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Citation for presentation of the 1999 C. C. Patterson Award to R. Lawrence Edwards

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Mr. President, Colleagues, Ladies and Gentlemen:

I received an urgent message that Larry Edwards had telephoned with a request that he should be called at work or at home. My first thought was—what in Heaven's name was wrong? When I reached him, he informed me that nothing was wrong. He was to be awarded the Patterson medal and asked if I would introduce him. That was wonderful news and I was most pleased to accept this happy task. It is a privilege and a delight for me to introduce R. Lawrence Edwards who is to be the recipient of the C. C. Patterson award of the Geochemical Society. This award is for a recent, innovative breakthrough in environmental geochemistry. Having known Claire Patterson since 1948, I have some basis of guessing what his views of Larry Edwards' contributions would be. I think Pud would point a very long, bony finger at Larry Edwards' face and say, "Edwards, you know I don't like engineers and technologists—but—you have made a scientific breakthrough and do wonderful science," and then he would shove the long, bony finger into Larry's chest and say, "You are a poet and a scientist."

R. L. Edwards was born in 1953 in Boston, MA. His father was a professor of art at the University of Michigan. This provided Larry with a sort of hereditary predisposition toward an academic career. Larry took his BS at MIT in 1976 and then went to the University of Michigan where he studied biotite—garnet stability with E. Essene and obtained a belated Masters of Science degree in 1986. In 1981, prior to getting his M.Sc. he came to Caltech as an NSF fellow to pursue a doctorate program. He started out as a graduate research assistant under my guidance. The first problem he attacked was a study of the Sm-Nd and Rb-Sr systematics to establish the age and emplacement of obducted Paleozoic oceanic crust in the Urals. This study was successful scientifically and was also successful in training Edwards in the laboratory procedures and approaches of isotopic geochemistry. With the completion of his study of the Paleozoic oceanic crust, the question then came up—What should he choose as a topic for a PhD thesis? It certainly should be a problem in a scientifically different area than the work he had just completed.

At about that time, Dr. James Chen and I had been wrestling with the problem of possible variations in $^{235}U/^{238}U$ in calcium-aluminum-inclusions in meteorites. This would be produced by the decay of ^{247}Cm to ^{235}U if there was early chemical fractionation of U from Cm in some mineral phases. After a major effort, it was shown that there was no evidence of high ^{247}Cu abundances in the early solar system (contrary to a major article in Nature). After this major effort on a cosmochemical problem what was left was a negative result and a new shovel (Fig. 1). Using a double spike and high ionization efficiency, the precise measurement of U isotopes in samples of $\sim 10^{11}$ atoms of U was now readily done. During a lecture in my course on mass spectrometry and isotopic geochemistry, I showed data of U from a sample of sea water that had been obtained by Dr. James Chen. This demonstrated that precise ^{234}U measurements were possible. Larry Edwards was in the class and grabbed on to this new approach to the U transport problem - he thought that it might prove to make an interesting thesis topic. Indeed, it was a fine choice.

The basic technical issue (sorry Pat) was that the precision of a TIMS measurement for a number of atoms N with ionization efficiency f is given by:

$$P_{TIMS} \propto \frac{1}{\sqrt{fN}}$$

while the precision of α counting for a time t is

$$P_{\alpha} \propto \frac{1}{\sqrt{\lambda N t}}.$$

When the precision for both techniques is equal [$P_\alpha = P_{TIMS}$] then $t\lambda = f$. At ionization efficiencies of $f \sim 10^{-3}$, for counting times of 10^{-1} y, this corresponds to $1/\lambda = 100$ years. This means that mass spectrometric measurement of rather short-lived nuclei can be far superior to α counting. This TIMS approach is the same as the AMS approach, but with a "small" standard instrument with no added costs. It follows that a host of rather short-lived nuclei can now be reliably and precisely measured using TIMS. If the lifetime is over several thousand years you don't even injure the pulse counting system. Now the problem that Larry had to face was how to get adequate Th^+ yields. The first try was to do the simpler problem of direct and precise determinations of ^{234}U, ^{238}U, ^{235}U and ^{232}Th in sea water.

