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Nonneutralities in Science Funding: Direction, Destabilization, and Distortion

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Nonneutralities in Science Funding: Direction, Destabilization, and Distortion

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Abstract

We treat science as a Hayekian social order whose distinctive emergent characteristic is the generation of knowledge. We model modern science as an institutional form that principally relies on publication with citation and its effects on individual reputation in order to study the possible effects of funding on science. We develop a taxonomy of three broad categories of effect: those having to do with the direction followed by scientific activity, those involving the operational and financial stability of both the physical institutions integral to scientific work and the scientists themselves, and those due to distortions of the basic knowledge-generating procedures of science. It is argued that, while directional effects of funding are ubiquitous, destabilizing and distorting effects are much more likely to emerge when funding sources are concentrated than when they are decentralized. Further, when funding is accompanied by regulatory oversight, the possibilities for distortion are significantly increased. Examples of such effects actually occurring under the current U.S. funding regime are discussed.

KEYWORDS: science, funding, knowledge-generating institutions

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1. Introduction

Science, as a social order, is not self-supporting – it does not generate the revenue necessary to support itself and so is reliant on external funding. And such funding, being inevitably limited with respect to the potential scientific research projects to which it could be applied, is, of necessity, nonneutral. It is impossible to fund science in general; any injection of funds must be directed to particular scientists or institutions and to particular activities in specific fields within science. How this nonneutrality actually affects science depends on two things:

1. The characteristics of the sources of funding. A major consideration here is the degree of decentralization of funding sources. If, for example, the pattern of funding involves a concentration of major funding in a small number of funding sources, the pressures on science are likely to be quite different from those experienced in a regime of many dispersed, unrelated funders each with its own particular agenda.
2. The mechanisms within science which react to the funding. It is obviously necessary to describe and understand the particular mechanisms at play – the procedures scientists follow in the process of generating scientific knowledge – if one is to be able to predict the types of effects that can be expected to result from funding pressures.

Our concentration here is on characterizing the possible effects of funding (from whatever source) on science, and we identify three broad categories of effect: those having to do with the direction followed by scientific activity, those involving the financial stability of both the physical institutions integral to scientific work and the scientists themselves, and those due to distortions of the basic knowledge-generating procedures of science. Although directional effects are inherent in all funding environments, our emphasis here is to highlight destabilizing and distorting effects. We argue that these effects are potentially more important than directional effects for maintaining the coherence of science and that they are more likely to emerge when funding sources are concentrated than when they are decentralized.

We are not suggesting policy, nor are we passing judgment on the output of any particular scientific activity. Our aim is more general: to identify and discuss the *kinds* of effects that science can experience as a consequence of external funding and to provide taxonomy of such effects to assist in clarifying the nature of the issues involved. We first discuss the theory behind our taxonomy and then elaborate the taxonomic categories of direction, destabilization, and distortion themselves. Then, in order to show that these rather abstract categories have relevance for understanding science funding in the real world, we discuss case studies drawn from the funding system that has existed in the U.S. since the end of WW II.

2. Science as a Knowledge-Generating System

The conception of science used here is of a social structure, a decentralized system of social interaction, whose principal and distinctive emergent characteristic is the generation of knowledge. We draw on Polanyi and Hayek in thinking of science as an evolving emergent order. The modern development of this approach to science can be credited to Polanyi's early work in countering attempts to centrally plan science.[1] And although Hayek first mentioned emergent (or "spontaneous") orders as early as 1933, his interest in evolutionary social phenomena became increasingly evident in his later work, as seen, for example, in the first volume of *Law, Legislation and Liberty* (Hayek 1973).[2]

We characterize modern science as a particular institutional form that principally relies on publication with citation and its effects on individual reputation.[3] Scientists publish speculations and observations; other scientists who find these useful to their own work (or who wish to criticize them) cite them; the citation feeds back to affect the reputation of the publishing scientist; and a scientist's reputation not only affects the notice given to his future publications and citations but also his ability to attract funding or to advance in academic position. This recursive set of procedures and feedback loops, hereafter referred to as "PCR" (for Publication-Citation-Reputation), implements the knowledge-generating characteristic of the scientific order. In this picture, "scientific knowledge" is not the knowledge of individual scientists; it is the end result of the PCR processes repeatedly acting on individual scientific contributions, ignoring, altering, merging, selectively abstracting, and reinterpreting them in the process.

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1. See Polanyi (1939; 1941; 1945; and 1962)
2. Also see Jacobs (1999) for a recent discussion of Polanyi. Hayek, in "The Trend of Economic Thinking" (1933, section VIII), took the occasion of his Inaugural Lecture for the Tooke Chair to draw attention to society as an "organism and not an organization" (p. 130) and noted that "From the time of Hume and Smith, the effect of every attempt to understand economic phenomena … has been to show that, in large part, the co-ordination of individual efforts is not the product of deliberate planning, but has been brought about, and in many cases could only have been brought about, by means which nobody wanted or understood" (p. 129). Elaborating these themes is a hallmark of much of Hayek's work after the 1940s. For recent discussions of science as an emergent complex order, see Wible (1998, ch. 8), McQuade (2007), and Butos and Koppl (2003).
3. See Merton (1973; 1996), Hull (1988), and McQuade and Butos (2003). We do not address here the different economic conceptions of science, such as those of Nelson (1959), Arrow (1962), or Dasgupta and David (1994); these are dealt with in detail in Butos and McQuade (2006, pp. 185-193).

