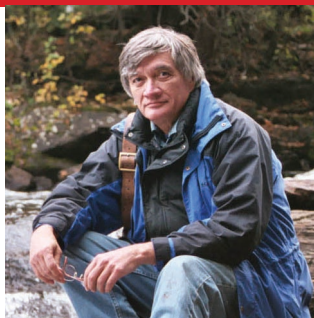


## LOST IN TRANSLATION\*



Rod Ewing

One of the advantages of forays into other disciplines is that one realizes there are many different ways to “see” and “do” science. Trips across disciplinary boundaries are like travel to a different country. The first shock of the differences in language and culture slowly gives way to new perspectives that one would never see from home. In many cases, “new” ideas result, but they are not new—rather they are just borrowed from the other culture. At this “borrowing-edge,” one finds new and exciting research

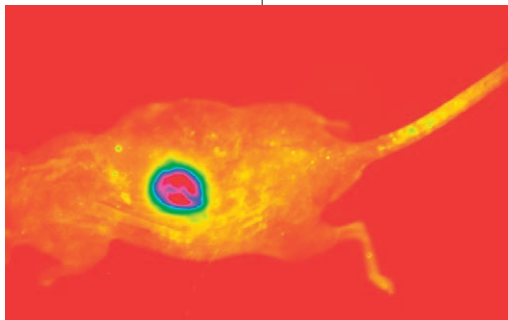
opportunities. As an example, during the past several years, I have had the pleasure and excitement of working with colleagues in a medical school. The goal of the research is to manipulate and functionalize carbon nanotubes for cancer diagnosis and treatment. The approach involves three rather remarkable ideas: (1) to functionalize the surfaces of the carbon nanotubes so that they will attach to specific types of tissue, such as tumors; (2) to image the distribution of the nanotubes by attaching glowing quantum dots to their surfaces (see the inset of the *in vivo* image of the “glowing” mouse); (3) to use the nanotube itself as a container that, on demand, releases drugs for treatment. This general strategy is a major subject of research in medical science, with many variations on this basic theme (*Nature Nanotechnology*, News & Views 2007, 2: 745–746). Our contribution to this research is small, but the payback is huge in new ideas and perspectives. As an example, can we extend the very specific functionalization of surfaces to the remediation of heavy metals using *in situ* reactive barriers? Can quantum dots, or something similar, on nanoparticles be used as tracers in groundwater studies? Can the geochemistry of soils be modified by the timed release of nutrients from nanoparticles?

In addition to new research ideas, there is the broader issue of how the different disciplines formulate their research strategies—such as the concept of *translational research*—designed to move the latest results of basic research to rapid application. The development of translational research strategies is particularly important in the medical sciences where new drugs and treatments can save lives. For this reason, there has been a growing concern that increased budgets for biomedical research have not had a large enough impact on human health. Biomedical research budgets have grown substantially during the past twenty-five years. The budget of the National Institutes of Health doubled between 1998 and 2003, although it has remained essentially flat during the past five years.

Despite the increased funding, the applications of basic biomedical research to the improved treatment of disease and better health have fallen far short of expectations. Even with the huge investment in medical science, the number of new drugs and treatments remains modest in comparison. This growing gap between basic research and beneficial application has been described as the “Valley of Death,” a landscape that separates the basic researchers on one peak from the clinical researchers on another distant peak. The brave explorers who

try to cross from one peak to the other—that is, move the new concepts in basic research to rapid application—are most often lost in the jungles of this deep and broad valley. Biomedical researchers are increasingly focused on cutting-edge, “transformational” basic research that will appear in the top, highly cited journals. Clinicians are focused on treating patients, have little time to absorb a growing literature, and, sad to say, must generate fees for their services. The bulk of biomedical research is done by highly specialized PhDs, while clinicians are mainly MDs. This is an overly simplified description of the situation, but the problem and the metaphor ring true across other disciplines.

Since 2000, the “Valley of Death” metaphor has been repeatedly used to discuss the barriers to the application of basic research (*Nature* 2008: 840–842). In nanoscience, the number of scientific publications is doubling every 2 to 3 years, clearly stimulated by over \$4 billion in federal funds (2003–2007) and nearly matching amounts from venture capitalists, but the commercialization of nanoscience lags far behind the scale of this investment. In energy research the Valley of Death is described as a major barrier to the implementation of technologies for increased energy efficiency. A National Research Council report, “Accelerating Technology Transition: Bridging the Valley of Death for Materials and Processes in Defense Systems,” shows that this metaphor and the problem extend to nearly every corner of the science and technology enterprise. In the “Basic Research Needs” reports from the Office of Science of the Department of Energy, user-inspired basic research is a phrase meant to capture the idea that basic research can be defined in terms of the more immediate needs of society.



