SCIENCE

We Need a New Science of Progress

Humanity needs to get better at knowing how to get better.

PATRICK COLLISON AND TYLER COWEN JUL 30, 2019

HARRY TODD / HULTON ARCHIVE / GETTY

In 1861, the American scientist and educator William Barton Rogers published a manifesto calling for a new kind of research institution. Recognizing the "daily increasing proofs of the happy influence of scientific culture on the industry and the civilization of the nations," and the growing importance of what he called "Industrial Arts," he proposed a new organization dedicated to practical knowledge. He named it the Massachusetts Institute of Technology.

Rogers was one of a number of late-19th-century reformers who saw that the United States' ability to generate progress could be substantially improved. These reformers looked to the successes of the German university models overseas and realized that a combination of focused professorial research and teaching could be a powerful engine for advance in research. Over the course of several decades, the group—Rogers, Charles Eliot, Henry Tappan, George Hale, John D. Rockefeller, and others—founded and restructured many of what are now America's best universities, including Harvard, MIT, Stanford, Caltech, Johns Hopkins, the University of Chicago, and more. By acting on their understanding, they engaged in a kind of conscious "progress engineering."

Progress itself is understudied. By "progress," we mean the combination of economic, technological, scientific, cultural, and organizational advancement that has transformed our lives and raised standards of living over the past couple of centuries. For a number of reasons, there is no broad-based intellectual movement focused on understanding the dynamics of progress, or targeting the deeper goal of speeding it up. We believe that it deserves a dedicated field of study. We suggest inaugurating the discipline of "Progress Studies."

Before digging into what Progress Studies would entail, it's worth noting that we still need a lot of progress. We haven't yet cured all diseases; we don't yet know how to solve climate change; we're still a very long way from enabling most of the world's population to live as comfortably as the wealthiest people do today; we don't yet understand how best to predict or mitigate all kinds of natural disasters; we aren't yet able to travel as cheaply and quickly as we'd like; we could be far better than we are at educating young people. The list of opportunities for improvement is still extremely long.

[Read: The 50 greatest breakthroughs since the wheel]

Those are major challenges. A lot of progress can also come from smaller advances: Thousands of lesser improvements that together build upon one another can together represent an enormous advance for society. For example, if our discoveries and inventions improve standards of living by 1 percent a year, children will by adulthood be 35 percent better off than their parents. If they improve livelihoods at 3 percent a year, those same children will grow up to be about 2.5 times better off.

Whether viewed in terms of large or small improvements, progress matters a lot.

Looking backwards, it's striking how unevenly distributed progress has been in the past. In antiquity, the ancient Greeks were discoverers of everything from the arch bridge to the spherical earth. By 1100, the successful pursuit of new knowledge was probably most concentrated in parts of China and the Middle East. Along the cultural dimension, the artists of Renaissance Florence enriched the heritage of all humankind, and in the process created the masterworks that are still the lifeblood of the local economy. The late 18th and early 19th century saw a burst of progress in Northern England, with the beginning of the Industrial Revolution. In each case, the discoveries that came to elevate standards of living for everyone arose in comparatively tiny geographic pockets of innovative effort. Present-day instances

include places like Silicon Valley in software and Switzerland's Basel region in life sciences.

These kinds of examples show that there *can* be ecosystems that are better at generating progress than others, perhaps by orders of magnitude. But what do they have in common? Just how productive can a cultural ecosystem be? Why did Silicon Valley happen in California rather than Japan or Boston? Why was early-20th-century science in Germany and Central Europe so strong? Can we deliberately engineer the conditions most hospitable to this kind of advancement or effectively tweak the systems that surround us today?

This is exactly what Progress Studies would investigate. It would consider the problem as broadly as possible. It would study the successful people, organizations, institutions, policies, and cultures that have arisen to date, and it would attempt to concoct policies and prescriptions that would help improve our ability to generate useful progress in the future.

[Read: Is 'progress' good for humanity?]

Along these lines, the world would benefit from an organized effort to understand how we should identify and train brilliant young people, how the most effective small groups exchange and share ideas, which incentives should exist for all sorts of participants in innovative ecosystems (including scientists, entrepreneurs, managers, and engineers), how much different organizations differ in productivity (and the drivers of those differences), how scientists should be selected and funded, and many other related issues besides.

Plenty of existing scholarship touches on these topics, but it takes place in a highly fragmented fashion and fails to directly confront some of the most important practical questions.

Imagine you want to know how to most effectively select and train the most talented students. While this is an important challenge facing educators, policy makers, and philanthropists, knowledge about how best to do so is dispersed across a very long list of different fields. Psychometrics literature investigates which tests predict success. Sociologists consider how networks are used to find talent. Anthropologists investigate how talent depends on circumstances, and a historiometric literature studies clusters of artistic creativity. There's a lively debate about when and whether "10,000 hours of practice" are required for truly excellent performance. The education literature studies talent-search programs such as the Center for Talented Youth. Personality psychologists investigate the

extent to which openness or conscientiousness affect earnings. More recently, there's work in sportometrics, looking at which numerical variables predict athletic success. In economics, Raj Chetty and his co-authors have examined the backgrounds and communities liable to best encourage innovators. Thinkers in these disciplines don't necessarily attend the same conferences, publish in the same journals, or work together to solve shared problems.

