

**Research Note****INVESTING IN KNOWLEDGE PRODUCTION:  
SHOULD POLITICAL SCIENTISTS BE PAID TO THINK?**

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**Introduction**

As a reviewer for the National Science Foundation's Political Science Program and also for the Law and Social Science Program, I have reviewed several proposals by political scientists (and also by economists) which ask NSF to provide them summer salary and even release-time in order that they can think about (and write about) some topic. The funds requested are exclusively or almost exclusively for personal salary. The topics proposed (usually some area in social choice theory) are often interesting, and the likelihood of research payoff quite high, especially given the researcher's customarily excellent previous track record. I have recommended most of the proposals positively; nonetheless, I began to have second thoughts. These second thoughts, and reflections on research priorities, form the basis of this research note.

In this paper we consider the problem of Political Science/Economics program research budget allocations in a simple heuristic way, beginning with a short-run 'societal' perspective on what is to be maximized, and gradually broadening our horizons to take into account both longer-run consequences of funding and potential deviations between 'societally oriented' objectives and other agency goals.

**The Short-run 'Societal' Perspective**

In the short-run 'societal' perspective, the funding agency is presumed to be seeking to maximize the total research in a given discipline given a fixed budget constraint. We assume for simplicity that this product can be measured in some single numeraire, in which quality and quantity considerations are combined, and we leave the problem of tradeoff between quality and quantity of research for another paper. Similarly, we slide over questions of measurement. Rather, we simply assume that a consensus exists as to the research merit of proposals and as to what constitutes research '*in the discipline*', and that the research share allo-

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cated to each discipline is fixed. Within the agency a separate program unit considers proposals within a given discipline. We also neglect the possibility of alternative funding sources for research within a given discipline.

Having simplified the issues with such heroic abandon, it might be thought that nothing is left of interest in the problem. Since the agency can judge the relative merits of proposals, it might be supposed that the agency should, *like any other consumer*, simply invest in such a way as to maximize the total productivity of its investments by investing its marginal dollar on whichever proposal yields the highest marginal return to investment measured in terms of research product.<sup>1</sup> There are two difficulties with this view. First, research proposals are quasi-lumpy goods which may have some rather strange-looking production functions, and thus maximizing total research product may involve some sticky technical problems.<sup>2</sup> Second, *and far more importantly*, there may be a considerable difference between investing in those projects which have, in sum, the greatest research merit (a pattern of investment which we refer to as the 'merit rule') and investing in such a way as to increase maximally the total research product of research in a given social science discipline (a pattern of investment which we refer to as the 'marginal productivity' rule, for reasons that become clear in the discussion).

Investing in research has both public goods and private goods aspects. Hence, modelling it as a problem in consumer choice can be quite misleading. For consumers, the goods they do not purchase they do not get to make use of. This is not so for a public agency concerning itself solely with maximizing the total research product, since contributions to the total research product by scholars unfunded or only partly funded by the agency are as relevant to such an agency's utility as is the research the agency itself supports. An agency with such a global utility function should invest, *not* so as to maximize the total research product of the projects *it* funds, but such as to maximize the total research product of *all* scholarship in the discipline. In this view, *projects that would take place even if the agency did not fund them should be given low priority*. Only when projects that would take place (or take place with high probability) even if the agency did not fund them are *quite* superior to other projects should funding be justified, since the value of such projects must be largely discounted; i.e.

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1. It is my opinion that this is the rule that NSF now tries to follow. Hence, the reader should be warned that what I call the 'societal' perspective throughout this paper is not one I believe agency officials to be following in practice.

2. For example, for some projects, for some ranges of investment, marginal research product may be negative throughout the range, since a less than adequate investigation may lower the sum total of human understanding when the researcher's dissemination of its findings lays claim to a certainty to which the work is not entitled (cf. 'A little learning is a dangerous thing'). Such technical complications are omitted in this discussion.

only the *marginal* effect of the agency contribution on research product should enter into the agency decision calculus.

