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Powerful Numbers or a short reflection on influential analyses in the history of science of science policy

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The quantitative analysis of issues relevant to science policy has a history dating back several decades. Over that time, there have been occasions in which scholarly analyses have escaped from the ivory tower and made an impact on policy discussions or on policy itself. In this paper, I review some of these occasions, looking at what type of analyses were used, who used such analyses, and for what purposes.

The first influential number I examine is 28 percent, associated with the eminent economist Edwin Mansfield, late of the University of Pennsylvania, and representing his empirically based estimate of the social rate of return to public research spending. The economic approach to assessing impact of agricultural R&D was introduced by Nobel laureate T.W. Schultz in 1953.[1] Today there is a very large literature doing this type of calculation, but Mansfield's calculation was the first outside agricultural research. This is probably the most influential number in the history of what is now called: "Science of Science Policy." Mansfield started this work as early as 1977, but that first paper did not report any single number for the social rate of return.[2] Nevertheless, the Government Accounting Office (GAO) picked up on it, presumably at the request of Congress, and reported that the work represented state-of-the-art methodology:

> *GAO was asked how the results of federally financed research and development spending could be measured . . . Edwin Mansfield's recently completed study of innovations' rate of return exemplifies this methodology's current applied state of the art.* [3]

The specific number was published 15 years later in *Research Policy* (1991).[4] "28" was scattered throughout this paper. Here is a part of the paper's conclusion:

> *A very tentative estimate of the social rate of return from academic research during 1975-78 is **28 percent**, a figure that is based on crude (but seemingly conservative) calculations and that is presented only for exploratory and discussion purposes. It is important that this figure be treated with proper caution and that the many assumptions and simplifications on which it is based (as well as the definition of a social rate of return used here) be borne in mind.*

Prof. Mansfield was encouraged to produce this study by the Policy Studies Unit in NSF, who funded the work. The resulting paper has been extremely influential in scholarly circles, that is very highly cited, as well as in the policy world. Crucial to the influence of this analysis is that Mansfield did put forth a number. This is a bold move, and one avoided by many scholars. Nevertheless Mansfield's number is surrounded by scholarly caveats – "treat with caution" "only exploratory" etc. Going forward, what happened to this number and what happened to the caveats?

Next year in an interview in *Science* magazine President Bush, Republican candidate for President, is quoted as saying:

> *Our support of basic research in these and other agencies is an investment in our future, but by its very nature it is impossible to predict where, when, or to whom the benefits will flow. Nevertheless, we can be sure that these benefits will be substantial. Professor Edwin Mansfield of the University of Pennsylvania has found that the social rate of return from such investments in academic research can very conservatively be estimated at 28%.*[5]

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[1] Schultz, T.W. (1953) *The Economic Organization of Agriculture*. New York: McGraw-Hill.
[2] Mansfield, E. et. al. (1977) "Social and Private Rates of Return From Industrial Innovations," *Quarterly Journal of Economics*.
[3] Report By The Comptroller General Of The United States (1979) *Assessing The "Output" Of Federal Commercially Directed R&D*.
[4] Edwin Mansfield (1991) Academic research and industrial innovation, *Research Policy*, 20, 1-12.
[5] *Science* Vol. 258, 16 October 1992, Policy Forum, Interview with George Bush, President of the United States and Republican candidate for President.

The President used the number in arguing for the value of research funding. A scholar could not ask for a more gratifying policy impact – not only is your number used, but you are mentioned by name. Not surprisingly however, the caveats have disappeared. Caveats do not work well in Presidential interviews.

Nowadays there is a very large economic literature estimating the private and social returns from R&D spending. Arguably Mansfield's influence was greatest, possibly because he was first. In 1993 the Congressional Budget Office (CBO) reviewed Mansfield's work in response to a request from a House Committee. The CBO positioned Mansfield's work as a validation of the vision of Vannevar Bush, the patron saint of U.S. basic research funding and even mentioned caveats:

