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Melvyn A. Goodale

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Brenda Milner Lecture in Cognitive Neuroscience, Montreal Neurological Institute (2004)
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Mel Goodale was a pioneer in the study of the neural substrates of visuomotor control, first in animals and later in humans. His early work in the 1980s, in which he demonstrated that the visual control of action is functionally independent of visual perception, laid the foundation for the "duplex" account of high-level vision, which he developed later, along with his long-time colleague, David Milner. This account provides a compelling resolution to conflicting views of visual function that have characterized much of the work in the field for the past 100 years. Over the past decade, Goodale has led much of the neuroimaging and psychophysical research that has refined and extended the two-visual-systems model. These ideas have had an enormous influence in the life sciences and medicine, and the two-visual-systems model is now part of almost every textbook in vision, cognitive neuroscience, and psychology. His work has also received considerable attention in philosophy, particularly among philosophers of mind and those interested in the nature of consciousness. The two-visual-systems model is also informing engineers who are trying to devise more efficient human-machine interfaces, bioinspired robots, and teleoperations.

Melvyn A. Goodale

The great end of life is not knowledge but action.
—Variously attributed to Francis Bacon, Thomas Henry Huxley, and Hyman G. Rickover

A Winter's Journey

In January 1949, I left England and sailed across the stormy North Atlantic for the New World. I was five years old. Along with my mother, father, grandfather, and younger sister, I joined more than a thousand other people looking forward to a new life in Canada after the austerity of the postwar years in Britain.

Our ship was the RMS *Aquitania*, a flagship of the Cunard Line. Although she had been launched as a four-funnel luxury ocean liner in 1914, the *Aquitania* was refitted as a troop ship in World War I and then World War II, and by 1949, she was ferrying British war brides and their children to Canada under charter from the Canadian government. My family's story, however, was quite different. My mother, Alice Bloor, was Canadian and she was bringing her English husband, and my father, Lawrence (Lawrie) Eric Goodale, back to her hometown of Calgary, Alberta. My father was most definitely not a war bride. So how did this come to be?

A Bit of Family History

My mother's father and my grandfather, Frederick (Fred) Bloor, was born in 1886 in Silverdale, a small village in Staffordshire in the West Midlands of England. Fred was 1 of 10 surviving children of Cornelius John Bloor and Mary Ann Bloor (née Gorse). By age 21, he was working as a bottle washer in a local brewery, not a particularly glamorous job—and certainly not one with much of a future. Understandably, he was feeling restless and (according to my mother) was looking for adventure beyond England's shores. Tales of the 1890s Klondike Gold Rush were still drawing young men to Canada. Even though gold fever had come to an end by 1899, Fred set off for Canada in 1907 in search of his fortune.

Stopping in Calgary, at the western edge of the Canadian prairies, Fred found work in construction. Jobs in the building trades were easy to come by in a fast-growing town like Calgary, which sat astride the new Canadian Pacific Railway. In 1910, he met a young woman, Hannah Reed, also a recent immigrant, in her case, not from the West Midlands, but

rather from the county of Essex in southeast England, close to London. She had arrived in Calgary only that year with her younger brother and her recently widowed father, William Jubilee Reed, who had worked as a blacksmith and ship scraper in Essex. Fred and Hannah married in 1911 and settled in the northern outreaches of Calgary on the edge of the prairies. Over the next 10 years, they had three daughters. My mother, Alice Mary Bloor (born July 7, 1913), was the eldest of the three.

By the 1930s, the Great Depression had arrived. At the same time, the Canadian prairies were experiencing devastating droughts. Fred and Hannah were beginning to think that they had made a mistake in coming to Canada. In 1937, the entire family gathered up their belongings and sailed back to Britain, where they were welcomed by Hannah's relatives in Essex. After only a few months, however, Fred and Hannah began to realize that day-to-day life in Britain was no better than in Canada, and perhaps worse, especially with the growing threat of war in Europe. So, in 1938, the whole family, with the exception of my mother, packed up again and went back to Canada.

My mother, Alice, who was then 24, had decided to stay. In the short time she had been in England, she had met and fallen in love with my father-to-be, Lawrie Goodale, whose family was friendly with the Reed's. Alice was also enjoying life in London, where she had found a job as a secretary in the buying office of one of Canada's largest department stores, the T. Eaton Company. It was all quite exciting.

Alice's new boyfriend, Lawrie, was born on August 16, 1914, to John Robert Goodale and Edith Lucy Goodale (née Williams) in Stepney, an ancient district in London's East End. But by the time Alice arrived in England, Lawrie, together with his parents and his older brother Jack, had moved into a large house in Leigh-on-Sea, a small village on the Thames Estuary about 60 kilometers east of London. When Alice's parents and sisters returned to Canada, she moved into a spare room in the Goodale home. Alice was continuing to work at Eaton's and Lawrie had a job in London as a warehouse foreman. They both commuted daily to central London by train.

Alice and Lawrie married in March 1940, just a few months after Britain had declared war on Germany following Hitler's invasion of Poland on September 1, 1939. By September 1940, London was being bombed relentlessly. My mother recalled that almost every evening in Leigh-on-Sea, they could hear the drone of the German bombers flying overhead on their way to ravage London and could see the flashes and explosions as Spitfires and other British fighter planes engaged with bombers and their fighter escorts. It was a frightening time.

My father enlisted in the Royal Army Ordnance Corps in 1940, initially serving in the Territorial Army in Britain, which meant that he was able to come home when he was on leave. By then Alice was no longer working

and did not have to make the somewhat perilous train journey to London every day. To be even safer, she and Lawrie decided in the spring of 1942 that she should leave the London area and move north to the cathedral city of York, which so far had been largely spared from bombing. But on April 29, after she had been there for only a few weeks, a fleet of German bombers arrived over the skies in York in the early hours of the morning. For more than ninety minutes, they dropped over 85 metric tons of incendiary and high-explosive bombs, setting the city ablaze and killing or injuring hundreds of people. This attack was one of the so-called Baedecker Raids. The story goes that Hitler was so enraged by the Royal Air Force's bombing of the historic cities of Rostock and Lübeck in Germany that he ordered the Luftwaffe to target all the historic cities in England with buildings marked with three stars in *Baedecker's 1937 Guide to Great Britain*.

Fortunately, my mother was not injured in the air raid—but she decided then and there that she might be safer after all in Leigh-on-Sea, which was neither a strategic target nor a three-star historical site (figure 1). She moved back in May 1942. I was born on January 22, 1943. By then, she and my father had moved into a small council flat on the London Road in Leigh-on-Sea. My memories of the war years are understandably almost nonexistent. I do know that my father was away for much of that period. After serving in the Territorial Army, he was shipped overseas in 1944 to France and Germany. He returned home in February 1946, an event I dimly remember.

The postwar years in Britain were tough. Rationing continued long after the war ended. Despite the day-to-day privation, I have largely pleasant recollections of that time: the hard candies that my parents somehow managed to find, helping my father dig up potatoes in our allotment, feeding the rabbits my grandparents kept in their back garden, and most of all, reading books with my mother. Unlike the homes of most other families in our working-class neighborhood, ours was always full of books.



Fig. 1. My mother and I sailing off the Essex coast on VE Day (Victory in Europe Day), May 8, 1945.

Canada

My parents were finding life increasingly difficult in England. They were entirely reliant on my father's salary and with the birth of my sister, Diana, in December 1946, an even bigger strain was put on their limited finances. Letters from my mother's sisters back in Canada described a life that seemed so much better than what they were experiencing in Britain. My parents were beginning to consider the possibility of joining them in Calgary.

Returning to Canada became a reality in late December 1948. That Christmas, my grandfather, Fred Bloor, arrived in Leigh-on-Sea with tickets in hand for the *Aquitania*. It was the first time he had met me and my sister. Everything that happened from the moment he appeared in our flat to our eventual arrival in Calgary is, for me, a series of vivid and surprisingly detailed memories. I remember the chocolate and the oranges he brought with him—and a View-Master that absolutely captivated me. (I wish I could say that it was the View-Master's amazing three-dimensional photographs that sparked my abiding interest in the human visual system, but alas, the reasons I became a vision scientist are far less interesting.) But the thing that stands out most in my memories of that time is the joyful excitement that filled our flat that Christmas as we prepared for our trip to Canada.

I have isolated but enduring memories of our trip across the North Atlantic. As mentioned earlier, the *Aquitania* had been used as a troop ship during the war, but in 1948, she was refitted for civilian life, although the sleeping arrangements were still a bit austere. My sister and I stayed with my mother in the women and children's quarters, while my father and grandfather stayed with the men. I remember the crowded three-tiered bunks in the large room where we slept. But in other ways, the voyage was more like a luxury cruise. Every night there were movies in the main ballroom, which I glimpsed once or twice, and, according to my parents, there was also entertainment, often provided by the passengers themselves. But most of all, I remember the crusty rolls, bacon, eggs, fresh vegetables, canned fruit, cake, and ice cream that were served at mealtimes, food items that were only infrequently available back in Britain.

On January 17, we arrived at Pier 21 in Halifax, the main gateway for immigrants to Canada at that time. But our journey was not over. Our final destination was still nearly 5,000 kilometers away. We boarded the train in Halifax and began our long journey along the Canadian Pacific Railway to Calgary. It took five days. I have few memories of that trip, but the ones I have are indelible. It was the middle of winter. I remember the clouds of steam billowing from the locomotive and the thick layers of ice covering the couplings between the carriages. I remember the dining car with its dazzling white tablecloths, big china plates, and heavy silver-plated flatware.

We arrived in Calgary on my sixth birthday, January 22, 1949. I suddenly found myself surrounded by aunts, uncles, and cousins whom I had never met. One of my aunts had baked a birthday cake—and someone had arranged for an announcer on a Calgary radio station to wish me happy birthday and to welcome me to Canada. Later that day I went tobogganing for the first time along with my cousins. What a welcome.

Life in Calgary

I began school in late January 1949, shortly after I arrived. Even though I had just turned six and it was more than halfway through the year, Tuxedo Park Elementary School, located only a block from our house, admitted me into first grade because I could already read and write—and count to 100—skills I had learned in school in England. My parents tell me that I rapidly lost my English accent. When spring approached in that first year, I had to persuade my mother that I could no longer be dressed in short pants, an article of clothing that might have been *de rigueur* in Britain but was certainly not in the school playgrounds of Calgary (figure 2).

But what I remember most vividly from my early years in Calgary is exploring the open prairie with its hills, valleys, sloughs, and streams, only a few short blocks from our home. The prairie landscape was populated by gophers, garter snakes, voles, deer, coyotes, and birds and butterflies of all kinds, and nearby Nose Creek was teeming with fish, although all I managed to catch were longnose suckers, big bottom feeders. I marveled at



Fig. 2. Cowboy Mel, spring 1949.

tadpoles, caddis fly larvae, and other peculiar creatures found in the small streams and sloughs.

My parents, aunts, and uncles were avid campers (figure 3). I have fond memories of camping in the Kananaskis, Banff National Park, and many other places in Alberta and British Columbia. I learned how to make a campfire; how to gut, prepare, and cook a trout; and, perhaps most important, how to roast a marshmallow to gooey perfection without it catching on fire. My parents also took my sister and me on road trips to Washington State and Oregon, where we visited my grandfather Bloor's brothers and their families, who had settled there after emigrating from England. Many wonderful days were spent hiking through the temperate rain forests in Washington State and exploring beaches and tidal pools along the Oregon coast.

My new life in Calgary had left me completely smitten with nature and the natural world. But there was another thing that captivated me at an early age: books. My mother was an avid reader and made sure I was, too. In addition to introducing me to young people's novels and collections of short stories, she made sure we had encyclopedias, dictionaries, and other reference books in the home. She signed me up at the local public library and encouraged me to bring home books regularly. One of my favorite genres then was science fiction, and I kept the librarians busy, signing out the early novels and collections of short stories by Isaac Asimov, Robert A. Heinlein, and Arthur C. Clarke, who were to become the known as the "Big Three." That interest in science fiction has stuck with me my entire life.

All this attention to books and reading was amplified tenfold a few years later. When I was 11 years old and my sister, Diana, was 8, my mother went back to work, eventually managing a bookshop in downtown Calgary. This was terrific for me. I was able to spend long hours hanging out in the shop reading books and occasionally meeting famous and not-so-famous authors at their book signings. When I was a little older, I worked in the shop in the summer months.

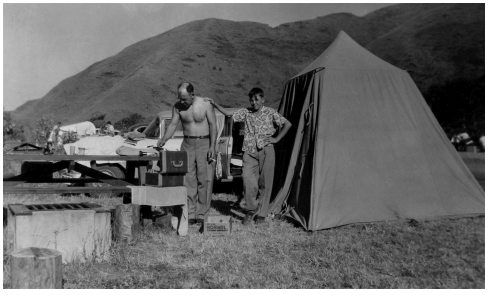


Fig. 3. My dad and me camping in the 1950s.

Reading and the natural world were foundational interests in my life. One Christmas, my parents gave me a small microscope (which I still have). Soon I was exploring the microscopic world, bringing home jars of smelly pond water and discovering all sorts of microorganisms from *Euglena* to *Daphnia*. When I was a little older, I read Paul de Kruif's *Microbe Hunters*, and by my mid-teens I had set my sights on becoming a bacteriologist, which not only echoed my burgeoning interest in the microscopic world but also sounded terribly scientific. By the time I finished high school, I was determined to become a doctor, but one who specialized in bacteriology. As I began my life as an undergraduate, however, all these plans began to unravel.

