The unity of science

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Received 24th July 2008, Accepted 18th August 2008
First published as an Advance Article on the web 5th September 2008
DOI: 10.1039/b812783m

The energy crisis that has come upon us so suddenly in the past few years has been, in a curious and ironic way, a stimulus for science. Today the world is beset by urgent problems that only science and technology can solve. Scientists and engineers nowadays are in the driver's seat. Nearly everybody—presidents and prime ministers, legislators, and the informed public—understands the centrality of science and technology to meeting our energy and environmental challenges, and everyone looks to science for solutions. Properly so. There is simply no good remedy to the dual problems of burgeoning global energy demand and growing greenhouse gas emissions in the absence of major scientific and technological breakthroughs. Today we have two options: either we develop new, transformational energy technologies that radically change the game, or we deliberately curtail economic growth and development. It's hard to see any other way to mitigate the impact of skyrocketing global energy demand.

But if we are not careful, this apparent blessing for science could well turn into a curse. Today the sense of urgency is so pervasive—the stress on the system so profound—that the temptation to follow short-term, pied piper-led solutions is almost overwhelming. In an era of oil prices hovering above $100 a barrel, governments around the globe are being pressed for immediate solutions. And legislators throughout the developed world are sorely tempted to redirect funds away from fundamental research toward short-term expedients, quick fixes, which may not be so quick, and may not turn out to be "fixes" at all.

In my view, this is the greatest challenge confronting policymakers in the present era. The skewing of priorities spurred by our energy emergency—as understandable as it may be—is making it very difficult to sensibly manage our national research portfolios. In this age, it is more than usually important for science policymakers to keep in mind what I call the "unity of science."

Human beings are practical creatures, focused on survival. And in times of crisis, the claims of applied research and short-term exigencies tend to overwhelm the claims of longer-term fundamental science. This is a natural reflex. But often forgotten are two truths: First, applied research is always living off the intellectual capital accumulated through basic science; if that capital accumulation slows, applied research will ultimately be sapped of its power. Second, in all but rare instances, only basic research can produce the real leaps needed to transcend the problems that confront us. As C. H. Llewellyn Smith, the former CERN Director-General, has written (paraphrasing the discoverer of the electron, J. J. Thomson), "applied science leads to improvements in old methods, while pure science leads to new methods."

But where and how these "new methods," these disruptive discoveries, arise is typically a surprise. Think of the multiple technologies—scientific, medical, and industrial—that have sprung from the well of accelerator science, originally pursued by particle physicists in the hunt for new subatomic particles. Many of our most important new tools in the Department of Energy (DOE) Office of Science National Laboratory system—for example, the high-intensity light sources that are the tools of modern pharmaceutical discoveries—are spin-offs of technologies developed by particle physicists in the service of pure discovery.

In September 2007, the DOE Office of Science established three new major DOE Bioenergy Research Centers—with a planned investment of $405 million through Fiscal Year 2012. These Centers—two led by National Laboratories, one by two major universities—are utilizing fundamental systems biology technologies and techniques, many of which grew out of the Human Genome Project, in the effort to crack the problem of producing cellulosic biofuels efficiently and cost-effectively: rapid genomic sequencing, high-throughput screening, and metagenomics, to name only a few. Who would have thought, when our Office at DOE initiated the Human Genome Project in 1986, that the technologies, techniques, and "new methods" developed under this project would prove

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critical to addressing our energy problems two decades later?

This is what I mean by the unity of science. Energy, in particular, is such a far-flung field that it is really impossible to predict which branch of science, which path of discovery, will yield the key solutions in the years ahead. To attempt to pick winners at this stage, to arbitrarily shift funds from one field to another, in the hopes that the particular horse we are betting on will reach the finish line first, is imprudent at best, and foolish at worst.

A more sensible approach, instead, is to maintain a balanced portfolio between what Donald Stokes, in his book *Pasteur's Quadrant*, classified as "pure basic research" and "use-inspired basic research." This is the approach we have tried to take in the DOE Office of Science. At the use-inspired end of the continuum, we have the DOE Bioenergy Research Centers. At a more basic, but still use-inspired, point on the research continuum, we have our proposed Energy Frontier Research Centers, funded at a total of $100 million per year, which are intended to attack energy science challenges such as electricity storage and transmission, or solar energy-to-fuels conversion, at a more fundamental level. We continue to invest in fusion energy, an investment with both high risk and high promise. And we have greatly expanded our capabilities in high-end computation, which serve the entire continuum of our programs. Yet we continue to provide robust support for Nuclear and High Energy Physics, which remain a wellspring of fundamental new insights and new tools that constantly nourish and replenish our more use-inspired research efforts.

Our energy and environmental challenges clearly have both a short- and a long-term dimension. The International Energy Agency predicts a 50% increase in the world's energy needs by 2030; so the problem of growing energy demand is going to be with us for the long haul. Elected leaders tend by nature to have a short time horizon and focus on the immediate needs of their constituents. But in directing funds toward short-term demonstration projects and incremental improvements in existing technologies, or in prematurely picking winners and losers, we are likely end up with a series of short-term steps that do not add up to an abiding solution to our problem. It is up to scientists and to science policymakers to explain to our elected officials the subtle and surprising paths of research and discovery, and to remind them of the unity of science required to achieve our nations' energy goals.