> *This staff memorandum was prepared in response to a request from the House Committee on Science, Space, and Technology. The Committee asked the Congressional Budget Office to comment on the policy relevance and statistical accuracy of Edwin Mansfield's estimates of the social rate of return from academic research. Since World War II, U.S. science policy has been guided by Vannevar Bush's vision that, if funded and left to set their own agenda, scientists would amply reward the nation for its investment. Mansfield has shown that, on average, academic scientists have indeed kept their part of the bargain. The return from academic research, despite measurement problems, is sufficiently high to justify overall federal investments in this area.*
>
> *Nevertheless, the very nature of the estimating methodology, as Mansfield has noted in his articles, does not lend itself to use in the annual process of setting the level of federal investment in R&D, nor to allocating that investment among its many claimants. Furthermore, given the nature of the assumptions, definitions, and other methodological questions, as Mansfield notes, his result is more properly regarded as indicating a broad range of likely orders of magnitude of the return from academic R&D than as a point estimate (**28 percent**) of the return from federal investment in this area.* [6]

In 1998, Mansfield produced an update in *Research Policy*[7] and his influence grew. In 1998 the Congressional Budget Office did another report:

> *Mansfield estimated that academic R&D gives society a **28 percent** return on its investment; given the uncertainties involved, a more appropriate summary of the study is a range from **20 percent to 40 percent**. Since most of the funding of those academic researchers came from the federal government, the returns should apply, at least roughly, to federal programs that fund academic research.*[8]

Not only the President, but Congress is using Mansfield's number and including some caveats.

However, accuracy cannot always be assured in the use of scholarly numbers. In 2006 the Advanced Technology Program (ATP) incorrectly reported:

> *Mansfield's pioneering work in the 1970s and subsequently in two studies sponsored by the National Science Foundation (NSF) showed private rates of return averaging **25%-36%** and social rates of return averaging **50%-70%.***[9]

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[6] 1993, CBO staff memorandum, *A review of Edwin Mansfield's estimate of the rate of return from academic research and its relevance to the federal budget process*

[7] Edwin Mansfield (1998) Academic research and industrial innovation: An update of empirical findings, *Research Policy,* 26, 7-8: 773-776.

[8] Congressional Budget Office (1998) The economic effects of federal spending on infrastructure and other investments.

[9] Jeanne Powell (2006) Economic Assessment Office, ATP, *Toward a Standard Benefit-Cost Methodology for Publicly Funded Science and Technology Programs*, NIST IR 7319

And in 2006 a report of the Task Force on Innovation which is an advocacy organization, not a government department, 28% grew to 40%

> *It is no wonder that economist Edwin Mansfield calculated as much as a **40%** rate of return for the Federal investment in basic university based research.*[10]

However, even today the number endures, and thankfully accuracy has returned. As recently as 2007 the number was used in testimony before the House Committee on Financial Services:

> *Mansfield concluded that the average annual rate of return to society from academic research was anywhere from 28 to 40 percent. The Congressional Budget Office, in a 1993 review of Mansfield's estimates, said that "the return from academic research, despite measurement problems, is sufficiently high to justify overall federal investments in this area."*[11]

Although this is no doubt an incomplete record, it does establish the enduring influence of Mansfield's number. Approaching two decades after the original paper was published, this number is still used to justify government expenditure on research.

The second influential number highlighted here is 73%. This number was published in a 1997 paper published in *Research Policy* by Francis Narin at CHI Research. The paper revealed that patents were making prior art references increasingly to scientific papers. This is interpreted to mean that industry patents build upon university science. It a very useful number in advocacy for increased science budgets because it is a very direct way of showing Congress that U.S. industry uses the research the government funds. Unlike Mansfield, Narin did not really focus his paper around producing a number. The author's summary of the paper would be that references from U.S. patents to U.S.-authored research papers tripled over a six-year period, from 1988 to 1994. Furthermore, the cited U.S. papers were quite basic, in influential journals, authored at top-flight research universities and laboratories, relatively recent, and heavily supported by NIH, NSF, and other public agencies. However, the introduction to the paper did contain this:

> *Seventy-three percent of the papers cited by U.S. industry patents are public science, authored at academic, governmental, and other public institutions . . .*

Again, this study was noticed and used by the media, advocates and policy makers. A 1997 New York Times article by Bill Broad focusing solely on this paper was headlined: "Study finds public science is pillar of industry."