After succeeding in this effort (Chen et al. (1986)), Edwards went on to the problem of dating corals and the sea level problem. This had been an old saw–the coral problem, sea level subsidence and uplift had been recognized by Charles Darwin during the voyage of the Beagle. Ionium (^{230}Th) dating has a grand and ancient history. Discovered by Boltwood in 1908 at Yale [this may have stimulated K. K. Turekian's early involvement in the field], there was an active community doing dating with α-counting techniques for both ^{230}Th and ^{234}U. Pioneering work in this area had been done by A. Kaufman and W. Broecker. However, the field had sat in a side alley for a couple of decades because of analytical problems, long counting times with substantial errors and the real threat of open system behavior of natural samples. Well, Edwards got started—he was discouraged in the beginning because of the technique problems. He wanted to do real science, not develop techniques. However, Larry stuck with it and the results were TIMS dating at 130,000 years with a precision of ± 1000 years (2 σ), 180 years at ± 5 years Edwards et al (1987a,b). The question

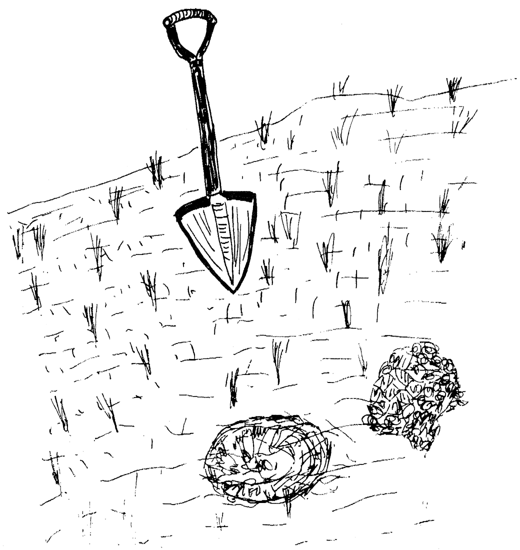

Fig. 1. Sometimes science advances because of a new shovel and the urge to dig a hole somewhere. Sometimes science progresses because there is a need to excavate at a particular place and an appropriate or inappropriate shovel is invented.

then was, what age would be measured on a zero age coral—any initial ^{230}Th could make life complicated. Edwards began measuring a modern coral sample on the old Lunatic I spectrometer and found a very small but real residual signal of ^{230}Th in it. "Well, how old is the coral?" someone asked. Larry ran down to his office in the basement, looked up the collection notes, ran back up to the lab and also calculated the age on line from the measurement. The next thing I knew, I got a call to go into the lab immediately. Edwards was performing some kind of small dance, levitating about 1 meter above the floor without any visible means of support, except for what I took to be propulsion by madness or joy. The sample of live coral had been collected 13 years previously and the Lunatic I was spewing out an age of 15 ± 5 years. Now I knew how it was that he could levitate! Larry pursued the work of coral dating, climate change and sea level and got his degree. A presentation of the results at the 1987 Geological Society of America conference on "Late Quaternary Sea-Level: The Marine and Terrestrial Record" generated lots of excitement. As a result of his work, Quaternary Science Reviews published a special page of congratulations (Fig. 2). Larry established contact with real experts on corals and Pleistocene stratigraphy and climate. He ventured to work on dating co-seismic uplift (Edwards et al., 1988) using corals in a study with F. W. Taylor of the University of Texas. All of this work was carried out without NSF support—our proposal was rejected. We were not in the legitimate club of alpha counters and carbonate people. The NSF did, however, find resources to fund a new α spectrometer for some group, thereby buying into immediate obsolescence.

