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.

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 “trickles down” theory of the benefits of excellent fundamental research ring hollow 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?

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TRIPLE POINT

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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 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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