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The Gentle Art of Scientific Trespassing

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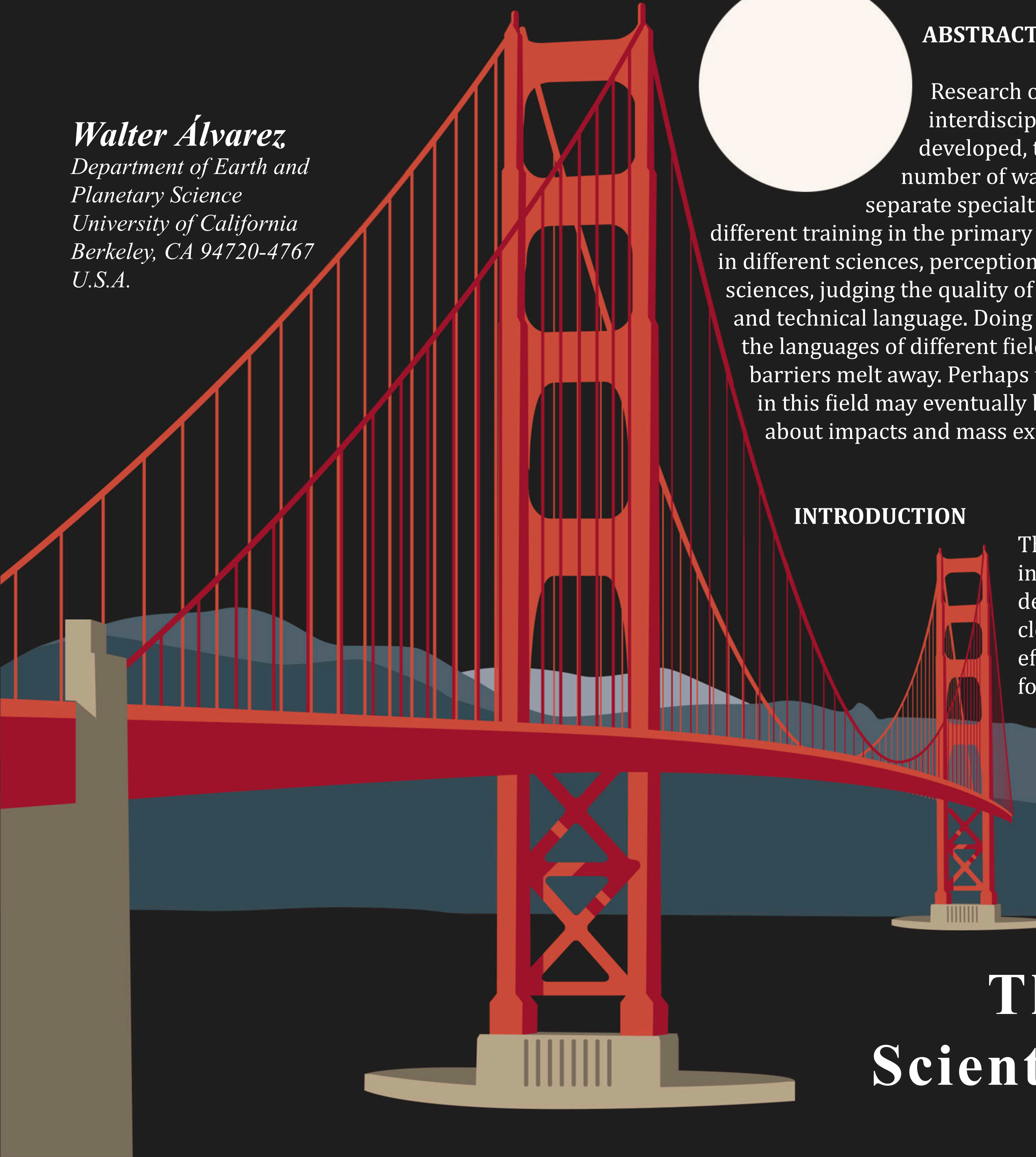
Big History in the Larger World