Scientific activity is immersed in society in a web of intertwined interactions, and it would be a daunting challenge to try to deal with such a complex as a whole. Our model simplifies by separating out the activities of scientists that are fundamental to the process of knowledge generation, regarding these as "the system", and everything else, including funding injections and political or commercial pressures, as coming from "the environment". Setting out the characteristics of the fundamental knowledge-generating activities of science enables us to more clearly see just how the various external influences are likely to affect scientists and their scientific activities. As noted above, the basic PCR processes of science work to affect (both positively and negatively) the reputations and credibility of the scientists involved. Reputation is important to scientists, and a good reputation among peers is highly desirable and conveys credibility to future work. But reputational gains from useful work can be magnified in various ways, and scientists are alert to such possibilities. They can be quite attentive to which areas of research are regarded as "cutting edge" and thus have enhanced visibility, which employment positions convey the most prestige, and where the most lucrative grants are to be had. Based on their appraisals of such information, they can modify their own transaction repertoire, perhaps turning attention to a phenomenon that they see as attractive to investigate due to current cachet, lack of competition, or availability of funding – engaging in new transactions which, from their point of view, represent paths to potentially appropriable reputational gains. In this way, novel local environmental stimuli (not only unexpected observations, but also new sources of funding or opportunities for occupying more prestigious positions) can have systemic effects, altering the existing configuration of scientific interactions and therefore affecting the content and structure of what eventually emerges as scientific knowledge.

The idea that the seeking of reputation is a significant motivating factor for the individual scientist may jar with the more usual characterization of scientists as curious truth-seekers. But close observers of scientific activity, for example, Hull (1988, pp. 281, 305-310), have highlighted reputation-seeking as a prominent aspect of scientific behavior. There is no doubt that it would be a gross oversimplification to portray scientists as being motivated *only* by reputation, but our approach here is analogous to the one economists make in representing firms as profit-seekers. Anyone who has ever been in business knows that the actual motivations of businessmen are much more complex than that, but it is, nonetheless, a suitable abstraction because, whether or not profit is the specific aim, the only firms that survive are those who do, sooner or later, make profits. Similarly with scientists – the only ones who survive *as scientists* are those who, sooner or later, earn some reputation by virtue of their participating in the scientific order.[4]

It is important at the outset to note that the principal institutions of the scientific order are *not* market institutions – they do not generate market prices, and they do not directly produce exchangeable commodities. The fundamental institutions of the market are those of property, contract, and exchange, and these affect the incomes and wealth of the interacting participants; those of science are publication and citation and these affect the reputations and credibility of the participants. Analyzing science as if it were a market obfuscates the operation of the relevant institutions of science and promotes the use of evaluative criteria possibly of use in the theory of market systems but unsuited for the positive analysis of science undertaken here.[5] This does not mean that "the economic way of thinking" is inapplicable to science; rather, that a conventional neoclassical "market failure" analysis is misplaced. Science is still populated with people who, within the context of the institutions and standard procedures of science, are motivated to pursue happiness – to be incented by perceived benefits and disincented by perceived costs.

The PCR processes are characteristic of scientific activity that empirically and conventionally would be identified as "basic science." But does it include applied R&D – in particular, that part of such activity that is more market-oriented carried out in-house in relative secret, and not circulated publicly to the scientific community at large? While PCR may not be the most prominent institutional form in the more secretive instances of in-house applied R&D, research in this mode is ordinarily based, at least indirectly, on science that has passed through PCR and been found useful by other scientists, including those whose principal endeavors are in the applied and development areas.[6] This allows us to think of scientific activity as a range of endeavors from publicly-disseminated basic and applied science to strictly in-house applied R&D, all to varying degrees driven by or reliant on the prior operation of PCR processes.

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4. As noted in Section 4 below and illustrated in our case study in Section 7.1, it is useful to differentiate reputation earned or withdrawn from other scientists and reputation dispensed from outside the scientific order.
5. See McQuade (2007).
6. This is not to suggest that basic science uniquely drives applied science; rather, the spectrum of activity associated with R&D activities is now generally characterized by bidirectional causal linkages within and between segments encompassing the full panorama of science and science-related activities, from pure science to technology and commercial applications.

