AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

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SUPPORTING SCIENCE

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The twilight of the 20th century is an age of enormous technological change. Every day brings new examples of advances in computing, communications, and biotechnology that change the way we live, and the way we look at the world. Economically speaking, technology companies founded in just the last 30 years have created an aggregate capital value of close to a trillion dollars,* making technology a major engine driving the U.S. economy.

The foundations of these advances are the funding of basic scientific research and the entrepreneurial spirit in the United States. After World War II the United States poured money into basic research, creating a strong and vibrant scientific community. It is no accident that Silicon Valley and other high-tech enclaves in computing and biotechnology are largely U.S. phenomena. Technology businesses grew up in the shadow of great universities and research institutes, and the apple did not fall far from the tree

Given this tremendous success, one might expect that basic science would receive commensurate support. Sadly, this is not the case, and indeed almost the opposite seems to be occurring. Basic research within industry is no longer fashionable—the great corporate labs of the past few decades have been subjected to cuts in funding and corporate breakups. Those that remain are often downsized, or have turned away from the challenge of profound discovery and toward immediate application. This trend does not come only from the boardroom—some science writers go even farther and pronounce basic science dead altogether,† an observation given intellectual backing by researchers, who document the decline of unfettered research.‡

Investing time and money into applied research and product development is important, and the challenge of reducing knowledge to practical applications in the form of a product is an intellectually satisfying pursuit and an obvious necessity to industry. I could harldly think otherwise, because I spend a fair amount of my own time in applied research. But technological progress cannot continue without the input of basic research and the conceptual breakthroughs it makes possible. In order to reduce knowledge to practice, one must have the knowledge in the first place. Science is the raw material that applied research and engineering refine into their products.

While older companies cut back on research, younger companies, born of the current technological revolution, simply ignore it. Apart from a handful of exceptions, the new technological companies in the Silicon Valley mold do not invest in long-range research. Start-up companies cannot afford it, and those well past the start-up stage may have the resources, but are not inclined to use them for basic research. An ironic example is the personal computer revolution, which was based on research done in industrial labs, notably the Xerox Palo Alto Research Center (PARC). Despite this undeniable origin, personal computer companies have not sought to renew the source of their success. Indeed, it is widely accepted in business circles that labs like Xerox PARC are a mistake because Xerox failed to capitalize on its invention of the personal computer.§ Xerox researchers invented the laser printer at PARC during the same time period, and the profits from laser printing and other inventions that they did capitalize on have more than repaid their investment in research. Despite this, the Xerox story has given the foes of research a ready rationalization for not funding science.

At the government level science has not been recognized as the wellspring of the technology miracle, and as a consequence support is cut, or worse, is subjected to a protracted dissection and review to see if it is "relevant" to short-term economic goals. This puts government funding bodies in the awkward position of second-guessing both the research and the marketplace. Defense-related funds have traditionally supported a wide range of long-term research. For example, the Internet, surely one of the most dynamic business and social developments of the decade, sprung from ARPANET, a network supported for a quarter century by defense funding|| before it blossomed into the Internet as we know it today. A less patient source might have cut the support; where would the Net be today if they had? With the end of the Cold War these defense-related funds have dropped dramatically in many fields. Peace has not been good for science.

Scientific research is by nature an uncertain undertaking. Like any exploratory process, it is not possible to predict what one will find or what its eventual utility might be. That is the whole point of research—investigating what we do not know. In other walks of life we have come to grips with the notion of stochastic phenomena that are not individually predictable, but can be tackled

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*"The Fortune 500," *Fortune* (27 April 1998). †J. Horgan, *The End of Science: Facing the Limits of Knowledge in the Twilight of the Scientific Age* (Addison-Wesley, Reading, MA, 1996). ‡A. Odlyzko, "The Decline of Unfettered Research," available at *http://math.washington.edu/Commentary/science.html* (4 October 1995). §R. C. Alexander and D. K. Smith, *Fumbling the Future: The Story of Xerox and Personal Computing* (Morrow, New York, 1988). ||K. Hafner and M. Lyon, *Where Wizards Stay Up Late: The Origins of the Internet* (Simon & Schuster, New York, 1996). ¶G.W. Hardy, *A Mathematician's Apology* (Cambridge Univ. Press, London, 1967). #W. Alvarez, *T. Rex and the Crater of Doom* (Princeton Univ. Press, Princeton, 1997). **National Center for Health Statistics, *Vital Statistics of the United States*. ††*Budget of the United States Government Fiscal Year 1999*. ‡‡"The Future of American Science," *Science* 1, 1 (February 1883).

ILLUSTRATION: ALLAN M. BURCH

as a group. Mutual fund managers do not expect every investment to have the same return, nor do actuaries expect every person to live precisely to the life expectancy. The whole point of a mutual fund, or an insurance company, is to create a portfolio of unpredictable entities that in the aggregate yield to statistical prediction. Given a sufficiently large portfolio of research projects, and enough time to bring them to fruition, the track record is clear: Science is a tremendous, and very predictable, investment.