The Gentle Art of Scientific Trespassing

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ABSTRACT

Research on impacts and mass extinctions has been interdisciplinary in the extreme. As the field has developed, the scientists involved have learned a number of ways of bridging the barriers that normally separate specialties. The most difficult problems involve different training in the primary and secondary sciences, different cultures in different sciences, perceptions of a hierarchy or pecking order of sciences, judging the quality of scientific work, and the barrier of jargon and technical language. Doing interdisciplinary science involves learning the languages of different fields, and when this is done, most of the other barriers melt away. Perhaps the interdisciplinary style that is growing up in this field may eventually be as important as the things we are learning about impacts and mass extinctions.

INTRODUCTION

There seems to be a close association between interdisciplinary science and revolutionary developments in geology, although it’s not clear which (if either) is cause and which is effect. You might disagree, but I think I see four revolutions in 20th century geology.

The first brought us radiometric dating. The interdisciplinary character of this development could be symbolized by the collaboration at Berkeley in the 1950s and 1960s between physicist John Reynolds, geologist Garniss Curtis, geophysicist Jack Evemden, and paleontologist Don Savage (Glen, 1982).

The second revolution, which brought us plate tectonics, had an aborted start with the meteorologist Alfred Wegener, then took off with geologist Harry Hess and geologists, geophysicists and paleontologists, physicists, and chemists too numerous to list.

Looming on the horizon is a coming revolution in understanding Earth as a system, which will surely involve people from biology, earth sciences, engineering, physics, chemistry, and mathematics.

Interdisciplinary work has also been characteristic of the currently active and controversial revolution over the role of impacts and other catastrophic events in Earth history. This development is forcing the rejection of classical uniformitarianism, as we realize that modem geologists must be able to think about both sudden and gradual changes in order to understand the history of Earth. Shortly before the discovery of the Italian Cretaceous-Tertiary iridium anomaly, we were already doing interdisciplinary research at Gubbio, in the Apennines, as a team ranging from paleomagnetist Bill Lowrie to micropaleontologist Isabella

The Basilica of Sant'Ubaldo, on the mountain overlooking Gubbio, Italy. A twelfth-century bishop of Gubbio, Saint Ubaldo, led the citizens up the Bottaccione Gorge at night (past the K/T boundary) and circled around, surprising and driving off the combined armies of eleven nearby towns which were besieging Gubbio. *Photo by Walter Alvarez.*

Premoli Silva correlated the biostratigraphic and magnetostratigraphic time scales (Alvarez et al., 1977). The iridium anomaly discovery paper (Alvarez et al., 1980) was written by a particle physicist, a geologist, and two nuclear chemists. Almost immediately, other interdisciplinary groups began to work on the problem. One early paper was written by an oceanographer, an atmospheric scientist, and a planetary geologist (Emiliani et al., 1981), and a more recent, extreme example was written by two astronomers, two geologists, and four paleontologists (Hut et al., 1987).

The Bottaccione Gorge at Gubbio. White pelagic limestones in the foreground are the Lower Cretaceous Majolica formation. In the distance are the pink pelagic limestones of the Upper Cretaceous-Eocene Scaglia rossa formation, with the K/T boundary about half way up the cliff. The near horizontal structure is a twelfth-century aqueduct that brought water to Gubbio (this is the "Bottaccione," or "big water barrel").

Many other questions in geology involve input from chemistry or biology or physics, but they do not often attract chemists and biologists and physicists to work on them; they stay strictly in the mainstream of geology. Why did this particular topic, the mass extinction 65 m.y. ago, draw in so many people from so many other fields? I think it is because the impact of a 10 km extraterrestrial body on Earth is such an unusual and extreme event that it led to unexplored parts of other fields, not to their central, well-known bodies of information. Suppose one had gone to a chemist or physicist and asked for help in understanding some aspect of the KIT boundary. If that chemist or physicist had been able to say, "Well, why don't you just look in the index of any elementary textbook?," there would have been little incentive for that person to join in the research.