3. Funding Sources and their Effects

We turn now to examine the particular effects of science funding on science itself. The three broad sources of outside funding are donors, businesses, and government agencies. These have in common that the funding, or its continuation, has strings attached. In addition to (and not exclusive of) the popularly cited motive of wishing to benefit society by facilitating scientific advance, donors may desire the immortality of association with a fundamental advance or a large institution, and businesses may be attuned to the possibility of the indirect profitability of such funding. Government agencies may have their own institutional predilections, but they are funded, ultimately, by legislatures, and legislators may be alert to the benefits that the expenditure of the funds produces in their districts or to the enhancement to their reelection prospects associated with their promotion of a good and popular cause. So, in no case, is external funding solely focused on purely scientific concerns, nor could it ever be adequate to fund every potential scientist's desire for funding. It is nonneutral and, as such, has particular effects on the scientific order which depend on the characteristics of the specific funders themselves and on the characteristics of the constellation of funders. The specific funders have their particular interests and capabilities; the funding environment as a whole may be a concentration of relatively few large funding sources, or a decentralized mix of a large number of smaller sources, or something in between.

Turning our attention to the scientific order itself, there are two basic channels through which funding may affect it: one that functions within an existing PCR setting and one that compromises the PCR procedures. Given an uncompromised PCR environment, the principal effects may be addressed in terms of *directional* and *destabilizing* effects of funding. The effect that funding may have in *distorting* PCR is somewhat more problematic to deal with, but potentially far more important in that it concerns how science functions and hence what counts as science.

Here, then, are the categories of our taxonomic framework, intended to support analysis of how different funding environments affect science:

1. *Direction*. Science funding affects science in terms of the selection and emphasis of R&D projects and research questions.
2. *Destabilization*. Changes in the level and focus of science funding impact the availability of resources integral to scientific work, and so affect the operational and financial stability of both the physical institutions of science and the scientists themselves.

3. *Distortion*. Certain aspects of the funding process may promote a knowledge-generating and certification process not consistent with the evolved mechanisms of science – in particular, the PCR processes – that confer scientific legitimacy at the system level to the knowledge produced by the interactions of scientists.

It is important to note that, whether the funding source is government or private, the presence of directional or destabilizing effects does not imply a systematic bias in the quality of the funded research or its findings. All funding sources necessarily affect the kind of science undertaken and just because, for example, the majority of basic science in the U.S. is government-funded, there is no necessary implication that the resulting directional reorientation of science produces knowledge inferior to what would have been generated had funding been directed in some other way or provided by other sources.[7] Changes in the level and focus of science funding may impose resource constraints on scientific work, but such changes do not necessarily systematically affect the way research findings are certified as knowledge. Given that PCR remains in effect, funding, no matter its source or its variability, will have no effect on the validity of the emergent scientific knowledge.

This is not the case, however, when distortive effects are introduced by changes to or replacement of the PCR processes. Whatever directional and stability effects may arise from science funding, there is now a separate compounding issue if the procedures of the scientific order that legitimate theories and empirical findings are compromised, bypassed, or replaced. It is one thing if a funder redirects scientific inquiry by changing funding from one project to another; it is quite another if the funder is able to enforce a decree that scientific results are to be accepted or rejected based on their compatibility with certain ideological propositions or, more subtly, if a scientist's *political* connections convey *scientific* credibility over and above that earned from scientific research.

Internal departures from PCR certainly occur since, on occasion, scientists succumb to the incentive to chisel, cut corners, or engage in unacceptable practices in search of cheap reputational gains. But the operation of the normal PCR processes serves to constrain such violations and greatly limit within the scientific order their potential long-term effects. Disreputable behavior of individual scientists becomes threatening to the scientific order only if such deviation is systemic or becomes embedded within the conventions and institutions of the scientific community, and such a tendency is much more likely

7. In 2008, for example, of the $69.1 billion that funded basic research in the U.S. in 2008, 57% was Federal money, while 17.7%, 14.7%, and 10.6% originated, respectively, from businesses, universities and colleges, and nonprofit organizations (National Science Board, 2010, pp. 4-15).

to be driven by external, rather than by internal, pressures. To cite a current example, some of the highly publicized instances of "bad behavior" by individual scientists working in climate research – backbiting, slandering, jumping to conclusions, over-hyping pet theories, sloppy statistical work, cherry-picking data series, even attempting to hide data[8] – does not necessarily mean that bad science will be the outcome. In fact, knowing that such behavior is widespread just means that there is more incentive to duplicate claimed results and to be skeptical. As long as PCR is working, there is no long-run *scientific* problem.

From an empirical standpoint, external pressures having the capacity to distort the evolved procedures of science are rarely so explicit or severe that damage to science may be unambiguously attributed to them, although such did occur during the era of Stalinist biology. Instead, distortions of PCR, especially when they are unintended, tend to be subtle and difficult to observe or document. For the observer of science, the juxtaposition of possibly all three effects presents an empirical challenge in identifying and documenting the separate independent effects and their linkages to each other.

