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A Science of Science and Innovation Policy Research Agenda

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I. Introduction

Dr. John Marburger's recent calls for a new science of science policy open up new opportunities to reconceptualize, retest, and revise as needed the theories, models, descriptions, and mainstream propositions underlying United States' science and innovation policies and programs.

We respond to these calls by presenting a research agenda directed at two objectives. First, as academic researchers who have long worked in the field of science and innovation policy, albeit from different analytical and disciplinary perspectives, we seek to insure that efforts to promote the "science" of science and technology, or innovation policy produce substantive scholarly work that in fact advances our fundamental understanding of underlying processes. Second, as participants in numerous U.S. and international science and innovation policy advisory forums and commissions, we seek to promote a closer, better fitting, coupling between the research communities who are addressing questions of the science of science policy -- themselves a disparate disciplinary lot -- with the policy communities who are seeking improved understandings of whether or how the decisions they have made or are being called upon to make in fact have led to the intended results. Our strategy to achieve these two objectives is to identify questions that are simultaneously intellectually challenging and policy relevant.

The earliest formulations of the call for a new science of science policy emphasized "models", especially those that might be helpful in answering questions concerning the total level of Federal investments in r&d and their allocation among fields. Implicitly, this formulation defined science as logical constructs about the workings of the U.S. science and innovation system that led to the formulation of testable, verifiable predictions of the consequences of adopting a specific course of action, relative to alternatives.

Science and innovation policies, indeed, are the articulation, explicitly or implicitly, of theoretical propositions. Some propositions are well documented, and have induced widespread consensus---e.g., contributions of public r&d expenditures to aggregate economic growth. Others remain in dispute or disagreement—e.g., crowding out effects of public investments in private sector technology programs. Others remain to be tested, or cannot be tested until well after large public investments have been made—e.g., direct contributions to knowledge and indirect contributions to other societal objectives from investments in elementary particle physics.

A science of science and innovation policy must address these and other equally differentiated questions. Consequently, it must be equivalently diverse and flexible in conceptual framework and methodology. We thus adopt here a broader, more eclectic definition of science than suggested by the concept of model, alone.

Our working concept of rigorous research in science and innovation policy includes description, taxonomy and data collection and analysis, as well as theory construction, hypothesis formulation and testing, and normative analysis. We do this in part to more fully and accurately describe the methodologies that can contribute to top quality research. We also do this to insure that the research base so developed is applicable to the broad set of societal objectives for which public funds are invested in science and technology. Research in this area can inform policy in many ways, including both prediction and enlightenment. Seeing the system through new eyes can be as important as modeling it quantitatively.

We start from the broad "policy for science" issues relating to the structure of the U.S. science and innovation system, move to principles concerning rationales for public investments in research and development, then to operational matters, and finally widen this domestic orientation to place the U.S. in a global science and innovation system. The proposed research agenda, not surprisingly, includes longstanding questions, standard issues restated to account for changed environmental conditions, and combinations of familiar issues that produce new issues. Left open for further treatment is the construction of a research agenda for "science in policy", especially how best to account for the use, nonuse, or misuse of science by policy makers in shaping public policies.

II. A Research Agenda

1. U.S. National Innovation System.

Even as the metaphor and implicit model of a U.S. national innovation system within which different sectors are funded to perform specific roles and which connect, tightly or loosely, to one another in well-known ways, continues to be wisely used, increased discontent is being voiced the model's descriptive or analytical adequacy. As expressed in two recent National Academies reports, *Measuring Research and Development Expenditures in the U.S. Economy* (2004) *and Understanding Business Dynamics,* (2007), there is increased recognition that the categories underpinning the surveys that flesh out the model are outdated. <u>Measuring R&D,</u> for example, notes that the models of innovation that underlie the data are "… increasingly unrepresentative of the whole of the R&D enterprise," omitting such factors as "the growth of the service sector, the growing … role of small firms in R&D, the shift in funding from manufacturing R&D to health-related R&D, changes in geographic location and the globalization of R&D." Further, some innovation scholars (for example, von Hippel (2006) are describing new modes of innovation that are not centered at all on corporate R&D and therefore obviate traditional vocabulary and concepts.

Without contemporary concepts for the workings of r&d in the economy, assessments of existing policies and predictions about the effects of new proposals can be misleading. Studies for example abound of the impacts of the Bayh-Dole Act's on university patenting and licensing. These studies however have little to say about whether university behaviors are leading to changes in the behavior of globally positioned r&d-intensive firms, which now report substituting university and research scientists in other countries for U.S. faculty and universities. Even less consideration has been given to the possible long term effects caused by such a substitution upon the strengthening of the academic research infrastructure of these countries that may reduce the flow of foreign nationals to U.S. universities for graduate degree training, followed in turn by the loss to the U.S. of the contribution these individuals make as entrepreneurs or faculty.

