

Large-scale structure, skiing, and a stroke

Marc Davis

Abstract: *This paper is a summary of the highlights of my working life, from my high school days to the present. There have been more students than are mentioned in this brief summary, and many of the stories involve interspersed scientific achievement with family comings and goings. I have in mind that this will be read by young students and postdocs who don't know what the field was like 40 years ago. My family will not understand the science, and for them the text highlighted in blue is the story of how Nancy and the children interface to my work. I also give a few of the highlights that led to my incredible enthusiasm for skiing. Finally, I must talk about my stroke, a pivotal event that took place at the end of June, 2003.*

Chapter 1: Childhood

I was born in 1947 and raised in Canton Ohio, a medium-sized town about an hour south of Cleveland. My family was incredibly loving – we had no disputes that I can remember. I do recall that in the 2nd grade my teacher was very concerned about my nervous tendency of perfectionism in class, which she suspected indicated problems at home. My mother explained that the perfectionism was simply an indication of my desire to excel, a mark of a strong type A personality. This has stuck and is common among academics.

My childhood was boringly normal, punctuated with occasional highlights. At age ~10, I remember making a skateboard from an old roller skate nailed onto a board. This was before neoprene wheels had been invented, and well before the commercial release of skateboards. The thrill of speed as I descended local slopes (small) was matched by the thrill of actually maneuvering the board. The crummy steel wheels quickly became flattened and had to be replaced every other week, but the system worked. I was also active in scouting and gradually earned their highest award, an Eagle Scout, when I was 15. This probably was responsible for my love of camping.

I was always interested in science and I remember once convincing my parents to take me to a NASA show in Cleveland. The show highlighted rockets under development, plans to go to the moon, and whatever else was important in the late 1950s. I remember a large blackboard stating that more printed information was available to anyone who could explain what an 'ion' was, which I knew. I don't remember if the information was interesting, but there was a lot of it.

I was approximately 10 years old and a new house was being built across the street. Like all kids I was playing in the construction, and the workmen were burning some debris. My sister, Tammy, and I were running around the fire when she tripped and her entire hand was burnt as she fell into the ashes. We ran home, she screaming, and my mom inserted her hand into a container of Crisco. She was bandaged at the hospital with second-degree burns, and a week later she had an appointment with our pediatrician to change the bandage. Up to this time, I thought that I would become a doctor when I grew up, which most of the bright Jewish boys just naturally did. There were six or seven future M.D.s living within a block of me! But when the doctor exposed Tammy's hand, which was doubled in size and white from all the dead skin around the blisters, I fainted and later decided I would never study medicine. Maybe this was a critical moment in my life?

When I was in the 6th grade, I had Mr. (Stanley) Freedman for a teacher, and he recognized I was bored. Since he also taught high school chemistry, he gave me the chemistry textbook to read over the summer. It was fascinating and a little over my head, but for years I wanted to become a chemist, without really knowing anything about physical science. I received a chemistry set as a present soon after, and it was such a 'safe' kit I couldn't make anything explode. I only recall making test tubes of brown gunk.

In the 9th grade, bored even in our General Science course, the teacher assigned to me a project, to make a model of a rocket destined for the moon. At the time NASA was sending rockets merely to hit the moon. My model was simply a painted Plaster of Paris rocket with some explanatory material, a rather stupid exercise. I do remember going to the local library and studying magazines with details of rockets, so perhaps it was not so stupid.

After my junior year I went to an NSF sponsored science program for high-school students at the Ohio State University. There were about 20 guys all staying in a dorm and it was eye opening for me. Here were students I could speak to, students who were my intellectual equal. For two months we took a number of courses in mathematics, physics and computer programming, and in the afternoon we were assigned individual researchers. My assignment was to help in a shock tube laboratory. I don't remember what physics I learned, but I did learn how to clean out a 20' long shock tube.

The chance to live with peers was what really mattered as I learned so much in talking to them. A number of the guys (guys only!) were students at the Bronx High School of science and were using a much better physics text, one that had been developed by physicists. When I got back to Canton I tried to convince the school to switch physics texts but it was a hopeless battle.

I remember discussions with my mother, who read most of the books I did, about Ayn Rand's philosophy of Objectivism, a stupid philosophy that I did not give up for several years. It is interesting how a young mind could be so attracted to such an

inhumane philosophy, but it has a lot to do with the incompleteness of my education, the slow realization that what matters is the progress of society, not of individuals.

Books that early on were especially influential to me were a popular book by Einstein, another by Bertrand Russell, who wrote *The ABC of Relativity* (1925), and George Gamow's famous text *123 Infinity*. I still have the Gamow text in my bookshelf, and I remember the magic of his explanations. Later I read a few of his other books, *Mr. Tomkins in Wonderland*, where he discusses relativistic shrinking of lengths and time dilation in a wonderful manner understandable by an untutored kid. Another of Gamow's books I recall was *The Creation of the Universe* (1952), where I first learned the basics of big bang cosmology. I was confused about his explanation of 'Ylem', which was what he termed the primordial plasma, in a failed attempt to explain the formation of all the elements. (He, like many others, did not appreciate the roll of supernovae in the formation of the heavier elements.) His books, more than any others, excited me to learn more about the physical universe.

In high school I was tied for valedictorian and was awarded numerous certificates of merit on the basis of statewide tests in virtually all high-school subjects. I was active on the debate team, an important exercise that gave me plenty of confidence for public speaking. I was elected the senior most likely to succeed.

I applied to the University of Michigan in the fall of my senior year and was immediately admitted. I knew I'd go to college, even though Michigan was my second choice. I was accepted to my first choice, MIT, in the spring and was incredibly excited.

Chapter 2: Life begins at MIT

MIT in the 60's required all freshmen to take a year of physics, a year of calculus, and a semester of chemistry, plus humanities. There was no question that I would major in physics; biology at the time, 1965, was not very exciting as its revolution was a decade in the future. MIT was incredible for the number of really smart students all working on the same physics problem sets, and the diversity of these brilliant minds was astounding to me. This was my first experience at witnessing the beautiful way in which students learn from each other, even more than what I could absorb from the spectacular lectures. The peer group was so different from Canton!

I remember an excellent course on electromagnetism for sophomores that was taught by Ray Weiss, and the final 3 semesters of undergraduate physics was taught by Arthur Kerman. Both of these lecturers were remarkably lucid and convinced me that I was most interested in becoming a physicist in academia.

I was pretty skinny when I was a freshman at MIT and thought I would row crew, a sport I had never attempted, to gain some strength. The problem was that I was on the dividing line of lightweights and heavyweights. I could try to bulk-up for

heavyweights, or lose 10 pounds and be a lightweight. Naturally I chose to be a heavyweight but I never was able to put on the pounds. Rowing was brutally tough but I stuck with it for a year. It is a tradition for the losing team to give their T-shirts to the winners after a race, and I was amazed that our opposition always outweighed us by around 20 pounds per man. I realized these guys were probably washouts from their football team. I also found that I was too tired to finish my weekly problem sets, and my grades suffered. One year of crew was enough!

For physical education, MIT required undergrads to select from a wide list of options. I learned basic sailing on the Charles River in the fall of my sophomore year, and for the winter I took a course on skiing. There is a miserable ski area, Blue Hills, south of Boston, reached by taking the MTA's Red line to the end and then hopping on a bus. For equipment I borrowed a pair of crummy, warped wooden skies, 215 cm long with antique bindings, and leather boots. That winter I got as far as learning stem-christie's on a very flat beginner's slope. Needless to say, my skiing ability stunk, but the bug had been implanted!

Late one night, upon returning to the dorm with all of my ski equipment, there was a 'mixer' happening on my floor, where I met Paula, an undergraduate from Simmons College who would become my first wife. (More later.) My roommate, Phil, owned a Honda 250 cm³ motorcycle and loaned it to me to pickup Paula. One time I was talking long-distance to my parents and I happened to mention this to them. They were so horrified I would do such a dangerous thing that they immediately bought me a new car, a '66 Ford Mustang. I of course thanked them very much and then realized I had executed a remarkable ploy!

With the car, I frequently drove to a ski area in New Hampshire in the middle of the week. (Wednesdays were a day I had no lectures, I think.) I recall that Waterville Valley cost \$5 if you had a college ID. I would take 3 or 4 fellow Baker House friends and we left at 6am to drive the 3 hours to the ski-slope. I was on better skies by then, improving all the time, and enjoying it immensely.

I guess our class was the earliest stage of the hippie generation, marked in my case by long hair, a beard, bell-bottom pants, and a leather vest with fringes. I listened non-stop to the astoundingly creative recordings of Bob Dylan, Janis Joplin, Jimi Hendrix, the Beatles, the Rolling Stones, and Cream, all of which were enhanced by doses of marijuana, mostly smoked in Hal Varian's room. Who knows how we ever completed our problem sets? (In graduate school, I remember smoking only on Friday nights, thus giving me a 'clear' head by Sunday.)

I believe I was a sophomore when Senator Wayne Morse of Oregon spoke in MIT's largest auditorium, Kresge Hall, to a packed audience of students. He was one of the few Senators who had voted against the Tonkin resolution that opened the gates to the American assault of Southeast Asia. After he spoke, the MIT students, I am sure, were uniformly opposed to the war, in which they could be cannon fodder.

In early 1968, the anger of our generation propelled myself and many others to distribute Gene McCarthy flyers to residents of New Hampshire in an effort to defeat President Lyndon Johnson. McCarthy was a US Senator from Minnesota also opposed to the Vietnam disaster and was running for President; Johnson retired from the race but Republican Richard Nixon beat the vice president, Hubert Humphry, and 4 years later the Democrats lost by 49:1. Oh well. Perhaps it would have been better for the country if Johnson had won?