There was again a Congressional Budget Office commentary in a report on the economic effects of federal spending:

> *CHI Research, a patent-citation consultancy, has collected indirect evidence on that point.**(65)** Patent applications include two types of citations: to other patents and to scientific literature. Of the scientific papers cited in patents, 73 percent were articles written by academic scientists or scientists at governmental or other institutions developing what the authors call "public science." The authors argue that industry has increased its reliance on public science over the last decade and that public science is, to a large extent, the product of federal funds.*[12]

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[10] The Task force on the future of American Innovation (2006) *Measuring the moment, Benchmarks of our Innovation Future II*.
[11] 2007 Testimony to the House Committee on Financial Services, Michael Drake, M.D. Chancellor, University of California Irvine
[12] 1998, Congressional Budget Office, *The economic effects of federal spending on infrastructure and other investments*.

Following the pattern set by the Mansfield number, Narin's number was also misquoted, this time in a report from the House of Representatives:

> *The above examples of basic research pursuits which led to economically important developments, while among the most well known, are hardly exceptions. Other instances of federally funded research that began as a search for understanding but gave rise to important applications abound. In fact, a recent study determined that 73 percent of the applicants for U.S. patents listed publicly-funded research as part or all of the foundation upon which their new, potentially patentable findings were based*[13]

If indeed 73% of patent applicants cited public science, that would be a much more powerful number than the actual result, which was that 73% of the cited papers originated in universities. So an element of wishful thinking appears here, as it did with the Mansfield misquotes. The errors are clearly not random. The tendency to ignore reality and pretend numbers are more powerful than they are is one thing that makes scholars queasy and reluctant to interact with policy makers.

Nevertheless, most users did quote the result correctly, even five years later when the National Science Board quoted the results in two documents:

> *An NSF-supported study found that 70 percent of the scientific papers cited in U.S. industry patents came from science supported by public funds and performed at universities, government labs, and other public agencies.*[14, 15]

Narin also briefed interested Congress members in a breakfast meeting organized by the NSF as well as briefing the National Science Board. The NSB got interested and convened a subcommittee to write a report on Industry Reliance on Publicly-funded Research (IRPR). Caveats were a worry for the subcommittee who found the topic to be more complex than anticipated. The minutes of a subsequent NSB meeting reported that:

> *There are other indicators to account for . . . It would be difficult to draw general conclusions, so the paper will contain a number of limited conclusions. Finally, there are issues of credibility to address. The Task Force was concerned that the paper not appear to be self-serving and that it be cautious about overstatement. Consequently, more study and discussion are needed as the Task Force's initial draft is revised.*

The Chairman applauded the Task Force for its caution and urged them to continue their efforts which resulted in an addendum to Science & Engineering Indicators 1998 entitled: Industry Trends in Research Support and Links to Public Research (NSB 98-99).

The next example did come up with a number - in private. This is a British example. In the mid 1980s Ben Martin and John Irvine, Science Policy Research Unit, University of Sussex produced a series of commentaries

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[13] 1998 Unlocking our Future: Towards a New National Science Policy, Committee Print 105-B, Committee on Science, U.S House of Representatives, One Hundred and Fifth Congress, September 1998

[14] 2003, National Science Board, Fulfilling the Promise: A Report to Congress on the Budgetary and Programmatic Expansion of the National Science Foundation, NSB 03-151

[15] 2005, National Science Board, 2020 Vision for the National Science Foundation, NSB 05-142

in *Nature*.[16] The titles tell the story: "Charting the Decline in British Science"; "Is Britain Spending Enough. . ."; and "The Continuing Decline . . ." The first one was an analysis of trends in publication output, the second compared levels of research funding in the UK with those of competitors. Martin and Irvine disliked existing funding data and went around the world talking to agencies to collect proper funding data; and their analysis was reported in the second commentary. The next year they updated the publication analysis in the third commentary.

As a result, John Irvine was called in to a meeting with the Minister and was asked, "How much is this funding gap?" The reply was, "£100 million." The Minister replied, "Well, we can do that." And £100 million was added to the science budget. So this series of analyses had a very significant influence on British science policy. At that time, they were doing an update of the funding part of the study and were asked what the new result would be because if it was $100 million that would be good because the Minister could probably get that a second time, because the Government had that much. But if it was $500 million they probably couldn't get that, because that was too much. And unfortunately to the academics it looked like $500 million which put them in an ethical quandary.[17] In this example we see some themes repeated, namely the same focus on numbers useful in advocacy for national science budgets as well as the absence of a single, simple number in the original papers. Again a single, simple number was what had some utility and the policy user extracted one.