Before discussing further these categories, it is worth noting that we should expect significant feedback between those effects of funding that are produced without changing the PCR arrangements and those effects that do involve a change. We have no reason to exempt any funding source from potentially affecting science within an existing PCR environment. And we cannot dismiss the ability of the order itself to adaptively evolve in ways that modify or change its own institutions. But the stakes seem to grow considerably when we consider the effects of centralized funding regimes, for now there is a greater likelihood that funding concentrations (for both principals *and* agents) will exert disproportionate effects on science, including pressures on the criteria by which whose reputations get made and whose do not. That is, the effects on the PCR processes cannot be assumed to be independent of the funding regime. How these effects in all institutional settings play out empirically is an additional consideration that complicates the interpretation of actual events and data.

8. For a relatively calm exposition and discussion of the so-called "climategate" saga, exhibiting the correspondence between climate scientists which was instrumental in provoking the uproar, see *http://en.wikipedia.org/wiki/Climategate_scandal*. Also see the Memorandum submitted to the House of Parliament by the Institute of Physics (U.K.), a professional society of physicists, at *http://www.publications.parliament.uk/pa/cm200910/cmselect/cmsctech/memo/climatedata/uc3902.htm*. The main thrust of the Memorandum reaffirms adherence to standard conduct scientific norms to ensure scientific credibility. We believe that as long as PCR remains intact, whatever abuses or malfeasance an outsider might associate with this episode are matters that the scientific community can competently address.

4. Directional Effects

Directional effects are treated here as outcomes which result from changes in the amount and focus of funding but which do not induce any changes in the PCR processes and which therefore do not result in the production of invalid science. In general, these effects arise as a consequence of the relative size of funding sources, the ways in which beneficiaries are chosen, and the objectives of those controlling the funding. The magnitude of these effects would be expected to increase not only with the degree of centralization of the funding environment but also with the willingness of funders to wield influence and expend resources to achieve their objectives. This latter attribute fits both government funding and funding by very large corporations or business consortia.

If we look, for example, at government funding, which has dominated funding since WW II, a detailed analysis of the directional effects of such funding is not a simple matter. The specific flows of government funding involve a complicated process that includes various players within academia, the private sector, and intra-government funding clients seeking government funds. In some cases, the resulting effects on the direction of science may be an unintended byproduct of the political process itself; in others, they reflect deliberate manipulation of the funding process, the imposition of political preferences, the playing out of political pressures, directing funds to specific projects, and the pursuit of short and long run policy goals. This suggests that directional effects associated with the process of government funding should be analyzed in terms of the institutions comprising the interplay among the relevant players and the incentives that guide their behaviors, questions would take us, however, far beyond the confines of this paper. Happily, such incentives and effects have been covered extensively by Cohen and Noll (1991), Martino (1992), Savage (1999), Kealey (1996), Greenberg (2001), Gough (2003), Hart (2003), Shane (2004), Butos and McQuade (2006), and others.

5. Destabilizing Effects

At the most general level, stability of a social order like science is a long-run dynamic concept that refers to the system's ability to maintain the structural coherence of its components such that its operative procedures and institutions support its functioning in generating its emergent outcomes through time.[9] This is dependent on the system's capacity for adapting, quite likely by modifying its own internal structure, to a changing environment. For science, long-run stability takes on special meaning in that it applies to science understood as a knowledge-

9. See McQuade (2007) and McQuade and Butos (2005).

generating order, and carries the implication that a well-functioning scientific order must be error-correcting in the long run. The spectacular success of science to date in this regard attests to the long-run stability with which its internal procedures have endowed it. So, while we cannot *a priori* rule out that science might evolve in ways which impair its adaptivity, say perhaps by reconfiguring or replacing its current PCR procedures by other institutions to produce something more akin to a bureaucracy, the more relevant cases for the discussion here are those driven by changes imposed from outside the scientific order itself. And, to the extent that such external pressures result in the undermining or abandonment of PCR, they are best discussed under the rubric of "distortion".

The main focus of our discussion of destabilizing effects, then, concerns the analysis of the system as it responds in the *short run* to exogenous pressures.[10] We will assume that the existing procedures and institutions of science are not impaired or seriously compromised by exogenous pressures, suggesting that scientific activity would still function along lines basically consistent with science as an adaptive system. We also assume the system's capacity for endogenously-generated change is not a significant short-run factor.

From the point of view of scientists and the universities, firms, and laboratories that house them, the pattern of funding is critically important. Scientists must associate success or failure with their ability to secure resources. Since a large proportion of basic science is performed by university-based researchers, scientists' professional careers and their incomes are connected to their securing grants. Indeed, for untenured researchers, their university posts in part hinge on securing grants. If the pattern of funding support changes, we expect scientists and universities to either reap benefits or bear costs as a consequence of the changes in the direction and magnitudes of funding. The full ramifications of such funding changes may well be subtle and empirically shadowy. Yet, such funding changes may empirically appear as changes in the allocation and scale of science inputs across specific areas of research, including capital expenses, employment of scientific personnel, the number of degrees awarded and areas of specialization of graduate students, as well as a connection between success in securing research grants and subsequent publications and citations.