I worked as a bus-boy in the cafeteria of my dormitory, Baker House, during my sophomore year, but the next year I landed a position at the MIT instrumentation lab which had a contract for computer systems for the Lunar Lander of the Apollo mission. I found it quite incredible that they had to hardwire the programs, but recall that it was more than 10 years before the invention of microprocessors.

In my senior year I got a job at a small computer company, Adage Inc., located on Commonwealth Ave. in Boston. My initial job consisted of writing basic software, such as the *sine* and *cosine* functions. At first, I didn't realize that these functions were not predefined, and I learned about low order polynomial expansions that were not simply the Taylor series expansions I had learned as a freshman. The coefficients were adjusted so that a 5th or any order expansion had minimal error up to $\theta = \pi / 3$. For larger angles, one simply used the symmetry of the functions. This was an interesting lesson in applied mathematics.

At the time the computer industry was very diverse, prior to the development of very large-scale integration, and there was, for example, no agreement on word length, and Adage used a 30-bit word. Machine instructions were very simple and for the most part were instructions to move a word to or from memory or another register. The circuit boards were enormous, typically 18" x 18". A memory board of 4K words had ferrite cores for each bit, hand-wired by women in the third world. The total core memory was of order 16K words. The bootstrap program could either be toggled in via switches on the front panel, or by reading a paper-tape that was always getting ripped. Adage's value was that their machine was a computer graphics terminal, with the ability to plot a vector list, i.e. a line drawing CRT monitor allowing displays of anything, with built-in 3-d rotation. They were among the first companies to have this ability, and it was incredibly novel when everyone was used to computers with card readers and half day turn-around. Such was the state-of-the-art in the '60s. All this was incredibly fascinating and instructive to me.

We all had to do a senior thesis and I worked with Bill Rose on supersonic outflows from OB stars. The topic of my thesis was to study various models of hydrodynamic mass loss and to apply them to observations and to theoretical models of stellar evolution. I learned numerical methods including the use of differential equation solvers, always handy.

I applied to many graduate schools and settled on Princeton; as a physicist I didn't consider myself an astronomer so I applied only to physics departments. My

undergraduate thesis was probably insufficient to get me into a good graduate school; it was likely the letter written by a vice president of Adage that did the job.

Before I went to Princeton, I had to decide what my intentions were about Paula. At the time, 'everybody' got married just out of college, and Paula's mother was very clear about her feelings: "I should shit or get off the pot". Paula and I got married in August of 1969 and headed to Princeton.

Chapter 3: Graduate school at Princeton

At Princeton the intensity of the students was immediately apparent. Some of my classmates included Jacob Bekenstein, Frank Wilczek, and Ed Witten. At this point I realized that perhaps I should do experimental work rather than theory? The entering class was mighty impressive!

The first crisis of graduate school was the during the Vietnam war, which was an ongoing debacle the whole time I was in college. In 1969, the year I started at Princeton, graduate student deferments were eliminated, thus making all of us entering graduate school now eligible for the draft. Eligibility for deferments was determined by one's birth date. All the drama was heightened by a random lottery of birthdates, nationally broadcast on TV. I think that year September 8, my birthday, was the 41st chosen, and that year the Selective Service agency took all eligible males up to a number of roughly 100. I soon had a military physical in Newark; I was very healthy and passed. The final step in determining whether or not I'd be drafted was a meeting with my draft board back in Canton.

I gathered the letters I had received from a local judge when I earned my Eagle Scout award years ago, as well as congratulatory letters for doing so well in the state of Ohio scholarship tests. I also had letters from Dave Wilkinson of Princeton, and finally a letter from the personnel manager of Adage, Inc. None of the letters were worth anything, except the last. It read something like "Pursuant to the American National Defense, Mr. Davis has been a key member of the following contracts, *DOD-1234-5678-abcd-efgh*, and *NSA-0987-6543-21zy-dcba*" (each took 2 lines and was full of gobbledy-gook). At that time, the Department of Defense and the National Security Agency were buying all types of computers to keep them in business and develop the technology, but I was a lowly systems programmer who knew nothing about the computer business side. Nonetheless my local draft board was highly impressed and said they would give me a permanent deferment if I would work for Adage the following summer. Adage agreed and offered me a salary of \$1000 per month. (Five years later I was earning \$915 per month as a Harvard Assistant Professor!)

Having settled my draft status, I could concentrate on classes, I recall taking a quantum mechanics course with John Schwarz (now a Professor at CalTech), as well as Jim Peebles's course on cosmology, from which grew his textbook *Physical Cosmology*. As a green, first year student, this course was way over my head. Jim's

second book, *The Large-Scale Structure of the Universe*, 1980, among other topics, details our work on the BBGKY equations, which started when I was at Princeton.

I initially started working in collaboration with Remo Ruffini on the gravitational radiation emitted by a particle falling into a Schwarzschild black hole. This work was the subject of my first four publications. It was motivated by Joe Weber's claim to have detected gravitational waves, presumably coming from the galactic center. This generated an enormous amount of theorizing, but our basic results were correct. We showed, for example, that gravitational synchrotron radiation does not exist, making it more difficult to understand Weber's results. Forty years later, gravitational waves have finally been detected using the amazing LIGO machine, incredibly more complicated than Weber's metal bars!

Students interested in experimental science were expected to take a course in the Jadwin lab student shop where you worked with a master machinist to construct a brass cannon. This was fun and I learned how to use virtually every machine in the shop. I still have the cannon, which actually fires. It was a super version of a high school Industrial Arts course, which of course none of us had taken. This knowledge of machining would come in handy when I was building the equipment for my thesis.

All physics students were required to take a week long qualifying exam by the end of their second year, and we spent a semester studying everything. I remember that Princeton students were rioting about the war, as were students across the nation. Much as we might have wanted to participate, we had spent so much time preparing that nobody would leave the exam room. We could hear the protestors' shouts as we did problems in QM or General Relativity. Most of the students, including me, passed this difficult exam, the studying for which had sharpened our general knowledge of physics to the highest level.

In searching for a thesis advisor, Jim Peebles discouraged me from working with him, saying the world needs new data, not new theory. This was certainly true and was reinforced when John Bahcall urged me to consider experimental work. I didn't think much about this but spoke with Dave Wilkinson, who offered me two projects to consider.

The first was to build a CMB polarization receiver for the roof of Jadwin lab, the second to build a machine for the search for primeval galaxies. (Others took on the first job and the polarization was finally detected by WMAP 30 years later.) The primeval galaxy search was based on a suggestion by Partridge and Peebles that there were collapsing galaxies at redshift $z > 1$. These had a high luminosity because of the burst of star formation at high redshift, but very low surface brightness because they were collapsing and thus had large angular extent, making them undetectable on photographic plates. Of course no one had a reasonable model for galaxy formation, and the highest redshift measured at the time was a radio galaxy, 3C-295 at $z = 0.46$.

Dave was planning to spend sabbatical the following year in Hawaii, and the primeval galaxies work could be done on the 88" telescope on Mauna Kea. For me the choice to go to Hawaii or spend time in hot, humid Princeton was easy. Such was how fateful decisions in one's career path are made!

Mike Hauser, a particle experimentalist working in Wilkinson's lab, was told to watch me for the year Dave would be away. Remember that this was a decade before the invention of CCD's. Photographic plates had poor efficiency, non-linear response, and lack of sensitivity for red objects, the region where we could expect the primeval galaxies.

The lack of a 2-dimensional detector meant the instrument had to use a point detector, a diode for example. I played around with early generations of avalanche photodiodes, which had sensitivity up to wavelength of $1400 \mu m$, but they were too unstable and eventually I settled on using a photomultiplier tube with the best red response available. The instrument featured a rapidly rotating half-moon in the focal plane, and the signal was synchronously detected and stored in four counters, allowing N-S and E-W signals. The synchronous detector was the mark of a Wilkinson student thesis! I didn't discover primeval galaxies, but we began to point at radio sources without optical counterparts, thinking perhaps they are primeval galaxies?

I remember the observing in Hawaii was very cold, as I had to spend several nights in the dome guiding the telescope the old-fashioned way, that is, by looking through an eyepiece. Once I was high in the air guiding, I couldn't go down to the warm observers room because raising or lowering the platform added random counts to my counters, and thus I had to stay up with the instrument and freeze, in Hawaii.

There were two students in my classes at Princeton, Ed Williams and Marlene Noble, who were as nuts about skiing as I, perhaps more so. We used to drive in Ed's old Rambler, with very loose steering, from Princeton to Hunter Mountain in New York, about four hours away. The mountain was incredibly icy but did feature 1350 feet of vertical, far better than anything in New Jersey. I think we went 3 or 4 times in a winter. They remain friends, and lived in the Bay Area for many years. Many times have we skied together at Squaw Valley and Alpine Meadows.

I finished the Ph.D. thesis by spring of my fourth year and Dave offered me a faculty position, i.e. a post in which I was paid 50% by the University and 50% by his grants. My obligation was to teach an undergraduate course; in my case it was junior level mechanics. Meanwhile I applied for a job advertised by the Harvard astronomy department, not thinking I would be accepted.

Upon finishing my thesis in 1973, I put on my theorist's hat and began to work with Peebles on a kinetic theory approach to galaxy clustering. We applied kinetic hierarchy methods first devised for plasmas where the interactions were all short range, as opposed to cosmology where the interactions are all long range. The method is known as the BBGKY (Born-Bogolubov-Green-Kirkwood-Yvone)

expansion. In the question of galaxy clustering, the basic idea is to seek a method for evolving the behavior of the two-point galaxy correlation function $\xi(x,t)$ in relation to the 3-point correlations. The equations are not closed unless you can specify the 3-pt correlations, and we tried writing the 3-point function as $\zeta = \xi\xi$. This fit the observations, roughly, but had no dynamical justification at all. Furthermore, the entire picture of the merging of galaxies is totally ignored; we effectively treated the galaxies like equal-mass points. But in defense, it was 1974, a long time before we knew anything about the true nature of clustering and biasing of galaxies. Jim and I talked quite a bit about how to proceed, and the effort stretched on for years. The topic was so crazy that I had virtually nobody else to bounce ideas off of. The method was insanely wrong, which was not too important as it gave me further experience in theory. It dragged into the time I had moved to Harvard.

