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'Absurd' studies of science's puzzles prove their worth

By Richard C. Atkinson and Michael J. Feuer

With much public and political attention focused on the links between scientific capacity, productivity, competitiveness and national security, it is reassuring that Congress is scrutinizing research budgets and demanding that funding agencies allocate scarce research dollars wisely.

But while it may be tempting to denounce wasteful spending on studies that appear nonsensical or disconnected from the nation's most urgent problems, history suggests caution. In one memorable example, now going back 30 years, research on the sex life of the screw-worm fly did not look like a good use of public dollars, and easily became the butt of political attack humor. The National Science Foundation won a "Golden Fleece Award" for funding that research, but years later even Sen. William Proxmire (the longtime Wisconsin Democrat who passed away last December), who had established the award to expose waste and fraud in public programs, acknowledged that the screw-worm's sexual habits were a key to unraveling the biology of pest control.

A seemingly ridiculous study contributed to major gains in agricultural safety and productivity, and in retrospect people were glad the project had been federally funded.

The social sciences are especially vulnerable to the critique of irrelevancy. (Some projects are decorated with insufferable ideological jargon, which further annoys the skeptics.) Should the government have supported research on why monkeys clench their jaws? Did we really need a study of why humans smile?

On the surface, such projects may appear frivolous. But it turns out that the former study contributed to understanding and coping with aggression, especially in tight quarters like submarines and space stations; and that the latter project led to research in facial-recognition techniques with potentially important applications in screening and security.

The simple fact is that many studies do not yield immediately useful findings, regardless of whether they are in the social, physical, biological or medical sciences; but one cannot know with certainty, in advance, which will be winners and losers.

Picking winners is especially difficult for social and behavioral science, which frequently addresses the most complex, multidimensional, value-laden, vexing and persistent problems.

Consider, for example, research on poverty and its relationship to educational achievement. If you think the sex life of screw-worms or the mandibular habits of monkeys is complicated, try untangling the historical, cultural, economic and even biological correlates of

economic inequality. Then try finding the roots of persistent achievement gaps between students of diverse ethnic and racial backgrounds, and figure out if:

- Poverty causes low achievement.
- Underinvestment in education causes poverty.
- Poverty and low achievement are both caused by other environmental or genetic factors.
- All of the above.

Fortunately, Sens. Kay Bailey Hutchison, R-Texas, Frank Lautenberg, D-N.J., and others in Congress have, for now, decided against slashing NSF's social and behavioral research budget. This was a good decision, reached after a rather testy debate, and they should expect good returns on continued investment — even if results take time and are sometimes tentative.

On the whole, the evidence is clear that funded research has paid off. We know much more today about poverty and education, and also about many oth-

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er complex problems that bear directly on economic and social well-being, than we did 50 or 100 years ago.

Today's knowledge in these and other areas stems from years of patient research, which could not have been conducted if public agencies had refused to take a chance on a wide range of studies — even those that may have at first sounded strange. Part of the beauty of science — which also makes it such an elusive target for rational public investment — is its uncertainty. Science is about inquiry and discovery, unearthing the unanticipated, spotting surprising relationships in complex systems, confronting and coping with divergent streams of evidence, looking at data skeptically, and allowing knowledge to accumulate over time.

Subject science to a calculus of low-risk return on investment and you quickly dry the well that irrigates our economic and technological garden.

That said, we can still do a better job of estimating in advance the potential value of different research studies; we applaud the administration's commitment to the development of a "science of science policy," the idea being to improve methods used to estimate likely benefits of diverse scientific ventures. Finding the right balance between the virtues of unfettered science and the legitimacy of public accountability is not easy — but lurching toward either extreme would be irresponsible and shortsighted.

We hope the friends of science will stay the course even against rising tides of doubt, and, especially in this age of evidence-based policy, reinforce our nation's commitment to data collection, experimentation and scientific analysis of complex behavioral and social systems. The alternative will be long-term erosion of our capacity to solve real problems, for the sake of trivial savings in the short run. It doesn't take rocket science — or social science — to know that would be a tragic miscalculation.

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