that transfer occurs may seem obvious, but it is in the questioning of the obvious that discoveries are often produced. In this case the question was: How can the learning with one eye appear with use of the other? Put in experimentally testable terms, where are the two eyes connected?

Experiments showed that the transfer actually occurs between the hemispheres by way of the corpus callosum. Sperry's scheme (with the cats, and later monkeys) was to split the optic chiasm so that the right eye goes only to the right cerebral hemisphere and the left eye to the left hemisphere, in addition to cutting the corpus callosum between the two hemispheres. This is a "split-brain cat." The cat can be trained with the right eye to choose a cross rather than a square, while the left eye is covered. (This was originally done by Ronnie Meyers using an asymmetrical blindfold. Subsequently it was done by inserting a corneal lens similar to what would be used by humans except it was opaque.) The cat, with one eye occluded, chooses one of two doors at the end of a runway. Two cards labeled with either the cross or square are attached to the doors randomly. Only the door with the cross leads to a food reward. After the cat has learned the problem (regularly picks whichever door has the cross), one can test the left eye with the right eye covered; the split-brain cat has to learn all over again, that is, it starts at 50% (chance). For each cat the learning curve for the second eye (and second hemisphere) is very similar to the learning curve for the right eye.

Because a split-brain cat has to learn all over from the beginning with the second eye, the cat can be trained to pick the square instead of the cross when using the second eye. It then depends on which eye is open which choice the cat makes. Thus, each hemisphere has developed a different memory about what is correct. In other words, each hemisphere has its own semantic system (i.e., a system that gives meaning to symbols). That the two hemispheres could be so disparate, giving different, even opposite meanings to symbols (cross and square) may be surprising because the two thalami in a cat are quite tightly coupled anatomically. Because the anatomical coupling of thalami in a monkey is a bit less, one might expect a similar duality of mentation in split-brain monkeys. Monkeys also make better subjects because the monkey visual system is more similar to humans than cats, monkeys learn much faster, and monkeys also have a considerable capacity for fine finger manipulation. It turns out that split-brain monkeys show even more than cats a duality of mentation.

It is important to understand that the duality of minds seen after hemisphere disconnection is not an inference solely from a few clinical cases and a handful of surgical patients. Split-brain experiments have been carried out with many different species by hundreds of investigators around the world. They are virtually unanimous in concluding that each of the disconnected hemispheres can act independently of the other (Bogen, 1977).

About Bill Jenkins and His Operation

I first met Bill Jenkins in the summer of 1960 when he was brought to the ER in status epilepticus; I was the neurology resident then on call. The heterogeneity as well as the intractability and severity of his multicentric seizure disorder became clearer to me over the next months. Both in the clinic and in the hospital I witnessed psychomotor spells, sudden tonic falls, and unilateral jerking, as well as generalized convulsions. In late 1960, I wrote to Maitland Baldwin, then Chief of Neurosurgery at the NIH in Bethesda, Maryland. A few months later, Bill was admitted to the NIH epilepsy service where he spent 6 weeks. He was sent home in the spring of 1961, having been informed that there was no treatment, standard or innovative, available for his problem.

Bill and his wife Fern were then told of Van Wagenen's results, mainly with partial sections of the cerebral commissures. I suggested that a complete section might help. Their enthusiasm encouraged me to approach Phil, because of his experience with removal of callosal arteriovenous malformations. He suggested that we practice a half-dozen times in the morgue. By the end of the summer (during which I was again on the neurosurgery service), the procedure seemed reasonably in hand. My plea to Sperry was that this was going to be a unique opportunity to test a human with the knowledge from his cat and monkey experiments and that his direction of the research was essential. He pointed out that a student about to graduate from Dartmouth had spent the previous summer in the lab and would be eager to test a human. Mike Gazzaniga started his graduate study in September and was, as Sperry said, eager to test a human subject. He and I soon became friends, and planned together experiments to be done before and after the surgery. There was some delay before the operation, during which Bill underwent testing in Sperry's laboratory. During this delay we also had an opportunity to keep a reasonably complete record of Bill's many seizures.

It was during this period of preoperative testing that Bill said, "You know, even if it doesn't help my seizures, if you learn something it will be more worthwhile than anything I've been able to do for years." He was operated on in February, 1962. It seems to me in retrospect that, if there had been a research committee at our hospital whose multimember approval was required, the procedure would never have been done. At that time, a chief of service could make such a decision alone, which I expect was similar to the situation at the University of Rochester in the late 1930s.

From the start, our procedure included not only complete callosotomy (requiring two skull openings) but also section of the anterior commissure, accessed in most cases by entering the third ventricle through its roof. We chose to perform as complete a section as possible for two reasons: (1) monkeys undergoing this procedure were without neurological disability and participated well in demanding psychological testing, and (2) if a complete neocommissural section failed in this ideal case (an intelligent, personable individual with supportive family whose multicentric seizure disorder could hardly have been much worse), then we would be through. Fortunately, it succeeded.

The completeness of our procedures as subsequently confirmed by magnetic resonance imaging was attained without the use of the operating microscope (which I first used in 1970), the good light that the scope provides, the bipolar cautery, osmotic diuretics, modern neuroanesthesia, and a variety of instruments only subsequently available. This is a tribute to Vogel's operative skills, including his sense of tolerable retraction and his remarkable vision at the usual operating distances (he never did take up the microscope). And how impressive it is that Van Wagenen worked under even less auspicious conditions!

Our next major step was to do a callosotomy (and anterior commissurotomy) that spared the splenium, whose section we believed by then to be the main source of disconnection symptoms. Throughout the 1960s, Vogel and I had been approaching lesions in or near the third ventricle via lengthy incisions in the middle of the corpus callosum; these patients did not show the disconnection effects of the complete section. I became increasingly confident of this conclusion, having by then considerable practice in detecting the disconnection effects by bedside examination.

These clinical findings had been stimulated by and gave increasing support to the view that the negative results of Akelaitis were not solely attributable to his lack of appropriate testing techniques. His negative results seemed also ascribable in part to the incompleteness of many of Van Wagenen's callosotomies, often described as "nearly complete" or as involving all but the most posterior end of the corpus callosum. By 1968, these considerations led to the expectation that section sparing the splenium could avoid most of the disconnection syndrome while at the same time ameliorating seizures having a rostral origin. Specifically relevant were complex partial seizures involving both anteromedial temporal regions, without generalization to the entire cerebrum when the patient was adequately medicated.

In 1968 and 1969, we operated on two patients whose seizures caused life-threatening psychomotor behavior and whose bitemporal foci appeared to be independent. Their seizure disorders were markedly improved (one subsequently obtained a steady job for the first time), and they had no discernible disconnection symptoms. In the words of Wada, this report ". . . revitalized our interest in re-examining brain bisection as a possible new treatment modality . . ."

By now, sparing both splenium and anterior commissure has become commonplace, particularly because a section restricted to the anterior two thirds to three fourths of the corpus callosum can alleviate drop attacks, and drop attacks are in the opinion of many the prime indication for callosotomy. That drop attacks could be eliminated by callosotomy never occurred to us, even by 1974 when we summarized our criteria for operation. This was in spite of the fact that the commissurotomy eliminated Bill Jenkins' drop attacks as well as his generalized convulsions (except for two occasions in 10 years). We were still influenced to some extent by the concept of "centrencephalic seizures," our theoretical views preventing us from recognizing a fact in front of our eyes.

The idea that extremely rapid generalization of seizures required a centrencephalon weighed even more heavily with others than with us and was probably responsible in part for the disbelief with which our reports were received. In addition, our work was done at a medical school (Loma Linda University), better known in those days for training medical missionaries than for scientific advances.

Not only was the procedure at odds with a well-known theory, it was worse! Had not this approach already been tried and failed? When I wrote to Frank Smith, then Chief of Neurosurgery at the University of Rochester, asking for as much information as he could provide, his reply was quite short, including that, "Dr. Van Wagenen always was sorry about what he did to those patients." For over a decade there were persons in Boston who referred to us as "the West Coast butchers." Without the excellent work of Wilson and Reeves, it is quite likely that our efforts (as well as of others, like Wada) would not have been widely accepted.

History teaches us much. Among other things, we see that a conception can repeatedly arise and be fashionable only to lose acceptance again in the face of reactive criticism, although in some cases sufficient support eventually accumulates so that the idea will survive somewhat longer with each reincarnation. Even so, we know that no matter how useful a therapeutic technique is, the odds are high that it will eventually be outmoded. Meanwhile, however, callosotomy has illustrated to a notable degree the interplay among social, scientific, and clinical concerns.

