



Michael S. Gazzaniga

BORN:

Los Angeles, California
December 12, 1939

EDUCATION:

Dartmouth College, A.B. (1961)
California Institute of Technology, Ph.D. (1964/1965)

APPOINTMENTS:

Assistant/Associate Professor, Psychology; Chairman, Psychology; University of California, Santa Barbara (1967–1969)
Associate/Full Professor, Psychology; NYU (1969–1973)
Professor, Psychology; SUNY, Stony Brook (1973–1978)
Director, Division of Cognitive Neuroscience; Professor of Neurology and Psychology; Cornell University Medical College, NY (1978–1988)
Andrew W. Thomson, Jr. Professor of Psychiatry; Director, Program in Cognitive Neuroscience; Dartmouth Medical School (1988–1992)
Director, Center for Neuroscience; Professor of Neurology and of Psychology; University of California, Davis (1992–1996)
David T. McLaughlin Distinguished Professor; Director, Center for Cognitive Neuroscience; Dartmouth College (1996–2002)
Dean of the Faculty; Dartmouth College (2002–2004)
David T. McLaughlin Distinguished University Professor; Dartmouth College (2004–2006)
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HONORS AND AWARDS:

Fellow of the Society of Experimental Psychologists (1982)
American Academy of Arts and Science (1997)
Ariens Kappers Medal for Neuroscience (The Royal Netherlands Academy of Arts and Science, 1999)
Institute of Medicine (2005)
Distinguished Scientific Contribution Award, American Psychological Association (2008)
Alexander von Humboldt Award, Germany (2008)
The Gifford Lectures, University of Edinburgh (2009)
Charles L. Branch Award, University of Texas, Dallas (2010)

Michael S. Gazzaniga carried out original studies of human brain laterality and function in split-brain patients, work that has rich implications for consciousness, free will, and the self. He introduced the term cognitive neuroscience, helped develop the discipline, and founded the discipline's flagship journal. More recently, he has effectively written several books for a broad audience about brain and mind, showing generations of readers the human face of science.

Michael S. Gazzaniga

There is a foie gras served at a hideaway of a restaurant in Edinburgh that is sublime. I had had a double Scotch to celebrate completing my first of six Gifford Lectures and was beginning to feel normal again. Lectures always exhaust me, and I really don't particularly want to see anybody at their conclusion. This night was different, however. Somehow the University knows about these matters and leaves you alone with your family to recap and savor the experience and honor, and to taste that magnificent city. As my wife, my daughter, and my sister assured me I had not made a fool of myself, we all soothed each other and submerged ourselves in what we all love the best: ideas, food, and drink—all with a twinkle in the eye.

I cannot separate my scientific life from my personal and nonscientific associations. Even though they are utterly different aspects of my time, each arena has helped sustain the other. While I like my evening cocktails, I prefer talking about ideas rather than what is on the sports page that day. This predilection has led my wife, Charlotte, and me into giving endless dinner parties over the years that have become a major part of our intellectual life. That impulse has served us well. My wife and I have had a grand life getting to know the minds of others.

Presenting the Gifford Lectures in 2009 was an enormous undertaking and over the course of 2 years of preparation, I discovered it was the mix of all of my friends, academics and nonacademics alike, that have colored my penultimate views on mind and brain issues. I came to realize how much we humans are the product of the entire milieu within which we live. A retrospective account of one's scientific contributions, as if all things linearly developed from a single logical framework, is just plain wrong, at least for me. In what follows, I will attempt to talk not only about the scientific work accomplished but also the social and personal setting in which they occurred. I have had the privilege of knowing several great scientists and public personalities, and they all have influenced me in profound ways. As they say, if you play tennis, don't play with a poor player. It brings your game down. I haven't.

The Caltech Years

Let me roll the camera back to my graduate days at Caltech. After a challenging 4 years at Dartmouth College, little did I know that my social life at Dartmouth would become a more noteworthy achievement at that time than

anything I ever accomplished academically. I was a member of the much fabled Animal House and played out my days as “Giraffe.” Actually, I was the nerd of the fraternity, preferring to spend more of my time working in the laboratory of the psychologist William B. Smith than drinking in the Alpha Delta Phi House basement. Smith had a passion for research and he had built a small lab on the top floor of McNutt Hall where we developed methods to measure eye movements. We worked together long into the night. Research was all new and exciting for me, and the first glimpse of the mystery of trying to figure out a mystery of Mother Nature became very real and compelling. While some of my dearest friends came from the Animal House, it also served to motivate one to do other things.

During the spring of my junior year I found myself reading a *Scientific American* paper by Roger Sperry on the mechanisms of neuronal specificity. It was a beautiful story, beautifully written, leading me to an idea. Caltech was close to my childhood home as it was to the home of the girl of my dreams who went to Wellesley. Maybe I could get a summer fellowship at Caltech and work on the problem of neural specificity, and see my girlfriend frequently. To shorten a long story, the job worked out, but the girlfriend didn't.

That was the beginning of my love affair with Caltech. It all started by being surrounded by an institution and a faculty that projected high expectations. After 50 years, I remain convinced Caltech is the finest scientific institution in the world. When I was there, Linus Pauling was down the hall. Seymour Benzer joined the Sperry lab and his office was across from mine. We talked all the time. Richard Feynman had a way of popping into graduate student offices and asking them what they were doing. And of course my own mentor Roger Sperry was the most insightful and engaging of all. We talked for a couple of hours each day that summer and for my entire subsequent five graduate education years. It was a special relationship at the time.

When that glorious summer was over, I was not trained in developmental neurobiology, as I had expected, but rather in the beginnings of what was called psychobiology. The Sperry lab continued to do work on neural development, but its major focus was on split-brain animal work. During that summer I contributed to the enterprise by developing a method for reversibly anesthetizing half of a rabbit brain. I was hooked.

Inspired by the excitement of the ongoing animal research on split-brain cats and monkeys, I returned to Dartmouth College determined to go to back to graduate school at Caltech. I couldn't help but become captivated by the question of what would happen to humans with callosal sections. During my senior year at Dartmouth I had the idea to try to retest a famous group of patients from Rochester New York who had had their callosum sectioned in the early 1940s in an attempt to limit their epileptic seizures to one half brain. The Akelaits's patients seemed like the perfect opportunity to confirm the animal work. So during my spring break I arranged to visit them

through the offices of Dr. Frank Smith, who at the time of the surgeries had been a resident.

I designed many experiments and exchanged letters with Sperry about the ideas and the plan. I applied to the Mary Hitchcock Foundation at Dartmouth Medical School and received a small grant to rent a car and to pay for my stay in Rochester. In the end I didn't get to see the Rochester patients, even though my car was loaded with borrowed tachistoscopes from the Dartmouth psychology department. The effort to reveal the effects of callosal disconnection in humans would come later.

My decision to go to graduate school and to forego medical school was accepted by my father, but he was never thrilled with the idea. He was a remarkable surgeon and one of the founders of the first prepaid medical plan in America, the Ross-Loos Medical Group in Los Angeles. He was utterly devoted to the medical profession and genuinely thought it was the finest and only way on earth to spend one's life. In the end he was pleased as one of my brothers and one of my sisters became physicians. The next question, of course, was would I get into Caltech. If one reads the qualifications for entrance, any normal person's assumption would be that he or she had better make other plans. Somehow it worked out, in large part I am sure to being sponsored by Sperry.

As soon as I arrived, for my first day of graduate work, Sperry gave me my assignment. The split-brain experiments I had designed during my senior year at Dartmouth would finally be implemented, but on Caltech patients rather than Rochester patients. Soon enough, I was in the thick of an exciting and consuming project. That was 50 years ago. Before I knew it I was examining a robust and charming man, WJ, who was about to undergo cerebral commissurotomy, the so-called split-brain surgery, to control his otherwise capricious epilepsy. He was the sort of level-headed person to instill respect in a young, green, graduate student like myself.

Dr. Joseph E. Bogen was also there, a resident at the time, and the person who had critically reviewed the medical literature and was convinced that split-brain surgery would have beneficial effects. He enlisted Dr. P. J. Vogel, a professor of neurosurgery at the Los Angeles-based Loma Linda Medical School, who performed the surgery. My chore was to quantify the psychological and neurological changes, if any, in the way WJ behaved once the connections between his hemispheres had been sectioned.

The conventional wisdom suggested that nothing would happen. Andrew Akelaitis, 20 years earlier, had found that callosal section in human subjects produced no behavioral or cognitive effects. Karl Lashley had seized on this finding to push his idea of mass action and "equipotentiality" of the cerebral cortex; discrete circuits of the brain were not important, he claimed—only cortical mass. After all, he concluded, cutting the massive nerve bundle that connected the two halves of the brain appeared to have no effect on inter-hemispheric transfer of information.

The electrifying beginnings of the human work might have been predicted by dozens of experiments on animals. Study after study had shown that corpus callosum section profoundly altered brain function in cats, monkeys, and chimpanzees. Specifically, information presented to one brain hemisphere remained isolated in that hemisphere. It was as if dividing the great cerebral commissure produced an animal with two minds, neither of which was aware of the workings of the other. Ronald E. Myers and Sperry had already coined the term “split-brain” to describe such animals. Yet the idea that callosal section would produce a similar condition in humans seemed bizarre.

