Letters written in memory of David Marr by his friends and colleagues:

Peter Rado

Tony Pay

G. S. Brindley

Benjamin Kaminer

Francis H. Crick

Whitman Richards

Tommy Poggio

Shimon Ullman

Ellen Hildreth

These letters were originally published in Lucia Vaina, Editor: *From the Retina to the Neocortex: Selected Papers of David Marr* (Boston: Birkhäuser, 1991) and are made available here with the kind permission of Lucia Vaina.

Peter Rado

I was a student at Trinity with David and saw a lot of him until I left Cambridge in 1967. David was a great source of strength to me—I have been ill myself (with kidney problems) since 1958, and when I first was given a place on a kidney machine, David offered me one of his kidneys if it would be any use—it is very ironic to think that I have survived him; neither he nor I would ever have expected that (until recently).

David's thoughtful approach to things was one of the things that I valued a lot. He would never give a snap decision, but always thought carefully about everything. He was always very generous with his time and efforts: he took me camping when I was told that I should go away for my last ever holiday before being tied down to a kidney machine for the rest of my life. He immediately suggested we go camping, borrowed a tent and whisked me off, for a very memorable "last fling" in Scotland.

Another holiday together was in Germany—he borrowed a Land Rover and a group of us went to a Youth Music Festival at Bayreuth. I vividly remember David prowling around with his clarinet, looking for a soprano ("any soprano!") to sing Schubert's "Shepherd on the Rocks" with him. He was a superb musician, who seemed to be able to play the clarinet quite effortlessly—and without practice.

November, 1980

Friend from college years.

Tony Pay

David was a person who brought excellence in one form or another to everything he did.

His excellence as a clarinet player was, to begin with, an excellence of attitude and response. He was always committed to what he did. He valued the immediate impulse as highly as considered action when he played, or when he discussed others' playing. Music sprang from the heart, and though he possessed a formidable intellect, he never tried to reduce this side of his life to anything theoretically manageable or systematizable. Indeed, his relationship with his instrument was a very physical one, and he obviously loved the act of performing. As sometimes happens, this physicality in one sense stood in the way of his further technical and expressive development, and it was a little time before he overcame the problem.

He once auditioned for a place in the University Orchestra, and was passed over (wrongly, we later thought!) in favor of another more obviously technically able instrumentalist. He told me afterward that he'd promised his supervisor that if he didn't play in the orchestra he would devote the time to a project involving a learning network. I never found out what happened to that particular project.

When we met again, much later, he had obviously developed further as a player and musician. He would sometimes say to his musician friends that when he was burnt out as a scientist he would take up the clarinet seriously. Few of us doubted that he would do rather well if such a moment came. The idea of performances being 'wrong' in some sense seemed not to be very important to him, and he was often excited by the possibilities revealed by other people's ideas. On the other hand, I remember him saying to me, "How could you do that to my slow movement?!" after a performance of the Brahms Clarinet Trio in which we had thrown away some of the more melting moments for the sake of showing the long phrases. But the remark was characteristically softened in his subsequent giggle. I also recall his glee at being included in the Orchestra to play Berlioz's 'Beatrice and Benedict'— "They think I'm good enough!"

Most of all, though, I remember his friendship, and his winning blend of seriousness and joy, and his laugh when there were no words to express what he felt. Though he said many memorable things, when there was nothing to say, David didn't say it.

March, 1990

Friend from college years.

Principal Clarinetist
Royal Symphony Orchestra
London, England

G. S. Brindley

David gained first-class Honours in Part III of the Mathematical Tripos in June, 1966. In the same month he came to me saying that he wanted to do theoretical research on the brain. I advised him that he must spend one year of full-time study on what was known empirically about the brain. In the academic year of 1966-1967 he attended courses on anatomy, physiology and biochemistry, all new subjects to him.

In the summer of 1967 he began theoretical research. I was nominally his supervisor, but I gave him no ideas beyond a few that I had published (IBRO Bulletin 3 , 80 (1964) and Proc. Roy. Soc. B. 168, 361 (1967) or was preparing for publication (Proc. Roy. Soc. B. 174, 173, [1969]). David read and wrote, and then brought me drafts of long chapters, in which, after hard work, I managed to find a few minor errors. We met to discuss these minor errors, and I gave David one or two pep talks whose purpose was to persuade him that unless his work led to experimentally testable predictions whose prior probability was neither almost zero nor almost unity, no experimenter would read his work.

With these exceptions, our only contact was musical. I was a mediocre bassoonist, David a much better clarinetist. Inevitably we read through the three Beethoven duos and the Poulenc sonata for clarinet and bassoon. We even rehearsed one of the Beethoven duos and played it in a college concert. Once or twice we joined three other wind players, and read through some quintets.

In August 1968, after just 13 months of research, David submitted a dissertation for a Title A Fellowship of Trinity College, and was elected. This was a high honor; only those familiar with Trinity will know just how high.

I spent the fall quarter of 1968 in Berkeley, California, and migrated permanently to London, after 16 years of Cambridge, in December, 1968. David moved, for that fall quarter, into the house that I had just bought for myself in London, and before my return from Berkeley he had rewritten as a paper for the Journal of Physiology the chapter on the cerebellum from his fellowship dissertation. It appeared in 1969 (Vol. 202, p. 437), and was the first substantial theoretical paper that the journal had ever published. David remained in London for 18 months after my return from Berkeley, at first full-time and then part-time, commuting from Cambridge. During these months, he wrote his paper on the classifying and memorizing functions of the cerebral cortex, Proc. Roy. Soc. B 176, 161-234, the outline of which was already in his fellowship dissertation.