Simple, perennial issues, but ones for which reliable, consistent answers remain difficult to provide.

2. Rethinking/Reviewing the Rationales for U.S. Science and Innovation Policies

National and state science and innovation policies and programs are a concatenation of big and small theories. "Big" theories include the contributions that science and technology have made and are projected to make—indeed are deemed as essential inputs--to overarching national interests, such as national defense, economic growth and international competitiveness and growth, and the inherent quest by members of a society to more fully understand the workings of the universe which they inhabit. Small rationales include the lure of scientific or technological opportunities—realistic or fanciful; various forms of geographic, institutional, demographic equity; and episodic analytical or policy fads.

Some combinations of the above are logically consistent, derived from theories of processes of scientific and technological discovery, suboptimal levels of private sector investment, and the contributions of scientific and technological innovation to national and/or regional economic growth; many are opportunistic or symbolic; a goodly number are inconsistent, requiring agencies and performers to torque their activities to meet wasteful, vague, or counterproductive performance expectations and outcomes.

This critique is double-edged, pointing both to the prospects of analytically flawed rejection and endorsement of policies and programs. As brief examples, the market failure paradigm continues to dominate OMB's formulation of its R&D Investment Criteria, and thus PART reviews for many federal agencies, even as contemporary economic analysis emphasizes the unlikelihood that there is any production function, as Foray has phrased it, "that can be used to forecast, even approximately, the effect that a unit of knowledge will have on economic performance." Conversely, if one likes, statements that existing programs are "working" are analytically vacuous without explicit statements of program objectives, predetermined agreement upon what constitutes success, use of comparison or control groups in evaluations, and consideration of foregone alternatives.

These observations point to the need for a two-part research approach: first, following Will Roger's adage that its not what we don't know that hurts us but the things we know that ain't so, reconsideration of the rationales for existing programs and contemporary proposals in light of emerging, empirically-tested findings and new analytical methodologies; second, systematic examination of the how and why theories and findings concerning the design and impacts of science and technology policies are used across levels of the Federal government.

3. Setting Research Priorities and Selection Mechanisms

The core challenges in setting priorities for allocating resources to fundamental science remain the presence of uncertainty about which lines of inquiry will yield the most important advances in understanding of natural and social phenomena, and the lack of consensus that can exist at times about onward directions suggested by initial sightings at the frontier. In this respect, Dr. Marburger's 2002 call for a new science of science policy that provided for "… a systematic way of ordering the opportunities so finite resources can be invested to best effect", sounds a familiar refrain that dates back to at least to the 1960s, when Alvin Weinberg's advanced his intrinsic and extrinsic criteria for scientific choice (Weinberg, 1963; 1964). In effect, many of the evaluative and predictive techniques of recent years, e.g., bibliometrics, patent analyses, foresight, roadmaping, etc. have been designed to provide evidence of past and current performance to better inform prospective actions. But one interpretation of recent calls for new models is an assessment that little if any progress has been made over the past 40 years, at least at the level of program and budget detail needed, or desired, by policy makers.

Our interest here is more than an assessment of past progress; rather it is to suggest the need to think ahead if current and future initiatives directed at producing more accurate predictive models prove chimerical. For example, a recent National Academies report, A Strategy for Assessing Science (2007), has concluded that "No theory exists that can reliably predict which research activities are most likely to lead to scientific advances or to societal benefit" (p. 89).

This assessment may be too pessimistic in forecasting what new theoretical or methodological pathways for forecasting and prioritizing science priorities lie just ahead. It also may set too high a standard for the form and content of the models and algorithms needed by policy makers to have increased *ex ante* confidence in the wisdom of their decisions and *ex post* records of success in having made the right investments—the perfect being the enemy of the good.

But accepting these assessments as valid, at least for the near term, means that answers to the science and technology policy questions cited above will likely continue to be made on the basis on a combination of expert judgment and competitive, peer review processes. Our own experiences as participants and researchers attests to the desirability and effectiveness of these processes, especially, in most cases, with alternatives such as earmarking or set asides.

This endorsement however does not constitute an uncritical reaffirmation of the views expressed in recent reports that expert review is the most effective means of evaluating federally funded research programs or that peer, merit review is an international gold standard for review of science and engineering proposals. The limitations of these mechanisms, at least as conventionally implemented, are increasingly evident in at least 3 important science and technology policy settings: (1) forecasting trends in fundamental research not only within but across fields of science; (2) receptivity to discontinuous, radical, transformative research; (3) receptivity to interdisciplinary research

Relatedly, a recent stream of empirical research and participant observation has called attention to the dependence of the outcomes generated by expert/merit review panels to panelist attitudes towards scientific risk and their cognitive maps concerning the structure of knowledge;

small group dynamics; the number and ordering of criteria; voting rules; instructions provided by funding agencies; role of program managers--and other factors.