Edwards submitted a thesis that was considered by the fac-

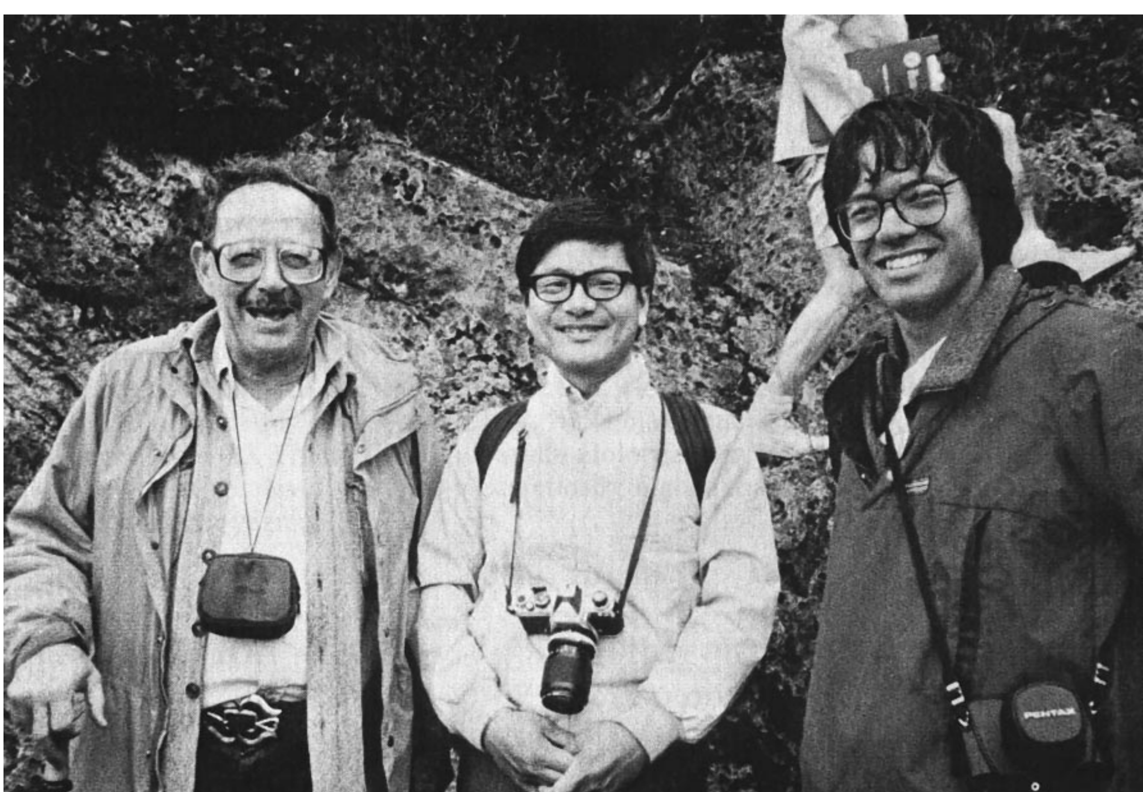

Fig. 2. Lawrence Edwards, J. H. Chen and Jerry Wasserburg relaxing in the field in Bermuda during the April 1987 GSA Penrose Meeting on "Late Quaternary Sea-Level: The Marine and Terrestrial Record". *Quaternary Science Reviews* **6,** 181 (1987).

