An Adventure in Science

Richard A. Muller

published in the New York Times Magazine, 24 March 1985, p. 34.
Condensed version printed in Reader's Digest, October 1985.

I loved dinosaurs. I drew a tyrannosaurus rex fighting a Triceratops nearly every day during recess. I tried to make my sketch look just like the painting in Life magazine. My class took a trip to the American Museum of Natural History, and I saw a Tyrannosaurus skeleton fighting a Triceratops skeleton. My favorite movie was "One Million B.C.," in which cavemen fought a giant dinosaur (which actually looked more like a tired Gila monster). I wanted to learn everything I could about dinosaurs.

My parents took me back to the museum, and I sketched the outlines of the dinosaur skeletons, carefully writing down the names of each of the species. The school library had only one book on the subject, called "The Dinosaur Hunters." I learned that the dinosaurs had all disappeared 65 million years ago, long before humans had appeared. (The movie had lied a little. People had never fought dinosaurs.) In fact, the mammals never really got a foothold until the dinosaurs were destroyed.

But nobody really knew why they disappeared. Perhaps clever little mammals with a taste for dinosaur eggs had been responsible. It was the first problem I ever heard about in science that was admittedly unsolved.

I made a pipe-cleaner model of a Tyrannosaurus rex fighting a Triceratops for fourth-grade science fair and traveled all the way from the Bronx to Brooklyn to enter. I didn't even receive honorable mention. It was my first failure in a whole series of failures in New York City science fairs. From "The Dinosaur Hunters," I learned that real dinosaur hunting was not much of an adventure. The book showed paleontologists digging up rocks, carefully scraping away the mineral around the fossilized bone, taking weeks to remove a single specimen. I told my parents that I wanted to be a paleontologist. (Decades later my mother told me her reaction: "I can't even pronounce it.") But deep down I didn't believe it. Real paleontology looked terribly boring.

My most valued possessions were a telescope and a microscope. The world I saw through these seemed particularly beautiful, crystal clear and sharp. From that point on, I associated science with beauty. I didn't realize until recently that the reason everything looked so sharp through my instruments was that I was becoming slightly nearsighted. Focusing a telescope and a microscope automatically compensates for myopia, and it was only through them that I had true 20/20 vision. Eventually, my nearsightedness was discovered and corrected with glasses, but I never lost the feeling of magic and beauty that I associated with optics. I became interested in astronomy because of my love of telescopes, not the other way around.

I saw a copy of the book "Biography of the Earth," by George Gammon, on a rack of pocket books at a drugstore. It had reproductions of dinosaur paintings as well as pictures of the moon and planets, and only cost 35 cents. I convinced my parents to buy it for me. Later I bought "One Two Three ... Infinity," also by Gamov. These books were full of excitement: discussions of infinity, photographs of molecules, theories about the beginning of time and the size of the universe, about continental drift (this in 1941!). It was physicists who did most of this work, and that was what I wanted to be.

In high school, even though I found biology more interesting than physics, I still knew I wanted to be a physicist. From Gamov's books I knew that real physicists didn't spend all their time with pulleys and inclined planes. They tried to solve the riddles of the origin of the universe and the nature of the atom.

In graduate school at the University of California at Berkeley, I chose for my thesis elementary particle physics, the study of the pieces that make up the nucleus of the atom. It was the field that everybody at the time found most exciting, and the Lawrence Berkeley Laboratory near campus seemed to be the center of the physics world. I appeared to have moved as far from the study of dinosaurs as one could imagine. I never would have guessed that my thesis adviser, Luis W. Alvarez, would lead the team that discovered the immediate cause of the destruction of the dinosaurs, and that I would be led from this to a search for the ultimate cause: a "death star" that orbits the sun.

