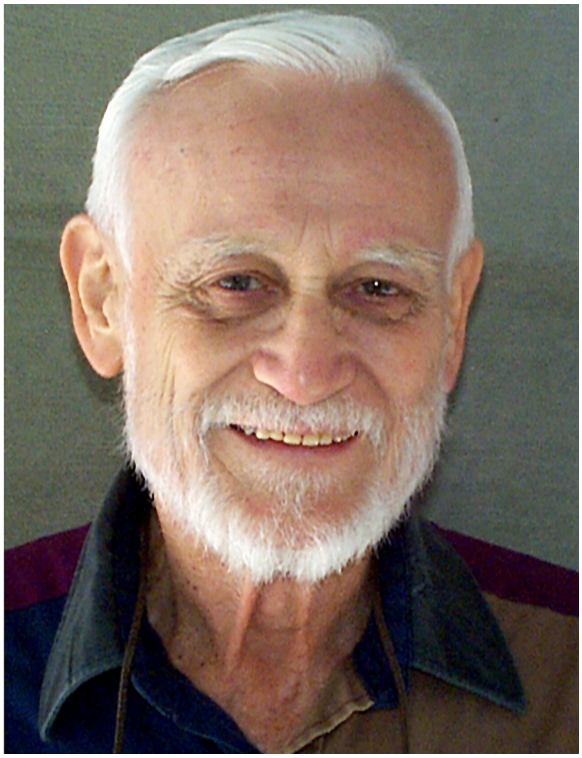

PAUL EDWARD DAMON
(1921–2005)

"I arrived at the University of Arizona in time for the fall semester of 1957. The Geology Department had been criticized by the Physics and Chemistry departments as a good pick and hammer department but behind the times. Urey, a chemist, had initiated the use of stable isotopes in geology, making use of Neir's advances in mass spectrometry. Neir, a physicist, had advanced geochronology; Blackett, a physicist, had initiated the field of geomagnetism; and Libby, a chemist, had developed ^{14}C dating. I was supposed to bring the department into the post-World War II modern age of science with the commission to build an up-to-date ^{14}C laboratory and to build a K Ar laboratory."

Paul Damon, quoted on American Geophysical Union Web site on Solar Variability and Climate Proxies, 2000. (Photo above courtesy of the Geosciences Department, University of Arizona.)

Paul E Damon was born in Brooklawn, New Jersey, in March 1921 and was educated at Bucknell University (BS 1943). Upon graduating from Bucknell, Paul served in the Navy during World War II, which became a source for many of his "old naval" stories. After the war, he continued his education at the Missouri School of Mines (MS 1949) and Columbia University (PhD 1957). His academic career included time at the University of Arkansas as a research associate and then assistant professor (1949–1954), followed by a research associateship at Columbia (1954–1957). At Columbia, he worked with Prof J Laurence Kulp, who had developed some early radiocarbon dating counters.

At Arizona, Paul was Associate Professor in the Geology Department (1957–1962) and then Professor of Geosciences from 1962–1989. He retired in 1989 but continued to have an active research program in studies of solar variability and climate right up to his death.

After Paul Damon arrived at Arizona, he was instrumental in building up the Geology (and later Geosciences) Department to its current level as a multidisciplinary, modern department with a strong emphasis on instrumental methods. Although a small ^{14}C laboratory had already existed at Arizona under the auspices of the well-known Prof Emil Haury, Paul set up the first ^{14}C laboratory based on gas counters, which was then the "state of the art." He was also instrumental in getting a National Science Foundation grant to establish the first dedicated accelerator mass spectrometry laboratory for ^{14}C measurements, a technique which revolutionized ^{14}C dating in the early 1980s.

During his tenure, Paul was instrumental in hiring many well-known members of the Geosciences community at the University of Arizona.

Paul also encouraged the early use of AMS technology, including the famous dating of the "Shroud of Turin," published in 1989 by Damon and 20 other authors.

He is remembered by his colleagues as a strong fighter for what he considered to be right and a man of great principle in both scientific and political matters. For example, Paul engaged in several high-profile scientific discussions both with "creationists" and those who claim climatic change was not a real phenomenon. Indeed, his last day at work before he died, he was working on a rebuttal to some scientists who had been asserting that observations of recent climatic change were not caused by human activity. His colleagues have noted that "this is how he would have wanted it."