In a funding regime of a small number of large funding sources, the shifting priorities of the funding institutions may result in a phenomenon of

10. It is quite unhelpful in this context to carry over the habits of thought long employed by economists concerning the meaning of short- versus long-run effects, especially when the long run in an economic context generally involves the attainment of or the tendency toward some steady state of the system or to an equilibrium or sustainable configuration of endogenous variables. The salient shortcomings of such a system description are the absence of emergent characteristics and the irrelevance of the system's adaptive capacities.

"boom and bust" within scientific subdisciplines. The boom is initiated as generous funding policies make funding more generally available and easier to obtain, and as "popularity" (for both real and political reasons) of a particular subdiscipline makes funding for that subdiscipline easier to get. This generates a herding effect as the increased activity in the subdiscipline is observed by both researchers and funders, the result of which is that research projects that under earlier funding conditions would not have been attempted are now able to be funded. Individual researchers cannot know whether the boom is fueled by real or artificial factors or some combination of these, and even if they did deduce an artificial component they could not reliably tell when its influence will end. Many researchers will follow the incentives of the science feedback loops, evaluated in the light of what they know about the current conditions of the subdiscipline and its funding, and only very few will have the reputational stability to avoid participating in the available possibilities for reputational enhancement in the short term.

The bust will come when it becomes apparent that many of the research projects in the subdiscipline enjoying the boom cannot be completed, either because, due to a shift of funding priorities, the funding for the projects is scaled back or terminated before the projects are completed, or because reality manifests itself in the form of lack of experimental validation, which makes continued participation more problematic. Failures are more likely in the riskier projects, which will tend to be those requiring capital investment a long time before useful results can be anticipated, or those which were funded with inadequate appraisal of the researcher's capability, the feasibility of his approach, or the likely usefulness to other researchers of his potential findings.

To the extent that failures in a heavily funded subdiscipline damage the credibility of the organizations funding the failed projects, the subsequent withdrawal of funding due to these organizations' reduced ability to generate new funds can induce curtailments in areas outside that affected subdiscipline. Failures and curtailments of science projects result in increased unemployment and resource idleness, since movement of people and resources to other projects and subdisciplines is a difficult and time-consuming process.

There is no shortage of candidate episodes for illustrating of science boom and bust. Two particularly prominent U.S. examples are:

- The rush to fund basic physics, mathematics, astronautics, and space science in the wake of the Sputnik launch in 1957,[11] culminating in the moon landings, after which both interest and funding fell off markedly and NASA's budget fell from $26 billion in 1968 to $11.5 billion by 1974[12] (in constant 2007 dollars).
- The renewed attention paid to computer science (particularly artificial intelligence) prompted by the perceived threat of the Japanese "Fifth Generation" project begun in 1982, going as far as the establishment in the U.S. of a major research consortium pooling the resources of several major corporations together with a major initiative by the Department of Defense to develop intelligent systems. With the failure of the Japanese project and disappointing results from artificial intelligence research, the wave of funding subsided considerably. Between 1984 and 1988, total federal funding for AI research rose from $106 million to $274 million.[13] However, DOD's Strategic Computing Program, slated to spend $1 billion over 10 years on the nurturing of "strong AI", had to be discontinued as commercial developments in computer workstations rendered its LISP-based technology obsolete. According to McCorduck (2004, pp. 426-429), the funding for AI software was cut "deeply and brutally" as attention shifted to other areas of research.

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11. The "Sputnik Crisis" led directly to the creation of NASA and the quadrupling of funding for NSF in 1958. Total federal R&D in aerospace rose from $934 million in 1950 to $2.4 billion in 1960 – see Gilbert (1981). And, in addition to the influx of funds designed to immediately enhance space science capability, an initiative was launched to provide for more scientists in the future. As documented by Dow (1991), "the launching of Sputnik released a torrent of federal funds. In 1958 the NSF increased its support for curriculum development at a rapid pace. … By 1960 the programs of the Education Directorate represented 42% of the NSF annual budget." Also, "the National Defense Education Act, the most far-reaching federally-sponsored education initiative in the nation's history … authorized expenditures of more than $1 billion for a wide range of reforms including new school construction, fellowships and loans to encourage promising students to seek higher education, new efforts in vocational education to meet critical manpower shortages in the defense industry, and a host of other programs."

12. Table 4.1 "Outlays by Agency" at *http://www.whitehouse.gov/omb/budget/Historicals*. Large swings within categories of NASA's R&D expenditures are often significant. See Table II-12 at *http://www.aaas.org/spp/rd/rdreport2010/tblii12.pdf*. For example, from fiscal year 2008-2009, "Space and Flight Support" increased 64%, "Astrophysics" decreased by 13%, "Space Exploration" increased 23%. These shifts reflect, in part, adjustments in Administration priorities, as evidenced, for example, by President Bush's support for the Constellation Systems (which comprised in 2009 $3.5 billion of "Space Exploration" or 27% NASA's budget), a set of programs whose funding was terminated in President Obama's 2010 NASA Budget.

