In Memory of

Kenneth Morse Crowe

(1926 - 2012)

by his friends and colleagues

March 5, 2012

Ken Crowe was many things to many people. For most of us, he was just what we needed at the time. Ken's magic might have been the way he supported and nourished so many of us to find our own way, even if he did not always agree with our direction. He defined Medium Energy Physics for the entire community and seeded several rich fields of science.

This compendium of personal stories was taken (on 06 March 2012) from the *wiki* at

`http://musr.physics.ubc.ca/dowiki/index.php/Ken_Crowe`

which is still available as a repository for individuals' memories of Ken — a place where those who knew him can tell their stories and where others can mine those stories for a deeper understanding of the man.

For the time being, the *wiki* will be open only to registered contributors; when it is deemed "converged" it will be frozen and opened up for public viewing. Until then, anyone who knew Ken can apply for an account and, if approved, view the stories there and/or add new ones.

1 Jess Brewer

In 1969, after completing Bevatron Experiment 95 on $\mathcal{CP}$ violation in $K^0_{\mu 3}$ decay, Don Miller (my first supervisor) left Berkeley (and what was then still called LRL) to become head of the Physics Department at Northwestern, leaving me to hunt for a new supervisor and a new PhD topic. It was the best choice anyone ever forced me to make. [I often advise graduate students to change advisors as soon as they learn the ropes, even if they get along famously with their first choice, because the first choice is based on limited information.] In my case, I had to choose between working for Owen Chamberlain (a famously nice guy and a Nobel laureate) on hypernuclei or for Ken Crowe (a notorious slavedriver) on muon depolarization in liquids, using methods now known as μSR.

I chose the latter because the whole idea of using an exotic particle physics phenomenon like $\mathcal{P}$ violation in μ^+ decay to probe "ordinary" materials seemed *really cool*, like being a character in my own science fiction novel! I went down to campus and asked Alan Portis if he thought muons could make a contribution to solid state research analogous to that of NMR; he said he seriously doubted it. That sounded to me like a challenge! [A few years later Alan came up and gave us all lectures on condensed matter physics so we could at least pretend to know what we were doing.] When I learned that the muonium (Mu) atom could be used as a double for H atoms to study otherwise inaccessible chemical reactions, that clinched it — I was in!

Then I set about getting to know Ken. I found this easy, actually, because all one really needed to realize was that Ken was a force of nature, obeying consistent rules of behavior like any other such force. When he set out on a mission, he gathered everything and everyone at his disposal to accomplish that mission as well as humanly possible (or better). Whether it was winching in the jib to reach that buoy before the next boat or working out the time evolution operators for the muonium spin system, perfection was the only acceptable — nay, the only *permissable* performance.

For some reason, many people found this hard to digest. I speculate that this was because they interpreted Ken's intolerance of imperfection as personal intolerance. Nothing could be further from the truth. Those who earned Ken's respect, on the water or in the lab, learned that his loyalty and generosity had no bounds, once he was sure that it was not wasted. Between our friendship and my observations of his devotion to his family, Ken taught me a lesson I have never forgotten:

> *Love is meaningless if it has to be earned; respect is meaningless if it doesn't.*

That being said, he could be daunting. The first two times I came into his office to give an update on my progress modeling muonium depolarization in liquids, I got thrown out in a hail of insults, all (in retrospect) richly deserved. The third time I was *sure* I knew what I was talking about, and when he started to give me a hard time I told him to shut up and listen! From that moment forward he treated me like an equal, and a respectful partnership began that lasted for decades.

The rest, as they say, is history. A fine scientific history it is, but this is a space for personal stories, so I will save the details for another page, saying only that I once was asked by the Nobel committee if there were anyone I'd like to nominate; I suggested Ken, for his key role in creating the field of μSR where I have toiled for the last 4 decades and had a wonderful time. Of course, just to keep it interesting I also nominated several others with whom Ken might have balked at sharing the stage in Stockholm. I think he would have gotten a chuckle out of that. :-)

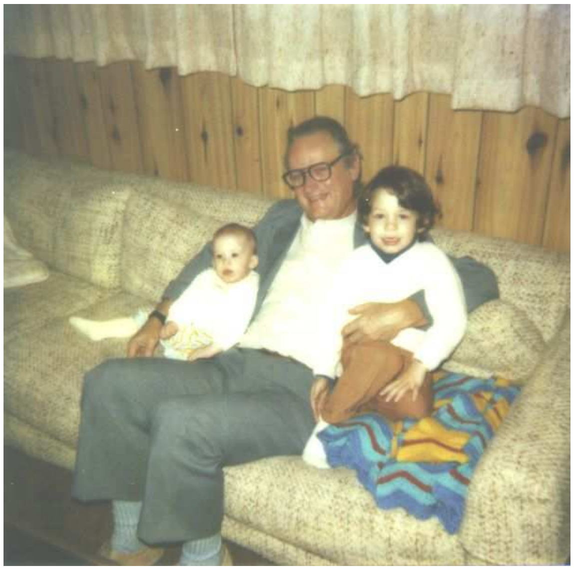

Figure 1: Ken with Rebecca and Jed Brewer, *ca.* 1984.

