

SUBMARINES, QUARKS, AND RADIOISOTOPE DATING¹

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My path to the invention of accelerator mass spectrometry—now just called AMS—was quirky and extraordinary, and it is a saga worth telling, particularly for young people who may have an oversimplified image of how progress in science is actually made. It was an adventurous journey, and like many adventures, it was often uncomfortable, haphazard, and frequently characterized by a feeling of being lost. In retrospect, the only reason I set out on this journey was my belief that I didn't know much about finding my path in physics, and that the best I could do was to follow the lead of the great physicist Luis Alvarez. He had an incredible ability to create new and ingenious projects that led in new directions, and I wanted to understand how he did that. So I had decided that I would work with him on any new idea he came up with, even if it inconvenienced the rest of my life. And true to my expectations, while working with Luie (that's what he wanted everyone to call him), I often felt like Odysseus, tossed between distant shores by capricious gods.

The AMS saga began in 1974, shortly after Luie nominated me to join a group of academic scientists called *Jason*, whose members spent 2 months every summer working on United States national security problems. (The group had been created in 1960, when names from mythology were popular for government projects; Jason is not an acronym, but is named after the Greek hero.) Luie had been a Jason for many years. The members applied their abilities and knowledge in science to such questions as strategic arms limitations verification, vulnerability of United States missiles to surprise attack, and the security of our fleet of nuclear submarines.

While working in Jason, I became particularly interested in the security of the United States nuclear fleet, because our confidence in the ability of our submarines to elude detection allowed the United States to adopt a relatively sane and stable defense policy. As long as we believed that our submarines could survive a surprise attack, it was unnecessary to “launch on warning”; instead, we could absorb such a strike and respond in a measured way. The ability of our submarines to hide had been investigated many times before. But with the advance of technology, it was necessary to reevaluate this security frequently. With my background in elementary particle physics, it seemed appropriate for me to reexamine the question of whether a nuclear submarine could be detected from the minute radioactivity left in its wake. Also working on the problem was Will Happer, a professor of physics at Columbia. After a few weeks of study, we tentatively came to the surprising conclusion that there was a new technology that could threaten the security of our submarines.

The new technology was “laser resonance fluorescence.” Happer was an expert in the use of lasers, and he knew that intense lasers could be tuned to excite particular atoms and even particular isotopes. The excited levels decay with a characteristic radiation that allows the presence of the atom to be detected. Happer told me how laser resonance fluorescence had been used to detect single atoms of cesium vapor in the presence of large backgrounds of other atoms. Unlike radioactive decay, which allows detection of only those atoms that disintegrate during a finite counting period, the laser method could, in principle, detect each atom in the sample. Happer suggested that a variant

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of this technique could be used to detect the small number of radioactive atoms in the wake of a nuclear submarine.

After a great deal of calculation and many attempts at invention, we finally concluded that the number of radioactive atoms was too small and the ocean too big to allow for a practical implementation of the method. The radioisotopes induced in seawater by a passing nuclear submarine are too short-lived to give the laser method a substantial advantage over detection of the radioactive decays. For most physics projects, failure of a new idea would be a disappointment, but on this project failure meant that our submarines were still secure, and Happer and I were delighted. Nonetheless, I was sufficiently intrigued with the idea of detecting radioactive atoms with a laser that when I returned to Berkeley, I described it to Luie, who had been my thesis advisor and mentor.

In one of the great leaps for which he is famous, Luie immediately suggested an application in a totally different field: radiocarbon dating. He told me about a proposal by Michael Anbar, then at SRI International in Stanford, California, to attempt ^{14}C dating with a mass spectrometer. I had difficulty understanding it because I knew almost nothing about the subject, but in the next few days I read extensively about Willard Libby's invention of ^{14}C dating in the late 1950s and began to appreciate the potential of Anbar's scheme.