Preoperatively, WJ could name stimuli presented to either visual field or placed in either hand. With his eyes closed, he could understand any command and carry it out with either hand—in short, he was entirely normal. The stage was set ideally to investigate what would happen following the disconnection of his cerebral hemispheres. The scientific context and the time were right for us to ask the right questions: Could it be that a disconnected right hemisphere was as conscious as a disconnected left hemisphere? Could it be that a state of co-consciousness could be produced in a human being? Where would positive answers to either or both of those enquiries lead us?

When WJ returned for testing after surgery, I experienced one of those pivotal moments in life. First, and to no one’s surprise, the subject named and described normally stimuli that were presented to his left hemisphere. Then came the critical test: what would happen when information was flashed to his verbally silent and physically isolated right hemisphere? Akelaitis’s work predicted that the subject would describe the stimulus normally, because his studies suggested that the corpus callosum played no essential role in the interhemispheric integration of cerebral information. On the other hand, the animal work suggested that something interesting might emerge. As it happened, something interesting did emerge: the idea that splitting the human brain produced two separate conscious systems (Gazzaniga et al., 1962). It was a revolutionary idea, and 50 years later it is one that still needs study and clarification. The context for the work was established by my thesis (1965), which in turn was composed of a set of papers (Gazzaniga et al., 1963, 1965, 1967).

It is curious that, despite centuries of study and speculation about consciousness, there is no general agreement even about what the term means. If you asked 20 students of the problem to finish the sentence, “Consciousness is. . .,” 20 different definitions would result. Still, most of us would agree that the term refers to that subjective state we all possess when awake and to our feelings about our mental capacities and functions. As is typical with vast and ill-defined concepts, it is easy to offer simple examples of what it means to be conscious, but, at the same time, lifetimes of inquiry will not divulge the entirety of its nature.

My time at Caltech was unforgettable and enduring. My daily conversations and good times with Roger Sperry were priceless at both a scientific and personal level. It is because of that that his abandoning me once I left Caltech was especially difficult on me. As Mitch Glickstein was to observe recently, Sperry had a hard time sharing credit with his former students (Glickstein and Berlucchi, 2010). What can you do? There is no doubt in my mind he was the greatest brain scientist to ever live.

I still do research on these special patients and talk and write about the research all the time (Gazzaniga, 2000, 2005). While split-brain research has been a dominant influence on my life, it has not been the only influence. The reason is that I am a dreamer. I am always thinking about other things. My mind wanders. At my core, I suppose, I am hopelessly restless.

I assume the value in an effort such as writing this article for the autobiographical volumes is to expose students to the many ways one experiences a life in science. For me, the excitement and wonder of scientific discovery is indescribable. You have to go there and do it. It not only gives meaning to one's own life, it gives meaning when reading about the discoveries of others and what they must have felt at the moment when, bingo, an insight is confirmed with hard experimental fact. In my case, this happened early in my career. What it did for me was clear the way to appreciate the life of the mind and mostly the minds of others. As a consequence of my early good fortune, I was never frantic to prove myself. Sheer luck played into a fast beginning, and I felt empowered to cultivate my general restlessness and venture into a wide range of issues. Of course, the other liberating reality was that split-brain research was thrilling when I began it and remains so today. There never is the sense of drudgery that can sometimes occur when something becomes routine. When you know your day job is rich and exciting, one can explore more one's secondary interests.

As a consequence, I liked examining other social questions as well as scientific questions bubbling up from the new human studies and wanted to explore them in animal models. It seemed so natural, being surrounded by an active primate research laboratory. Sperry was approving, as he was a comparative biologist as much as anyone could be. It almost was expected of you to come at problems from a variety of approaches. I dove into animal research as well from the very beginning.

Animal Research

After my stint with rabbits as I described earlier, my first extended efforts were in trying to understand something about the neural mechanisms of a relatively simple sensory motor act, accurately reaching for an object. My colleague Colwyn Trevarthen had cleverly invented a way to determine which hemisphere a split-brain monkey was attending through when solving a dual-task discrimination. I went after a simpler question. How does

the left hemisphere control the left arm and hand following split-brain surgery? After all, hemisphere disconnection disrupted the flow of information from the sensory process of the left hemisphere to the motor systems of the right hemisphere, the hemisphere that had dominant control of the left hand. This issue arose immediately in our human split-brain work. Working with primates I could pursue it more aggressively (Gazzaniga, 1964).

The entire Sperry lab was contributing to the building of better and better automated behavioral testing devices for the rhesus monkey. We all had slightly different ideas on how to make them better. Trevarthen liked to transport the monkey from the room where it was kept into his own testing box/apparatus. I preferred to build a unit that could be attached to the back of the monkey's home cage and to arrange that the discriminanda were presented automatically, sort of out the back door of the monkey cage. I built an elaborate automatic data recording system, a closed circuit TV for watching and all the rest.

Sometimes we had to resort to using monkey restraining chairs, while at other times we could test the animals in a large cage where we were able to limit sensory input to one eye or the other by using contact lenses. It was under these conditions I was able to spot a major mechanism used by monkeys to cross-integrate information from one hemisphere to the other following their split-brain surgery. Again, the question became, how does the left or right brain guide and control its ipsilateral hand? Sensory-motor information was easily integrated when both the sensory information and the motor information were being processed in the same hemisphere. But following split-brain surgery, how did the sensory information from one hemisphere get together with the motor mechanisms of the other?

It all became evident in the open cage testing. With one eye—say, the left eye—occluded by our opaque contact lens, the right eye and therefore the right hemisphere alone was seeing, as all the animals also had their optic chiasm split down the middle. I would hold up a tantalizing grape on the end of a stick and let the animal reach out for the grape and grab it with his right hand. The left hand was restricted in various ways. I also filmed the event using slow-motion photography.

It was in the films that it all became evident what was happening. The animal would get set and then stare at the food with the seeing eye. Because the sensory afferents from the head and neck project bilaterally, all the proprioceptive information available from the head and neck important in cueing position in space was coursing not only to the seeing hemisphere but also to the nonseeing hemisphere (Gazzaniga, 1966, 1969). The arm/hand would then go out to the correct point in space, even though there was no information about the third dimension of the object since no visual information had been communicated. As a consequence, the hand was not forming the appropriate anticipatory posture to grab the grape. Then quickly as the hand brushed by the grape, bumping into it—in a way, blindly—the hand

would instantly adopt the correct posture for grabbing the grape. It all was an astounding cascade of self-cueing on how to appear as if the whole act was initiated as a whole piece from the beginning when it was not. Some planning, yes, followed by local control mechanisms.

The capacity for monkeys, for humans, indeed for even rats to adapt to challenge and self-cue themselves into completing their goals became a pervasive idea in our research and remains as one of the fundamental discoveries of how biological systems seem to keep it together. The goals are set at the highest level of nervous activity, and implementation is done in any way possible to complete the action.

The split-brain monkey work was exciting and the preparation was unbelievably powerful. With cross-cueing emerging as a big theme, it was natural enough to see whether emotional cues were also cross-integrated between the hemispheres. We figured out how to attach goggles to monkeys with each eye covered by a different colored filter, one red, one blue. In this way we could let the monkey view his cage through one eye by having the room lit in red. Then we could carefully turn the room light to blue, giving the other hemisphere a chance to see some additional information that would be emotionally arousing, such as a snake. Would that influence the red brain that was not able to see it? It did, of course, and that became the larger story about all the cues, coming from independent systems playing into the final action or behavior of an animal.

Another idea that was prominent at the time was the idea of using split-brain animals in the following context. Keep one hemisphere completely intact and have the other open for systematic lesions in an effort to determine structure–function relationships. This overall strategy proved highly useful both in studying visuomotor coordination and in studying basic perceptual mechanisms. In one series of studies I wondered whether an isolated intact visual system might not actually see if it was disconnected from parietal and frontal cortex. Using the split-brain preparation, this was a possible experiment (Gazzaniga, 1966). One hemisphere was left completely intact while the other was lesioned to various degrees. Thus, as we lesioned more and more cortex in one isolated hemisphere, we could see whether there came a point where the animal could no longer perform a visual discrimination, even though the visual system was intact. To our surprise it worked and overall the experiments were consistent with Sperry's view that "Perception is the preparation for response."

I continued my interest in animal research during my postdoctoral years. When I left Caltech, I was fortunate enough to receive a fellowship to study at the Institute of Physiology in Pisa. I had come to know Giovanni Berlucchi when he had been a visiting fellow in Sperry's lab. To know Giovanni is to love him, and he encouraged the fellowship to Pisa. The idea was that I was going to learn how to do neurophysiology and, by doing so, be enabled to study the actual neural code of the corpus callosum. While there,

I also met Giacomo Rizzolatti and the three of us embarked on our journey to figure out the brain code (Berlucchi et al., 1967).

Of course, both Berlucchi and Rizzolatti went on to do groundbreaking neurophysiological research. I discovered in the process that the method was not for me. It took far too much patience and the capacity to spend dozens of hours sitting in one place. My restless ways triumphed, and I returned to studying patients.

It was not, however, before embarking on one last set of experiments in both rats and monkeys involving motivation. Upon returning to my new job at UCSB in 1966, I got to know David Premack. It is hard to think of a more important living psychologist than David Premack. As one considers our origins, our antecedents, our uniqueness as humans, it is Premack's work that has guided us to so much of our current understanding of the way we are.