Undergraduate Years

I was what today would be called a first-generation student: I was the first of my extended family to go to university. I did well in high school and was lucky enough to win a City of Calgary scholarship. In September 1960, I was admitted to the University of Alberta in Calgary, which only that year had erected a new campus on the bald-headed prairie to the northwest of the city near the Bow River. Calling it a campus was perhaps an exaggeration. There were only two buildings that first year: the Arts and Education Building and the Science and Engineering Building. The big event of the winter was a snowball fight between the students in the two buildings. The big event of the spring was Bermuda Shorts Day, a tradition that still goes on today, in which students, particularly men, were encouraged to wear the eponymous attire to reduce the stigma of men wearing shorts and flowery shirts in public. It's hard to comprehend these days how "risqué" that was at the time.

Several things happened in my undergraduate years that changed the direction of my life. One was that I discovered psychology. On a whim, I had enrolled in an introductory psychology course taught by David Schonfield, who had recently arrived in Calgary from the United Kingdom, where he had read experimental psychology and moral sciences at Cambridge. He was the only psychologist in our small university at that time, and certainly the only one I had ever met. But he was a mesmerizing lecturer (with a great sense of humor), and I became completely captivated by experimental psychology—particularly as it related to brain function.

I also had the good fortune to study philosophy with Plato. No, not that Plato, but rather Plato Mamo, who had graduated from the prestigious Leonteio Lyceum in Athens in the 1940s. Plato had emigrated to Canada in 1949, the same year as my family, and, as a young man, had studied philosophy at the University of Toronto. After finishing his doctorate, he was recruited by the University of Alberta (Calgary), where he taught

several courses in philosophy, including ethics and Greek philosophy, both of which I took.

There were only three of us in the Greek philosophy course, so Plato decided to hold the weekly classes at his house in the early evening. It all seemed quite grown up and sophisticated. As we sat there in his living room, he queried us about our weekly readings, using what amounted to the Socratic method. Toward the end of the class, he would offer us each a glass of wine, a gesture which today would have almost certainly got him into a lot of trouble. But for me, and I suspect for the other two students as well, the whole experience was rather magical. We felt like real scholars.

But it wasn't all learning and scholarship. I became completely swept up in the social life of the university. I was only 17 when I began my first year of studies, and a young 17 at that. I began drinking, smoking, and partying hard. I even grew a scraggly beard. By my third and final year, my grades had plummeted. Medical school was no longer an option. In fact, I wasn't even certain anymore that I wanted to be a doctor (or so I told myself). I was really at loose ends, not knowing what to do with the rest of my life. I was still living at home with my parents, who were worried about me, but didn't know how to help. And I didn't make it easy for them. Most days, I was moody and unwilling to talk to them about my future. I did meet now and then with David Schonfield, who recognized my ennui and did his best to convince me that there were other options in life besides medicine, including graduate school. But in the end, after (barely) graduating, I decided to take a break from studies—and to leave Calgary altogether.

Much to my parents' consternation, I left home in the summer of 1963 and took a job working on a wheat farm near High River, Alberta. I can't remember how I got the job, but I do remember that when I showed up for work, the young farmer who had hired me, sight unseen, said he would never have taken me on had he known I had a beard. In those days, beards were rare in young men, and were associated with beatniks, poetry, jazz, coffee houses, and anti-establishment attitudes—not things typically popular with rural Albertans. But he and his young family were impressed enough with my performance over the first week that they kept me on for the entire harvest. It was exhausting work. Most days, I drove a truck alongside the combine harvester until it was full of grain, which I then took to the grain elevator in a nearby little town. Sometimes I drove the tractor, hauling straw bales back to the barn or doing some late-summer tilling. I ate dinner with the family every evening and then would stagger up the stairs to my tiny attic bedroom. I remember lying in bed trying to read F. Scott Fitzgerald's *Tender Is the Night*. After only a few pages, I would fall asleep from sheer physical exhaustion. But the story and the characters have stayed with me over the years—probably because the elegant and

exotic life of Dick and Nicole Diver seemed so very different from life on the farm.

Back to Britain

During those long days working on the farm, I began to think about going to Britain, not forever, but for an extended visit. I had no firm idea of what I wanted to do there, but I did know that I needed to get away. Eventually, with money from my work on the farm and with a "loan" from my parents, who were more understanding of my need to leave home than I perhaps deserved, I bought a ticket for the RMS *Empress of England*. After a long journey by train to Montreal, I embarked on my transatlantic journey to Liverpool in late September 1963.

I arrived in Liverpool in early October. My mother had written to my relatives in Essex to let them know I was on my way to the United Kingdom. I took the train from Liverpool to Leigh-on-Sea, where I was warmly greeted by my relatives, who were excited to meet a Canadian relative. After a few weeks living with them in Leigh, however, I had run out of interesting stories about life in western Canada, and it became clear that I was overstaying my welcome. I said my goodbyes and set off for London to look for a job and a place to stay. Fortunately, in those days, there was almost full employment in the United Kingdom and jobs were easy to come by.

Almost immediately, I landed a position as a salesclerk in Boot's on Regent Street near Picadilly Circus in the heart of London. It was a great place to work. Most days, I would get a pint and a pub lunch with my workmates in one of the many drinking establishments in the area. I worked at Boots right through the Christmas holidays. When sales slowed down in January, I was let go. But I wasn't unemployed for long. I got a job as a hospital porter at St Thomas' Teaching Hospital, right across the Thames from the Houses of Parliament. The hospital is one of the oldest in Britain. Lots of famous people have been associated with St Thomas: Florence Nightingale, who in later life established the first nursing school in Britain; William Somerset Maugham, who wrote his first novel, *Liza of Lambeth*, while he was a medical student at the hospital; and, more recently, Harold Ridley, who invented the intraocular lens and pioneered surgery for cataract patients. My stint as a hospital porter was not nearly so notable. After minimal training, I was let loose on the patients. I took them in wheelchairs from the wards to treatment facilities and back again, turned them in bed to prevent bedsores, and helped them to the toilet.

After a couple of months working at St Thomas, I decided to move on from London and explore other parts of the United Kingdom. For one thing, I had been living in a succession of less than salubrious flats, sharing with people I had met through work. One of the last flats I lived in, just

off Baker Street (of Sherlock Holmes fame), had outdoor toilets. I needed a change. Through an employment agency, I found a job in Buckinghamshire working on a rural estate during the lambing season. Because I could drive a tractor and was familiar with farm life, it turned out I was an excellent fit. Again, life on the farm was not easy. I had to get up at the crack of dawn and drive out to the fields in a tractor pulling a trailer-load of feed for the ewes. The fields were damp with heavy dew, and it was often raining. As I unloaded sacks of feed to pour into the wooden troughs, the sheep became quite excited. It was hard to keep my balance as they pushed me from behind to get at the feed. Occasionally, one of them would get between my legs, and the water in their thick wool would be squeezed out like water from a sponge and run down my legs filling up my Wellington boots. Those experiences working as a farmhand—first on a wheat farm in Alberta and then on a rural estate in Buckinghamshire—taught me a lot.

After the lambing season was over, I left Buckinghamshire, returned to London, and moved back into the same flat off Baker Street I had been sharing with friends. I eventually found a job as a laborer, hefting bags of foundry sand into the back of trucks bound for steel mills. It was back-breaking work, and my workmates were a bit unsavory. The worst part was that by the end of the day, my clothes, hair, and nether regions were completely covered in fine sand. There was no shower or bath at our flat, and after I tried rinsing myself clean in a nearby public washroom, I was banned by the cleaner who couldn't cope with all the sand I left behind. I was fed up—not only with the job but with my living conditions. I decided that it was time to return to Canada. I wrote to David Schonfield, my undergraduate mentor, asking if there was any possibility of entering the graduate program at the University of Alberta (Calgary). When I got an encouraging reply, I immediately quit my job and, because I had an open ticket, arranged to sail back to Canada in July (it was then early June). But how to spend the rest of my time in Britain?

It was nearly summer, and the days were getting warmer. For no particular reason, other than that it sounded like a nice place, I decided to hitchhike down to Torquay, a seaside town in Devon. By the end of my first day there, I had landed a job as a dishwasher in the Osborne Hotel, a luxury hotel on a beautiful Regency-style crescent overlooking the sea. My dishwashing job put me near the bottom of the kitchen staff hierarchy. Only the pot scrubber was below me in status. But after work, kitchen-staff status was forgotten. My workmates and I explored the local pubs (when we could afford it) and had riotous parties in the staff accommodation, often on the same night.

In July, I left Torquay and headed off for Liverpool and the boat back to Canada. On the way, I stopped in Staffordshire to visit my mother's relatives in Stone. They made me welcome but had some bad news. They had received a letter from my mother who had been unable to get hold of me

since I left London. My Grandfather Bloor was in the hospital with cancer. I quickly made a collect call home. My parents were relieved to hear from me after more than a month of not knowing where I was or whether I was still alive. In mid-July 1964, I boarded the RMS *Empress of Canada* in Liverpool and a week later disembarked in Montreal. My parents had arranged for me to fly from Montreal to Calgary that very day, rather than taking the train. Landing in Calgary a few hours later, I had a tearful reunion with my parents and my sister. The next morning, we visited my grandfather in the hospital. He was near death, but I think he recognized me. He died on August 2, 1964, the last of my grandparents to pass.

Graduate School in Calgary

In September 1964, I enrolled in the master's program in psychology at the University of Alberta (Calgary). David Schonfield had saved me again. He was then acting head of the department and was able to waive some of the entry requirements for me—like grades! My assigned master's thesis supervisor was a newly arrived recruit, Roderick (Rod) Cooper, who had studied what was then called physiological psychology at McGill with Donald O. Hebb, one of Canada's most famous behavioral neuroscientists. After graduating from McGill, Rod completed a postdoc at the Albert Einstein School of Medicine at the Bronx. In 1964, the same year I began graduate studies, Rod moved with his young family to Calgary, where he accepted a position in the Psychology Department. He remained there until his death in 2024.

In addition to me, Rod took on five other master's students that year (figure 4). Remarkably, all of them were destined to have successful careers, with some becoming major figures in neuroscience. We became good friends and spent a lot of time together talking science and arguing politics. Some of us are still close friends today, although as we get older, we are seeing less of one another.



Fig. 4. Standing left to right: Michael Peters, John Pinel, me, Lance Taylor, Rod Cooper. Kneeling left to right: Brian Bland, Ian Whishaw. Notice that Rod has a cigarette in his hand. In those days, people smoked anywhere and everywhere.

Rod ran a tough lab. He was in by 8 a.m. and often didn't leave until late in the evening—and he expected all of us to do the same. But we didn't complain. He was a wonderful supervisor, and we felt lucky to be in his lab. Although he came across as something of a curmudgeon, that gruff exterior betrayed a kind heart and a sly sense of humor. He once asked students to compose a poem about neurons as part of their final exam. Rod was an avid tennis player and enjoyed playing pickup basketball. What I hadn't realized at the time was that during his undergraduate years at the University of Manitoba, he was known as "Rocket Rod" on the basketball court. He and his team, the Manitoba Bisons, are enshrined in the Manitoba Basketball Hall of Fame.

As I was wondering about what to do for my master's thesis, Rod introduced me to an issue that he had investigated some years earlier: whether the disruption of a preoperatively learned brightness discrimination following a lesion of visual cortex in the rat was due to the loss of pattern vision or the loss of a stored engram. The background to this question is interesting, and harks back to early work of Karl Lashley, Hebb's doctoral supervisor, and Rod's academic grandfather. Lashley had proposed that the poor postoperative performance on a preoperatively learned brightness discrimination task after removal of visual cortex was due to the loss of a stored memory rather than a disruption of pattern vision. Lashley argued that normal rats do not always rely on pattern vision and solve brightness discrimination in the same way as animals with lesions of visual cortex: namely, using luminous flux (the total amount of light entering the eye) rather than brightness (the amount of light per unit area, which requires pattern vision). Thus, he argued, the poor performance after lesions of visual cortex must be due to a loss of the engram for the discrimination task rather than a disruption in pattern vision.

I took on this question for my thesis and managed to show that the deficit was almost certainly due to a disruption of pattern vision. The paper was published in *Psychonomic Science*, my first publication (Goodale and Cooper, 1965). I was ecstatic. By pure luck, I had discovered a topic that I found totally captivating, and one that continues to drive my research today: how the brain allows us to see and interact with the world.

In retrospect, I must admit that our reasoning and conclusions back then were somewhat naïve. For one thing, it was becoming increasingly evident that some animals with lesions of primary visual cortex were still capable of some sort of pattern vision and visually guided movements. Moreover, there were many pathways from the eye to the brain that were quite independent of the familiar geniculostriate pathway. Across the Atlantic, Larry Weiskrantz and Alan Cowey (see volume 5) then at Cambridge were exploring the remarkable residual visual abilities of monkeys with lesions of primary visual cortex. I had no idea at the time that a few years later, I would be working with them in Oxford.