Libby had recognized that the atmosphere contains a nearly unchanging level of the radioactive isotope of carbon, ^{14}C . Cosmic rays constantly bombard the upper atmosphere, creating free neutrons. These neutrons are absorbed by nitrogen nuclei, which, after proton emission, become ^{14}C . This new ^{14}C replenishes that lost through radioactive decay with a half-life of 5700 yr; at equilibrium there is about one atom of ^{14}C for every 10^{12} atoms of stable carbon. All plants and animals have this level until they die and the ^{14}C in their cells is no longer being replenished. One gram of carbon from a living organism has 14 decays per minute of ^{14}C ; from the smaller fraction of ^{14}C in a dead sample one can deduce the "age" of a sample, the length of time since it went out of equilibrium with the atmosphere. Although the decay rate is low, the absolute number of ^{14}C atoms in a gram of carbon is huge: 6×10^{10} . Anbar planned to detect and count these atoms with a mass spectrometer. If he succeeded at counting the large number of atoms rather than the infrequent decays, he could greatly extend the sensitivity of ^{14}C dating. Much smaller samples could then be used and older ages could be measured.

Anbar's method failed, largely due to the inability of the mass spectrometer technique to suppress all sources of background that could simulate a ^{14}C atom. Luie suggested that Happer's scheme of laser resonance fluorescence might be used for single-atom detection of ^{14}C . I spent the next several months talking to experts, reading, and learning everything I could about laser fluorescence. I discovered that the problem was much more difficult than either of us had anticipated. This laser method also had problems with background, from pressure-broadened ^{13}C lines and from continuum emission of trace contaminants. I realized that if one is searching for a signal at the 10^{-12} level, one must consider every conceivable stable atom or isotope to be a potential background. (Our blood contains arsenic at 3000 times this level and gold at 10 times this level.) I finally concluded that the laser method would not work, at least not until it became much more highly developed. I temporarily forgot about ^{14}C and went back to my main basic research project at the time: I had instigated and was the principal investigator on an experiment to study the 3-degree Kelvin cosmic microwave background radiation.

Luie soon interrupted my peace with a new and brilliant idea. He had been thinking about quarks, the particles hypothesized by Gell-Mann and Zweig to make up the proton and neutron but which had never been seen as separate entities. The standard explanation for their absence was that they

were “confined” by forces that increased indefinitely as one tried to pull a quark from a nucleus. Luie considered this explanation unphysical and suggested instead that quarks hadn’t been found because experimenters had looked for the wrong signature. The charge of the quark was predicted to be a fraction (1/3 or 2/3) of the proton charge, and most experiments depended on this unique characteristic. If instead the quarks had integral charge, as first suggested by theorists Han and Nambu (1965), they might have been seen but mistaken for other singly charged particles. The recent introduction of the quantum number called “color” had made integral quarks an attractive alternative to fractional quarks, and Luie had devised a method to search for integral quarks with the incredible sensitivity of one part in 10^{18} .

Luie proposed that we use the Lawrence Berkeley Laboratory 88-inch cyclotron as a large mass spectrometer. He had used a cyclotron for a similar purpose once before, in 1939, in his discovery of the natural existence of ^3He and the radioactivity of ^3H (Alvarez and Cornog 1939). He used a cyclotron then because he had one (and didn’t have a mass spectrometer), but he had recognized its remarkable resolution even under conditions of high-beam current. This was just the property needed for an integral quark search, so for the first time in 35 yr a cyclotron would be again used in this mode. We would tune the cyclotron through various mass regions and look for a singly charged object with a mass different from that of known particles. If they are stable, quarks would have accumulated in air from cosmic-ray production and could be found in atmospheric hydrogen gas, which has the same chemistry as singly charged quarks. We would be able to identify individual quarks even in a background of 10^{11} hydrogen nuclei (the number we could accelerate in a reasonable counting period), for when the cyclotron is detuned from the hydrogen resonance frequency, no hydrogen is accelerated. Luie thought that we could complete the experiment in a few months.

