



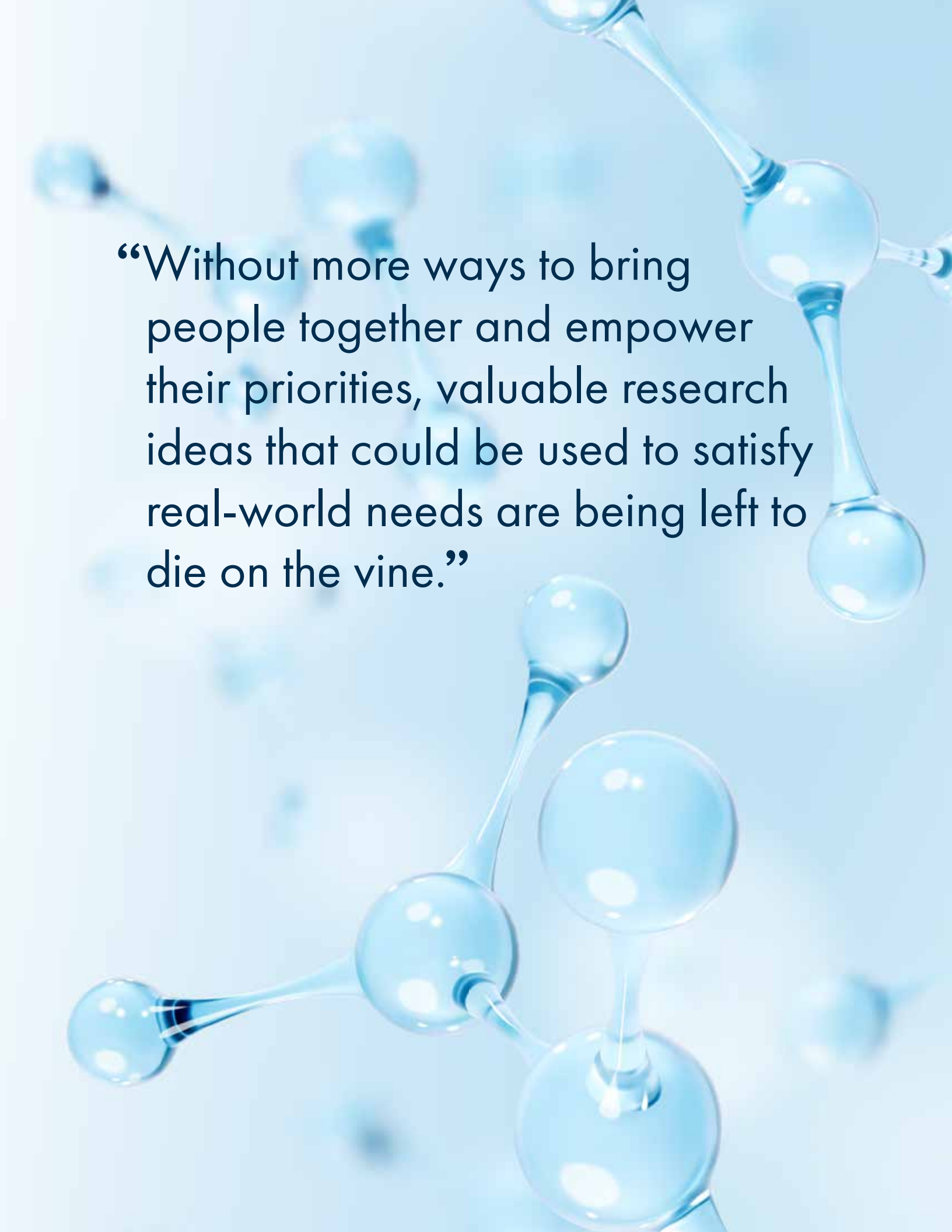
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The New Gap in America's R&D Funding Landscape

An Essay by Melissa Flagg, PhD

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Melissa Flagg is the founder and president of Flagg Consulting LLC. She is also a fellow at the Acquisition Innovation Research Center (AIRC), a visiting fellow at the Perry World House, and a senior advisor to the Center for Security and Emerging Technology (CSET) at Georgetown University where she was previously a senior fellow. Prior to this, she served as the Deputy Assistant Secretary of Defense for Research, responsible for policy and oversight of Defense Department science and technology programs. She has worked at the State Department, the Office of Naval Research, the Office of the Secretary of Defense for Research and Engineering, the John D. and Catherine T. MacArthur Foundation, and the Army Research Laboratory. Dr. Flagg has served on numerous boards including the National Academy of Sciences Air Force Studies Board and the Department of Commerce Emerging Technology Research Advisory Committee. She is currently on the Board of World Forest ID and the Advisory Board for the Andrew W. Marshall Foundation. She holds a Ph.D. in Pharmaceutical Chemistry and a B.S. in Pharmacy.

