Twitter Thread by Michael Nielsen



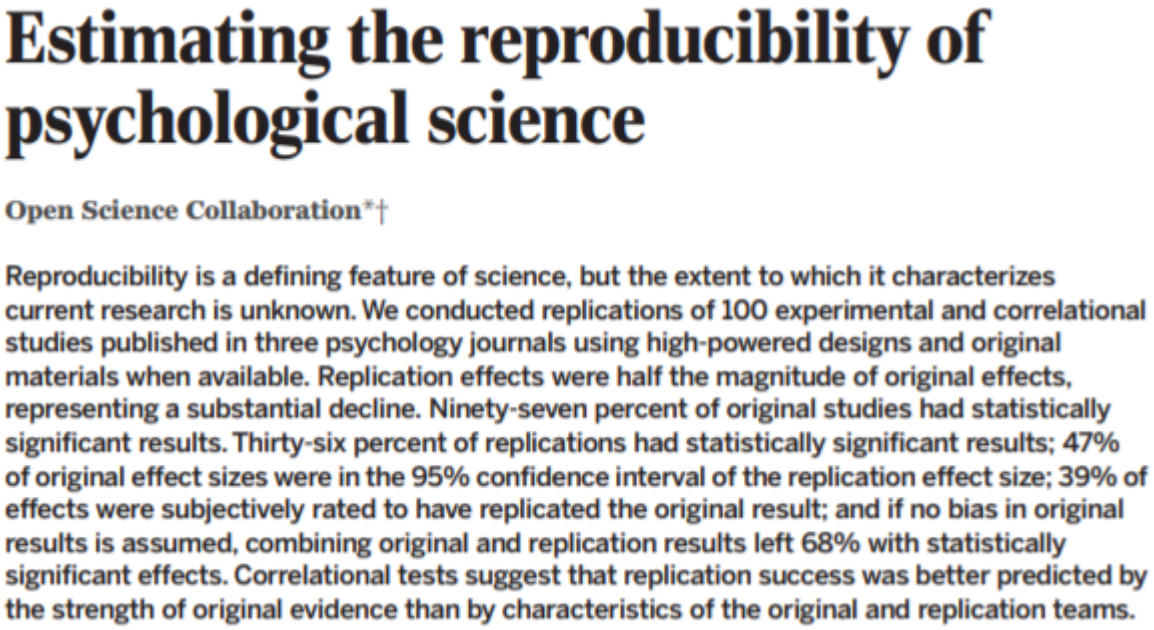
Michael Nielsen
@michael_nielsen

What are the classics of the "Science of Science" or "Meta Science"? If you were teaching a class on the subject, what would go in the syllabus?

Here's a (very disorganized and incomplete) handful of suggestions, which I may add to. Suggestions welcome, especially if you've dug into relevant literatures.

1. The already classic "Estimating the reproducibility of psychological science" from the Open Science Collaboration of @BrianNosek et al. https://t.co/yjGczLZ6Je

(Look at that abstract, wow!)



Estimating the reproducibility of psychological science

Open Science Collaboration*†

Reproducibility is a defining feature of science, but the extent to which it characterizes current research is unknown. We conducted replications of 100 experimental and correlational studies published in three psychology journals using high-powered designs and original materials when available. Replication effects were half the magnitude of original effects, representing a substantial decline. Ninety-seven percent of original studies had statistically significant results. Thirty-six percent of replications had statistically significant results; 47% of original effect sizes were in the 95% confidence interval of the replication effect size; 39% of effects were subjectively rated to have replicated the original result; and if no bias in original results is assumed, combining original and replication results left 68% with statistically significant effects. Correlational tests suggest that replication success was better predicted by the strength of original evidence than by characteristics of the original and replication teams.

Many people had pointed out problems with standard statistical methods, going back decades (what are the best refs?). But this paper was a sledgehammer, making it impossible to ignore the question: what, if anything, were we actually learning from all those statistical studies?