In the spring of 1975, after a semester of teaching at Princeton, I was sick in bed with the flu at the summit of KPNO when I was contacted by Harvard; they were prepared to offer me the Assistant Professorship. I later found that I was their third or fourth choice, but no matter. It was the only job I applied for as I could have stayed at Princeton another year.

Chapter 4: Junior Faculty at Harvard

I came to Harvard's astronomy department (note that Harvard astronomy is integrated with the Smithsonian Astrophysical Observatory and the amalgamation is called the Center for Astrophysics, or CfA) with a physics degree. Director George Field asked how comfortable I would be teaching the astronomy curriculum. I replied I thought it would be okay except when it came to stellar atmospheres. He laughed. I mostly taught courses in extragalactic astronomy and cosmology, a new subject for Harvard. There was much astronomy to learn, but I managed.

Meanwhile Paula and I were not a happy couple and were divorced a few years after moving to Cambridge. She was trying to complete her degree at Rutgers and had found a more compatible mate. I was living alone in Cambridge near Harvard yard. My immediate neighbors included Herman Feshbach and Julia Child, who lived a few blocks away; our house was just a block from her favorite grocery store, Savor's. I felt pretty low, and I focused on work and on meeting women, often at tennis courts or by taking cooking classes at the Cambridge adult education program.

Naturally, I joined a ski house in Vermont near Sugarbush and Mad River Glen, about a four-hour drive from Cambridge. Vermont is incredibly cold, in contrast to a wallpaper photograph displayed in a McDonald's restaurant on the way up. The photo showed 5 or 6 guys dressed in sweaters only, skiing on very soft moguls, of the sort you never find in the East. I wondered where in the world was this picture taken, but now I know it was probably at Squaw Valley, California. I was incredibly envious.

The CfA owned a tennis court and had a shower in the basement. That was an amazing fringe benefit, and I made good use of it. My office smelled like a locker room as I played once or twice per week and always had wet clothes hanging. At the time the Radcliffe gymnasium did not exist and in its place were four clay tennis courts, which were much better than the asphalt court of the CfA.

For a few years I was engaged in work I had started with Peebles on the BBGKY hierarchy equations. In 1977 I finally published the long paper with Jim, as well as a short paper with Peebles and Groth in which we discussed the importance of measuring the relative velocity dispersion of pairs of galaxies, something that wasn't possible with existing data. The measurement could tell us about the density of the Universe and the self-consistency of the whole picture. It was a perfect project for the CfA as the observatory owned a little used 60" telescope. I would use it to do a redshift survey!

This was my original motivation for the redshift survey now known as CfA1. John Huchra, a young postdoc with plenty of observing experience, became a close collaborator, as well as David Latham, a Smithsonian astronomer who was a solid instrumentalist. As we got going, I picked up a physics graduate student John Tonry whom I met when he was a undergraduate math major at Princeton. That was it - the four of us carried out the CfA1 survey, with help in the observing.

I went to Mount Hopkins, in southern Arizona, to assess the existing spectrograph of the 60" CfA telescope known as the Tillinghast. The spectrograph was okay but lacked a TV guider, and most importantly, the detector, using an image tube with photographic plates, was way behind our needs. I suppose it was suitable for spectra of 10th magnitude stars, but we were planning on doing 15th magnitude galaxies. Furthermore, since we were planning to measure of order 2000 spectra, we did not want to measure plates, but would rather install an electronic device. At that time, ~1975, such devices were extremely rare.

I tried testing the spectrograph with a TV camera as detector, which was huge and impractical, and broke upon use. Other devices were suggested but only existed as vaporware. I finally decided that Steve Shectman of the Carnegie Institution had built the most practical machine, and it would be ideal for me to go to Pasadena to reproduce his circuits.

The device counted individual photons and consisted of a chain of image-intensifier tubes coupled to a Reticon photodiode array of dimension 2 by 936 which was scanned at 2000 frames per second. The individual photoelectrons, now magnified by 10⁷, could be seen by eye on the last phosphor of the chain - they were bright enough to overcome the read-noise of the diode array. The image-tube's phosphor had a rapid rise time but a slow decay time, and the rapid scanning of the photodiode array was well matched to the rise time. The circuits subtracted the previous frame from the current frame in real time, saving only the difference. Thus a newly arriving photoelectron was counted only once. The mean position of the broadened spot is then measured to half a pixel, and the location of the spot is

communicated to the computer, where it generates an interrupt to increment that buffer location by one (1872 x 2 array). The buffer storing the spectrum can of course be interrogated while the integration is ongoing. The second row of the Reticon measures the background sky signal and the usual practice was to integrate with the galaxy positioned on the first row, and then to move the telescope so that the galaxy was on the other row. The quantum efficiency of the first image tube was typical for photo-cathodes, around 2%, some 30-40 times lower than for a CCD, but this was still much better than photographic plates! A good thing to say about these devices is that they were not bothered by cosmic rays as are CCD's, because it would only count the CR event as a single photon.

The Smithsonian put serious funds into the instrument and we were finally rolling! I went to Pasadena for two months to copy Steve's circuits, resulting in messily wire wrapped circuit boards, and Latham took charge of the three image tubes stacked together. The electric voltages of the image tubes were ~20,000 volts and required rubber potting.

The CfA was building equipment for the MMT (Multiple Mirror Telescope) on the summit of Mt. Hopkins and we used the same type of computer, a Data General Nova 2 mini-computer. Our machine had 64 Kbytes of memory with two 350 Kbyte floppy disks (they were really floppy!), and no hard disk. Tonry was put in charge of our software development, all written in FORTH, a totally reverse-Polish language that is ideal when the computer is memory limited. Tom Stephenson wrote a "multi-overlay" FORTH system that moved programs stored on the floppy disk into a designated section of memory, thus enlarging the memory size at the cost of speed.

The resulting instrument and software was called the z-machine!

The list of galaxies was taken from Zwicky and included all galaxies with $m_B < 14.5$, $|b| > 40^\circ$, $\delta > -2.5^\circ$, which was some 2000 galaxies without redshift and 400 galaxies with redshift. Because the telescope did not always point precisely, we had Polaroid snapshots taken of the galaxies from the Palomar sky survey and taped to the back of large index cards, with coordinates and observing notes written on the front of the card. One card per galaxy. Huchra was in charge of making the cards, but we all helped. Thus we prepared to observe them all, one galaxy at a time, approximately 30 minutes of integration per object, or about 20 objects per night. However, because we could see the spectrum build up in real time, we could cut this integration short or lengthen it as we judged the quality to be sufficient.

A few weeks before we took the z-machine to Arizona, I met Nancy, my second (and current!) wife while playing tennis at the Mount Auburn tennis courts. The most memorable event, apart from her big frizzy hairstyle, was when I was serving with Nancy at the net, and my errant serve, never very good, hit her square in the back. She fortunately continued to speak to me, but I failed to mention that in a few weeks I was going to Arizona for six months. She tried calling me in my absence and assumed that I wasn't interested in her. When I returned I happened to see her

again at the tennis courts (I was on a date with another woman). We started dating and were married within two years. Together we loved doing everything, outdoors and indoors, including skiing. I am indeed very lucky to have found such a beautiful person as Nancy.

Just as we were doing final tests of the system in Cambridge, there was a huge snowstorm that completely shutdown everything for several days, and I had to use my cross-country skis to get across Harvard square to the CfA. Latham had to drive to the CfA, and he put his wife's doctor's bag on the seat next him to fool the policemen blocking the road, allowing only emergency vehicles through.

Tonry and I took the completed system to Mount Hopkins a few days later. After two nights on a 24" telescope, less time than anyone had estimated, the z-machine worked! I remember when we pointed the telescope at Orion and we were somewhat surprised to detect all the emission lines, as neither of us had ever taken a course on the interstellar medium. Live and learn.

The z-machine worked well enough to become the default instrument on the 60" Tellinghast reflector. Our team took virtually all of the dark-time, which was unheard of in those days! It is extremely fortunate that I was at the CfA at the time, because the survey could not have been executed without the very generous allocation of time that only Harvard/Smithsonian could grant.

The z-machine is probably 1 million times slower than, for example, the SDSS, but in 1978 it was the best available. Including the lousy weather, two years were required to complete the survey of 2000 galaxies.

In 1979 there was a meeting in Tallinn, Estonia, where the Soviets, including Zeldovich, the leader of cosmological research in Russia, would be present. I was invited to attend, and I recall staying overnight in Helsinki and taking a local ferry to Tallinn the next morning, the most efficient way to Tallinn. That night I met George Efstathiou, a British graduate student working on numerical simulations of cosmology. He had adopted a code of Eastwood that could evolve 32^3 particles, as opposed to 1000 particles in the Aarseth code that was the alternative at the time. We became friendly, particularly since I paid for supper for the two of us, given that George had no money. I did not realize at the time how important George's code would be for my future research.

At the meeting, I recall standing at a urinal with Zeldovich standing on my left. While doing our business, Zeldovich asked me how the work on BBGKY was proceeding. I told him that I had set it aside, and that I was heavily involved in an experiment, the measurement of 2000 galaxy redshifts. Zeldovich shook his head slowly, as if to say that is a shame. This is an example of how the Russians placed theory far above experiment. I smiled.