By *we* I knew Luie meant *me*. He had recognized the importance of the measurement and figured out how to do it, and it would be my job to do the detailed experiment design and to make the measurements. I didn’t resent this division of labor; I was grateful that Luie thought highly enough of me that I was the one he chose for this collaboration. The conception of such a project is the difficult part; carrying it through is relatively straightforward, although time-consuming. Yet I would be a coauthor of the discovery papers, if any. It seemed like a very good deal. It would also be an opportunity to learn how to use the cyclotron. But I was worried that I would have to neglect my principal research project, the cosmic microwave background measurements, a project I had initiated, and for which I was the principal investigator. I guessed as best I could the probability of a major discovery in the quark search and somehow came up with the figure of 10%. I weighed this probability against the importance of such a discovery and decided to take the risk. I turned more and more of the effort of the cosmic microwave experiment over to my colleague George Smoot and graduate student Marc Gorenstein.

Several decades later, Smoot won the Nobel Prize in physics for his work on the 3K project I had initiated. But I never regretted my decision to hand over most of the work to him; he had to put up with a great deal of misery in getting the satellite into orbit, and it took full time for over a decade to do it; in the end he was the first to see the pattern in the Big Bang that led to his prize. But I feel that I was much better off continuing to learn the art from Luie of how to create totally new research directions. My decision to study his methods led me directly to the invention of AMS, as I’ll explain, and even more. Following his approach to science enabled me to create other innovative projects, including an automated supernova search program that resulted, eventually, in the discovery of dark energy. Of course, had I decided to stick to the 3K microwave project, I never would have known what I missed, and I suspect I would have decided that to have been one of my great life decisions!

Soon after we began the quark search, we read a paper by Zeldovich et al. (1966) that showed that stable heavy particles in the atmosphere should exist at levels much higher than the 10^{-11} level we had initially calculated. The primary source would not be cosmic rays but the cataclysmic explosion at the creation of the universe, the “Big Bang.” Quarks, if stable, would be found in ordinary matter at the 10^{-10} level as remnants of nuclear reactions during the explosion, just as the cosmic microwave background is a remnant of the tremendous heat released at that time. Measurement of the density of quarks would tell us about the temperature and density of the universe during the first second of its existence. Our project suddenly transformed from a study of elementary particles to one which also could be the most fundamental measurement in cosmology since the discovery of the background radiation a decade earlier.

As if research in fundamental particles and cosmology weren't enough, we soon realized that discovery of negatively charged quarks might provide a new source of energy. Such quarks could be absorbed on the nuclei of hydrogen atoms to form neutral particles that could fuse with protons without requiring the high temperature normally required to overcome the Coulomb repulsion. If the quark were ejected during the fusion, it could catalyze another fusion. Similar catalysis of fusion had been observed with negative mu-mesons, but the mu-meson was too readily captured by the fused nucleus to assure continued catalysis. With the heavier quarks, it might prove practical. I envisioned quark separation plants to distill the quarks from seawater and power plants which would mix the quarks with deuterium. The quark search was now much more than an academic discovery; it could change the world!

The search took 2 years. Soon after we began, we were joined by Edward Stephenson, who taught us about low-energy particle identification, and by William Holley, who taught us about the complexities and subtleties of modern cyclotron operations. We tuned the cyclotron continuously over the mass range 1/3 to 12 amu (the mass of a ^{12}C atom). Alas, we found no integrally charged quarks (Muller et al. 1977b). We got the sensitivity that Luie had originally estimated; we were able to demonstrate that if such particles do exist in nature, their abundance is less than one part in 10^{18} . (Recall that the predicted level from the Big Bang was one part in 10^{10} .) That means that they really don't exist. Despite the null result, I didn't regret my decision to become involved in the search. I had taken a calculated risk and had lost. One more paper would join the enormous literature of null results.