Graduate School in London, Ontario

I graduated with my master's thesis in 1966. All of us in the lab had decided that we wanted to pursue a doctorate and sought advice from Rod. As it turned out, he had already decided to recommend us to the graduate program in physiological psychology at the University of Western Ontario in London, Ontario. John Pinel decided to go to McGill but the rest of us went off to Western. It was the Hebb connection that helped. The director of the program at Western, Gordon Mogenson, and two other faculty members, Cornelius (Case) Vanderwolf and Doreen Kimura, had been supervised by Hebb. Doreen's other supervisor was Brenda Milner (see volume 2), who herself had been supervised by Hebb. Rod had shared offices with Gordon, Case, and Doreen during his time at McGill. So perhaps it was no surprise that we were all accepted. (It is no doubt becoming increasingly clear to the reader that neuroscience in Canada, particularly behavioral and cognitive neuroscience, was then a small and somewhat inbred community—and at the heart of it was Donald Hebb, a regular Johnny Appleseed, spreading neuroscience across the country.)

In September 1966, I climbed on the train and began the long journey to London, Ontario. I left behind not only my parents, friends, and relatives, but also my new girlfriend, Linda Craig, an undergraduate studying English and sociology at the University of Calgary. We had met at a party in the last year of my master's program and had fallen very much in love. But now I was leaving, and we knew we wouldn't see each other until the Christmas holidays. At least we had that to look forward to.

The first person I met at Western was Gordon Mogenson who held a joint appointment in the Department of Physiology and the Department of Psychology—and was therefore well-positioned to lead the program in physiological psychology at Western, which included graduate students from both departments. He met with all the incoming graduate students individually to work out which supervisor would be the best match for them. Gordon looked a bit like a Baptist preacher. He almost always wore a dark suit and tie and had a demeanor suggesting that he would be firm but fair. But he also had a twinkle in his eye and a subtle sense of humor. Gordon was a prairie lad like me. He had been born and raised in the small town of Delisle, Saskatchewan—but he was bright and eventually ended up at McGill. When Gordon and I met that first day, we discussed the thesis work I had carried out with Rod back in Calgary, and I expressed my interest in continuing to explore vision and the visual system. As it happened, Western had just recruited a young researcher from Bob Doty's (see volume 3) lab, Andrew (Andy) Monjan, who had an interest in visually driven behavior. Gordon sent me off to meet Andy.

I found Andy in his office in the Department of Psychology, which at that time was housed in Middlesex College, a neo-Gothic sandstone and

limestone building erected in 1960. The thing that struck me first was how young Andy looked. It turned out that he was only a few years older than me and had never supervised a graduate student before. Nevertheless, when we began to discuss possible projects, we hit it off immediately. Andy told me about a paper just published in *Science* by James (Jim) Sprague (see volume 1) from the University of Pennsylvania (UPenn) that was generating a lot of excitement at the time. Sprague (1966) had shown that the failure to respond to visual stimuli in the contralesional field after unilateral damage to primary visual cortex in the cat could be rescued by making a lesion in the superior colliculus contralateral to the damaged visual cortex. Sprague speculated that the initial failure to orient to stimuli presented in visual field contralateral to the damaged visual cortex was due to a depression of function of the superior colliculus on the same side because of the absence of facilitation from ipsilateral cortical input and the influx of inhibition from the contralateral colliculus. Thus, by removing the contralateral colliculus (or splitting the intercollicular commissure), the inhibition was eliminated, allowing the return of function in the remaining ipsilateral colliculus, and with it the return of relatively normal orienting behavior. This speculation about cortical facilitation and intercollicular inhibition, however, had yet to be demonstrated by electrophysiological recording. As we talked, I realized that this was a perfect topic for my doctoral thesis. In retrospect, I now realize that Andy was gently leading me to that conclusion.

We decided to test Sprague's hypothesis in the rat rather than the cat, in part because rats were more readily available and easier to house than cats. Moreover, because we were not experienced in single-unit recording, we decided to record local field potentials, a technique that is not nearly so challenging. After some minor setbacks that first year, I was able to reliably record light-evoked potentials in the rat superior colliculus. In those initial experiments, I showed that electrolytic destruction of one superior colliculus resulted in a significant increase in the amplitude of the light-evoked potential recorded in the remaining colliculus on the other side of the brain, suggesting that there might indeed be mutual inhibition between them. So far, it seemed, Jim Sprague was right.

My electrophysiological setup was in a small windowless room on the second floor of the Collip building where those of us doing animal experiments were housed. The only downside was that the room got quite warm over the long recording sessions. There was no air-conditioning. On some days in the summer, it got so hot that I stripped down to my underwear. On one particularly warm day, the door suddenly opened and standing there was Gordon Mogenson with a group of surprised visitors. I must have been quite a sight in my underwear—my body covered in sweat with my long hair stuck to my back. Gordon didn't miss a beat. "Oh," he said. "This

is Mel Goodale, our resident hippie," quickly closing the door and continuing the tour. He never mentioned the incident again.

Those long days and nights of recording paid off. I was able to show that unilateral lesions of the visual cortex produced a depression of the light-evoked potential in the ipsilateral superior colliculus. In these same animals, subsequent destruction of the contralateral superior colliculus resulted in an increase in the amplitude of the collicular response to near "normal" levels. Taken together, my results supported Sprague's hypothesis, that the activity of the superior colliculus is subject to two opposing and tonic influences: cortico-collicular facilitation and intercollicular inhibition. I had a thesis, which I published four years later (Goodale, 1973).

The Seminar

All of the grad students and faculty members in the Physiological Psychology Program were expected to attend a weekly seminar, where we gave presentations on journal papers and our research projects. Gordon and Andy were regulars, along with several other faculty members, including Case Vanderwolf and Doreen Kimura (who were then husband and wife). The seminar was a bit intimidating, and for some of us, quite terrifying. Doreen could be quite critical—but her feedback was always spot on. Nevertheless, every time I gave a talk, my heart would be pounding, and my legs would be a little wobbly. One time I lost my train of thought completely and froze. After what seemed to be an interminable length of time, but was probably only a few seconds, Doreen saved me by saying: "You were saying, Mel," finishing my last thought for me. It was then that I realized she had the students' best interests at heart. For an amusing and informative account of Doreen's life and research career, see her acceptance speech for the 2005 Donald O. Hebb Distinguished Award from the Canadian Society for Brain, Behaviour, and Cognitive Science (Kimura, 2006).

One thing that both Case and Doreen would not tolerate was the assignment of abstract psychological concepts to particular brain regions. Instead, they emphasized the importance of studying the relationship between the brain and observable behavior—and by "behavior," they meant the actual movements made by the animal (or person). Case was particularly adamant about this. In his work on the hippocampus, he studiously avoided using hypothetical concepts like attention, memory, and perception, which he believed were often ill-defined (for an affectionate tribute to Case, see Buzsáki and Bland, 2015). This somewhat Calvinistic approach to understanding the brain, in which theoretical accounts of brain function were stripped of any flowery abstractions, stuck with me—and shaped a good deal of my own thinking about the nature and function of the visual system.

Life Outside the Lab

A few days after I arrived in London, I managed to find a small apartment above a men's clothing store on Dundas Street right in the center of town. In 1966, downtown London was a happening place with shops, bars, movie theaters, and restaurants. On Saturday night, the vibe felt very much like George Lucas's *American Graffiti*. The sidewalks were full of young people, and muscle cars cruised slowly back and forth along a neon-lit Dundas Street.

At Christmas in that first year at Western, I took the train back to Calgary. My parents were somewhat shocked at my appearance. My hair was down to my shoulders, and I had let my beard grow. Long hair was becoming the fashion among some young men in the wicked east (including me), but the trend had not yet made its way to the prairies. In fact, my appearance was so unsettling to some that, when I was out with some Calgary friends, I was summarily booted out of a popular pizza restaurant. But none of that mattered. At long last, I was reunited with my girlfriend, Linda. She was impressed with the psychedelic mini dress that came in a can (yes, that was a thing), which I had bought for her in Toronto—her parents, not so much. Over that Christmas break, we made plans for her to join me back east, and later in the new year she came to London.

Linda had not yet completed her undergraduate training at the University of Calgary but was able to enroll in some courses at Western and managed to secure enough credits to get a bachelor's degree in English and sociology. It soon became obvious that the small apartment I had rented downtown was just too small for the two of us and we moved to a much larger attic apartment in east London. In our new digs, we entertained frequently, making big pots of chili and the occasional curry. When the weather was warm, we often got together with our friends at the beach in Port Stanley on Lake Erie or the Pinery Provincial Park on Lake Huron. Life was good.

Finishing My Doctorate

In April 1969, I presented my thesis results at the 40th annual meeting of the Eastern Psychological Association (EPA) in Philadelphia. Although I was incredibly nervous (and a bit hungover from partying the night before), I managed to get through my talk at the EPA without losing my train of thought. Afterward, Alan Rosenquist, one of Jim Sprague's younger colleagues, approached me and invited me to lunch. He was pleased to see that my research had supported Sprague's (and his) ideas and questioned me closely about my work. Even though I was experiencing brain fog from all the drinking the night before, I managed to tell him what he wanted to know. I was quite flattered by the attention. At the end of the lunch, he

proposed that I visit the Sprague lab later in the year with a view to doing a postdoc there.

In October 1969, I defended my doctoral thesis. Jim Sprague was the external examiner, and I learned later that he had submitted a glowing report. The oral exam was held in a room in the medical building where, over the years, countless other doctoral exams in medical sciences had been held. The fear pheromones lingering in the room were palpable (even today I get slightly anxious when attending a meeting there). The defense went surprisingly well, despite the fact that one of my examiners admonished me (quite correctly) for citing a paper written in German, a language that I was (and still am) completely unable to read. This was directly related to the other requirement for obtaining a doctorate in those days: demonstrating that one could read scientific papers in another language. I had chosen French and, even though I was raised in Alberta where everything French was regarded with some suspicion, I managed to translate the article I'd been assigned. Once that was done, I was a full-fledged doctor of philosophy.

Shortly after I graduated, my supervisor, Andy Monjan, left Western. Perhaps it was the experience of supervising me or perhaps it was something else, but he left conventional academic life behind and completed a master's in public health at Johns Hopkins University. He ended up as the chief of the neurobiology of aging at the NIH before he retired. He also worked closely with NASA on incorporating research on aging into their space program—and played a role in a 1998 space shuttle mission in which former Project Mercury astronaut, John Glenn, returned to space for his second space flight at age 77, at that time the oldest person to have gone into space.

As for me, I had been infected by the research bug and knew that I wanted to continue life as an academic, but I had some potentially life-changing decisions to make. Although I was considering joining the Sprague lab at UPenn, I had also written to Larry Weiskrantz at Oxford about the possibility of doing a postdoc with him. Larry and his colleagues had recently demonstrated that monkeys with complete lesions of primary visual cortex could still perform a variety of visual tasks. Their work, which resonated deeply with my own ideas about how the visual system worked, was garnering a lot of attention. In the flimsy blue airletter I mailed to him, I expressed an interest in a postdoctoral position at Oxford, outlining my work on the Sprague effect. Just over a week later I found an airletter addressed to me in the graduate mailbox in the department at Western (mail was much more reliable in those days). It was from Larry. He encouraged me to come to Oxford, although he warned me that he had no funds to offer, and I would have to find my own salary. But, as it happened, I had already applied for a postdoctoral award from National Research Council (now the Natural Sciences and Engineering Research Council) and my application had been successful.

But there was still the possibility of working with Jim Sprague at UPenn. So, in July, a couple of months before my thesis defense, I visited his lab in Philadelphia at his invitation. The Sprague lab was impressive, and everyone I met including Jim were extremely welcoming. It seemed like a real opportunity. In the end though, the lure of Oxford was too strong. I wrote back to Larry and accepted his offer—and let Jim know that I wouldn't be coming to his lab. I must admit that the opportunity of going to Oxford, one of the most famous universities in the world, was a powerful draw, but ultimately, it was the chance to work with Larry that sealed the deal.

The Oxford Years

In the fall of 1969, Linda and I flew to England, excited but not knowing what to expect. We made our way to Oxford by train but soon discovered the "sweet city with her dreaming spires" (Matthew Arnold, "Thyrsis," 1866) was a difficult place to find affordable accommodation. After an unsuccessful day of looking for a decent place to stay, I rented a car and we explored possibilities in nearby towns and villages. Amazingly enough, we found a rather nice place, a detached cottage on the property of a large estate in the small village of Standlake, which was about 25 kilometers west of Oxford. Moreover, there was an efficient bus service to Oxford.

I remember clearly my first meeting with Larry. He had moved to Oxford from Cambridge only two years earlier to take up a position as head and chair of experimental psychology. A new building had been promised, but until then, the department was housed in an old three-story Victorian building on South Parks Road. Larry's office was on the top floor. To get to his office, I had to deal with a steep staircase—and I also had to get past Judith Shingler, his secretary, who always had several dogs lying on the floor around her desk. Larry's office was notable not only for its small size but also because the only fire escape was a knotted rope coiled beneath the window. Just how he, Judith Shingler, and three or four dogs would make it down the rope in the event of a fire was not clear.