However, this extraordinary event has led to new kinds of thinking in every branch of science it has touched. In biology, it required thinking about non-Darwinian mechanisms of evolution. In geology, it forced a reevaluation of the central geological doctrine of "uniformitarianism" or "gradualism," which for 150 years had discouraged any thinking about catastrophic events. In chemistry, it focused on iridium, an almost comically obscure element, and created a demand for very fast analytical capabilities at the parts-per-trillion level. And new problems have been opened up in ecology, geophysics, astrophysics, and atmospheric science, as well.

Impact research has thus led to forefront work in a variety of different sciences. But progress in working out the implications for each science has depended on keeping in touch with what is happening in each of the other sciences. For example, think about astrophysicists, exploring the idea that a hypothetical companion star to the Sun (Davis et al., 1984; Whitmire and Jackson, 1984) might cause periodic impacts and mass extinctions on Earth by gravitationally disrupting the Oort comet cloud of the outer Solar System as it comes dose to the Sun every 25 to 30 m.y. Calculations as to whether such a wide binary star system would be stable (Hut, 1984) depend on the latest information from geology and paleontology bearing on the timing of impacts and extinctions: are impacts periodic or aperiodic (Raup and Sepkoski, 1984, 1986; Grieve et al., 1985; Shoemaker and Wolfe, 1986; Baksi, 1990)? If they are periodic, what is the time interval between them?

The whole field of research on impact crises has been built on interdisciplinary research, and trespassing on other people's fields has become a privilege and a pleasure for those of us involved in it, as has welcoming visitors from other parts of science who get interested in our own disciplines. So let us consider the experience of crossing discipline boundaries in science.

BARRIERS TO CROSSING DISCIPLINE BOUNDARIES

It seems to me that there are several barriers to crossing discipline boundaries, some minor and others more difficult. In practice, however, it is quite possible to bridge these barriers, and doing so brings great rewards, both personal and scientific.

Academic Departmental Structure

First of all, interdisciplinary work is hindered by the departmental structure of the universities. In academia, at least, we live our lives surrounded by people in the same general field. Yet this is largely a matter of habit. At Berkeley, and I am sure elsewhere, there are many opportunities, both formal and informal, for moving out of the confines of one's department; this is no excuse!

Disciplinary Structure of Funding Agencies

A second obvious problem is that interdisciplinary research tends to fall into the cracks between programs at funding agencies like NSF. Perhaps there ought to be a special division at NSF, or a separate agency, aimed at funding maverick interdisciplinary proposals. Meanwhile, as we wait for this Utopian dream to come true, it is worth noting that interdisciplinary research topics are more likely to interest private donors and the generalists who run private foundations than are the narrowly focused projects that appeal to specialists.

Asymmetry in Training Between Primary and Secondary Sciences

Turning to the more subtle problems that raise barriers to interdisciplinary science, our third problem concerns the difference between what we might call primary and secondary sciences. As students we are all trained in the primary or basic sciences — mathematics, physics, and chemistry. However, the secondary sciences — geology, paleontology, biology — are studied almost exclusively by practitioners of those sciences. Almost all geologists have a basic understanding of chemistry, but few chemists know anything at all about geology. This puts a one-way valve in the communications system, and as you will see, good communications are the prime consideration and the prime difficulty in doing good interdisciplinary science. Because of the asymmetry in training, a somewhat harder burden falls on people from the basic sciences, but anyone wishing to cross disciplinary boundaries will have to learn — or will have the pleasure of learning — someone else's science.

Varying Cultures and Traditions in Different Sciences

The fourth problem concerns the different cultures and traditions of the different sciences. Because of our different subject matter, scientists in various disciplines must work in different ways. Chemists and physicists work in controlled laboratory settings, isolating the phenomenon they wish to study, and carrying out elegant and repeatable experiments. Geologists and paleontologists are restricted to studying what nature has preserved for us — or, sometimes, what the highway department has chosen to excavate, and has not chosen to pave over.