While we were making the search, Ed Stephenson told me about an interesting new idea he had been discussing with Arvand Jain, a visiting physicist at the 88-inch cyclotron. We had all read in the newspapers about the reported discovery of “super heavy elements” in rock samples. (Superheavy elements are elements higher in mass than those in the known periodic table, which are predicted to be stable or semistable by some nuclear shell structure theories.) Ed's new idea was to verify the existence of these particles using the cyclotron as a mass spectrometer, just as we were doing in the quark search.

It was an excellent idea, but I was deeply disturbed that I hadn't thought of it. I had been working with the cyclotron for over a year, and I too had heard about the superheavy report, but I hadn't taken the trouble to put the two together. I realized that I had been lazy and had become too narrowly focused on the details of one experiment. I had been using the cyclotron in a virtually unexploited mode and achieved a sensitivity far greater than most people knew was possible, but I hadn't even bothered to think about other potential uses. I had not been practicing the key idea that Luie had been teaching me: always keep thinking! Integrate, every week, all the new things you have learned, and look for new connections.

The idea of looking for superheavy elements eventually led nowhere, for the original report was mistaken and was eventually retracted, but the moment I heard from Stephenson about his idea for such a search I promised myself that I would take time to look for other possible applications.

Less than an hour later, as I was going home, I decided to *think*. Okay—what else could the cyclotron as mass spectrometer be used for? The first thing that popped into my mind was: what about the old idea of direct detection of ^{14}C ?

At that moment, I felt that strange excitement that comes when you think you may have just had a great idea, but aren't sure. I almost didn't want to think any more, because I know really great ideas are rare, and I would probably be able to show that this one would not work. I tried to work out the numbers in my head, and then realized that I needed to pay full attention to my driving.

When I got home, I put the key numbers down on paper. They seemed to work out. As much as I wanted to telephone Luie, I knew that I shouldn't. In a few hours, I would probably find the flaw, and I needed to find it myself, rather than have him point it out to me. I worked out the numbers once again, without looking at the previous ones. It seemed to check. But how much beam current could I get for carbon? I called Bill Holley and got the values (but didn't tell him what I was thinking about, not yet). There was plenty! I knew there would be lots of nitrogen background, so I looked very hard at the particle identification method. It would be swamped. But maybe I could use range separation. I worked out the details, and it seemed to be enough. The high energy of the beam allowed it to work in a way that would have proven impossible for a low-energy beam that Anbar had tried. The cyclotron as mass spectrometer would take care of all other backgrounds. It looked good. I started a notebook in which I summarized my calculations; the first page is shown in Figure 1. I realized the same method could be used for other radioisotopes, so I looked up a book of radioisotope dating to see what else might be practical. ^3H looked like the easiest; next best was ^{10}Be . Heavier ones also looked possible, including ^{37}Cl and ^{53}Mn .

I checked and rechecked my calculations, and 2 days later felt it was finally time to share it with Luie, to see if he could find a flaw that I had missed. Just as I was thinking this, he abruptly walked into my office. But I didn't immediately announce my invention; I felt as if I should follow a convention that he had used with me in the past. When he solved an important challenge, he would first tell the problem, and then he would give me a few moments to solve it myself. When I failed (always the case up to then), I would more deeply appreciate his solution. (This is analogous to the fact that you don't want to learn how a magic trick is done until after you are fooled; if you learn the trick first, you'll claim that you never would have fallen for it.)

So I told Luie, simply, that I had “solved” the problem of direct detection of ^{14}C . He knew the convention, so he paused and started thinking. I could not believe he would miss the method; he had invented the quark search, and we were still in the last stages of that. He had been coming to the cyclotron in the early hours of the morning when we got our “beam time” and had even been bringing his young son Donald to observe me and Ed Stephenson running the machine. So he was bound to put it all together and invent the method—either that, or maybe he would find a flaw.

Finally, after what seemed like a few minutes (but which I think was actually only a few seconds), he looked at me skeptically and asked for my solution. I said, “Use the cyclotron as a mass spectrometer, just as we did in the quark search.”