13. See Committee on Innovations in Computing and Communications & National Research Council (1999, pp. 213-216), at *http://www.nap.edu/openbook.php?record_id=6323&page=213.*

The current situation in climate science has all of the earmarks of a science boom. There has been a massive influx of funding for both basic science and technological R&D directed specifically to climate research.[14] In addition to the reasonable concern to understand and document any and all factors contributing to global warming, there are strong political pressures at play. The developments in climate science would be nothing more than a normal-enough ongoing scientific dispute were it not for the insertion of political agendas external to the science itself. Public choice considerations point to the likelihood that it would be to the advantage of most politicians and many corporations all over the world if there were a significant human component to global warming – vast opportunities for regulation, taxation, control, and the farming of rents in the developed world, and equally vast opportunities for attracting and extracting aid money in the less developed world. Hence the incentives for expansionary funding are in concert with actual funding growth, giving credibility to the hypothesis that this is, indeed, a funding bubble.[15] If and when this bubble bursts, there will be a significant downsizing of climate science research institutions and a concomitant reduction of job opportunities for climate scientists.

6. Distorting Effects

Distorting effects are those effects which work to impair or circumvent those evolved institutions fundamental to science's functioning as an adaptive classifying system. When the procedures and feedback loops crucial for the long run viability and robustness of science are bypassed or impaired, the functioning of the scientific order becomes maladaptive to science's normal environment and the so-called knowledge generated in these conditions is tainted, if not totally invalid – in any case it has not emerged from the free operation of the procedures that are an inseparable aspect of scientific knowledge generation. In short, distortions in the generation of scientific knowledge are brought about by subverting or bypassing PCR. An obvious example was the tragic fate of the

14. In 1989, the first specific U.S. climate-related research funding agency was created with an annual budget of $134 million (in 1989 dollars). In 2008, the total funding by U.S. government agencies was $5,781 million (in 2008 dollars). The requested funding total for 2009 was $6,841 million. For the sources of these numbers, see Nova (2009). Note that we cite this article for its accumulation of the relevant funding statistics from government publications, not for its analysis of them.

15. Although this expansive funding has been directed overwhelmingly toward scientists who seem to be producing results that purport to show man-made enhancement of a general warming trend, this does not in itself imply that results are being faked or that scientific integrity is being compromised. To the extent that large sources of funding directed toward particular extra-scientific ends are a problem for the credibility of the scientific knowledge produced, it is addressed below under "distortion."

biological sciences under Stalin, when PCR was essentially replaced by brutally enforced Stalinist ideological criteria. However, one does not need to invoke such extreme political intervention to populate this category – distortions can arise from pressures coming from both inside and outside the science community.

Fraud and misconduct in science,[16] the deliberate attempt by scientists to plagiarize or to manipulate or falsify research data and results, is a distortion which may affect which articles are published and cited and hence affect the reputational status of researchers. The requirement for adherence to PCR is an effective constraint on achieving success (recognition) in science; hence, we must acknowledge that incentives exist for scientists and funders to circumvent, subvert, or eliminate that mechanism. To the extent that instances of fraud and misconduct remain hidden, other researchers may use such fabricated results in their own research, potentially inadvertently compounding the initial fraudulent research. As a result, the *wrong* science – wrong in the sense of violating scientific norms, not wrong in the sense of simply being in error – gets legitimated. Even if dishonest researchers are never unmasked, however, we would expect that eventually the work of other researchers will supersede the original fraudulent results. Because *some* fraud in science is likely always to be with us, the relevant issue becomes the speed at which non-fraudulent results weed out fraudulent ones or allegations of fraud can be assessed by scientists themselves. But as long as there is competition (or the threat of it) in the relevant scientific arena itself, there will be not only a strong deterrent to fraud but also an established procedure for unmasking it. Scientists and funders, of course, have an incentive also to uncover fraud, if only to protect their reputational capital; and, significantly, various sorts of arrangements have emerged from within science itself that work to maintain the integrity of the scientific order, such as review boards and university committees that investigate charges of plagiarism, fraud, and chiseling.[17]

It is worth emphasizing that we do not conflate *error* in science with *distortions* of PCR. Because knowledge is an emergent outcome of science, its generation will typically involve displacing existing knowledge by providing a more complete understanding of reality or identifying accepted knowledge as having been mistaken. The replacement of existing knowledge by new knowledge is precisely what we would expect the scientific order to accomplish. Even a modest assumption of fallibility implies that scientific error cannot be

16. See Wible (1992).

17. The Federal government requires that institutional review boards adjudicate allegations of scientific impropriety for all publicly funded research and research involving human subjects. Oversight of these provisions is conducted by the Office of Research Integrity of the Department of Health and Human Services. See *http://ori.dhhs.gov*. For a critique of these mandated review boards, see Hyman (2007).

ruled out, and hence so-called new knowledge is not necessarily better knowledge. Yet, we do expect that, over some relevant time horizon, manifest errors can be eliminated. If that is to happen, however, our argument is that PCR must function on an ongoing and unrestricted basis if the knowledge-generating process of the scientific order is to be maintained. If it does not, then we cannot rule out that errors, which under functioning PCR processes would tend to be weeded out, will survive as knowledge distortions with little prospect for their systematic elimination.

