

A delicate balance

Near-term and long-term research are vying for attention. **David Goldston** says that a fuller congressional debate is needed.

Most of the debate on research policy in Washington DC is over how much to spend, but there is a growing, if inchoate, discussion about what type of work to support. As is often the case, the trend can be spotted through the increasing appearance of buzzwords; two current examples are the somewhat opposing notions of 'translational research' (shorter-term work focused on coming up with new products) and 'transformative research' (longer-term work aimed at coming up with new ideas).

Discussions about whether the federal government is supporting the right types of research crop up periodically, usually at times of economic distress. The underlying question is always: 'is the country gaining the greatest possible practical benefit from its research investment?'

Academics can resent this question, longing for an era when research was supported for knowledge's sake. But such a time never existed. The foundational document for US science policy after the Second World War, Vannevar Bush's 1945 report *Science: The Endless Frontier*, begins with these words: "We all know how much the new drug, penicillin, has meant to our grievously wounded men." Bush, the overseer of US wartime research, promised that federal funding will "bring higher standards of living, will lead to the prevention or cure of diseases, will promote conservation of our limited national resources and will assure means of defense against aggression".

But it's not obvious how to run a research enterprise to maximize such benefits, and policy-makers continue to fiddle with the system. In the 1980s, for example, spurred by the fear that US competitiveness was being undermined by Japan, Congress passed bills designed to help translate research prowess into commercial success. The 1980 Bayh-Dole Act, for instance, made it easier for universities to patent the results of federally funded research, encouraging the growth of spin-off companies, especially in biotechnology. No one called that translational research, but the goal was the same.

The current interest in biomedical translational research reflects not only the resurgence of concerns about US competitiveness, but also worries about whether the drug-discov-



PARTY OF ONE

ery pipeline is drying up and a desire to show that the doubling of the budget of the National Institutes of Health (NIH) between 1998 and 2003 can produce measurable results.

That those issues are specific to biomedicine is one reason there has been much less talk in Congress about translational work in the physical sciences, even though the gap between basic research and commercial advance is generally considered greater in the physical sciences. A nanotechnology bill passed last week by the House of Representatives does authorize funding for partnerships between academia and industry to conduct research in 'areas of national importance' (including health care), but that would still be early-stage work. And in general, the prescription for the physical sciences has just been for increased spending on conventional basic research.



Discussions about taking further steps in the physical sciences inevitably lead to ideological debates about the roles of government and industry, with conservatives branding efforts to use taxpayers' money to translate research into products as industrial policy — a derisive term that began life as a positive buzzword in the 1980s. Somehow in biomedicine — perhaps because government, academia and industry are so obviously entwined, perhaps because health is seen as a legitimate government concern — such ideological debates are absent or muted.

Translational programmes in biomedicine are also being exempted from another standard line of attack. They can be seen, often by researchers themselves, as being too focused on near-term, incremental work. It is curious,

then, that translational research in biomedicine is becoming all the rage just as reports are calling for an emphasis on transformative or high-risk, high-return research. For example, a report released last week by the American Academy of Arts and Sciences (for which I sat on the panel) argued that the priorities in biomedical research should be more funding for transformative research and for early-career researchers (see page 953).

Theoretically, the research system could simultaneously give special attention to shorter-term research to convert ideas into products, and to longer-term research to generate novel ideas, but that's a hard balance to pull off, and the political debate has tended to stress one or the other. The NIH's own efforts highlight what happens with insufficient focus. Its Pioneer awards are designed to fund transformative research, but the programme is so small that fewer than 4% of applications win funding even though the number of applications plummeted as word spread about the poor odds. The NIH last week announced that it would increase funding for transformative programmes (see page 835), but the money still may be spread too thinly.

A new bill by Senator Joe Lieberman (Independent, Connecticut) also shows how policy tends to focus on just one end of the spectrum. The legislation is designed to further translational research, although it also would create a new programme for longer-range research targeted at specific health problems. With its focus on results, the bill would require every grant application to the NIH to state how the research could be used "for detecting, treating or curing" a medical condition. Such a mandate might help to ensure that basic research translates into advances in public health. Long-term and even transformative research can be targeted at solving specific problems, as Bell Laboratories proved, gaining Nobel prizes while inventing the transistor. But if viewed narrowly, the provision could easily stymie inventive work that is not far enough along to be associated with a specific medical condition. It hardly seems like the cure for an agency that is often criticized as being too conservative.

But the Lieberman bill could open up a useful debate on how to develop a better-balanced research enterprise — one that keeps its eye on real problems without becoming risk averse. That discussion needs to take place irrespective of what happens to overall spending levels. And it has to consist of more than throwing around contradictory buzzwords. ■

David Goldston is a visiting lecturer at Harvard University's Center for the Environment. Reach him at partyofonecolumn@gmail.com.

See also Editorial, page 823 and online at <http://tinyurl.com/3tt3y3>