In that first meeting, Larry and I discussed what I might do in the two years of my postdoc. As it turned out, we were both excited by a paper that had appeared in *Science* only a few months earlier (Schneider, 1969). The paper was remarkable, partly because the author, Gerald Schneider, was a young scientist who had only just finished his doctorate (at the Massachusetts Institute of Technology) and had managed to snag a single-authored publication in one of the world's most prestigious scientific journals—but mainly because the data reported in the paper offered a clear and apparently straightforward account of the functional differences between the two main visual pathways in the mammalian brain. According to Schneider, the ancient pathway from the eye to the midbrain

enables animals to *localize* a visual stimulus, whereas the phylogenetically newer pathway to the cerebral cortex enables them to *identify* that stimulus. Schneider's evidence for this distinction came largely from his thesis work in which he showed that Golden hamsters (*Mesocricetus auratus*) with lesions of the superior colliculus were unable to localize visual targets, even though they could identify them, whereas hamsters with lesions of primary visual cortex could localize visual targets but could not identify them.

These findings paralleled Larry's work with monkeys but offered the possibility of developing a rodent model that could be used to conduct a range of experiments that would be difficult (and definitely more expensive) with monkeys. In that initial meeting, the two of us agreed that we would first replicate Schneider's original study and then branch out from there. Larry explained that rodent housing and testing facilities were located in a building about a kilometer away on Keble Road—and that I would have office space there as well. He also suggested that I consult Alan Cowey, his former graduate student at Cambridge, now a lecturer at Oxford, who worked closely with Larry on residual visual abilities in monkeys with V1 lesions. Over the next few weeks, I sought out Alan, ordered some hamsters, and set up shop in my new lab on Keble Road, consulting Alan regularly as I moved forward with replicating Schneider's experiments.

Alan was not only a first-rate scientist, but he was also a gifted athlete. In his autobiography (see volume 5), he admits that as a young man he "flirted with the idea of a sporting life." His natural athleticism and good nature came to my rescue one chilly fall evening, when he single-handedly pushed my old Wolseley, whose battery had died, down South Parks Road until it was going fast enough for me to start it by slipping it into first gear. I remember him waving and grinning in my rear-view mirror as I drove off home.

I can't leave this part of my Oxford story without reporting another fond memory I have about my meetings with Larry. Although he had been born and raised in the United States, the many years he had spent in Oxford and Cambridge had shifted his vowels quite a bit. He sounded quite mid-Atlantic, if not English. When we got into a protracted discussion, however, my Canadian accent seemed to draw out his American—and after some time, he sounded much more like someone from Philadelphia, his hometown. On one occasion, after we had been talking for some time, either the intercom buzzed or the phone rang (I can't remember which), and Larry answered. Immediately, his accent reverted from American to Oxbridge. I don't think he even noticed. No matter what his accent might have been at any one time, our discussions were always stimulating. Larry often challenged my ideas, but he was always supportive and encouraging. Moreover, he loved wordplay. Conversations with him were marked not

only with insightful comments on research but also with unexpected puns and other amusing verbal pranks. For more about Larry, I direct you to his biography, which I had the honor of writing shortly after his death in 2018 (Goodale, 2020).

Keble Road

Several of us were crammed into the row house on Keble Road, which had been turned into a lab and office space for graduate students and postdocs. Because of our isolation from the main building, we became a tightly knit group and spent many hours, both in the common room at Keble Road and at the nearby Lamb and Flag pub on St Giles', talking politics, particularly about the Vietnam War and the Cultural Revolution in China, as well as science.

The hamster project in the lab at Keble Road was a complete dud. It took forever to get things set up, even with the expert help and guidance of Alan Cowey. The most frustrating thing was that the hamsters kept falling asleep in the training apparatus—and even when they were "awake," they seemed uninterested in doing anything. What neither Alan nor I appreciated at the time is that hamsters often enter torpor, a survival mechanism that is triggered when food is scarce and the temperature falls. In retrospect, their behavior was not surprising. Not only did we have the hamsters on a food deprivation schedule but also, in the winter months, the building at Keble Road, could be quite cold. In the end, we abandoned the whole project. The upshot was that my two years at Oxford were remarkably unproductive in terms of actual research output. Even today, except for published reminiscences (Goodale, 2000, 2018), I have no scientific papers to show for my time with Larry.

But the two years I spent at Oxford were immensely stimulating. Those were heady days in Larry's lab. He and his team had already shown that that monkeys with complete lesions of primary visual cortex (V1) could still perform a variety of visual tasks, including reaching out and picking up small objects such as raisins or discriminating among different visual patterns. We had endless discussions about why monkeys seemed to be different from humans. After similar damage to V1, patients in the clinic typically claim that they are completely blind. The disparity between the residual abilities of monkeys and humans was a real puzzle. But even as early as 1963, Larry speculated (Weiskrantz and Cowey, 1963) that the difference could be one of "seeing" vs. "experiencing." Indeed, only a few years after I had left Oxford, Larry was able to show that if patients with V1 lesions were tested the same way as monkeys — requiring them not to tell you what they saw but to simply move their eyes and hand toward a visual target presented in their blind field — they did remarkably well (much to the patients' surprise). True to form, Larry displayed his usual

wordplay by calling this paradoxical ability "blindsight" (Weiskrantz et al., 1974). The presumption was that in patients (and monkeys) with blindsight, intact pathways from the eye to other visual structures in the brain, such as the superior colliculus, were responsible for the spared visually guided behavior observed in patients (and monkeys) with V1 lesions. This was an idea I could get behind.

Life in Oxford and a New Job

Despite my scientific disappointments, Linda and I enjoyed our two years at Oxford. We made many friends, attended a lot of parties and dinners, and spent far too many evenings in the pub (at least I did), particularly after we moved from our cottage in Standlake to a flat on Abingdon Road near the center of Oxford.

By the beginning of 1971, however, as the inevitability of the end of my postdoctoral fellowship began to sink in, I realized that I had to find a job, preferably back in Canada. But here I was on the wrong side of the Atlantic with only one publication to my name. I began a flurry of letter-writing to various universities back home and received one rejection letter after another. It was not looking good—but my luck was about to change. One day, after a meeting in one of the outbuildings behind the department on South Parks Road, Larry stopped me in the garden as I was making my way back into the main building. He asked if I would be interested in a research job that had come up at the University of St Andrews. Without thinking for even a nanosecond, I said yes. I knew virtually nothing about St Andrews, and I had never been to Scotland, but a job was a job after all.

In June 1971, I set off up north for my interview at the University of St Andrews. After a long train journey, I found myself in Leuchars railway station, about 8 kilometers from St Andrews itself. As the train pulled away, all I could see were farmers' fields stretching off into the distance. The town of St Andrews was nowhere to be seen, and it wasn't immediately clear how I was to get there. As I stood there on the platform somewhat bewildered, I spotted a small red dot on the horizon to the east, slowly becoming larger as it got closer to the station. It was the local bus. I followed the other passengers and boarded the bus, arriving in St Andrews about 9:30 in the evening. It was almost the summer solstice, and the sun was still high in the sky, although the streets were almost completely empty. But, at the stroke of 10, the pubs began to empty out and suddenly the streets were full of people blinking in the bright sunlight. I followed some of them to Pete's Café at the end of Market Street, where I devoured some fish and chips, before settling down in the room provided for me in St Salvator's Hall.

After breakfast the next morning, I made my way to the Hebdomadar's Room in St Salvator's quadrangle where the interview was to take place.

The hiring committee, chaired by Malcolm Jeeves, head of the School of Psychology, was waiting for me. I was nervous, but all went well—even though, when asked why I wanted to stay on that side of the Atlantic, I replied that I was rather fond of England. It was perhaps not the best thing to say while being interviewed for a job at a Scottish university founded by Papal Bull in 1413. Fortunately, I did have the imprimatur of Larry Weiskrantz (and Oxford) working for me. At any rate, without much further discussion, I was offered the job. I later learned that I was the only candidate.

When I returned to Oxford, Linda and I began to make plans for our move to Scotland. She had decided that she wanted to pursue a career in social work and began to investigate opportunities in nearby Dundee, where the university had a program in social administration. We also decided it was time to make our relationship official. We were married at the Oxford Registry Office that summer and set off for St Andrews in late August in our old Wolseley.

Our Arrival in St Andrews

When we finally made it to St Andrews, we discovered that the hotels were quite expensive. St Andrews is not just a university town; it's the "home of golf." St Andrews' famous Old Course can be traced as far back as the 1500s, perhaps earlier. The town caters to golfers and even the cheapest B&Bs were beyond our means. So, that evening, we pitched a tent at Kinkell Braes, a caravan site high up on a hill overlooking the town.

The next morning, I made my way to the School of Psychology and introduced myself. Everyone was welcoming. In fact, one of my new colleagues had heard we were living in a tent and kindly offered up his house, which was in the center of town. He and his wife were about to go on holiday, and it suited them to have someone looking after their place. And it certainly suited us. Within a couple of days, we found a cottage just outside St Andrews that we could (barely) afford. Linda and I soon settled into our new life. In September, Linda began her program in Social Administration at the University of Dundee, which was across the Firth of Tay about 13 kilometers from St Andrews. I began preparing for teaching and getting to know my new colleagues in the School of Psychology. We were a remarkably young group. With the exception of Malcolm, who was in his mid-forties, we were all in our twenties.

To be fair, that wasn't strictly true. There was also Henry Ferguson, who retired the year I arrived. He had been appointed to a lectureship in 1947. When almost the entire department relocated to Queen's College of St Andrews in Dundee in 1959, it was Henry who stayed behind and kept psychology afloat on the main Saint Andrews campus. When Queen's College became the University of Dundee in 1967, senior administration

of the University of St Andrews realized that experimental psychology, which was fast becoming a popular and influential discipline, needed to be preserved, particularly since the last standing psychologist on site, Henry Ferguson, was nearing retirement. After some consultation, they appointed Malcolm Jeeves as Foundation Professor of Psychology in 1969. Malcolm had completed his doctorate at Cambridge under the supervision of Frederick Bartlett, the university's first professor of experimental psychology. Not long after completing his degree, Malcolm had gone off to Australia where, as a relatively young man, he established the Department of Psychology at the University of Adelaide. The senior administration at St Andrews felt that Malcolm had the background and experience to lead St Andrews' newly established School of Psychology. Malcolm didn't waste any time. Over the next few years, he recruited several young and talented researchers, many of whom went on to make significant and highly visible contributions to psychology and neuroscience.

Teaching at St Andrews

Even though I had been ostensibly hired as a research fellow, it soon became apparent that I was to carry a teaching load that was equivalent to that of the lecturers in the department. I didn't mind. Malcolm had given strong hints that, if all went well, I would be "promoted" to a lectureship at the end of the year. Even so, my salary as a research fellow was certainly not equivalent to that of a lecturer. In fact, I had taken a considerable a cut in pay after leaving Oxford, where I was on Canadian dollars and earned $6,000 a year plus travel support. Indeed, as far as I could figure out, the only person who earned more than me during my years at Oxford was Larry. Fortunately, as Malcolm promised, I was promoted to university lecturer at year's end, and my salary was increased to nearly a living wage. That year, Linda and I left the cottage at Claremont Farm and moved into a spacious university flat in town. As my salary increased over the years, we managed to buy a small house in Market Street, right in the center of St Andrews.

The two courses I taught in my first year and for several years after that were Introductory Psychology and Comparative Psychology. The Introductory Psychology course was the most intimidating. I had never taught a large undergraduate class before, and I was dreading the first lecture. I wrote the whole thing out on lined paper with lots of cello-taped corrections. The morning of the class I walked into the lecture hall at St Salvator's to find a hundred or so students waiting eagerly for the first lecture of the year. After a brief introduction in which I explained that I was a Canadian, to prepare them for my accent, I read out my carefully prepared lecture word for word. To my horror I was finished in under 20 minutes, even though the class was scheduled to run an hour.

I panicked. I couldn't simply let them go. "Tell you what," I said, "Let's do that again." And without missing a beat, I delivered the lecture once more, this time a little more slowly. To this day, I can't understand what possessed me to do that. I could have asked them if they had any questions. I could have encouraged some discussion. Nope. I decided to give the lecture twice. The class did not seem to mind, at least outwardly. Perhaps they were grateful. Perhaps they thought this was common practice in university. After all they were first-year students. I have no idea. One thing was clear though. It was not an auspicious beginning to my teaching career.

The Comparative Psychology class went much better. There were only 20 or so students in the class and my plan was to have as much discussion as possible. Moreover, it gave me an opportunity to incorporate biology and evolution into the course material. At that time, the American tradition of comparative psychology was still dominated by the theoretical writings of comparative psychologists like Edward L. Thorndike and Morton E. Bitterman. Their approach was to study differences in the learning and cognitive abilities of different species by training them on the same basic operant tasks. Little or no attention was paid to a species phylogeny or the ecological niche it inhabited—only how rapidly they could learn a task under different training regimens. This approach stood in stark contrast to the work of European ethologists such as Nikolaas Tinbergen, Konrad Lorenz, and Karl von Frisch, who studied the complex behavior of organisms in their natural world. I was particularly influenced by the ideas of Tinbergen, who was Professor Emeritus at Oxford when I was there.