Our differing traditions go back centuries and are picked up and internalized by each of us as students. Chemists honor Marie Curie and Mendeleev; physicists honor Newton, Einstein, and Fermi; biologists honor Wallace and Darwin. As a geologist, I count G. K. Gilbert, Alfred Wegener, and Harry Hess among my heroes. Although we are all scientists, we have had to develop quite different ways of doing science, and when people with these different backgrounds join together to work on a common problem there is inevitably misunderstanding at first, and friction. However, our experience is that these problems do not last long when people get together to work on an intriguing interdisciplinary problem.

The Spectrum or Hierarchy of Sciences

One of the misunderstandings emerges as we look at the fifth problem, which concerns the hierarchy, or pecking order, of the sciences. The scientific pecking order appears to reflect the prestige of the various disciplines. Why does this hierarchy exist? I'm leaning toward the view that the higher prestige disciplines are able to formulate general laws that require considerable mathematical sophistication to understand, whereas the lower prestige disciplines deal with subject matter of great complexity, which must be described and classified before it can be understood. In this view, the hierarchy of sciences has nothing to do with the relative merits of the different sciences, but is instead a function of the kind of subject matter with which they deal. If we drop the loaded terms like "hierarchy" and "pecking order" and simply arrange the sciences in a spectrum from mathematically sophisticated at one end to descriptively complex at the other, we would probably not differ too much in assigning a sequence something like the following: mathematics, physics, chemistry, astronomy, geology, paleontology, biology, psychology, sociology.

Let us trace one strand of impact-extinction research across the spectrum of sciences and watch the complexity increase. Nuclear chemists like Frank Asaro, Helen Michel, and Carl Orth use techniques from physics to do neutron activation analysis for elements like iridium. They measure the neutron flux that irradiates their sample, and as the radioactivity decays they measure the energy and release time of de-excitation gamma rays. They end up with a reliable value and uncertainty for the concentration of iridium in a sample, — say 37.9 ±2.3 (1 SD) x 10^{-12} g Ir/g whole rock.

Stratigraphers like Sandro Montanari and Jan Smit, studying an Ir profile across the KIT boundary, must consider less quantifiable uncertainties, including sedimentary reworking, burrowing by bottom-dwelling organisms, and chemical remobilization as they determine whether the Ir was deposited instantaneously.

Paleontologists like Gerta Keller, Hans Thierstein and Peter Ward, trying to decide whether the Ir input coincided in time with a mass extinction, must decide how to define a mass extinction — they have to choose the taxonomic level to use and whether to focus on taxa lost or on biomass destruction — and then they must consider whether hiatuses and fossil reworking are complicating the record, and whether an apparent diversity decline is real or just a sampling artifact.

If the evidence for impact seems to coincide with the extinction level, paleoecologists like David Milne and David Jablonski have to consider what the geographical extinction pattern was, what were the life styles of victims and survivors, and which of the suggested killing mechanisms — darkness, acid rain, greenhouse heating, fires, etc. (Gilmour et al., 1989) — might have affected each group.

Finally, if it is concluded that impact causes mass extinctions, evolutionists like Steven Gould and Digby McLaren must consider the extent to which this forces us to revise Darwin's concept of evolution by natural selection. From counting gamma rays to revising Darwin there is an unbroken chain of interdisciplinary science, but the levels of mathematical sophistication and descriptive complexity vary dramatically.

What is the effect of this spectrum of sciences on interactions across the disciplines? It causes real problems because the spectrum is often interpreted as a ranking in order of merit. But when a healthy interdisciplinary field grows up, most of the people in it simply see through the fallacy of this pecking order and recognize that each science has developed the techniques it needs for its kind of problem. My father once told me, after visiting me in the field, that he admired the work of geologists, but that he would stick to physics, thank you, because geology was just too complicated for him.

Judging the Validity of Scientific Results in Someone Else's Field

Continuing the list of barriers to interdisciplinary work, number six is this: How do you estimate the level of confidence you can have in data and interpretations from someone else's field? We are all accustomed to doing this every day in our own field, where we have the experience to evaluate the quality of a particular piece of research, or where we have worked on the same topic ourselves, or where we know the reputations of the people involved. Judging the quality of a piece of research in a completely different science is much more difficult, and the criteria may be quite different. At least at the beginning, one is probably dependent on the judgments of colleagues from that other science. It is of course even more difficult for the press and the public to make accurate judgments about the validity of particular scientific results.