His career subjects ran a wide range of geochronology (both carbon-14 and argon dating), geochemistry, isotopic studies, paleoclimatology, atmospheric evolution, the origins of ore deposits and volcanic rocks, and the geology of the North American Cordillera. He was the author or co-author of some 200 scientific publications.

Paul is survived by his wife, Mary Janet (Jinx) Damon, a sister (Lucille Damon Gallo), and 2 sons (John Edward and Timothy Winter Damon), nieces Ellen Gallo Verdi and Bonnie Burchardt Crocetti and his 2 grandchildren, Edward and William. He died on April 14, 2005 at the age of 84.

AJ Timothy Jull
Editor

Paul had an important influence on my career and approach to science. His contributions to the 1969 Nobel Symposium in Uppsala were seminal to the PhD project I began a few months later at the Australian National University. Jim Bowler had attended the symposium, and lent me copies of manuscripts and notes; the collection of papers opened my eyes to the important issues in radiocarbon variations and the links with geomagnetism. With those ideas firmly imprinted on my mind, I began simultaneous ^{14}C and paleomagnetic measurements on fireplaces from archaeological sites in southeastern Australia.

I met Paul at the 8th International Radiocarbon Conference in Wellington, New Zealand, in 1972. After the conference, Paul was one of the group of ^{14}C *aficionados* who traveled to Canberra on a flight from Wellington, interrupted by a bomb hoax and an emergency landing at Auckland airport.

During the long wait in a warehouse while the plane and all luggage was thoroughly searched, the group of us consumed a bottle of duty-free whisky and conversation flowed easily (see the picture below). Bryant Bannister was briefly escorted back to the runway by police and asked to explain why there was a length of pipe and a clock in his bag, by then thoroughly doused in foam. The police were not amused as Bryant explained the workings of a tree-ring corer (unrelated to the alarm clock).

Paul Damon (standing and pouring the whiskey) making the best of an unexpected stopover at the Auckland airport in 1972, at the conclusion of the International Radiocarbon Conference in Wellington, New Zealand. Also pictured are Bill Buddemeier (at center, standing and wearing a black jacket) and Henry Polach (far right, in white shirt), among others. Photo courtesy Mike Barbetti.

Most of my research dealt with late Pleistocene variations, but of course all that had to be closely related to the growing body of knowledge on Holocene variations. While writing my thesis, I began to see that ^{14}C variations were better known and understood than the global geomagnetic field strength, and that any *discordance* between the two must be the result of insufficient and sometimes inaccurate geomagnetic field strength measurements. I wrote to Paul and proposed that we might prepare a joint paper for the IAGA meeting in Kyoto later in 1973. After a few weeks I telephoned, and he responded that he had toyed with the idea but was not ready to put pen to paper. I duly completed my PhD thesis, with Paul appointed to be one of my examiners, and met up with him again at Kyoto. We were standing in the grounds of the Conference Center after the fireworks at the opening reception, and he remarked that the *yakitori* was very good! He talked about Arizona, his home, how he and his wife Jinx would sleep outside under the stars.

It became clear that Paul was not at all sure about the proposal I had made, and I began to worry about the effect that might have on his appraisal of my thesis. Towards the end of the meeting, I was due to give a review of geomagnetic field strength for the late Pleistocene, and I decided to compress that part and deal mostly with the Holocene. I used one of Paul's and Ed Wallick's diagrams, with a smooth, near-sinusoidal fit to ^{14}C variation over the last 7500 ^{14}C yr, to illustrate the idea that the peak in global geomagnetic intensity must have been earlier than the peaks from Europe in the 1st and 2nd millennia AD, and that the European data was not representative of the global field. Paul

was the first to stand up in question time; he moved from his seat at the back of the room to the front of the audience and spoke about the general *concordance* of geomagnetic and long-term ^{14}C variations. Each of us had explained our views, and they differed. As Paul completed his long speech I had no answer, so I asked, "Do you really think the two sets of data are concordant?" His reply amazed me: "I'll have to sit down and think about that!" At the end of question time, Paul came forward again with some calculations on the back of an envelope, and said that he could not make the data sets match, so there was indeed some discordance. I felt a great sense of relief, and Paul must have later written a good appraisal because my PhD was awarded without difficulty in 1974. Paul had shown generosity of spirit and an ability to adapt, while reminding me of the need for critical appraisal and rigorous analysis of ideas in science; those were four of his hallmarks as a scientist. Eventually, Ron Merrill, Charlie Barton, and I published the idea in 1979, while Paul and Rob Sternberg did the rigorous modeling, which they published in *Radiocarbon* in 1983 (Vol. 25[2]:239–48).