I met him when he was still carrying out his pioneering work untangling the morass of information that had painted a simple but incorrect picture of the nature of motivation. Behaviorism had developed a view of the learning animal as being motivated only by external contingencies and did not consider internal states and preferences. He turned the whole thinking about the nature of reinforcement on its ear by seeing beyond the easily observable and ascertaining, using the methods of science, the underlying secrets of what motivates animals, including us, to do anything. Seeing through easy explanations is his specialty.

In what later became known as the Premack Principle, he showed that what served as a reinforcer was reversible and could be predicted by the preference structure of an animal. Thus, a rat deprived of running would drink water if that gave it the opportunity to run. Conversely, if a rat was deprived of water, it would run in order to have the opportunity to drink. This is a powerful idea and it stuck with me. When I moved to NYU after early years at UCSB, Premack gave me one of his unique testing systems to test an idea I had. Would an adipsic rat, which is to say a rat that will not drink as the result of a lateral hypothalamic lesion, drink nonetheless if given the opportunity to run? If true, it would urge a more dynamic view of brain function and caution against the ever-growing tendency to see static models relating structure to function. In fact, we learned adipsic rats gladly drank if that is what they had to do to in order to run (Gazzaniga et al., 1974).

Premack was just starting his work on chimpanzee language and beginning to articulate the intellectual limits of our closest living relative and, in doing so, unearthing the factors that make us unique. He started with a particular chimp, Sarah. I know, as she lived down the hall from me for years when our time overlapped at UCSB. I don't care for chimps. I have always found them too aggressive and bestial and quite frankly would walk in the other direction when Sarah approached with her trainer.

Nonetheless, following his work proved valuable again as the meta-language he developed for chimps proved useful in studying global aphasic patients, a project he and I did together with my graduate student Andrea Velletri Glass (Velletri-Glass et al., 1973).

I kept my animal research active until one cold day the NIH told me to pick my poison: either do animal research or human research, but not both. This came at a time when I had Richard Nakamura finishing up his thesis work on studying various memory phenomena in primates. My other student at the time was Joseph LeDoux. We were just beginning to launch a major set of studies on a new group of split-brain patients operated on at Dartmouth Medical School. The transition made sense and we complied. No more animal research for me, and LeDoux switched with me from monkey work to human studies.

Parallel Interest: Going outside Science

As I already mentioned, I had other interests in social issues. I had begun to question my social/political assumptions. During my undergraduate years, I had possessed all the normal interest for liberalism. In fact, I was a Catholic liberal, the worst kind; I believed that everything could be fixed and, if not fixed, forgiven. At some point, however, I started to think of liberalism as a cruel hoax. Things were not as mutable as the liberal activist must believe them to be. I was beginning to doubt fancy psychological theories of development and to become convinced that it is almost impossible to change anybody's behavior in a serious way.

Anyway, some friends and I started something called the Graduate Committee for Political Education. We were tired of all the liberal speakers who got invited to Caltech. Where were the conservatives? We knew Caltech wouldn't come along quickly or quietly, so we started our own group, rented a public auditorium in nearby Monrovia, and arranged for William F. Buckley, Jr. to give an evening lecture. I met Bill the day before at the home of his sister-in-law, who turned out to head the Red Cross and live in Pasadena. It was a pool-side lunch with, I will never forget, onion sandwiches. Now, have you ever had onion sandwiches? They are a delight, and Bill was quick to put me at ease, even at his boyish age of 36, and so we chatted about anything from his sister-in-law's sandwiches to JFK. I remember using the word *potentiate*, which is a commonly used term in pharmacology, and his informing me that no such word existed in the English language. That was the last time that I was right in a dispute between the two of us that had to do with language.

That weekend, a friendship was born and survived over 50 years. Bill was naturally friendly and unflaggingly generous, though I believe he had no concept of the many implicit gifts he made to his friends. Most of my friends are in science, which is to say that they reflexively try to dissect

assumptions on matters scientific. Yet as a group they tend to be painfully unable to apply those skills with wit to social and political agendas. When they do, the action tends to be tedious and joyless. Bill challenged everything, but always with a grin and with humor. His was a disposition that made it hard for others to rattle his resolve. He always was on top of things with the big picture. Experiencing that attitude about life served those who knew him in ways he was never able to appreciate.

Soon after the lecture in Monrovia, I discovered that there was a bit of Sol Hurok in me. After a couple of weeks of that evening's great success, we decided to go big time. Why not arrange a series of debates on the American Constitution? Why not put out a book? Why not have fun? So I asked Bill if he would lead off such a series debating Steve Allen on the American Presidency. He said, "Sure." Then I asked if he would write to Steve Allen since I didn't know him. "Sure," he said, adding that Allen's wife, Jane Meadows, had grown up in Bill's hometown. Bill wrote the letter, Steve said yes, and within a couple of weeks, I had arranged for two other debates. I had Robert Hutchins debating Bill's brother-in-law, L. Brent Bozell, about the Supreme Court, and somehow I had arranged for James MacGregor Burns, one of JFK's biographers, to debate Willmoore Kendall, the maverick conservative political theorist, on the Congress. I don't know what I was thinking. A few weeks later, I realized I had signed contracts for auditoriums and speakers that totaled more than \$10,000. The Graduate Committee for Political Education had \$200 to its name.

On the morning of the debate, only 200 people had purchased tickets. Steve had taped his TV show the night before with Bill as a guest. They had warmed up for their debate about JFK, but the show wouldn't air for 2 weeks. I was concerned about the lack of ticket sales and told Steve. Steve very matter-of-factly said, "Don't worry, Mike—3000 people would show up to watch me play tiddlywinks." As an aside, JFK was assassinated on the Thursday following the debate but before the taped show was to air. When that American tragedy occurred, both Steve and Bill saw to it that the show never aired.

As it turned out, 3300 people bought tickets that night, and two of them were Mr. and Mrs. Groucho Marx. Dozens of limousines and Rolls Royces pulled up for the big event to buy tickets for \$2.75.

Backstage, Bill and his entourage waited in one room, and Steve and his waited in another. Since it was to be a debate, there would be prepared opening statements but, following those, the participants were to think on their feet. Bill Buckley did this better than anyone and, in that sense, it was an unfair match. But Steve had prepared as if for war. To guard against freezing up, he had prepared remarks for his rebuttal as well.

Out front, the crowd was boisterous. This was going to be the event of the century: Steve Allen, head of SANE, the movie-community chapter of the national antinuclear activist group, and Hollywood's favorite liberal,

pitted against William F. Buckley, American's leading conservative, who was ready to tell the Soviets that we would nuke them if they made a false move. They were going to march through JFK's foreign policy and examine it from Vietnam, to Cuba, to the Soviet Union. When the debaters took to the stage, the crowd rose to their feet and cheered them into battle. I walked to the very back of the auditorium, in a stunned state. What had I done? There were only two security guards.

The rest of the evening took care of itself. Here were two great showmen arguing their views. At one point, Buckley spotted Groucho Marx sitting in the front row. Sensing that the crowd needed a little jolt, he, without blinking, incorporated the opportunity into his rebuttal. He stared at Steve Allen and exclaimed, "Let's face it, Steve, President Kennedy's foreign policy might as well have been written by the Marx Brothers." Now most folks hadn't noticed his presence. Groucho stood up on cue, walked up on stage, strolled across to thunderous applause, and smoked his cigar all the while.

Bill was a pragmatist and, as such, he promoted the idea of legalizing drugs. His good friend Milton Friedman also promoted the idea, but more from his perspective of being a libertarian, something the conservative Bill Buckley was not. Bill's position was that drugs were a plight and our policy was a disaster, so we had to find ways of fixing the situation. At one point, while he was preparing for a conference in Los Angeles, Bill started to grill me, which led to my own formation of opinions on the topic: "What do you know about drugs? What can you tell me about their action on the brain? Do you know anybody that has thought about the problem from the medical perspective?" I could see that I was about to learn a lot more about drugs than I already knew. I went to work on the problems, which turned out to be fascinating, and I eventually wrote the results of my work up for *National Review* in the form of an interview in 1995, which we conducted over e-mail. But that was only the beginning.

During all of this in 1990 Bill called one day to ask me to appear on *Firing Line*. "That's television," I said blankly. "Oh, you'll be fine," he replied, and, at length, I agreed and then proceeded to memorize every fact there is to know about drugs. That really wasn't a very useful strategy, but it was the only one I could think of, and it was better than nothing. For weeks before the show, I pestered him with this or that question and went over in my mind how the show might go. Finally, the day arrived, and I went to a 23rd Street studio for the taping. In typical fashion, Bill was doing three shows in a row with 5-minute breaks in between. I went into the makeup room to wait for them to tape the two other shows, one of which featured the exiled King of Greece and the other, two English professors from Oxford talking about a book on Chaucer. Now, it was my turn to talk about drugs. Well, I survived and Bill was off to catch the shuttle to Washington. He had to give a speech that night.