ulty at Caltech to be both exciting and distinguished. His work has led to a very precise absolute time scale that is independent of cosmic ray production and the CO_2 input/output into the atmosphere. In 1988 he joined the University of Minnesota as an assistant professor and began to set up his own shop. Al Nier was a great fan of this work. At Minnesota Edwards distinguished himself with his teaching and research. He established a world class research group with students, post-doctoral fellows and collaborators on a worldwide scale. He directed his efforts toward problems of geochemistry, oceanography and climatology. Minnesota became a center of new approaches and exciting new data. Edwards and his associates have carried out studies of the precise (and maybe accurate) timing of high sea level stands over the past 200 Ky in speleothem deposits and related these ages to continental climate changes, seeking to bridge the gap between the marine and continental records and vegetation shifts (Gallup et al, 1994; Dorale et al., 1998). They have also studied the shift in atmospheric $^{14}C/^{12}C$ and calibrated part of the ^{14}C "time scale", in particular showing that atmospheric $^{14}C/^{12}C$ sharply dropped during the younger Dryas (Edwards et al., 1993). With Jess Adkins and his associates, he has been involved in measurements of $^{14}C/^{12}C$ in benthic corals dated by ^{230}Th which show that the ventilation rate of the N. Atlantic varied greatly locally during the last deglaciation (Adkins et al., 1998). A very impressive application of protactinium-U-Th dating using TIMS follows up on the original LANL report and provides a new and independent means of dating and establishing an independent test of concordance with these different chronometers (Edwards et al., 1997; Cheng et al., 1998). The problem of precise ages that are not "correct" because of diagenetic alteration may now be addressed. A most exciting development was determination of sea-surface temperatures from Sr/Ca ratios (and then U/Ca ratios) in coral skeletons which were found to record seasonal changes in tropical sea-surface temperature over the past 10^5 years (Beck et al., 1992; Min et al., 1995). This temperature-dependent, chemical fractionation was again a potential scientific problem that sat around for many decades because of a lack of adequate analytical approaches and careful formulation of the problem. Edwards and associates resolved this problem beautifully. The result of these investigations now permits us to study seasonal changes in temperature over this critical period of climate change. Unfortunately, Edwards and his colleagues have not been able to date things to within 6 months back at 10^5 years ago, so that the seasonal change cannot be correlated on a global scale over the Pleistocene, but only locally. In time I expect that he may come up with a solution to this problem!

At Minnesota, R. Lawrence Edwards has created a flower garden of science in problems of isotopic geochemistry, climatic change and chronology. Whole new approaches have been developed and applied by Larry and his coworkers. In particular, he has been able to set up effective collaborative studies in very diverse areas with people from "all over". These approaches are being emulated and followed in laboratories throughout the world. Some of the exciting new results are being reported in the special sessions at this V. M. Goldschmidt meeting. If any of you want to see some of the flowers in the garden, I suggest you attend these sessions.

Mr. President, I have the privilege to present to you R. Lawrence Edwards for the Patterson Award. Unfortunately, the stipulation of the award is for "a recent innovative breakthrough in environmental geochemistry of fundamental significance, published in a peer review journal." I leave it to you to select which one of Larry's many contributions to use as the basis for this award.

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C. C. Patterson Award Acceptance Speech

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Mr. President, fellow geochemists, and friends, I would like to thank you all for this very special honor. Professor Wasserburg, I would like to thank you for, among many gifts, your very generous citation.

Thus far in my career, I have been blessed with wonderful advisors, advisees, collaborators and colleagues, tremendous family support, and resources to pursue interesting problems. I have been happy leading a modest research effort, and frankly awards and the like have been far from my mind. For this reason, when Mike Drake contacted me about the Patterson Medal, I was overwhelmed and particularly gratified for this show of support from the geochemistry community.

For me, the Patterson Medal has a personal connection. I remember vividly my first meeting with Pat as a prospective graduate student visiting Caltech. I knew of Pat's work and was keen on meeting him. I had met with a number of faculty members who discussed research possibilities and encouraged me to attend Caltech. Toward the end of the day I knocked on Pat's office door and explained the purpose of my visit. His immediate reaction was, "You don't want to come here." Nevertheless he invited me in and we discussed the age of the earth, lead pollution, and communication between the scientific community and the public. Throughout this discussion he interjected this refrain, "You don't want to come here." Of course, I rejected this advice, and as I got to know Pat I came to realize that he left out a qualification in his refrain. He meant to say, "You don't want to come here unless you are completely dedicated to science". So for me, the Patterson Medal stands for this sort of devotion. If, in some small way I can live up to this trait of Pat's, that would make me very happy.

The site of this presentation also has a personal connection, as I was born in Boston and spent my early years just a stone's throw away from here in a small house on Harvard Street and my college years down the road at M.I.T. My mother, who had emigrated from China, and my father instilled in us, myself and my three sisters, a sense of exploration, a sense of curiosity about nature, and the idea that in our professional careers we should not necessarily "be someone", but that we should pursue an activity that was of great interest to us. I have followed these principles along a winding path ultimately into our field of geochemistry.