2. Dean Keith Simonton's book "Creativity in Science: Chance, Logic, Genius, and Zeitgeist". If an essentially scientometric book could be described as a fun romp through science & creativity, this would be it https://t.co/RQ935H1fKs

3. From the philosophy of science literature, I especially like Lakatos's "Proofs and Refutations", Feyerabend's "Against Method", and Kuhn's "The Structure of Scientific Revolutions". I should probably dig deeper into other schools (recs?) (Yes, I've read Popper's main works.)

4. Speaking of Feyerabend, Steve Weinberg had some surprisingly sympathetic & characteristically insightful comments on Feyerabend, which I expect would bear re-reading. I've lost the reference.

5. Switching genre, there's the work of @pierre_azoulay and collaborators, studying the HHMI versus NIH approaches to discovery in the life sciences: https://t.co/9eta708dVf

ABSTRACT

Despite its presumed role as an engine of economic growth, we know surprisingly little about the drivers of scientific creativity. In this paper, we exploit key differences across funding streams within the academic life sciences to estimate the impact of incentives on the rate and direction of scientific exploration. Specifically, we study the careers of investigators of the Howard Hughes Medical Institute (HHMI), which tolerates early failure, rewards long-term success, and gives its appointees great freedom to experiment; and grantees from the National Institute of Health, which are subject to short review cycles, pre-defined deliverables, and renewal policies unforgiving of failure. Using a combination of propensity-score weighting and difference-in-differences estimation strategies, we find that HHMI investigators produce high- impact papers at a much higher rate than a control group of similarly-accomplished NIH-funded scientists. Moreover, the direction of their research changes in ways that suggest the program induces them to explore novel lines of inquiry.

6. From 2018, a nice review paper on "The Science of Science", coming from a network science / scientometrics point of view. There are tonnes of interesting observations in the paper, many of which I bundled up in this thread: https://t.co/potbylhgxt

https://t.co/xduj2A8c8q

Science of science

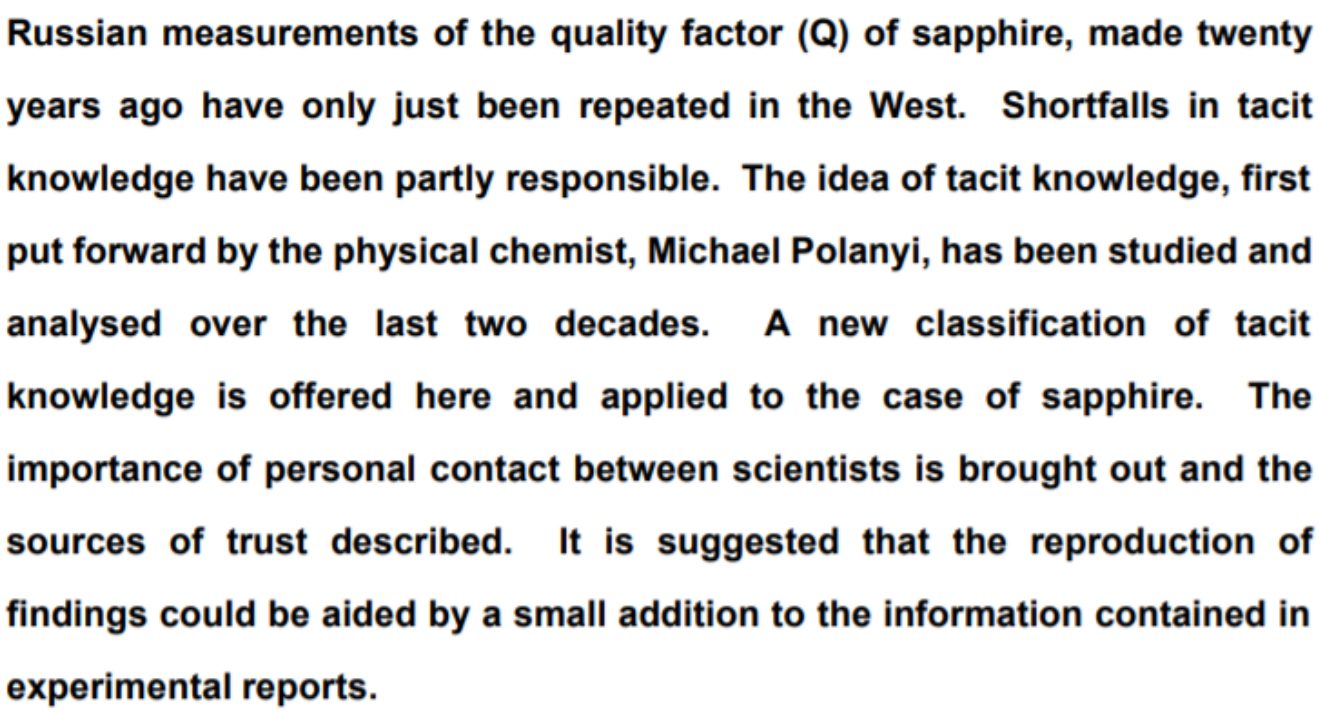
Santo Fortunato,[1,2]* Carl T. Bergstrom,[3] Katy Börner,[2,4] James A. Evans,[5] Dirk Helbing,[6] Staša Milojević,[1] Alexander M. Petersen,[7] Filippo Radicchi,[1] Roberta Sinatra,[8,9,10] Brian Uzzi,[11,12] Alessandro Vespignani,[10,13,14] Ludo Waltman,[15] Dashun Wang,[11,12] Albert-László Barabási[8,10,16]*