Our next example also changed policy. In Australia all universities have been required to submit to the government details of their publication output.

> *Since 1992, all universities have been required to supply details of their publication output, initially through the Australian Vice-Chancellors' Committee, to the Department of Employment, Education and Training. The distribution of that part of the operational grants of universities earmarked for research (known as the Research Quantum) has to a limited degree depended on this information. As the categories covered by this collection have been refined and reduced in number, the importance of ISI-indexed journal publications has increased. It is possible for university researchers to put a dollar value (either to themselves or to their university) on their ability to place an article in an ISI journal. Other refereed journals provide similar rewards, but the difficulty of having their status accepted by independent auditors results in an increasing focus on the ISI journal literature.*[18] (*p. 150)*

In other words, the distribution of core research funding for universities was to some extent dependent on bibliometric data. As this exercise was refined over time the importance of having papers in a journal indexed in the *Web of Science* (WoS) increased tremendously and the universities could put a dollar value on their ability to place an article in a *Web of Science* indexed journal.

Linda Butler of the Australian National University discovered the striking consequences. She found that the Australian share of world publication output grew after the national evaluation scheme was introduced in 1992, contrasting with a steady state in the decade prior. This is a great result for a policy that was far cheaper than expanding the resources expended upon scientific research. It looks fabulous. But there was a problem. The citation performance of Australia fell. Among countries ranked on ratio of share of world

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[16] Martin, B & J. Irvine (1985) Charting the Decline in British Science, Commentary, *Nature*, v 316, 587-590; Martin, B & J. Irvine (1986) Is Britain Spending Enough on Science? Commentary, *Nature*, v 323, 591-594; Martin, B & J. Irvine (1987) The Continuing Decline of British Science, Commentary, *Nature*, v 330, 123 -126.

[17] Private communication, Ben Martin, 2009.

[18] Linda Butler, Explaining Australia's increased share of ISI publications—the effects of a funding formula based on publication counts, *Research Policy*, Volume 32, Issue 1, January 2003, Pages 143-155

citations to share of world publications, Australia fell from number 6 in 1981 to number 10 in 1999. What had happened?

Butler demonstrated that the impact factor of journals publishing Australian papers had declined in this period. Australians were publishing in higher impact journals before the policy was introduced than after. Once the policy took effect and authors prioritized producing more papers, more Australian papers went to lower quality, yet still WoS indexed, journals. Butler concluded: "Australia's research evaluation policy had become a disincentive to research excellence," and the analysis illustrated this quite clearly.

This analysis provided policy makers with an evidence base for an alternative research evaluation policy premised on assessing the quality of research rather than just publication counts. For the assessment of their own research impact, Butler group's produced a diagram detailing the funded research projects on the topic, the resulting papers, the government White Papers that cited their papers and their participation in government working groups developing the new Research Quality Framework system. The full chain of influence was recorded.

This example differs from the others in that it did not concern advocacy for science budgets and did not revolve around a single number. Rather the authors changed the way people think, perhaps the highest goal of scholarly work in the social sciences. Another differentiator is that the authors also participated in the policy design process. Furthermore, the influence of the work was international. The state-of-the-art in national bibliometric evaluation systems now is to have 2 to 4 weighted categories of publications, rather than simply relying on WoS indexing as a marker of quality. This feature directly responded to the conclusions of Butler's analysis.[19]

The next example is a personal favorite because I'm claiming this number although there is no attribution. President Obama on September 21 2009 in a speech in Troy, New York, said:

> *I've also proposed reducing to zero the capital gains tax for investments in small or startup businesses, because small businesses are innovative businesses; they produce 13 times more patents per employee than large companies do. (Applause.)*[20]

The 13x number could only have come from a study for the Small Business Administration by Anthony Breitzman of 1790 Analytics and myself.[21] Although a mention in Presidential speech announcing new policy is somewhat better than an interview by the science press, it was unfortunate that our names were not mentioned, like Mansfield. As in most of the other examples, impact was made by a single number. However, our report did not actually focus on a number, or even contain this number. The number was pulled out of a table in the report and conservative rounding was done to slightly reduce the magnitude of the reported value. Alas, as in other examples, the statement is not entirely correct. Small firms do not in fact produce 13 times more patents per employee. Rather, *among America's most innovative firms*, small firms produce 13 times more patents per employee. The study was not concerned with the entire population of firms because most small firms actually run away from innovations as fast as they can. Only the firms with the most patents were studied. I e-mailed the correction to whitehouse.gov.