I built the course around Tinbergen's four questions about behavior: causation (mechanism), development (ontogeny), evolution (phylogeny), and function (adaptation). The first two questions are essentially about the proximate causes of behavior and the last two are about the ultimate causes. As I have grown older, I have begun to appreciate how much his ideas have infused my thinking about the relationship between the brain and behavior.

As far as I could tell, the class enjoyed the course. We had lots of spirited discussions about the "causes" of behavior, the dangers of just-so stories, and the complex lives of honeybees, sticklebacks, and herring gulls. One student always had questions. That student was David Perrett, who, after graduating, went on to complete a doctorate at Oxford with Edmund Rolls, exploring face cells in monkey, first reported by Charlie Gross (see volume 6) at Princeton. Dave ended up returning to St Andrews, where he is now professor emeritus in the renamed School of Psychology and Neuroscience. He was only one of a good number of undergraduate and graduate students at St Andrews who went on to make their mark in research.

Collaborating with David Milner

I met David Milner shortly after arriving in St Andrews. He had been hired by Malcolm the year before I arrived. David was an Oxford man too (even more than I was; I had been just a postdoc, but he was an undergraduate there). After finishing his studies at Oxford, David spent just over a year at the Maudsley Hospital in London, where he completed a program in clinical psychology. Although he seemed destined for a career as a clinician, it was at the Maudsley that he developed a passion for research, particularly research in brain and behavior. He met George Ettlinger, who worked at the nearby Institute of Psychiatry, and often shared a coffee with colleagues at the Maudsley. Learning of David's burgeoning interest in the brain, George suggested that David do a doctorate with him. Ettlinger was a leading figure in human neuropsychology in the United Kingdom and was also a pioneer in the use of nonhuman primate models of human cognition. After David completed his doctorate, he dallied with the idea of doing a postdoc at a nonhuman primate lab in Cambridge, but in the end, he decided to take up a lectureship that Malcolm offered him at St Andrews, where he was promised the opportunity of creating a nonhuman primate facility.

David's research interests and mine complemented each other's rather nicely, and we soon became fast friends and collaborators. In some ways, our friendship was unusual. David was already a family man. He and his wife, Christine, had two young boys and, not surprisingly, were homebodies. Linda and I were free of these domestic responsibilities—and spent far more time in the pub and at parties, made frequent trips to Edinburgh on the weekend, and took holidays on the continent. When David and I talked science, however, we resonated completely.

Even before David and I began to collaborate on research, I was revisiting the question I had tried to address (unsuccessfully) during my two years at Oxford: What were the respective roles of the visual cortex and superior colliculus in the control of visually guided behavior? As mentioned earlier, the original "two visual systems" model put forward by Schneider represented a significant departure from earlier descriptions of how vision worked, which had tended to treat the visual system as a single, albeit complex, entity. But even though Schneider had taken a more modular approach, the term "localization" that he used to characterize the function of the superior colliculus seemed poorly defined and vague. After all, an animal might localize a piece of food while foraging, it might localize the edge of a barrier during locomotion, or it might localize and turn away from a sudden looming shadow. Each of these disparate behaviors could be considered as localization, but not all would necessarily involve the colliculus. I suspected that lesions of the superior colliculus in the rat would interfere with rapid orientation toward or away from a peripheral visual stimulus but not necessarily other forms of localization.

To test this idea, I carried out my very first experiment in the brand-spanking new animal building, which I am proud to say I had helped design and shepherd through the Home Office's approval process. The building was located down by St Andrews' small harbor next to the famous Gatty Marine Lab. I was able to devise a setup that allowed me to film rats from above (not hamsters; I had learned my lesson at Oxford) as they ran back and forth from one set of nose-push doors to another at opposite ends of the apparatus for liquid food reward. One of the five doors at each end would illuminate and if the rat pushed open that door, the rat would gain access to some evaporated milk. I knew from previous work that rats with lesions of visual cortex were capable of a brightness (or luminous flux) discrimination. Here, I was interested in whether they would run directly to the illuminated door. The entire thing was automated and worked remarkably well. In a second experiment, I introduced distracting visual stimuli in the visual periphery that would suddenly illuminate as the animal ran toward the illuminated door. In carrying out these experiments, I had the help of a clever psychology honors student, Robert Murison, who went on to complete a doctorate at the University of Leicester, and is now a professor emeritus at the University of Bergen in Norway.

The results of the experiment were clear and unequivocal. Rats with bilateral lesions of the superior colliculus ran directly toward the illuminated doors, often more efficiently than unoperated animals. In sharp contrast, rats with bilateral lesions of the visual cortex had real problems localizing the illuminated door and often ran straight ahead, until they reached the wall where the five doors were located. Only then did they manage to find the illuminated one. It was a very different story with the peripheral distractors. The rats with collicular lesions were completely unfazed by the flashing light and continued to run toward the illuminated door without stopping. The rats with lesions of visual cortex were just like the unoperated controls. They oriented toward (or sometimes away) from the flashing light or froze in their tracks. As we concluded in our paper (Goodale and Murison, 1975), although the superior colliculus plays a pivotal role in orienting to (or away from) a sudden peripheral stimulus, it is not essential for the visual control of locomotor orientation. In contrast, visual cortex contributes to the guidance of locomotor movements but does not play a significant role in the control of the orienting reflex. These results made it clear that Schneider's "localization vs. identification" distinction between the functions of the superior colliculus and visual cortex was almost certainly premature.

I conveyed my excitement about the findings with David, and as our conversations continued, we soon conceived a whole program of research aimed at investigating what the superior colliculus might be contributing to visually driven behavior. Although David's nonhuman primate lab was up and running, the idea of collaborating on projects with rats for

which the investment in time and energy was far less than it would be with macaques was enticing. Moreover, David and I shared many of the same ideas about the functional organization of the visual system, including the role of the superior colliculus in orientation and shifts of attention. We were convinced that we were on the right track and applied for a grant from the then Science Research Council (SRC) in 1973. In addition to the proposed rat work, we also included some ambitious monkey experiments. To our delight, our application was successful, but the funding was conditional on us seeking advice from Larry Weiskrantz about how best to approach the research questions we wanted to address. We dutifully made the journey down to Oxford. After a congenial meeting with Larry in which we discussed our research plans, he made some insightful suggestions, signed off on the grant, took us to lunch, and wished us well.

The work with David Milner went remarkably well. We published several papers together on the role of the superior colliculus in visually guided behavior, many of which continue to be cited. I also had the good fortune to supervise a talented graduate student, Jefferson Alden Graves, an American who had completed a master of letters in logic and metaphysics at St Andrews on a Fulbright scholarship. Although Jeff had trained as a philosopher, his real passion was the study of animal behavior, particularly in the wild. He approached me about doing a doctorate in animal behavior, but I made it clear that I didn't feel comfortable supervising fieldwork. So, we compromised and embarked on a project looking at interocular transfer of training in the pigeon (*Columba livia*), a topic that certainly interested me and I hoped him as well. The experiments showed that the presence or absence of interocular transfer depended on the part of the retina that the pigeons were using to discriminate between the stimuli and thus which visual pathways were engaged (Graves and Goodale, 1977). Jeff duly got his doctorate and went on to have a successful research career in the School of Biology at St Andrews, studying behavior in a range of animal species both in the field and in the lab. For my part, I continued to work on the organization of the visual system in the pigeon well after I left St Andrews.

Back to Canada (via Cambridge, Massachusetts)

Although my academic career was going well, the same could not be said for the relationship between Linda and me. She was spending more time in Edinburgh, where she had moved to complete a diploma in social administration, and was becoming interested in psychodynamic therapy. She returned to St Andrews only on the weekends or I went to her flat in Edinburgh. I was spending more and more time working in the lab, writing papers, and drinking in the pub (perhaps more than I should have). Our lives and careers were going in very different directions. In the end, we

separated. Linda moved entirely to Edinburgh, where she eventually got a position at the Royal Hospital for Sick Children in Edinburgh, and I stayed in St Andrews, a rather unhappy man.

I was becoming increasingly homesick for Canada and yearned for the life I remembered (perhaps too fondly) from my graduate student days at Western. An opportunity to visit Canada arose in 1976. I was contacted by David Ingle, who was then working at the Harvard Medical School. He had read my 1975 paper with Robert Murison and was pleased to see that our findings were consistent with the conclusions of his recent paper, "Two Visual Systems in the Frog," which had appeared in *Science* in 1973. In contrast to Schneider's much more abstract account of the functions of the superior colliculus, Ingle (1973) was quite specific in stating that different visually guided movements are linked to distinct and functionally separate visuomotor pathways from the eye to motor nuclei in the frog's brain. According to Ingle, one set of pathways projecting from the eye to the optic tectum (homologous to the superior colliculus in mammals) mediates visually guided snapping at prey and jumping away from looming visual threats, while a separate set of pathways projecting to the pretectum, just anterior to the optic tectum, mediates barrier avoidance during locomotion. Not surprisingly, his take on the organization of the visual system made great sense to me. It appealed to my strong belief that the brain's primary job is to generate and control an animal's movements.

In any case, David was clearly taken with our work on visuomotor behavior in the rat and invited me to take part in a symposium on visually guided behavior that was going to be held alongside the Sixth Annual Meeting of the Society of Neuroscience in Toronto, the first meeting of the society to be held in Canada. I immediately said yes, not only because I was keen to attend the symposium but also because it gave me an opportunity to visit Western, which was only a couple of hours away by road from Toronto.

The SfN meeting was amazing. At the symposium and elsewhere at the meeting, I was able to talk science for the first time with researchers like Harvey Karten, Vivian Casagrande, and Jack Pettigrew, whose papers I had read. I also attended plenary talks by David Hubel (see volume 1), Patricia Goldman-Rakic, Walle Nauta, and other luminaries. After the meeting, I took a bus to London and visited Western, where I had been invited to give a colloquium. I was quite nervous about presenting my work to former colleagues and mentors, particularly when, in this case, they included Doreen Kimura and Case Vanderwolf. To my great relief, the talk went well—and I received a number of compliments from several members of the audience, including the then chair of the Psychology Department, Bill McClelland, who I hadn't realized was in the audience. That evening, I went out for dinner and drinks at a local steakhouse with Doreen, Case,

and several graduate students and postdocs. After dinner, some of us went dancing at a club. At that moment, I realized that my memories of life in London were pretty accurate. It was indeed a far more "happening place" than St Andrews.

I returned to St Andrews in late November, not looking forward to the long winter in Scotland where I had to turn on my office lights by 3 p.m. Not long after I got back, however, two things happened that lifted my spirits enormously.

First, the department secretary knocked on my office door and told me that I had a long-distance call from Canada. In those days, none of us had our own phone, and we had to use the one in the main office. I ran downstairs to the office, picked up the phone, and was surprised to hear Bill McClelland at the other end. He said something like: "Mel, would you like a job at Western? We have a position in physiological psychology available." I could scarcely believe it. I was being offered a job on the phone! Without hesitation, I said yes. A week or so later, an official letter of offer arrived. The position as associate professor was to begin on July 1, 1977. What a change of fortune.

Second, David Ingle contacted me again and asked if I would be interested in working in his lab at McLean Hospital, an affiliate of the Harvard Medical School, over the summer of 1977. He said he could provide support for me. After clearing this possibility with Bill McClelland, I said yes. My teaching at Western was not scheduled to begin until September, and Bill thought that spending some time in Ingle's lab would be good for me.

As the summer approached, I said my goodbyes to my friends and colleagues at St Andrews. David Milner and I agreed that we would keep collaboration going, even though we would be separated by the Atlantic. For one thing, our SRC grant had been renewed for another couple of years. Little did I know then that our work together would not only flourish but also last a lifetime.

In July 1977, I flew to Boston. David Ingle picked me up and put me up for the night at his place in Cambridge. The next day he took me to a small apartment on Massachusetts Avenue that a grad student, who had gone off to California for the summer, had generously offered as a place for me to stay. It was not the most salubrious of apartments. It was rather small with little natural light, sat right over an ice cream shop, and was infested with cockroaches—but it had two enormous advantages: It was free and it was just a stone's throw from Harvard Square. I had a great two months exploring the bookshops, restaurants, and bars in the square—and getting to know the Harvard campus.

David's lab was based in McLean Hospital in Belmont, made famous by Susan Kaysen's 1993 memoir, *Girl, Interrupted*, later adapted as a movie starring Winona Ryder and Angelina Jolie. The lab was in the basement

of one of the hospital buildings. David was still working with frogs but had also begun to study behavior in Mongolian gerbils (*Meriones unguiculatus*), the same animal found in pet stores. I often wondered what some of the patients would have thought had they wandered into the building's basement where one could hear the croaking of frogs and the scrabbling of gerbils.