Identifying fundamental drivers of science and developing predictive models to capture its evolution are instrumental for the design of policies that can improve the scientific enterprise—for example, through enhanced career paths for scientists, better performance evaluation for organizations hosting research, discovery of novel effective funding vehicles, and even identification of promising regions along the scientific frontier. The science of science uses large-scale data on the production of science to search for universal and domain-specific patterns. Here, we review recent developments in this transdisciplinary field.

> A few observations from "The Science of Science", a useful review article appearing in this week's Science: https://t.co/D6DENKF4rS
>
> — Michael Nielsen (@michael_nielsen) March 9, 2018

7. One of our best long-term observers of science and science policy was Daniel Greenberg (who passed away last year). Many possibilities to choose from, but here's one I got a lot out of: "Science, Money, and Politics": https://t.co/8QANkuL6ZA

8. Harry Collins has done some wonderful work on the central role of tacit knowledge in science. Here's one of his classics, on the role of tacit knowledge in figuring out how good sapphire is as a lasing material: https://t.co/Oaz2VWySmM



That sounds very specialized. It's not. It goes to questions at the very heart of science, both institutionally and methodologically. Rather, Collins' paper is a beautiful detailed study of tacit knowledge.

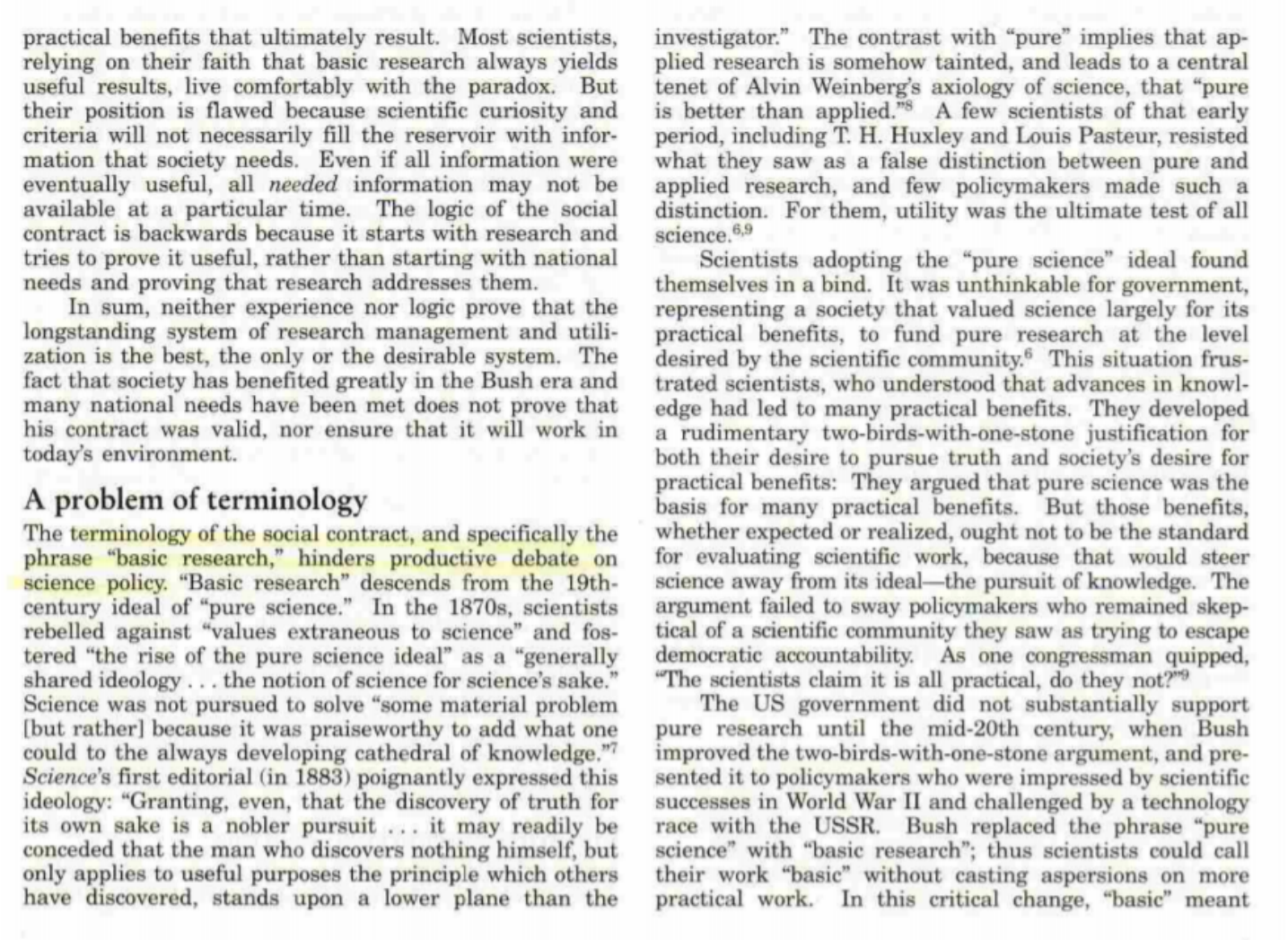
(Tangentially: one way my thinking has changed is in gradually understand how tied together our institutions and our methodologies are. There's a kind of Conway's Law in action: our institutions tend to mirror our methodology, and vice versa.)

9. The institutions around us are, of course, all made up, out of ideas - things like universities, PhDs, journals, etc, even the notion of "Science", are first and foremost conceptual innovations. I'd love to understand the history of those ideas better.