Our main concern, however, is the potential for distortion that derives from pressures external to science – in particular, those inherent in the characteristics of the funders and the degree of decentralization of the funding environment. In an environment composed of many funders, the effectiveness of any single one to interfere with the PCR processes and their operation is likely to be small. They lack the ability to impose their will on a significant segment of the scientific order. Thus, while there may be grounds for claiming that, no matter how dispersed the funding environment, science cannot fully insulate itself from interferences with PCR, there is little reason to think that such impairments are likely to be systematic or injurious to the overall functioning of the scientific order.

But that is less likely to be true for more centralized funding arrangements. With funding lodged in a small number of related sources, actions by the funder (intentional or otherwise) that have the effect of compromising the PCR processes have the capacity to affect a large number of scientists and their activities. One channel through which unintentional distortion could occur in such an environment would be via an alteration of the incentives facing scientists. The ability to attract funding from a large and reliable source of funds confers prestige and administrative power to scientists who are successful in doing so. For these scientists, professional reputations now reflect not only their scientific accomplishments but also their ability to raise and dispense research funds. Given the tight connection between reputation on the one hand and influence over scientific research on the other, success in securing and controlling funding resources may itself exert a distorting effect on science. Reputation derived from funding success can substitute for or augment reputation derived from the production of useful scientific work, so that future scientific work is reviewed with less scrutiny and perhaps more readily accepted than it would have been were reputation based purely on scientific accomplishment. Also, in order to attract necessary funding, scientists may feel the incentive to produce results or shade conclusions in a way that they see as being in sync with, or furthering the aims of, one or more of the funding organizations. Again, this incentive will be present in any funding regime, but is much more likely to have systemic effects when the funding sources are concentrated.

On the face of it, one would not expect that a funding regime in which the majority of funding was provided with public funds would necessarily exhibit the characteristic of high concentration, because such funding is administered through a variety of funding agencies whose concentrations are in different fields and whose funding imperatives are independent of each other. But, to the extent that political considerations external to science play a part in funding decisions, there is the possibility that such government agencies will, motivated by the same political pressure, effectively act in concert – in other words, not only take on the appearance of a highly centralized funding arrangement but effectively act as one. In any case, from the point of view of a scientist in a particular field, the funding for that field may be dominated by a single agency. And, in the final analysis, with government funding, the ultimate source of funding and the interests of the ultimate funder are not dispersed, but concentrated in a single, if often fractious, entity, the legislature.

Concentration of the funding environment is not the only characteristic of public funding with distortion potential. Government funding of science comes equipped with political or even constitutional prerogatives for overseeing science not available to private funders, and these are ordinarily justified as providing the oversight and accountability taxpayers sometimes expect from government. The use of regulatory vehicles (with access to an arsenal of resources – staff, lawyers, and raw political muscle), such as Congressional hearings and access to media outlets, can, whether intentionally or not, exert an influence scientists' *scientific* reputations for good or ill, and can lead to the circumvention of standard evaluative procedures and criteria used to review and ascertain the publication worthiness of scientific work. There is a clear sense, then, in which the simple expedient of government funding science may generate incompatibilities with the institutions of science.

To summarize: there are serious implications for science as an adaptive classifying system if the PCR processes and their constraints and feedback loops are seriously weakened. Scientific findings will be less reliable. The corruption of PCR would promote the spread and installation of ideas, findings, etc. that have not gone through the standard procedures of critical evaluation or acceptance by other scientists of contributions they find useful. Such effects are considerably more likely in an environment in which funding sources are concentrated (as is the case when government assumes a major role) compared to when they are decentralized. Further, when funding is accompanied by regulatory oversight, the possibilities for distortion are significantly increased.

7. Case Studies

To show that our taxonomy of funding effects has relevance in the real world of science funding, we wish to exhibit specific instances of such effects. To that end, we place our discussion in the context of the current regime of science funding in the U.S., dominated by a small number of very large, government-affiliated agencies.[18] We give examples of effects actually occurring under this funding regime in two different contexts – first, in the David Baltimore case of "scientific misconduct", and second, in the circumstances surrounding the appearance of the federal government's dietary guidelines. Note that, in both cases, we take no position on the scientific merits of the issues discussed – our concentration is exclusively on the directional, destabilizing, or distorting, effects engendered by the funding environment of the research in question.