One reminiscence from this London period: Local amateur players performed Mozart's concerto for flute and harp. My six-year-old daughter, who had a remarkable musical memory and an excellent whistling technique, had heard recordings and some rehearsals and wanted to hear the performance. My wife and I were unavailable, and David offered to take her. She sat on his lap and, he reported, whistled the flute part, faultlessly and very quietly, throughout almost the whole performance. He didn't stop her, judging that it spoiled neither his enjoyment nor that of people in neighboring seats.

March, 1990

David's doctoral thesis advisor.

Professor
Fellow of the Royal Society
Honorary Director
Medical Research Council
Neurological Prostheses Unit
Institute of Psychiatry
De Crespigny Park
London, England

Benjamin Kaminer

I never anticipated that the "workshop" I organized in May, 1972 would have been a turning point in the life of David Marr. And many might wonder why I, not being a neurobiologist, organized this workshop centered around David's ideas at the time, on the organization of the cerebral cortex. True, when I became Chairman of the Department of Physiology in 1970, I decided to develop a section of neurophysiology, but why gather a group of eminent molecular biologists, theoretical physicists, computer scientists and mathematicians together with classical neurobiologists? The time, I thought, appeared ripe for such interdisciplinary interaction and interchange in the search for an understanding of the human brain. As was well known, a number of molecular biologists, having cracked the genetic code, had challenged themselves with the task of unraveling the mysteries of the brain and mathematicians and computer scientists were designing networks and models creating "artificial intelligence." Such intellectual reasoning on the timeliness for a meeting of the minds, however, would not have been enough impetus for me to arrange it. Superimposed personal factors played a decisive role.

Sydney Brenner, Seymour Papert, and I were friends in South Africa. In the 1950s Sydney and I were in the Department of Physiology in the University of Witwatersrand, Johannesburg and Seymour, from the Department of Mathematics, became associated with our department, learning and experimenting on the nervous system as he developed his interests in psychology. In the latter part of that decade the three of us left South Africa. Sydney joined Francis Crick at the MRC, Cambridge, England. Seymour went to the Department of Mathematics, Cambridge, England for further study, then to France, and later joined Piaget in Switzerland. I went to work in Albert Szent-Gyorgyi's laboratory at the Marine Biological Laboratory, Woods Hole, in Massachusetts. Sydney and I remained in touch and we saw each other frequently in Woods Hole, but I had lost contact with Seymour Papert. When I took my current position in 1970, I heard that Seymour was at MIT, visited him at the Artificial Intelligence Laboratory and there met Marvin Minsky and learned of the inroads they were making in their field. I, of course, was aware of the progress Sydney was making on the nervous system of the nematode C. elegans and soon my idea emerged of getting these two "foreign" groups together with the "native" neurobiologists on common territory. When I discussed the proposition with Sydney he told me that David Marr had joined their group and suggested that a good take-off point for discussion would be David Marr's recent papers on the cerebral cortex.

After a few more visits to MIT, I soon realized that many students in artificial intelligence, smart mathematicians, had never seen a synapse or a human brain and students in biology or neurobiology had little idea of the language of artificial intelligence. Since I decided to invite selected students from Harvard, MIT and our school to sit in at the workshop, I arranged for a series of lectures and demonstrations for these students to prepare them in advance. Marvin Minsky lectured on concepts in artificial intelligence to the biology students and the students from the Artificial Intelligence Lab learned about the ultrastructure and organization of the brain from Alan Peters and Walle Nauta and about basic neurophysiology from Ken Muller. The workshop was held at Boston University School of Medicine for three days on May 24-26, 1972. The main participants were David Marr, Sydney Brenner, Francis Crick and Stephen Blomfield (MRC, Cambridge); Freeman J. Dyson (Institute of Advanced Studies, Princeton); Seymour Papert, Marvin Minsky (Artificial Intelligence Lab, MIT); Stephen Kuffler, David Hubel, Torsten Wiesel, John Dowl- ing and Ken Muller (Harvard); Horace B. Barlow (University of California, Berkeley); Anthony Gorman and Alan Peters (Boston University); and I acted as chairman of the meeting. Together with 20 students, a total of 50 attended.

My first meeting with David Marr several days before the workshop, I shall never forget. His gentle manner, lovely smile, and joyful laughter remain as vivid perceptions in my brain. He agreed with my plan not to have a formal program except for introductory talks, first by David and then by Seymour Papert. I opened the first session with the briefest survey of past developments in neurobiology beginning with Cajal, and emphasized the main purpose of the workshop, to exchange ideas freely without any publication of the proceedings. David in his opening remarks applied the "inverse square law" to theoretical research, suggesting that its value varies inversely with the square of the generality (!) stressing the need to establish structure-functional relationships either from the bottom up, from structure to function, or top down, from function to structure. He developed his ideas by posing the questions: What does the brain do? What are the logical equivalents? What are the actual mechanisms?

Seymour Papert conceptualized on "artificial brains," "perceptrons" and analyzed a "simple system" involved in catching a piece of chalk. And for the rest of the three days we had lively and provocative discussion interspersed with mini-talks. During tea breaks and lunchtime, I would approach various individuals encouraging them to give short presentations. The main participants continued with informal discussion at a dinner in my house and after the workshop social interaction and intense intellectual discussion continued during the weekend, which was spent by some of the participants in Woods Hole. Albert Szent-Gyorgi invited us to his beach cottage, where he also exchanged ideas, and we also went across to Martha's Vineyard by boat. Relaxation and fun did not hinder the intellectual vibrations in our brains.