Block Design

Having standardized psychological testing on Bill Jenkins (as well as for subsequent split-brain patients) seemed obviously desirable to me. This required finding some psychologists accustomed to giving standardized tests, and some way to pay them. Vogel shrugged off the idea. I supposed it was a result of his having learned surgery at a time when, if a brain-operated patient left the hospital speaking sensibly and walking without assistance, the operation was a success. Even Sperry shrugged: what theoretical preconception would be falsified? His interest in "useful information" can be illuminated by the time I returned from a meeting, finding him eager to hear what had transpired. I had been going on for about 5 minutes, when he asked, "Was there anything that would change how we look at things?" By this time I had read almost all of his writing. "Well, I think not." He shrugged and was no longer interested in the report.

After Bill had recovered from his surgical ordeal (and was feeling better), he was eager to participate in some laboratory experiments. After some months a helpful social worker got in touch with a psychologist who occasionally tested clinic patients. She arranged some funding and he agreed to meet. He seemed to me not only quite elderly but actually quite infirm. I explained the patient and how interesting it was. I asked him, "Do you give the standard tests?" "Oh yes, the Wechsler." I didn't know much about the test, and neither did Mike, and he reluctantly agreed after some argumentation.

"Old Daddy Edwards" as I learned he was sometimes called around the hospital, acceded to our request. He sat across from a card table (part of his equipment) from Bill. Mike and I sat on the other two sides watching. The testing went along for an hour or so, somewhat tediously from our point of view, until Dr. Edwards pulled out the Block Design subtest. Bill pushed the blocks around somewhat ineffectually. Meanwhile Edwards was timing in his usual fashion and ended up with a zero score. I suggested that he use one hand at a time. Dr. Edwards objected because it was customary for subjects to use both hands. However, he was persuaded to try this momentarily, so we asked Bill to use only his right hand only while sitting on his left hand. Then we asked him to do it with his left hand. He had considerable success. Mike and I looked at each other as if we had caught a glimpse of the Holy Grail. "Now try it with just your left hand," I asked. He was quite successful! "Now try the next pattern." With his left hand he did the next one quite quickly.

"No!" Edwards said, "He is supposed use both hands." It was getting a little tense, because he insisted on doing it the standard way and we were anxious to further pursue our Grail. Dr. Edwards quietly prevailed and he finished the tests. We thanked him, and he replied, "Yes, it was interesting. We should test 20 or 30 more of these epileptic patients with various lesions." So this fumbling with the two hands seemed to be an example of what Akelaitis called "diagnostic dyspraxia" and which we had subsequently termed "intermanual conflict."

We realized Edwards was in the dark as to what had happened and what sort of patient Bill was, or why we were so wreathed in smiles. My next move was to borrow a set of these Wechsler blocks (one needed a license to buy them), and then I eventually obtained a set of the Kohs colored blocks. We retested Bill, and sure enough he had the same discrepancy between the left hand doing well, and the right hand doing poorly, and this persisted for at least 2 years. When we showed the data to Sperry he commented in his usual soft, skeptical way, "Well, I guess you boys have got that fellow pretty well trained by now." It was true that not all patients showed this discrepancy. The second patient did not show this discrepancy, being actually rather poor at the test with either hand. However, occasional patients did show the discrepancy (Bogen, 1987).

The variation in this result contrasts greatly with those features found with great regularity among the patients, for instance the inability to name or describe an object in the left hand (with vision obscured), in spite of the fact that the object can be readily retrieved by feel from a collection. That is, there was excellent tactile, same-hand retrieval. This inability we called anomia, although it is greater than that because the deficit also involves a failure of description. Some of the brighter patients eventually learned to use minimal cues, such as temperature or a pain-producing feature of an object, in order to guess at its name.

Intermanual Conflict

Intermanual conflict has been noted in split-brain patients by a number of people, and when one of the patients first told me about it I just could not tell anybody else because I did not think they would believe it. He and his wife came to the office and I said, "How are things?" He said, "All right except I'm having a little bit of trouble with my left hand." I said, "How's that?" He said, "Well, I picked up the paper to read it and my left hand took the paper away and set it down. So I picked it up again and my left hand came up and set it down again. So I picked it up and this time the left hand came up and picked up the paper and threw it on the floor." I never reported that because it was just too exotic. But better than that he came to see me in the office another time. He was operated in February and this was sometime in the summer. The patient told me he was pretty good because he had gone to the ball game and this was something he had not done in years because of the convulsions he was having so often. He enjoyed the ball game and went shopping with his wife. Nothing seemed unusual, and it was good that he was able to do those things. But then he said, "Well, what was unusual was she bought some licorice, which I don't like, and we had this shopping bag between us on the way home and my left hand reached in the bag and pulled out the licorice, which I don't like." I said, "Well, what happened?" He said, "The left hand brought it up to my mouth so I ate it but I didn't like it." You cannot put that in a professional journal but it was a true story.

There are a lot of these stories about intermanual conflict. The one I did put in a paper was about when the occupational therapist came to me and said, "You should have seen Rocky yesterday. He was buttoning up his shirt with one hand and the other hand was coming right up undoing the buttons right behind it." Akelaitis noted it in his split-brain patients in 1939. He saw some of that and called it diagnostic dyspraxia. Intermanual conflict you can understand. But what if you put a person's left hand behind his back and then he cannot tell with his right hand whether he is feeling his own left hand or whether he is feeling your left hand? He cannot tell the difference. That is where the whole idea of "alien hand" came in.

We also wanted to see if patients had a lack of transfer like the monkeys. So Mike Gazzaniga fixed up two handles under the table where the patient could not see them. There was a smooth one and a rough one. The idea was that the patient would be rewarded if he pulled on one and would hear some kind of noise if he pulled the other one. Then as soon as he learned with one hand, we would test him with the other hand and see if he learned from the beginning—the same way a split-brain cat would have to learn from the beginning if you switched hemispheres. The question was how to reward him. So I asked him, "What would you really like to be rewarded with?" He said, "A chocolate malt." I said, "OK, come to the lab hungry. Don't eat breakfast. We'll have a chocolate malt there and we'll give you a sip of chocolate malt every time you get the right answer." So we had the chocolate malt there. He pulls the wrong handle and gets the noise then he pulls the other handle and gets a sip of chocolate malt. He just keeps pulling the proper handle and getting the malt. One trial was all it took. It would take a monkey days to figure out what was going on. With a human being one trial was enough. It was obvious if we wanted to do some kind of testing that we could not do it with the monkeys now. We had to do something else. Then a lot of things came after that. In the beginning Sperry was not that interested. He just thought he would let me and Gazzaniga do it. But it became apparent to Sperry after the second patient that anything you could do with a monkey you could do a lot faster with human beings. He got a lot more interested.

Mike Gazzaniga was a good friend of mine when we started out. But eventually I developed a bunch of negative feelings about him because I think he just kind of muddied everything up for everybody. He keeps changing what he says. I wrote him one time and said, "I am going to criticize some of your views at the Neuroscience meeting, you may want to show up." He wrote back that he had another meeting elsewhere. However, he said, his views were evolving. I would say they revolve from year to year. People have asked me, "Do you agree with Gazzaniga?" When they ask me now my answer is, "Which?"

At the same time that the Split Brain Project was beginning, Joe also began his medical practice. He was not sure how to proceed until he met

Walter Holleran, the Medical Director at one of the first HMOs (a term not used until much later), The Ross-Loos Medical Group.

Going to Work

Walter Holleran was a lovely man, more than personable, with the engaging good humor often attributed to the Irish. He had another quality, often also attributed. He drank heavily, which eventually resulted in disaster. Walter valued the intellect and readily appreciated my desire to devote time to research. He understood my desire to avoid certain kinds of neurosurgery, including infants. "Charlie Carton has been doing that for us." Another was stereotaxy for Parkinsonism. "You are right that doing one or two cases a year of a specialized nature is inconsistent with good practice, especially, if as you say, Ed Todd is readily available." In fact, we referred only three or four such cases in the 18 years that I was at Ross-Loos, largely due to the introduction of dopamine agonists only a few years after I started in practice in January 1964. A third area that I had hoped to avoid was operating on carotid artery obstructions. It had never seemed like neurosurgery to me. And I was not very comfortable clamping off the blood supply to somebody's brain. I was quite aware of the territorial dispute between neurosurgeons and vascular surgeons, both claiming to do carotid surgery. What I was not aware of was what Walter said next, "Sure and you are correct. Did you know that I am a vascular surgeon?" So our discussion went swimmingly with me getting about one third of the usual surgery and Ross-Loos getting what amounted to two thirds of my time. Everybody was happy with this arrangement, with the possible exception of my wife.