I did start out as a geology major as an undergraduate. Among the many great scientists at M.I.T. at the time, I consider the late Roger Burns to be my undergraduate mentor. Because I had a scheduling conflict, Roger taught me Mineralogy one-on-one. I remember being flabbergasted at the time that a professor would take the time to do this. In retrospect, I am even more flabbergasted.

LARRY EDWARDS

Despite Roger's and others' guidance, when I graduated from M.I.T., I thought I was leaving academics for good. I headed off to the north woods of Minnesota to work as a naturalist. In the next few years I flirted several times with the idea of pursuing a medical career, but ultimately returned to the field of geology, which was of great interest to me.

At the University of Michigan, my master's advisor, Eric Essene introduced me to research and encouraged me to think critically, but more importantly Eric has a wonderful quality of instilling confidence in his students, an attribute for which I am grateful.

As I worked my way through career decisions, others were carrying on a line of research that would prove to follow a similar winding path. In the early 1980s, Jim Chen and Jerry Wasserburg developed a high ionization efficiency, double-spike technique for measuring the Uranium-235/Uranium-238 ratio in small amounts of uranium (Chen and Wasserburg, 1981). The impetus was to address early solar system problems. With a large number of careful measurements on meteorite uranium, they demonstrated that there was no evidence for high levels of live Curium-247, a parent of U-235, in the early solar system. Jim and Jerry reasoned that if they could measure picogram levels of U-235 by thermal ionization mass spectroscopy, they should be able to measure picogram levels of U-234, a much lower abundance isotope of uranium with the same

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methods, and with the encouragement of John Edmond and Karl Turekian set off to do so.

About this time, the mid-1980s, I was a doctoral student at Caltech casting about for a thesis topic. Jerry and Jim invited me to work on their project. It was the chance of a lifetime, and I will always be grateful to both of them for providing me with this opportunity and guiding me through it. Together we performed the final tests on the U-234 measurements and began exploring the possibility of measuring Thorium-230, generally an even rarer isotope. My background at the time was in petrology and the original thought was to use the new measurements to study magmatic processes.

As we began to measure thorium standards, it became clear that the most straightforward materials to measure would be those with the highest Th-230/Th-232 ratios. A trip to the library revealed that corals had the highest reported Th-230/Th-232 ratios of any natural material. Our first measurements (Edwards et al., 1987) showed that coral values were even higher, orders of magnitude higher than the values that had been reported. From a technical standpoint, corals were the ideal materials to measure. From a scientific standpoint, it also became evident that the high precision dating that measurements on corals could provide could open up a number of fields in the general areas of paleoclimatology, paleoceanography, chemical oceanography, and Quaternary geology. So, I set out to learn about, what to me was a new area of geology.

During this development, I was very much welcomed by a host of scientists in these fields. Early on I was helped by, Richard Ku, down the road at U. Southern California and by Fred Taylor, Art Bloom, Karl Turekian, and Rob Matthews. Through the years there have been many others: Jay Banner, Ed Boyle, Wally Broecker, Ken Buessler, George Burr, John Chappell, Doug Donahue, Ellen Druffel, Hedy Edmonds, Luis Gonzalez, Lisa Grant, Paul Hearty, Ken Ludwig, Paul Mann, Dorothy Merritts, Brad Moran, Terry Quinn, Glen Shen, Mark Reagan, Kerry Sieh, Pete Smart, and John Wehmiller to name a few. Suffice it to say I was welcomed by many of the top scientists in this area of geology.

The development the thorium-230 measurements (Edwards et al., 1987) began a portion of my career that has been personally very exciting and rewarding. I think that Jerry Wasserburg described my demeanor at the time as that of a “kid in a candy store”. That characterization was not far from the truth, and I don’t think I have outgrown that stage yet. Although at the time I did not accuse Jerry of acting the same way, his excitement about the work was evident.

I moved on from Caltech to set up my own laboratory at the University of Minnesota. The University’s support was strong, and I particularly must thank Peter Hudleston and Bill Seyfried, the chairmen while I have been at Minnesota and Rama Murthy for helping maintain that strong support, as well as other colleagues at Minnesota with whom I have interacted extensively: Calvin Alexander, Subir Banerjee, Marc Hirschman, Emi Ito, Bob Johnson, Kerry Kelts, Christian Teyssier, and Herb Wright.