I kept in contact with Paul by occasional correspondence and discussions at various conferences for the next 30 years, and my admiration for the great scientist never waned. Perhaps fittingly, our last meeting was at the 18th International Radiocarbon Conference in Wellington in 2003. Paul's recent work focused on Earth's climate and solar-terrestrial relations over the last millennium, but his interests included supernovae and the stars beyond. Vale, Paul.

Mike Barbetti
University of Sydney

Remembering Paul E Damon - My Mentor

When I decided in 1961 to return to academia to seek a PhD, I was encouraged by John P Miller, Luna Leopold, Marie Wormington, and J O Brew to attend Harvard University to study geomorphology under John Miller. Upon his tragic death from bubonic plague, there was no point in going to Harvard because John was the last to teach in the Kirk Bryan tradition of geomorphology. Luna Leopold then suggested I apply to either Johns Hopkins with Reds Wolman or the University of Arizona with John Harshbarger. I decided on Arizona because the presence there of: (1) Emil Haury, who had excavated the fascinating Naco and Lehner Clovis sites; (2) the Geochronology Laboratory under the direction of Ted Smiley had an international reputation of excellence in Quaternary studies; (3) Paul Martin, who pioneered alluvial palynology for paleoenvironmental reconstruction; (4) John F Lance, an outstanding teacher, vertebrate paleontologist, and geologist; and (5) an up-to-date radiocarbon laboratory directed by Paul Damon, who had brought the lab up to state-of-the-art efficiency.

Upon my arrival in Tucson in 1962 for the spring semester, John Lance introduced me to Paul Damon. I explained that my interest in ^{14}C dating was to better understand the method and its limitations, whereupon Paul suggested I take both his isotope geology course and his nuclear geology course. He then asked how I had done in calculus, phy-chem, and thermodynamics at Colorado School of Mines. I explained that I had passed them, but they were not my strong points, and I had been away from them for quite a while. Paul, in his typical upbeat style, said no problem, "They will all came back to you in class." I had real anxiety that first day of class, and when Paul started out deriving a big integral equation on the blackboard I worried about how many of these he might ask us to derive on exams. Well, as it turned out, Paul took great pride in showing us how to derive such

equations but never asked us to derive them. All we had to do was know how and when to use them. Basic math never appealed to me but using math to solve problems that interested me was not a problem, as long as I had someone like Paul to check my results. He had an incredible analytical mind. I always marveled at his ability to look at a problem and immediately derive a formula to solve it.

When I came to Arizona, George Agogino and I had conducted geoarchaeological investigations in Sandia Cave in New Mexico, where Frank Hibben had reported on the Sandia Complex as being the first pre-Folsom archaeology in America. We wanted to collect samples for ^{14}C dating and provide good stratigraphic control. With Hibben's encouragement and USGS WAE support, thanks to Luna Leopold, I worked out the stratigraphy from remnants of strata adhering to the cave walls and in undisturbed parts of the sedimentary fill. I brought all of the samples with me to Arizona with the idea that this research would be the subject of my PhD dissertation. Paul had me understudy Austin Long in the ^{14}C lab, where Austin taught me how to chemically pretreat samples, operate the combustion system, purify the CO_2, fill the proportional counters, check the high voltage curve, count beta particles, and calculate ages and standard deviations. When Austin obtained employment running the Smithsonian's ^{14}C lab, Paul had me take his place in running the Arizona lab as a research assistant. He allowed me to initially devote full time to analyzing my own samples. I don't know if Paul considered this, but I thought this is great, if I screw up the only loss will be my own sample, not somebody else's. So, before summer fieldwork began I had successfully ^{14}C dated most of my Sandia Cave samples. Paul had decided that I could operate the ^{14}C laboratory well enough that he put me on as co-investigator on his next proposal to NSF.