The New Gap in America's R&D Funding Landscape


America invests a lot of federal money into academic research, and the government employs well over a hundred thousand scientists and engineers. This has reaped tremendous benefits for the economic power of the nation. However, as this system has matured, it has become more about profit and prestige than it has about making the lives of people better. A gap in the system has emerged and that gap is in harvesting the results of our tremendous research power to not only inform but solve the problems that are most important to its citizens. At some point, America stopped believing it was worthwhile to fund local and regional solutions that aren't one size fits all.

If Vannever Bush, whose *Endless Frontier* remains the guidestar for our scientific system, were here today, he would probably be confused that we continue to focus on doubling down on a strength while ignoring new gaps that have emerged. He would want America to ensure it is solving its current problems. A commitment to Bush's prescriptions for the problems and gaps of 1945 overlooks the fact that the country has fundamentally different resources and gaps today.

The scientific community, including funders across sectors of government, philanthropy, and industry, seem to focus on two versions of success: novelty or scale. They bestow awards and grants on those who show either "revolutionary" new ideas or those who purport to solve a problem for millions that can make someone rich. Anything else falls into this category of incremental and it is dismissed. Somehow harvesting the science we have already invested in to solve individual, local, or regional problems that don't necessarily lend themselves to market rewards is not incentivized. We owe it to American communities to address longstanding and emerging goals and concerns that may not have clear market drivers and may require diverse approaches, such as challenges of clean water and sanitation; drought, flooding, and wildfires; crumbling infrastructure; preventable chronic diseases; opioid addiction—the list is long. The details of these concerns differ across communities, so solutions need to be localized.

A big gap that seems to have grown unintentionally out of the binary focus on novelty and scale is that there are not enough mechanisms and platforms that communities can use to help set a research agenda that prioritizes their real-world challenges. Without more ways to bring people together and empower their priorities, valuable research ideas that could be used to satisfy real-world needs are being left to die on the vine. This research may not produce big profits for anyone but if supported could improve community resilience and quality of life. Connective tissue is missing between the ideas, inventions, and innovations produced by research funding and the problems faced at local, state, and regional levels.

One reason for this is that the largest science funding agencies (NIH, NSF, DOE, DOD, etc.) are centralized. Even where policy and advisory processes incorporate broad input when prioritizing research domains, a small number of program managers ultimately decide which topics and questions are included in grant solicitations. Despite their best intentions, these program managers are ill equipped to articulate localized questions and are often incentivized to solve "bigger" challenges. This leads them to roll up specific challenges into questions that may feel representative but do not solve any specific problem. This centralized approach favors research that is abstract and theoretical and pays little attention to building



relevant intellectual capacity in regions that need solutions and certainly does not fund those solutions.

When the federal government does tackle tangible issues, it often cannot account for their local and regional aspects. This limits the applicability of research output. Just to take the first example above—the rural water sanitation problems that plague so many U.S. communities—questions about target microbes, priority climate zones, and infrastructure solutions will be location dependent. That does not even cover all the water and sanitation issues: in an urban environment, the pathogens and infrastructure are different. Across jurisdictions, the money and local skill base available to tackle the problem will be different, as will the applicable regulations. Similarly, climate change manifests in myriad concrete local problems: wildfire control in California, flood control in areas as different as New York City and Louisiana, drought-tolerant agriculture in Arizona, and sustainable fishing as habitat zones change on the coasts.

Water and climate hardly cover the regional differences within the U.S. Midwesterners may be more concerned about the opioid crisis, which is radically changing both the workforce and families. Some areas focus on border security. Others fear terrorism because they have a nuclear site nearby. Still others care about economic security because the manufacturing base has been eroded and they worry about supply chains. There are no mechanisms, networks, or incentives to drive research programs toward tangible benefits at the local and regional level.