In 1979, the CfA1 survey was nearing completion. I remember Latham telling me that I should stay at Harvard instead of traveling to Mt. Hopkins, as somebody who understood what we were doing had to write-up our results; therefore I started

writing. The first paper with Huchra, Tonry, and Latham talked about Virgo-centric flows and the mass density of the Universe. This is a topic to which I have come back many times, as it affords perhaps the best way to locally measure Ω_0 . The next two papers detailed Tonry's thesis in which he and I presented velocity dispersions of elliptical and lenticular galaxies as measured by the CfA1 survey, and then used them to measure the local infall of the Milky Way toward the Virgo cluster, as we had outlined earlier. In addition to properties of early type galaxies, this presented a method for measuring the local velocity fields and to solve for Ω_0 . But in order to measure Ω_0 , one needed a full sky estimate of the local gravitational field and it would be 15 years before such field was available. At the time there were few methods that could measure Ω_0 , so that this type of measurement was extremely important.

Meanwhile, Nancy and I together had bought a 3-decker house in Watertown. Fixing it up became our major activity. We often went to Vermont for weekends to ski, and one week, we flew with my friends from graduate school, Ed and Marlene, in Ed's 4-seater airplane, to Jackson Hole, Wyoming. The skiing there is truly magnificent but much of it was way above Nancy's abilities at the time. One day when Nancy and I were in a chairlift very high off the ground, I proposed we get married, in a rather awkward fashion. She could have pushed me off the chair, but instead she accepted. That was early in 1980 and we were married at the MIT chapel in August of that year.

The paper detailing the maps of CfA1 survey was written next, but the results did not appear until 1982, after I had left Harvard. The data shows the large voids and filamentary structures with clusters of galaxies at their intersection. I included Efstathiou's results of an isothermal large-scale structure model, which at the time was thought to be a good model of large-scale structure. The model compared to the data was appallingly bad! At that point I knew that explaining this discrepancy was the most important project I could imagine, far more significant than collecting more data.

At this point, I had been promoted at Harvard and completed the Assistant Professorship of six years. In 1980 I was promoted to Associate Professor, again without tenure. With hardly any publications and little interaction with the regular faculty, it was no surprise that my recommendations from inside Harvard were weak. I had been working so hard to build and complete the survey, and simply did not have time to write a paper every month.

Just as the redshift survey was breaking into paradigmatic status, I found out that Harvard astronomy was looking for a senior professor, and that their favorite candidate had done his best work over a decade earlier. At the time, Harvard's approach to senior appointments was very conservative and self-defeating. The list of faculty that were denied tenure at Harvard through the early '80s was very impressive and would make a fine department, but now Harvard has learned their lesson and tenure their young stars.

At that point, in early 1981, I decided to leave. Harvard finally decided to offer me tenure, but only after I already had an offer from Berkeley. Of course the offer came in handy to negotiate my salary at Berkeley.

Chapter 5: Going to Berkeley

Living in Berkeley, with its proximity to fabulous skiing, was not my only consideration in leaving Harvard. It was also a chance to work with Simon White to attempt computer simulations to try to understand the peculiar fluctuations in the CfA1 survey, which seemed both filamentary and frothy, looking nothing like the power law initial conditions we thought were 'reasonable'.

Soon after arriving at Berkeley Nancy and I went to a Vatican sponsored meeting on galaxy formation and large-scale structure, which met in a very fancy villa in the Vatican that had been constructed for some Pope's mistress 400 years ago. We met the current Pope who was weak from a recent gunshot wound, and I remember his comments that astronomers can provide the details of the universe but faith was needed to explain 'why' the universe exists. Was he talking about inflation?

Once Nancy and I got back to Berkeley, we realized she was pregnant – perhaps the baby was blessed by the Pope? We were living in a rented house and I spent most weekends riding my bike to scan houses for sale in the Berkeley hills. We were a bit nervous about whether we would get any assistance to our mortgage from the campus, as inflation had caused interest rates to climb to outrageously high levels - 18%. (This is the same time as the 'inflation' model of the early universe was proposed – coincidence?) This certainly put a damper on housing prices but even then it was very difficult to afford anything with a professor's salary. After 8 months we were finally told that the University would give us a loan at 12%. Well, 12% is better than 18%, so we gratefully accepted. I found a house that had a remarkable view and layout. The only problem was that it had been owned and renovated by a '60s hippie DIY'er and the decorations were absolutely atrocious. Shingles in the bathroom that made it look like an outhouse? Red and silver wallpaper in a bedroom? The 'Bezerkeley' wiring that brightened living room lamps as the toaster in the kitchen was turned on? (This turned out to be a disconnected neutral wire outside.) This was why the house had not been sold in six months on the market. The owner was getting desperate, so we were able to negotiate a decent deal. Nancy and I had had plenty of practice dealing with 'fixer-upper's like this house.

The house closed in May 1982, with Nancy very pregnant by this time. Looking over some old files, I found a letter I had written to Peebles in which, among other things, I mention how large Nancy's belly was! A month later she delivered our son, Jeremy, 10 lb. at birth! We had been working feverishly to complete decorations in his room, needing a neutral colored wallpaper since we had no idea of his gender. Next we removed all the shingles in the bathroom and discovered they were all covering

serious leaks in the ceiling. The housing inspector was none too happy as he was supposed to find all these problems and instead ended up paying to redo the walls!

At work, I wrote the final papers from the CfA1 survey. Huchra took charge of our data paper, and Peebles and I wrote Paper V of the CfA1 survey. This paper introduced quite a few of the figures that have become standard in all redshift surveys, such as the two dimensional plot of $\xi(r_p, \pi)$, and the discussion of the fingers of god and the collapse on larger scales. Given the relatively small size of the survey, we were unable to measure the correlations on scales larger than $20h^{-1}$ Mpc, compared to the scale of the SDSS correlations, $140h$! It was abundantly clear that isothermal power law models did not fit. Of course, CfA1 is only a first approximation to the correlations seen from much larger surveys.

Carlos Frenk was hired as a postdoc and by 1982 Simon, Carlos, and I had already demonstrated that massive neutrinos could not be the dark matter. Cold Dark Matter had recently been discussed and as it was new and promising, we decided to use George Efstathiou's nbody code which could be run with 32768 particles, a big step up from the 1000 particles our miserable computers could manage until then. Thus was formed the famous collaboration known as DEFW, or the "gang of four".

The biggest computer available to us was a Digital Equipment VAX 11/780, featuring all of 16 Mbytes of memory! Our job had to fit within half of that and therefore the largest potential grid we could manage was 64^3 and 32768 particles. The Berkeley astronomy department had purchased one of these giant machines with the aid of a grant from the NSF. The VAX occupied half of a large air-conditioned room. Our giant simulations ran only at night, giving the remainder of the department a chance to use the machine. I was fortunate enough to have a VT/100 terminal, not a graphics terminal, in my office.

In 1984 there was a seminal 4-month workshop at Santa Barbara's Institute for Theoretical Physics where all the workers on large-scale structure gathered to discuss the current thinking and problems. Dick Bond, Nick Kaiser, Alex Szalay, Jim Bardeen, Bernard Carr and DEFW, plus many others were all in attendance for four months. We were still running models of cold dark matter on VAX's, both in Berkeley and at Santa Barbara. Santa Barbara was where we wrote the paper presenting results from our nbody simulations, trying to meet the data in whatever method we could quantify.



Figure 1: DEFW, 1983

Cold dark matter models certainly were an improvement for matching the observed large-scale structure. However, pure cold dark matter models with $\Omega_0 = 1$ did not match the observations if galaxies are unbiased tracers of the underlying mass distribution; the peculiar velocities of galaxies were predicted to be much too large in this case. Furthermore, there was insufficient large-scale power unless Hubble's constant was of order 20 km/s/Mpc, a nonstarter. Better agreement was obtained for $\Omega_0 = 0.2$, and we found that the model with a cosmological constant Λ closely resembled an open model with the same value of Ω_0 . But we followed the dictum of Dave Schramm, who often stated at important conferences that "one could not have two tooth-fairies in one paper" (the cold dark matter particle and the cosmological constant). Thus we had no choice, cold dark matter was our theme, and Λ was therefore disallowed. In those days, an open cosmology was not considered to be a tooth-fairy.

Nick Kaiser had recently shown that in a Gaussian distribution of noise, regions at high density threshold are 'biased' and could explain the large correlation length of the distribution of rich clusters of galaxies. We figured that if galaxies themselves are heavily biased relative to the dark matter, one could possibly explain the discrepancies with $\Omega_0 = 1$ models. DEFW tried some simple biasing prescriptions and showed the mechanism was extremely effective. However, in order to make the galaxies compatible with $\Omega_0 = 1$ required a bias amplitude of bright galaxies, of the order of $b = 2-2.5$. Such high values of bias were not ruled out at the time.

One of our papers showed the CDM model predicts galaxies to be more clustered than the underlying mass distribution and that the bias increased with mass density, as seen very clearly in recent redshift surveys. This is confirming our hunch, but does not indicate the bias is as large as we needed. In fact, the modern values of bias are now of order 1.5, a strike against our work, but due to our unwillingness to include Λ in the model, we had to state the universe was open.

Nancy, Jeremy and I lived in a rather relaxing Santa Barbara house a few miles from the Institute. The house was less than a quarter-mile from the ocean and in the mornings I would often run along the beach, disturbing the pelicans, in an effort to clear my head. I would bicycle to and from work, transporting Jeremy, now approaching two, to nursery school in a seat on the back. The house had several mature avocado trees and therefore Nancy and I were the official guacamole providers for weekly get-togethers. The Santa Barbara retreat was a great time, particularly since we had apparently solved a major astrophysical puzzle, lifting us all to another plane.



Figure 2: DEFW taken at my Festschrift, 2007

I was invited to give summaries of our simulation results on cold dark matter to many physics conferences. This community was enthusiastic about our recent results because if astronomers could tell them *something* about the dark matter, then indeed that was real progress. Suddenly, physicists of all types recognized that the large scale structure of the universe was an essential clue to what happened at the very beginning.

After the Santa Barbara workshop, the gang at Berkeley broke up. Simon did not receive a faculty offer from Berkeley and soon left. I was devastated, but there was little I could do.