*In vivo* image of a glowing mouse. From Shi et al. (2007) *Advanced Materials* 19: 4033–4037

The Valley of Death has been recognized as a problem by politicians, policy makers, and funding agencies. Government agencies are actively responding to congressional and state pressure to strengthen the connection between basic research and application. In 2003, the National Institutes of Health created the first Clinical and Translational Science Centers, which are expected to finally number over 50 by 2012 and projected to consume 1 to 2% of the NIH budget. This is controversial because even though the percentage of the investment is low, in tight times, this requires diverting “basic” research funding to the new concept of translational research. The metrics of success for translational research are difficult to define, perhaps placing less emphasis on individual productivity and impact and more emphasis on collaborative efforts and evidence of application. In 2005, the Nanomanufacturing Investment Act was introduced in Congress to support nanomanufacturing projects, explicitly excluding basic research. In California, an Emerging Technologies program funds utility energy-efficiency programs, but not basic research for increased energy production. Each is an example of politicians and funding agencies pressing on the science and engineering enterprise in order to quickly and efficiently bring basic research results to bear on important societal problems—health, nanoscience, energy, and defense. The old arguments supporting the passive “trickle down” theory of the benefits of excellent fundamental research ring hollow in today’s discussion of the value of basic research.

What of the geosciences? We seem barely touched by the concern for societal impact, and I have certainly never heard a geoscientist call on the Valley of Death metaphor to describe our research landscape. As academic scientists, we are used to waving our wand of words over the section titled “Broader Impacts” in NSF proposals. More than a few proposals and papers, including my own, will describe a serious environmental problem as the “hook” to a description of a basic research proposal. Dozens of papers on arsenic mobility begin with mention of the mass poisoning in Bangladesh and West Bengal, but how many of these papers actually contribute to the solution of this problem?

\* When I started writing this column, I was rather proud of the clever title with the same name as the film starring Scarlett Johansson and Bill Murray. I was disappointed to learn that I was scooped (see article with the same title by Declan Butler in *Nature* 2007, 449: 158–159). I recommend the film.

## USGS SCIENTIST MARTIN GOLDHABER RECEIVES PRESIDENTIAL RANK AWARD



Dr. Martin Goldhaber, a U.S. Geological Survey senior scientist, was recently awarded a prestigious 2008 Presidential Rank Award. During his 33-year career, Dr. Goldhaber has played a pivotal role as a national and international scientific leader. His research has yielded numerous high-impact contributions to the field of geochemistry. Recipients of this prestigious award are strong leaders, professionals, and scientists who achieve results and consistently demonstrate strength, integrity, industry, and a relentless commitment to

excellence in public service. They are nominated by their agency heads, evaluated by boards comprised of private citizens, and approved by the president of the United States.

Throughout his career, Dr. Goldhaber has shown an exceptional ability to develop innovative approaches to solving impressively complex problems in diverse fields of the Earth sciences. His early work on sulfur dynamics fundamentally altered the course of international research on the mechanisms of microbial and inorganic sulfur transformations in sediments. His seminal papers in this area provided a comprehensive understanding of these “diagenetic” processes. Such interactions dominate the long-term global sulfur cycle, are critical to understanding the formation of mineral deposits, and aid in the ability of soils and rocks to recover from and mitigate contamination.

In response to the energy crisis of the mid-1970s, Goldhaber applied his geochemical knowledge to sedimentary mineral deposits in the western United States and rapidly gained international recognition as one of the world’s foremost authorities on the origin of sediment-hosted uranium ores, which represent approximately 90 percent of U.S. uranium reserves.

Dr. Goldhaber has been a leader in characterizing environmental impacts from coal and mineral mining. He is currently cochief of a USGS project to map the baseline geochemistry in soil of the entire continental United States and Alaska. In collaboration with the Canadian and Mexican geological surveys, this project will encompass all of North America. Because the quality of soil is of vital importance to our well-being, this study will help inform the complex choices we make in using and managing this vital resource.

Most recent and most significant of Dr. Goldhaber’s contributions to the USGS is his guidance as cochair and scientific leader of the USGS Science Strategy Team. The goal of this team was to develop a comprehensive vision based on clearly stated science goals and priorities that would unite all Bureau capabilities toward challenges for the future. The strategy has subsequently become the basis for long-range decision making in the USGS. A summary version published in *Science* received international exposure and elicited broad praise from outside scientists and managers and from USGS colleagues.