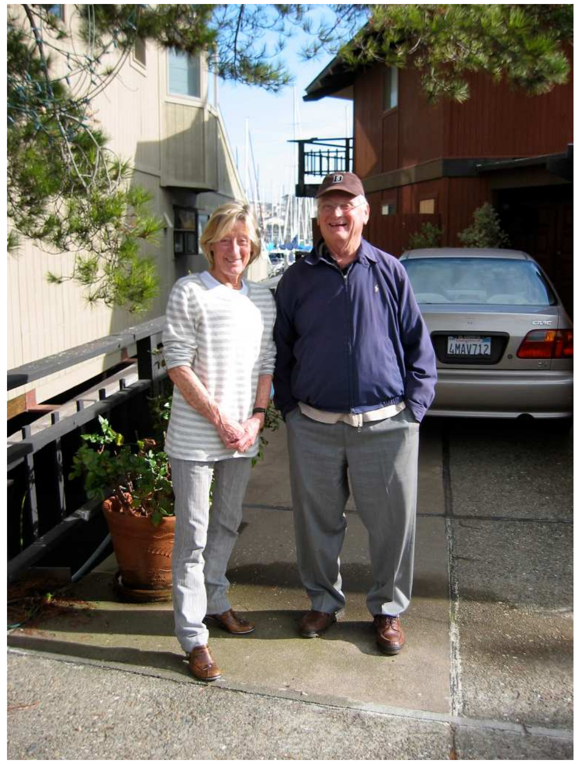

Figure 2: Ken and Penny beside their house in Point Richmond, January 2009.

2 Bob Budnitz

I came to the Rad Lab (later the Lawrence Berkeley National Laboratory) in the fall of 1967, a brand-new, wet-behind-the-ears postdoc. I had been hired by Don Miller, a high-energy physicist and physics professor at UC-Berkeley who had planned a Bevatron experiment and needed two post-docs (and a couple of graduate students) to pull it off.

The experiment, which involved a complicated piece of apparatus sitting in a neutral external beam outside the Bevatron, was planning to study the $K^0_{\mu 3}$ "charge asymmetry" in the decay of K^0_L mesons to π-μ-ν - the difference between the decay rates to the end states $(\pi^+ + \mu^- + \bar{\nu}_\mu)$ and $(\pi^- + \mu^+ + \nu_\mu)$. This difference is an indication of $\mathcal{CP}$ violation and the effect is tiny, less than half a percent. Don hired me and Bill Ross as the two new post-docs, and then he brought in Bob McCarthy as the student whose PhD would be this experiment, and Jess Brewer as a younger student who would learn the ropes helping with the experiment. We also hired a very fine electrical-electronic technician, Bob Graven, and a handyman-worker Mike Jones. It took us about a year to build the apparatus, and the plan was for the 6 of us, with Don Miller supervising, would do the year-long experiment. But just as we were setting it up in the beam, Miller left! He decided to take a faculty job at Northwestern Univ. in Evanston, Illinois (north of Chicago), and he basically disappeared forever, albeit he did come around every few months to say hello.

Enter *Ken Crowe*. Our little group's offices in Bldg. 50 were fortuitously in the second-floor wing where Ken and his group resided. So from the start all of us made close friendships with the Crowe group's gang of postdocs and students, although Ken himself was not involved with us much at first. Ken's group was doing experiments at the 184-inch Cyclotron, and they were involved with very different physics and apparatus issues, and very different administrative issues too. So we saw a lot of the Crowe gang informally, although we didn't work with them.

But Don Miller had precipitously departed, and we were sometimes in desperate need of advice from an experienced physicist - just the sort of advice that a faculty advisor and group leader is there to provide but that Don Miller was now not providing. As a new postdoc, I was often pretty far adrift, way over my head trying to run a complicated experiment without the supervision that was supposed to be the whole point of a postdoc position! (Bill Ross, the other postdoc, was similarly affected. So were the students, McCarthy and Brewer.) And to make matters worse, we learned the hard way that it was going to take more than the 6 of us to run that experiment day-and-night for a year or more. Bluntly, we were short-handed too, and remained so for the duration, a great strain.

Fortunately, as I said, enter *Ken Crowe*. Ken took the little group under his wing - and especially took me under his wing. He provided the physics advice we sought, "times ten", but more importantly he provided the intellectual environment and the nurturing environment too that are an essential part of any complicated project like the experiment that we were trying to do. His personality was fantastic, as was his physics insight. As was his way of dealing with each of us individually as a special person, each with his own "issues". Ken basically "saved us" in terms of getting that physics experiment done right, but also in terms of making the experience educational, enjoyable, and useful.

Jess Brewer, in case anybody reading this doesn't know it, went from the entry-level-student job with our Bevatron experiment to become Ken's thesis student at the 184-inch, and later to become one of Ken's closest physics colleagues and friends in muon-spin-rotation/relaxation/resonance studies.

Although after that Bevatron experiment I promptly went on to do other very different things (I became a nuclear engineer), that wonderful two-year-plus experience with Ken Crowe remains a vital part of why it all worked out for me. I for one will never forget it.

3 Tom Case

I was a graduate student of Ken's from around 1988 till 1993. I already had a background in solid state physics and building MRI scanners but had little experience in particle detectors or anything in the MeV to GeV range.

There were many projects at Berkeley searching for the next highest energy particle (at the time the Top quark) or maybe something you could spend your graduate career *not* finding, like dark matter, but I just wanted to get well grounded in all the "mundane", "well-known" particles in-between (and actually witness a lot of them myself. Ken had been there laying the foundations so people could scale higher peaks and was still working on many interesting projects in parallel on these "older" particles. In Ken's group I could brush up on atomic physics, nuclear physics, weak interactions, strong interactions, fusion research, heavy ion collisions... all at the same time. I could watch anti-protons eat protons at CERN and wander around all the old and new experiments and hear the history from Ken.

At PSI I could watch muons cause 5 of the 6 hydrogen fusion reactions and finally get past the lies I was told in Junior high school about how the sun works. (We did figure we could witness the 6th reaction, the one that actually runs the sun and has never been seen on earth, but it would take about 10 years).