At the laboratory, Alvarez had a project that used cosmic rays to study the properties of subnuclear matter. He hung a superconducting magnet from a balloon flown at 100,000 feet of altitude in order to reach the cosmic rays before they were attenuated by the atmosphere. Alvarez seemed to be one of the few physicists around developing new methods to investigate new areas of science, rather than using old techniques to fill in missing details. Working with him proved to be constantly exciting. I had found someone to teach me what I really wanted to learn. After six years of formal physics training, my education was finally beginning. I found myself studying Alvarez, trying to understand how he approaches problems.

Our conversations were just as likely to be on topics of archeology or history as on my physics research. He often explained to me that he felt more like an explorer than a scholar; had he been born in the 1500's, he would have been searching for new continents rather than for new laws of physics. He had been inspired by the accounts of Captain Cook exploring the Sandwich Islands, and of Sir Richard Burton, disguised as a Moslem, risking his life to visit the Kaaba at Mecca. But the earth was now well explored, and the "frontier of space" was overrated. (Traveling in a computer-controlled spaceship like a monkey had none of the adventure of true exploration.) The great unknowns of this century were in science. To be an explorer today is to be a scientist.

People are often told that scientists are motivated by curiosity. I don't believe it. The truly curious can satisfy their needs by reading books. Basic research is extremely slow and inefficient; it may take years to uncover a single new fact. It is a poor way to satisfy curiosity. There are scientists who are truly curious, who become serious scholars of science and learn much of what there is to know about science. But these scholars are not usually the ones who make discoveries, for they are too busy satisfying their curiosities. Alvarez has made more discoveries than any living physicist I know, and I could sense he did it for the excitement, the adventure. He was not a scholar; I was frequently amazed at the facts of physics he did not know. He didn't clutter his mind needlessly, but he had an extensive knowledge of unsolved problems. When he needed to know some area of physics to solve such a problem, he learned it with amazing concentration and speed. People said that Alvarez had a "killer instinct." When he found a problem that was worthy of attack, he behaved like a shark sensing blood. He bit into it and kept hold until he was satisfied.

My own future research would be modeled on the way I saw him attack his "Pyramid project," the first experiment I watched him do from beginning to end. He realized that the Egyptian Pyramids are constantly bombarded by natural cosmic radiation from space. With a suitable detector (film was too insensitive), he could use this radiation to "X-ray" one of the Pyramids of Giza, and perhaps find a hidden chamber. I guessed that the mysteries of the Pyramids were problems Alvarez had read about as a child, just as I had read about the dinosaurs. (I once heard a definition of "success" as "a dream of childhood come true.") If Alvarez were to discover a new chamber full of treasures, just as Howard Carter had found Tutankhamen's chamber during Alvarez's boyhood, it could be the greatest achievement of his life; greater than the discovery of all the proliferation of elementary particles that led to his Nobel Prize in 1968.

I noticed that he did his "experimental physics" from a desk, thinking the problem through with great thoroughness before assembling any equipment for a measurement. He found others interested in the problem and solicited their help. The political problems were by far the most difficult for him. He had to convince an American archeologist that the Pyramids would not be damaged by the cosmic radiation. (It was easier to convince him of this than to explain that the radiation came from natural processes and passes through the Pyramids whether Alvarez set up his detectors or no.) This whole procedure of organization was his greatest lesson to me.

Alvarez successfully X-rayed the Pyramid, just as he said he could. But there were no hidden chambers. Some newspapers reported that he hadn't found any chambers. Alvarez always corrected them: He had found that there were no unknown chambers. Still it was a terrible disappointment. The experiment itself had succeeded--there were glorious X-ray photos that showed the structure of the Pyramid in detail, including the four walls and the cap. But there were no hidden chambers. The childhood dream had not come true. That is the nature of exploration. There may be nothing there.

One day Alvarez showed me a present he had received from his son Walter, who was then a geologist at Columbia University. The present was a piece of rock 65 million years old from a site in Italy. The rock had been built up from sediment in the ocean, and it had trapped in it many small fossils from sea creatures that lived at that time. But the fossils could be found in only half the rock; something had killed virtually all the microscopic life before the second half of the rock was laid down. Walter had told his father that this catastrophe struck the microscopic creatures at the same time as it struck the dinosaurs. Paleontologists had determined that more than half of all species alive at that time, large and small, had vanished. (The theory that mammals had killed the dinosaurs by eating their eggs could not account for the widespread disaster and had never been taken seriously by experts.)