The two months I spent in Cambridge were not enough to conduct experiments, but David and I had lots of time to talk about the organization of the visual system and where the field was going. I was also able to help David and one of his colleagues at Harvard, Richard Mansfield, plan a meeting on some of the recent advances in the study of visually guided behavior, which was held a year later at Brandeis University. Speakers included Jörg-Peter Ewert, Tom Collett, Barrie Frost, Jacques Paillard, Marc Jeannerod, David Lee, Jim Sprague, Paul Dean, Giovanni Berlucchi (see volume 8), and many other leading vision researchers. The presentation I remember best was given by Leslie Ungerleider, entitled "Two Cortical Visual Systems," which she later published with her postdoctoral supervisor, Mortimer Mishkin, in the conference proceedings of the meeting (Ungerleider and Mishkin, 1982). Leslie proposed a functional distinction between the two prominent "streams" of visual pathways arising from early visual cortex in the primate brain that was reminiscent of the earlier distinction put forward by Gerry Schneider between visual cortex and the superior colliculus. According to Leslie and Mort, "The ventral or occipitotemporal pathway is specialized for object perception (identifying *what* an object is), whereas the dorsal or occipitoparietal pathway is specialized for spatial perception (locating *where* an object is)." This idea was to have a profound effect on my own thinking about the organization of the cortical visual pathways a decade later.

Returning to Western

At the end of the summer of 1977, I set off for the University of Western Ontario, not knowing at the time that I was to spend the rest of my academic life there. The campus had changed enormously over the eight years I had been in the United Kingdom, with several large buildings now filling spaces that had been open fields in 1969. The Department of Psychology was no longer in Middlesex College but was housed on the top floors of a new nine-story building, the Social Science Center (SSC). The SSC had been built in the concrete brutalism style, like so many university buildings in Canada and elsewhere that were being erected in the early 1970s. Psychology departments were expanding rapidly at the same time and hence were commonly found in such buildings. But I couldn't complain. I was assigned a large office and had more than adequate lab space and animal housing.

Early Research with Animals at Western

Having become familiar with gerbils in David Ingle's lab, I was determined to use them as my animal model for studying the visual control of movement. They offered a couple of big advantages over rats: They have nearly twice the rat's visual acuity and most important they don't freeze when an unexpected noise occurs. Thus, they were better suited for experiments on vision and did not waste precious film by "freezing on set." I soon discovered that they had another huge advantage, particularly for me: I was not allergic to them. Like a lot of researchers who worked with rats, I had developed a strong allergy, including breathing problems and contact dermatitis. I had to be very careful to limit my time in the same room as rats and to wash my hands regularly after handling them. Not so with gerbils. I had no sign of any allergies whatsoever, and I soon had several lines of research going.

In the first series of experiments, I looked at the effect of lesions of the superior colliculus, the pretectum, and the primary visual cortex in a range of different visually guided movements in the gerbil, including running toward targets in an open arena, barrier avoidance, and reflexive orienting toward peripheral stimuli. The results of these experiments converged nicely on our earlier work in St Andrews (for a review of all the gerbil and rat experiments, see Goodale and Milner, 1982). In a later set of experiments with Colin Ellard, an exceptionally bright doctoral student, I explored the role of the brainstem projections from the gerbil's superior colliculus in the avoidance of an overhead visual threat, orientation toward a visual stimulus, and negotiation of a large barrier (Ellard and Goodale, 1988). In all these experiments, we used cinematography and, later, videography to record the animals' movements. Taken together, the results of all our experiments in the rat and gerbil made it clear that the functional organization of the subcortical visual pathways in the mammalian brain is remarkably similar to what is seen in the amphibians, again reinforcing the idea that different classes of visually guided behavior are mediated by separate visuomotor pathways—and that this organization is phylogenetically ancient. I should add that Colin is now professor emeritus at the University of Waterloo, where he carries out research that sits at the intersection of psychology, architecture, and urban design. I like to think (perhaps fancifully) that his early work on the "visual ecology" of the Mongolian gerbil shaped his thinking about how the urban landscape affects human behavior.

In those early days at Western, I continued to analyze the data that Jeff Graves and I had collected on how scanning patterns in pigeons affect interocular transfer (Goodale and Graves, 1980). At the same time, I began a new project at Western, in which I used high-speed cinematography to analyze the sequence of head movements made by pigeons pecking at small

visual targets and to determine where on the retina the target fell at different points in the sequence (Goodale, 1983a).

This approach to the study of the visual control of an animal's movements stemmed from my conviction that vision evolved initially for the distal control of action, an idea largely ignored by most visual neuroscientists, who were focused instead on its role in perception and our cognitive life. I set out this view in a book chapter (Goodale, 1983b) titled "Vision as a Sensorimotor System," which appeared in *Behavioral Approaches to Brain Research*, edited by Terry Robinson, a former graduate student of Case Vanderwolf. In that chapter, I wrote: "Most contemporary theories of perception share with Descartes the view that the function of the visual system is to provide us with an internal representation of the outside world and the objects and events within it, [resulting] in a theoretical stance and a methodological tradition that have limited our understanding of how the neural pathways comprising the visual system are organized."

I was convinced that a complete understanding of our visual system would also require going well beyond studying performance on perceptual tasks. Yet, with the notable exception of eye movements, which were typically regarded as an information-seeking adjunct to perception, little attention had been paid to the role of vision in the control of our actions, particularly movements of our hands and limbs. This problem was compounded by the fact that motor physiologists studying limb movements typically regarded vision as a black box that delivered the requisite information. The notion of dedicated visuomotor pathways was not something that was being explored. In fact, 30 years ago, there was a sharp division between research on sensory and motor systems in classical psychology and physiology. The chapters in undergraduate textbooks dealing with vision were quite separate from those discussing motor control, and similar divisions existed in scientific societies and journals. The occasional meeting brought together researchers from both fields but, in general, research on the visual and motor systems were largely isolated from one another. These considerations led me to embark on a research journey in which I moved from using animal models of the human visual system to studying humans themselves. Amid this change in research direction, my life was changing in other ways as well.

True Romance

Returning to my alma mater, I reconnected with old friends and made new ones. Like many psychology departments in the 1970s, the department at Western was in a rapid-growth phase. It turned out I was not the only one hired on the phone. There were a lot of young faculty members and, not surprisingly, a lot of social events. On Friday afternoons, we typically repaired to the Faculty Club or the newly established Graduate Student

Pub in the old cafeteria in Middlesex College. Bill McClelland, the chair of the department, hosted an (in)famous welcome-back party at the beginning of the fall term, and an annual Halloween party was hosted (fittingly) by the social psychology area.

It was at one of these Halloween parties in 1979 that I met a smart and beautiful young woman, Joan Finegan, who was then a graduate student in the social psychology area. She came as a flower dressed in green leotards with white cardboard daisy petals arranged around her face. I, perhaps appropriately, came as an Anglican missionary from the South Pacific dressed in a white linen suit, black shirt, clerical collar, and a Panama hat (think Somerset Maugham's 1921 short story *Rain*). We immediately found ourselves attracted to one another and from that evening onward our romance began—and has continued to this day. Nearly 50 years later, we have three wonderful children and four grandchildren (with perhaps more to come).

Research with Humans

Although I had been working with animal models of visuomotor behavior for more than a decade, I was determined to extend this approach directly to humans—skipping the modeling part. An opportunity came when Doreen Kimura helped me secure a small room on the seventh floor of University Hospital where I could test in-patients in the neurology and neurosurgery wards. Calling it a room is a bit of an exaggeration; it was more like a broom closet. But it was big enough to house some equipment for recording eye and limb movements, a patient in a wheelchair, and a single experimenter.

I had just recruited a new graduate student, John Fisk, who had some experience recording eye movements and was keen to use his skills to study eye-hand coordination in visually guided reaching, first in healthy volunteers and then, if everything went well, in patients with right- vs. left-hemisphere strokes. In the first experiment with healthy volunteers, we showed that the timing of pointing movements to a visual target determined the timing of the saccadic eye movement preceding the limb movements (Fisk and Goodale, 1985). This finding laid the groundwork for later work in our lab (and others) on target-directed reaching.

Although this initial experiment went reasonably quickly, the same was not true for the experiment with patients (Fisk and Goodale, 1988). I was used to experimenting with rodents and pigeons, where one could be reasonably confident that once an animal was placed in the apparatus it would complete the testing session. I soon discovered that this was not the case with patients. First, there was the problem of calibrating the eye tracker. It took far longer than with healthy volunteers. Second, there were the constant interruptions. Often, after spending 20 minutes or more

calibrating the equipment, the patient would announce that they had to visit the restroom. Once they were done, I would have to go through the whole process of calibration again. Then, halfway through testing, which had already been delayed, an orderly would arrive to take the patient to scheduled physiotherapy. After a particularly frustrating day of testing, I apparently remarked to Joan that I was fed up working with patients and that I was going to go back to working entirely with animals. But in the end, I persisted, gaining a new respect for neuropsychologists.

Neuroscience Around the World

In late 1981, I was excited to receive a letter from Marc Jeannerod, then director of the Institut National de la Santé et de la Recherche Médicale (INSERM) Unité 94 in Bron (a commune of Lyon), inviting me to take part in a workshop on spatially oriented behavior that he and Alan Hein were organizing in Lyon the following year. I quickly wrote back accepting the invitation. I had met Marc at the meeting David Ingle had organized in Brandeis University, but that was not the first time our paths had crossed. Our first encounter nearly 10 years earlier was rather embarrassing, at least for me.

When I was at St Andrews, Hans-Lukas Teuber, head of the Department of Psychology at the Massachusetts Institute of Technology, came for a visit in early 1972 at the invitation of Malcom Jeeves. During his talk, Teuber mentioned the work of Gerry Schneider. At the reception afterward, I shared with Teuber my doubts about Gerry's account of the function of the superior colliculus. Teuber not only listened attentively, but he suggested that I attend an upcoming meeting of the International Neuropsychology Symposium (INS), where he was chairing a session on subcortical vision. I knew nothing about the INS, which (I learned later) is essentially a private, invitation-only, group that meets in June every year, usually somewhere in the Mediterranean.

Teuber told me when and where the meeting was being held and encouraged me to come. He assured me that it would be okay. So, in June 1972, together with a couple of friends, I set off by car for the small town of Puyricard, near Aix-en-Provence, where the INS meeting was to be held. The plan was to have a camping holiday during which I could attend the INS meeting. After we set up camp nearby, I showed up at the hotel where the meeting was being held. I wandered about the place, eventually found the meeting room, and sat quietly in the back during the first session, which, as I remember, was on the comparative neuropsychology of human and nonhuman primates. It was a terrific session. During the coffee break, I was approached by an elegant and soft-spoken Frenchman, who turned out to be Marc Jeannerod. He was the local organizer of the meeting. Marc politely asked me who I was and why I was there. As I told my story, it

soon became evident that Teuber had failed to mention that he had invited me. My guess is that he forgot. Marc gently explained that the meeting was for members and invited speakers only. He was sympathetic to my situation, however, and agreed to turn a blind eye to my presence. At Teuber's symposium, I was able to raise my misgivings about Gerry Schneider's characterization of the functions of the superior colliculus. So, all was good in the end. Moreover, I had the distinction of being the only one at the meeting staying in a tent.

Ten years later, things were different. This time I was an invitee at the workshop in Lyon, and I wasn't staying in a tent. Marc had arranged to hold our workshop in the home of the Frères Lumière, who were pioneers in cinematography. The building, now a museum, is stunning, decorated in both art nouveau and art deco style. The workshop was excellent with an impressive lineup of speakers, including Ken Nakayama, Bruce Bridgeman, Emilio Bizzi (see volume 6), Richard Held (see volume 6), Len Matin, Peter Hoffman, David Sparks (see volume 12), and, of course, Alan and Marc (for the proceedings, see Hein and Jeannerod, 1983). Our social dinner took place at the country estate where Claude Bernard, widely regarded as the father of modern physiology, was born. The estate still produced its own wine (Claude Bernard's father was a winemaker), which was served at our dinner. All in all, it was a memorable meeting. It was at this meeting that I raised with Marc the possibility of spending some time in his lab in Lyon during my upcoming sabbatical in 1983. He kindly agreed.

When I returned home and discussed this with Joan, we began to put together a rather bold plan. She and I had already decided to get married and had scheduled our wedding for October 1982 (figure 5). We were thinking about some sort of extended honeymoon starting in the new year while I was on sabbatical. So, we came up with the idea of spending the entirety of 1983 circumnavigating the globe, ending up in Lyon in the fall before returning home for Christmas. The notion was that while I was working in the lab, Joan would work on her doctoral proposal.



Fig. 5. Wedding day, October 8, 1982.

We found an understanding travel agent, who secured for us two round-the-world tickets, a fat booklet of airline coupons that we could use to purchase tickets from almost any airline as we traveled west from Toronto until we arrived back again from the east. We could fly to any destination with an airport, provided we didn't backtrack. In those days, there was no internet, and almost no businesses accepted credit cards. I had an American Express card, which allowed us to pick up mail in their offices in major cities and to cash a cheque every three weeks for $1,000 in local currency. Otherwise, we were dependent on traveler's checks.

Our plan was to fly first to Australia, where I had arranged to spend some time in Jack Pettigrew's lab at the National Vision Research Institute (NVRI) at the University of Melbourne. I was familiar with Jack's work on binocular processing in the owl, which resonated with my own work in the pigeon. I thought that I could learn a lot by spending some time in his lab—and it would give us a great opportunity to explore Australia.

On December 26, 1982, we set off, with nothing but two large backpacks and considerable excitement. We took our time getting to Australia. Our first port of call was Tahiti, where we spent a few jet-lagged days before flying to Bora Bora for New Year's Eve. After that, we flew to Rarotonga in the Cook Islands. These three destinations were the only ones where the travel agent had booked hotels for us. After that we were on our own. We left the Cook Islands for Fiji, and then New Zealand, before arriving in Australia at the end of January 1983.