Given this problem, it is important for workers in an interdisciplinary subject to go out of their way to make it possible for scientists from remote fields to judge published results. One needs to take more care in documentation than when writing for fellow specialists. This may mean (Editors, take note!) giving explanations or making citations that would be considered unnecessary or patronizing in most technical literature.

To facilitate judgments about the reliability of results, we can make use of a whole variety of techniques available to scientists. Familiar approaches include the determination of analytical confidence limits, estimating confidence levels for less quantitative observations, rigorous statistical testing of hypotheses, interlaboratory calibration of analytical standards, and the independent analysis of blind samples from critical locations. (Blind analysis of some critical, disputed levels across the Italian K/T boundary is currently being carried out under the supervision of Robert N. Ginsburg of the University of Miami.) One can often invent or modify special techniques suited to particular questions; Muller's (1988) description of the use of the "Game Program" to decide a confidence level in a proposed periodicity is an excellent example.

The key to judging research results across disciplines thus comes down to rigorous care and full explanation on the part of the producer, and the willingness of the reader to delve deeply into an unfamiliar literature. This last consideration brings us to the question of how well a scientist from one field can understand what a practitioner of a remote specialty is saying or writing.

Jargon and Technical Language as a Barrier to Communications

The final item in this list of problems in crossing disciplinary barriers is thus the matter of technical language and jargon. I have come to see this as a major barrier to communication, both in reading the literature and in conversation with scientists from other disciplines. Nevertheless, this barrier can be overcome, and overcoming it is in itself an interesting process.

What is the role of jargon and technical language in science? Why do they exist? Technical language is clearly a necessary part of science. We need new words to describe new phenomena that are not covered by the vocabulary of the common tongue. But jargon seems to play two additional roles in science, one detrimental and the other beneficial. In its detrimental role, jargon serves to exclude the untrained from a specific high priesthood — those who are initiated in a particular discipline or specialty. In its more beneficial role, jargon serves as a tool for calibrating the level of expertise of a new acquaintance, and helping you choose the level on which to communicate.

To me, jargon and technical language present the highest barrier to crossing discipline boundaries. The other major barriers, especially cultural differences and notions about a hierarchy of sciences, melt away once the language problem is surmounted.

AN APPROACH TO CROSSING DISCIPLINE BOUNDARIES

So how does one overcome the language barrier between disciplines? It seems to me that language fluency comes almost automatically, if we treat the boundaries between disciplines not as barriers, but as gateways leading to new things to explore. After all, as scientists we are driven by curiosity about nature. Why can't we be just as curious about the workings of somebody else's field of science? Each field has its own history, its own traditions and ways of thinking and working, its own folklore, and even its own language.

l have come to view language learning as the key to interdisciplinary work. There is no practical way to get different specialists to use the same tongue, so those wanting to cross barriers simply must learn other scientists' languages.

What does this language learning involve? First of all, we need to know what the words mean. The same word may carry very different meanings when used by two different people. We know about this in foreign languages; for example, *burro* means donkey in Spanish, but it means butter in Italian. Or to take an extreme case, *ne* means no in Yugoslavia, but across the border in Greece, it means yes. No wonder Balkan history has been so troubled. Different meanings for the same word arise through time in the same language. In order to understand Shakespeare's plays, we need to know that words like *compass* and *conceit* meant something quite different to the Elizabethans than they do to us. To a chemist, *radiation* means light, but to a paleontologist it means appearance of new species from a common ancestor. However, even this doesn't end the problem, for *species* has different meanings to a paleontologist and a chemist.

