Problem Solving in Complex Societies

Neal F. Lane

Today I want to share a few thoughts about a system that delivers with remarkable consistency. We know our society today is complex, as are the problems we face collectively. But we also know we have a system of problem solving that works. It is tested by history. It is a system based on strong, stable public support for research and education across the spectrum of science and engineering.

Today I intend to first discuss the historical motivation for this highly successful system. This discussion will take us back a few centuries, but it also should bring to light a few points that are especially relevant as we approach a new century.

Then I want to turn to the present day and examine our current conundrum. We can all see that this is a remarkable era for discovery and opportunity in science and engineering, mathematics in particular. Yet we also sense tremendous uncertainty and apprehension—notably regarding the budget outlook and the job market for young (and not-so-young) mathematicians and other scientific professionals. Both of these situations deserve our immediate attention and action.

The National Science Board is currently revisiting the NSF's merit review criteria. Merit and impact form the central focus for the new criteria. This is not new at NSF. Indeed these two concepts loom large over the history of public support for science. To make this clear, I would like to recall the exploits of Admiral Sir Clowdisley Shovell. Who was he, you ask? Admiral Shovell was a heroic commander in the British Navy in the early eighteenth century, but he figures into the history of science, mathematics, and technology for other reasons.

In the fall of 1707 Admiral Shovell led his fleet of five gunships to triumph over the French Mediterranean forces at Gibraltar. In the wake of this victory, he sailed his fleet toward home, expecting a hero's welcome for himself and the thousands of troops under his command.

But when they sailed within twenty miles of the British coast, disaster struck. Four out of five ships were sunk, and over 2,000 lives were lost.

This disaster was not the result of a trap laid by the enemy. It was not caused by storm or sabotage. The culprit in fact was the single greatest challenge facing seafaring nations of the day. It was longitude—or to be more precise, an inability to determine longitude. Unable to navigate by sight because of fog, the ships struck rocky shoals off a small group of offshore islands.

There is much more to this story—including an ironic twist involving Admiral Shovell's own fate as he washed ashore. It is all recounted in a book that has reached the bestseller lists without much fanfare. The book is entitled *Longitude*, and its author is Dava Sobel, a science writer formerly with the *New York Times*.

You will recall from your studies of geography that lines of latitude parallel the equator, and capable sailors can determine latitude at any

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point on the globe through calculations based on the position and height of the sun above the horizon, say at high noon on a given day. It's a tricky calculation, but the basic methods date back to the third century B.C.

Longitude, by contrast, comprises the great circles of the planet that intersect at the poles. There is no longitudinal analogue to the equator. The prime meridian, now at Greenwich, is set by humans, not by nature. Longitude consequently defies simple determination. Celestial navigation provides one method—but one requiring data, calculations, and difficult shipboard observations—all beyond the abilities of most eighteenth century sailors. Another method requires knowing the exact time at two places on the globe at once.

In the days before quartz watches and instant communication, this was no simple determination. The absence of timepieces that could remain accurate over months at sea proved the undoing of many great sea captains. Sobel writes that every great sea captain of the era of exploration, from da Gama to Balboa and Magellan to Drake, became lost through an inability to gauge longitude, though most were not in such dire straights as Admiral Shovell.

For this reason, the story of longitude is a story of scientific research being enlisted to address a societal challenge. Sobel writes:

> The active quest for a solution to the problem of longitude persisted over four centuries and across the whole continent of Europe…Renowned astronomers approached the longitude challenge by appealing to the clockwork universe: Galileo Galilei, Jean Dominique Cassini, Christian Huygens, Sir Isaac Newton, and Edmund Halley, of comet fame, all entreated the moon and stars for help. Palatial observatories were founded at Paris, London, and Berlin for the express purpose of determining longitude by the heavens…
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> In the course of their struggle to find longitude, scientists struck upon other discoveries that changed their view of the universe. They include the first accurate determinations of the weight of the earth, the distance to the stars, and the speed of light.