Gerstmann Syndrome? Taking the Board Exams

The time came for me to take the board exams in Chicago. When I took it you only had to pass it once. If you do not pass the board exam, it does not keep you from practicing because you can get a state license but it is almost essential if you are going to be a specialist of any kind. You have to practice for 2 years before you can take the boards. That's the way it was then. So after you practice neurosurgery, in my case for 2 years, then you hand in all your cases. Every case has to be summarized, a story describing the case and what was the matter and what you did and how it turned out. All that has to be handed in, 2 years worth when you take the exam, and they look at that. They had just introduced the written exam at that time. I think it was one of the first years, so they did not give it much weight. Now the written exam has much more weight than it had in 1966 when I took the board exam.

I figured I knew what one examiner meant when he asked about the language areas of the brain, so I said, "Well, there's a focus area." He said, "Well, where is it?" I said, "It's right there, the posterior third of the anterior frontal gyrus." He said, "All right, what else?" I said, "Well, there's Wernicke's area." I later wrote a paper called "Wernicke's Region—Where Is It?" and I got 22 different answers from the literature (Bogen and Bogen, 1976). But I was not going to argue. I said, "Well, it's about here you see." I waved my finger around. The posterior third of the superior temporal gyrus wrapped around in there, you see. Although of course, a lot of people think the supramarginal gyrus is more important, on and on. Anyway, he said, "Well, what else?" I said, "You know Penfield claims that there's a supplementary speech area." He said, "What do you mean claims?" I could hardly keep from laughing. He said, "You don't think it's true?" I said, "Well, it's probably as true as most things." I didn't know how I was going to be graded in that exam. The way they had it set up was in each section was that there was a neurosurgeon and a specialist in that field. So for the neuroanatomy they had a neurosurgeon asking questions together with an anatomist.

Then for neurology there was a neurologist that I had never heard of. He was some local neurologist they brought in. But I had heard of the neurosurgeon in the exam and I heard he was really a bear. So I came in and they were very polite at first, "Sit down doctor, we're very glad to see you." And then there was a little silence and he looked at me and said, "What would be the first sign of a left parietal tumor?" I didn't give him the smart-ass answer which would have been "You mean in the right handed?" because I knew that was what he meant. I said, "Well, the first sign might be a seizure, a generalized seizure." The reason I said that was because I had a patient just like that who had a left parietal tumor and the way the case presented was with a convulsion. And I knew they had my cases so I thought he was talking about that. He said, "I don't mean that. I mean a neurologic problem. What would be the first sign?" I said, "Well, the first sign of a left parietal tumor might very well be some mild, vaguely describable difficulty in thinking. The person would have a hard time characterizing." He gave me a really hard stare and said, "You're from Los Angeles?" I said, "Yes sir." He said, "Did you ever hear of Jan Neilsen?" I said, "Yes sir, he was my first professor of neurology." He said, "Well, didn't he teach you about Gerstmann syndrome?" I said, "Yes sir, he did." He said, "What is it?" I said, "Well, it's agraphia, acalculia, finger agnosia, and right-left disorientation." He said, "Well, why didn't you say so in the first place?" And at that point, the neurologist who had been sitting there smiling through this whole thing spoke up and said, "Maybe the doctor doesn't believe in it." I said, "That's right." The guy just about blew a gasket. It really was funny.

Now the reason for not believing it is that they do not necessarily have to go together. There are plenty of people who have agraphia and alexia as a sign of an angular gyrus lesion. And that is the main thing they have got. As you know they can also have alexia without agraphia, but it is much more common to have agraphia and alexia together. Dejerine pointed that out in 1890. But what Neilsen always emphasized was that if the lesion is at all deep in the angular gyrus, you are going to get a lower quadrantanopia, maybe even a hemianopia because it gets down into the optic radiations. The optic radiation goes right underneath there. The angular gyrus is around the end of the superior temporal fissure. Draw a superior temporal fissure and the gyrus around that is the angular gyrus. If you draw the Rolandic fissure back the gyrus around that is the supramarginal gyrus. So if the lesion is in the angular gyrus, it is back a little further. In fact that would be the location if you had a bilateral, balance syndrome. A little further forward in the supramarginal gyrus, if it is unilateral, you would get hemineglect. If you get it bilaterally in the supramarginal, you do not get hemineglect. So hemianopia is frequently an accompaniment to agraphia or alexia or acalculia.

Arthur Benton is one of the great figures in neuropsychology. He showed that you can get any combination you want of agraphia, acalculia, and alexia. And it is an individually variable thing, and it just depends on the extent of the lesion. Now it is true that if you see somebody who has agraphia and acalculia and finger agnosia and right-left disorientation, that tells you exactly where the lesion is. That is true. It is in the left angular gyrus. There is probably hardly any such thing as an exception. But that combination does not very often occur. It is a rare thing. It is much more common for people to get alexia along with the agraphia. And it is fairly common for people to have acalculia without finger agnosia. Gerstmann had a whole theory that he did not think that these things went together just by the coincidence of where they are represented in the brain. He thought that, in fact, they told you something about how people learned to calculate. They start with their fingers, so having finger agnosia explained why they have got acalculia. Well, that is not the case, because people have acalculia without finger agnosia all the time. So the whole Gerstmann theorizing is what I did not believe. And the idea that those four things are fairly commonly together is not so accurate. It is much more common to see agraphia together with alexia, and if the lesion is a little deeper, hemianopia.

Joe left Chicago not knowing whether or not he had passed he boards. However, he looked forward to meeting Aaron Smith in Omaha, a trip he had planned to take on the way home. He began a long friendship and correspondence with Smith, which he intended to describe further here, but this was never completed.

It was about a month after the Board exam and trip to Omaha that I received a letter from Professor Guy Odom at Duke University and Secretary of the Board. It read, "We are happy to inform you that you have passed the Board Exam. We wish to emphasize that you did poorly on the radiology subset of the exams and should study those areas."

Summer of 1969

I got a telephone call from Roger Sperry and he said, "How would you like to go to New York and give a talk?" I said, "Sure, what's that all about?" He said, "Well, maybe you've heard there's an International Neurological Congress. They have them every four years. It's a big deal. So they've got it at the biggest hotel they could find, the New York Hilton. There's a man named Derek Denny-Brown, a professor at Harvard, and neurologist-in-chief at the Boston City Hospital and various other things, a political power in neurology." Derek Denny-Brown was very strongly opposed to the idea of complementary hemispheric specialization. And whenever someone would come up with some evidence from lesions that a right hemisphere was special in some way, he would come up with some kind of argument to show that it had been misinterpreted. But in spite of his strong feelings on the subject, he was apparently a man of open mind because in this International Congress he decided to put on a plenary symposium for everybody on cerebral dominance. It turns out that he invited Oliver Zangwill, Henri Hecaen, Wilder Penfield, Brenda Milner, and Roger Sperry. He asked Roger Sperry to bring Gazzaniga along but by this time he and Gazzaniga were not on speaking terms so he said, "How about if I take you along?" I said, "Sounds good to me." I was kind of amused by this whole thing anyway. I did not realize what a sterling, major-league cast was involved until I got there but I was pretty impressed with the whole thing. So I worked very hard on a 20-minute talk. I rehearsed it repeatedly. When I found out who was on the program I was not sure who was going to precede me. But since I knew all these people and I knew what they all had to say, I made up about a half-dozen ad libs that I would throw in just in case they were appropriate, depending where I came into the program. In fact, I actually used two or three of them. It appeared as if I was very clever, but the fact is I had worked very hard dreaming these up the night before. I rehearsed and spent most of the night before working on this talk.