A second observation about language is that certain key phrases act as passwords for recognition among speakers of the same dialect. If we hear phrases like "right on" or "jolly good," we immediately know which side of the Atlantic the speaker comes from. The same thing holds true in scientific dialects. Trivial as it may seem, I found that my main breakthrough into the physics community came when I stopped saying that something "was a hundred times larger," as a geologist would, and began saying "two orders of magnitude greater."

At a more subtle level, one finds that cadence and style reflect the complexity, the traditions, and the folkways of a particular science and define recognizable dialects. For example, there is a dialect known as Physics Macho, in which any derivation that takes a sophisticated mathematician less than a week is referred to as "an exercise for the student." Another example is a dialect called Ecologic Jargon Overkill. Here is a sample from the literature, only slightly edited: "Dissimilatory anoxic oxidation is carried out in the sulfuretum by photolithotrophic bacteria like the Chlorobiaceae, which are obligate photolithoautotrophs and strict anaerobes, the Chromatiaceae which are partly obligate, partly facultative photolithotrophs, and the Rhodospirillaceae, which are photoheterotrophs ... although many of them are able to grow photolithotrophically as well."

Geological dialect undoubtedly has its own sillinesses, too, which I would like to report to you if I could, but they are much harder for a native speaker like me to recognize. Perhaps an outside observer would find the dialect of geology to be colored by the description and classification of complex phenomena, which has been a major task of our science. Thus our dialect might be represented by a paper, published in the last century, with this title: "A Description of the Dessicated Human Remains in the California State Mining Bureau" (Anderson, 1888).

The difficulty of learning a language or a scientific dialect is clearly related to its complexity. Russian, with its ornate system of declensions, is harder for English speakers to

Geology is more complicated than physics: When physicist Luis W. Alvarez visited the K/T boundary at Gubbio, it disturbed him that the beds were dipping at 45°. He leaned over and had this picture taken with the camera tilted, so that audiences of physicists would understand the originally horizontal bedding.

learn than are Romance languages. Geology is a more complexly descriptive subject than physics (though not necessarily more difficult), and as a result, its dialect is harder for physicists to learn than vice versa. For the same reason biologese has been very difficult for me to learn. I still can't speak Ecologic Jargon Overkill, but I'm working on it.

Serious understanding of another field does not immediately result from learning scientific dialects. But with the language mastered, you have the tools for discussing the subject matter and reading the literature in depth, and the practitioners of the field will take you seriously. Many people have done this in the general field of research on impacts and mass extinctions, and have found it to be scientifically and personally rewarding. I believe it is the key to successful interdisciplinary research.

CONCLUSION

As science penetrates deeper and deeper into the unknown, most fields become of necessity more and more separated and specialized. Yet some topics seem naturally to bridge the gaps between fields. The study of impacts and mass extinctions seems to be one of these bridging topics. Perhaps the scientific style that is growing up in this field may eventually be as important as the things we are learning about nature.

ACKNOWLEDGMENTS

This paper is based on things l have learned from and with many people interested in impacts and mass extinctions. Foremost among them are my original colleagues in the Berkeley group — Luis Alvarez, Frank Asaro, and Helen Michel. I especially thank Frank for organizing the American Chemical Society symposium that was the spur to think these matters through. The thoughtful books by David Raup (1986) and Rich Muller (1988) were a further stimulus to think not only about what science learns, but about how science is done. Despite the fact that our work was often difficult to pigeonhole in the structure of academic disciplines, my colleagues and I have received general financial support from DOE, NSF, NASA, and the California Space Institute, and more specialized support from the Murdoch Charitable Trust, the Hewlett-Packard Company Foundation, Dr. John Lawrence, Gordon Getty, and the U.C. Berkeley Foundation.

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Author's Note: In 1988 Frank Asaro was organizing a symposium about research on impacts and mass extinctions at a meeting of the American Chemical Society and asked me to speak on the topic, "How geologists view chemists." Recognizing the potential for disaster inherent in that title, I convinced him to let me speak instead on "How scientists view each other across discipline boundaries." Versions of that talk were published in the Proceedings of the 1988 Snowbird II Conference (Alvarez, 1991a), and in GSA Today (1991b). With the advent of Big History, it has again become important for scientists and scholars to learn to communicate across disciplinary boundaries. Fred Spier and Esther Quaedackers asked if this essay could be reprinted for Big Historians, and the Geological Society of America granted permission. The 1991 text has not been revised, but I hope that even in its original form it may be relevant and helpful to Big Historians.