### The Middle-run 'Societal' Perspective

If the short-run 'societal' perspective we have outlined above is to be followed by a funding agency, agency decisions based on a marginal productivity rule can have middle-run consequences of a somewhat pernicious sort. In a discipline where some research requires little more than pencil and paper (or perhaps access to a library) and other research requires costly interviewing or computer budgets, in the short run we would anticipate that projects that can be done without research funding will be done regardless of whether research funding is provided, and projects that require extensive funding will be dropped when such funding is not available. Thus, in the short run, denying funds to 'theorists' will not hurt the total research product much, whereas denying funding to survey researchers, data compilers and experimentalists will.

In the middle run, if funds for theorists become harder to come by because theoretical research, though as good as or even better than the research of other more empirically minded scholars, goes unfunded because the funding agency 'knows' it will be done anyway, then theorists unhappy with contributing their research as a 'public good', and feeling strongly the inequity of the agency decision calculus (albeit impotent to argue against it, since it is, after all, based entirely on notions of 'economic' rationality), may begin to shift into other lines of research.

Of course, since most theorists have invested considerable time and energy in their present research directions, because of both ingrained tastes and specialized skills, changes in research direction will be slow in coming and only some theorists will change their established patterns of research activity.<sup>3</sup> Furthermore, even though theorists will no longer be able to earn summer salary for thinking, the personal and professional rewards of publication (and concomitant salary increases) remain to motivate research efforts; and it is quite unlikely that the names of quasi-convexity and quasi-transitivity will be heard no more in the land.<sup>4</sup> Nonetheless, especially at institutions where merit is measured at least in part

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3. 'Man can do anything', Pascal once said, 'except sit quietly and *not* think.' This is presumably especially true for theorists. (I am indebted to Sheen Kassouf for calling this quote to my attention.) Of course, there are ways of spending one's summer productively that still involve thinking, but for a funding source other than NSF.

4. That the work of theorists is less likely to be funded does not, in our idealized world, in the short run at least, affect perceptions of the *scholarly* merits of their work on the part of, e.g., journal editors. Recall that we are assuming that discrimination against theorists on the part of the funding agency is caused not by the fact that they do inferior work but because they are likely to do work *whether or not they get special funding for it*.

by grants brought in (which is to say most, if not all, institutions), the status of political science theorists would be falling, and this would be communicated to students, leading to a shift among the next generation of scholars away from pure theory and toward more empirically focused research. Such a shift would have consequences for the present generation of theorists as well, since falling enrollments in their classes would increase the pressure for them to shift in a more applied direction so as to avoid a complete divorce between research and teaching interests.

### **The Long-run ‘Societal’ Perspective**

In the long run, although the NSF Political Science (Economics) Program is only one of the factors in the political science (economics) research environment, a structuring of allocation decisions along the ‘marginal productivity’ of investment lines indicated above would reverberate throughout the profession and significantly affect choice of research topics and, at the margin, career choice as well.<sup>5</sup> NSF awards ‘signal’ merit. Hence, an NSF award has a multiplier effect on an individual’s career, making it easier for him or her to get other funding and leading to increases in within-university rewards (e.g. office space, research assistants, pay raises, etc.) and a greater likelihood of desirable outside offers. There would, of course, be some long-run equilibrating effect, since, if it were no longer true that most theoretical work would go on in the absence of funding, then the marginal value of funding theorists would rise. But, since funding decisions would be based on short-run rather than long-run calculations, this equilibrating effect should not compensate for the shift in priority toward capital-intensive research, and the total research product in the area of theory would fall.<sup>6</sup> On the other hand, insofar as capital-intensive projects were being funded that would not be funded if funds were not awarded on marginal productivity of investment grounds (and because some good theorists would have shifted their research interests into such projects, thus making those projects more competitive in merit terms), it is not clear whether or not the total research product would be larger or smaller than under a strict merit investment rule.

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5. If different rules are followed in different programs, if, say, economics rewards mathematical theorists and political science does not, some who might have become political scientists will not.

6. Since calculations are based on the proposal before the agency, the question would be whether a particular piece of research would be done in the absence of funding, not whether the researcher would *in the future* shift research priorities away from theoretical research. Moreover, theorists would largely cease to submit grants, since the cost of grant preparation would exceed the expected return.