He paused again and I guessed that he was doing in his mind the same calculations that I had been doing for the last day, beam current (which he probably knew, and didn't have to ask Holley about),

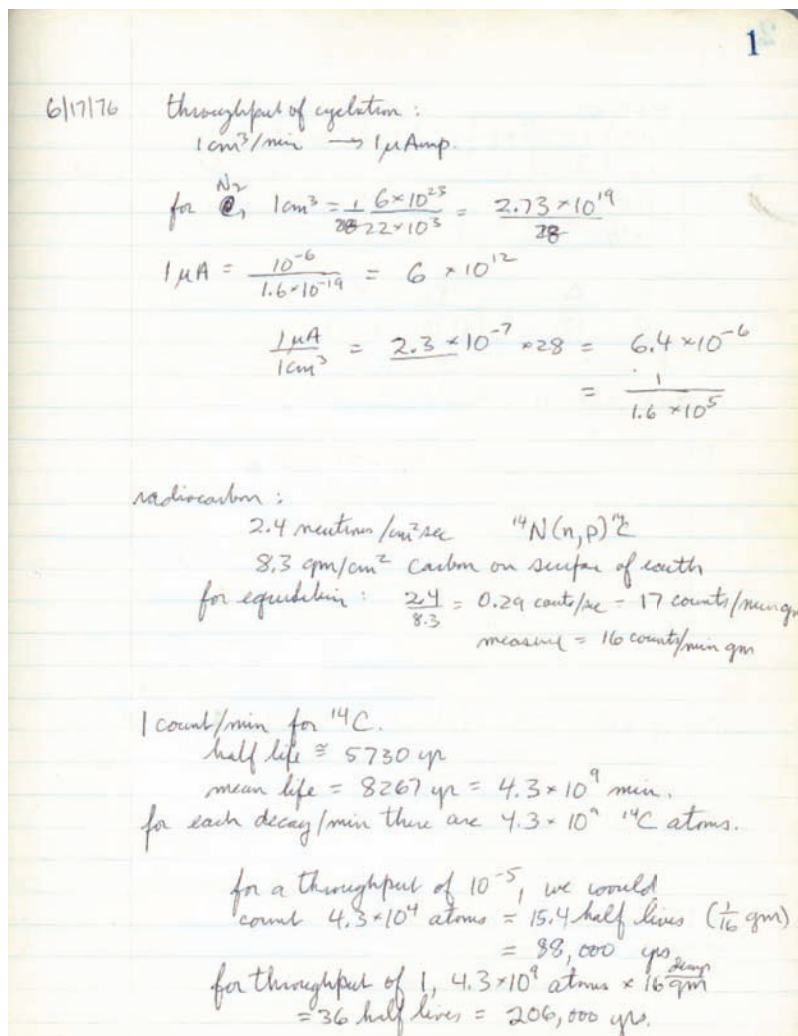


Figure 1 The author's notebook showing the first rough calculations for AMS, dated 17 June 1976.

and the abundance of ^{14}C . Then, I imagined he was thinking of the nitrogen background, and then inventing the range separation method—the natural way to do it, once you had cyclotron-like energies.

Suddenly he smiled, put out his hand to shake mine, and said simply, “Congratulations!”

I wrote an internal report on the method dated 4 July 1976 (Muller 1976a). (Yes, I was so excited about it that I was writing on Independence Day.) As soon as we obtained our first date (the age of a ^2H sample was obtained by measuring the radioisotope ^3H), I wrote the results as a Lawrence Berkeley Laboratory Report (Muller 1976b)—a more significant step since copies of such reports were sent to all major nuclear and particle laboratories. Such preprints were the standard quick way to disseminate ideas around the world. The report was later accepted for publication in *Science* (Muller 1977).