I enjoyed sitting with Ken doing simple experiments that really made principles sink in; like sticking a pad of paper between a radiation source and detectors and changing the particle energy by adding and removing pages. I could tell he loved the ultimate simplicity behind a good scientific measurement and he really liked to share such things.

At CERN Ken and Penny lived nearby in a village in France. It was always a great pleasure to have dinner with them and talk about something other than physics. Usually with a wildflower arrangement on the table that Penny had picked in the surrounding hills that day. Sometimes we would head off to some small old french village nearby for lunch and enjoy the views of the French alps. Equally nice was visiting the hexagonal house on stilts on the San Francisco bay with Ken's boat docked out back.

I learned a great deal from Ken and his students and colleagues and will remember those times fondly.

Tom Case

Senior Staff Scientist
Xradia Inc.
Pleasanton CA

Feb 28, 2012

4 Don Fleming

I first met Ken Crowe in the spring of 1972, if memory serves. I had just joined the the UBC Chemistry Dept. in July 1971, drawn by the construction then underway of the TRIUMF cyclotron. I arrived from the Niels Bohr Institute with an interest in nuclear reactions and nuclear structure physics, the subject also of my PhD thesis in Nuclear Chemistry from UC Berkeley. A chance phone call from my former PhD supervisor (Joe Cerny) caused me to radically change direction.

Joe told me that he had just attended a seminar given by Ken Crowe on the subject of "Muonium Chemistry". It sounded intriguing so I called Ken and went down to meet him. I was ushered into his office by the comely Corrine. That was my first impression of Ken, good taste in secretaries! My next one was that he was used to being in charge but at the same time I found his enthusiasm about this developing new field of muonium chemistry and his high praise for his grad student who was carrying the ball at the time, Jess Brewer, infectious. I "signed on" for the long haul. Jess was putting literally half-liter Mylar buckets of solutions of different reagents in the backward muon beam at the old 184" and measuring muon spin relaxation rates in order to determine chemical rate constants for Mu, the lightest H atom; it was a bit crude but it was a start, and led to some early papers in the field, co-authored with Fredy Gygax, who was a PDF (from "SIN") in Ken's group at the time.

After his PhD, Jess spent a year or so as a PDF with Ted Bowen, Burt Pifer and the

Arizona group, developing "Arizona muons" (later called "surface muons"). As an item of historical graffiti, we all did the last non-medical experiments at the old 184", the first studies of Mu reactivity in low pressure gases.

Jess then came to UBC, first to the Chemistry Dept., drawn both by TRIUMF and the steelhead fishing in BC, and he, I and my first grad student, Dave Garner (who also contributed at the 184") got μSR started at TRIUMF on the old (and recently dismantled) M20 beam line. Money was tight (it still is) and we had to beg and borrow what we needed to get operational. Ken Crowe once again entered the picture and played a huge role in lending a helping hand. He procured about 25 (yellow) quads for us from the decay (Anderson?) muon channel of the old Chicago synchrocyclotron (some of which are still in use on the M15 beam line), a bending magnet from Cal Tech (the first bender was a dipole magnet "Patty Jane" from Harvard, which David Measday of UBC Physics arranged for us) and two huge old (selenium rectifier) power supplies from the Bevatron at Berkeley. It all arrived at TRIUMF one summer day in 1974 and two years later we were operational, with help from Toshi Yamazaki and Ken Nagamine as well, who were spending time at TRIUMF then.

The first μSR spectrum taken in Canada was recorded (on polaroid!) in July 1976. Today μSR at TRIUMF is an integral part of the new "CMMS" (Centre for Molecular and Materials Science), along with beta-NMR at ISAC, but likely neither would have come to pass without Ken Crowe's initial interest and the huge help he provided. It may sound inadequate but does bear repeating: "Thanks, Ken!" in gratitude. Until we meet again.

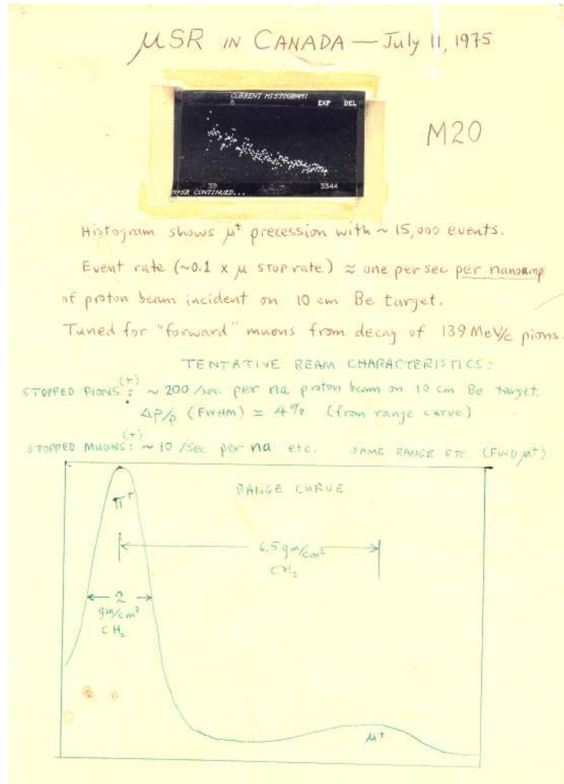

Figure 3: First μSR in Canada: 11 July 1975

5 Mark Lakata

I found Ken by noticing his posting in Berkeley Physics department advertising trips to Switzerland! Well it was either that or Illinois. At this point Ken was already retired, so I didn't actually have too much technical face time with him (his postdocs David Armstrong, Roy Bossingham, Tom Case and Peter Kammel were the ones that kept me on the path to graduating). In fact, during my thesis defense, he just sat and smiled the whole time, and didn't torture me with any questions at all. I couldn't ask for a nicer advisor.