We were picked up at Melbourne airport by Barrie Frost, a friend and colleague from Queen's University in Canada, who by coincidence, was also spending a sabbatical in Jack Pettigrew's lab. Barrie drove us to a lovely apartment near the NVRI where Jack had arranged for us to stay. It was a treat to have so much space—and a kitchen—after spending a month living in hotel rooms or camping.

I met Jack in his lab the next day. I had been forewarned that he was a larger-than-life character, and Jack didn't disappoint. He had a huge bushy beard, was wearing shorts, and almost immediately asked me lots of questions about my research—all the while showing me around his lab, which was crammed with pieces of equipment, books, and papers with no evident organization. Jack always wore shorts—even when he was invited to speak at the Royal Society of London. The only time I (and others in his lab) remember Jack in anything but shorts was toward the end of our stay in Melbourne, when, as a joke, he wore an impressive black suit, white shirt, and a tie for his farewell talk just before leaving to take up the chair in physiology at the University of Queensland. Jack might have been a character, but he was a wonderful host. Joan particularly remembers him pointing out all the different constellations, including the Southern Cross, while we were standing in his backyard one night looking at an unfamiliar

sky. Sadly, Jack was killed in a car accident in 2019. For more about Jack, see this tribute from his good friend and colleague Mike Calford (2020).

During the four months I spent in Jack's lab, I worked mostly with Barrie, who, like Jack, was an expert on avian electrophysiology and behavior. After some discussion, we decided to see if we could locate neurons in the visual Wulst of the pigeon that were sensitive to visual stimuli presented to the red field. The red field is a specific region in the upper temporal quadrant of the pigeon retina that has a high density of red and orange oil droplets in the photoreceptors, a feature that is thought to enhance color vision. The red field corresponds to the pigeon's high acuity binocular field in front of and below the beak and is believed to play a critical role in detecting and pecking at small objects (Goodale, 1983a). After some unsuccessful exploration of visual activity in the Wulst to stimuli presented on a conventional projection screen some distance away, we had the bright idea of presenting the visual stimuli 10 to 15 centimeters from the pigeon's eye, at roughly the same distance a seed or other piece of food would typically be located as a pigeon foraged. Now we were able to record neurons reliably, although we were surprised that we did not find many binocular cells. We reasoned that in our anaesthetized preparations, the binocular overlap was only around 20 degrees, whereas in the alert pigeon the converged eyes doubled the overlap to more than 40 degrees. We speculated that monocular processing might be favored when the eyes are diverged, even though there is still some binocular overlap. Unfortunately, we were unable to follow up on this idea. Barrie's stint at the lab and mine were coming to an end, and Jack was preoccupied with moving to the University of Queensland. Nevertheless, Barrie was able to present our work at the 1983 Society for Neuroscience meeting (Frost et al., 1983).

Joan and I took full advantage of our time in Australia, exploring as much of the country as we could, including the spectacular coastline along the Southern Ocean, the arid outback of the Flinders Ranges, and the lush tropics of northern Queensland. We experienced some remarkable and frightening events as well, including the Melbourne Dust Storm on February 8, 1983, when red soil, dust, and sand from central Australia were swept up in high winds and carried southeast toward Melbourne and southeastern Victoria. The dust storm was followed only eight days later by the Ash Wednesday Bushfires that raged through Victoria and South Australia fanned by 110-kilometer per hour winds that created deadly firestorms. The whole sky above Melbourne turned orange, and ash covered the trees, buildings, and parked cars throughout the city. More than 70 people died along with more than 300,000 sheep and cattle and countless kangaroos and other native fauna. It was truly terrifying. Yet only six weeks later, bright green leaves were sprouting from the blackened trunks and branches of the burned eucalyptus trees.

By the end of May, Joan and I were ready to continue our circumnavigation of the globe. But first we planned to visit Cairns and points beyond in Australia's tropics. We said goodbye to the NVRI crowd and set off for Queensland in a rental car, making it all the way to Port Douglas north of Cairns where the paved road ran out. We camped all the way, returning to Sydney mid-June to catch our plane to Singapore. From Singapore, we went to Hong Kong, Thailand, and then India. We arrived in Delhi to find temperatures hovering well over 40 degrees Celsius. Despite the heat, we managed to visit the Taj Mahal and other famous sites near Delhi, before escaping to a houseboat on Dal Lake in the Vale of Kashmir in the Himalayas. After a couple of weeks living like the Raj (and recovering from the inevitable *E. coli* infections that plague tourists in India), we left for Egypt. We visited the pyramids at Giza on the edge of Cairo, before taking the "European train" to Luxor where we rode bicycles to the temple of Hatshepsut and hiked over the hills to the Valley of the Kings. It was an incredible trip—one that would be difficult to replicate today as the world has become much more dangerous.

After Egypt, we flew to Athens and then took a ferry to Serifos, an island in the Aegean, where we spend an idyllic three days, before flying to Dubrovnik in the then Yugoslavia. Dubrovnik was nothing like it is today: no tour groups, far fewer souvenir shops, and much less crowded. There were also no holes in the city walls from artillery shells.

In mid-August, we left Dubrovnik and flew to Hungary, which was then part of the Soviet bloc. I had already contacted György Buzsáki (see volume 13), at the University of Pécs (a venerable institution founded in 1367). I had got to know György a couple of years earlier when he was a postdoc with Case Vanderwolf at Western. György was waiting for us when we landed at Budapest airport. He immediately whisked us off to the center of the city and gave us a whirlwind tour of the major sites. Later that same day we met his lovely wife, Veronika. They were wonderful hosts during our short stay in Budapest, making sure that we sampled the best of Hungarian fare. They took us to several iconic restaurants, including Ménes Csárda (loosely translated as the Horses' Tavern), where György regaled us with stories from Hungary's long and complicated history, including occupation by the Turks and the Soviets. They also revealed that they were planning to leave Hungary and were actively exploring the possibility of working in the United States. So animated was our conversation that we closed the place.

The next morning, György drove us to Pécs where I was to give a talk to the Department of Physiology. I was a bit nervous because I was going to have to give the whole talk without slides. In the end it went well. In fact, I found it quite liberating to give a talk without slides—in large part because it ended up being more of a discussion than a formal presentation. It should happen more often.

The next day, we returned to Budapest to catch our flight to Paris. György asked if we would mind taking some of his belongings to Paris, where a friend there would collect them in anticipation of their move to the United States. He proceeded to give us two large suitcases, one of which I could barely lift. It was jam-packed with reprints that György had accumulated over the years. He was loath to part with them. I tried to explain to him that once he was in the States, he would be able to photocopy any paper he liked. In Hungary at that time, few people had access to photocopying machines, presumably because the authorities were afraid that people might use them to print anti-Soviet newsletters or posters. In the end, he understood, and with great relief on our part, we left that suitcase behind.

In Paris, we picked up a new Renault, which we had leased for our four months in France, and set off for Lyon. After a few frustrating days locating rental properties in France, we found a nice apartment in Caluire-et-Cuire, a commune of Lyon situated between the two rivers that flow through the city, the Rhone and the Soane. After we had settled in, I made my way to the INSERM unit in Bron. Marc welcomed me, set me up in a nice little office, and introduced me to Claude Prablanc, an INSERM research scientist, with whom I would be working directly. We also had the help of a graduate student, Denis Pélisson, who was destined to become the director of the institute a couple of decades later. We spent the next week or so figuring out how we might best explore the role of visual feedback in the control of aiming movements. It was a straightforward problem, ideally suited to a four-month visit. According to popular models at the time, the first part of an aiming movement is ballistic. Later corrections are based on dynamic visual feedback about the relative positions of the hand and the target. To put this account to the test, we designed a series of experiments to determine whether the trajectory of the moving hand would be influenced by sudden changes in target position during the execution of the aiming movement, even when the hand was not visible.

To do this, Claude devised an ingenious setup that allowed the experimenter to compute the velocity of the eye movement and use that information to change the position of the target the moment the saccade reached peak velocity. The trajectory of the aiming movement was reconstructed from a photograph of a flashing infrared light-emitting diode attached to the participant's finger. The target was presented through a half-silvered mirror that prevented the participants from seeing their moving hand. The results were clear and unequivocal. Participants could adjust the trajectory of their aiming movement in response to a relatively large and unpredictable change in the position of the target, even though they could not see their moving hand. But something even more startling happened in these experiments that foreshadowed my future work on the organization of the human visual system.

Some of the participants commented that they never saw the target change position. Some did not believe that the target ever jumped. Nevertheless, they always corrected their aiming movement. This led us to carry out a two-alternative forced choice experiment in which we again jumped the target randomly on half the trials, but this time we simply asked the participants whether the target had jumped or remained in its original position. The participants had no idea; their guesses were completely at chance. As we concluded in the paper that emerged from this work, the results suggested that "the neural mechanisms mediating the perception of target position can be dissociated from those mediating visually guided reaching movements directed at that target" (Goodale et al., 1986). In some ways, what we observed was reminiscent of blindsight, but in neurologically intact observers, not in patients with V1 lesions.

Life in Lyon

Not only was the science excellent in Lyon but also the city itself was a delight, as was the surrounding countryside. Joan and I spent many hours exploring the different neighborhoods, including the many restaurants that helped make Lyon the gastronomic capital of France. We drove through Beaujolais and Provence, and on one memorable long weekend in October, we joined a group of friends from the lab on a three-day hike through the Luberon. We hiked about 20 or 30 kilometers each day, stopping now and then to pick and devour grapes from the after-harvest vineyards. The sky was a deep blue, and the air was bracing. Each night we would arrive exhausted and hungry in a different small village where food and lodging had been arranged by our friends. It was wonderful.

At the end of November, Joan returned to Canada with a completed thesis proposal in hand. Her time in France and earlier in Australia had been productive. I stayed in Lyon a bit longer, finishing up the experiments with Claude and Denis. I arrived back in Canada just before Christmas, where I received some wonderful news. Joan was pregnant with our first child, Kate.

Family Life in London, Ontario

Kate was born in August 1984. At the time, we were living in an adult-only apartment building, and we needed to move. We realized that it was time to buy something rather than continuing to rent. We eventually found an old redbrick house with a large open porch in London's Old South neighborhood, not far from our apartment. It was built in 1917 and had been converted some 50 years later into a triplex. It was in rough shape. When Joan's mother came for a visit, she took one look and burst into tears. The thought of her daughter living in such a hovel was just too upsetting.

Joan and I thought that all it needed was some wallpaper removed, a few coats of paint, and some minor repairs. How naïve we were. We spent the next few years installing new drywall, replacing the old knob and tube wiring, lowering the basement floor 15 centimeters, replacing a supporting wall with an I-beam, putting on a new roof and two additional dormers in the attic, replacing the garage, redesigning the entire backyard, and on and on and on. Fortunately, because I had learned basic carpentry from my father, I was able to do many of the repairs myself. The house now looks lovely, and we still live here more than 40 years later.

Our second daughter, Elizabeth, was born in 1986, the same year that Joan got her doctorate. Joan did not take maternity leave for Kate or Elizabeth, but continued to work, first on her doctorate and then as an assistant professor at Western. Luckily, as academics, both Joan and I had flexible schedules. Ten years later, our son Michael was born—and Joan did take several months of well-earned parental leave. After spending 20 years doing research and teaching, Joan moved up the administrative ladder, eventually becoming associate dean, undergraduate studies, in the Faculty of Social Science. She proved to be a skilled administrator and in later years has been called upon several times to steady the ship as acting chair of psychology and as acting dean. Life has been rewarding for both of us.

Back at Western

I returned to Western a full professor. I had been promoted while we were somewhere in Asia, but I didn't find out until we arrived in Lyon where a letter from the dean was waiting for me. A couple of months later, I learned that my application to the Natural Sciences and Engineering Research Council (NSERC), which I had written while we were in France, was successful. With this news, I started to set up my lab for recording limb, hand, and finger movements during grasping. When I was in Lyon, I had been impressed with the work that Marc Jeannerod was already doing on grasping (Jeannerod, 1984), and I wanted follow suit. Marc had been using film to record the hand movements, but I thought that it might be much more efficient (and perhaps more accurate) to use an active tracking method. I purchased an optoelectronic system that tracked the movement of infrared light-emitting diodes that could be attached to the hand and fingers. At the time, Marc's lab, mine, and one or two others were the only labs studying the visual control of grasping. It was essentially an empty field of study, giving us an opportunity to do foundational work.

Over the next 10 years, I wound down my animal work and ramped up my study of visuomotor control in humans, concentrating on reaching and grasping. I was lucky to be blessed with some exceptionally talented graduate students who helped me in this new research enterprise: Lorna

Jakobson, Philip Servos, and David Carey. Together, we looked at the effect of prismatically displacing targets on the control of aiming movements in real time (Jakobson and Goodale, 1989), how the size and distance of goal objects affected the scaling of grip aperture and the velocity of limb movements (Jakobson and Goodale, 1991), and the role of binocular vision and motion parallax in the control of grasping (Servos et al., 1992).