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Big History in the Larger World

Bill Cox

Ph.D., Russian History, University of Michigan - Ann Arbor, 1970, retired

I shall leave the issues of making coherent history of the revelations in the sciences to others. Now that the large scaffolding has been accomplished, the question of what we will do to create a "discipline" is a worthy enterprise and was a major theme of our international meetings at Dominican University of California in August 2014. That and silo bridging are still major undertakings and ones that must be carried forward within the politics of universities and secondary schools. These are big and pressing issues and are thought to be questions of academic legitimacy.

BH seems solid in its general architecture but we know that research in the sciences is the basis for this understanding and that each agreement has its skeptics (bless them) at any time questioning several of its elements. Recently the meteor as probable explanation of the K/T dinosaur extinction met new challenges from volcanologists (Feb. 3rd NY Times). I'm sure Walter Alvarez and his colleagues whose research found the geologic evidence for the meteor in the iridium layer (see feature article in this issue) will be responding soon to this new information. Historians are used to this provisional understanding and often excited by new research even when it augments or rearranges their plowed fields.

Outside the confines of higher education, BH is making its way. Inside it confronts, on the one hand, those invested in an older cosmology, one that gave them their authority, which explains their current resistance. On the other hand, those for whom teaching is an honest, if difficult living, see the new framework as another intrusion, just one of many they must bear up against to teach our young. (See the comments on Andrew Sorkin's story of Bill Gates and the BH Project in the NY Times). There is much concern for precedence and pedigree, for status, and for not making worse an already bad situation in the academy.

But, imagine, we are sitting in the theater of Ptolemy's construction, the walls its boundary, the ceiling its celestial comfort, and on the proscenium, with its great painted sets, parade the moral stories of our original sinfulness and the cramped skits of our contractual redemption. These are the tableaux that have framed our studies and into which we fit them..Then,asn the intent of the Santa Fe opera house the walls fall away, the ceiling peels back, and we see above us the vaulting stars in the infinite black of the universe. We realize that the small, endlessly rehearsed stories no longer capture the enormous scale of our reality. That's how it came to me after reading in the secondary sources of astrophysics, paleontology, and biology---a hair-raising story of origins and emergence in the great empty sea of the universe. This is a conception long over-due and breath-taking in its implications!

Historians in the past, I among them, seemed content to leave these big questions to others, but in quick accumulation the sciences created explanatory understandings that begged to be stitched together.
The agreement to divide knowledge into the impressive disciplines constructed in universities began to unravel a bit; a few brave souls in the late 20th c., in Australia, the Netherlands, at Harvard and Berkeley began speaking to each other across the boundaries, combining disparate information sources, and from that collaboration a much more powerful history of "everything" emerged.

"What do we know, how do we know it, and how does it fit together?" This work, widely popularized by Bryson and David Christian's

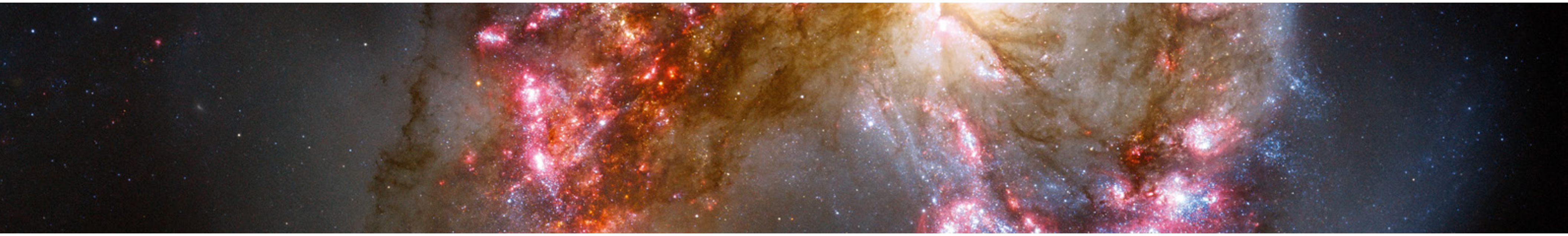
Teaching Company course among others, became the impetus for the broad cultural work we are all engaged in. Scientists in my experience are more grateful for that work (although they have reservations, too, among them related to the parochial histories of their disciplines), than are the liberal arts colleagues of our practicing Big Historians. So much of our literature accepted the division between God and Caesar, the tacit exceptionalism of religion. Our long-standing concession that time be divided by a "supernatural" event, that we count what we called ancient history backward, as a count-down to that "event," still haunts us.