During these five days David Marr spent much time with Minsky and Papert, and as I said, this event was a turning point that led to David subsequently joining the Artificial Intelligence Laboratory. That in itself was a very positive outcome of the workshop. Since the proceedings were not published, any other outcome is intangible. But let me include here two of the letters I received:

MRC Laboratory of Molecular Biology
9 June, 1972

Dear Bennie:

Thank you so much for such a marvelous few days in Boston. And your hospitality was quite wonderful. I was almost embarrassed by the trouble to which you went. The meeting itself was very useful for me. I learned a good deal from contact with the AI Laboratory and hopefully some good will come of it. I hope the neurophysiologists also gained, though I doubt if any will admit that they did. Many, many, thanks once again. It was really wonderful.

With very best wishes to you both,
David Marr

MRC Laboratory of Molecular Biology
31 May, 1972

Dear Bennie:

Many thanks for such a relaxed and informal meeting and for the very smooth organization behind it. I'm sorry I didn't come to Woods Hole but I felt really wretched and by the time I got to London my sore throat turned to a streaming cold. It seemed to me that the meeting really brought the various groups into intellectual speaking distance of each other—I hope the continuations at Woods Hole cemented the process. Of course I was really a spectator, but I think that for Sydney and David it was especially stimulating. Perhaps history will consider it seminal. If so, the credit will be yours. It was certainly much less of a strain than any meeting I've attended for years and that allows people to develop ideas at the back of their heads.

Best wishes,
Yours sincerely,
Francis
F.H.C. Crick

When David moved to MIT we kept in touch. He gave a wonderful seminar in our department and he visited me in Woods Hole in the summers. I became very fond of David and admired his intellectual prowess (coupled with modesty). He was well recognized and respected for his important scientific contributions, which continued during his brave struggle with a fatal condition. The ending of his life prematurely was a sad loss to all who knew, admired and loved him, and also to the science of neurobiology.

March, 1990

Ben provided one side of the bridge for David's crossing the Atlantic. Sydney Brenner provided the other side.

Professor and Chairman
Department of Physiology
Boston University School of Medicine
Boston, Massachusetts

Francis H. Crick

I first heard of David under unusual circumstances. One day in the early 1960s I was standing about in the basement of the University Arms Hotel with a group of scientists, waiting to be photographed for a London newspaper. I had recently been dipping into several papers on theoretical neuroscience and I remarked to Alan Hodgkin that much of it seemed rather pretentious stuff to me. "I agree with you," he said, "but what about David Marr?" "Who," I said, "is David Marr?"

Sydney Brenner and I got in touch with David and one afternoon he came to talk to us in our joint office at the Molecular Biology Lab on Hills Road. David had been working on his theory of the cerebellum. At that time I didn't know the difference between a parallel fiber and a climbing fiber, so he had to explain it all from the beginning. This took several hours. When he left, Sydney and I were exhausted but undeniably impressed. A little later, when David was wondering where to go, we provided a home for him in a small office down the corridor.

I didn't see much of him during this period, though I remember struggling to follow several difficult seminars based on his cortical papers. Then came the meeting at the other Cambridge that brought together several neurophysiologists (Horace Barlow, David Hubel, etc.), members of the MIT AI group, together with David, Sydney and myself. The boat trip after the meeting was David's Road to Damascus. (I wasn't there, as I had a wretched cold.) He became converted to AI and before long moved to MIT.

I moved to the Salk Institute, in La Jolla in 1976 and decided to switch to the neurosciences. David by then had become close friends with Tomaso Poggio, and I was lucky enough to persuade them to visit me for a month. (Happy days! Who could spare a whole month now for such a visit?) It was April, 1979. The weather was perfect for the entire time. David and Tommy worked together during the morning. We all had lunch together and the three of us talked each day until tea-time.

It was very educational for me. They kept telephoning somebody called Westheimer, whom I'd never heard of. Apparently he did something called psychophysics, but what was that? I didn't entirely agree with David's rather functionalist approach. You can find echoes of our conversations in the last chapter of his book Vision. Most of the time David is arguing against me, though I also detect traces of Marvin Minsky in his imaginary antagonist. It was a happy period in David's life. He thought he was cured of his leukemia. Only a few weeks after he left it recurred again.

David's work clearly falls into two phases. Marr I was concerned with neural circuitry and what it might compute. Marr II (the AI phase) was more functional. The emphasis was on the theory of the process and possible algorithms, with much less attention to realistic implementation. I believe that if he had lived he would have moved to a synthesis of these two approaches.

His early death was a great loss. He was trained as a mathematician and had outstanding intellectual powers. Most of his papers are not easy reading, as he was striving for precision and vigor, but behind it there was always a well thought out set of ideas. His book, much of which he wrote during his last illness, is written clearly and in a compelling style. It remains to be seen how much of his detailed work will survive, but his influence is everywhere. If he had lived, I have no doubt he would have come to dominate the field. His early death was a great loss, both for the subject and particularly for his close friends.

March, 1990

Collaborator and friend, from Cambridge (UK) to Cambridge (USA)

Kieckhefer Professor
The Salk Institute
San Diego, California

Whitman Richards

I can't remember exactly when I first met David Marr. Perhaps the first time was when he and Tommy Poggio attended an NRP meeting on Neuronal Mechanisms in Visual Perception in December, 1973, where they spoke about "Levels of Understanding." I know for certain that this first brush made little impact. My own work on stereopsis was going very well then, with the discovery that stereo resembled color vision in having three distinct "channels" or "pools," any one of which could be absent in an observer, just as in color blindness. About that same time, or perhaps a year or so later, a squash friend from Cambridge asked what I thought of Marr's work. Somewhat embarrassed, I was motivated to review the cerebellar and archicortex papers, but again gained little profit because they were remote from my psychophysical studies. It would be some ten years later that I would return again to David's paper on the archicortex, and, with Aaron Bobick, see its relevance to perception. In the meantime, others, of course, had already come to appreciate Marr's early understanding of the cerebellum.