I remember dinners at his house in Point Richmond with his wife Penny, and how he liked to go home early and talk of his dog and his sailboat mast!

The following pictures were taken at my first stay with the Crystal Barrel in 1994. Ken let me stay at his home for a night or 2 before I got access to the hostel at CERN.

Figure 4: Ken at home in Thoiry, July 1994.

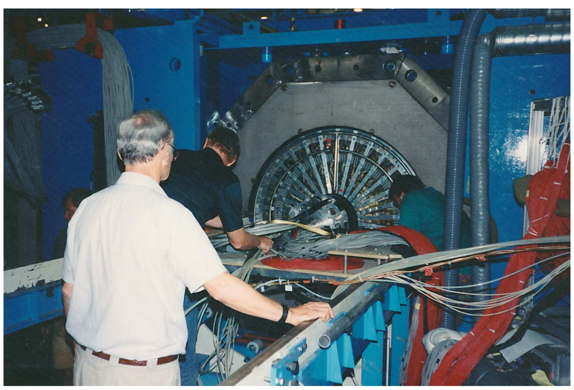

Figure 5: A nice shot of the back of Ken's head at the Crystal Barrel, July 1994.

Figure 6: Here, the Berkeley group (Tom Case, Ken Crowe, Peter Kammel, and I) is headed to a restaurant across the border to celebrate the end of another beam run.

Figure 7: And here we are are the restaurant in St Genis-Pouilly, France.

6 Peter Kammel

In the early eighties Ken and his group joined forces with our European collaboration working on Muon-Catalyzed Fusion at Paul Scherrer Institute in Switzerland. It was a historic moment in this field, which has is origins in Luis Alvarez' discovery of the process at the Bevelac and Dave Jacksons seminal papers. A new resonance mechanism had been discovered enhancing the expected yield to more than 100 fusions per muon. The next experimental step was the systematic study of the most promising deuterium-tritium mixtures. Ken had recently finished his tritium experiment at LAMPF, and had the unique scientific stature, the connections to Los Alamos and the technical capabilities to mount such an experiment in Switzerland.

I had just finished my PhD. thesis, based on an important, albeit serendipitous discovery in related dd fusion, and, as a fresh postdoc, was deeply involved in developing this program. Needless to say, this project naturally attracted Kens strongest interests. The effort was very competitive at the international level. It potential high-impact attracted lots of speculations, where rigorous and critical people with the experience of Ken were required to distinguish science from wishful thinking. And it needed subtle experimental techniques, including the risk involved with handling significant quantities of tritium. The ensuing joint experiments were a highlight in my career, but most importantly established a close relationship to Ken, which would last and guide me through the rest of his and my life. I am writing these lines as faculty at the University of Washington in Seattle. It was largely Kens trust and mentoring that brought me from my native Vienna to UC Berkeley, followed by the University of Illinois and now Washington.

Returning to our early encounters, I was most impressed how a famous Berkeley professor was willing to fight in the trenches to make an experiment happen, and how he shared and instilled young students and postdocs with this desire. Unlike the European style, there was no hierarchical difference between a young student and a senior professor, once the student had earned Kens respect. But, as echoed by many colleagues on this page, earning his respect one had to, which could be a somewhat painful and rattling process until you reached the level at Kens expectations. But once that was achieved the student could count on support, mentoring and encouragement, which at times even generated bolder ideas exceeding your own aspirations before talking to Ken.

In our own career, we often wondered about Kens magic to nourish so many of us to find our own way in science and life. Even, if he did not always agree with our direction. I think, one aspect was his firm believe and respect in personal freedom. Ken was well known for his opinions, and he could argue forcefully. But in the end, he was convinced that every student must find and define his own path, and it was the responsibility of the advisor to encourage, not interfere with this development. He had the greatness of personality to let his students or postdocs run new initiatives, never demanding priority, rather encouraging and pushing them to new responsibilities with great sympathy and understanding. As regards his methods towards this end, he did not believe that success in science is easy. It has to be earned by hard work, disappointments, followed by inspiring rewards. I still remember when I was questioning his judgment when he gave an unduly difficult experimental task to a young student. He explained, that he is aware of the challenge, but if it really is too hard, the student has to come back and ask for help, that will make him grow up. But Ken did not demand anything from others, which he did not demand from himself; he was a risk taker, adventurer and entrepreneur. I vividly remember his role at the inception of the Crystal Barrel experiment. At this time the approval of the new project was in jeopardy, as both European and US funding agencies were expecting the first step from the other side. Ken's determination, negotiating skills and willingness to take on risky and initially underfunded hardware projects were instrumental to bringing this experiment to life and making it one of the most successful experiments of LEAR at CERN and his career. It very significantly contributed to the search for exotic QCD states. Kens group was responsible for its main tracking detector and many analyses. He spend several happy years with his wife Penny living across the French border at CERN during this time.

I worked as a postdoc in Kens group in Berkeley in 1985 and then returned as a research scientist from 1994-2000, where I worked closely with Ken in directing his group, after he retired from UC Berkeley. Our joint research covered a wide spectrum, from Muon-Catalyzed Fusion, to QCD studies with the Crystal Barrel experiments and finally high precision muon lifetime measurements of positive muons and negative muon capture in hydrogen. These latest experiments are just being finalized and they return to some roots Ken planted with his pioneering experiments at Berkeley. But he did much more than that, he defined the field of Medium Energy Physics and seeded a rich field of science.