Nancy and I have had membership in ski houses for every year since we moved to Berkeley, except when we were in Santa Barbara and then on sabbatical in Europe. There was a conference in Berkeley after the Santa Barbara workshop that featured several days of skiing just before it began. A pile of astronomers, including Dick, Simon, Avery, Carlos, and several others went up to the house we were renting in Truckee. That night there was a 4 foot snowstorm, fairly normal, but it closed down everything until the roads were cleared. I remember Dick's car being completely buried. After two days stuck in the house one ski area was finally opened, so we went.

I was the only one who had a car with four-wheel-drive, and all of us piled into my little Toyota 4WD wagon by sitting on laps. The road to the house had not been plowed, but there were tracks of a Jeep that defined a path. The snow was deep enough to generate a plume that blocked my vision, and as a consequence I rammed a snowbank, lifting all 4 wheels off the road. It took a while to dig out, but fortunately another Jeep came by and pulled us out.

We finally got to NorthStar, a beginners area that is pretty flat, not what you want in very deep snow. We took the lift to the top and started to go. The snow was up to my waist and we were basically stuck even when pointing our skis straight downhill; I remember going about one mile per hour in the deep snow. To make the situation worse, several of us were first time skiers, including Carlos and Avery. Fortunately the snow was so cushiony that it was just about impossible to break your bones. After an exhausting day and a half, we made it back to Berkeley. The conference was great, but who can remember what was said?

Soon afterwards, our second son, Adam, a redhead, was born, another 10 lb. baby. He was incredibly active, never wanting to sit on our lap so we could read him a book, but tremendous fun. Such a character, with loads of personalty.

In the spring of 1987, Alex Szalay organized a workshop at Lake Balaton, Hungary. The kids were old enough to go to Europe for a few weeks, although Adam was only 18 months old. It was a fine conference and a chance to engage with European astronomers. After the conference we spent time in Budapest and went to Dubrovnik, Yugoslavia, where we stayed for about a week. Dubrovnik was a beautiful old walled city on the sunny Adriatic. The history of the cities in this region is fascinating.



Figure 3: Jeremy & Adam with wound, coming from Dubrovnik, 1987

There were three important things we learned about visiting a communist country in 1987, and they all converged in one evening. The first was that decent disposable diapers were not available. Second, you had to be certain to spend the last of your local currency, because the authorities wouldn't allow you to exchange it for dollars or anything else useful outside of Yugoslavia. And third, the

medical care was circa 1930s.

On the night before we left, we had made sure we'd have just enough currency for dinner and a ride to the airport. Nancy and I were washing up in the bathroom, the boys were as usual jumping on our bed, when suddenly Adam gave out a scream. Rushing into the room we saw blood pouring down his face. He had banged his head, just between the eyes, on a corner of the headboard. Nancy grabbed Adam, dressed in only his lousy diaper, and wrapped him in his 'blankie'. The man at the desk downstairs called a cab to take us to the local hospital, which bore a strong resemblance to a World War I clinic. The doctor was already gone for the night and had to be called back, thus giving Adam a chance to bleed some more. Eventually the doctor showed up and proceeded to stitch Adam with what looked like black yarn. Nancy worried for weeks about whether Adam would have a discernible scar, and he did – just like any kid. We were given a bill that we misunderstood, thinking it was 100 times higher than it was. It took some time for us to communicate, but the dinner money was sufficient. We went back to the hotel on a bus that we didn't pay for – I think the driver took pity on us carry a baby in a totally bloodied blanket and otherwise naked. We had enough money to get to the airport, where a lovely stranger bought us espressos!

That morning we flew to London to spend a few weeks at Cambridge where I would meet the remainder of DEFW and strategize for the coming year. The Gang of Four published nine articles over the period of 1984 to 1992, all of them detailing aspects of a CDM universe. In the final paper there is discussion of the recent observational result by Efstathiou et al. detailing how CDM did not have enough power on large scales to match the observations. Once again, we could not accept the rather simple idea that Λ could play a role. The demise of CDM seemed near.

We tried some ill-fated models of Lyman alpha clouds with CDM; I did the runs with more particles on a Cray supercomputer located in San Diego that was accessed via a dedicated line from Berkeley. The process was cumbersome, and I was rather

tired of fighting the computers. So, I gave up, particularly since we were having trouble understanding how to identify the baryonic clouds. Remember this was before nbody codes included gas physics. Now, with everything so accessible, I can simply take from online files those simulations that I need for a given purpose.

I returned to more observational work. In particular I finished my work with Luiz DaCosta on the Southern Sky Redshift Survey, opposite to the CfA1 in the sky. Instead of the large Virgo cluster of galaxies in the foreground, the new survey had instead a low density region. But the statistical features of the two surveys were equivalent.

In 1991 I was elected to the NAS (National Academy of Sciences), a tremendous honor especially since I was so young. The following year I was elected to the AAAS (American Academy of Arts and Sciences). The work of the CfA1 and DEFW had by now become the proof of the standard explanation of the observed large-scale structure.



Figure 4: Marc stands with the Einstein statue in front of the NAS building in Wash. DC, 1992.

The NAS likes to appoint its young members to chair important committees, and in 1993 I was appointed to lead the CAA (Committee on Astronomy and Astrophysics). This committee talked to leaders of NASA and the NSF and made recommendations, everything at a very high level. We did one study to evaluate a proposal for NSF

astronomy to redirect the effort of NOAO. At the time they were thinking of building numerous small telescopes to satisfy the demand from small institutions around the country. The committee didn't like the proposal at all, and as chair, I shared their unhappiness. But I wasn't the most polite or politic chairman, and I didn't make friends. I was later called before another committee where they gave me hell, politely, after which I didn't get invited to join any more committees. Students have experienced my unwillingness to suffer incompetence, so this is no surprise to them.

A question of considerable interest in cosmology is the anisotropy and inhomogeneity of the galaxy distribution and the rate of approach to homogeneity on large scales. Linear perturbation theory in an expanding universe has very simple boundary conditions: the observed peculiar *velocity* of a galaxy is proportional to the present value of the gravitational *acceleration* acting on it. The earliest motivation was to derive a value of Ω_o , but even though the question has since been answered by CMB experiments, it is of interest to detect the mass fluctuations that cause it. If we presume that galaxies at least coarsely trace the mass on large scales, then with complete red shift information we should be able to measure directly the peculiar acceleration acting on us. To measure the peculiar acceleration derived from the galaxy distribution, we should use a whole sky catalog unaffected by galactic reddening, which is certainly not the case with optically selected catalogs. With the advent of the IRAS (Infrared Astronomy Satellite) point source catalog (This is a list of point sources at infrared wavelengths which are external galaxies), it became possible for the first time to collect a list of galaxies over nearly the whole sky, unaffected by reddening because the wavelength of the selection was 60 microns. An optically selected sample would be limited at $|b| > 40^\circ$, but the IRAS selection allows us to choose all galaxies with $|b| > 10^\circ$, more than double the solid angle of sky covered.

The IRAS sample of galaxies probes to substantial distance, well beyond the local supercluster, making it ideal for measurement of our peculiar acceleration. Avery Meiksin and I first studied the IRAS catalog and compiled a list of galaxies for further study. In 1986, Michael Strauss and I, plus collaborators Amos Yahil and John Huchra, initiated a redshift survey of the brightest 2652 IRAS selected galaxies. Later Karl Fisher and I extended the IRAS survey to some 5000 galaxies. This catalog of IRAS galaxies served as the best estimator of the local gravitation field for approximately 15 years, and has recently been superseded by the 2MRS distribution, which is a full-sky sample in the K band.

At the time, all my students worked on related projects. David Schlegel, for example, did his thesis on obtaining Tully-Fisher information for a subset of IRAS galaxies, but the scatter was too large to be useful. It was not my best suggestion for a thesis topic! Jeff Newman, using the HST, did a difficult measurement of the Cepheid distances to a few galaxies in the Hydra-Centaurus cluster.

In the early nineties, Adi Nusser, a student of Avishai Dekel, came to work with me on analysis of the velocity fields. Adi devised an outstanding method of expanding

the velocity field of noisy TF data to an orthogonalized basis set of velocity functions. The velocity problem was reduced to measurement of the amplitudes of 20 or 40 basis functions. The filtering length was sufficient to smooth out nonlinear effects. The velocities derived from the gravity field could be filtered by the same basis functions, and the problem became one of simply comparing the amplitudes, v-v. This method was very, very slick; it is, in my opinion, the best possible that could be devised.

Of course we used full nbody simulations to calibrate the performance of the method, and it worked as expected. But when I compared the real fields, there was serious disagreement, leading to very poor chi-squared/DOF =2. Something was wrong.

There was a tremendous fight 20 years ago for the derived value of Ω_0 based on analysis of the local velocity field. Avishai Dekel and Ed Bertschinger had devised a method known as POTENT, which compared the density field obtained from the IRAS maps to the density derived from the divergence of the flows obtained from Tully-Fisher velocity fields. Adi Nusser and I, instead of comparing density-density, devised a test that compared velocity-velocity. That is, the velocity inferred from the Tully-Fisher data is compared to the linear theory generated velocity based on the IRAS density field. The POTENT method consistently gave $\beta = \Omega^{0.6} / b = 1$, whereas the v-v method consistently gave $\beta = 0.4$, with very poor chi-squared/DOF =2, as I've said. POTENT is a maximum likelihood procedure for which one cannot derive the quality of the fit. The discrepancy between the methods turns out to be due to systematic errors in the TF catalog that was constructed out of four distinct catalogs with poor overlap, leading to false large-scale flows. This was unfortunate.

When Avishai and I would argue at conferences, the audience would throw up their hands in dismay. Here were two acclaimed experts disagreeing on a data analysis technique. I strongly suspected a bad TF data set, but there was little I could do. The method was therefore judged unreliable, which is a shame because we would have had a value for β well ahead of the CMB results.