Sobel also pointed out that the quest for longitude also marked the first large-scale investment of public treasuries into science and engineering research. European governments offered generous prizes for workable methods. The British Parliament's Longitude Act of 1714 set a prize of 20,000 pounds for a reliable method—a sum that translates into several million of today's dollars.

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The prize eventually went to John Harrison, a brilliant clockmaker with no formal training, but extraordinary skill and tenacity, in a result that miffed the scientific establishment of the day. Harrison's timepieces earned the name "chronometers", a term still reserved for only the most accurate timepieces. The astronomical approach, based on exhaustive mappings of the heavens, has remained a valuable navigational aid, but it never proved practical as a standalone method for determining longitude. It is nevertheless noteworthy that even after nearly three centuries, the charts Edmund Halley and his contemporaries developed in their quest for longitude remain among the most accurate accountings of the stars and planets ever produced. And obviously mathematics played an important role, but also was stimulated by all this activity, as well.

While I cannot do justice to the richness of this story and the complexity and human struggle behind all of these accomplishments, there is one valuable moral I would like to pull from this story. We see here research responding to society's need and at the same time sparking progress in both fundamental science and the development of new tools and technologies simultaneously—and in a mutually reinforcing way!

This same storyline emerges from countless other great quests we have tackled as a society—such as putting a human on the moon, battling polio and cancer, and securing victory in World

War II. In these and countless other areas, we have risen to the call of great societal challenges, and at the same time opened new frontiers for exploration through research and education.

This storyline runs to the core of the origins and purposes of the highly successful system of research we enjoy here in America. It's no secret that our system of public funding for research emerged from the contributions of science to the Allied victory in the Second World War. This connection between societal goals and scientific progress is also evident in the mission of the National Science Foundation. Our enabling legislation directs us "to promote the progress of science [and engineering]" and "to advance the national health, welfare, and prosperity..."

This duality of purpose is one of the secrets to the success of our system of science and engineering. It allows individual initiative and creativity to flourish without rigid centralized control and at the same time works to achieve larger national objectives.

One often hears terms like "basic" and "applied" attached to research, implying that one form has utility and the other does not—and that we can tell the difference. What we have learned from history is that research defies any such pigeonholing. More often than not research opens new frontiers for exploration and improves the quality of our lives simultaneously. What really matters in discovery is the quality of the researchers and their having the freedom to explore wherever their minds take them.

Mathematics is replete with examples that testify to this. The fundamental mathematics developed to understand surfaces, matrices, and complex geometries made possible advanced computer graphics, visualization, and computer-assisted design technologies among other things. These are what enabled Boeing to roll out its new 777 jumbo jet without ever building a mockup—saving millions along the way while setting new standards for safety and reliability.

Other examples abound. All of us here today know it took giant leaps forward in algorithms and data management in order to view magnetic resonance images of the brain in real time. A team at the Pittsburgh Supercomputing Center did this for the first time this past November, with support from NSF's Division of Mathematical Sciences. In this same way, it took progress in such areas as systems theory and cutting-edge statistics to shed light on population dynamics and the underlying behavior of ecological systems.

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Of course, I know that when some people hear about these improbable connections between fundamental research and societal benefits, they say, "that's just luck." They say it's not something you can ever expect, let alone use as an investment strategy for public monies.

I prefer to think of these examples in light of Louis Pasteur's observation that, "chance favors the prepared mind." New York Yankees manager Joe Torre came up with another way to explain this in the wake of his team's surprising World Series victory. To use his words: "The harder you work, the luckier you get."

We have learned from history that the hard work made possible by our system of support for science and engineering brings our nation much more than just good luck. It in fact brings the good fortune of progress and prosperity. There is no need to take my word for this. Consider, for example, the data on U.S. economic growth since World War II. Our real GDP has grown by a factor of six over the past five decades, thanks in large part to scientific and technological progress.