A few years ago, I saw an article he wrote about his boyhood home, Great Elm in Sharon, Connecticut. His father had designed an exquisite indoor room, which was beautifully photographed for the article. I mentioned to Bill that I would love to see it someday. Within a few months, he arranged a rendezvous in Stamford, and we took the drive out to Sharon. It was a gorgeous fall Sunday, and when he picked me up at the train station, we took off immediately for what I thought was roughly a 20-minute drive. In fact, Sharon was almost 2 hours away, although somewhat shorter with Buckley driving. What a day. We hadn't seen each other for quite awhile, so there was lots of catching up to do. I was telling him about various new findings on the brain and behavior, including the new survey finding that only 1.8% of the population was gay, as opposed to the often-claimed 10%. "Oh," he said, "Do you believe that? I don't, because if it were true, why I think I know every one of them." You couldn't get the wit out of him.

In all of this another friendship was born. Steve Allen became intrigued with my research and over the years we saw a great deal of one another. At one point he brought his family over to the laboratory at Caltech on a Sunday afternoon. He wanted to see how things were actually done. We talked for several hours and with his sharp wit and grace I found myself doing my first "outreach" of science education.

A few years later, when I was an Assistant Professor at the University of California, Santa Barbara, I had Psychology I duty (introductory psychology). One thousand students three times a week showed up at Campbell Hall. There is a bit of crowd control and mass motivation required when the numbers are this big. In short, one has to keep the students motivated. So I called Steve and asked if he would like to come up and give a lecture. . . . gratis. He accepted in a flash and, as the lecture was at 8 am, he drove up the night before, stayed at a motel near the University, and showed up bright and early.

We had agreed he should talk about the creative process. I had arranged for a piano to be on the stage and the rest, as they say, is history. Steve was a man of many talents, songwriting being one of them. He told the story of what was behind his greatest hit, "Picnic." Steve wrote the lyrics after receiving a call from the producer, and he wrote them in record time. As he looked back on it, he basically came up with a resource allocation model for creativity. Usually, he said, he would be asked to write a song with absolutely no constraints put on the assignment. In this case, the producer said, "I want you to write the lyrics to our movie theme that stars William Holden and Kim Novak and they will be dancing at a picnic." As Steve pointed out, all his energies became focused on the task at hand. In the unconstrained situation, so much energy is lost trying to define the context and idea for a song, one is depleted by the time the actual task is apparent.

Steve Allen, Jr., his son, is a physician and we became quite close. He is hysterically funny and humane and he, like his father, was completely

captivated by brain research. In fact, at one point I cooked up an idea of making a film about the brain and creativity. I had just moved to Stony Brook and, as part of a startup package, I had purchased a new Beaulieu 16 movie camera that allowed for sound to be recorded right on the film during filming. This made editing and production quite easy, or so I thought. While the camera was bought to use for my patient work, I took up the cause of making a science education film.

The camera, parabolic sound microphones, lights, and all the rest required several bags, all of them heavy or awkward to move around. I was undaunted, however, and proceeded to call Steve senior and ask if I could fly out to LA and film him as I fired at him questions about creativity. He couldn't have been more agreeable, and off to LA I went.

I arrived at his home one Saturday morning and Steve was still lounging in his robe. I didn't think people actually lounged in their robes except in the movies. Well, here we are. He walks me into his living room and suggests how I should set up the lighting, tripods, and all the rest. It all started to become surreal, and this little voice in me starts in: "What are you doing? Why are you bothering this guy in his robe? What are you going to do? Why aren't you back at Stony Brook doing your research? Who do you think you are—Fellini?"

I was just about to leave, making some excuse, when Steve, said, "Looks like you are ready." With that he starts to play one of his own compositions, *This Could Be the Start of Something Big*, and for a moment, I did think I was Fellini. The experience was exhilarating, and I vowed to take my gear everywhere to capture moments for the film. In fact, I took the gear to Paris a bit later, set it up in my hotel room at the Paris Hilton, threw open the window, and with the camera on automatic filmed myself standing in front of the window, with the Eiffel Tower in the background, and thought my second career was launched.

Oh my, we do crazy things. After filming riding up the Eiffel Tower and half of Paris, I went home, loaded up with my footage. With great anticipation I waited for it to come back from being developed, stuck it on my projector, and sat back to watch and savor my ingenuity. Let me simply say my escapade into filmmaking ended quickly. My favorite disastrous scene is in the hotel window. Because my camera was reading the light level of the bright Paris sky, the guy in the foreground looks like he is in a witness protection program and working with the CIA. Of course, one always looks for the silver lining. Steve looked terrific in his robe.

Of course, the real lesson in all of this is that human nature is a constant. We all get channeled into our little groups and tend not to keep active relationships outside of our academic world. That is a big mistake. Both Bill Buckley and Steve Allen taught me that all of us are trying to figure out the big picture. And both insisted on having fun while doing it. It was this experience that emboldened me to work on the President's Council on Bioethics

later in life. Tough, reasoned discourse with politically sophisticated minds from both the left and right motivated me to explore a range of social problems.

Moving to New York

One day I got a call from Leon Festinger. He had just moved to New York. Years before, while I was still a lowly assistant professor at UCSB and he was a famous Stanford professor, he had invited me to a seminar at his home. A mutual friend of ours at Stanford assured me that the seminar would be interesting, adding, “Oh, and Leon is really smart, Mike—you’d better prepare.”

This was one of Leon’s quirks: As he became acquainted with people in foreign fields, he offered them his gracious hospitality, assuming they would have something to say. And so it was that I sat down in an easy chair in his living room, with Leon about 3 feet from me, smoking his ever-present Camel, his students clustered behind him. Drinks were served, and we were off to the races.

Leon was not going to miss a single word of this neophyte’s remarks, and it turned out to be a glorious evening. There I was, with the smartest man in the world listening attentively to me talk about my experiments, and with deference at that. I would say that over the next 20 years or so, we talked about our own research programs no more than 5% of the time when we were together. What is it about discovering a true intellect that causes conversations to wander? Leon viewed and considered everything from a perspective informed not only by years of experience but also by enormous knowledge of almost every topic.

Anyway, Leon urged me to apply for a job at New York University and to move east. Eventually I did and for the subsequent 20 years was based in New York. First it was NYU, then Stony Brook, and finally Cornell Medical School. What was common to all those endeavors was my weekly lunch with Leon (Gazzaniga, 2006). I could not get enough of him, and we launched several intellectual ventures as well. Over the years we started study groups on behavioral economics with Stan Schachter. Leon and I also started a group studying human origins, his new passion, that deeply impacted my own thinking as well and which led to my last book, *HUMAN*.

New York University and Cornell Medical School

In New York I finally was able to study the neurologically damaged patient, which before had not been possible in Santa Barbara. Even though I had moved to the NYU Graduate School, the Medical School was close by and very welcoming. Before long, my student Andrea Velletri Glass and I were following up on an idea I had at Santa Barbara: Could a meta-language

system that Premack had developed for chimps be used to communicate with patients who were globally aphasic? The results were striking. Patients, using artificial symbols that they had learned to associate with objects, actions, and descriptors could use these associations to describe actions carried out by the experimenter. It was like unlocking the secrets of a jailed mind (Veletri-Glass et al., 1973).

Over the years, the neurologic patient proved to be a rich source of information about the many ways the unconscious brain actively carries out critical operations that ultimately influence our conscious life. At Cornell Medical School we began to study our first patients with neglect. In one study Bruce Volpe, Joseph Ledoux, and I (1979) revealed that perceptual information presented in their neglected half space nonetheless influenced perceptual judgments in their opposite conscious visual field. This was unheard of at the time but slowly grew into a small cottage industry. Overall, the simple introduction of forced-choice techniques revealed the unshakable fact that cognitive capacity remained evident in patients and it was processed outside the realm of their conscious processes.

In a related observation, Gail Risse and I examined the limits of access that conscious processes have to memories when “secrets” were established in the right hemisphere of patients who temporarily had their left brain anesthetized. In this experiment, objects were placed in the patients’ functioning left hand and palpated while their left brain/right hand was asleep. Minutes later when the left brain woke up, the patients were first asked to verbally say what they had held in their left hands. They claimed to have no idea. Seconds later, however, the left hand, now awake and nonparetic, easily pointed to matching objects (Risse et al., 1978).

I have always been captivated by the wealth of knowledge that can be gained by studying patients with focal disease. Fred Plum and Jerry Posner at Cornell really encouraged this interest and in the good old-fashioned way. Just do it! So that is what my students and I did along with an entirely talented group of young neurology residents at Cornell, such as Bruce Volpe, Ruth Nass, John Dougherty, Jonathan Victor, and others. From NYU Graduate School to Stony Brook to Cornell, there were always studies going on with patients. We studied anmesics, patients with neglect (Volpe et al., 1979), blindsight (Fendrich et al., 1992; Wessinger et al., 1997), aphasia (Gazzaniga et al., 1973), motor disorders (Volpe et al., 1979), and more (see Nass and Gazzaniga, 1987). Perhaps our most extensive effort with neurologic focal lesioned patients was on the blindsight phenomeon.

Probing the Unconscious

When unconscious processes are occurring, it used to be thought they were mainly happening in subcortical structures. The split-brain patient has

these structures intact and it was of great interest to us to determine which unconscious phenomenon might still be connected and which were not following cortical disconnection. A series of reports in the 1970s indicated that, even though a patient may not consciously see in a blind visual field of vision due to a cortical lesion, the patient's hand might be able to respond to stimuli presented in the blind field. To explain this bizarre finding, dubbed blindsight, researchers proposed that the phylogenetically older parts of the midbrain were carrying out the task, not the cortex. We wanted to know more.