Ken loved his family. Some of his favorite moments were Monday cafeteria lunches where he could report about the adventures he had over the weekend, fixing houses or sailboats for his children. With a combination of family and hardware he was in his elements. He and Penny were very close to my family, so let me end with a picture of Ken playing a duet with my daughter Laura in his famous self designed Richmond house. (See next page.)

Peter Kammel

Seattle

March 4, 2012

Figure 8: Ken and Laura play a duet.

7 Jeff Martoff

I started working for Ken as a graduate student at Berkeley in 1978. The first intense experience I had with Ken was an incredible 50-hour-straight μSR run at TRIUMF. It was a fantastic experience for me, the first time I had experienced such an intellectually, technically and physically demanding task, and I was in heaven. We had a jury-rigged cryostat/sample holder with a huge heat capacity, an overpowered heater, and a poor thermal link between the heater and the sample. You all know what that does. Oh, did I mention that we had NO temperature controller and that temperature control was the whole point of the experiment?

It took us until about hour 20 to get the thing going and the beam tuned. Ken then proceeded to manually control the temperature in this nightmare apparatus for the next twenty hours well with in the requirements. Throughout the ordeal he was also explaining what he was doing to me, *and* playing and winning at one of those little hand-held mini-pinball type puzzles. To me this was his signature characteristic — incredible intuition for physics. When the run was over we stumbled out into the sunshine (yes, it was actually shining in Vancouver that day) and went looking for breakfast. I had hair down to my backside at the time and Ken's habitual suit and tie were rather the worse for wear — I'm surprised we didn't get picked up by the campus police.

Ken's humanity and kindness don't play a big role in many people's memory of him, but I certainly recall how he took graduate student Cynthia Cattell (my girlfriend at the time) under his wing, tutoring her for hours through her multiple tries at the dreaded Prelims, and then reputedly speaking up for her in the faculty's decision meeting. She's now a full professor at the University of Minnesota and a Fellow of the AGU and the APS, so his intuition for people was apparently as good as that for physics.

One last story abut how Ken brought out the best performance in people. We arrived at LAMPF when it was relatively new, at the very beginning of Ken's experience with what he called "traveling suitcase physics", doing experiments away from Berkeley. This was a pair spectrometer experiment on radiative pion capture, another one of those fields Ken seeded, which was to be my thesis experiment. First off Bill Zajc and I spent a month or six weeks in a room in the basement of the LOB repairing all the trigger scintillators that had been broken during shipping, while Jim Bistirlich labored shirtless in a tent inside the experimental hall to repair the big MWPCs that had suffered a similar fate. Finally the detector system was installed (an epic job involving among other things six or eight guys moving the 30-ton "twin-C" magnet into position in the cement block cave on un-guided air pads, a death-defying maneuver that would never be permitted today), Ken then told me to go up to the counting house and wait for the guy from the electronics pool to deliver the trigger electronics. When the modules arrived and were unceremoniously dumped on the floor, Ken handed me an old logbook from the previous pair spectrometer run in Berkeley, and told me to put together the trigger electronics! This was just about one rack of NIM electronics, nothing by current standards, but I didn't know what a discriminator was at the time. Nevertheless Ken said to build the trigger and build it I did, with distracted assistance from postdocs. When I was finished my fingers were bleeding from doing and undoing BNC's in the dry air of the mesa, but I had done it and it worked.

Ken was always intensely aware of what was going on around him, always sensitive to a new possible branch-line or application or improvement. He was not always aware of (or concerned with) his effect on other people, but he contributed a tremendous amount to the development of science and of his many students and other associates, and engendered great loyalty in them.

8 Curtis Meyer

I first met Ken during my first year of graduate study at Berkeley in 1983 when I approached him about doing research in his group. By that summer, I was working full time in his group. The thing that I most vividly recall was the number of different projects I was able to work on. Ken provided an environment where a motivated student could both thrive and excel. He suggested topics, and then let me run with them.

In thinking back about those days, I recall starting by trying to develop a $\mathcal{CP}$-violation measurement as part of the the TRIUMF

Kaon Factory. In addition to that I worked on the heavy-ion experiments being performed at the Bevatron, ran shifts on a muon-decay experiment at TRIUMF, and then worked on a successful proposal to measure pion scattering on Helium at TRIUMF. Ultimately, I became involved in developing the proposal for my thesis experiment at SIN (now PSI) measuring radiative pion scattering off the proton. Then, during my last year or so, I became involved in the design and construction of the "jet drift chamber" for the Crystal Barrel experiment at LEAR. It was just a great environment for a graduate student to really do physics and Ken made that possible both with the resources and support to do the work, his sense as to what was good physics, and ultimately his style of just letting me run with it all.

I then jump ahead about eight years to the time that I was an Assistant Professor and we were working midnight shifts together at LEAR. It seemed that we both like to fly in from the states and start out with that shift. The late night conversations we had about potential physics directions and his advice on where I was going were invaluable to me at that early stage in my career.

I like to think that it was Ken's style of letting his students run has been what I have also tried to do with my students at CMU. We talk about the problems a lot, but they are the ones solving them, and then making the progress and showing the results. While I did not have a lot of contact with Ken after we wound down Crystal Barrel around 2002 or so, I do think of him often. Both for allowing me reach my potential, but also showing me how to let my students do the same.