There were conferences that I attended at the Aspen Center for Physics, and I especially liked the winter workshops. Their usual habit was to hold talks in the mornings and then resume after the slopes had closed. I remember how Bernard Sadoulet, Katie Freese, David Schramm and I would be dressed for the morning talks in our ski gear so we could maximize the time in the snow. We all loved the Aspen skiing scene, but since I would typically ski 15-30 days per *year* back home, I had had plenty of practice. I must confess that I had hubris over the fact that my skiing technique was so much better than everybody else's at the conference; I did enjoy coming down the face of Aspen on a black diamond slope, leaving my colleagues, 15 years younger or more, way above me. I remember having a great time skiing with Uros Seljak and his girlfriend, now wife, Petra, who were as good as me, and we flew down the mountain.

From our earliest days in Berkeley, Nancy and I had organized ski houses. We were mostly in a group of about 6 families that rented a ski cabin in Tahoe Donner, about a half hour from Squaw Valley. There were 6 boys close to Jeremy's age in the house, and no skiers of Adam's age. (Does this explain his dauntless technique as he tried to keep up with the older boys?) The kids were all members of a ski school at Squaw called Mighty Mites, and it was my job to get them to the 'school' by 8:15. Every weekend they would ski with the same group of ~8 kids and their 'coach' {instructor}, and they were little bombers to behold. Squaw trains experts, all of whom were Mighty Mite skiers at a young age.

Jon Arons and Claire Max, father of Sam, were cabin members whom I had known from my days at Princeton. Claire decided to buy a cabin in ski country in 1998 or so; the 'cabin' is twice as large as our house, and Nancy and I have our own bedroom, with plenty of space to store all of our skis and clothes. This makes it incredibly easy to simply go up before a major storm hits.

At Squaw Valley I remember taking Scott Tremaine down a very steep slope. I believe he thought that once on that slope was enough. David Schramm, Bernard Sadoulet, and I had great times in the soft snow. Dick McCray, an old friend, was with me the day Jeremy told me that a ski patrolman had taken his ski pass and we had to go talk to him. So I accompanied my sheepish kid, then ~14 years old, to the patrol shack where the patrolman told me that Jeremy and two other friends were speeding in a slow zone.

Well, I suppose that speed is part of the game. Both of my boys had their first day on the snow at age 3 and developed into incredibly polished skiers, compared to me who was a rank beginner at age 19. When I was invited to give talks in Vancouver, I took each of them with me when they were ~11 years old; we would go to Whistler Mountain for a few days of awesome skiing.

In the year 1995, I purchased a GRAPE processor (GRAVity PipELine), a board that that was connected to my MicroVAX. The GRAPE board was very efficient at calculating the $1/r^2$ forces and for a while they were very popular among nbody simulators. Matt Craig and I used the machine to calculate the shape of a dark matter halo dominated by annihilating dark matter, a model briefly in focus at the time. We could flatten the halo's core if the annihilation cross-section was large enough. But there are other explanations for flattened halos. I believe the GRAPE is now with Matt.

Also in 1995, I was talking to Schlegel about his beautiful results which showed that the APM galaxy counts were anti-correlated with the dust density measured by the FIRAS (Far IR map, a COBE product). We agreed to get another student to work on this project, possibly as a way to produce a reddening map. Doug Finkbeiner was a student who was planning to work on DEIMOS, but the project was very late and analysis of the FIRAS map would be a 'simple job that would perhaps take a summer'. Two and a half years later, after Doug managed to fold in the higher resolution DIRBE map, we produced our famous dust map that has now been cited

by some 9500 papers. A slight underestimate of the work involved! Seven papers resulted from the collaboration.

In approximately 1998, the UCO optical shop began to build a new spectrograph for the Keck telescope, known as DEIMOS, with advice from Sandra Faber and me. We had in mind a project in cooperation with David Koo and Raja Guhathakurta that was known as DEEP2. The purpose of the project was to obtain a large spectroscopic sample of galaxies at $z \sim 1$, roughly 50,000 galaxies, that would allow us to study properties of galaxies from the spectroscopy, as well as their clustering. DEIMOS has a field of view of approximately 16×4 arcmin and allows approximately 150 faint galaxies for long-slit spectroscopy in each mask. Approximately 360 masks were milled for the DEEP2 project.

I gathered a substantial research group for this project, including graduate students Jeff Newman, Alison Coil, Douglas Finkbeiner, Michael Cooper, Brian Gerke, and



Figure 5: SFD at my Festschrift, 2007

Renbin Yan, postdocs Christian Marinoni and Darren Madgwick, and the undergraduate Charlie Conroy. The group initially was charged with the data reduction software project and fortunately we could use much of the code written by my ace former graduate student, David Schlegel, that reduces all the SDSS

spectra. I believe we

started the package in 2001. One night Doug and I were at Keck with Sandy and Drew Philips as DEIMOS was being debugged. Doug and I were rapidly debugging our software in an effort to keep up with the data flow. A great time and I learned so much.

I remember some of the good times Doug and I had together, for example the time we biked in the Berkeley hills. We took the Pinehurst loop from my house, a 30 mile ride with plenty of hills and redwood forests that takes you to the top of the Berkeley hills. I was strong at the time and pushed Doug all the way. Or the time we went skiing at Squaw Valley under pristine conditions. Doug was a beginner and I took him up a short, steep trail that seemed pretty flat to me. At the top of the trail,

only ~300 feet up, I realized that the snow was really quite good, so I left him and took off for the steeper terrain above. But Doug did get down without me.

Chapter 6: Stroke!

In November, 2002, I was attending a business meeting at the Space Telescope Science Institute in Baltimore. It was the end of a rather boring day, the type that makes you rather sleepy. I started having very odd feelings, my left arm was dragging, I was losing vision in the left eye, and when I stood I felt unstable. I walked, with difficulty, to the cocktail room in our hotel before dinner. Steve Beckwith noticed that the right side of my face was drooping, recognized that I was having a TIA (trans-ischemic attack), and immediately drove me to a Johns Hopkins hospital.

A TIA is a transitory stroke in which a clot gets stuck temporarily in your brain, denying it of oxygen and causing a change in some aspect of activity, such as speech or movement. The clot clears and all symptoms disappear, as mine had by the time Steve and I got to the hospital. The doctors were alarmed, however, and ordered brain scans and a sonogram of my heart, in which they heard a slight murmur, I'd never been told I had one but didn't register this as a change to worry about. The doctors suspected a clot had come from the leaking mitral valve that caused the murmur, but nothing showed up on the sonogram. The doctors kept me awake throughout the night but we never could find the source of blood clots.

Having a TIA is extremely dangerous, because between the episodes the patient feels so normal that there is a tendency to disregard everything. Which is why I wasn't too concerned when several months later I had several weird episodes, like losing my peripheral vision

The doctors would have kept me in the hospital for further testing, but since I wanted to visit Jeremy at Princeton, I told them that I would check into this as soon as I got back to Berkeley. I did see my PCP, who referred me to a neurologist, who then referred me to a cardiologist. Within a week my heart had been scanned from closer up via a Trans Esophageal (down the throat) Echocardiogram, with the same result as before – no sign of a clot.

I was invincible and did not realize what was happening. All I knew was that I felt fine. When the doctor says that the clot is not there, what does he mean? Is it ruled out with 95% likelihood? Is it ruled out with 99% likelihood? 99.9%? I was so stupid that I did not ask the obvious question, but had I realized that when he said there is no clot on the mitral valve, he meant 95% likelihood. Had I realized this, I would have told him to keep looking, or use some other test! The cardiologist started to look for a possible clot in my leg, but that would require the clot to go through a hole in my heart, because the blood from the legs heads to the lungs. This scenario was extremely improbable and was contraindicated by my athletic ability.

A clot in the mitral valve is usually caused by blood getting caught in the small portion of the flapping valve that doesn't close completely. If there is an infection present, a flower-let of infected material can also form. In June, I was feeling tired from what I thought was overwork, and I also developed muscle spasms in my back. These can both be symptoms of infection, but no one put two and two together. I cancelled a scheduled trip to Israel to talk at a scientific meeting, and instead spoke over the phone while my viewgraphs were presented. I have never done that before! Little did I know that there was in fact a clot on the heart valve that had grown quite large by then. I received the results of a blood test 4 days before the stroke that indicated some inflammation somewhere in the body, but again, the doctors missed the connection. A blood culture would have found the infection, but during the preceding months, no one ordered one.

The morning of Friday, June 27, 2003, I exercised as usual on my NordicTrack, pushing myself so as to raise my heart rate and to generate some sweat. I then walked upstairs and had a glass of orange juice., I remember falling, or tripping, over the chair in the kitchen. Adam fortunately heard me fall, got out of bed, and called 911. I awoke to the scene of four giant men standing over me, ready to take me to the hospital. Nancy was in Fresno for work that morning and Jeremy was at Princeton. A dear friend, Merle Fajans, came to the hospital to be with Adam. Once again, I appeared to have had a TIA, as I was talking with the doctors about astronomy and could move all my limbs. The doctors could have given me TPA, a blood thinner now given to all stroke patients coming into an emergency room, but since TPA can cause bleeding in the brain, and it looked like I was out of danger, they didn't. Within several hours, however, it was clear I'd had a stroke, and it was too late for TPA. Within a short time, I was completely out of commission. Given its prominence in my life, I'll hereafter call this disaster the Big Event (BE).

Chapter 7: Recovery in the hospital

I spent the first two weeks after the stroke in an acute care hospital. Nancy had mistakenly given me water while I was waiting for a CT scan, not realizing that the stroke had hit and I couldn't swallow. This caused an aspiration pneumonia and resulted in my being fed through the nose while there, because anything I ate would go to my lungs. To makeup for it, Nancy stayed with me in the hospital, attempting to sleep on a pink naugehyde fold-out chair. As if anyone can sleep in a hospital. I lost 25 pounds in two weeks, which threatened to delay the start of intense physical therapy. Also, the rehab facility was reluctant to take me because I was still on IV antibiotics to kill off the infection that caused the stroke. My feisty Israeli-born neurologist saw to it that I got admitted and started therapy immediately.