It is a fact that 98% of what the brain does, it does outside of conscious awareness. All of our sensory-motor activities are unconsciously planned and executed. As I sit here and type this sentence, I have no idea how the brain actually pulls off the task of directing my fingers to the correct keys on the keyboard. I have no idea how the bird, sitting on the outside deck, a glimpse of which I must have caught in my peripheral vision, just caught my attention, while I nonetheless continue to type these words. Furthermore, the same goes for rational behaviors. I am not aware of how the neural messages arise from various parts of my brain and are programmed into something resembling a rational argument. It all just sort of happens.

It is easy to see why very clever psychologists began to wonder whether formal cognitive psychology had missed the boat. Perhaps the challenge is to study that great platform of life, the unconscious. Larry Weiskrantz had coined the term "blindsight" to account for the remarkable phenomenon, the ability to see even though the visual cortex had been damaged or removed. Immediately following his original report of the phenomenon of blindsight, philosophers, psychologists, and neuroscientists became fascinated with the observation and with the possibility that a means to study the great mystery of the unconscious had been found.

As the early reports accumulated, we began to examine related issues in other types of brain-damaged patients. Damage to the parietal lobe of the brain, for example, causes strange symptoms to appear. If the damage occurs on the right side of the brain, most patients experience a phenomenon called neglect. Thus, when looking straight ahead, they deny seeing anything to the left of where they are looking, even though their primary visual system is perfectly intact. This fascinating region of the cerebral cortex generates a veritable constellation of exotic disruptions when it is damaged. Patients also frequently deny that they are ill, even though they experience weakness of the left hand and arm. When they are shown their left hand and asked whose hand it is, they claim that it certainly isn't theirs.

When all of these behaviors are considered, it becomes clear that, somehow, the parietal lobe is involved with the attentional mechanism. Something is at work, distinct from the parts of the brain that simply represent visual information. Information is getting into the brain, but this process is occurring outside the realm of conscious experience. We showed this basic

fact in a number of ways. In one study, we asked patients with neglect to judge whether two lateralized visual stimuli, one appearing in each visual field, were the same or different (Volpe et al., 1979). So a patient might see an apple in one part of his or her visual field and an orange in the other. Conversely, two apples or two oranges might be presented, one in each half visual field. The patients were able to perform this task accurately. However, when they were questioned as to the nature of the stimuli after a trial, they could easily name the stimulus in their right visual field but denied having seen the stimulus presented in their neglected left field.

These studies were the first in a long series that have now been carried out by several laboratories. Taken together, they show that the information presented in the neglected field could be used to make decisions, even though it could not be consciously described. A decision was correctly made that two objects were different, but the patient could only name one of them.

In the early 1980s, Jeffrey Holtzman began to study blindsight in my lab. We were fortunate to have a piece of equipment, the Purkinje eye-tracker, which allowed for the very careful assessment of the position of the eye in relation to where a stimulus might appear, allowing for the precise presentation of stimuli within the scotoma.

We first studied a 34-year-old woman who had undergone surgery to clip an aneurysm in her right half brain. These nasty rats' nests of vessels can break, causing severe brain damage, so when they are detected they are usually operated on. The surgery was expected to have the consequence of producing blindness in part of the patient's vision, since damage would occur to her right occipital lobe, the brain area with the aneurysm. Sure enough, after surgery, there was what is called a dense left homonymous hemianopia. In other words, she couldn't see to the left of a point she was looking at. Her magnetic resonance image (MRI) revealed an occipital lesion that clearly spared both extra-striate regions as well as the main midbrain candidate for residual vision, the superior colliculus. These intact areas should have been able to support many of the blindsight phenomena commonly reported.

This patient was truly blind, even though she had the brain structures intact that should support the phenomenon of blindsight. He studied her for months and got nothing. He wrote the work up and published it in one of the finest scientific journals (Holtzman, 1984). It met with deafening silence. Blindsight was too big an idea to be shot down by one experiment, even a great experiment beautifully executed. Jeff said, "Great Mike, I come to your lab to learn some new tricks and you know what I discover? Blind people are blind. That kind of brilliance ought to get me a job at Harvard." Actually Jeff made a huge difference in the lives of all of us. No funnier man ever lived and, horrifically, his life was snuffed out as a young man. His work on attentional mechanisms are classics (Holtzman et al., 1981, 1982, 1985; see also Luck et al., 1989; Luck et al., 1991).

We left the problem alone for a few years until a new graduate student, Mark Wessinger, came along to the laboratory and we took up our interest again. Also, my colleague Bob Fendrich, a fellow student with Jeff, came to the lab. By this time, we had moved to Dartmouth Medical School and were calling upon a different kind of patient. Our first case was a woodsman from New Hampshire who had had a stroke that knocked out his right visual cortex (Fendrich et al., 1992). Nonetheless, he pursued life with vigor and was quite a marksman. Before studying what the woodsman's brain could or couldn't do in his blind visual field, Bob argued that we should do perimetry to discover where and how big the blind spot was. He, too, was an expert at managing the complex eye-tracking device, and Bob likes to start whatever he does at the beginning.

For these tests, we were also armed with a newly acquired image stabilizer, which allowed us to keep images steady on the retina despite any eye motion a patient might make. Our woodsman's scotoma was carefully explored using high-contrast black dots on a white background. In fact, a whole matrix of dots was presented in an area of his scotoma. Hundreds of trials were presented over many testing sessions. It is always heart-warming to see the deep respect the average citizen pays to the increase of human knowledge. Once matters are explained to them, they are almost always enthusiastic participants.

The efforts paid off. In the sea of blindness, we found what we called a "hot spot," an island of vision. In one small area, about 1-degree in diameter, the woodsman could detect the presence of visual information. Now if it was truly a 1-degree window, a 2-degree spot should not be detected. Even though a 2-degree spot is larger and under normal conditions would be easier to detect, he should not be able to see it because the black dot would be larger than the window. That is exactly what was found. The patient could not see the larger stimulus. Follow-up testing revealed that the patient could detect differences between light of different wavelengths in the "hot spot." The technology was crucial for the success of the experiment—we were able to reveal the island of vision only because of the particular device we had for testing him, and very few researchers in the world had access to such equipment. Could it be that the island was the source of so-called blindsight?

None of this, of course, is to suggest that unconscious processes are not of constant and primary importance to our vision. It is yet another demonstration of the truth that most of what our brain does, it does outside of the realm of conscious awareness. I and my colleagues do reject, however, the proposition that, since blindsight demonstrates vision outside the realm of conscious awareness, it supports the view that perception can occur in the absence of sensation. Because the role of primary visual cortex is to process sensory inputs, advocates of this view have found it useful to attribute blindsight to alternative visual processing pathways. I submit that this formulation is unnecessary and implausible. It is commonplace to design demanding

perceptual tasks where nonneurologic subjects routinely report low confidence values for tasks they are performing above chance. However, it is not necessary to propose secondary visual systems to account for such data, since the primary visual system is intact and fully functional.

The Dartmouth Split-Brain Cases: Discovering the Interpreter

The second decade of my professional life gave rise to the idea of modular concepts and the interpreter. By the 1970s, the passage of time had provided more studies and more patients, and the original nature of split-brain studies had been modified substantially. The field had drifted into thinking about different kinds of consciousness, and the notion that mind left dealt with the world differently than mind right was the major conclusion of studies during this era. Though interesting in its own right, this characterization of how each hemisphere processes information still begged the question of what consciousness actually was and how the brain enabled it to be experienced. In many ways, the work in the early 1970s was misleading.

Reports with chimeric stimuli found that split-brain patients favor the right hemisphere for “gestalt” stimuli and the left hemisphere for “analytical tasks,” and our hypotheses briefly took on a new direction. We began to argue that it wasn’t so much that there were separate conscious systems following commissure section but simply that each hemisphere possessed different cognitive duties. The earlier overcharacterization of left brain/right brain function was short lived in the scientific community, but it has been annoyingly persistent in the popular press.

During the mid-1970s, a number of reports emphasized an additional feature of right hemisphere specialization. Brenda Milner and Laughlin Taylor reported superior performance in the right hemisphere on nonverbal tactile stimuli. Joseph LeDoux (1978) and I found the manipulations of a stimulus to be critical in bringing out right hemispheric superiorities. For example, right hemispheric superiority was revealed only in a block design test in which the patient manipulated the blocks to make the patterns required; in an equivalent, “match to sample” test, in which patterns were only visually inspected, right superiority disappeared. While these new observations were challenging enough to the simple view of hemispheric functioning and to ideas about dual consciousness, the new conceptual framework was even more antithetical to existing concepts about the unity of conscious experience. In brief, the new view suggested that the brain was organized in a modular fashion with multiple subsystems active at all levels of the nervous system, and that each subsystem could process data outside the realm of conscious awareness. These modular systems were fully capable of producing behaviors, mood changes, and cognitive activity. Such activities were in turn monitored and collated by a special system in the left hemisphere that I called the “interpreter” (Gazzaniga, 1985).