What are we talking about here? Life on this planet is 3.8 billion years old, but human "history," since it is based on our much more recent development of symbolic language, is a late, late emergence. It would be helpful if we all made an effort to undo this chronology based on a miracle, a calendar which only gained traction in the West thirteen centuries ago and which moved with colonialism/imperialism to the rest of the world that needed uniformity of measuring and recording time. Can we fix this? Or do we relent and continue with this artifact of the flat earth cosmology? There are many hinges and many such awkward remnants.

And yet those who "get it," as David Christian so perfectly said, get it. And more and more people are crossing that threshold. It is exciting to see what they will do with this expansive, evolving realization. I know that much of the drag on our Dominican meetings came from individuals protecting religions, perhaps seeing in the metaphysical implications of BH a broad assault on their craft or personal justifications. That is their threshold, too, and they will cross it as they eventually must. It is more difficult for some, but I think all of us who have participated in the exegesis of western culture/religion/history have different tolerances for pain and "loss." And some of us are ecstatic!

And some of us have made some very interesting adjustments. I am most impressed, speaking of religion, by Michael Dowd's six agreements, which lay out a rich and rewarding ontology based upon BH, which Michael calls our "modern evidence-based creation story." He was prominently present at the meetings in Grand Rapids and perhaps, I think, distrusted, since his interest derived not from academic history, but from his desire to combine science and religion, as the Darwin amphibian kissing the Jesus fish on the side of his traveling van portrays. Trained to preach and ardently dedicated to knowing and teaching the most true, Michael comes to us through Thomas Berry, but Big History has given him a ground worth standing on. As it has given the same, one way or the other, to all of us.

So in the larger world BH makes its way, a major element of our zeitgeist, entering into situation comedies, Facebook pages and internet blogs, popping up here and there, gradually becoming a new and fundamentally startling appraisal of reality. No matter how we came to BH, we are all so lucky to live at this moment, in these exciting times of the birth of a new cosmology.




Third IBHA Conference
July 15 - 17, 2016
Amsterdam

New and Returning IBHA Members

One of the key purposes of the IBHA is for those of us who are interested in Big History to have a place to associate. We benefit from learning of each other's Big History activities and thoughts through associating with each other. One way of doing this is through reading the IBHA members' pieces that are in this newsletter. Another is to participate in IBHA conferences, such as the upcoming onein Amsterdam in 2016. So we are delighted to welcome new members to IBHA membership. And we are delighted by the vote of confidence and recognition of the value of our association by those who have renewed their membership. It is a pleasure to have each of you with us.

February 1st - **Georges Depeyrot** – new member (France)

February 1st - **Penelope J Corfield** - renewal

February 1st – **Juan Alvarez de Lorenzana** - renewal

February 2nd - **Heathe Kyle Yeakley** - renewal

February 2nd – **Paul Adams** – renewal

February 3rd – **David Osleger** – renewal

February 10th – **Davidson Loehr** – renewal

February 13th – **Jesus Tagle, Jr.** – renewal

February 17th – **Meg Bridgeman** – new member

February 21st – **Barry Wood** – renewal

February 24th – **Paul Sullivan** – new member

International Big History Association

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