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[19] Gunner Sivertsen, personal communication, 2009.

[20] Remarks By The President On Innovation And Sustainable Growth, Hudson Valley Community College, Troy, New York, September 21, 2009.

[21] A. Breitzman & D. Hicks, An Analysis of Small Business Patents by Industry and Firm Size, Report for the Office of Advocacy, Small Business Administration on contract SBAHQ-07-Q-0010, November 2008, http://www.sba.gov/advo/research/rs335tot.pdf

Another example of use of numbers in a more informal setting concerns a consultant's report for the Veteran's Administration. The sponsor wanted material he could use in informal conversations, in a cocktail party for example. The sponsor wanted to be ready to lobby Congress people when he met them in social settings. The study found that: "On average, a VA sponsored researcher appears in either the New England Journal of Medicine or JAMA every week." This is a nice, concrete and clear example of the organization producing a lot of prestigious research. But because it is quantitative, it carries more weight than an anecdote would. The VA sponsor was very happy.

My final example is the number 675,000. This number was produced by the Policy Research Analysis unit of the National Science Foundation. 675,000 was the predicted shortfall in engineering graduates between 1986 and 2010. The number was the result of a very basic demographic analysis. The number of babies declined in the 1960s baby bust, so looking 22 years ahead when those people would be getting bachelors degrees, the analysts predicted bachelors degrees would decrease and therefore MS and PhD degrees would also decrease. Daniel Greenberg's discussion of this incident traces the origins to the appointment of Eric Bloch as director of NSF in 1984.[22] In 1987, the Policy Research Analysis division, which reported directly to Boch, began issuing a series of "pipeline" reports such as its 1989 reports: *Future Scarcity of Scientists and Engineers: Problems and Solutions* (working draft, NSF PRA) and *The State of Academic Science and Engineering* (NSF PRA). Bloch used the number in quite a few public statements. But NSF never officially authorized release of any of the PRA pipeline reports and there were issues with peer review of them. In 1990 Eric Bloch left NSF.

The number was very influential; there were a series of stories in major newspapers including *Science*, the *Los Angeles Times*, *Wall Street Journal*, and *Christian Science Monitor*. Things seemed to be going well until the President of the National Academy of Engineering went after the number in his Presidential address to the Academy.[23] He noted that there was no sign of a shortfall in real life, quite the contrary the job market for engineers was terrible. After this, articles began to appear about the terrible job market for engineers in the early 1990s. In addition, a postdoc in the Naval Research Laboratory, Kevin Aylesworth took an informal poll, published dire results concerning the job market in *Physics Today* and contacted lots of Congressional staffers with his information. It was a bad time for engineers, so these people were really angry at the NSF because the 675,000 number was used to advocate for even more funding for PhDs and more generous immigration quotas. And the engineers who had PhDs in this country and couldn't find jobs were very annoyed.

Congress held a hearing. Rep. Howard Wolpe had this to say to Peter House director of NSF Policy Research and Analysis division (PRA):

> *Well, we're here today because of a terrible misunderstanding. I mean, that's really the bottom line. Hundreds if not thousands, of people believed that your study had something definitive to say about the scientific and engineering needs of this country. . .*
> *Science education, immigration policy in this country have been affected by the study and by the number that was its product.*[24]

NSF was engaged in advocacy. Bloch used the number to advocate for greater NSF funding in Congress. NSF numbers were supposed to be above reproach because NSF people were scientists. Congress understood that the numbers produced by their colleagues demanded heavy scrutiny, but they didn't think NSF numbers were

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[22] Commentary here is a summary of Greenberg's analysis in chapters 8 and 9 of Daniel S. Greenberg, 2001, *Science, Money, and Politics*, The University of Chicago Press: Chicago.
[23] Robert M. White (1990) "Science, Engineering and the Sorcerer's Apprentice," Address to the annual meeting of the National Academy of Engineering.
[24] Hearing before The Subcommittee on Investigations and Oversight of the Committee on Science, Space, and Technology, U.S. House of Representatives, 102nd Congress, April 8, 1992, pg. 556-558.

in the same category. Congressional disillusionment set in, and NSF's reputation was tarnished. This was a difficult time for NSF because they considered their reputation for being above reproach regarding numbers to be crucial for the continued support of science by Congress.