There was a lot going on at this meeting, and there were various specialized meetings running in parallel all over the hotel. I was going to some of them, and I was walking down the hall to the elevator and there stood two icons of neurology, Edwin Weinstein and a man named Eberhart Bay from Germany. These two guys were particularly prominent, even more prominent than Derek Denny-Brown, in urging a sort of holistic view and an anti-localizationist view which for a lot of people also meant an anti-hemispheric specialization view, which is a mistake but that was the way they felt. I saw these two guys whom I had heard about a lot, and I recognized who they were but they did not know me. I stood there for a while as they were talking and then they stopped and they turned to see what I was doing. And they said, "Yes?" I said, "Well, I wanted to ask you a question being that you are world-famous experts." One said, "Well, what's the question?" I said, "My question is that since most people agree that the left hemisphere in most humans, right handers at least, is dominant for language is there any *a priori* reason why the right hemisphere might not be dominant for something else?" Professor Bay being a little older and from across the ocean and taller besides, answered first. He said, "No, there is no *a priori* reason but nobody has produced any really persuasive evidence in my opinion that such a thing exists." Dr. Weinstein spoke up and said, "Your *a priori* reason ... you don't understand how the brain works. The brain works as a whole. You don't have these compartments and so on." I said, "Well, language is in the left hemisphere, maybe the right hemisphere is good for something else like nature, or music or I don't know..." At that point he said this wonderful line (which I should have had a recorder but I did not), "In my experience the people who best appreciate nature are the people who can talk about it the best." And at that point it was clear to me that a conversation could not proceed any further in a constructive fashion.

The next day came the symposium and it was pretty impressive because Zangwill rolled out all this lateralized lesion data and then came Brenda Milner and Roger Sperry. Penfield was getting a little old and kind of wandered around a little. Then Henri Hecaen was supposed to give a 20-minute talk, but it was in French and it lasted at least 45 minutes and it just went on forever. But these 4000 neurologists in this huge ballroom stayed and paid attention. It was a good thing my talk was so well rehearsed because I had never addressed that large an audience before. My feet were a little colder even than they are now with my peripheral neuropathy. But I had rehearsed it so often that the whole thing rolled off fairly well. And Sperry was quite flattering afterwards. I said, "Penfield gave a pretty good talk." He said, "Well, he's not alert, it was all old stuff and he wasn't really tuned in to the rest of the speakers like you were." I thought that was flattering.

The main point was that after that symposium, I do not think there were any serious objections among neurologists to the idea of hemispheric specialization. It was as if it had just turned over that one afternoon. I never met Professor Bay again but I met Ed Weinstein a few times and subsequently he was very friendly and he was no longer offering objections to the idea of hemispheric specialization. Of course that kind of data from lateralized lesions, that is loss of function when a lesion is in one hemisphere or the other, and the split-brain data have now been kind of overshadowed or superseded by imaging. Because instead of inferring function from loss or instead of having this highly specialized situation with the split brain, you can take normal people with positron emission tomography (PET) scanning or functional magnetic resonance imaging (fMRI) and you can see the hemispheric specialization. It is pretty dramatic in pictures. That, of course, has made the whole story almost unarguable. Anyway, that was interesting, a paradigmatic shift overnight.

Hemispheric Specialization

A small example of the application of the complementary hemispheric specialization idea involved a friend of mine, Gabriele Rico. Ms. Rico obtained a Ph.D. at Stanford University in 1979 with her thesis describing her highly successful method for teaching composition to backward English students. The method emphasized the linking on paper of clouds of visual images. She included as a rationale for her method the emerging theories of right brain/left brain function: She had asked that I be a member of her doctoral committee to serve as a resource with respect to this rationale. "Clustering," as the process she developed is called, is today in university composition texts as well as elementary and high school texts and has been turned into a software program for use in schools.

Subsequently Gabriele wrote a small book entitled *Balancing the Hemispheres.* There was an impressive demand for this booklet, but the publisher was reluctant to print more copies because of an article by Professor Curtis Hardyck debunking the right brain/left brain story. At Gabriele's request I wrote a letter in rebuttal of Professor Hardyck's critique. Gabriele subsequently wrote that because of my letter, the book was reprinted. Indeed it continues to be in print over 2 decades later. Her trade book, *Writing the Natural Way,* based on split-brain research, is still in print 23 years after publication. There has been widespread acceptance of the basic ideas not only by the general public but also by the scientific community as evidenced by the advertisement in *Science,* placed by the American Association for the Advancement of Science, to request submissions for their contest entitled Science and Engineering Visualization Challenge, with the slogan, "It takes both sides of the brain." (See *Science* October 2004, p. 610 and October 15, 2004, p. 508.)

During the past 20 to 30 years (a full generation), the scientific community has come to accept as a well-established fact what was called complementary hemispheric specialization. Scientists discussing the evidence, as well as its implications, have generally spoken in probabilistic terms of continuities, of relative rather than absolute differences. But the media have emphasized polarities, have ignored reservations, and have simplified their accounts for a general public whose capacity for understanding science is apparently believed by most media folk to be even less than their own. As a result the general public acquires a view that is not only simplified and sensational but, especially in television, distorted and degraded, in a word, vulgarized.

In Between

All the time that I was at Ross-Loos, Walter Holleran was supportive of our special work arrangement. It was only when he became quite ill and eventually died that this changed. What happened to Walter could be considered comic if it had not been so tragic. Walter loved rumaki (appetizers held together by toothpicks) as much as he loved alcohol. Indulging in both one night at a party he was apparently too hasty. The next day he was ill and was eventually diagnosed with peritonitis, and at operation it was found that a toothpick had perforated his bowel. He was hospitalized for months and never fully recovered. The arrangement I had made with Walter disappeared entirely when Ross-Loos was swallowed up by Cigna. This was perhaps best illustrated by the occasion when a Cigna executive happened to look through the open door of my office. Seeing me sitting at my desk, pencil in one hand as I gazed off into space, he asked "What are you doing?"

> "I am working on a paper."
> "A paper? What kind of paper?"
> "A scientific paper—you know, for a journal."
> "Well! We don't work on scientific papers around here. We do patient care. Does Dr. B— know about this?"
> "Why don't you go tell him," I snarled.

My situation at Ross-Loos had been steadily deteriorating, especially when it developed that if I ordered a scan of somebody's head, it had to be approved by somebody on the "8th floor." This seemed to me outrageous, and it became obvious that I had to leave Ross-Loos. Then what happened was that they were getting ready to shut down Professor Vogel's residency program at the White Memorial Hospital. I said, "Look, you're the professor, but why don't you give up being the director of the residency program and give it to 'what's his name' who is a good surgeon and let him be the director." I do not think he said anything. He just shook his head. Now the guy I suggested he should make the director was an African American guy, and a smart guy who would have jumped at the chance. He would have been one of the very few program directors who was black. But Vogel would not do that. But he was from the old school, and he felt he knew what he was doing and that he was doing things right, which in my opinion he was not. Of course there is the possibility that they would have shut down the residency no matter what because the school, Loma Linda Medical School, had moved out to Loma Linda. That had been the teaching hospital and they were in the process of getting rid of the training programs that were not part of universities. Anyway I was a little upset that he would not change anything in order to continue the program.

It was about this time that Ted Kurze asked me how many patients I was carrying usually. I said, "Anywhere from six to ten. I hardly every get more than twelve. I usually have around six to eight in the hospital at a time." He said, "Why don't you bring them up to Huntington." So Kurze wanted me to come to the Huntington Hospital and I jumped at the chance because I could see that the residency was dying down there at White. Vogel was not going to change anything. And this was 10 minutes from my home. I thought this was terrific. It is still a little country clubbish but it was even more so in those days. That was over 25 years ago. Well, what I found out eventually working at the Huntington is that it is not anything like the White Memorial Hospital where everybody tries to be civil as possible and never yell. Here if you want to get something done you have got to scream. They just will not do it right unless you yell at them. Because they really do not think you are sincere unless you yell. It was a great disappointment to me. Actually I only had to do that a couple times. It was very hard for me to change the whole style of dealing with people. There were people who just would not do what you expected them to do unless you raised your voice and then they knew you meant what you said.

Joe left Ross-Loos in 1981, returning after 18 years to a small office at the White Memorial Hospital, where he saw patients and assisted other neurosurgeons at area hospitals including the Huntington Hospital in Pasadena. He joined the New Hope Pain Clinic and worked there until 1986 when he discontinued doing much surgery to concentrate on his writing and speaking engagements. His income during this time, for the most part, came from expert testimony and review of malpractice cases.

One project he worked on in particular was the republication of the book A New View of Insanity: the Duality of the Mind, by A.L. Wigan, 1844. Joe considered Wigan his personal hero, calling himself the founder of "Neo-Wiganism," (Bogen, 1971), and having his own personalized auto license plate that read "4Wigan." Having met publisher Joe Simon, he introduced him to Wigan's work and together they researched and designed, and Bogen financed the reissue of the "nearly extinct" work of Arthur Ladbroke Wigan.