Yet this is not how science is viewed by those who fund it in government or industry. There, projects are subjected to a scrutiny wholly inappropriate to their nature. The most reliable way to get research funds is to predict the research results up front, to guarantee low risk of failure, and to present a clear and certain path from results to great commercial utility. The trouble is that a research proposal that meets those criteria, and meets the additional burden of being readily understandable by a congressman or funding official, is almost certainly not worth doing—at least as basic research. It is rare for ambitious basic research to make it through the gauntlet of second-guessing. Well-intentioned conservatism can eviscerate the very essence of what science is about. This is particularly true of the trend toward "relevant" research aimed at near-term application. Applied work can be very valuable, but all too often what the process selects is timid research—an awkward hybrid that is neither good science nor good product development.

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There is no useless research. Many discoveries reach their full potential, given enough time. In some cases this potential is a direct commercial reward; in others the rewards are intellectual: New vistas are opened and new avenues for inquiry inspired, and no matter how "pure" an area of research, the odds are that it will eventually contribute to our understanding of other aspects of science—or even to everyday life—in ways that even the researcher cannot always predict. The British mathematician G. W. Hardy opined¶ that his work in number theory and complex analysis would be forever useless, yet today complex analysis is central to modern engineering, and number theory is the basis of coding theory and cryptography.

My favorite example of unexpected utility is dinosaur paleontology. What could be more useless than studying these extinct giants? Recent work on the mysterious extinction of the dinosaurs has built a credible case that their demise was caused by the impact of an asteroid or comet.# Although this explanation remains controversial among experts in the field, the inquiry has sparked the realization that a future impact by a near-earth asteroid could kill millions of people, destroy civilization, or even drive our species to extinction. Active research is now focused on this threat and on technological means to avoid it. It is thus entirely possible that the "useless" study of dinosaurs might some day, decades or even centuries from now, lead to saving the human race.

Meanwhile, the entire cost of funding dinosaur paleontology, from its inception in 1841 to the present, is less than the production cost of the film *Jurassic Park*. Paradoxes like this abound. Defense spending at some level is surely needed in this dangerous world, but consider that cancer kills a half million Americans every year**—more than were killed in both world wars combined. If a foreign enemy inflicted those casualties, we would be up in arms. Nevertheless, research spending is only about 1 to 2 percent of what is spent on the defense budget.†† Can such priorities really be correct? Consider further that dramatic advances in genetics and cancer research suggest that cures for cancer and many other diseases are within our grasp—perhaps within the next decade at the current rate of research advances. Yet in that intervening time 5 million people will die prematurely. Would it not make sense to spend another percent or so to accelerate the cure and save them?

If one asked business executives whether a company that might not be around in a year or two should do long-range research, the answer would be a resounding no, and rightly so. However, the converse of this argument is equally true—a technological company that expects to survive and thrive decades hence is losing money and opportunity if it does not have long-range research programs.

The same holds true for governments or societies as a whole. Even the most fiscally conservative politician should realize that supporting science makes money and brings tangible nonmonetary benefits. The technological nature of the modern world has moved support for science from a "want to have" squarely into the "need to have" column. One cannot expect corporate shareholders to support all fundamental research because some of it may take 50 to 100 years to be applied, and by then tax laws and other uncertainties make it unlikely that today's shareholders will be the direct beneficiaries. A government, on the other hand, should consider the longer time scales and make investments accordingly.

In 1883 *Science* published an essay entitled "The Future of American Science."‡‡ The essay is an optimistic call to arms celebrating the advances of Americans like Agassiz and Henry, who made an impact in a scientific community that was still dominated by Europeans, and predicting the growth of American science. The essay concludes as follows:

> The year 1883 opens auspiciously. The Scientific sky is clear, and the outlook promising. If true to itself and to its surroundings, American science has nothing to fear from the future. With the increase of a generous people, and the spread of intelligent scientific thought, it has everything to hope. Under these favorable circumstances, *Science* enters upon its career. May it early recognize the conditions of this certain progress, and ever be on the alert to help it forward.

With the vantage of 115 years of progress behind us, we know that science, both in the United States and around the world, did in fact blossom to a degree beyond the wildest dreams of this optimistic essay.

Will the final days of the 20th century be as auspicious as those of the 19th? Although the opportunity is clearly there, it is hard to muster the unbridled optimism of the earlier age. As a society we are shirking our support for basic science at the very time when our previous support is reaping great returns. In doing so, we jeopardize not only our legacy of scientific achievement, but also the economic prosperity of the near future. It is clear that we *can* afford to spend more on science. It is also clear that we *need* to spend more if we want to continue to enjoy a technologically based economy. The missing elements are the will and the vision to bet on the scientific enterprise, vital to the realization of the full potential of the next millennium.

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