Many top economists, including a number of Nobel laureates, have studied in depth the drivers of this growth—and they've come to one clear conclusion. Innovations emerging from science and technology account for roughly one third, maybe more, of all economic growth over the past half-century. That's strong evidence, but I would argue that it's only the beginning.

That brings me to the last part of my talk and our current conundrum. When we reflect back on the twentieth century, we see an amazing array of advances—air travel, computing, the Internet, rising living standards, increased longevity, and a cleaner and healthier environment, to name but a few. It is hard to believe we can possibly improve on this record, but it looks like the best may be yet to come.

In just the past year, the centuries-long quest to prove Fermat's Last Theorem has met with success, and supercomputers have broken the teraflop barrier, finding ever-larger prime numbers along the way. We've also witnessed the discovery of planets orbiting stars beyond our solar system, gained new insights into the development of life on Earth—and possibly on other planets as well (I won't ask for a show of hands!)—and, we've launched a national effort to move education into the twenty-first century by connecting schools and classrooms to the Internet.

All of this makes our current situation all the more confusing and confounding. We can see that we are standing just a step away from an amazing era of possibility and opportunity. It is all made possible by progress across the spectrum of science and engineering—from computing to cognition and mathematics to manufacturing.

It is therefore both ironic and somewhat frustrating that just as these possibilities have come within reach, all signs are that we are stepping back from the promise they hold and perhaps squandering our best hopes for the future.

Erich Bloch, a former vice president of IBM and one of my distinguished predecessors as NSF director, recently wrote in *Science* magazine that: "The whole U.S. research and development system is in the midst of a crucial transition. Its rate of growth has leveled off and could decline. We cannot assume that we will stay at the forefront of science and technology as we have for fifty years."

Erich's observation comes home to all of us in human terms. At meetings like this one, the queues for job interviews and openings seem to get longer and longer every year.

This is occurring in virtually all of science and engineering. It is perhaps most acute in the mathematical and physical sciences, though no fields are immune. Where twenty years ago the majority of science and engineering Ph.D.s embarked on academic careers after graduation, today a majority pursue careers outside the academy.

I know that the AMS's own surveys suggest the situation has improved over the past year. The latest survey found that some 9.4 percent of the most recent class of Ph.D.s was unemployed as of this past September—not a good number. But, a year ago, the number was close to 15 percent, so it has come down by one third. But one year is not much of a trend, so we can't assume all is well.

In fact, it is the long-term trends that give us the most to think about. The Survey of Earned Doctorates sponsored by NSF's Division of Science Resources Studies makes this clear. This survey brings to light an important set of trends over the past twenty or so years that deserve our attention.

The share of recent mathematics Ph.D.s entering postdocs has jumped from only 5 to 6 percent in the early '70s to roughly one in four today, while the percentage obtaining immediate employment has dropped by a corresponding amount.

We've also seen startling increases in the numbers of students with no definite work or study plans upon graduation. One expects this of high school graduates, but not for superbly capable people who have just spent as many as seven years obtaining an advanced degree in an analytically rigorous and technologically demanding area like mathematics. That's not the way things are supposed to work.

Again, while no one of these trends is in itself alarming, collectively they send a clear wake-up call. Programs and policies at both federal agencies and universities have been slow to recognize and respond to these shifts, but we are now beginning to see promising signs of progress and change.

Most of these efforts are occurring at the disciplinary and departmental level. I know Don Lewis and NSF's Division of Mathematical Sciences encourage these efforts however they can. We've been getting favorable reports, for example, about mechanisms we've established that enable graduate students and postdocs to spend time in industry—such as the GOALI program, short for Grant Opportunities for Academic Liaison with Industry.