The senior Alvarez was fascinated, and he thought that a new technique of radioactive dating that I had invented could be used to study this rock and perhaps untangle the mystery of what had killed all these creatures. I visited Walter, we decided to collaborate and for about a month I thought I could make the critical measurement that was needed. By using the isotope beryllium 10 to determine the rate of sedimentation, we would try to determine whether the catastrophe had been swift or drawn out. But the idea failed. The data we had on the lifetime of beryllium 10 was based on a misprint published many years earlier and never noticed. When we used the correct value of beryllium 10, we found there would be no beryllium 10 left in the sample. The measurement was impossible.

Alvarez had taught me that most new ideas fail. One simply had to keep trying. One out of 10 ideas might be worth actually trying, and out of these, one out of 10 might lead to an important discovery. You need to have 100 ideas to have a chance at a real discovery. The important thing, the tough thing, is to keep on trying, to keep on having new ideas.

My failure seemed to sharpen Luis Alvarez's interest. The full power of nuclear physics had never been applied to the study of the catastrophe, and he was sure that there must be some method to measure the required sedimentation rate. I could tell from his absence that he was onto something; the only times he disappeared were when he was deep into a problem. Sometimes I stumbled upon him in the library reading a paper published 30 years ago. More often, he would suddenly appear in my office with a new idea that he was dying to tell someone about.

He searched for months until he found a solution he was certain would work. The key was a rare element, iridium, which is constantly raining down on the earth in minute quantities from micrometeorites vaporizing in the atmosphere. Iridium is extremely rare in the earth's crust. Most of the primordial iridium sank to the earth's core along with iron. Alvarez calculated that a measurable amount of iridium should have been trapped in sedimentary rock from the micrometeorites. He proposed that neutron activation analysis could be used to detect this iridium, and from it, he could deduce the abruptness of the catastrophe. I was a little disappointed to realize that none of my talents was applicable. Instead, Alvarez solicited the help of Frank Asaro and Helen V. Michel of the Lawrence Berkeley Laboratory, both experts in neutron activation analysis, and the search was on.

They soon found the iridium, but there was a surprise. The ratio of iridium to clay changed in the layer of the rock that had been formed right at the time of the disaster. This seemed to me a minor point; I'm sure that most scientists wouldn't have been bothered by it. But Alvarez and his team refused to ignore it. Most likely, someone was making a silly mistake, but it had to be checked out. Alvarez had once missed making the discovery of artificial nuclear fission because he had failed to follow up on a seemingly unimportant observation. And many of his most important discoveries, such as the radioactivity of tritium, had come from his pursuit of such stray observations.

The iridium-to-clay ratio proved difficult to explain away. The shark sensed blood. When there is something difficult to understand, it means your paradigm may be wrong. Perhaps the iridium came from volcanoes, not from micrometeorites. No, that could be ruled out. Perhaps it came from a chemical precipitation in the oceans. Alvarez found that implausible; eventually similar finds in a lake bed in New Mexico ruled that theory out. Alvarez seemed to come up with one new explanation per week. He patiently explained the new theories to me and my colleagues, and by the time we had understood all the details (usually a week or so later), he had disproved his own hypothesis and come up with an alternative. There was no way I could keep up with him in this fascinating job of disproving his latest model. I think I also felt that he was wasting his time; if and when he found an explanation that stood up under all criticism, I thought it would probably be mundane and uninteresting.

Alvarez finally found an explanation that stuck, and it was hardly mundane. A nearby star, he postulated, had exploded as a supernova, creating a rain of newly manufactured elements including iridium; there were many side effects of this event that could have caused the extinction of life. But this was not a good theory unless it made a prediction that was testable, and Alvarez found one: The isotope plutonium 244 should also be found in the layer of rock that was formed at the time of the catastrophe. Plutonium 244 is radioactive and has virtually disappeared from the earth's crust. But it would have been newly created in a supernova explosion, and so it should be found with the iridium.