Memory

Jun Wada had a wild idea, which he first introduced in Japan. When he got to Montreal, Ted Rasmussen agreed that it was something that they ought to do. The problem was knowing which hemisphere a person is talking with. If one does a temporal lobectomy, we take off less if it is the left temporal lobe than if it were the right temporal lobe. That is a longstanding idea. We measure back from the tip of the temporal lobe, say, 5 cm on the left and 7 cm on the right, because on the right you can afford to take off more and have a lesser chance of impairing the person's language. Sometimes the right hemisphere function can be just as important as the left hemisphere function but it is not nearly so obvious. For most people to earn a living they have to be able to have pretty good language. However, it turns out that it can also be hard to get along with a damaged right hemisphere.

Okay, so, we would like to know what the language hemisphere is, and that is how Rasmussen decided that the Wada procedure, as it has been called ever since, was something worth trying. What you do is shoot sodium Amytal up one carotid artery. It goes into the left hemisphere on that side, blanks out the hemisphere, and paralyzes the contralateral arm and leg for 4 to 5 minutes. During that time if you talk to the person and carry on a nice conversation while that hemisphere is narcotized, and the person keeps carrying on a reasonable conversation, then you know it is not the language hemisphere. Then you do it with the other side and usually find that they do not talk anymore, so that is the language hemisphere.

Then came another reason to do Amytal studies, which is to test memory. It is well known that if you lose both temporal lobes you are in a fix because you will be left with severe amnesia. You can even develop Klüver-Bucy syndrome, which is pretty horrible. Nowadays it mostly happens to people from getting herpes encephalitis. People with bilateral temporal lobe damage do not have any learning ability left; it is called anterograde memory loss. They still have some old memories and they may have immediate memory but they do not have the kind of memory you need to learn anything new. If one (lobe) is so badly damaged that the person cannot remember anything with that one, then he or she certainly needs the other one, and you do not want to take that one off, although that has happened. Sometimes only one temporal lobe was taken off and the person still ended up with a bilateral defect because the unoperated (lobe) was scarred up. So you do the sodium Amytal test not only to find out which hemisphere the person is talking with. You also find out if a patient can learn things when one hemisphere is not working. You can do that with the hemisphere that has the language and also with the other one. You show material to the patient and then you ask the patient to pick it out later on. If he or she can remember what was shown, then you figure the person has got the ability to store the facts away. That means the hippocampus on the unnarcotized side is still in good enough shape, and you can afford to take the other one off it you have to. Then you would test the other side just to make sure of what is going on.

One thing we did was we wanted to find out which hemisphere they would sing with, which nobody else ever did. So we would inject one hemisphere (usually the right), and they would keep talking. For example we would say, "Say (a sentence) with the word hospital" and the guy would say, "I'd like to get out of this hospital." The way we did this then was a bit gross. The patient got these big needles stuck in his neck. It is done in the groin now, but then there was a big needle stuck in the carotid artery, which was bobbing up and down with the pulse rate in the guy's neck. Then we said, "Sing with me" and we would sing happy birthday. The guy would join in singing "happy birthday to you, happy birthday dear doctor, happy birthday" (monotone). No melody! And when we would inject the other side, he could not talk. You would say, "Hold up your hand," and he did not understand what you were talking about. You would look at him and sing "happy birthday to you" and the guy would join in singing, da, da, da, da and *carry a tune* with the nonspeaking hemisphere! Sometimes they would sing the words because the words kind of come automatically. It is like an aphasic who can sometimes sing or utter overlearned phrases like "Hail Mary full of grace the Lord is with thee." If you have some kind of overrehearsed stuff it will come out just like song lyrics but it is not really language. You are not creating a new sentence with, for example, "happy birthday" in it. Creating some kind of novel sentence or understanding a complex sentence, that is language.

Some people thought our Amytal paper was quite important. I remember Frank Benson went around saying how great it was. These days everybody is familiar with the right brain/left brain idea showing that a person can sing when they cannot talk and when they can talk they cannot carry a melody so well. So that is not such big news anymore, as when we published this paper, about 1965 or 1966.

Heilman and Valenstein wanted to put out a book on behavioral neurology called *Clinical Neuropsychology,* and they asked me to write a chapter about the corpus callosum. That was in 1979. It had some mistakes in it, but it took me years to figure out what was wrong: meanwhile other people read it and quoted it so that the error has been passed on for generations of students, and I was the guy who started the whole stupidity. Anyway, it is the term "alien hand," which I introduced and which is wrong.

Eventually they wanted to put out a second edition, which did appear in 1985. I said, "Okay, I'll update my chapter" and I spent a lot of time and worked hard on it. Then I got the galley proofs back in 1984, and Valenstein had taken out big portions of what I wrote. I had written a big thing about faces, and he took that out. Well, that could be justified because the same subject, facial recognition, was discussed by someone else in another chapter. But still, I thought he should have talked to me about it, not just taken it out without saying anything. He took out some other things for reasons I could not understand. Perhaps he thought he knew more about some things than I do. So finally I decided, all right, I will sign the agreement, I will send the galley proof back, and then I took the material on faces and put it in a book edited by Benson and Zaidel.

The second edition in 1985 was a big success. Then came 1991 and Valenstein calls me up again. He says, "We're going to do a third edition. Would you update your chapter?" I said, "Yes, Ed, I will update the chapter with the understanding that if you don't like something about it we're going to talk about it. You know, you're not going to take stuff out without talking about it." He said, "No problem." One reason that I was willing to go to all the work and effort to bring the thing up to date was because I wanted to chide Mike Gazzaniga for giving these different opinions. Every year he would come out and say something a little different from last year. I said to myself, "How about some ongoing truth? How about a little stability." But his style was that every time you write a paper you give "our new interpretation based on our recent new evidence." He would not explain why differing views were wrong, nor would he mention that he had said something differently the year before. In fact, in 1985 he came out with two opinions that were mutually inconsistent in two different journals in the same year. I decided to put all of this in my chapter. I thought this chapter was going to be my swan song because by this time I had already had kidney cancer in 1987 and found out that I had diabetes, and also my blood pressure was a little elevated. So I figured this was my last chance to put it all in. I also wanted to blast Marcel Kinsbourne, who is a long-time friend of mine, because he wrote a chapter in which on one page he said that we cannot depend on introspection to tell us what the situation is, although it may give us some clues, and then five pages later he says the objective evidence would suggest that you can have two minds inside of one head with the two hemispheres but our unquestioning understanding of the unity of self would be against that. I said, "Wait a minute, a few pages ago he was against introspection. Now this introspective feeling that we're one unified person means all the other evidence should be put aside?" So I wrote about this. I was unhappy back then because my position for more than 30 years has been that the objective evidence is that everybody has got two minds in the usual sense of mind, not one. No matter how unified you think you are, and how unitary everybody says they are, they are not. Because that is an "introspection" and it is no more reliable than a hundred other introspections, which have already been disproved.

So I got a big long letter from Ed Valenstein—three pages. The first two pages had a lot of pretty good suggestions, I thought. The third page of his letter attempted to get me to take out all of the stuff about Gazzaniga and Kinsbourne, which was my main motivation for writing in the first place. He says, "We should not deal with personalities" so I wrote back thanking him for all of his good suggestions in the first two pages and then I said this is not a matter of personalities. I am not the least bit critical of their personalities. In fact, as far as I am concerned they both have delightful personalities. I have enjoyed their company on many occasions. Charming guys, both of them. What I am complaining about is what they have written not their personalities. He did not answer the letter. Then I got the galley proof. He took it out! Another thing he took out was a long paragraph with the definitions of the words that were going to be used through the rest of the chapter. He took out the definitions! What's the sense in that? Because he thought that anyone who would read the book should know what those words mean?

Norman Geschwind was Kenny Heilman's teacher. So Kenny Heilman believes in Geschwind and he passes that on to people including Valenstein. But a lot of stuff that Norman said he never believed himself. I know that because I knew Norman. Furthermore, a lot of what he said turns out to be wrong. This is because Norm would turn out a couple of bright new ideas every day for lunch. If you keep coming up with a bright new idea everyday, they are not all going to be right. However, he would teach it to people, sometimes even after it was shown to be wrong. Like his claim that what makes humans different from chimpanzees is the angular gyrus, and it is the angular gyrus that gives humans cross-modal associations. If a human learns to tell things apart, say, squares and triangles by sight, then he can tell them by feel in the dark. Ettlinger claimed monkeys cannot do that. Well, it is not true. At the very time he was saying that, Davenport showed that monkeys could do it. Then, later on, Weiskrantz and Cowey showed that monkeys could do it. So the whole beautiful theory came tumbling down.