We first revealed the interpreter using a simultaneous concept test. In this type of test, the subject is shown two pictures—one exclusively to the left hemisphere and one exclusively to the right (Ledoux et al., 1977; Gazzaniga and Ledoux, 1978). The subject is then asked to choose pictures that are associated with those lateralized images from an array of pictures placed in full view in front of them. For example, a picture of a chicken claw is flashed to the right visual field, and a picture of a snow scene to the left visual field. Of the pictures placed in front of the subject, the obviously correct association is a chicken for the chicken claw and a snow shovel for the snow scene. Accordingly, subject PS responded by choosing the chicken picture with his right hand and the snow-shovel picture with his left. The left hemisphere, however, was aware only of the chicken-claw image, while the right hemisphere was aware only of the snow-scene image. When asked why he chose these items, his speaking left hemisphere replied, “Oh, that’s simple. The chicken claw goes with the chicken, and you need a shovel to clean out the chicken shed.” The left hemisphere, observing the left hand’s response, interpreted that response only in the context of its own sphere of knowledge—a sphere that did not include information about the snow scene that had been presented to the right hemisphere.

In a related experiment, in the few patients with right- as well as left-hemisphere language, we lateralized written commands by presenting them tachistoscopically to the subject’s left visual field. In an example where the command was “laugh,” the patient laughed and, when asked why, replied, “You guys come up and test us every month. What a way to make a living!” If the command “walk” was flashed to the right hemisphere, the patient would stand up from his or her chair and start to leave the testing van. When asked where they were going, the left brain might say, “I’m going into the house to get a coke.” Again, the left hemisphere observes and interprets the actions of the isolated right hemisphere in order to create a verbal response.

At this point, our research had shown that there are many ways to influence the left brain interpreter, and we were still interested in determining whether emotional states presented in one hemisphere would have an influence on the affective tone of the other hemisphere. At this point we met VP, a dazzling 28-year-old woman with a keen sense of life who was a patient of Dr. Mark Rayport of the Medical College of Ohio. She is introspective about her medical history and articulate in expressing her feelings. When we first met her, her right hemisphere skills were limited to simple writing of answers and the capacity to carry out verbal commands. Flash the command “smile” to her right hemisphere and VP could do it. Ask her why she was smiling and her left hemisphere would concoct an answer. But 2 years later, VP’s right hemisphere could directly tell us why, because by then it had developed the capacity to talk. During the time when only her left brain could speak, however, we were able to set up mood states in her nontalking

hemisphere and study whether the talking hemisphere was aware of the mood, and, if so, how it dealt with the induced mood. From all of the other studies, of course, it was clear that the left brain was not directly knowledgeable about the actual pictures or movies that had been shown to the right brain. But could it detect the mood?

Using a very elaborate optical computer system that detects the slightest movement of the eyes, we were able to project a movie exclusively to the left visual field. If the patient tried to cheat and move her eyes toward the movie image, the projector would automatically shut off. The movie her right hemisphere saw was about a vicious man pushing another man off a balcony and then throwing a fire bomb on top of him. It then showed other men trying to put out the fire. When VP was first tested on this problem, she could not access speech from her right hemisphere. When asked about what she had seen, she said, "I don't really know what I saw. I think just a white flash." I asked, "Were there people in it?" VP replied, "I don't think so. Maybe just some trees, red trees like in the fall." I asked, "Did it make you feel any emotion?" VP said, "Maybe I don't like this room, or maybe it's you, you're getting me nervous." Then VP turned to one of the research assistants and said, "I know I like Dr. Gazzaniga, but right now I'm scared of him for some reason."

This experimental evidence merely illustrates a rather extreme case of a phenomenon that commonly occurs to all of us. Our mental systems set up a mood that alters the general physiology of the brain. In response, the verbal system notes the mood and attributes a cause to the feeling based on available evidence. Once this powerful mechanism is clearly demonstrated, given the complexity of real-life emotional stimuli, one cannot help but wonder how often we are victims of spurious emotional/cognitive correlations.

Although our split-brain subjects always possess at least some understanding of their surgery, they never say things like, "Well, I chose this because I have a split-brain and the information went to the right, nonverbal hemisphere." Even patients who have exceptional IQs tend to view their responses as behaviors emanating from their own volitional selves. As a result, they incorporate those behaviors into theories to explain why they behave as they do. One can imagine that, at some point, a patient might be studied who would choose not to interpret such behaviors because of an overlying psychological structure that prevented the response. Or one can imagine a patient learning by rote what a "split-brain" is all about and why, therefore, a certain behavior most likely occurred. Such a circumstance would certainly complicate the role of the researcher, and such subjects might well not be able to offer explanations for their behaviors. There are occasions when a patient who is having trouble controlling his or her left arm due to a transient state of dyspraxia will tend to dismiss anything that he or she does under the direction of the right brain. This makes the simultaneous concept test inappropriate. In such situations, a single set of pictures is

presented and only one hand is allowed to make the response. For example, the word *pink* is flashed to the right hemisphere and the word *bottle* to the left. Placed in front of the subject are pictures of at least 10 bottles of different colors and shapes, and the subject is required to respond using the right hand.

When this test was run on split-brain subject JW, on a particular day when he said that he could not control his left hand, he immediately pointed to the pink bottle with the right hand. When asked why he had done this, JW said, “Pink is a nice color.” In this case, JW responded to a stimulus that had been presented to his right hemisphere using his right hand, in defiance of our expectation that he would be unable to do so. When he was pressed to explain how he had done it, his left-hemisphere speech apparatus was unable to provide an explanation, and so the interpreter responded as best it could, claiming that the subject had made a simple aesthetic choice. It has been well established that the human brain follows a modular organization, and that those “modules” do manifest themselves through function-specific physical regions of the brain.

The precise nature of the neural networks that carry out those functions is less clear, however. What is apparent is that they operate largely outside the realm of awareness, and that they announce their computational products to various executive systems that result in behavior or cognitive states. Managing and interpreting all of this constant and parallel activity is the role of the left hemisphere’s interpreter module. The interpreter is of primary importance to our identity as human beings; it is what allows for the formation of beliefs, which in turn yield mental constructs that allow us to do more than simply respond to stimuli.

Developing a Field: The Birth of Cognitive Neuroscience

They used to say that Jack Benny, the world’s funniest man, was also the best audience; everyone loved to try out his stuff on Jack. If George Miller is in an audience, he is usually asleep. If he *is* the audience—that is, if you are one on one—he is the best. He analyzes constantly as you pour out your story. He asks probing questions and then, as you hear your own answers, and as those silly formulations bounce off his deadpan expression, you begin revising them. There is not much that is new in this world, and certainly not much new about the psychological nature of human beings. What passes for discovery these days tends to be an individual scientist’s rediscovery and reterming of some well-established phenomenon. Most of these “discoveries” are soon forgotten, but George knows all of them. So on the hundredth trip to the well, you are overjoyed to see a glimmer in his eye and to realize that perhaps there was something to your last idea.

We started exchanging stories in the late 1970s, mine about episodes in the clinic and his about new experimental strategies. I would tell him about

patients with high verbal IQs who lacked a grammar school child's ability to solve simple problems. He would tell me that psychologists do not yet have anything resembling a theory of intelligence or mind. He urged the continued collection of dissociations in cognition, as seen in the clinic, in the hope that a theory would emerge from these seemingly bizarre and scattered observations (see Hirst, 1993).

I took him on my rounds one day and showed him a variety of phenomena that ranged from perceptual disorders to language disorders. He had never seen anything like it and commented afterward that the neurologic patient was really what many psychologists were looking for. After all, he observed, psychologists try to test the brain's limits by making college sophomores work fast or by presenting stimuli rapidly to provoke errors. In the clinic, the errors pour out of otherwise sound brains with little or no effort.

One patient we saw was a distinguished New York executive who had fallen down a staircase. He was reported to be globally aphasic, which means that he would not understand much, if anything, and would speak only a little. As we arrived in his room, the computer tomography technicians were fetching him for a scan, so George and I tagged along. The technician asked Mr. C. to slide over to the gurney, to which he replied, "Yes, sir." Once positioned and rolling down the hallway to the scanner, he was asked about his comfort. "Are you feeling okay?" "Yes, sir," said Mr. C. After arriving at the scanner, the technician slid the patient off the gurney onto the table and again asked if he felt all right. "Yes, sir," said Mr. C. The scan was performed and Mr. C. was returned to his room. The technician, who was familiar with my studies, turned to me and asked why we were interested in this patient, as he felt there was nothing wrong with him. I turned to the patient and said, "Mr. C., are you the King of Siam?" "Yes, sir," he replied with great assuredness. George grinned and observed that success is always grounded in simply asking the right question.

As we continued to consider how best to launch our new field, we talked about everything from neglect to neologisms during our evening rendezvous. It was on one of those evenings that we coined the term "cognitive neuroscience." What we meant by cognitive neuroscience would emerge, slowly. We already knew that neuropsychology was not what we had in mind; tying specific functions to lesioned brain areas would not be our enterprise. The intellectual impoverishment of that idea seemed self-evident, especially with the advent of new brain imaging techniques that revealed the extent of the damage to the surrounding area following what had previously been thought to be focal damage.