9 Jim Miller

I first got to know Ken in the late seventies when I was a postdoctoral fellow in his group. He had his own way of running a group. He knew what he wanted, but it was up to you to get the details right. And if you didn't, he would recognize it right away and he would let you know how it should have been done. Ken always had great ideas, and he often let the younger members of the group run with them. He was also receptive to ideas from other members of the group, it didn't matter how green you were as a physicist, he'd listen. Ken had a nose for physics and he knew a good idea when he saw it, and if it was a lousy idea, he'd tell you that too! Working with Ken turned out to be an incredible learning experience for me, as it no doubt was for the many other students and postdocs who passed through his group over the years. In physics it is sometimes easy to find your niche and work on that in all your experiments, be it DAQ, analysis, electronics, detectors, or.... You couldn't get away with that in Ken's group, and as a result I found plenty of holes in my own training and I hopefully filled a number of them, for which I'm indebted to Ken.

I can look back now at some of the incidents that are amusing now but maybe not so much then. Like dealing with a highly radioactive liquid tritium target at Los Alamos, or dealing with water dripping on wire chambers at Berkeley every time it rained, or fixing detectors that are massively damaged in shipment, or watching melted copper drip from a magnet power supply while I was mapping the magnet, or whiling away precious run time while chambers were being repaired, or building a monstrosity of a tape reading module to try to get around all the data tape reading errors we had. In the end the experiments succeeded — Ken had a way of drawing the best from his people and they got it done. Through it all Ken's groups were remarkably productive from a physics point of view, and the training he gave to people has launched many a great career. There are a lot of people indebted to Ken.

Ken's interests were wide and this was reflected in the fact that he typically had several projects going on at once. For example, in addition to the radiative pion capture and heavy ion experiments I was working on, he was also working on muon spin rotation. Most (all?) of our projects were cash-limited and held together with tape. This didn't stop Ken. He was a whiz with building mechanical things — usually out of junk lying around. On one occasion we needed a remote target positioning device asap, since it was during a data run. He cobbled one together in record time using surplus items he found laying around the lab.

Over the years, I'd see Ken from time to time at conferences, having a chat about physics, or the old times, Ken was great that way. I will miss him. Physics will miss him.

10 Bruce Patterson

I had the pleasure of being Ken's grad student during the years 1972-1975, during the formative period of the muon spin rotation (μSR) technique, at the 184-inch cyclotron. Group members at the time included Jess Brewer, Richard Johnson, and, from Switzerland, Fredy Gygax. It was the Swiss connection initiated by Fredy that ultimately brought me to my present home, at the University of Zürich and the Paul Scherrer Institut.

The Swiss connection also almost destroyed my office in Ken's suite in Building 50. My thesis project was to use μSR to measure internal fields in ferromagnets. The muon beam at the 184 inch cyclotron required big samples, and Ken allowed me to buy what was probably the one of the world's largest single crystals of nickel — a phallic rod, 4 cm in diameter and 20 cm long. My first job, upon receiving it, was to check its crystal quality. Upon Fredy's recommendation, I did this by going to the second member of the Swiss conspiracy, his high-school buddy Gervais Chapuis, who was doing a post-doc at LBL in crystallography. Gervais, in turn, took me to the third Swiss, Eugene Haller, who prepared a horrible mixture of strong acids with which to etch the surface of the phallus, in order to make visible any grain boundaries. In a non-typical Swiss fashion, Eugene mixed far too much of the acid solution. After performing the etch — and determininig that the crystal was anything but single — he thoughtfully put the remainder in a 5 liter polyethelene bottle — "in case you want to repeat the etch", making sure to tightly screw on the lid.

I knew enough chemistry to understand the danger of this mixture, and although I temporarily placed the bottle on the desk in my office, I was planning after lunch to move it to the dangerous chemical storage at the cyclotron. Unknown to me, there was a small piece of nickel left in the acid in the closed bottle. So, during lunch, we heard fire sirens and an emergency announcement of "an acid spill in Building 50". I rushed to the office, to find a squad of white-clad firemen with gas masks and thick rubber gloves spraying neutralizing agents and installing ventilators. Of course, the etching reaction had continued on the small Ni piece, creating sufficient pressure to rupture the bottle, which then became a rocket — flying around the office and spraying acid everywhere. My physics textbooks carry the scars to this day.

A result of this Swiss misadventure was that several days later, an offical emergency action report was posted on all the bulletin boards of LBL. Here was written that "an experimental physicist neglected to follow correct safety procedures". On one of the copies, some wag had scrawled "Acid bust at LBL!" Ken's reaction to the whole affair was remarkably relaxed. In a memorable piece of Crowe ironic humor, he told me to "save the safety report as a confirmation that you are indeed an experimental physicist."

Thanks, Ken, for your inspiration, guidance and friendship. I will miss you.

11 Alex Schenck

Kenneth M. Crowe played a crucial role in my professional life. I first got to know him in the spring of 1969 when, as a post doc, I came to Berkeley as a member of the particle physics group of the University of Washington (Prof. R.W. Williams) to perform in collaboration with Ken a high precision determination of the magnetic moment of the positive muon at the 184 inch cyclotron. After the experiment was set up and commissioned, only D.L. Williams (the Ph.D. student from the U.of W.) and I remained in Berkeley to collect the data, now coming in much closer contact with Ken, who saw to it that the experiment continued smoothly and all possible obstacles were solved.

During this time it occured to me that the implanted muons should behave like protons in the same environment, *e.g.* by showing spin relaxation phenomena corresponding to those studied by the NMR technique. This was tested by stopping muons in a strong paramagnetic solution and — *eureka!* — the muons relaxed as expected. It also became immediately clear that the implanted muons, *via* muonium formation, were subject to chemical reactions just like atomic hydrogen. From the beginning, Ken supported, encouraged and contributed to these studies with much and never ending enthusiasm, providing extra beam time and even organizing suitable targets like, as I remember vividly, a huge single crystal of gypsum. He also arranged meetings with the big shots in solid state physics at UC Berkeley at that time.