In 1975 David had targeted stereopsis as an information processing module for study. He needed someone to critique his ideas from a psychophysical viewpoint. I recall vividly our first real encounter, where he challenged me to explain my "three pool" model of stereopsis with sufficient precision that it could be run on a computer. This challenge struck home, for it then became quite clear to me that simple circuit or block diagrams were woefully inadequate. Left unspecified were a host of "details" such as the features to be matched, the hemisphere of eye that serves as the base representation, epipolar problems, false target elimination, etc. From that moment on, David had hooked me on the value of a computational approach to perception.

During those early years, my role in Marr's group was largely as a psychophysical bangboard. The most profitable exchanges were at the Newbury steak house just across the river in Boston, where we would walk for lunch. (Later, the original Legal's restaurant at Inman square became a favorite.) We usually loosened up with a gin and tonic, followed by a $2.00 steak and fries special, with coffee as an antidote for the appetizer. On a hot day we'd pick up a cone at Steve's across the street before heading back over the Charles River basin to MIT. Most of our discussions revolved around stereopsis—the hottest problem, the size and number of channels, early primal sketch issues, and later, object recognition and why "segmentation" was misguided. At these lunches, we rarely spoke about anything but vision. The one marked exception was flying, which had become his principal hobby.

The uniqueness of these times is very difficult to express in writing. There were a dozen or so in Marr's group. The energy level, excitement, and dedication was exceptional. When David was not in Tubingen with Tommy Poggio, we would meet weekly to discuss each individual's progress or special topics such as the Retinex, short and long range motion, grouping, axis finding, and the building of a vision machine. Once a month or so a visitor came through and offered their latest ideas. Typical of MIT, the visitor was battered with questions from the small audience, each anxious to find the soft spot. Yet, after all of us had had our chances, David would sum up the work in a few sentences and then proceed to point out any serious weakness that all of us had missed. He had a remarkably clear view and an exceptional ability to cut to the quick in a Mozart-like manner.

In late 1976, we approached NSF with a proposal called "Vision Algorithms and Psychophysics" which was to be the principal vehicle for funding (with AFOSR) of the more biological aspects of the group's work. (The machine-oriented aspect was provided by the AI Lab through the efforts of Patrick Winston and Mike Brady.) This was a significant award not only because of its interdisciplinary nature, but because it proposed a specific strategy for attacking the visual system that included both computational and experimental components. While writing the introduction, we realized that the "three levels" of understanding could also be recast as a scientific protocol for attacking a vision problem: Step 1: Propose a computational theory; Step 2: Write an algorithm embodying the theory; Step 3: Check out its (biological) merit with psychophysics (or neurophysiology). This simple paradigm was used first on the stereoscopic problem, leading to a rejection of the cooperative model by psychophysics in favor of the later noncooperative models culminating with the Grimson-Marr-Poggio version. The impact of psychophysics on this development was critical, and a major benefit of the interdisciplinary approach. (As an aside, a visit by the two Johns—John Frisby and John Mayhew—who presented extensive psychophysical findings in support of matching features other than zero crossings, also had caused considerable impact, especially on the development of later models.) Unfortunately, over the course of the years, as the problems became harder, the enforcement of the 1-2-3 method became lax, and often we did not get closure when studying a problem. However, I believe the best theses of the group were those that completed at least one iteration of these three steps.

During the next seven years, the activity level of Marr's vision group was prodigious. In this period not only was stereo seen from a different, more computational viewpoint, but also motion, color, edge detection, some aspects of object recognition, and the age-old grouping problem. Our confidence was enormous (and unfortunately, overbearing to many!). In October, 1977, we decided to invade the annual Optical Society meeting in Toronto to "show the others just how vision should be studied." I arranged a symposium on "Vision by Man and Machine" that ended with Marr as the key speaker. (At this time, David was still relatively unknown.) Unfortunately, the presenta- tions were late starting and ran too long, leaving David only half his allotted time. So with the approval of Lorrin Riggs, the presider, we arranged for David to complete his talk in the technical discussion session that evening. This was a disaster! Already most were irritated by our arrogance, and the last straw was for the technical discussion period now to be replaced by still more lectures from MIT. Leo Hurwich spoke strongly against this policy change, and left in protest. Nevertheless, a majority stayed, realizing that no one leaves a technical discussion session unscathed. Quite true! Richard Blackwell, always ready with penetrating questions, led the attack, targeting Michael Riley's work on texture boundaries. (Michael was the youngest member of the group, just in the process of completing his bachelor's thesis on texture.) Finally, the discussion moved away from the MIT papers, and eventually the discussion/critique session ended. Afterward, I recall that we all, still undaunted, headed off to a restaurant opposite the Needle, where we had a grand evening, still convinced of the merit of our new computational wave. Now, the Optical Society hosts one of the most positive and supportive groups espousing computational approaches to biological vision.

The reaction of the Optical Society to our approach was not unusual. However, by 1980 there was a significant shift in the balance of work on vision in favor of computational approaches. David's activities were stirring interest, and groups at other universities and laboratories were developing interdisciplinary groups similar to ours, with competing ideas. Unfortunately, on occasion this led to some strain, but overall the competition was a very positive factor, for it challenged us to remain productive and thorough. Within MIT the recognition of David's work of course came earlier, and in 1977 there was sufficient interest that a faculty appointment was proposed in the Department of Brain and Cognitive Sciences (then inappropriately named "Psychology"). This was to be a joint appointment with the AI Lab, where he was currently a Research Scientist. As with all new faculty positions, the candidate must give a "job talk" to the faculty. David elected to speak on one aspect of his Primal Sketch paper, namely to show what would be required for a so-called feature detector like a bar mask ("simple cell") to assert the presence of a line. The talk was another disaster. First, most of the audience did not see why, in the first place, a simple bar mask couldn't report the presence of a single line (a collection of such masks is required). And second, the mathematical details and computational recipes were boring to most experimental psychologists, and especially so to the philosophers! However, in spite of the negative reception to his talk, there were enough respected supporters for David to get the appointment. The result was formal recognition of the potential of building a bridge between AI and Psychology—a bridge that David worked hard to maintain while at MIT.