So it was while I was having this big surprise looking at the galleys that I began to have nose bleeds. I thought my blood pressure might be elevated and asked my wife to measure it right then and it was 220/110. It was at that moment that I decided that I was not going to have anything more to do with Ed Valenstein. I called up Jeff House at Oxford University Press of New York. Because it was the Thanksgiving Holiday, I left a voicemail saying that if they replaced what had been cut out by Valenstein and if I could have a page proof to reassure myself that the restoration had actually taken place, then I would send the revised galley proof to him. Otherwise, they should just forget my chapter, leave it out. The next Monday I received phone calls from New York and Florida (from Kenny Heilman) telling me that the chapter was essential for the book and I could have it the way I wanted. This is a nice happy ending except for the fact that I started having those nose bleeds and it chased my blood pressure higher than it had ever been before and that is a good reason never to write a chapter for somebody else's book!

Zaidel

Joe became good friends with Eran Zaidel when he and his wife Dahlia Zaidel were at Cal Tech working in Sperry's lab. They continued their association over the years. This story is about one of their later collaborations.

What happened was his grant application was turned down. This was about 1986 or 1985, maybe. So he said to me, "Well, I would like to make you a co-principal investigator, a Co-PI." He thought that would increase the chances of him getting the grant from NIH. I said, "Well, applications aren't my style of writing, but I tell you what, I've got to make some contribution for you to stick my name on it. So my contribution, besides just lending you my name, will be for me to rewrite the first page (which was the abstract). Then I don't care if you have 30, 40, 50 pages of garrulousness, so long as the first page is not embarrassingly wordy." This was the way I talked to him. I got away with it because he has got a tough hide. You know what a sabra is—it is a kind of cactus that grows in Israel. For years the people who were born in Israel called themselves Sabras. They named themselves after the cactus because it has such a leathery exterior, but it is nice and soft and tasty inside.

So I rewrote the first page and he put it in and they did not give the money right off. They wanted to have a site visit, so they sent a committee. One person came from New York City, a pompous ophthalmologist; then the neurology chairman from Tucson, Al Rubens, who I knew reasonably well; then some lady I had known about but I never saw before, a linguist from some place like Johns Hopkins; and then one more person I cannot remember. So I guess there were four of them and the fifth wheel was an administrative bureaucrat from NIH who did not know anything about the subject but he knew all about the money, or rules, or whatever. Now I did not think it had anything to do with me but I got a phone call from Zaidel that said, "You have to come out here. The site team is coming tomorrow and you've got to make a presentation." I said, "That was not part of the deal." He said, "Well, you've got to. You're co-investigator and they want to see you and hear what you have to say."

So, I went out there and heard a pitch that he made for how a certain line of investigation was going to produce interesting results and how another line they were following might be enormously illuminating if it turns out but it is not so clear. You do not know how things are going to turn out. Anyway, then it came to my turn. And I had thought about it a little bit so I stood and went to the head of the table and said, "Now these projects are promising, it seems to me, although of course there's a possible chance of some failure. But what I am proposing as my part of this is something that cannot possibly be a failure. That is that we have all these split-brain patients and they've had all these psychological investigations, literally hundreds of papers. And the anatomy is not really well known. The only reason we know that they are split is that all these psychologists have had to take my word for it. We've tried to get CAT scans but you can't see anything. What we have to do is MRIs and I'll tell you people very frankly that I am not the kind of guy who applies for grants very often. As a matter of fact, everything I've done I've done on my own money. I pay for my own trips to meetings, I buy my own slides, but getting MRIs is really more than I can manage. It's just too expensive for me to pay for. So we have to get some money to get MRIs to document the anatomy on these patients. One way or another we're going to get the anatomy, but it would be a lot nicer if we could do it legally and NIH got some of the credit, so it's done right." And Zaidel was absolutely ... he thought sure I had killed the whole project talking to these people that way. He got his grant and it has been renewed about five times since. I think it was 1985. So we got the money, and I took them out there to my favorite radiologist, and he did MRIs and we sent the bill to Zaidel on his grant and he paid. He did not pay the full price. They gave him a price break. Anyway, the radiologist got paid a fair amount, and he got his name on the paper. I called up Bob Joynt, who was the editor of the *Archives of Neurology* at that time. I said, "Bob, all these people have been pushing me really hard to get this anatomy thing on the split-brain patients so they can refer to it and say they're dealing with split-brain patients. If I send you a paper and keep it really short..." He said, "Yes, sure we can publish that. Just a few pages." So I said, "All right." So I sent him a three-page paper and it came out in 6 weeks.

I told my friend the plastic surgeon, "That's the fastest turnaround I ever saw." And he said, "Well, it must not be a very good journal." But I did not mind. People have been referring to it ever since. I got reprints, so I sent a reprint to each one of those people who was on the site team with a little note saying thank you and we did what we said. And I never heard back from any of them. Zaidel said I ought to benefit a little bit from this grant once in a while so he helped me get a computer. But the latest thing is that he hired this guy, Dave Kaiser, who was a student of mine 10 years ago, this computer person, because he needs that sort of expert help. And when I mentioned I was having trouble with my web page, he said, "Dave will do it." So tomorrow I am going to go there with a whole brief case full of stuff and Dave Kaiser, I think, is going to upload it into my web page. So it is going to be a lot bigger after David Kaiser gets done with it.

Consciousness

In the spring of 1994, there was a conference on consciousness at Claremont. More than half the speakers there were philosophers, and I became progressively fed up throughout the day with their arguing amongst themselves about their favorite intangibles. After dinner came the featured speaker of the evening, Ned Block. When he finished, there was, as usual, time for questions. I was sitting in the front row, so after raising my hand, I jumped up on the stage saying, "Do you mind?" I grabbed the podium. "If you really want to make some progress in this subject," I said, "let's start talking about how brains work. If you don't, you're just going to be flailing around here with a lot more unknowns than you have equations, and you're going to get nowhere..."

Half the people in the audience booed, and the other half clapped and said "Yeah, yeah!" It was really amazing. A little later they had a wine and cheese reception. Bernie Baars, the editor of *Consciousness and Cognition,* came up to me. "Would you write that up? We would like to publish that in our journal." The background for this was that beginning about 1990, I spent a lot of time trying to figure out just what do I believe about consciousness. So when Bernie asked me for a paper, I was ready. So I turned what I had already been writing for myself into a two-part paper for Bernie. We had a lot of long distance phone conversations. The article appeared in 1995. About 2 years later, Bernie called me on the phone. I had read his latest book by then and knew that he had used a lot of the stuff that I told him without saying where he got it. There was also the fact that his book gave Gazzaniga credit for thinking up the split-brain. He was complimenting me and thanking me, and he said, "You know, I was just reading your article a few days ago, the two-part paper in our journal, about consciousness..." I said, "Yeah." He said, "You know there's some pretty good stuff in there." This was 2 years after it appeared.

In the same conversation he went on to tell me his new theory. I said, "Bernie, don't tell me consciousness comes from prefrontal cortex. You may need prefrontal cortex to think ahead, or to be socially responsible, but you don't need it to be conscious."

He says, "Well, why don't you send us a paper that says that. We'll publish it in our journal."

I said, "Have you got the journal handy? Look in part two of that paper you published. Can you find it?"

"Of course, I have it right here."

"Well, open it up and look at page 147. Page 147 begins, "Human C does not require prefrontal cortex. I've written it for you already, Bernie." He said, "Well, maybe for people to notice it, it would have to be in a separate paper."