Then came the Tule Springs project in Nevada. During the summer of 1962 Jim Hester and I worked for Fred Wendorf and the Museum of New Mexico at the famous Clovis type site in Blackwater Draw, New Mexico, where I recorded stratigraphy and Jim and I collected pollen and ^{14}C samples. It was here that I received a telegram from Marie Wormington, curator of archaeology at the Denver Museum of Natural History, asking if I would be interested in being the geologist on the Tule Springs project to evaluate the geoarchaeological claims for 28,000-yr-old evidence of early man at Tule Springs, Nevada, 10 miles north of Las Vegas. Of course! So I dropped out of school again and spent the winter of 1962–63 working out the stratigraphy and geochronology of this part of the Las Vegas valley. John Lance, Paul Damon, Paul Martin, and Emil Haury were all advisors on the project and encouraged me to make Tule Springs my dissertation subject rather than Sandia Cave. So, once again back in the ^{14}C lab Paul had me run the lab and run my own samples through with whatever samples were in the pipeline. I was then employed as Paul's assistant in operating the Arizona Radiocarbon Laboratory.

On the next proposal to NSF, Paul had me be the principal investigator. This was obviously Paul's effort to get me into the ^{14}C dating "fraternity," and it worked. I will be forever grateful to Paul Damon for his foresight and his role in starting me out in a new career.

There are several humorous instances I could relate, but I'll mention just two. I knew Paul had confidence in my research when, in 1967, he asked me to accompany him to a symposium of the International Atomic Energy Agency in Monaco on radioactive dating and to present a paper on ^{14}C dating of bone. During the mid-conference break at Monaco, Paul and I went to Nice and thoroughly enjoyed good food and excellent wine, maybe a little too much wine. We took a taxi back to Monaco in the wee hours of the morning. After collecting our room keys at the front desk and climbing to the third floor, we found the hallway totally dark. So Paul felt his way to his room at one end of the hall while I made it to mine at the other end. The dead silence was interrupted by the sound of our keys

rattling in the door locks. They weren't opening. We both realized at the same time that we were at each other's room. We both got on a laughing jag and could not stop laughing as we passed each other in the dark and got to our proper rooms. As we turned our keys over to the desk clerk the next morning Paul apologized for the noise we had made coming in that night, but it was not the same clerk. So, he did not have a clue as to what Paul was talking about.

In the mid-1960s, I introduced Paul to Donald C Grey, a physics teacher at Sheridan Junior College, Wyoming. Don was a remarkable individual who had designed and built his own ^{14}C lab at the college in Sheridan. He actually successfully dated several samples, but without financial support he could not operate the lab. While working with Don on the geoarchaeology of the Sisters Hill site in Wyoming, it was apparent that Don had an incredible knowledge of all aspects of ^{14}C dating as well as geology and archaeology. He also had a remarkable ability with mental calculations. When he expressed an interest in going somewhere for a PhD, I, of course, suggested the University of Arizona and Paul Damon. With Paul's encouragement, Don came to Arizona and worked as my RA in the ^{14}C lab as he studied. One day, Paul was working on some calculations with a slide rule and Don was looking over his shoulder. The calculation involved multiplication, division, and addition. Don rattled off the answer to two decimal places from his head. Paul, a bit annoyed, said to hold-off, he would have the correct answer in a minute, and continued sliding his rule. He came up with exactly the same number. Paul and I were both dumbfounded. How could Don do that in his head?!

I always marveled at Paul's incredible ability to keep so many balls in the air at one time. In addition to his teaching load he not only ran the ^{14}C lab, he ran the potassium-argon lab, the lead-uranium lab, a helium lab as well as other operations, and still played an active role in the Geosciences Department and the Geochronology Laboratory. He was often in the field collecting samples. Furthermore, he and Jinx got thoroughly involved in helping graduate students with their personal problems. Meanwhile, he always maintained his good sense of humor and an upbeat attitude. He was always available for discussions, be they scientific, political, or personal.

I miss him dearly.

Vance Haynes
University of Arizona