October 20th, 2016 Genomics Salon Utility of Basic Research

Curiosity-led research drives technological progress. Since the end of World War II, public funding for science in the United States has been built upon this principle, yet basic research occasionally draws public scrutiny for its seeming whimsy and irrelevance. To guide this salon session, two competing stances on the role and pragmatism of basic research are presented.

Pro-Research (Damien Wilburn)

Research should not need to be translational. Every federally funded grant requires at least one paragraph justifying the utility of research to humanity, and how the outcomes of the proposed research might one day be useful to people. The argument for accountability arises from NIH/NSF being tax-supported institutions, and the public should know that their money is being used wisely. This is faulty logic, as it assumes all government agencies should have full transparency of how their resources are allocated (which this data does not exist on a "per contract" basis) – and by extension tax payers should be able to "weigh in" on which contracts they support. This is neither practical nor feasible, but also shouldn't matter, as basic research has a high return on investment (ROI). While the numbers are highly contested, the ROI for different scientific disciplines range from 9-400% – better than your average mutual fund – and does not include the benefits to education and our societal knowledgebase.

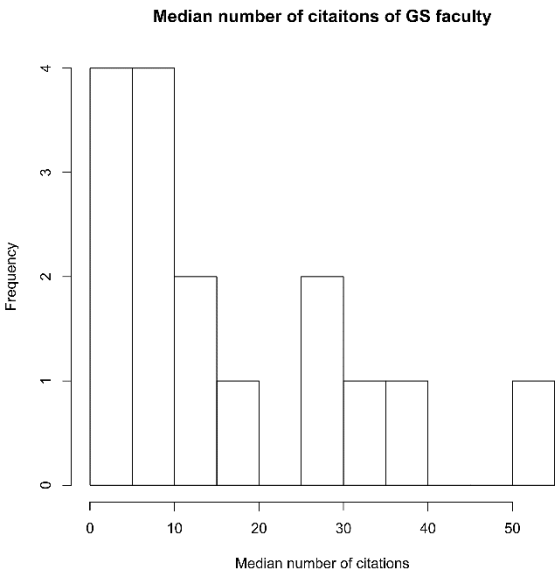
Additionally, the notion that outcomes/potential needs to be defined from the outset of a project is stifling – it creates biases and limits creative investigation. As scientists, it's important to remain agnostic to alternative hypotheses: this expectation of a product at the completion of a project is one of the core differences between engineering and science. The need to "produce" also more intimately affects an individual's personal success (defined as wealth, prestige, or any more capitalistic metric), creating incentive to commit fraud, and de-incentivizing the need to "hype" or "oversell" science, which is likely one of the many factors that has led to distrust of academic institutions. Instead, curiosity based research should be encouraged and expanded, as there is no crystal ball to predict what findings will be of benefit in the future, and I highlight a few examples:

- Molecular biology and genetic testing have been revolutionized by PCR, partially facilitated by *Taq* polymerase from *Thermus aquaticus*, which was studied because of interest in how bacteria can grow and thrive in hot springs.
- Famed biologist Alexander Fleming only discovered penicillin (the first antibiotic) after returning from vacation and finding a strange blue-green fungus colonized one of his *Staphylococcus* plates, and managed to kill the bacteria – and was fascinated to understand the mechanisms.
- The first laser was built by Theodore Maiman, described as "a solution without a problem," as he expanded on Einstein's work from 40 years earlier demonstrating electrons of an object could be stimulated to make light of a specific color.
- Isabor Rabi, by studying the magnetic properties of various atomic nuclei, won the 1944 Nobel Prize in Physics for the development of nuclear magnetic resonance (NMR), which ~40 years later led to the development of the first MRI (the development of which earned the 2003 Nobel Prize in Medicine).
- CRISPR-Cas9 was only discovered because of interest in bacterial immunity to viruses, and was not anticipated to be a potential genetic engineering tool.

Anti-Research (Josh Schraiber)

Many of us take for granted that basic research makes people's lives better via technological and medical advancement. But how often is this assertion critically assessed? When writing grants, we all wave our hands about how the work we do will be important for downstream application—but certainly that's not the main take away of the grant, nor the main reason it's funded. In my mind, science serves much the same purpose as art: it is life *enriching*, but not life *changing*.

In fact, basic research very rarely leads to anything even remotely useful, let alone world changing. A whopping 12% of medical articles and 27% of articles in natural sciences and engineering are *never cited* once (Lariviére et al). To make this point a little more personal, I crawled Google Scholar for GS faculty and calculated the median number of citations of their last 100 papers. As can be seen in the figure, approximately half have a median number of citations less than 10.



Of course the argument is usually that we cannot predict what will be useful and result in things that change people's lives. But that seems to be less true than we think. Matt Ridley argued in the Wall Street Journal that technological breakthroughs more often than not *precede* the basic scientific breakthroughs that supposedly bring them about. Moreover, technology advances not in jerks and starts, but as a result of people building off of each other's work, thus resulting in multiple independent inventions of similar technology contemporaneously.

There is also a myth that private industry does not fund basic research. The obvious counterexample is Bell Labs, which is responsible for essentially inventing modern life: transistors, information theory, lasers, unix, etc. But even today companies like Microsoft invest heavily in basic machine learning research, often recruiting the best scientists from academia to work for them.

This would be fine if federal funding weren't a zero sum game. But every tax dollar the government spends on basic research is a tax dollar that *isn't* spent doing something with higher marginal utility. Not to mention the large amount of non-human animal lives that are sacrificed in the name of biomedical research. Do we really want to make the world a better place? Reallocate the NIH budget to subsidizing a basic income for the poorest among us.

Questions for discussion:

1) What return on investment should we expect from basic science? Should NSF/NIH budgets be increased or decreased?

2) Who should be deciding what research to fund: scientists, the public, government officials? What qualifications should be required, if any?

3) What improves people's lives more: UW Football, or all basic research done at UW?

4) Why do we publicly fund basic research so much more than we fund art?

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