When we consider other major determinants of progress, we see insufficient engagement with the central questions. For example, there's a growing body of evidence suggesting that management practices determine a great deal of the difference in performance between organizations. One recent study found that a particular intervention—teaching better management practices to firms in Italy—improved productivity by 49 percent over 15 years when compared with peer firms that didn't receive the training. How widely does this apply, and can it be repeated? Economists have been learning that firm productivity commonly varies within a given sector by a factor of two or three, which implies that a priority in management science and organizational psychology should be understanding the drivers of these differences. In a related vein, we're coming to appreciate more and more that organizations with higher levels of trust can delegate authority more effectively, thereby boosting their responsiveness and ability to handle problems. Organizations as varied as Y Combinator, MIT's Radiation Lab, and ARPA have astonishing track records in catalyzing progress far beyond their confines. While research exists on all of these fronts, we're underinvesting considerably. These examples collectively indicate that one of our highest priorities should be figuring out interventions that increase the efficacy, productivity, and innovative capacity of human organizations.

Similarly, while science generates much of our prosperity, scientists and researchers themselves do not sufficiently obsess over how it should be organized. In a recent paper, Pierre Azoulay and co-authors concluded that Howard Hughes Medical Institute's long-term grants to high-potential scientists made those scientists 96 percent more likely to produce breakthrough work. If this finding is borne out, it suggests that present funding mechanisms are likely to be far from optimal, in part because they do not focus enough on research autonomy and risk taking.

[Read: Small teams of scientists have fresher ideas]

More broadly, demographics and institutional momentum have caused enormous but invisible changes in the way we support science. For example, the National

Institutes of Health (the largest science-funding body in the U.S.) reports that, in 1980, it gave 12 times more funding to early-career scientists (under 40) than it did to later-career scientists (over 50). Today, that has flipped: Five times more money now goes to scientists of age 50 or older. Is this skew toward funding older scientists an improvement? If not, how should science funding be allocated? We might also wonder: Do prizes matter? Or fellowships, or sabbaticals? Should other countries organize their scientific bodies along the lines of those in the U.S. or pursue deliberate variation? Despite the importance of the issues, critical evaluation of how science is practiced and funded is in short supply, for perhaps unsurprising reasons. Doing so would be an important part of Progress Studies.

Progress Studies has antecedents, both within fields and institutions. The economics of innovation is a critical topic and should assume a much larger place within economics. The Center for Science and the Imagination at Arizona State University seeks to encourage optimistic thinking about the future through fiction and narrative: It observes, almost certainly correctly, that imagination and ambition themselves play a large role. Graham Allison and Niall Ferguson have called for an "applied history" movement, to better draw lessons from history and apply them to real-world problems, including through the advising of political leaders. Ideas and institutions like these could be more effective if part of an explicit, broader movement.

In a world with Progress Studies, academic departments and degree programs would not necessarily have to be reorganized. That's probably going to be costly and time-consuming. Instead, a new focus on progress would be more comparable to a school of thought that would prompt a decentralized shift in priorities among academics, philanthropists, and funding agencies. Over time, we'd like to see communities, journals, and conferences devoted to these questions.

Such shifts have occurred before. A lot of excellent climate-science research—in environmental science, physics, chemistry, oceanography, mathematics and modeling, computer science, biology, ecology, and other fields—was being pursued before we recognized "climate science" as a discipline unto itself. Similarly, the designation of "Keynesian economics" helped economists focus on fiscal policy as a tool for recession fighting, just as the name "monetarism" created a focal interest in questions surrounding the money supply.

An important distinction between our proposed Progress Studies and a lot of existing scholarship is that mere comprehension is not the goal. When anthropologists look at scientists, they're trying to understand the species. But

when viewed through the lens of Progress Studies, the implicit question is how scientists (or funders or evaluators of scientists) *should* be acting. The success of Progress Studies will come from its ability to identify effective progress-increasing interventions and the extent to which they are adopted by universities, funding agencies, philanthropists, entrepreneurs, policy makers, and other institutions. In that sense, Progress Studies is closer to medicine than biology: The goal is to *treat*, not merely to understand.

We know that, to some readers, the word *progress* may sound too normative. However, it is the explicit bedrock upon which Vannevar Bush made his case for postwar funding of science, a case that led to the establishment of the National Science Foundation. In an era where funding for good projects can be hard to come by, or is even endangered, we must affirmatively make the case for the study of how to improve human well-being. This possibility is a fundamental reason why the American public is interested in supporting the pursuit of knowledge, and rightly so.

If we look to history, the organization of intellectual fields, as generally recognized realms of effort and funding, has mattered a great deal. Areas of study have expanded greatly since the early European universities were formed to advance theological thinking. Organized study of philosophy and the natural sciences later spawned deeper examination of—to name a few—mathematics, physics, chemistry, biology, and economics. Each discipline, in turn with its subfields, has spawned many subsequent transformative discoveries. Our point, quite simply, is that this process has yet to reach a natural end, and that a more focused, explicit study of progress itself should be one of the next steps.

We want to hear what you think about this article. Submit a letter to the editor or write to letters@theatlantic.com.