Years have intervened, but the idea that neuroscience needs cognitive science has prevailed. The early imaging work of Marcus Raichle, Michael Posner, Steve Petersen, and others set the standard in this regard. So too, for the emerging field of human electrical recordings led by Steven Hillyard and his colleagues. The scientists in both these areas knew that the molecular

approach, in the absence of the cognitive context, limits the neuroscientist to pursuing answers to biological questions in a manner not unlike that of the kidney physiologist. Although such approaches represent an admirable enterprise, when put in that light they make it impossible for the neuroscientist to attack the central integrative questions of mind-brain research. Cognitive neuroscience has now become something of a household word because of these advances and many others. The field now has its own journal, society, and conferences. In fact, the most highly attended meetings at the huge Society for Neuroscience convention are always on topics in cognitive neuroscience. In recent times, most of the presenting scientists are former members of the cognitive neuroscience summer institute.

Summer Institute in Cognitive Neuroscience

When the James S. McDonnell Foundation decided to fund the field of cognitive neuroscience, they set aside some funds for training. The event started out at Harvard and Stephen Kosslyn led the way. He did a fantastic job the first year and set a high standard. The plan has not changed much during the past 20 years. Every summer, 70 students from all over the world participate in a 2-week course that runs all day long. I don't think there has been one failure.

The original group that cooked up this idea thought the Institute would move around from year to year from one campus to another. It turned out that was neither practical nor desirable. After Kosslyn's launching, it fell to me to shepherd the program for the past 20 years. It has been one of the best experiences of my life. Where else can you have 30 top scientists come to town and lecture to 70 bright fellows? Where else can you have outstanding neuroscientists dissect human brains, report on clinical studies, and present basic science lectures on neuronal function capped off with a touch of cognitive theory? I hope it runs forever or at least until we have solved the mind/brain problem.

The President's Bioethics Council

In the fall of 2001, I received a call from Professor Leon Kass, the bioethicist from the University of Chicago. President George W. Bush had just appointed him to head up a new Council on Bioethics, and he wondered whether I might be interested in participating. We had all just been through the horror of 9/11 and not a person in America was not thinking about how to best serve and help our country. Naturally, I agreed, and I have never regretted the decision.

Nothing really prepares one for such assignments. When I took it on, I had never thought deeply about issues in bioethics. Dr. Kass assured me not to worry about that. It was to be a committee about bioethical issues, but not a committee of professional bioethicists. (In fact, most bioethicists

are not scientists, but rather philosophers, theologians, and others interested in public policy on morality.) All points of view and varying kinds of expertise were wanted. I was supposed to help with issues in neuroscience.

Feeling a bit out of my league as I headed off to our first meeting, I told myself I would sit back and listen and learn. After all, bioethics was an area of study founded to monitor medical practices—from organ transplants to determining brain death. I am a Ph.D., not an M.D. If questions related to neuroscience came up, I'd be happy to supply the best information I could.

Those who have known me might be amused to hear that this was my intention; they won't be surprised that as it turned out, I couldn't keep quiet. We started with embryonic stem cell research, something that, to me, is an obviously needed field of study; yet many were opposed on issues that seemed to have nothing to do with bioethics or even science. It became clear to me that the beliefs we all hold can color our judgments on issues that should be considered independent of personal belief systems. Having made a career of trying to understand how the brain enables mind, I had some insight into how beliefs are formed, and I was none too thrilled to see the future treatment of debilitating diseases, let alone the future of our nation's scientific research, being decided on the basis of such capriciousness. I soon found that neuroscience had much to say on issues of bioethics.

Around this time, the term "neuroethics" was coined by William Safire to describe "the field of philosophy that discusses the rights and wrongs of the treatment of, or enhancement of, the human brain." In this sense, neuroethics is a spinoff of bioethics. The field of bioethics was developed and defined to take medical ethics further, as scientific findings became more advanced and needed more specialized philosophers thinking about the impact of science on things such as genetic engineering, reproductive science, brain death, and so on. Clearly, many of these traditionally bioethical topics can be looked at through the lens of neuroethics. One way to look at neuroethics is that any time a bioethical issue involves the brain or central nervous system, neuroethics should have a say.

But neuroethics is more than just bioethics for the brain. As the field develops, we need to expand its scope and its mission. Much of the discussion in neuroethics so far has, once again, been among nonscientists. It is time for neuroscientists to jump into the fray. I define neuroethics as looking at how we want to deal with the social issues of disease, mortality, lifestyle, and the philosophy of living, *informed by our understanding of underlying brain mechanisms*. Neuroethics should be an effort to come up with a brain-based philosophy of life (Gazzaniga, 2005).

Events

My passion for true and thoughtful interactions among scientists was solved by a simple social device. I started to organize small 10-person conferences

on almost a yearly basis for about 10 years. Since I was a one-man show, my strategy was to pick a topic of great interest, pick a venue people loved to visit, and let each of them have a full half day to talk about their research. It worked. The venues included Venice, Barcelona, Paris, Kushadasi, Moorea, and Napa.

The meeting at Moorea provided a bit more than I hoped: I learned that appearances are everything. Moorea was an exquisite place, and I discovered that with various package deals a meeting in the South Seas cost less than one in Omaha. I have found such arrangements to be effective in getting first-class people to participate in a conference, usually after making only one phone call. Indeed, this meeting proved no different. What was different came subsequently. I took heat for the effort from various foundation chiefs who believed the exotic setting created the perception that the participants were not serious.

The participants were, in fact, stellar. Francis Crick, R. Duncan Luce, Gary Lynch, Ira Black, David Orton, Gordon Shepherd, Jon Allman, Geoffrey Hinton, Corey Goodman, Herb Killackey, and the indefatigable Leon Festinger. There was also a wonderful young foundation officer, Eric Warner. We met each morning and afternoon. I converted my grass hut into the meeting room, and matters really began to rock. The topic was the biology of memory and both Lynch and Orton had prepared manuscripts before the meeting that served as a launch pad for discussion. To simply observe there were a variety of perspectives would be to miss the moment.

At another unforgettable meeting, one of those golden moments occurred. A group of us that included Stephen J. Gould met in Venice to think about evolution and the brain. For his presentation, Gould decided to make it a walking tour through San Marco Cathedral. There we were being lectured on adaptations and his spandrel theory beneath the very spandrels that triggered this idea in Gould. It was a moment to remember. A couple of years before we had also gathered in Venice to hear Gould, Changeux, Premack, Pinker, Singer, Lynch, and others discuss selection versus instruction theory. Everyone seemed to love those meetings. As for me, I wrote a book about the topic (Gazzaniga, 1994).

Developing University Programs

I truly detest academic committee meetings. Nowhere else has so much been said about so little. Early in my career I decided to basically not show up. Instead, when I thought a new academic goal was desirable I went out and did it but always with the support of kindred souls. That was how George Miller and I worked for years. The same system enabled me to build other programs at Dartmouth, Davis, and now UCSB.

After developing one of the first programs in cognitive neuroscience at Cornell Medical School, where I successfully constructed a large program

project grant from the NINDS on a wide variety of behavioral neurologic problems that dealt with attention, perception, memory, and more, I decided to move to the woods of Vermont. My family was growing and our two-bedroom New York apartment was not doing the trick for us. Indeed, with so much of my time being spent on the Dartmouth series of patients, it made a lot of sense.

We loved our time at Dartmouth and in between two different stints, one at the medical school and one at the College, we moved to Davis to establish the UC Davis Center for Neuroscience. All of these assignments involved administrative duties as well as active research programs and the ever-present need to obtain funding. None of the successes of these programs would have been possible without this attitude—"Let Mike do it until he screws up. Then rein him in." I am deeply grateful for all of my colleagues who let it all happen.

Enduring Issues and Thoughts

In the 1980s I became convinced that our understanding of consciousness is best enabled by placing the phenomenon in an evolutionary perspective. That context causes certain truths to emerge for me that give rise to the idea that, at its core, human consciousness is a *feeling* about specialized capacities. Throughout the development of split-brain research, one salient fact has remained: disconnecting the two cerebral hemispheres, while eliminating direct interaction between the halves of the cortex, does not typically disrupt cognitive and verbal intelligence. The left hemisphere remains the dominant cognitive entity following such surgery, and this dominance seems to be sustained not by the entire cortex but by specialized circuits within the left hemisphere. In short, the unique properties of the inordinately large human brain are engendered by its circuitry, not simply by its size. It is the accumulation of specialized brain circuits, then, that accounts for the human conscious experience.

Furthermore, our sense of being conscious never changes during the normal aging process. Taken together, these two views lead to the conclusion that what we refer to as "consciousness" is nothing more or less than a collection of feelings that we have about our specialized capacities. We have feelings about people and objects we interact with, and about our capacities to think, to believe, and to use language. In other words, consciousness is not a distinct system—it reflects the affective component of specialized systems that have evolved to enable human cognitive processes. Combined with the human inferential system, which seems to be limited to the left hemisphere, it empowers all sorts of mental activity. Our consciousness of those mental activities depends on our capacity to assign feelings to them, and that is what distinguishes human consciousness from everything else, including the electronic artifacts with which we surround ourselves.