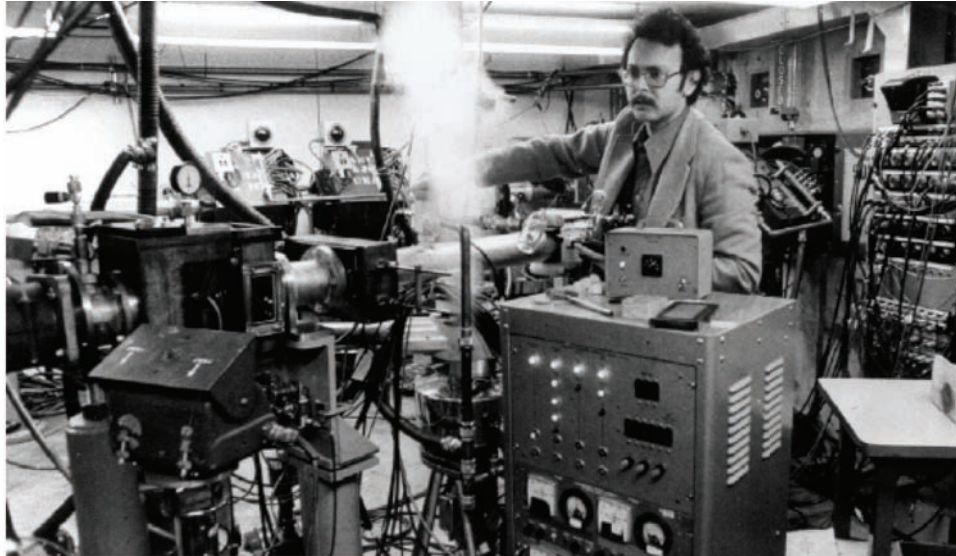


Figure 2 The author in 1976, the beam cave of the 88-inch cyclotron, refilling a liquid-nitrogen dewar for the particle identifier.

It was time for the Jason summer study, so I went down to La Jolla, California, where it was taking place. Far from this being a distraction, it was an opportunity to present the idea to some of the best scientists in the country, including Richard Garwin, Will Happer, Walter Munk, and Freeman Dyson. In fact, Dyson alerted me to a possible problem I hadn't anticipated: if I were to use a gold foil to stop the nitrogen, it might undergo a nuclear reaction with the gold. In a letter to me dated July 7, he said,

This C-14 thing is nice. But I think you will have trouble with the reaction $N-16 + Au-197 \rightarrow C-14 + Hg-197 - 0.58 \text{ MeV}$. This can go by charge-exchange ... There is a simple cure for this disease. Use lead.

On 22 July 1976, I presented a colloquium at the Lawrence Berkeley Laboratory. Sitting in the audience was Grant Raisbeck. Raisbeck et al. (1978b:43) later wrote:

The realization that counting long lived radioactive atoms can be much more sensitive than counting their decays is, of course, far from new. (See, for example, Figure 10 of H. Hintenberger, *Ann. Rev. Nucl. Sci.* 12, 435 (1962).) We nevertheless acknowledge that our own interest in the idea of accelerating such atoms to a high enough energy to permit unique isotope identification was stimulated by the attendance of one of the authors (GMR) at a seminar given by R. A. Muller at Berkeley in Sept., 1976.

Luie sent my memo to his old friend Willard Libby, who had won the Nobel Prize for his invention of the original ^{14}C method. Libby invited me to come to Los Angeles, where I discussed the method with him at length. I asked him to write a letter to Andrew Sessler, the director of the Lawrence Berkeley Laboratory, to support my request for more funding. Libby's letter read:

I write on behalf of Dr. Richard A. Muller who visited our Radiocarbon lab here at UCLA yesterday [Sept 23, 1976].

His plan to use the 88" cyclotron for Radiocarbon Dating is the most exciting idea in the whole field of radioactive dating I have heard in many years. If it works it will double the span of time we can cover with radiocarbon and allow the use of much smaller samples.

We are most anxious to collaborate with him and Professor Alvarez in this research. Professor Berger and Leona Marshall Libby and I discussed his plans with him in depth yesterday and we are convinced that he has a very good chance.