The future looked bright and conquerable in 1977 until December. Then we learned that David would probably have only another three years. It may be hard to understand that these years, although painful, were also rewarding, exciting, and at times a lot of fun. Most of the dark days were spared us, for David now spent much time in England for treatment and recovery with his parents. Ideas still flowed at a fast pace; we had tremendous momentum. In one three-year span alone there were 120 publications. During this period David brought much of this together in his book Vision, ably assisted in the transcription by our secretary Carol Bonomo, who was the world's fastest talker and a perpetual optimist. We were all excited by the prospect of David's book, recognizing it would be a milestone and hence eager to learn of its progress. We looked forward to David's return and to the days he would lead our research meetings, which continued even in his absence. Any little victory was celebrated, reserving lobsters and champagne for special occasions. Now, with Lucia, the simple things of life were enjoyed, which previously had often been passed by.

Yet the vision never wavered. David's principal aim was to unravel the mysteries of the human mind, choosing as his route the understanding of the information processing carried out by the human nervous system. To this end, David accomplished more in a few years than most of us can in a lifetime, and set in motion a wave for the future. He believed that these first steps toward understanding how our brains work would eventually "change man's image of himself, and that most current philosophies of life and thought would have to undergo a profound transformation to deal with this new knowledge." His work represents the beginnings of this great new adventure.

March, 1990

Colleague, friend and "guardian angel" who protected David from bureaucratic nuisances.

Professor
Department of Brain and Cognitive Sciences
Massachusetts Institute of Technology
Cambridge, Massachusetts

Tommy Poggio

I met David for the first time in the fall of 1973 when I came to Boston to chat with Marvin (Minsky) at the Artificial Intelligence Lab. Boston was wet, foreign and dark. David came out of his office in the "playroom." We exchanged a few words. His name was known to me, of course, because of his cerebellum paper, which was highly praised by many VIPs in the biological sciences.

Three weeks later I was again in Boston for an NRP meeting. David was also at this meeting. We had both been invited at the last moment and were not scheduled to speak. David sat quietly the whole time, listening to what people were saying about psychophysics and physiology. John Dowling joked with him about David's red Mustang. Back at the AI lab for the first time, a scientific conversation took place about the ideas on the retina he was then developing. I was the messenger of an invitation for him to give a series of lectures at the Spring course on Biophysics in Erice, Sicily. I was happy that he accepted immediately, and so our next meeting was already arranged.

Erice is a beautiful old village on top of a mountain overlooking the Mediterranean sea. For two weeks the participants–among them Mike Fuortes, David Hubel, Bela Julesz, John Szentagothai, Sir John Eccles, Michael Arbib—gathered together mornings and afternoons. At lunch and dinner we divided into small groups to explore the five restaurants of Erice, all above scientific average. There were also several expeditions to the various beaches down the hill. I was impressed and obviously pleased by the interest and the respect David had for my lectures and my comments. David's approach was by far the most unconventional and for me the most interesting. We discussed at length, dining, lying on the beach.

One year later, I came to MIT to the AI lab to work with David, to clear my head and decide what to do next. For the first week or so I was left relatively alone, free to play with Macsyma and Lisp. I spoke about the visual system of the fly at David's vision seminar. It went well. After the lecture we had a beer together in Harvard Square. David was very happy about my lecture. His enthusiasm, his praise, were contagious. I felt great and alive!

Our lunches together—at the Tech cafeteria—were still trying to define the nature of our approaches to the problem of the brain. Our views were already very near and converged rapidly.

In the meantime, I was understanding more and more David's work on vision. In retrospect it took a lot of time. Really new ways of thinking cannot be understood at once. A thousand different facets must be communicated with the magic of a language and the fascination of a style. David's papers on early vision all have these rare properties.

A good way to understand something new is to try to criticize it, playing the role of the advocat du diable. In doing this with David, I found myself at some point defending recurrent networks. David formulated the challenge of solving stereopsis in his way. He told me in terms of his analysis of the computational problem of stereopsis which "cells" had to be excited and which ones had to be uninhibited. On the back of a paper napkin at the Tech cafeteria I wrote down the obvious equations for the recurrent network. I claimed that it would be very easy to prove its convergence. Liapunov-like functions constructed from conditional expectations was what I had in mind. On the same evening, back at the lab, after a dinner at the Greek restaurant in Central Square, David programmed the recurrent algorithm which seemed to work well in 1-D. The day after, the 2-D version of the algorithm gave encouraging signs of liking Julesz' stereograms. One week later, when I had finally understood David's computational analysis, I also realized that an analysis of the convergence of the algorithm was going to be very difficult. All general stand- ard methods failed. When I told David that I had to turn to the last resort—a probabilistic approach—he teased me. The teasing became even more intense when I had to write a program to compute the result of the probabilistic analysis.

In the meantime, creative life was exciting, despite the headaches from the convergence problem. We started working closely together. It was a fantastic experience. David was very sharp; he had clear ideas about almost everything and they were usually right. At that time I was also slowly discovering various facets of him. He was passionate about music—Italian opera—for instance. I heard him improvising a few times on the piano. He played with ease and emotion. I was impressed. I had to wait another year, however, before experiencing him playing his instrument, the clarinet.