These mechanisms are missing because the U.S. R&D ecosystem is largely driven by profits and prestige. The former motivates industry, which measures success in terms of earnings and shareholder returns. The latter motivates academia, which measures success in terms of publications in high-impact journals and renewed federal grants. Once a researcher finishes a project and publishes a paper, they go on to the next proposal—the next big, new idea, constantly chasing novelty, the bleeding edge of science; or they found a startup and chase profit.

The focus on novelty detracts from learning and applying lessons. A publishable solution may theoretically be effective, but follow-up work lacks prestige because it is seen as incremental. It is vital to learn whether the invention was real and sustainable, if it was really an innovation. What local conditions allow the solution to work so it can be applied elsewhere? All of this appears at the end of articles as recommendations for further research, but too often that research is not done, not funded, not published.

What a waste. So many publicly funded ideas are left incomplete; even if patented, many are locked away with no intention of development by the inventor and no incentive for someone else to pick up the idea.

Researchers, if they think about it at all, assume the next steps will be taken by actors motivated by different incentives. It is convenient to think the market will step in to turn an invention into an innovation, but it is hard to make money on some of the most pressing issues: mental health, education, conservation. Safer roads, more reliable water supplies, and more resilient communities are not consumer products.

At times even profit is not enough, researchers flatly ignore business applications. A researcher once asked me to connect his

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work to industry. I reached out to some people in companies my organization worked with, and they said his research used a catalyst that was recently banned in Europe. Although the compound was still legal in the U.S., they weren't interested in his research unless a different catalyst were used so they could export to the European market. I brought this feedback to the researcher, who looked me dead in the eye and said, “We'll just keep doing the research.” They were confident they would keep getting funding because they were publishing. It was technically good science, but it would literally never be useful. Again, what a waste.

The result is discoveries without impact. If there is no obvious route to profit, the coin of industry, or to prestigious publications or prizes, the coin of the academic realm, then people and their problems are left behind. Academia needs to clear a path toward practical solutions that is enticing to researchers. There are incentives for industry profit, there are incentives for academic prestige—but there are no incentives for helping Americans thrive.

Scientific funders in the federal government contribute to this problem by requiring grant recipients to report their outputs, measured in terms of papers produced. In applications for federal funding, determinations of merit typically place high value on the investigators' publications and prizes. Questions of community benefit such as NSF's Broader Impacts are often framed vaguely and evaluated inconsistently.


These metrics must be expanded to include making real-world changes in communities and embracing more qualitative inputs. Funders should require reports on whether the research had the promised effect or what the researchers would look for going forward to indicate that effect was accruing over time. Researchers should explain what they will produce. Will they incorporate it into teaching beyond just their classroom? Will they make curriculum available for free? Will they invite the community into educational conversations so people know what's happening and can give feedback on utility and future directions? Is that sufficient?

Is there a vision for producing something from collective support of science and engineering beyond prestige and profit?

Researchers should directly engage with the community as well. Ask the diabetes researcher whether they spoke to those with diabetes to find out if their ideas are relevant and if there are more important challenges to be addressed. What is the real problem that needs to be solved? What keeps people from getting treatment for something that is treatable? To encourage this, time spent doing direct community engagement, such as working with local governments and community leaders, should not only be an allowable expense on a grant but also highly rewarded during grant review. Requiring reports on direct community engagement efforts and beneficial community outcomes would incentivize local action.

Additionally, federal funders should consider new programs that award small planning grants that fund this type of interaction so that later proposals have a grounding in real world problems and a constituency of champions once the work is done. Beyond teaching and community outreach, partnering social and natural scientists with communities from the beginning will ensure proposals meet the most pressing needs rather than strictly the most interesting science problems.

I'm sure this is uncomfortable for some, and there will be a reflexive defense of foundational science. Let's be clear -- basic science is extremely valuable, but it is simply not the gap in 2024 that it was in 1944. Funders should sustain support for foundational work but make room to nurture the gap that has emerged in the funding ecosystem.



The federal government, or the broader ecosystem, needs to create programs, structures, or institutions that incentivize work on specific regional and local problems. These new programs may be collaborations or traditional public-private partnerships, or they may look completely different. It is possible to imagine new ways of doing business, but it is difficult, and creativity will be needed broadly. For example, the current institutional processes and practices that have developed, especially in universities but even in some companies, around federal grant management make it onerous to accept other forms of funding. Federal incentives thus dominate institutional priorities as well as those of individual researchers—and hinder them from conducting research that is relevant to the communities where they live.