This was the beginning of what later was called μSR spectroscopy in Berkeley and became in later years an important research tool in solid state physics and chemistry at the so-called *meson factories* (SIN, LAMPF and TRIUMF), as well as at other laboratories in England and Japan, all inspired by the work started in Berkeley. My involvement in these early studies also paved the way for my later career at ETH Zrich and SIN (now PSI) and would not have been possible without Kens generous support and permission to "misuse" some of the allocated beam time for these exploratory measurements.

Whoever worked with Ken sooner or later would be invited (or rather conscripted) to serve as a crew member on his sailing boat when a race was up to take place. I had the pleasure only once in the summer of 1973 and it was a remarkable experience. That time there were only three persons on the boat: Ken as the skipper, myself and Heinz Graf, a Ph.D. student from the University of Zrich. The competion was fierce, the wind blew strongly and Heinz and I were rather ignorant of sailing. At a certain moment Ken was yelling to Heinz: "*Enough, enough!*" which meant to loosen the rope to the jib, but Heinz was instead pulling the rope tighter and tighter and we nearly tipped over. Another participating boat accused Ken of some unfair manoeuvre in getting in its way with the result that a trial was to take place a few days later at the fashionable Yacht Club in Sausalito. Heinz and I were called to be witnesses. Our testimony must have been quite contradictory and Ken lost the case. But it ended in a very amiable mood as Ken was inviting us for a drink at the bar of the Yacht Club, an unforgettable experience, like so many others in the course of many later years.

Alexander Schenck

Untersiggenthal, Switzerland

21. Feb. 2012

12 Peter Truöl

I arrived in Berkeley in September 1967 with a PhD in low-energy nuclear physics and a small Swiss scholarship. The latter was provided by ETH Zürich in view of training young Swiss physicists as potential users of the planned meson factory, which provided first beams at the Swiss Institute of Nuclear Research in 1975 and still operates today at about 25 times the design intensity. Ken Crowes group in Berkeley and Val Telegdis group in Chicago were recommended by my supervisors in Zürich as possible options, in retrospect I considered myself fortunate to have chosen Berkeley.

The Lawrence Radiation Laboratory at that time was an intellectually very stimulating place, with in-house particle physics still possible at the Bevatron and the 184" Cyclotron. Though I knew next to nothing about particle physics in general and low energy pion physics in particular, I was received immediately as a full member of the group and entrusted by Ken with designing a beam line for an upcoming time-reversal violation test checking detailed balance in the reaction $\pi p \leftrightarrow \gamma n$ in collaboration with UCLA. What I found most characteristic for the way Ken operated his group was the independence and freedom to develop ones own ideas, which he allowed and supported for all his members including the non-academic staff. We knew that for a discussion of our ideas we could always rely on his advice, he seemed to always ask the right questions, and it was quite clear that his immense experience with carrying out pioneering experiments in pion and muon physics would provide guidance so that we would not go astray with our ideas. Many of the principal ideas of these early experiments in the fifties and sixties of the last century in which Ken participated are still with us today or have been recently repeated, *e.g.* search for lepton-flavor violation in muon-electron conversion, precise measurement of the muon-decay parameters, a bent-crystal spectrometer to investigate pionic X-rays and determine the pion mass, time-of-flight methods to determine the π^0 mass, pion-pion scattering investigated through double pion production leading to the observation of a low energy isoscalar enhancement later to be known as the ABC-effect (A. Abashian, H. Booth, K. Crowe) whose origin is still being debated in theoretical circles, measurement of the neutron-neutron scattering length *etc.*

In my case he supported the construction of a pair spectrometer for intermediate energy photons to investigate radiative pion capture in nuclei, a process related to nuclear muon capture, electron scattering and (n,p) charge exchange reactions. This instrument, first equipped with optical and later with magnetostrictive readout spark chambers and finally with multiwire proportional chambers, served first for a series of experiments at the 184" Cyclotron, later at the Los Alamos meson factory LAMPF and was lastly modified for heavy ion physics at the Bevalac. This program was continued as a collaboration after I returned to Zürich in 1971, and profited greatly from the late Helmut Baer who joined Kens group after me. Characteristic for Kens way to support the careers of his young collaborators was also the invitation both Helmut Baer and I received from him to coauthor book chapters and review articles, a rewarding experience.

The only way one could really anger Ken was when we were invited as crew members for a sailing race on the San Francisco Bay: no matter what manoeuvres were to be carried out, we could only go wrong. Otherwise picnics in Tilden park or later on the platform which was to provide the base for Kens new home at the waterfront, and reunions at LaVals Pizza were part of the group's active social life. Needless to say that the late sixties made Berkeley, where many cultural, social and political movements were rooted and changes started, a truly exciting and inspiring place to live.

Though not especially successful scientifically, one of the most entertaining and courageous enterprises was the experiment where a liquid tritium target (58 kCi activity !) was exposed to a pion beam. The goal was to study the trineutron system. I guess only someone like Ken could overcome the administrative hurdles and convince LAMPF director Louis Rosen to allow this idea to materialize, which required extensive safety training and *e.g.* the evacuation of the whole site during transfer of the tritium to the target vessel.

Our LBL and LAMPF experience inspired a continuation of the program with a new spectrometer at SIN/PSI extending into the late eighties, in its last part (measurement of the Δ^{++} magnetic moment) again in collaboration with Kens group.