During those three months in Boston I often went sailing on the Charles. But the really new experience was flying. David used to fly and my presence triggered anew this passion. I took a few flying lessons. Several times we flew together with a rented Cessna. On those occasions I used to stay overnight at David's house. Early in the morning we heard the weather forecasts and then drove to Hanscom where we would get a plane from Patriot Aviation for the day and share expenses. One of the most beautiful flights brought us to the Lakes Region. The sky was clear and deep blue as the water beneath us. We landed on a grass strip on a little island. It was very quiet. We walked to the water a few hundred yards away. There we sat for a few hours. We reviewed what we had done on stereopsis and decided to write a short paper for Science about it. David already had the opening sentence and the overall formulation was clear. We just had to sit down the next day and write. There was only one white sail on the lake. Blue air. Green, and blue, and silence. David was happy and relaxed. So many more ideas and flights and forests and lakes were waiting for us!

On our way back to Hanscom the weather changed quickly. The rain started. On the final approach, David was very tense, his mind totally concentrated on the plane, the control tower and the instruments. Several people were afraid of this concentration, which they misled as a sign of unfriendliness. I knew well the alertness of David's mind when he was discussing science, lecturing, playing music or flying I could physically feel the presence of his thoughts, his total concentration. There was often an incredible "intensity" in his thinking. His reactions and his answers were then incredibly quick and at the same time crystal clear, sure and sharp. Our landing on the wet field at Hanscom was perfect. Five minutes later the airport closed down.

On another flight expedition to Nantucket and Martha's Vineyard, we got sunburned on the beach. Two days later, red like two lobsters, we gave a lecture on stereo at Harvard Medical School. We were making grand plans of flying across America for one month or so. Life was going to be a lot of fun!

A few weeks before my return to Germany, we were right in the middle of bicentennial time. The tall ships were coming into Newport on their way to New York. On the weekend (July 2nd) the weather was beautiful and David decided to fly down to Newport. We came above the bay with our Cessna 170 to find out that the blue sky was filled with flying objects. Balloons, choppers, a Goodyear "blimp" and many other planes. Tower instructions were to circle at x feet above the tall ships. The surface of the sea was covered by little white traces, glittering under the sun. There were hundreds of boats of all sizes that came to meet the tall ships. The scenery was superb. It was simply great, circling above the ships, together with so many other planes and boats. Down in Newport airport hundreds of planes were scattered all over the field, many of them old timers, happy and colorful. In the afternoon the weather deteriorated quite suddenly. There was a storm and ghastly winds. Back to the airport, we thought for a while to leave the plane and go back to Boston in some other way. David phoned several times to inquire about the weather at Hanscom. It was clear and so he decided to start. Airborne again, drops of rain slashed across the windscreen until we came from the low clouds out in the sun. It was the eternally beautiful weather to which poets have accustomed us. But the feeling in a small plane without instruments is quite different. David, however, was relaxed. There was nothing to do but fly straight and wait for the clouds to dissolve. Near Boston, the ground started to appear at short intervals through foggy holes in the white carpet above which we were flying. When we landed at Hanscom the sun was setting down against a clear sky.

It is not by chance that my deep friendship with David was associated with flying together. Flying and friendship, joy and beauty, freedom and living are things that are made of the same substance. I did not fly anymore with a light plane after David got ill. I don't know whether I am going to do it ever again.

David came to Tubingen in the beginning of 1977. He stayed in the guest room of the Institute and walked over to our home every morning for breakfast. We worked on the probabilistic analysis of stereopsis, every day discovering some more difficulties. David wanted us to think about a theory of human stereopsis. Eye movements were important. At that time I had just heard from Jack Cowan of his and Hugh Wilson's work on spatial frequency channels. David brought Mayhew and Frisby's Nature paper on rivalrous stereograms. These two ingredients, together with the refusal of our first algorithm and the need of eye movements, formed our starting point. We read everything on stereo from Barlow's seminal paper to all of Julesz'. At some point we were suffocating in my office under piles of bound volumes of the Journal of Physiology and Vision Research. We even did some informal experiments. At the end of the three weeks we had written three-fourths of the analysis of the cooperative algorithm paper (I had to write the final quarter with Gunther Palm) and had some rough ideas about a new model of stereopsis.

David brought his clarinet. I introduced him to Eric Buchner, a good cello player. With another friend, a very good pianist, they played together several times. All of us were deeply impressed by David's music. During David's visit in Tubingen, the members of the Scientific Curatorium of the Institute came one day to meet with the members of our Institute. In the evening after dinner, David and other friends played one of Beethoven's Quartets. I never was so deeply struck by music as I was that evening by David's clarinet. It was so beautiful and perfect, so full of emotion as to be almost unbearable. The audience—it was quite clear afterwards—had a similar experience.

He was quite alone in his work at that time. He did not have anybody back at the lab with whom to work in the same way we did. I suggested to him to try to work with Shimon and share with him responsibilities of the group and of the students. At that time I knew Shimon only superficially but my feelings and what David thought about him and his work left no doubts. David promised he would do it. It was an easy promise to fulfill. He also promised several times that he would finally get out of his "craziness" and his "women problem." But he never managed until he met Lucia, one year later.

Those three weeks in Tubingen were a lot of fun; life was full, warm and happy.

In June, 1977, David again came to Tubingen. He stayed a full month in "his" room in the Institute. We worked hard, developing our stereo ideas and writing them down at the same time. The days were productive. The theory took form. Through all my work with David it was often impossible to say who had a specific idea; almost everything came out from discussions and thinking together and reciprocal criticisms. David had the power of vetoing: if I was unable to convince him, that was it. He also had the ability of keeping us right on course.