A few months later, Alvarez came to my office with something fascinating to tell me, but first he pledged me to secrecy. Frank and Helen had found the plutonium. The theory was proved! I carefully kept my promise not to tell anyone, although I made the usual exception and told my wife, Rosemary. There were fewer than a dozen people in the world who knew the startling fact that an exploding star had killed the dinosaurs. It was very exciting. Unfortunately, it was wrong. A second measurement by Frank and Helen showed no plutonium; the first sample had been accidentally contaminated. I began to understand why great "discoveries" should be kept secret until they are confirmed.

But Alvarez still didn't give up, and within a month he had found a new theory that fit all the facts: An asteroid had struck the earth. He could estimate the size of the asteroid in three independent ways, and they all agreed: about 10 kilometers in diameter. Dust thrown up by the impact could account for the loss of life, since sunlight would have been blocked for several months. None of us could find anything wrong with his reasoning. And the theory made many predictions that were verified over the subsequent years--for instance, that the chemical composition of the clay would be similar in samples taken all over the world. The theory was published in 1979. At first it was met with skepticism, particularly by scientists who didn't realize how the new theory was firmly based on the iridium discovery. But by the mid-1980's, there were virtually no experts in the world who still disputed Alvarez's contention that an asteroid had hit the earth at the same time as the mass extinctions.

I had watched all this work happen but had made no essential contribution of my own. It was a fascinating lesson. I could follow Alvarez's work, but he was almost moving too fast for me to keep up. Then an unexpected opening for me came along. Alvarez received a paper in the mail from David M. Raup and J. John Sepkoski, Jr., paleontologists at the University of Chicago. They had made a careful compilation of extinctions and found that large catastrophes among sea creatures were not rare events but occurred on a regular time schedule: every 26 million years. We are roughly half-way between periodic extinctions now, the paper went on. The next one isn't due for another 13 million years.

Alvarez didn't believe their analysis, and he composed a letter to Raup and Sepkoski outlining his objections. He asked me to play the role of devil's advocate, to read his letter and look for flaws in it. So I was cast in the role of defender of the periodic extinction hypothesis. I read the Raup and Sepkoski paper carefully, and I found it difficult to dismiss. I argued forcefully that Alvarez's criticisms were wrong; I didn't convince Alvarez, but I convinced myself.

It's a strangely uncomfortable feeling to believe something true when most of your colleagues don't. I felt that the periodic extinctions were real, but my arguments didn't win any converts. The obvious conclusion was that I must have been fooling myself. It was very tempting to forget the whole thing, and go back to my usual astrophysics research. But there was no ready explanation for the periodic extinctions. Even Raup and Sepkoski didn't offer anything plausible. Either the data were wrong, or our paradigm was wrong. It was tempting to dismiss the data, the analysis of Raup and Sepkoski. But I couldn't find any good justification for doing that; although I couldn't convince anybody else, I could not in good faith convince myself that there was nothing there. And I realized that I was in a spot similar to the one Alvarez was in a few years earlier when he puzzled over the iridium-to-clay ratio. There might be something there, but there might not be.

I could be wasting my time. To mimic Alvarez's approach meant essentially to abandon all other projects and attack this new one with all my energy. I had no research funds for this kind of work. I had commitments to fulfill, other projects to pursue. This work was unlikely to lead to any publication, the standard measure of fruitful academic work. All of the pressures of normal academic life pushed me to abandon the search. All of the pressures except one: Alvarez. Although he thought I was wrong, he, too, recognized the situation. And he knew that when you have such a problem, and the arguments of others don't convince you that your analysis is wrong, then you must stick with it. So every day he asked me how my work was coming. Could I fit everything together? Did I have a theory that could explain the periodic extinctions (that he didn't believe in)?