Needless to say, a letter like that from Libby thrilled me, and still does. I deeply cherish it. Unfortunately, Libby died 3 years later—but not until he had the pleasure of seeing the success of AMS in improving ^{14}C sensitivity by a factor of 1000. The new method greatly enhanced the range of scientific problems that ^{14}C dating could address.

Luie told his son Walter Alvarez about my ideas, and Walter invited me to give a colloquium at the Columbia University Lamont-Doherty Geologic Observatory. There I met Wally Broecker, who confessed to me (a few decades later) that he was so convinced that my method would not work that he made a \$10 bet with a colleague that it would fail—a bet he paid off a few years later.

I also visited Jim Arnold at the University of California at San Diego. Arnold tried to convince me that I should put all of my effort into the detection of ^{10}Be , an isotope he considered far more interesting (at least for cosmochemistry, a field he had invented). In fact, the next radioisotope that we measured at Berkeley was ^{10}Be (we were slower with ^{14}C). We announced our success (at a meeting of the American Chemical Society) just about the same time that Raisbeck and Yiou were succeeding with ^{10}Be in France (Raisbeck et al. 1978a).

It felt like a whirlwind of excitement. When my *Science* paper was published, it was read by Erle Nelson, who later wrote about it (Nelson et al. 1978:47–48):

The paper published a year ago by Dr. Richard Muller in *Science* provided us with a clue for the construction of exactly such a triple-collector mass-spectrometer for radio-carbon dating. ... Dr. Muller's suggested solution to the problem of detecting ^{14}C at natural concentration was ingenious. ... Dr. Muller suggested the use of a cyclotron for this purpose. However, we felt that a tandem Van de Graaff was a much better choice ... It has a negative ion source which almost completely eliminates the ^{14}N ions ... This advantage was pointed out to us by Dr. Gordon Brown.

Even though the cyclotron had the advantage of eliminating everything except nitrogen, by its tuning condition, I had been so impressed with the particle identification technique that I knew that other kinds of high-energy accelerators could also be used. That's why I had specifically mentioned the possibility of using a linear accelerator in my *Science* paper. (Note from the Raisbeck quote that he too stated it was the high energy that allowed particle identification to count the ^{14}C .) But I had not appreciated the value of negative ions in eliminating nitrogen contamination. And even Luis Alvarez, who had *invented* the tandem Van de Graaff, hadn't heard of the recent discovery of the instability of negative nitrogen ions. Thanks to such nitrogen suppression, Nelson and collaborators were among the first to detect ^{14}C (Nelson et al. 1977). Cyclotrons were the first, however, to successfully detect ^3H and ^{10}Be .

The highest compliment that a scientist can receive is praise from his scientific heroes. One of my proudest moments came when Luie showed me part of a letter that he had written about my work, nominating me for a prize, in which he said:

Dr. Muller solved a problem which is two decades old (increased sensitivity for radioisotope dating) with a technique which is three decades old. I am particularly appreciative of Dr. Muller's work because I was aware of all the necessary ingredients including the importance of the problem, yet I failed to bring them together.

Yet another group also began accelerator mass spectrometry, about 9 months after our first successful demonstration of the method. It was a joint collaboration including Ken Purser (General Ionex Corporation), Harry Gove (University of Rochester), and Ted Litherland (University of Toronto). According to Gove, this group discussed the subject among themselves for the first time at the very American Physical Society meeting (April 1977) at which we had just presented a description of our measurements with ^3H and our attempts with ^{14}C . Gove told me that their team was unaware of our talk, and had missed it. Moreover, he assured me that they all had failed to see my September preprint (although it had been sent to his laboratory library), or my *Science* paper that had appeared in the mail a week before that meeting. Nor had they heard anything of my colloquia at Columbia, UCLA, UC San Diego, or Berkeley; nor were they familiar with the earlier unpublished report that I had circulated among the ^{14}C and nuclear physics communities, and which had been so highly praised by Willard Libby. In fact, unlike Raisbeck and unlike Nelson, Gove felt he had invented the idea totally independently, a point he was to make many times in his presentations (although the first papers published by the collaboration did give reference to my *Science* paper) (Bennett et al. 1977).