I felt honoured and extremely touched when Ken flew over from California to join the small symposium and party for my retirement at the end of 2006. He seemed more fragile at that time already, but was in good spirits. This was the last encounter during our more than 40 years of collaboration and friendship; my own personal circumstances and schedule unfortunately prevented me from visiting California after that.

With a publication record extending over 62 years in such diverse areas as muon spin resonance in solid state physics and chemistry, muon catalysed fusion, pion-interferometry in high energy heavy ion interactions, pion induced electromagnetic interactions in nuclear physics, fundamental properties of pions and muons and their decays, and discovering and clarifying a major part of the meson spectrum through antiproton-annihilation experiments, the physics community has lost with Ken Crowe a role model for those few remaining who still find it intellectually stimulating and rewarding to occasionally try to transgress the boundaries of their own narrow fields. Quite a few of his former graduate students and postdocs, who had the chance to build their own groups, have benefitted from his continued encouragement and support and taken his example as a guideline for their own work.

Peter Truøl

29 Feb 2012

13 Bill Zajc

Ken Crowe was an enabler, in the very best sense of the word. He enabled each of his students to pursue a direction of his (I do not know if Ken had any female students) choosing. Once the direction was set, Ken provided support, resources and his own unique form of encouragement. I know this was true in my own case, and my entire subsequent career is directly attributable to his guidance during my time as a graduate student.

I approached Ken in 1977 at the end of the summer following my second year in graduate school. I had done nothing that summer other than read a little physics between workouts and parties. I had somehow developed an interest in soft-pion theorems and current algebras, and asked if I could do a thesis that combined experimental work with some phenomenology. Ken agreed immediately, and was instrumental in getting me paid quickly when he inferred I was dead broke. I honestly don't know if he thought it was possible to do what I proposed, but his willingness to make an on-the-spot decision encouraged me enormously.

At the time I joined Ken's group, he had a major program in muon spin resonance at TRIUMF, and was gearing up for a radiative pion capture experiment at LAMPF (Los Alamos) that would be Jeff Martoff's thesis experiment. I was building power supplies, impressing Jim Bistirlich with my ability to produce cold solder joints and trying to determine if radiative capture on tritium could help understand the 3-neutron final-state interaction (this went nowhere). At some point early in 1978, I attended a seminar in the Nuclear Science Division by a Miklos Gyulassy, then a post-doc at LBNL (now a valued colleague at Columbia). Miklos spoke about using two-pion interferometry to search for coherent pion emission in heavy ion collisions, in rough analogy to two-photon interferometry (the Hanbury-BrownTwiss effect) in astronomy. I had learned just enough from Gordon Baym's QM text about HBT to be dangerous, and these ideas really intrigued me. I tried to explain them to Ken, perhaps the same afternoon. I am sure my attempts to explain coherence were incoherent, but in the end he asked me, "Why don't you think about how to do an experiment to *measure* this stuff?"

From that point on, I was obsessed with the topic. I knew nothing about experimental design, but with huge amounts of help from members of the group (and Miklos) managed to cobble together a design that resulted in a proposal to Bevalac Review Committee in April, 1978. We received some lukewarm encouragement, but by November, 1978 had full approval; this after spending the summer with the group's attention focused on the experiment at LAMPF. At the time, I was frustrated with the time spent away from "my" experiment. In hindsight, I realize what an extraordinary gift Ken gave to me — a flaky graduate student interested in a flaky experimental technique in a flaky (in Ken's view) field is given the go ahead to pursue an entirely new effort in a small group that was undoubtedly cash and resource strapped. Extraordinary indeed, and a tribute to Ken's ability to see and to seize a physics opportunity.

Of course, much of this work took place in response to Ken's "own unique sense of encouragement". He was literally hopping mad the morning of the presentation to the Bevalac Review Committee when I informed him I had just discovered a factor of 6 error in the rate calculation (of course in the wrong direction; I was so ignorant I had not realized the Bevalac was a pulsed machine). He could not hop while he was driving me and a (greatly embarrassed) Japanese collaborator back to the mesa in Los Alamos, but he made it clear how stupid my design was for a counter mount. These were painful, but well-deserved, moments in my education as a graduate student. Ken was decades in front of the "tough love" movement, but he was a master at it. It helped tremendously to know that through it all, he supported his people. I have a vivid memory of a telephone conversation coming through his closed office door at very high volume as he dressed down the head of the lab's Real Time Systems Group — one of the RTSG techs had manage to wipe out my entire collection of good events I had spent weeks culling from tapes processed by our PDP-11. Ken got the tech assigned to go through the same multi-week exercise, freeing me to work on other parts of my data analysis.

Upon graduating, I went off in a somewhat different direction from Ken's main interests, and never had the opportunity again to collaborate with him. I take some small solace in knowing that the last two times we met, at a symposium celebrating Miklos Gyulassy's 60th birthday — see `http://www-nsdth.lbl.gov/mg60/program.htm` — in 2008 and at Berkeley Physics Colloquium last fall, I had a chance to publicly acknowledge my indebtedness to him. I only wish that I could have one more chance to do so, for he was a most extraordinary mentor.

14 Mike Zeller

Ken was my friend and mentor. As a student from UCLA doing an experiment at the Bevatron, I was fortunate enough to be assigned to Ken's group. For better or worse he treated me like one of his own. He worked me just as hard, was on my back just a hard, and taught and mentored me just as he did his own students. And when it came time for me to find a job he went to bat for me, and I've been at Yale ever since. So I only owe my whole career to Ken.

He was my friend too. We crossed paths often at PSI and a few years back at Peter Truoel's retirement (Peter was a post doc when I was a student). In hindsight, I think the world felt a little safer for me knowing Ken was there. I will miss him.