In the spirit of a science of science and innovation policy, these concerns and findings should be seen as framing a research agenda that seeks to examine the inner workings of expert judgment/peer review processes. Numerous modifications or variants of existing expert/peer review procedures exist. These include criteria for selecting participants, voting rules, or even allowing for a modicum of randomness in the selection of proposals. A science of science policy needs to allow for experimentation. Each of these proposals may be seen a hypothesis about how to improve outcomes. They need to be tested.

4. Human Resources.

If there is one area of STI policy where there should be a solid research base, this is it. In fact, as highlighted in recent congressional testimony and reports, policy prescriptions relating to “shortages” or “surpluses” in scientific and technical personnel are beset with conceptual and empirical problems. Recent legislation, however well intentioned, points though to a continuing propensity to make policy on the basis on simple solutions and simple formula. The empirical and analytic support underlying the <u>Gathering Storm</u> report was weak, for example. The report made little use of major human resource surveys with roots as far back as the 1950s and subsequent research by sociologists and economists that provide important insights into s&t career patterns. These studies have tracked the steady influx of white women into science and engineering careers in the U.S., and punctured various myths.

Research on the s&t labor force also wears thin in addressing the challenge of how the Federal government can most effectively pursue national s&t objectives in the context of historic divisions across levels of government for K-12 education, and distributed responsibilities among agencies, primarily as between the National Science Foundation and the Department of Education. COMPETES proposes major infusions of Federal funds to increase the number of math and science teachers in K-12 education; at question though are the basis for its specific numerical objectives and the effectiveness of the math and science education programs it seeks to foster.

The (unfavorable) international comparisons that underpin much contemporary discourse and policy also suggest deeper strata of causation than obvious through test scores or graduation rates alone. Many OECD countries are facing a science and engineering recruitment challenge that looks much like the U.S. These also are "advanced" economies, in which bright young people can obtain much better salaries in service industries than they would earn in science or engineering careers. A reasonable hypothesis is that this major structural shift in the global economy is connected to the recruitment issues, but the S&T policy research communities have barely begun to look at the connections or begin to generate policy options under these conditions.

5. Defense R&D

The statistic that most sharply separates public sector r&d in the U.S. from that of other industrialized economies is the higher percent allocated to defense related ends. Defense r&d needs more systematic attention precisely because it is such a large portion of total Federal r&d and thus potentially has impacts on the performance of the larger U.S. science, technology and innovation. In particular, improved models and methods are needed to test hypothesis concerning the impacts of military r&d on non-defense technological innovation, the competitiveness of U.S. industries, regional growth patterns, and the recruitment, training and distribution of s&t personnel.

For example, well documented examples exist of major industries that have started or been sustained by military R&D, including the computer and aerospace industries. On the other hand, efforts of major defense contractors to diversify into the civilian sectors suggests that military requirements often do not stimulate the development of products that can survive in civilian markets. Relatedly, military installations are spread around the country, but military laboratories and contractors are more spatially concentrated. Does the R&D-intensive side of military activity produce different effects in local economies than a more standard installation?

6. Political Science of Science and Innovation Policy

Science policy, as directed towards the attainment of specific national objectives and the production of benefits for different constituencies, is manifestly a political process. It reflects the interaction of national objectives and local interests, the structures and workings of Congress, the Executive, and Federal agencies, as influenced by various constituencies, as well at any point in time of the sway of partisan control and dominant ideologies. All this is obvious.

What is less obvious-or at least not systematically understood- is how these actors and factors interact at various points in time to produce specific outcomes. Retrospectively, one can perhaps account for the constellation of influences that led to the doubling of NIH's appropropriations. Is it though simply a matter of "balance" that pending Federal budget proposals call for substantial increases in funding for the physical sciences and engineering (and thus the Department of Energy, NIST, and NSF) while NIH's funding is essentially static? Is it instead something else or different, relating to the dynamics shaping the formation, force, and staying power of coalitions of interest, as suggested in recent theoretically sophisticated and empirically rich public economics and political science literatures?. More importantly, given the widespread view that these sharp discontinuities have been inefficient—leading alternately to feast and famine conditions that disrupt scientific careers and induce under- and over-investment in physical infrastructure, what changes, if any, in the structures, processes, or criteria by which the executive and legislative branches form science and innovation policies and budgets might produce a more efficient and stable funding pattern?

Similarly, with a view towards understanding, and to a degree predicting, the outcomes of the dynamics of political processes upon science and technology policy, how are coalitions of interest formed to champion or oppose specific initiatives? For example, what coalitions, pro and con, might be expected to form, using what arguments and with what influence, around recently advanced proposals to increase the SBIR set aside above 2.5%?