The research that has and continues to be most exciting to me centers on the combined use of thorium-230 and protactinium-231 as chronometers and tracers. The application of these tools has and will continue to impact many fields in the earth sciences. The combined use of thorium-230 and protactinium-231 to date carbonates is one of those rare instances where nature has provided us with an elegant set of checks (Cheng et al., 1998). Although not a perfect analogue, the closest is uranium-lead dating of zircons. Combined thorium-protactinium dating gives us the ability to test not only the precision, but the accuracy of ages, and in a time range (the last few hundred thousand years) that traditionally has been very difficult to access (Edwards et al., 1997).

The history of uranium-series work goes back to the turn of the century and Rutherford and Soddy’s determination of the radioactive decay equation in 1902 (Rutherford and Soddy, 1902), from the decay of what later was determined to be thorium-234. Many of the basic principles of uranium-series systematics were worked out early in this century and many first order problems that could be addressed with uranium-series measurements were addressed years ago by among others, some of the people in this room. However, ultimately the field became limited by the analytical capabilities of the time. The big hurdle then was technical in nature. The development of the mass spectrometric thorium methods goes back to my days at Caltech (Edwards et al., 1987). The first mass spectrometric protactinium measurements were done in the mid-90s by David Pickett with Mike Murrell and Ross Williams at Los Alamos (Pickett et al., 1994). My group then collaborated with the Los Alamos group to evaluate protactinium dating of carbonates (Edwards et al., 1997). As part of this collaboration, Hai Cheng developed protactinium measurement capabilities at Minnesota.

As a community, we have just begun to use high resolution thorium and protactinium dating to refine the key aspects of late Quaternary chronology. Some of the important chronologies that we have and will continue to pursue are records of Quaternary sea level (e.g., Edwards et al., 1997 and references therein); stable isotope records of regional vegetation and climate history from inorganic calcite deposits (e.g., Winograd et al., 1992; Dorale et al., 1998 and references therein), largely from caves; records of tectonism and earthquake history (e.g., Zachariasen et al., 1998); and chronologies of the latter stages of human evolution (e.g., Cheng et al., 1997). On another front, I believe that these dating techniques will ultimately result in the calibration of the remainder of the carbon-14 time scale, an ongoing project of the scientific community for the last 50 years (Bard et al., 1990; Edwards et al., 1993 and references therein). The approaches may also be used to recover past deep-sea ventilation ages (Adkins et al., 1998). As a whole, the ability to place a wide range of late Quaternary events on a common, high resolution, and absolute time scale will be of great importance in the earth sciences.

Beyond the use of protactinium and thorium for dating, the pair can and has been used a tracer of ocean scavenging and advection processes as well as igneous processes (e.g., Asmerom et al., 1999). In the latter case, we, as a community, are using these measurements to place truly new constraints on aspects of melting and melt migration, in a field that is arguably quite mature.

A rapidly growing number of research groups worldwide have been pursuing research in the areas that I have outlined. The field is vibrant. My colleagues outside of Minnesota have done most of the excellent work in this field. However, at this time, I would like to acknowledge some of my current and former doctoral and post-doctoral advisees and some long-term student visitors that have really been the backbone of the Minnesota contributions in these areas: Jess Adkins, Yemane Asmerom, Andy Baker, Warren Beck, Hai Cheng, Kirsten Cutler, Jeff Dorale, Christina Gallup, Sarah Gray, John Hoff, Jerry Magloughlin, Guangrong Min, David Richards, River Shen, Rebecca Thomas, and Judy Zachariasen. One of the truly rewarding aspects of my work has been my interaction with this group as well as my colleagues in the broader earth science community.

In closing, I would like to thank my family: my parents, Richard and Vee-Tsung Ling Edwards; sisters, Margo, Joan, and Edith Edwards; wife, Missy McDonald, and young daughters Louise and Eliza. From the oldest to the youngest they have always reminded me of the important aspects of life. And I would like to thank you all once again for this wonderful honor.

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