### More Realistic Perspectives on Agency Decision-making

In the discussion so far we have made three rather sweeping assumptions that we now wish to reconsider. First, we have assumed that the funding agency does not distinguish between the research contribution of the projects it funds and the contributions to the total research product of other (non-funded) research, i.e. that all it cares about is maximizing the total research product. Second, we have taken the agency's budget as fixed and have not looked to see how its funding decisions might affect its budget, and thus the long-run total research product as well. Third, we have assumed that the agency is interested only in research product and not in equity considerations.

Because funding agencies must justify themselves in terms of the productivity of the research they themselves fund, we would expect that they would be more likely to use a 'merit' rule rather than what we have called a 'marginal productivity' rule, even if this meant that the total research product would be reduced because the agency would be diverting funds from capital-intensive projects into projects that would be completed even in the absence of agency funding *but for which the agency could not claim credit if it did not fund. The agency can never get credit for leaving well enough alone.*

If the agency's budget is not fixed, the long-run consequences, for both the agency and the discipline it funds, of moving *away* from a marginal productivity rule are uniformly positive for the agency and mixed for the profession. By funding projects on the basis of simple research merit and productivity, the agency will establish a strong track record and thus increase both its budget share and its resultant contribution to research efforts in the discipline it serves. Moreover, it will not have to cope with the displeasure of theorists who submit superior proposals only to see them rejected in favor of capital-intensive projects. Furthermore, it will fund more projects (since theoretical projects are generally cheaper than capital-intensive ones) and make more individual scholars happy. On the other hand, as noted before, the total research product may be lowered because the agency is 'wasting' money on work that would have been done anyway, and the increased research funding available to it may or may not compensate for this.<sup>7</sup> In the long run, too, some shift in research directions toward theory and away from empirical research will occur. This may well mean a further loss in total research product as technically ill-trained scholars, whose priorities shift in the direction of more theoretic-

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7. A closely analogous issue is investing in special education for gifted children vs. the educational needs of the ordinary child. Gifted children may 'deserve' more, but they can also probably do more with less. If what we have called the marginal productivity rule were adopted, using a very short time perspective, some gifted children might actually have *less* money spent on their education than their less gifted counterparts.

cal (and, in their judgment, more readily fundable) work, do research (not necessarily funded) that is a *negative* contribution to human knowledge.

On balance, however, the merit rule would seem to have advantages from the agency's realistic perspectives that more than outweigh potential disadvantages relative to the marginal productivity rule. This is especially true if agency officials realize that agency awards signal not just meritorious research but meritorious individuals. *If the long-run total research product would be much the same under the two rules, then the agency might wish to adopt the merit rule rather than the rule of maximizing the (short-run) marginal productivity of its research allocations, since the former appears far more 'ethically' appealing than the latter.*<sup>8</sup>

On the other hand, *if equity considerations are neglected*, the optimum strategy for the agency would seem to be something like the following: 'Fund projects which are likely to produce results, even if not funded, but fund them only *nominally* – thus getting full credit for a large number of projects but retaining budgetary flexibility to fund massive capital-intensive projects'. In this case the conclusion would seem to be: *Social scientists should be paid to think, but not paid too much.*

If the funding agency wishes to increase total research product, a marginal productivity rule may (in the long run) be best; however, such a rule cannot in the short run be sustained against the bureaucratic necessities of justifying continued and indeed expanded funding and against equity arguments for the support of meritorious research and meritorious individuals per se. Thus, in our simplified model (where problems of discerning research merit do not exist), the funding agency can be expected to adopt something like a merit rule, even though that may go against the 'societal' view of what is to be maximized, i.e. the agency maximizing the *marginal* product of its research allocation.<sup>9</sup>

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8. This result may be compared with Severin Dardin's classic demonstration that fish think – but not fast enough.

9. Implicitly we have been assuming that NSF research contributions yield diminishing marginal utility with respect to total research output. In particular, we have assumed that a great deal of work would be done even if NSF never funded it, but that to move much beyond this level would probably require sizable investment.