One striking text in this vein is Francis Bacon's 1627 "The New Atlantis", which introduces "Salamon's House", which strongly influenced the design of the Royal Society (1660), and modern universities. https://t.co/bznUG1eTH5

10. Another good one in this vein is Vannevar Bush's "Science: the Endless Frontier", which helped establish the concepts underpinning the modern basic research ecosystem https://t.co/bpc1nPg8A7

11. Indeed, I've heard it argued that Bush is the person most responsible for developing the concept of "basic research", and this was done in part as a way of winning a political fight to motivate funding for pure research. The argument is made here: https://t.co/QPMCAEYSLE



practical benefits that ultimately result. Most scientists, relying on their faith that basic research always yields useful results, live comfortably with the paradox. But their position is flawed because scientific curiosity and criteria will not necessarily fill the reservoir with information that society needs. Even if all information were eventually useful, all *needed* information may not be available at a particular time. The logic of the social contract is backwards because it starts with research and tries to prove it useful, rather than starting with national needs and proving that research addresses them.

In sum, neither experience nor logic prove that the longstanding system of research management and utilization is the best, the only or the desirable system. The fact that society has benefited greatly in the Bush era and many national needs have been met does not prove that his contract was valid, nor ensure that it will work in today's environment.

A problem of terminology

The terminology of the social contract, and specifically the phrase "basic research," hinders productive debate on science policy. "Basic research" descends from the 19th-century ideal of "pure science." In the 1870s, scientists rebelled against "values extraneous to science" and fostered "the rise of the pure science ideal" as a "generally shared ideology . . . the notion of science for science's sake." Science was not pursued to solve "some material problem [but rather] because it was praiseworthy to add what one could to the always developing cathedral of knowledge."[7] *Science*'s first editorial (in 1883) poignantly expressed this ideology: "Granting, even, that the discovery of truth for its own sake is a nobler pursuit . . . it may readily be conceded that the man who discovers nothing himself, but only applies to useful purposes the principle which others have discovered, stands upon a lower plane than the investigator." The contrast with "pure" implies that applied research is somehow tainted, and leads to a central tenet of Alvin Weinberg's axiology of science, that "pure is better than applied."[8] A few scientists of that early period, including T. H. Huxley and Louis Pasteur, resisted what they saw as a false distinction between pure and applied research, and few policymakers made such a distinction. For them, utility was the ultimate test of all science.[6,9]

Scientists adopting the "pure science" ideal found themselves in a bind. It was unthinkable for government, representing a society that valued science largely for its practical benefits, to fund pure research at the level desired by the scientific community.[6] This situation frustrated scientists, who understood that advances in knowledge had led to many practical benefits. They developed a rudimentary two-birds-with-one-stone justification for both their desire to pursue truth and society's desire for practical benefits: They argued that pure science was the basis for many practical benefits. But those benefits, whether expected or realized, ought not to be the standard for evaluating scientific work, because that would steer science away from its ideal—the pursuit of knowledge. The argument failed to sway policymakers who remained skeptical of a scientific community they saw as trying to escape democratic accountability. As one congressman quipped, "The scientists claim it is all practical, do they not?"[9]

The US government did not substantially support pure research until the mid-20th century, when Bush improved the two-birds-with-one-stone argument, and presented it to policymakers who were impressed by scientific successes in World War II and challenged by a technology race with the USSR. Bush replaced the phrase "pure science" with "basic research"; thus scientists could call their work "basic" without casting aspersions on more practical work. In this critical change, "basic" meant

12. David Lang suggests Paula Stephan's book here: https://t.co/zQiuTlWOdR

The book has been in my queue for some time, and is almost certainly a good overview of a huge chunk of economic thinking about science.

> Great thread. I\u2019d add Paula Stephan\u2019s book: https://t.co/QdIPBchdym
>
> — David Lang (@davidtlang) January 14, 2021

Interlude: thanks for the many wonderful replies!!

Twitter threading makes it a little hard to skim the thread. Expandable tree version here, thanks to @paulgb's great treeverse Chrome extension: https://t.co/vF4JF0Gf4J

I'll indulge myself a bit, and ask @dabacon, @AndrewDohertyQu, @quantum_aram, @uncatherio, @albrgr, @DGoroff, @BrianNosek, @juliagalef, @juanbenet, @AdamMarblestone, @patrickc, @pierre_azoulay if you have any particular favorite additions for the list?