Within two months, I found six potential theories to explain the periodic extinctions, but I managed to prove each of them wrong. The only consolation was that I managed to do so before any of my colleagues could, even before Luis Alvarez could. I had one advantage over them: I believed that Raup and Sepkoski were right. That gave me a level of motivation that Alvarez and the others couldn't match. I sought other experts to help me.

I solicited the help of a real astronomer, Professor Marc Davis of the University of California (I had never even taken a course in astronomy). I very consciously mimicked the method I had watched so closely when Alvarez was studying the iridium problem. The hardest thing to mimic was his concentration, the belief that all this work was worthwhile. In fact, I almost gave up after a month of intense effort, when I was able to disprove my most clever theory. k I had come up with a model for a solar companion star orbiting in a pretty pattern, a "tulip orbit," that was so ingenious that I was really proud of myself. The eccentricity of this orbit varied with a 26 million to 30 million year period as galactic tidal forces caused the major axis to oscillate in and out of the galactic plane. But in the end the numbers didn't work out. I couldn't get the period short enough for any stable orbit. It was clever, but it was wrong. I felt I could never again equal my brilliance in creating that model; I might as well give up. But the answer turned out to be in a less clever theory.

The breakthrough came just before Christmas, 1983, when Marc Davis called and said that Piet Hut, then with the Institute for Advanced Study in Princeton, was in Berkeley. Piet was an expert in orbit dynamics, just the area where Marc and I had the most difficulty. I called Piet, and we arranged to get together. l The next morning, we met in Marc's office on campus. Since it was the Christmas break, there was no heat, and Marc made us both expressions in his pot. I showed Piet and Marc all of my latest models, and explained why each one failed. And then Piet suggested a modification to one of my theories that removed the objection I had had to it. I had imagined that a solar companion star came within the asteroid belt every 26 million to 30 million years, sending a shower of asteroids toward the earth. But the model didn't work; I could show that the orbit was unstable and the star would never return a second time. Piet liked the basic idea, but suggested that I also consider the effect of the companion star on comets; the required orbit would not be very eccentric, and it could be stable.

The moment that Piet made that suggestion is the closest I ever came to the proverbial "Eureka!" I could see immediately that most of the problems of my solar companion theory would vanish with this seemingly minor change from asteroids to comets. An hour later we had checked the numbers. The theory worked. It took us a week to make sure that the theory violated no other established facts of physics, geology or astronomy. We had to make sure that it was plausible for a companion star to the sun to have eluded detection by astronomers. We found that relatively few stars have had their distance measured, but that of the stars already catalogued, there were at least several thousand candidates, any one of which could be orbiting the sun. We mailed off our paper to the British magazine Nature for publication.

The theory was simple: The sun has a companion star that orbits in a large, moderately eccentric orbit. Every 26 million to 30 million years, the star comes relatively close to the comets that inhabit the outer reaches of the solar system. It was simple to show that their orbits would be perturbed, and a storm of more than a billion comets would enter the inner solar system. A few would likely hit the earth. (Alvarez had always maintained that it could have been a comet rather than an asteroid that struck.) From that point, the now "classical" Alvarez scenario was unchanged.

As we were writing the paper, I realized I had a rare opportunity. If this companion star does exist, then we could suggest a name for it. Murray Gell-Mann had suggested the name "quarks" for subnuclear particles when he published his theory that they existed, so why couldn't we suggest a name for an unfound, but theoretically predicted, star? I somewhat playfully added a footnote to the paper, suggesting that, if the star is found, it might be given one of the following names:

Nemesis, after the Greek goddess who relentlessly persecutes the excessively rich, proud, and powerful (e.g., the dinosaurs), or

Kali, the "black," after the Hindu goddess who is the destroyer of men and animals, yet who is infinitely generous and kind to those she loves (e.g., the mammals), or

Indra, the Vedic god of storms and war, who uses a thunderbolt (comet?) to slay a serpent (dinosaur?), thereby releasing life-giving waters from the mountains, or

George, after the saint who slew the dragon.