I remember a crazy dream that I had the first week in the hospital. In it quantum effects in a many-worlds interpretation had divided the Universe and made a fully symmetric replica of me. Its left side was damaged and the right side was intact. For some reason this universe touched ours on Tuesday nights, and I had to hide in the

closet with a big knife so I could split this image (me?) down the middle and then paste the undamaged right side onto myself. But to do this I had to disengage all the tubes attached to my body including my feeding tube, my IV, etc. Then I realized that I would fall down if I got out of bed. I did end up pulling them all out. A nurse came in and said 'oh no', so I pretended to be asleep while she reinserted my feeding tube.

In the rehab hospital, to which I was transferred after two weeks, I still wasn't able to close my glottis (i.e. swallow). To deal with this I was forced to drink only thickened water, which was pretty yucky, and eat pureed food. Double Yuck! The hospital tried its best - they artfully formed mashed carrots into the shape of carrots, for example.

Lab tests showed that the bacteria responsible for the blood clot was a common species that thrives on your teeth. Apparently some of them snuck into my blood stream when I was flossing my teeth, which often brought out a little blood. The bacteria must have made it to the mitral valve before it was eaten by white blood cells, which have trouble reaching the bacteria hiding in the flaps. All this occurred probably months before my TIA episode. This is why doctors prescribe antibiotics for patients with heart murmurs to take before having any dental work done.

It was a simple matter to kill whatever bacteria remained on the mitral valve. They threaded a thin tube from an artery in my arm to my heart, and simply flooded the heart with antibiotic. Just another tube stuck into my arm! It was pretty neat that the tube had a radioactive tip that the physician could 'see' as it was threaded to the heart.

Early in my stay at the rehab hospital, I asked Nancy to bring a framed photograph of us and the kids at the top of Squaw Valley. It shows a gorgeous day, blue skies, beautiful snow, and Lake Tahoe in the background. I thought that there was no reason that I couldn't take up skiing again. After all, you don't have to lift your legs - rather it is a matter of facing downhill, weighting your skis, and bending your knees. Simple! I decided that I would get back to the snow as soon as I was able. But the doctors were noncommittal when I talked to them about this fantasy.

I was assigned a wonderful, compassionate neuropsychologist at Herrick, who explained why I had such extensive damage to my right side. The clot hit at the trifurcation of the carotid artery as blood enters the brain. The neurons closest to the neck control the head and arms; those furthest up the legs. My clot hit very close to my neck, affecting, therefore, my whole body. As my therapist said, instead of blocking the river far upstream, say in Pittsburgh, the stroke had lodged at Vicksburg and destroyed everything to the north. She likened its blood supply as the Mississippi River flowing backward.

The damage was to the first motor cortex on the left-hand side of my brain. An MRI scan shows a tremendous hole and dead neurons and axons, looking like tubes, running down into the neck. The affected region controls the arm and leg, so it was no surprise that the damage was so extensive. My right shoulder and right arm

remain completely useless, and my leg should be too. It appears that there were several ancillary blood vessels that had developed outside the artery that rerouted some blood from the “south” further to the “north”. My right leg is perhaps 50%-70% under the control of my cortex.

The speech center on the left side was also destroyed, and by all accounts I should not be able to speak. One of the few silver linings of the stroke was that I was already left-handed. About 70% of southpaws develop secondary speech centers on the right side of the brain. The doctors said that my right-sided center must have been fully developed, as I had only minor loss of nouns. When I can't think of a word, I say it must be because of the stroke, but I don't really know that it isn't my age. Anyhow, it's a good excuse.

Early on, I was shown a drawing of a toothbrush and was unable to recall the name. I knew what it did and I rubbed my finger on my teeth as a demonstration, but I was simply unable to recall the word 'toothbrush'. At this I got very scared. How would I ever teach class?

The swelling next to the actual damage is called the penumbra (a smart choice!), and within a few weeks the penumbra decreased and I regained some brain functions. That was incredibly good news, as I understood the word 'toothbrush' and lots of other words! In fact the news was so good that I didn't need any speech therapy.

I was in the rehab hospital for almost 11 weeks as the therapists worked to get me to stand, to sit, and to take a few steps, including climbing stairs. I was otherwise confined to a wheelchair. Painful work on my arm led nowhere. I look at the old photos and see myself leaning far to the left and putting my weight onto a four-prong cane, thus reducing the weight on the right leg. All the time I was stuck at the hospital I saw stroke patients, many considerably older than I, making incredible progress very quickly. But my damage was more extensive.

Martin White and Joanne Cohn visited me in the hospital and brought Martin's lecture notes for extragalactic astronomy, but I just couldn't focus well enough to look at them. Perhaps it was the medicine that was fogging my brain or perhaps I just didn't give a damn? That first year, and ever since, I was unable to read scientific literature at anywhere near the speed nor process it as quickly as needed to keep up with the ever changing field of cosmology. As a result, I have been increasingly disengaged from astrophysics, even though I read all the time and attend scientific talks. I do maintain an interest in science, however, and am finding a few things to dabble in.

Chapter 8: Recovery at home

Finally, after 11 weeks in the two hospitals, I was released to go home. Therapy continued at home and I spent most of the time in a wheelchair. But at least I was home, able to sleep with Nancy!

There is a medicine called baclofen that I must take every day as it relaxes my nerves that have no connection to the cortex. Otherwise they would always be in a state of tension, so that for example the quadriceps would constantly pull in opposition to the hamstrings, and my leg would be too stiff to walk. I would otherwise have to resort to a wheelchair. The problem is that oral baclofen fogged my thinking and couldn't be taken in high enough doses to be really effective. To avoid this I had an implant the size of a hockey puck buried into my abdomen with a thin plastic output tube that was inserted into my backbone. Every 10 weeks the implant is filled via an external needle holding 40 mL of baclofen that is poked through a rubber stopper into the implant. The device then talks to an external home machine on a network similar to WiFi, and through this connection the doctor can adjust the rate at which the baclofen is dispensed, so the implant is really bionic. I had one implanted about two years after the BE.

I have a second device, a WalkAide, that I wear every day, strapped around my leg just below the knee. Electronics in the device contain an accelerometer that senses when the leg is moving forward while walking. At the right time, it shocks my peroneal nerve, which runs down to my foot, causing it to dorsiflex (bend upward). Without it my foot would drag on each step since I can't move it on my own. These two electronic devices make me a bionic man, although the cost of them was considerably less than the \$6M they spent building the bionic man on the old TV series.

My damage is entirely to the motor cortex; in the affected areas I can feel everything but I cannot move anything. I have no pain. It is basically like a burned out electronic motor controller. I used to joke with people telling them I have looked for the right motor controller at RadioShack but just couldn't find it. There is space in my skull for a thin board and for its fiber-optic cable as well. Some day? I asked a Cal physician about pouring stem cells into the empty space, but he was not too encouraging, stating that this was more likely a good way to get cancer. And besides, how would a nerve cell be able to grow the long dendrite that connects the brain to the muscles?

Only the wiring from the cortex has been affected, whereas the remainder of the brain, including the autonomous nervous system, is still connected. I call this system my "frog brain", as it controls functions not consciously directed, including the fight or flight reflex. For my right side the frog brain has become my dominant nervous system, without the more advanced cortex to filter information and allow logical thinking. It is truly astounding how it screams at me when it judges I'm doing something too dangerous. For example, take walking downhill on a very slightly inclined slope. Unless I angle myself so that the left leg is in front, with the cane and right leg behind in a triangular stance, the frog brain just won't shut up. The fear exhibited is too much for me to deal with; it is much easier to hold a rail or Nancy's hand.

The fight-or-flight reflex has some humorous aspects to it. For example, when I am sitting in my chair reading a book, if an unexpected noise suddenly occurs, such as a

dropped plate, my left arm doesn't move, but my right arm jumps way higher than I can ever command it to move. It is the damndest thing! In talks to graduate medical students at Berkeley, I've suggested that they get a 'temporary' cut in their cortex nerves and try it for themselves. The funny thing is there were no volunteers.

A year after BE I slowly got back to work, including teaching an introductory course and a course on cosmology at the senior level. Meanwhile my group was working hard to extract science from DEEP2.

The access provided to the community by the SDSS redshift survey is a good testimony of a new way of doing astronomy. We were all thinking that with DEEP2, we could provide similar access but at redshift $z \sim 1$. If we were successful, scientific ideas using the DEEP2 data would be far greater than we had at first estimated. That has certainly been the case for SDSS and I think for DEEP2 as well.

On BE, my group size was quite substantial, and very fortunately Jeff Newman, who had completed his degree a few years earlier, acted as group manager. Thank you Jeff! I am especially indebted to his dedication to me and to DEEP2. Jeff managed the project and even continued after he left for a faculty position in 2006.

Allison Coil was the most senior of my students and her thesis was nearly finished. She had gone to Hawaii to work with Nick Kaiser on fixing up the photometric databases that we were going to use for the entire survey. In addition she was planning to study the correlation functions in the new data. Her results were basically confirming that the LCDM model was a perfect fit to the $z \sim 1$ universe as shown in our data. I was a little annoyed when I first saw these figures which confirmed what we basically knew. LCDM works amazingly well. Why did we bother?

Michael Cooper studied the DEEP2 database looking for variations of the color-density relationships at high redshift and finding a curve that is basically unchanged from lower redshift. Michael also took charge of data reductions and keeping our database current, a job first held by Charlie Conroy before he went to graduate school at Princeton. Michael was also responsible for a lot of the final software. Brian Gerke took charge of a possible evolutionary test in which one counts the number of small groups as a function of redshift. This test didn't work so well as the groups were too small and evolution was too large, but it was a good try. Renbin Yan examined the spectra of the DEEP2 galaxies and measured the fraction of Balmer lines in each object. The Balmer lines are an indicator of recent bursts of star formation, and therefore we could measure how old the individual galaxies were. Julie Comerford joined the group late, and she studied unusual spectra showing dual AGNs within a galaxy. Often the image of the galaxy was perfectly elliptical. This is a beautiful demonstration that the merging of galaxies and their nuclei is an ongoing process.