For example, I know a researcher whose work had been published in a high-impact journal and was almost immediately applicable to a local problem. Their state wanted to pay them to try to develop that connection, but the university's initial response was that it wouldn't be enough money to be worth figuring out how to accept the type of available state funds as their processes were all tuned to federal rules. They eventually worked it out, but it took over a year.

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Another researcher was asked by their state to analyze the cost effectiveness of a conservation approach. This practical work was discouraged by an academic system that regarded that work as volunteer service rather than the scholarly output that is prioritized in tenure decisions. Again, a happy ending—they improvised a way to get credit and ultimately tenure—but academic incentives should not make this so difficult.

In many more cases, smaller institutions lack the resources to manage proposals and review processes. This narrows the pool of ideas and potential grantees and deepens the divide between the rich and the rest.

This divide is reinforced by federal barriers that prevent communities from building local research capability and capacity. Most research money is reserved for direct research and cannot be used for developing infrastructure or buildings. But research often requires specialized facilities, so this policy privileges wealthier states, regions, and institutions that can afford to build their own facilities. The Defense University Research Instrumentation Program (DURIP) is a notable exception.

For nonscientists to value federally supported R&D and trust its outcomes, they need to see it in their communities, they need to know it can and will be brought to bear on the problems that matter to them, and they need to know the scientists and engineers who are developing these solutions. The U.S. Department of Agriculture's Cooperative Extension System is a model for this; however, even this program has been shrinking over time. It would be valuable to brainstorm modern approaches to these challenges.

Behind these structural issues lies the issue of trust. Scientific leaders need to understand that we can both support basic science, a system that drives new discoveries and revolutionary change, while also recognizing that this is insufficient for a healthy ecosystem. It can be hard for American science leaders to hear what lay people prioritize without trying to explain why they are wrong. It can be hard for them to trust

people and appreciate that they know their own situation. Ironically, this lack of trust may foster skepticism and a loss of trust by those they are trying to convince. Solving people's problems requires listening to them, not talking at them. Experts are not taught to listen to people who have a fundamentally different type of education and expertise, to hear beyond their grammar and accents. Hear what they're saying, respect it, and value aspects that are not science problems. However, building trust is a high-touch endeavor. Trust doesn't scale quickly and easily, but it can be lost quickly and easily. The desire for efficiency and scale drives one-size-fits-all solutions and kills the ability to value individual people, communities, and even regions.

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One example of organizations trying to elevate community concerns through peer review boards and advisory bodies is the Environmental Protection Agency's National Environmental Justice Advisory Council that includes members with diverse experiences, including community engagement, local leadership, and small business owners. Another suggestion is to match problems and solutions more broadly, beyond the federal government. For example, by creating an independent proposal marketplace where researchers could post white papers or proposals and funders could post their questions and problems. Such a marketplace could incentivize the full diversity of research, encouraging both short-term, problem-focused proposals and long-term, high-risk research on a myriad of specific topics and geographies. This single, streamlined process would respect the time and expertise of researchers, funders, and citizens.

America's diverse problem set is not being effectively addressed or even supported by the current system. Scientists who want to do societally relevant work often cannot find institutional support. Communities and regions that seek research-based solutions to their problems struggle to marshal the needed resources. Policymakers at local and state levels try to navigate untested novel technologies as well as uncharted health, education, and climate related problems but struggle to trust accessible technical advice. Philanthropies seek to fund solutions to long-standing societal challenges but often will not sustain support over time.

These disconnects provide an opening for the many players inside and outside of government to help bridge gaps to support a more responsive and inclusive research enterprise. A mix of top-down and bottom-up mechanisms and collaboration may allow the full range of R&D actors across industry, academia, philanthropy, states, and localities to fully engage in shaping a new component of research culture to fill this gap.

To become the steward of a domestic R&D enterprise aimed at meeting the needs of the 21st century, the science community must adopt the ethos of Vannevar Bush and identify the primary gaps in the system today. The federal government has an opportunity to embrace America's decentralized innovation system as a strength instead of as a complication or weakness. In addition to cultivating cutting-edge scientific knowledge, it is time for the government to ensure that the knowledge funded by tax dollars yields solutions to the local and regional problems prioritized by communities across America.

Let's do what Vannevar Bush did and tackle a clear gap in our system. Let's focus on reinvigorating America's domestic capacity for innovation— let's move beyond profit and prestige and ensure we are improving the lives of people who ultimately make this ecosystem possible.



The Andrew W. Marshall Foundation

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