ADAPTED FROM THE USGS WEBSITE (DECEMBER 18, 2008)

## TRIPLE POINT (Cont'd from page 5)

Our reasoning seems to be that as long as we are doing good or even transformational research, then this is good enough. The results will somehow, someday, find application. But what if we are wrong? What if very good scientific research does not find proper or timely application?

If the medical and materials scientists see this problem, why don’t we have the same problem or feel the urgency to find solutions to environmental problems? Is there someone else who has the responsibility for carrying through on our work? For the medical sciences, the sense of urgency is driven by the desire to live a long and healthy life. Stretching the medical science analogy, our planet is sick, suffering from huge environmental and ecological trauma. Is there a role for translational research in finding solutions to these environmental

## CARL SAGAN MEDAL TO JEFF TAYLOR



G. Jeffrey Taylor, a planetary scientist at the Hawai'i Institute of Geophysics and Planetology (HIGP) at the University of Hawai'i at Manoa, has been named the 2008 recipient of the Carl Sagan Medal for excellence in public communication in planetary science. The prize is named after Carl Sagan (1934–1996), a distinguished planetary scientist who, through public lectures, television, and books, contributed significantly to the public’s understanding of planetary science. The Sagan Medal is awarded by the Division for Planetary Sciences

of the American Astronomical Society to recognize and honor outstanding contributions by active planetary scientists to the public’s understanding of, and enthusiasm for, planetary science. Taylor is the ninth recipient of the Sagan Medal.

Taylor’s gift for science writing spans time, space, and audiences. His work includes his novel *Impact!*, coauthored with R. V. Fodor and published in 1979. Taylor’s first book for children, *A Close Look at the Moon*, was awarded the Outstanding Science Trade Book for Children in 1980 and was followed in 1983 by his second award-winning book for young readers, *Volcanoes in Our Solar System*. Taylor’s popular writing appears in *Scientific American*, *Natural History*, *The Planetary Report*, and *Elements*, among other publications. Taylor’s educational products include a program *Explore the Planets* (on CD-ROM, produced in collaboration with Tasa Graphic Arts), NASA classroom activities, a teacher’s guide on exploring the Moon, online hands-on activities (*Exploring Planets in the Classroom*: [www.spacegrant.hawaii.edu/class\\_acts/](http://www.spacegrant.hawaii.edu/class_acts/)), and the online educational science journal *Planetary Science Research Discoveries* ([www.psrds.hawaii.edu](http://www.psrds.hawaii.edu)), which he cofounded with HIGP colleague Linda Martel. The PSRD website, now in its twelfth year, sheds light on the science questions that researchers are actively pursuing about our solar system and explains how the answers are discovered and what they mean.

An HIGP faculty member since 1990, Taylor’s professional interests range from laboratory studies of rocks and meteorites, through geologic field observations, to remote sensing, all of which he combines to further our understanding of planetary processes that operate on Earth, the Moon, the planets, and asteroids. He is involved in planning future missions to the Moon and Mars, in the use of robotics in field geological studies, and in remote sensing mapping.

In addition, Taylor serves as associate director for space science for the Hawaii Space Grant Consortium and was director of the consortium, from 1998 to 2002, overseeing statewide K-14 educational programs in space science and exploration. He serves tirelessly at the university as academic advisor, teacher, mentor, and leader, and on a variety of teams and panels for NASA and the greater scientific community.

ADAPTED FROM A PRESS RELEASE WRITTEN BY L. MARTEL, AVAILABLE ON THE WEBSITE [WWW.SOEST.HAWAII.EDU/SOEST\\_WEB/2008\\_NEWS\\_PDFs/TAYLOR\\_SAGANMEDAL.PDF](http://WWW.SOEST.HAWAII.EDU/SOEST_WEB/2008_NEWS_PDFs/TAYLOR_SAGANMEDAL.PDF)

traumas? The readership of *Elements* numbers over 10,000, representing a large fraction of the geoscience community working on environmentally relevant issues. Yet, what fraction of our work finally has an important impact? What would an Institute for Translational Geochemical Research look like? What types of research would be supported? Do we want to continue to rely on the trickle down theory of the benefits of basic research? Or, should we adopt more directed and demanding goals for our discipline? I do not have answers to these questions, but when I see other disciplines struggling with the issue of the societal impact of basic research, I wonder why we are not.

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