Finally, I added the sentence: "We worry that if the companion is not found, this paper will be our nemesis." I hoped the tongue-in-cheek humor of the footnote would prevent anyone from getting angry over the thought of naming something which had not yet been found. To my surprise and delight, my co-authors, Marc Davis and Piet Hut, did not veto the footnote; they liked it. Somewhat to my annoyance (and without my permission), an editor of Nature edited out all of the suggested names except the first. The theory soon became known as the "Nemesis hypothesis."

I was very excited by our theory, and yet still uncomfortable. Few of my colleagues found the theory exciting. In retrospect, I realize now that most of my colleagues didn't appreciate how difficult it had been to find any theory that worked, so our companion star model sounded like just one more speculation in a field that already suffered from too much speculation. Luis Alvarez, of course, played devil's advocate. It was the role I had played for him, and the role I wanted him to play. But that didn't make it pleasant. He was constantly trying to poke holes in everything I said, and he was extremely clever in doing so. Although I was ultimately able to answer all of his objections, it wasn't always easy.

One person who took the Nemesis hypothesis seriously was Alvarez's son Walter, who realized that there might be an immediately testable consequence of the new model. If comets hit the earth in storms, then there should be evidence of this in known impact craters on the earth. He and I studied the compiled data on such craters and discovered, to our delight (and amazement), that the large craters had been formed on a schedule indistinguishable from that of Raup and Sepkoski's mass extinctions. It was the first "verification" of the model. And it was sufficiently impressive that Luis Alvarez decided to quit his role of devil's advocate and endorsed the model as one that he believes true.

A year has passed since we submitted the paper, and our model has held up very well to examination by outside experts. In fact, a very similar model was proposed independently by Daniel P. Whitmire of the University of Southwestern Louisiana and Albert A. Jackson of the Computer Sciences Corporation in Houston and was also published in Nature. Alternative theories were proposed by other scientists, but most of them were theories I had already considered in my early searches, and I convinced myself that they all could be proved wrong.

If our theory is right, the consequences for evolution are staggering. Classical Darwinian evolution has species competing against species, but now we hypothesize that this is the case only during the relatively benign periods between comet storms. Every 26 million to 30 million years, the earth is subjected to a trauma that the otherwise successful species can't anticipate or prepare for. New ecological niches are opened, allowing previously suppressed species to gain a foothold. Had it not been for the large comet that hit 65 million years ago, mammals might never have wrested the earth from the dinosaurs. At the time they vanished, the dinosaurs were more intelligent than the mammals, and they might have stayed ahead. Highly intelligent creatures might still have evolved, but with very reptilian features.

It is strange and wonderful suddenly to be thinking seriously about dinosaurs for the first time since elementary school, to be on the trail of the first unsolved scientific question I had ever known. I recently drew a picture for my 6-year-old daughter of a Tyrannosaurus rex fighting a Triceratops. As the picture flowed out of my hand, I realized that my skill had not diminished, or progressed, one bit. It was as if I was sensing the fact that I was still the same person as that child I only dimly remember. I had not become a paleontologist after all, but I doubt that if I had, I could have made such a potentially major contribution to the mysteries of the dinosaur extinctions.

Of course the Nemesis hypothesis has not yet been proved correct. l The best evidence for the theory so far is simply the lack of other viable hypotheses consistent with everything we know about nature. If the sun does have a companion star, we should be able to find it. We now have a team of physicists and astronomers at Berkeley mounting an effort to find this star, and we have a good chance of finding it in the near future.[k] It is probably a red star, about 10th magnitude (a factor of 100 too dim to be seen without a telescope), and about three light-years away.[l] There are 5,000 candidate stars we are now examining. If and when we do find it, nearly every observatory in the world will be able to confirm that it is orbiting the sun and comes close every 26 million to 30 million years. There will be little room left for controversy. This latest adventure is just under way.