8. Performance: Expectations, Accomplishments, and Assessment

How are expectations for gauging the performance of the U.S. scientific enterprise, in its totality or with respect to any specific field of science or performing sector set? What criteria are appropriate for gauging performance, effectiveness, or success?

Of especial import here is how the claims on behalf of basic science investments are made. A notable and commendable feature of recent proposals for increased U.S. investments in elementary particle physics and plasma science is that they have not been cast in terms of direct or near-term links to technological innovation and economic competitiveness. But how then justify not only initial commitments but end state outcomes?

A different set of performance issues surfaces sharply for a cross-section of Federal technology or academic research programs. What constitutes “evidence” of success, effectiveness, or efficiency? Compared to what questions have loomed large in PART reviews and in evaluations of the Advanced Technology Program, Manufacturing Extension Program, Small Business Innovation Research Program, and the Experimental Programs to Stimulate Competitive Research of several agencies.

Any large-scale program, whether measured by amount of dollars expended or participating entities can be expected to generate some number of positive results—success stories—be it in terms of newly competitive firms, singular faculty or institutional research awards, or start up of new firms. Conversely, the inherent uncertainties surrounding r&d, especially basic research undertakings, implies some number or percent of “unsuccessful” projects. Absent clearly defined expectations and initial agreements on what science and technology programs are designed to achieve, current reviews of Federal science and technology undertakings, whether by OMB or via Congressional hearings, are guaranteed to produce findings that accommodate differing positions.

9. U.S. Science and Innovation Policy in an International Setting

How do U.S. investments in science and technology co-evolve with the larger society, national and global? As recently as a decade ago, the leaders of the U.S. science and engineering enterprise listed “world leadership” among their goals. Yet as the evidence of growing strength elsewhere mounts, the U.S. clearly needs to plan for a future in which it is among the world leaders, rather than the dominant force, in most areas of science and engineering. Does, for example, the earlier formulation that the U.S. should seek to be a leader in some fields of science and a fast second in others still hold, given the high costs of major scientific undertakings, increased patterns of international scientific cooperation, polycentric location of r&d laboratories by multinational firms, emergence of new and potentially significant contributors to international science, such as China and India, and rapid diffusion and transfer of scientific and technological knowledge across national borders? What strategic principles should guide U.S. science and technology policy in a world increasing characterized by complex sets of interactions – partly cooperative, partly competitive, partly independent – with other countries?

These questions also spill over to innovation policy and the import of the above trends on U.S. economic competitiveness. For example, if scientific leadership shifts to other countries, are U.S. industries equipped to be fast followers: is their absorptive capacity equal to their innovative capacity? Will U.S. universities be as capable of providing a window on the rest of the world as they have been of providing a window for the rest of the world? Under what conditions will U.S.-based economic activities thrive in the new, open, global economy?

Research on these topics has to move fast to keep up with the changing realities. But existing national industry surveys do not do much to track international patterns, and national data sets on the workforce include scant information on the increasing flows of scientists and engineers between countries and regions. With a few exceptions, the research on international collaboration in science and technology is not charting this process. Part of that literature focuses on international collaboration in Big Science, a game that mostly the rich countries play. A large part draws on evaluations of foreign aid programs and thus neglects he much larger pattern of technology transfer through multinational firms. The literature that exists on the latter tends to focus on local firms in developing countries; the U.S. angle still needs to be addressed.

There is thus considerable scope for new research on the S&T elements of relationships in a developing world economy. Since the dominant theoretical model relies on the idea of science as a self-organizing system, there is plenty of scope for testing the effect of policy interventions in that system. Mapping technology transfer through its public and private routes will be an important part of the agenda, and studying the conditions under which technology transfer builds lasting capacity and continuing partnerships will also be important.

III. Conclusions

Many items on the above agenda are familiar ones, but this should not be a surprise. Their inclusion essentially reflects the challenges that researchers and policy makers confront as they try to reduce the uncertainties and complexities surrounding processes of scientific discovery, technological innovation, and the ways in which new things impact upon a society into operational policies and programs.

Given these challenges, modest expectations about outcomes are in order by those calling for and funding the new science of science and innovation policy. Perhaps even more so, modesty may be needed on the part of those advancing claims about new and improved theories, models, or algorithms. Whether the current initiative for a science of science policy will succeed or fail is likely to depend less on having more pieces of research to play with as on how well the pieces are put together. We have tried to indicate here that the pieces currently available to fill some parts of the picture in detail and leave others blank. Agreeing with Dr. Marburger's call for evidence-based policy, we believe that a deeper dialogue between policy and research communities across many venues is a necessary precondition for completing the puzzle successfully.

Suggested Reading

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