Even though we found the results of our experiments incredibly interesting, it was hard to get the attention of other researchers. Our work seemed to fall between the interests of the vision community and those studying motor control. My students often felt quite lonely in the poster sessions at various meetings they attended. But all this began to change after a visit from David Milner in 1989. He was keen to tell me about a patient he had been testing, a young Scottish woman who had developed severe visual problems after carbon monoxide poisoning from a faulty water heater in her newly renovated home in Italy. DF, as we later came to refer to her, was left with a dense visual form agnosia: Not only was she unable to recognize common objects from their shape or the faces of her relatives or friends, but also, as David showed, she was unable to discriminate between simple geometric shapes, such as a triangle and a square. At the same time, DF had no trouble recognizing people from their voices or objects placed in her hands. Her perceptual deficit appeared to be exclusively visual and limited to the geometric shape of objects. Her ability to perceive the color and other surface properties of objects was largely spared.

But the story didn't end there. One day, when David was testing her ability to recognize common objects, he held up a pencil in front of her. He was not surprised that she couldn't identify it—although she did comment that it was yellow (which it was). Her deficit was so profound that she couldn't even say whether David was holding the pencil vertically or horizontally. But then something quite extraordinary happened. DF suddenly reached out and grabbed the pencil, presumably to examine it more closely. David was flabbergasted. Her grasping movement was fluid and accurate: Her fingers opened appropriately, and her hand rotated to the correct angle so that her fingers encompassed the pencil. David quickly tried different orientations of the pencil, and in every case, her grasping movement was normal. She showed none of the groping movements one might have expected in someone who could not report the shape and orientation of objects. Somehow, her hand "knew" what she didn't.

David's observations of DF resonated not only with Larry Weiskrantz's demonstration of blindsight in patients with V1 lesions but also with the dissociation I had observed with normal observers in our jumping-target experiments in Lyon. I was excited. DF's remarkable behavior was consistent with my long-standing conviction that vision did so much more than mediate our perception of the world. Even though David and his colleagues

in St Andrews had already begun a systematic study of her impaired and spared visual abilities, we thought it might be a good idea to bring her to Western so we could test her behavior in much greater detail in my new visuomotor lab. We contacted her, she happily agreed, and in the spring of 1990, she and her partner made their first visit to Canada (for a detailed account of DF and the testing we carried out with her, see Goodale and Milner, 2013).

DF was wonderful to work with. Without complaint, she did all the tasks we set before her. Because she was with us for only a week, we often had to make test objects on the fly. I spent several evenings at home in my basement workshop, fashioning wooden cubes and other objects for testing the next day. We simply couldn't wait for the carpentry shops at the university. Throughout this frenetic week of testing, I was ably assisted by two of my graduate students, Lorna Jakobson and David Carey, who not only carried out a lot of the testing but also charmed DF. As we tested her, a clear picture began to emerge, confirming David's informal observations with the pencil and his more systematic studies exploring DF's intact visuomotor performance in a "posting" task. Despite her inability to report the shape and orientation of objects, she was able to use those same object features to shape the posture of her grasping hand as she reached out to pick them up. Early the next year, we published two papers documenting this dissociation in DF, one in *Nature* from my lab at Western (Goodale et al., 1991) and the other in *Brain* from David's earlier work at St Andrews (Milner et al., 1991).

The papers got a lot of attention in the neuroscientific community and even in the popular press. Soon after the *Nature* paper appeared, I was interviewed by a science reporter for the *New York Times*, a first for me. In May of that year, I presented the findings at the annual meeting of the Association for Research in Vision and Ophthalmology (ARVO) in Sarasota, and unlike what I experienced in my earlier presentations on visuomotor control, the room was packed. People were really interested, and I was swamped with questions both in the session and throughout the rest of the meeting.

So where was the damage in DF's brain? Although the MRIs and CT scans showed indications of widespread damage associated with carbon monoxide poisoning, the lateral occipital regions in both hemispheres were particularly affected. In other words, the lesions were largely in the ventral visual stream of pathways projecting from early visual areas to the occipitotemporal cortex. Shortly after we tested DF, we had an opportunity to test another patient, VK, who had bilateral lesions, not in the ventral stream but in the posterior parietal cortex in the dorsal visual stream (Jakobson et al., 1991). Like many patients with damage to this brain region, VK showed great difficulty using vision to point to targets (optic ataxia), particularly in the visual periphery. But importantly, she also showed clear deficits in her

ability to grasp objects, even in central vision, opening her hand extremely wide and haphazardly changing her grip aperture midflight. Nevertheless, she had no trouble recognizing objects or line drawings. This double dissociation between DF and VK (along with a plethora of findings emerging from monkey electrophysiology) motivated David and me to start writing an opinion piece for a new review journal, *Trends in Neurosciences*. By then David was back in St Andrews. So, we decided to write the whole thing using an early email server, Eudora, even sending attachments of our drafts, back and forth across the Atlantic. We really felt like we were ahead of the curve.

At the time we were writing the article, the dominant theory about the division of labor between the ventral and dorsal streams was the "what vs. where" (object perception vs. spatial perception) distinction put forward by Leslie Ungerleider and Mort Mishkin (1982). On the basis of our observations in patients and a range of other evidence from both human and monkey work (including anatomical differences in the outputs of the two streams), we proposed that the distinction was better characterized as "what vs. how." We argued that the ventral stream delivers up our perception of the world, providing a visual foundation for cognitive operations, whereas the dorsal stream converts the visual input into the coordinates required for action. Notice that the distinction here is not so much between the inputs to the two streams as it is between the specific transformations carried out on that input for the "consumers" of the visual information. Both require information about objects and their location but for different purposes: cognitive operations, on the one hand, and the control of action, on the other. The paper (Goodale and Milner, 1992) and a much longer monograph that David and I wrote a few years later (Milner and Goodale, 1995) turned out to be persuasive enough that they are still highly cited even today.

Throughout the 1990s, DF continued to be the centerpiece of our work with neurological patients. In 1992, I visited her in Italy to do some additional testing of her visual abilities. Armed with a fancy new video camera, I planned to record her performing a range of visual tasks, from catching a ball to discriminating between and picking up objects of different sizes and shapes. DF and her partner had kindly offered to put me up for a few days. They were still living in the same old house where DF had been overcome with carbon monoxide poisoning. The renovations were nearly complete, but nevertheless, it was still rather unsettling to venture into the bathroom where the water heater (no longer connected) had emitted the deadly carbon monoxide that had led to her visual form agnosia. But that aside, the visit was wonderful—and productive. I spent the next few days testing DF, often seated at a table in their beautiful garden, presenting her with line drawings and small objects, as I made notes of her responses. I felt very much like a nineteenth-century neurologist. The work with DF at her home in Italy cemented our earlier observations that even though she

had a profound deficit in form vision, she was still able to pick up objects of different shapes and sizes almost as deftly as someone with normal vision—and could even play catch with her partner.

In addition to work with neurological patients like DF, we have also tested some of the predictions of the two visual system proposal in healthy volunteers. Because vision-for-perception relies more on scene-based and contextual cues, whereas vision-for action uses effector-based computations, we reasoned that the visual control of grasping would escape the effects of pictorial illusions of size. Together with Salvatore Aglioti, an Italian neurologist visiting my lab, we tested this idea and showed that, when people reach out to grasp an object embedded in a size-contrast illusion (in this case, the Ebbinghaus), the opening between their finger and thumb scales in flight to the real, not the illusory, size of the object (Aglioti et al., 1995). Some researchers (e.g., Kopiske et al., 2016) have reported that grasping is not always immune to the Ebbinghaus illusion (for a critique, see Whitwell and Goodale, 2017), but many more labs have gone on to replicate our findings, not only with the Ebbinghaus but also across a broad range of different illusions (e.g., Jackson and Shaw, 2000; Ozana and Ganel, 2020; Stöttinger et al., 2009). Nevertheless, the resistance of grip scaling to size-contrast illusions, particularly the Ebbinghaus illusion, is clearly affected by many factors. When I began this line of research 30 years ago, it never crossed my mind that it would generate such a lively, but informative, debate.

The Dawn of fMRI

In the early 1990s, the neuroscience community was abuzz with talk about fMRI, a new technique that allowed researchers to localize functional activity in the human brain. In 1993, a group of us at Western began to plot ways of getting our own fMRI scanner and recruiting someone who knew how to use it. We had heard about an exceptionally talented young biophysicist, Ravi Menon, who was then finishing up a postdoc with Kamil Uğurbil at the University of Minnesota. He and Ugurbil were working closely with Seiji Ogawa and David Tank from Bell Labs on the development of fMRI. Because Ravi had completed his entire university education in Canada and was familiar with how things work here, we thought it was possible that he might be open to coming to Western. We realized too that this was unlikely to happen unless we could provide him with a high-field magnet. We began some serious lobbying of the senior administration at Western, University Hospital, and the newly founded Robarts Research Institute. Through some miracle (and a substantial gift from a local donor with matching money from the province), Robarts and Western eventually ended up recruiting Ravi, purchasing a 4T MRI machine, and erecting a building to house it. By 1996, the 4T was up and running.

Growing Neuroscience at Western

Since those early days, fMRI has become vastly more sophisticated—and the neuroscientists and imagers at Western have continued to be at the forefront. We have been successful in securing major infrastructure grants from the federal and provincial governments that have allowed us to expand our MRI resources and establish a nonhuman primate facility. In short, neuroscience, particularly cognitive neuroscience, has flourished at Western. In 2003, we were able to persuade the university to set up a Centre for Brain and Mind (CBM), which is now housed in the seven-story Western Interdisciplinary Research Building erected in 2017.

These early developments allowed us to recruit leading researchers from Canada and around the world. One of those hires was Adrian Owen, who joined us in 2010 from Cambridge (United Kingdom), through the Canada Excellence Research Chair program. Adrian had recently made a splash in both the scientific and popular press with his 2006 fMRI study in which he showed that an unresponsive patient who was thought to be in vegetative state was actually conscious (Owen et al., 2006). His recruitment and the evident success of the CBM catalyzed additional recruitments and infrastructure grants, culminating in a $66 million grant (BrainsCAN) from the Canada First Research Excellence Fund in 2016, the largest grant in Western's history. I feel privileged (and lucky) to have played a role in all of these achievements.

Imaging Action in the Magnet

One of the biggest game changers in my research has been neuroimaging. With the advent of high-field fMRI at Western, we have been able to show how the two streams are differentially activated for visual perception and the visual control of action. None of this would have been possible without the help of my neuroimaging colleagues, particularly Ravi Menon. But one individual was pivotal in enabling our lab to study visually guided grasping in the magnet. That person was Jody Culham, who came to my lab as a postdoc in 1997 from Patrick Cavanagh's lab in Harvard. She had also worked closely with Nancy Kanwisher and others who were pioneers in the use of fMRI. Because of that experience, she soon became an invaluable part of our team as we developed ways of identifying the neural substrates of the visual control of grasping using fMRI. I am pleased to say that Jody is now a full professor in Western's Department of Psychology and holds the Canada Research Chair in Immersive Neuroscience.

One of the challenges we faced was how to have people grasp real three-dimensional objects in the high magnetic field of the MRI scanner. Jody was instrumental in helping us devise a piece of equipment to do just that. Using this "grasparatus," as we affectionately called it, we were able to show that when people reach out and grasp objects, they show robust

differential activation in their dorsal visual stream, but no grasp-related activity in their ventral stream (Culham et al., 2003). Importantly, when we tested DF in the same way, she too showed activation in her spared dorsal stream, but no differential activation in her ventral stream to the shape of objects, even those she was grasping (James et al., 2003). Taken together, these fMRI results converged beautifully on our earlier neuropsychological findings with DF and other patients, supporting the distinction between a dorsal "action" stream and a ventral "perception" stream in the human brain (see Goodale and Milner, 2013).

I should make it clear that my research at Western has not been entirely focused on the two visual systems proposal. I have also been involved in other projects, some serendipitous, some arising from conversations with my colleagues and trainees. These include exploring the neural substrates of echolocation in blind humans (Thaler and Goodale, 2016), face perception (Ganel and Goodale, 2018), size and material weight illusions (Paulun et al., 2019; Buckingham and Goodale, 2010), and size constancy (Sperandio et al., 2012; Chen et al., 2018). Most recently, I have returned to my roots and have begun to investigate the role of the superior colliculus in the initiation of visually guided reaching movements, both automatic and voluntary, but this time in humans (Mekhaiel et al., 2026). In all this work, I have been driven by the conviction that neuroscientists need to take a long evolutionary view and understand that ultimately the brain's main job is to drive action.

Epilogue

Throughout my career, I have been fortunate to have worked with a succession of talented young graduate students and postdocs. Although I have mentioned some of them in this account of my research, there are many others whose contributions have been equally impactful. I thank them all.

I stepped down as director of the Centre for Brain and Mind in 2018 after a long 15 years. After a short stint as interim director of the new Western Institute for Neuroscience, I retired in 2024. I still cosupervise graduate students and a postdoc but no longer run a big lab. I continue to collaborate with former trainees and colleagues around the world. I have to say it is rather liberating to be able to keep my hand in research and writing and to participate in the intellectual life of the university without having to attend departmental meetings or get involved in the inevitable political machinations of the university.

Joan retired shortly after I did. We now have more time to spend with our children and grandchildren. Watching all of them grow up and find their own way in life has been one of the most rewarding experiences of our lives.

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