Gerald Edelman got a Nobel Prize for his work in immunology and now directs the Neuroscience Institute near San Diego. Edelman wrote several books about consciousness, one called, *The Remembered Present* in which he says, "You have to have language to be humanly conscious." Primitive consciousness other animals might have, but to be humanly conscious, he says you have got to have language. I wrote something about his views, and one of his minions said, "You just don't understand Gerald. When he says you have to have language to be conscious he means to have a fully developed consciousness." I wrote back, "Look, I knew Richard Feynman at Cal Tech. And after having met Feynman, it's my conviction that neither you nor I, nor anyone else we know, is going to have 'fully developed consciousness.' "

The problem is, of course, that one confuses the property of consciousness with its contents. If you are a dolphin, you are not going to have the same contents of consciousness as humans do. They are conscious of auditory information that humans are not conscious of. Once philosopher Dave Chalmers was invited to the Helmholtz Club. He came and like most philosophers he referred to Nagel's article, "What is it like to be a bat?" and to Nagel's characterization of consciousness as "being what it is like to be something." At the end of his talk I said, "Look, I read this article by Nagel and he said at least three things. The first thing he says is that consciousness is the reason we'll never solve the mind/brain problem. However, if you look at the second paragraph, he says, 'Consciousness is a property we share with many other species, although it's difficult to say exactly what it is.' Now that's the consciousness we are trying to understand. A third thing he says, and this is what everybody's referring to, is that the reason we'll never be able to figure out consciousness is because we'll never know what it is like to be a bat. Of course you'll never know. That doesn't mean we won't understand consciousness. I don't have the slightest idea what it's like to be my wife."

Well, the place cracked up. You cannot know fully what it is like to be somebody else, it is very hard. Bats may have a better chance of knowing what it is like to be a dolphin than we do because they are using echoes all of the time, like dolphins. People continue to confuse the contents of consciousness with the property of consciousness, which we share with a lot of other species. Francis (Crick) had the right idea, which is, if you want to understand consciousness, you first have to recognize not only that it is produced by the brain, but that this means you have got to learn neuroanatomy. You are not going to be able to think constructively about how brains work if you do not know the structure of it. How things work depends on how they are put together.

Going to Helmholtz

In 1995 Professor V. Ramachandran (Rama) invited me to give a talk at the Helmholtz Club. This was a group of neuroscientists having day-long meetings once a month at UC Irvine. It was organized in 1992 by Rama, Francis Crick, and Gordon Shaw, who were soon joined by fellows from UCLA (including Joaquin Fuster and the Schlags), USC (including Michael Arbib and Irv Biederman), and Cal Tech (including John Allman), as well as participants from UC San Diego and Salk. From 1986 onwards, Terry Sejnowski acted as chair, secretary, and treasurer although the group had no formal structure.

In my talk on hemispherectomy, I included Aaron Smith's movie of Earl Cozad stressing the presence of consciousness in a global aphasic and, more important, consciousness in someone with only one hemisphere. After the talk Pat Churchland asked me, "How come Gazzaniga never refers to hemispherectomy?"

"I don't know. Why don't you ask him?" I suggested. So far as I know, she never pursued the subject. In this connection she has lots of company. Hemispherectomy is not mentioned in any of the 35 books on consciousness that I have read. Only a few of them bother with the split-brain and, with a few exceptions (Christof Koch), the ones who do discuss the split-brain are often confused on the facts and as a consequence incoherent in their conclusions. It seems what was all the rage in 1981 when Roger Sperry shared in the Nobel Prize has become by 2000 quite passé.

The principal value of speaking at the Helmholtz Club was being added by Terry Sejnowski to his list of invitees. In the years I attended, 1995–2003, the group varied in size depending on the invited speaker and other considerations, from as few as a dozen to three times that many. Throughout the years the most faithful attendee, in addition to Terry, was Francis Crick whose presence was essential. It was for me a sad development that my deafness and dialysis eventually ended my participation in 2003.

Anencephalic Psychology

Here are a couple examples of what I call "closed-box psychology." It resembles an engineer comparing output to input without knowing anything about the contents of the box. (I am aware that the usual term is "black box.") The outstanding example of closed-box psychology is probably psychoanalysis because of its theoretical complexity and extensively developed lexicon (rivaling astrology). I have been a fan, of sorts, of psychoanalysis ever since my mother took it up in 1948 when she found a teaching analyst, Martin Grotjahn. He soon nominated her for the Los Angeles Psychoanalytic Institute; but they were reluctant to let her in. By this time she was a clinical professor of psychiatry at USC, and worse there was that Ph.D. in biochemistry. They were afraid she would cause trouble. It turned out later that they were right.

In those days there was still a bitter antagonism between the so-called biological psychiatrists and the psychoanalysts. There is still some of that; but it was really intense then. She already had a national reputation as an "organic" psychiatrist. So for somebody like her, one of the first people to use insulin coma and then electroshock, to apply for training in the Psychoanalytic Institute was rare, perhaps unique.

When my mother died in 1960, I went through her old papers and there was a copy of a letter she had sent to Ralph Greenson. Ralph Greenson was the therapist for many movie stars, including Marilyn Monroe at the time of her death. And he was President of the Institute. The letter explained why she wanted to join the Los Angeles Psychoanalytic Institute. She was volunteering at the Camarillo State Hospital and there were some young psychiatrists there who had just gotten out of the Army, having previously had psychoanalytic training. She wrote that in spite of the fact that she had so much more experience with schizophrenic patients than they had, she had the impression that they were sometimes able to communicate with the patients when she could not. They seemed to have some understanding she did not have, and she wanted it. The Institute eventually let her in.

In the 1940s, I had a girlfriend who was in psychotherapy with an analyst. She was full of gossip about the psychoanalytic community and told me that some of the analysts called my mother Madame DeFarge. This was the character in the *Tale of Two Cities* who would sit knitting at the guillotine. My mother would sit in the class when guys were lecturing about Freud et al. and she would knit continuously. From time to time she would ask a question that had the effect of dropping the guillotine knife on somebody. So they referred to her as Madame DeFarge—rather typical of psychoanalysts to find a literary metaphor.

Eventually it came to a head. The medical people, including teaching analysts Grotjahn and Judd Marmor and some others with medical training, got fed up with the lay analysts who were so antagonistic to physiological or chemical considerations. So they left and formed a new group called the Los Angeles Society for Psychoanalytic Medicine, and they took about half the people out of the Institute. I understand there have been further schisms. It seemed to me partly because psychoanalysis served, for a lot of people, the same function as religion serves true believers. And if you are a true believer, then you will differ not only with nonbelievers but with other true believers from time to time.

On the other hand, there are the critics of psychoanalysis. Christof Koch asked me to read the chapters in his book on consciousness. In an early version he was pretty vitriolic about the nonscientific nature of most of the psychoanalytic stuff. It was not his usual measured attempt at scientific objectivity. I said, "I'm not sure why you're writing in this style unless it's your view that if you put the hammer to psychoanalysis you'll win a lot of friends. The fact that you alienate the psychoanalytic community is not important because they are not as influential as they once were; the number of people who don't like them is much greater. So I can see that if you're doing this on purpose it would make sense. Otherwise, it's just gratuitous, and not in the style of the rest of what you're writing."

I do not know for sure why they are so vitriolic. But I think I know why. It is not simply because they are dogmatic, which many are. And it is not simply because so much of psychoanalytic theory is baloney. And it is not just because there are so many cozeners among them. The most important reason (I believe) is that some of what they say is so true it hurts. This is the part that John Kihlstrom was getting at when he wrote of "the cognitive unconscious," although he assiduously avoided any mention of Freud. And it asserts what Wegner's *The Illusion of Will* was about, that we rationalize so much of our behavior, often not knowing its true origins.

What the castigators of psychoanalysis rarely say, but what seems to me glaringly obvious, is that psychoanalysis exemplifies the pitfalls of attempting to understand mentation without reference to brain. Such an approach may have been reasonable when adopted by Freud, but it is not reasonable now.

In June 1978 The Massachusetts Institute of Technology sponsored a 3-day conference, chaired by Noam Chomsky, on the subject of brain and language. The meeting took place at a large estate in the woods about an hour's drive from Boston. I was picked up by a limousine at Logan airport. Several minutes later, the limo picked up the famous psychologist/philosopher Jerry Fodor. During the hour long ride, about 45 minutes consisted of Fodor's explaining that a scientific psychology has no need of brain any more than understanding software requires a knowledge of computer hardware. For me this seemed similar to claiming that a scientific understanding of cars need not involve any mechanical knowledge, just because people can drive cars without knowing anything much about carburetors, universal joints, or even fan belts. That was 25 years ago and there are still plenty of people preaching this sort of anencephalic philosophy/psychology.