I remember the origin of part of the zero-crossing idea. Coming out of the cafeteria I expressed my uneasiness about taking zero-crossing and peaks of the filtered images, since filtering the images was roughly equivalent to making their second derivative zero-crossing correspond to extrema of the first derivative. This made sense. But peaks were something strange, at least at this level. For simplicity, and because of the relations between derivatives, difference of gaussians and bandpass channels, I wanted to flush peaks and retain zero-crossing only. David thought a while and then decided that—for reasons I did not think of—the idea was not too bad. It is still unclear whether he was right.

We finished our manuscript right on schedule with some time left to take Polaroid pictures of the two authors sitting with the title in one hand and stereoglasses in the other. (In the original draft of the manuscript there were a few lines warning the secretary that at that particular point we had just had too much Courvoisier and therefore the following sentences were going to be particularly immortal.)

The whole month was continuous, concentrated, happy playing. As so often with David, science was fun and freedom! I often ask myself why David's presence had this fascination, this incredible power. I still find it very difficult to give a full answer. But I know that part of it was the clarity and especially the force of his mind, of his thoughts. To think with David was for me an inebriating experience, a special feeling of playing and creating. Skiing beautifully downhill on a sunny day in the Alps gives me some hint of this intellectual fun.

Werner (Reichardt) organized a Neurobiology meeting for the 500 years of Tubingen University. I had helped in setting the framework of the lectures. Many friends came: David, Vincent Torre, David Hubel, Dennis Baylor, Emilio Bizzi, Gunther Stent, Jack and Max Cowan, Bela Julesz and others.

David's lecture was beautiful, crystal clear, a jewel of intellectual brilliance and improvisation. We had the feeling that the world was there for us to play.

In the middle of October I flew to Toronto for the annual meeting of the Optical Society of America. I was invited by Whitman Richards who organized a special session. The whole MIT Vision group came. It was fun, although short and chaotic. A couple of days later I flew to Boston to work with David for three weeks. It was a fight with LISP and probability (again!). At the end of my stay, we drove together in a rented car through a colorful New Jersey down to Bell Labs. I gave a lecture for Bela Julesz and his small group on a topic that was completely uninteresting to them, synapses. When we mentioned our probabilistic analysis of zero-crossings, Bela named some mathe- maticians at Bell Labs who had worked on somewhat similar topics. Among them there was a name that we did not know, Ben Logan. We asked for the paper and Bela sent his secretary to get reprints. Glancing through it I saw that his theorem was very suggestive of our notion of independent bandpass channels. In the hotel and later, in the car, I tried to convince David, who remained quite skeptical. The zero-crossing idea and its connection with

Logan's theorem is of the kind I immediately like. Unfortunately, such ideas are often too nice to be correct and David was certainly right in his skepticism.

November, 1980

David's closest collaborator and great friend.

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Shimon Ullman

When I came to MIT as a graduate student in the summer of 1973, David Marr was already there, having arrived from England a couple of months before to work at the AI lab. This was extremely fortunate for me. I came to the AI lab with the intention of studying brain functions, and in particular visual perception, using mathematical models and computer simulations. From the limited literature I had seen about MIT's AI lab I had the impression that this was the main focus of the scientific activity there. As it turned out, however, the emphasis at the time was primarily on machine intelligence, and nobody at the lab was actively involved in the study of biological brain functions. When I started to talk with David soon after my arrival at the lab, it immediately became clear to me that he was the person I wanted to do my Ph.D. work with. We had similar interests, and a similar background that started in an interest in pure mathematics, then shifting to biology, with an interest in the brain and its functions, and then to artificial intelligence in an attempt to model some aspects of the human visual system. David could not be my formal thesis advisor at the time, since he was not yet a faculty member. Soon, however, he became my unofficial advisor, with Marvin Minsky's tacit blessing. Although he was my advisor, he was only slightly older than me, and after a short while we also became personal friends.

Working with David was always challenging, exciting and rewarding. It was hard work, but it was a lot of fun. We had the feeling that our small group, centered around David (that included Whitman Richards, Tommy Poggio and a number of David's students), was creating something new and exciting. Around the time I had finished my Ph.D. work, David and I worked together extensively for a period of a few months on some problems in motion perception, and later wrote a paper on this work. It was for me the first, and in fact still the only time, that I wrote a paper with someone in this mode, actually sitting together for long hours at a time, composing sentence after sentence, and discussing each paragraph as we wrote it. The experience was very intense and enjoyable. I think we both enjoyed it, and we were both exhausted by the time the paper was finished.

David was extremely quick, and expected others to be equally quick and alert. We once went down to Washington, D.C., to meet one of the sponsors of our work at the lab. We discussed with him some of our vision work, and then he asked if we had any views regarding new directions his agency should perhaps be looking into. David snapped, "grow wires," without offering any additional explanation. I knew what he meant: he became interested at the time in possible hardware implementation of vision devices, and thought that the restrictions imposed by standard semiconductor technology on the number of interconnections among functional elements (much smaller than the number of connections among neurons in the brain) was a severe limitation on the way to producing compact and practical vision devices. I could see the puzzlement in the other person's expression, but David saw no need to elaborate the issue further.

David's illness came as a shock. He called me from the MIT infirmary on the day he was diagnosed with the disease, and asked me to close my office door. He then said briefly and without any introduction that he had acute leukemia. The period that followed was very painful. Twice during his illness we thought that there was some hope. The first was during his first remission. Everyone hoped that perhaps, by some miracle, the disease would not come back. We took a vacation together in Vermont, and he resumed his work with his usual intensity. After a period, he felt weaker and went to the hospital for some tests. He came to my office to call the hospital about the tests' results, and found out that it was indeed a relapse. We sat in my office for a long time, devastated by the news.