Unfortunately, when it comes to developing creative approaches to addressing these and other challenges, we know our hands are tied somewhat. Whatever approaches we adopt must work within the confines of a highly constrained budget environment. That should be old news to everyone in this room. The president will release his proposed budget for fiscal year 1998 in just over three weeks. I can't reveal any budget details, but it's no secret that the push to balance the budget severely limits the prospects for growth in any federal program.

When it comes to research funding, I often tell people the devil is not just in the details, it's in the totals. There is a lot of talk about what the future holds for science funding. You can even find detailed projections being tossed about for what NSF's and other agencies' budgets will look like in the year 2002. While those figures get our attention, they are not the most reliable of projections. We should not place great stock in the levels for individual agencies. The actual funding allocations are revisited each year by the president and the Congress in the budget process.

Nevertheless, the aggregated totals projected for the major categories of federal spending do deserve our attention, particularly the category known as domestic discretionary spending. This includes most of what we think of as the day-to-day running of the government parks, highways, prisons, NSF, NASA, education, and scores of other programs and agencies. You might be surprised to learn that this category makes up less than one sixth of the total federal budget.

Even more surprising and of real concern is that this small slice of the pie is slated to bear the lion's share of the spending reductions needed to balance the budget. In fact, this one-sixth slice of the pie is expected to drop to one seventh of the pie by 2002 according to most projections. That reflects a decline in purchasing power of some 15 to 20 percent, depending on whose CPI you like best.

Again, while we can't predict with any precision how this will affect NSF or any other agency, we do know that there will be increased competition for funds from this shrinking slice of the pie. We also know that for several decades, federal support for research and development has tracked very closely with total domestic discretionary spending.

It would be folly to ignore the possibility that the federal investment in research, including that in universities, could decrease in real terms by up to one fifth over the next five to ten years if trends continue as they are now. In a way, our nation is getting ready to carry out an experiment it has never run before: to see if we can significantly reduce the purchasing power of federal research investments and still be a world leader in the twenty-first century. That is a high-risk experiment.

Let me leave you therefore with a few thoughts on how to approach this confusing and confounding conundrum. We must first recognize that the long-term threat to science is real. The drive to balance the budget will bring some rocky times, and all of us have good reason to feel apprehensive about the future.

The key is that we cannot let our apprehension slow us down. I believe we will have a future golden age of science, but it will be much more than just a reflection of past glory.

I believe that scientific research will continue to explore the most fundamental questions of nature. And mathematics will continue to drive and catalyze progress across all of science and engineering and provide tangible benefits to our society.

I nevertheless predict our system of research and education will do much more than this. In a future golden age, research will also emphasize the integration and dissemination of knowledge beyond publishing in journals and presenting papers. We will rely on yet-to-be-established networks (not just electronic) of discoverers and users. This new partnership will make the benefits of research more apparent and, at least some of the time, more immediate.

Higher education, particularly at the doctoral and masters levels, will include practical knowledge and skills, such as communication, teamwork, management, and leadership that will enable more graduates in science and engineering to excel in a wide range of professions as they face the realities of today's and future job markets.

But perhaps more important is the fact that future leaders in the world of business, law, medicine, and politics will need to understand science and technology to a degree society has never recognized and certainly not required before.

Will all of this come to pass? I don't know. But it will not come to pass unless we expand our views of research, of the university, of connections and partnerships involving the doers and users of science, and of graduate education.

Thanks to your work, our nation has realized the benefits offered by science and technology, and what we've seen to date is only the beginning. We will see even greater returns in the future—provided we muster the national will to press forward and continue extending the frontiers of research and education.

From the story of longitude and countless other examples that include the extraordinary contributions of mathematics to our economy and society, we have learned a valuable lesson. When we extend the frontiers of research, we also move forward as a society, as a civilization, and we plant the vital seeds of human progress and prosperity.

That is an ideal system of problem solving for our complex society. Now, it is up to all of us working together to ensure that this system is better understood and that it remains fully vibrant and vital as we move into the twenty-first century. That is a challenge we must not fail to meet.