I noted that the original demographic analysis was very simple, simplistic even; proper workforce arguments are much more complex. That should have been caught in peer review in NSF. Internal problems in NSF were revealed in the hearings, including compromised or absent peer review of these reports. For example, text in *Science and Engineering Indicators* was reworked by the PRA unit anonymously in review to support the shortage analysis. It is ironic that around this time PRA funded the project that produced the most influential and respected number in the history of science policy – Mansfield's 28%.

Conclusion

This paper has reflected on a few examples of scholarly work in science policy that have proved highly influential on policy. There are no doubt many more examples. In addition, we should not forget those highly influential sources of numbers that are more diffuse, and exert influence over a longer time span – NSF's *Science and Engineering Indicators* for example. The recent release of *Science and Engineering Indicators* has gathered a lot of press coverage for showing the strength of the Chinese and Asian research systems now almost equaling that of the United States. Shanghai Jaio Tong's ranking of universities around the world has been very influential in Europe.

There are several lessons to be drawn from the examples considered here. Policy impact is made by numbers, single numbers. Only Mansfield actually gave the number in his paper. Other scholars did not and policy makers drew out the number that was subsequently used. Those who want to have an impact might be better taking the Mansfield approach and clearly focusing part of their paper on a number. This may help with the accurate reporting of the result in policy circles. Policy influence only comes through the pithy number.

For the most part, the numbers were used to advocate for more research funding (in this as in several other respects the Australian example is an exception). One can advocate for more money in two ways, either by saying the money being spent already is producing very high value (like Mansfield), or by pointing out national crisis and decline due to a funding gap (like Marting & Irvine). In the United States the numbers that were influential were numbers celebrating the strength of the scientific system – 28%, 73% and 13x. Crisis and decline worked better elsewhere – Britain and Australia, but proved toxic here – 675,000.

Scholars whose work is influential cannot afford to worry too much about caveats or even accurate representation of their results. Some advocates are going to get it wrong. However, scholars must attend to caveats and quality in their original papers. Independent peer review of influential analyses is absolutely critical. If peer review becomes compromised or is absent scholars need to extract themselves from the situation very quickly. This is what went wrong in the 675,000 example.

Finally, it is not clear any of these studies were funded through peer reviewed grant funding mechanisms. Most underwent review, but were commissioned. Mansfield's study for example was solicited by PRA; Narin's work was funded by a larger contract to produce tables for *Science & Engineering Indicators*; my study was for the Small Business Administration which solicits studies on topics of interest and internally reviews the proposals; Martin & Irvine were funded by a consortium of agencies, not the social science funding council in the UK. The PRA work was internal to the NSF. The results discussed here were descriptive and comprehensive. In the United States, social science peer review privileges empirical work that is theoretically interesting and analytically complex (complexity is not ideal for producing a clear number). Comprehensive, systemic analysis and description is not well respected in social science scholarly circles. Having said that, the published papers discussed here are all highly cited.

We may conclude that science policy makers over the past few decades have drawn upon analytical scholarly work, and so scholars have produced useful analyses. However, the relationship between policy and scholarship contains tensions. Policy users need a clear number. Scholars seem afraid to draw a strong conclusion, and do not encapsulate their discoveries in simple numbers. Scholars prize accuracy and caveats. Policy makers find caveats muddy the message and their staffs' comprehension of the results they are summarizing is not always accurate. Scholars aiming for impact should attend to cultural differences and avoid stories of decline in the US and of supremacy abroad. Finally, standard peer review granting mechanisms may never fund analyses with the impact of the studies reported here because of differences between the ethos of peer reviewers and the needs of policy makers. Nevertheless, peer review of the results of studies is crucial and must never be compromised.