The Rochester collaboration realized, as had Nelson, that the tandem accelerator offered a particular advantage over the cyclotron for ^{14}C detection, because at the main background expected, ^{14}N does not form negative ions and hence is not accelerated. The high sensitivity achieved in the first experiments of this collaboration, along with those of Nelson et al., were particularly potent in convincing scientists around the world that the method had promise.

Although I had to drop other important projects to follow the quark project of Luie Alvarez, I always felt the decision was worth it. And although I may have missed the Nobel Prize in physics because I left the 3K microwave measurements in the hands of George Smoot, I was amply rewarded. In 1977, the AMS method was cited as a key reason for getting the Texas Instruments Founders Prize, and similarly the next year when I won the Alan T Waterman Award of the National Science Foundation. Based largely on the invention of AMS, John Reynolds (famous for his contributions to potassium-argon dating) nominated me for a faculty position at Berkeley; he succeeded and I became an Associate Professor. In 1982, I was given a MacArthur Prize. And now I get to give this honorary lecture!

Since so many others were pursuing high-energy AMS, I decided to try yet another new approach. I wanted to see if I could use a combination of negative ions and the cyclotron resonance to be able to detect natural ^{14}C , without having to go to high energies. We built a small, virtually table-top, negative-ion cyclotron, and did indeed manage ^{14}C detection (Bertsche et al. 1990). I called this device a “cyclotrino,” and two of my students earned PhD degrees for the work they did developing it: Jim Welch and Kirk Bertsche. My hope was that this new approach would allow every scientist to have his own machine, but even after we got it to work, there seemed to be little interest. By that time, there were major laboratories around the world that would do ^{14}C AMS measurements for a small fee for any scientist who needed a date. So, just as in nuclear physics where large central laboratories replaced table-top experiments, the cyclotron proved to be no competition for those running the larger machines and offering services to those who needed them.

I have learned several lessons from this adventure. The clearest is the interrelation of apparently different fields of science. I have had a great deal of fun talking to scientists in unfamiliar fields about the most important problems they were pursuing. Many apparently wild jumps in research are really the revival of old ideas (integral quarks, fusion catalysis, direct detection of ^{14}C atoms) not forgotten and given new life by developments in theory or technology. Yet it is easy to miss the interconnec-

tions simply by not bothering to look for them. I searched for quarks with the cyclotron for over a year before I made any effort to think about other applications, and I did force myself to think only when shamed into doing so by a colleague who had an idea that I thought I should have had. The surprising lesson is how easy it is to be lazy. I feel certain that there is more to be learned from this story, but I am not really sure what it is. What is the optimal strategy for productive research? How much time should be spent thinking, exploring new ideas, and how much should be spent concentrating on one project? How can one estimate the risk of a jump to a new area of research? In retrospect, I believe that I came close to having nothing of importance to show for several years of effort. I worried at the time that I had become involved in too many projects, a worry encouraged by some of my colleagues who tried to be helpful by advising me not to spread my efforts too thin. During the period of this story, in addition to the 3K project I discussed, I was also the principal investigator on a project in adaptive optics for astronomy, a participant in the American Physical Society study on nuclear reactor safety, and lecturer for an upper division course in the Physics Department at Berkeley (before I was appointed to a regular faculty position). My spare time was spent trying to get financial support for my research. Was I a dilettante or an interdisciplinary scientist? The difference is substantial but subtle. Was I chasing chimeras? An element of self-doubt and uncertainty may have helped keep me on the track. Most important of all was the model and encouragement of Luie Alvarez, who never seemed to think for a moment that I was wasting my time or doing anything wrong. Perhaps this is why Luie has caught more chimeras than anyone else I know.

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