After almost two years of recovery, two years since BE, it was necessary to have my heart's mitral valve repaired; it had been damaged by the clot as it grew, and now

leaked about 30% of the blood pumped through. That meant open-heart surgery, which was done at Stanford. Except for the feeling that a truck had run over my chest, recovery was a lot faster than from the stroke. The doctor tells me that my heart is now strong enough that I can live to see my grandchildren.

In 1996, I realized that Huchra and collaborators had finished the redshift survey of the brightest 45,000 galaxies in the 2MRS list of K band galaxies over nearly the entire sky. This list of galaxies supersedes the 5000 IRAS galaxies selected at 60 μm for which my students and I had determined the redshifts 30 years ago. But the gravity field of 2MASS is virtually identical to that from IRAS. There had also been recent work on the Tully-Fisher galaxies which one can use to measure the local velocity field. The old, poorly normalized, velocity field has been replaced by one that is larger and has much better normalization. I worked again with Adi Nusser and resurrected my old Fortran code, finding once again $\beta=0.4$, but now with $\text{chisq}/\text{dof} = 1$. That is, the velocity field matches the linear gravity field, mode by mode; I was correct in thinking long ago that the velocity field was corrupted! It would have been nice to have this result 20 years ago, but better late than never.

This result is an important demonstration of linear theory and a beautiful confirmation of all our basic models. The material causing our 600 km/s motion relative to the CMB is almost all due to the inhomogeneities out to a distance of $\sim 80h^{-1}$ Mpc. Now that we finally derived a believable answer, I have finished my unsettled questions, and therefore retired as of July, 2014.



Figure 6: Most of my students over the years. Taken at Festschrift, 2007

Cooking has always been a major part of my life, an activity that I find rewarding and absorbing, to fill my time now that my scientific activities have diminished. When I was an undergraduate, I needed to cook several times a week but had no idea how; I worked my way through various cookbooks. When I was a graduate student living with Paula I remember working through Julia Child's first volume, *Mastering the Art of French Cooking*, and cooking virtually every recipe; one definitely learned the basic techniques this way. Once I broke up with Paula and eventually met Nancy, my facility as a cook was extremely attractive. She didn't know what to do with a Mouli or a Cuisinart (37 years ago), but she was impressed.

Cooking was something that I wanted to regain post BE, but I have only one hand and the kitchen at the time was not designed for a one-armed cook. For example, the refrigerator was too far from the cutting area and stove. Our kitchen was falling apart and we decided to remodel it from scratch. Fortunately, in 2011 DEFW received a Gruber prize, for work done 30 years ago, and the money was just in time for the kitchen remodel. Now we have a beautiful kitchen that is perfect for me. Everything is within one step and is much, much more convenient. In fact, I've told Nancy that I will cook supper most days (she washes dishes), as I don't really do very much astrophysics. I buy all the groceries, usually at Magnani's and Monterey Market, and the folk there know me very well.

It's very important that I exercise, yet there are few exercises that I can do. I can walk, slowly, and do. I was swimming for quite a while, with short flippers and a breathing tube, and holding my paralyzed right arm by the left arm. That was okay but pretty boring. Now I have taken up biking, using a recumbent trike that I cannot fall off of. The bike has bicycle clips to tie my feet to the bike, and has a bar going up to the right knee to hold the leg straight. The bike has been customized so that I can fully control it with the left hand. It has one lever that brakes both front wheels, and shifters for the front and back derailleurs, all on the left hand. There is an armrest for the right arm. The bike is pretty slick and I have ridden it all over the Bay Area. It is necessary as I ride to focus on the right leg as it pushes or pulls, because as soon as I think of something else the right leg stops working; I can't walk and chew gum at the same time!

However there was just one thing wrong with the trike, and that was that I could never keep up to Nancy when we were going uphill. I decided to get an electric motor in 2015 and it solved the problem. The motor, made by BionX, turns the rear wheel in proportion to the effort one puts into pedaling. If you don't pedal at all, the motor is inert, but the harder you pedal the more the engine works. The proportionality constant between pedaling effort and the engine's output is adjustable. A lower setting is suitable for biking with Nancy, which we try to do 2 or 3 times week, and a higher setting is just right when I ride with my friend Maxim Schroggin. Now I can pedal 3-4 times as far in a given time with the same effort. -



Figure 7: On my recumbent trike, in SF on way to GG bridge, 2000

so I arrive home equally sweaty. A typical ride with Nancy is to take Wildcat Canyon Road and the Three Bears, a ~35 mile round-trip, in the same amount of time that I used to simply ride Wildcat Canyon Road. Now I can ride with friends and keep up with them. Nancy and I have taken rides in the countryside and perhaps will go on a bike trip in coming years, just like a year before the stroke when we rode with a local company in the Canadian Rockies between Banff and Jasper Parks.

When I got out of the hospital, I at first did not drive and was waiting for my right leg to have more feel like before BE. I soon realized it was never going to happen and that I had better learn to drive with my left foot. I practiced driving in a parking lot, with Jeremy in the passenger seat supposedly acting as the backup, like I did when I first taught him to drive. However, Jer was so nervous that I don't think he would have done any good had I lost control. Because my right hand is useless, power steering is essential but of course every car has power steering. Also with only one foot I could not manage the manual shift on my car, so I had to get one with automatic transmission. Now I'm completely used to driving and think nothing of highway driving for three hours, the maximum interval I can go before I need a potty break (reduced bladder capacity since muscles on the right side don't work).

My right hand and shoulder had no connection to my cortex, and lacking a signal, the bicep and triceps were constantly pulling. The same fight occurred in my right

forearm; the muscles to open the hand were fighting the pull of the muscles that close it, and the former were losing. All this despite having the baclofen pump, which gives you some sense of how life would have been had I not chosen to get one.

Since I was never going to have use of the arm, I went to a surgeon in ~1998 to have him cut it off at the elbow. After discussing the danger of phantom pain, we decided to instead snip all the tendons at the wrist. That made the hand very floppy, so he stabilized it by putting a metal rod into the wrist. The hand now looks better and works as before, i.e. not at all. In addition, my right shoulder's muscles are flaccid and have a tendency to sag, stretching the tendons and causing pain. You may have noticed that I like to sit with my right arm resting on an armrest; this gives the whole arm a rest, especially when I push or lean a bit to the right.

The second winter after the stroke (i.e. 18 months after BE) I decided to get back on the snow. Alpine Meadows runs a wonderful ski school for disabled skiers, such as disabled soldiers, children with developmental disorders and very occasionally stroke victims like me. The first day I had two ski instructors with me. (One of them, Andy Robbins, became a close friend and now has a relationship with our dear friend Carol). They and many instructors after them, tried many different approaches and types of equipment to see what would work best for me.

A major goal has been for me to put weight on my right leg, for balance, to initiate turns and to take pressure off my left leg. Some things worked, others didn't; I continued skiing on my left foot, putting minimal weight on the right. It's possible to ski on both edges of a single ski, and it is a useful exercise for a skier to take off a ski and go down the hill, turning left and right on a single ski. But it is tiring on the leg to do so. I always thought I would regain strength in the right leg, but there is nothing that will make the right leg stronger and more responsive. You should think of it as a wooden post that happens to look like my leg.

Early on, both the ski school and Nancy tried to talk me into skiing with an outrigger (a small ski at the end of a pole), but I resisted. One day I was skiing rather poorly, and Jeremy showed me exactly how far out of position my body was. He put his arms around me and slowly pulled me to the correct position, balanced evenly between my left and right legs. My frog brain complained and complained, even though Jer was holding me and I wasn't going to fall. After 10 years, I finally tried to give the outrigger a chance (you can see how stubborn I am), and it's made a big difference in my skiing.

My frog brain usually does not bother me while I'm skiing because I know what I'm doing, and because the snow is soft and I can fall without hurting myself. But if the snow is icy, my frog brain screams and screams so seriously that I must quit for the day. I have taken many falls and I get up with the assistance of the person skiing with me – I don't dare ski alone. No more double black diamonds (super expert terrain) for me, as I am stuck on easy intermediate trails. But it is still magnificent to be out on the mountain, whether it is during a snowstorm or bright sunshine. I look forward to getting up to Tahoe once again.



Figure 8: Jeremy, Nancy, me, and Adam at top of SV, 2006

Chapter 10: Acknowledgements

My career has been wonderful to me and I am blessed by the remarkable advances that I have witnessed. The delight of scientists discovering how the universe works, step-by-step, I find to be amazingly exhilarating. (It is a hell of a lot more interesting than running my father's housewares distribution business, an offer he made to me when I was in high school!) It is truly astonishing to think back through the 50 years of my career, to realize how far we've come in scientific understanding and technical power. Being a university professor is very special, because I am surrounded by brilliant minds always discussing the latest topics.

When I think of the past, I remember that I was often very excited to get to work and to talk to my students. This was my best time as a faculty member, a chance to interact with these bright scholars; I felt that I learned more from them than they

ever learned from me, and I just did not see when they could have assimilated so much. So many superior minds!

Nancy and I have been married for 35 years, through good times and bad, bringing up two wonderful boys and enjoying every minute of it. We have traveled and skied together and have done so much - it has been a truly wonderful life. We (I say 'we' because my recovery would never have occurred without Nancy's support and endless love) have weathered this unexpected and not always easy journey for 12.5 years. Some things we loved doing together, like hiking, are no longer part of our repertoire, but she rarely complains. I love her so much, now and forever.

January, 2016