The second hope came when a physician in Cambridge, England, had some initial success with a vaccine against leukemia. David was hospitalized in Cambridge. He was very weak, and worked on this book. When I came to visit, I met the physician, who was very supportive and promised to help as much as he could. When David came back to the U.S., Tommy Poggio managed to bring some of the Cambridge vaccine with him, but it was too late to actually use it (and it did not prove effective in later clinical trials anyway).

The final period, when David already suspected that the battle was lost, was in fact a quietly happy one. He was happily married to Lucia, and was working intensively on his book and a number of other projects. In his premature death, the scientific world lost an intellectual giant, who, in a short time, made a huge impact on his field. We also lost a warm, brilliant, exciting, unusual friend.

March, 1990

Colleague, studentt, close friend

Professor
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Ellen Hildreth

When I first came to work with David, I was overwhelmed by him; by both his brilliance as a scientist, and his personal magnetism. It was perhaps a year before I could really feel comfortable with David. We have an expression for being overwhelmed by something, which is being "blown away." I sometimes had visions of myself going to talk with David, and being whooshed out of his office, chair and all, by a big gust of wind, and hanging on for dear life to the edge of his door, in order to hear every word he had to say. You always remembered the things David would tell you; it was often the case that you wouldn't understand him at first, but something about the way he said things would make the words stay in your mind, and hours, days, even weeks later, bright lights would snap on in your head, as you figured out what he meant by his cryptic message.

David was always very generous with his ideas; he'd solve half the problem for you, and then later insist that you did it all yourself. Ideas would come to him any time; sometimes the phone would ring at 9:00 on a Sunday morning (only my mother, or David would call at 9:00 Sunday morning)—David would greet us with a glorious "Hello!" and "I was just wondering if by chance you might have planned to come into work today; I have a new idea you might like to try out." Whether I had planned to come in or not, without a moment's thought, I'd answer "Of course, David! I was just on my way out the door!"

It was the enthusiasm that David instilled in us that made us want to do these things; everything we did was so important to him, so vital. This always made us feel somewhat under pressure; it wasn't a pressure that David placed on us directly—his enthusiasm just made us want to be always working madly at our research. And he was always so busy himself, you felt bad if you weren't working at least as hard.

David always thought big, and tried to teach his students to do this as well; it wasn't enough to study an aspect of stereo, a subproblem of motion, or a particular type of texture problem. You gritted your teeth, and went in to tackle the whole problem of stereo, motion or texture head on. He'd always prefer to present a "whole theory" of something (which might be a bit lacking in detail), than to present an explanation for any part of a problem.

He'd have little "favors" for you to do make a glossy of something, a demo, run an experiment and was so overly courteous and charming in the way he asked. With a twinkle in his eye, he'd ask us to do it whenever you had the time—there was no rush, take a week, a month, whatever. The moment he was out the door, you'd drop everything, work on his project non-stop, and have the results on his desk the next day. (Do you suppose he knew you'd do this?)

Getting back a paper with David's comments was really something that took getting used to—we were never quite sure how to interpret the "GUFFAWs" and "tee hees" and "oh really?'s", but the bright, red, bold "rubbish" and "No!'s" were a little more obvious—David could really be devastating at times. We were all very much "in tune" with David's moods. If he was in a jovial mood, so were we, but if he was unhappy about something (particularly if it was something we did), our emotions could be destroyed for days.

David meant so much to us, and his teachings were so important, but I must admit that I was quite taken aback one day when a visiting scientist asked me if David was like a "guru" around the lab, because that he was not. He was human, like the rest of us, and could be wrong sometimes too (he just made mistakes with so much more style than the rest of us). What he believed in, he believed very strongly, but if you presented a convincing argument for the other side, he'd change his mind. He trained his students to stand on their own two feet, and be their own people—sometimes playing the devil's advocate, just to get us to argue with him.

David held a strong presence in the lab; you always knew when he was in: the word would get around. Someone would spot him up at the XGP (printer), or wandering down to the Xerox machine, or logged in, or would notice that his office door was shut (a sure sign that he was both in, and not wanting to be disturbed), and would spread the word around that David was in. He'd make a point of popping in from day to day to see what you were up to, so you had to be sure you looked busy. We'd feel horrible if he caught us chatting in the playroom or bullshitting about politics in the office. He wasn't a slave driver by any means (although we used to kid him about the 30-foot bullwhip he kept hidden in the office); on the contrary, he was one of the most gentle and gracious people I have ever met (second only to his mother). He was just a very stimulating person; the energy level in the lab would suddenly double when David walked in (it would quadruple if Tommy [Poggio] was around too we used to refer to the two of them as the "Dynamic Duo").

I had worked with the Logo group for three years before I came to work with David. I came to him knowing almost nothing about human vision, and very little in my background to offer, except some applied math. But that didn't matter. He felt it was important for a person to have some background in an analytic discipline, but beyond that, all that was necessary was an eagerness to learn; everything else would come. It took a tremendous amount of time and patience for David to work with someone with so little background in his field of research, and I just can't say in words how much I admire David for having that time and patience; it's helped me to establish my life's work, and to develop me in many personal ways, as well.

The quiet courage with which he faced the last three years was very hard on us. David would try so hard to not let his illness interfere with his work and interacting with his students. He always kept things to himself; the time that we had with him was so valuable that every moment was spent talking about vision, and how we were doing in our work. His students were always so important to him. When he had so many more important things in his life to be concerned about, he'd be worrying about the Vision group. He always worried about me much more than I worried about myself. But in the times that I've had with David, he's given me far more than I need to keep going for a lifetime.

November, 1980

Student, collaborator, friend.

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