Naturally, viewing consciousness as a myriad of feelings about specialized abilities predicts that the consciousness emanating from one hemisphere would differ radically from that emanating from the other. Whereas left-hemisphere consciousness would reflect what we refer to as normal conscious experience, right-hemisphere consciousness would vary as a function of the specialized circuitry that half-brain possesses. Mind left, with its complex cognitive machinery, can distinguish between the states of sorrow and pity, for instance, and it appreciates the feelings associated with each state. The right hemisphere does not possess the cognitive apparatus to create such distinctions and, as a consequence, its state of awareness is relatively low. Specific types of reduced right-hemisphere capacity, therefore, have specific implications for the states of consciousness of the subjects in which they are found.

Patients with a split brain without right-hemisphere language capability exhibit a limited capacity to respond to patterned stimuli that ranges from no capacity at all to the ability to make simple matching judgments at above-chance levels of performance (Siddis et al., 1981). Patients who possess the capacity to make perceptual judgments that do not involve language do not exhibit the ability to make a simple same/different judgment within the right brain when both stimuli are lateralized simultaneously. In other words, when two simultaneously presented figures required the judgment "same," the right hemisphere failed. This profile is commonly seen in all kinds of patients with a silent right hemisphere, and it seems to be independent of overall subject intelligence. This minimal-capacity profile stands in marked contrast to that of patients who possess right-hemisphere language. The right brain of these patients is responsive, and their overall capacity to respond to both language and nonlanguage stimuli has been well catalogued and reported.

In the East Coast series of patients we study, this observation includes the case of JW, whose right hemisphere has understood language and has had a rich lexicon throughout our association with him, as assessed by the Peabody Picture Vocabulary Test and other specialized tests. Until recently, however, JW could not generate speech from his right hemisphere (Baynes et al., 1992). Studies with VP and PS revealed that these patients were able to understand language and to speak from either half-brain. It would be reasonable to suppose that this extra skill would add to their right-brain capacities to think, which is to say to interpret the events of the world.

It turns out, however, that the right hemispheres of both patient groups are poor at making simple inferences (Gazzaniga and Smylie, 1984). The subjects were tested by being asked semantically to combine the content of two pictures that were presented one after the other to their left visual fields. Presented with a picture of a match and then a picture of a woodpile, for example, neither group was successful in deducing that a burning woodpile was the correct result. In another test, simple words were presented one

after another to the subject's left visual field, and the subject was instructed to choose the word that reflected the causal relationship between them from a list of six possible answers. The subjects also failed these trials, a typical one of which might consist of the words "pin" and "finger" being flashed to the right brain, the correct answer being "bleed." Although the right hemisphere could always find a close lexical associate of a word that was given by itself, it could not perform the interpretive function necessary to recognize relationships between two words.

In this light, it is hard to imagine that the left and right hemispheres have similar conscious experiences. The right cannot make inferences and, as a consequence, is extremely limited in what it can have feelings about. The left hemisphere, on the other hand, constantly and almost reflexively labels stimuli, making causal inferences and carrying out a host of other cognitive activities. Recent studies have shown that the left brain carries out visual search tasks in a methodical manner, whereas the right hemisphere tends to perform haphazardly. The evidence surrounds us that the left hemisphere is predisposed to analyze and differentiate the workings of the world, whereas the right hemisphere simply monitors its surroundings (Phelps and Gazzaniga, 1992; Metcalfe et al, 1995).

I was recently asked by a *Time Magazine* reporter: "If we could build a robot or an android that duplicated the processes behind human consciousness, would it actually be conscious?" It is a provocative question and it is one that persists, especially as one tries to capture the differences between the spheres of consciousness that exist between separated left and right brains. Much of what I have written here has appeared before in other forums and, for students of split-brain research, is not all that new. Yet I find the way we all nuance our understanding of complex topics to be ever changing, as none of us holds the true answers in our hip pocket. I found myself answering the reporter with what I feel is a new twist. Underlying this question is the assumption that consciousness reflects some kind of process that brings all of our zillions of thoughts into a special energy and reality called personal or phenomenal consciousness. That is not how it works. Consciousness is an emergent property and not a process in and of itself—much like the taste of salt that is the emergent and unpredictable product of sodium and chloride coming together. Our cognitive capacities, memories, dreams, and so on reflect distributed processes throughout the brain, and each of those entities produces its own emergent state of consciousness.

Consider one fact. A human split-brain patient who has had the two halves of her brain disconnected from one another does not find that one side of the brain misses the other. Her left brain has lost all consciousness about the mental processes managed by her right brain, and vice versa. This is just as with aging or with focal neurological disease. When it comes to cognitive content, we don't miss what we no longer have access to as opposed

to missing capacity due to disruptions in more sensory-motor processes such as vision, hearing, and speech. The emergent conscious state arises out of each capacity. If they are disconnected or damaged, there is no underlying circuitry from which the emergent property arises. The thousands, if not millions, of conscious moments that we experience each reflect one of our networks being “up for duty.” When it finishes, the next one pops up and the pipe organ-like device plays its tune all day long. What makes emergent human consciousness so vibrant is that our pipe organs have lots of tunes to play, whereas rats, in contrast, have few. And the more we know, the richer the concert becomes.

Closing Thoughts

Ever since I heard a lecture a few years back by an astrophysicist who laid out the reasons why extraterrestrials must exist, I have wondered what they would think about us humans and the ideas we think we are studying. I recently wrote a short thought piece on the matter (Gazzaniga, 2010). In brief, it points out much of the type of neuroscience that has been done to date has not squared up with the large fact sitting in the room. The mind is an emergent phenomenon, generated by neurons alone that can in turn constrain its progenitors. A fancy way of saying this is that our field of neuroscience is at a choice point. No longer can we simply make observations at one level of organization. As Roger Sperry put it toward the end of his career, “Instead of an exclusive bottom-up microdeterminism, we substitute a bi-directional model adding a reciprocal top-down emergent or holistic form of downward control The emergent whole, that is, constantly exerts downward control over its parts.” I have arrived at the same place, and I only wish I were young enough to start over and tackle that issue.

I have only captured a small dimension of the personal, social, and intellectual forces that shaped my life and made it such a fine run. I have saved the best notes for last: my family. I have been married twice, both to fine people. The fruits of my marriage have delivered us six children, all of them bright, beautiful, funny, and hardworking. There never seem to be “teen problems” around our home. My wife Charlotte has for the past 33 years made sure no such downward mood swings could occur. In addition to being mother to her four step-children and our two own children, she never wavered one second with equal treatment under the rule that family is supreme. Somehow while doing this, she help cofound the *Journal of Cognitive Neuroscience*, ran the household, held the dinner parties, arranged for all the trips, ran experiments on patients, and drove our special motor home for patient testing.

To see one’s children grow and blossom, of course, is the joy of all parents. It is the wit that keeps a family together. A dinner conversation might go like this. Dad: “Hey kids, I am coming up with a new idea about

brain organization, that it is modular.” Zazzy: “Modular? Dad, our eighth grade curriculum is modular. We have the English module, the math module. . .and so on. You are going to have to do better than that.” Dad: “Eat your vegetables.” Or while during my days on the Bioethics Council. . . Dad: “So ok, when does life begin?” Zack: “Only after an open field tackle.” Life still goes on apace. Life is good.

Acknowledgments

Much of the foregoing has been collected together from other articles and essays I have written over the years. This too-long statement finds me aghast about whom I have not commented on more thoroughly. The literally dozens of colleagues that I have been close to will have to be thanked in another way. Most important, being in academics for 50 years allows one to train and to learn from dozens of outstanding colleagues. I want to at least note those that have made my life so fulfilling.

Jacopo Annese, Ph.D.	Abigail Baird, Ph.D.
Kathleen Baynes, Ph.D.	Paul Corballis, Ph.D.
Karl Doron, Ph.D.	Robert Fendrich, Ph.D.
Brett Foxman, M.D.	Andrew Francis, Ph.D., M.D.
Chadd Funk	Margaret Funnell, Ph.D.
Scott Johnson Frey, Ph.D.	James Eliassen, Ph.D.
Alan Gibson, Ph.D.	Pamela Greenwood, Ph.D.
Todd Handy, Ph.D.	Jeffrey Holtzman, Ph.D.
Jeffrey Hutsler, Ph.D.	David Johnson, Ph.D.
Marc Jouandet, Ph.D., M.D.	Alan Kingstone, Ph.D.
Kenneth Leslie, Ph.D.	Joseph LeDoux, Ph.D.
William C. Lofuts, Ph.D.	Ruth Nass, M.D.
George R. Mangun, Ph.D.	Michael Miller, Ph.D.
Richard Nakamura, Ph.D.	Molly Colvin Putnam, Ph.D.
Kathleen Redington, Ph.D.	Patricia Reuter-Lorenz, Ph.D.
Jo Renn, M.D., Ph.D.	Gail Risse, Ph.D.
Matt Roser, Ph.D.	John Seamon, Ph.D.
John J. Sidtis, Ph.D.	Valerie Stone, Ph.D.
Mark Jude Tramo, M.D.	David Turk, Ph.D.
Betty Tuller, Ph.D.	Jack Van Horn, Ph.D.
Andrea Velettri-Glass, Ph.D.	Jonathan Victor, Ph.D., M.D.
Bruce T. Volpe, M.D.	C. Mark Wessinger, Ph.D.

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