Someone might say, "But that was over two decades ago." Sadly, many philosophers have not changed. In the spring of 1997, Owen Flanagan came to Cal Tech at the invitation of Christof Koch. At dinner he mentioned that on leaving Duke, he informed his philosophy colleagues that he might see some neurolab or neurocase material. They recoiled in horror. He may have embellished the tale a bit but the message was clear. Philosophers (those anyway) do not dirty their hands with data. Is it really the sad truth that natural philosophy (what we now call science) has so far separated off from its origins that it has left behind only papyrologists—people who take paper in, put paper out, and while reading and writing assiduously, earnestly avoid the tangible? Do they consider direct contact with data to be of negative value? Are they, like some redneck in the novel *Tobacco Road,* actually proud of their ignorance?

Flanagan can be rewarding because he explains how other philosophers are wrong, and he does it in a readable style. Unfortunately, even Flanagan reveals a surprising neuroignorance. It seems that philosophers are still devoting time to whether or not consciousness is epiphenomenal. This is the idea that consciousness is like heart sounds. The sounds can tell us some of what is going on in our hearts (just as consciousness can tell us some of what is going on in our brains), but the sounds do not have any effect on the function of the heart.

To explain epiphenomenality, Flanagan contrasts two pictures: in the first, a hot stimulus to the hand causes a feeling of pain, which leads to withdrawal of the limb; he calls this "the standard view." In the second, the stimulus causes the pain and the withdrawal in parallel; he calls this (correctly) the epiphenomenalist view. The fact is the second has been "the standard view" for over a century. The withdrawal is a spinal reflex and the pain is epiphenomenal for the behavior, although likely not for the memory of the occasion.

The reader will have by now recognized some of my convictions about consciousness: (1) There is such a thing. We routinely ascribe consciousness to some entities and not others and with fairly widespread agreement. Moreover, we label levels of consciousness (as with the Glasgow Coma Score) for both diagnostic and therapeutic purposes, again with fairly good agreement. (2) Consciousness depends on brains and is to be understood (so far as we can) in naturalistic terms. (3) Whatever the mechanism producing consciousness, it exists in duplicate. In each hemisphere exists the machinery for consciousness. Of course, we all know that almost all cerebral anatomy exists in pairs; it is obvious in any frontal or horizontal section of the cerebrum. However, few authors connect this fact explicitly with the problem of consciousness. Is the duality of anatomy like the runners of a sleigh, such that if one is damaged or removed the sleigh cannot go? Is the duality more like two harnessed horses, such that if one is removed, the remaining member of the pair can still pull the sleigh, not as fast or as far, but enough? The answer unquestionably is the latter. Otherwise hemispherectomy would not have become a routine procedure in 18 of 25 epilepsy centers.

Not only is the cerebral anatomy double, and not only is it unarguable that one hemisphere is enough for consciousness; beyond that, two hemispheres following callosotomy have been shown to be conscious simultaneously and independently. As Nagel said of the split-brain, "What the right hemisphere can do on its own is too elaborate, too intentionally directed, and too psychologically intelligible to be regarded merely as a collection of unconscious automatic responses." And, "If the patients did not deny awareness of what is being done [by their right hemispheres], no doubts about their consciousness would arise at all."

Much of the meandering inconclusiveness of discussions on consciousness results from there being so many different usages of the word. However, almost all usages have in common the idea of subjectivity. Hence, I believe: (4) Explaining subjectivity should have priority. Finding a physiologic basis for subjectivity is hard enough without trying to explain all the other different stuff that people mean or might mean when they say "consciousness." (5) Mammalian brains have considerable power for generalized computation but special functions (e.g., subjectivity) commonly require specialized structures. Such an hypothesized structure has been facetiously termed a "subjectivity pump" by Marcel Kinsbourne. Well, that is exactly what some of us are looking for. And the mechanism for subjectivity is double, as shown by the duality of the anatomy, the success of hemispherectomy and the split brain results (in cats and monkeys as well as humans).

A bit more gentle opinion about philosophy was expressed by Crick and Koch. "...while philosophers have, in the past, raised interesting questions and pointed to possible conceptual confusions, they have a very poor record, historically, of arriving at valid scientific answers." One frustrated scientist's opinion was less generous. In his chapter in what still remains one of the best books ever about consciousness, A.E. Fessard wrote, "...we doubt that epistemological discussions and metaphysical hypotheses, which in this field cannot be easily avoided, can ever be of real utility. By their subtleties and intricacies of points of view, by the fallacy of certain analogies, the mixture of facts with respectable but unverifiable beliefs, they have obscured, more often than clarified, the naive notion every normal man has of his own consciousness." Perhaps the last word in this review should be left to a professional philosopher: Because I believe consciousness requires brain, it seems to me that before people go on about consciousness, they should know something about brain. I asked my cousin Jim Bogen, a Philosophy Professor, "If someone wants to philosophize about quantum mechanics, shouldn't he know how to do QM first?" "The good ones do," Jim replied. "Well then, if someone wants to philosophize about consciousness..." He interrupted, "They should be conscious."

During the previous 2 years, Christof Koch had sent me two or three chapters of his own book, *The Quest for Consciousness,* as acknowledged on the back cover of that beautiful book, which appeared January 2004. With this book in my hands I thought, "This promises to be the most deeply informed and most scientifically thoughtful book written on the subject." In the course of my review of these chapters, I noticed he needed some help with various clinical aspects I knew he would not be familiar with. Once a month he came to my home for dinner, and I would indicate what might be improved in the manuscript. In the course of writing my own book, Christof said he would be happy to return the favor and review my chapters in the same way. I gave him Chapter 1, The Prologue, the Notes for Chapter 1, and the First Interlude (as well as Appendix A containing some philosophic considerations).

A month later we met again for dinner at my home. Turning to the subject of my book, he said, "Well, I showed it to Francis, and he had the same impression."

> "Well, what's that?"
> "We think you're trying to write two books in one, and we think it's probably a mistake." He went on, "You've got a scientific book here, and you have a lot of autobiography, and combining the two is unlikely to find a welcome with almost any publisher."

Well, it was partly the continued urging of Gabriele that led me to continue along this path as she had an entirely different view of the matter, and I figured since I was writing the book at her urging, I thought I should listen to her than to this particular opinion of two people whom I admired very highly, or I should say whom I considered the very best that science could offer.

Again Christof came to dinner, now in February 2004, having read Chapter 2 and having given it considerable attention, his impressions scribbled in the margins. The criticisms that Christof had to offer were multiple, the most important being: "This stuff is really harder to read than it should be," followed by "It seems kind of archaic, the way you present material." I asked him, "What's difficult about it?" "Well, for example, here is this expression 'cerebellar cortex,'" and I had to ask myself, "What is he talking about?"

"People don't talk that way. I mean you could talk about the 'cortex,' or you could talk about the 'cerebellum,' but this is a really funny expression, this 'cerebellar cortex.'" I mean here is a man who is as prominent as one can get with an endowed Chair at Cal Tech, a man responsible for my being a professor each year for the last several years, and he is no doubt as good a scientist as anyone can be. His chosen field is computational neuroscience and the man does not know what the word 'cerebellar cortex' means!

The next day, instead of thinking "This is ridiculous," I said to myself, "Well, the fact that throughout his entire career (he was 47 years old) the only contact he has had with the word 'cortex' was when it has referred to the 'cerebral cortex.' By contrast, by the age of 30, I was comfortable with the terms 'adrenal cortex,' 'cerebral cortex,' 'cerebellar cortex,' even 'pulmonary cortex.' I had to keep in mind that all of us have clay feet, and it may well be that my biggest clay foot is getting outraged at people's ignorance.

Epilogue

On April 10th Joe and Glenda celebrated their 50th wedding anniversary, and on April 13 he was admitted to the hospital. He died on April 22, 2005. The following was the last thing Joe wrote. It was found on his computer desktop dated March 8, 2005.

After a rather aimless youth, my fascination with brain function and the inspiration of outstanding investigators instilled in me a capacity for hard work and reawakened a childlike joy of discovery, of arriving at fresh understandings. These were accompanied by a desire for celebrity. With increasing maturity this desire to be acclaimed became distilled as it were, into a desire for approbation by the few I most admired rather than the crowd. And there came that special reward of scientific endeavor, the friendship of inquiring minds plus the reward of meeting folks of like interest throughout the world. With the progressive restriction of activity attributable to age and severity of disease, all that has faded relative to the companionship of family, including two loving and accomplished daughters who so wisely chose admirable husbands. The importance of family is a truth seemingly known all along to Glenda, my wife of 50 years. Ours has been a tumultuous marriage, often suffering from my devotion to work and verging on divorce on two occasions followed by wonderful reconciliations. Nearing the end, it is her love that turns out to be most important.

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