We have selected case studies on government funding for two reasons. First, government is the largest single source of basic research monies in the U.S., thereby qualifying as a funding "Big Player".[19] Second, the legislative and executive branches of the government have Constitutional prerogatives and responsibilities that enable and even require regulatory oversight and rule-making authority possibly affecting the PCR processes of science and which are unavailable to other funding sources. Consequently, the panorama of effects we have discussed in this paper are more likely to be brought into relief by using these particular case studies.

7.1. The Baltimore Case

David Baltimore, a high-profile 1975 Nobel Prize recipient in Medicine, collaborated with T. Imanishi-Kari (the principal author) and four other researchers on an immunology paper published in *Cell*. A post-doc lab assistant in Imanishi-Kari's lab disputed some of the published findings. Allegations of misconduct by Baltimore and Imanishi-Kari led to internal reviews of the paper by committees convened at Tufts University and MIT. The committees supported the paper, holding that any errors would be addressed by the normal processes by which science works.

The whole matter was rapidly deflating until two National Institutes of Health (NIH) scientists at the Department of Health and Human Services (DHHS), Ned Feder and Walter Stewart, resurrected it as part of their ongoing, unauthorized, and self-appointed crusade to ferret out unethical behavior of

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18. Other countries may have more or less concentrated regimes of science funding, but such comparisons are not our focus. Funding in the U.S. is sufficiently concentrated to be taken as a source of examples of the sorts of concentration effects we describe.

19. See Koppl and Yeager (1996).

government-funded scientists.[20] But the key escalation occurred when Feder and Stewart brought the case to the attention of Representative John Dingell of the House Energy and Commerce Committee. The Congressional investigations and hearings that followed under Dingell in the Subcommittee on Oversight and Investigations were legitimated by the oversight responsibility of Congress regarding the use of Federal funds. Baltimore and Imanishi-Kari soon found themselves targets of the full array of government assets – the NIH Fraud Unit, the Office of Scientific Integrity (OSI) of the DHHS, U.S. Treasury document examiners, U.S. Secret Service forensic specialists, and Subcommittee investigators and lawyers.

There are two aspects of the Baltimore Case that illustrate the connection between distortion effects in science and the funding source (here, the Federal government). One concerns the reputational effects on Baltimore and especially Imanishi-Kari arising from the Subcommittee's "show trial" use of "innuendo and rumors" to "harass, embarrass, and vilify" them (Kevles, 1998, p. 185). The tactics of Dingell and others severely impacted the professional careers of Baltimore (who, although never formally charged with misconduct, was pressured to resign as President of Rockefeller University) and Imanishi-Kari (who was found guilty of misconduct by the OSI, losing all NIH funding for ten years only to be cleared of all charges by a HHS appeals panel in 1996). Eventually, Imanishi-Kari got her position back with tenure at Tufts; Baltimore is now a chaired professor at Caltech.

Once allegations of scientific misconduct had been made against Baltimore and Imanishi-Kari, the government had reason to exercise its oversight function for ensuring that taxpayer funds were properly used. Thus, it is not implausible to suggest that, once the case reached the hardball court of the Subcommittee, the accused, whether guilty or not, had implicitly exposed themselves to scrutiny (fair or not) by virtue of their acceptance of federal monies. But this does not fully address the effect on science of the sorts of tactics, backed by the weight of an intimidating array of government resources, which can be brought to bear on scientists testifying before Congressional committees. The reputations of Baltimore and Imanishi-Kari *as scientists* were severely compromised, not as a result of the PCR processes acting on their work, but because of their very public exposure to the adversarial investigation.

20. Culliton (1988b) notes that Feder and Stewart were employed as researchers at NIH. They received NIH approval to spend up to 20% of their time policing published scientific papers, endeavors that would actually absorb nearly 100% of their paid time. Stewart, Culliton emphasizes, "maintains that error, once known, should be explicitly corrected" by the reviewer, not a scientific board of inquiry, examining "the published literature for error" (p. 241). Also see Steele (1999).

But an even more serious distortion of PCR in the Baltimore Case came about because the Subcommittee's oversight prerogative was exercised in ways that moved beyond an investigation into misconduct and into the area of actually "auditing … scientific disputes" (Culliton 1988a, p. 1721). The paper that thrust Baltimore and Imanishi-Kari into the national limelight had survived the journal's editor, peer reviewers, and two university committees of inquiry; nonetheless, determining whether it constituted *good* science emerged as relevant to Dingell's case because he equated scientific error with fraud. As biologist Robert Pollack's op-ed in the *New York Times* noted, Dingell believed that "published science must be free of error, and that error itself indicates bad faith and fraudulent intent" (quoted in Kelves, p. 185). The Committee's members and investigators set their sights on determining the quality of the science contained in the paper and in the laboratory practices used by Imanishi-Kari, as they really had to do if the charges of misconduct and data fabrication were to be sustained. In effect, it is not clear how the investigation of alleged scientific misconduct can be isolated and objectively analyzed without also making judgments about the scientific methods, choices, and interpretations inhering in scientific work.

In the schema presented in this paper, the creation of a government agency to adjudicate scientific disputes would be seen as an imposed change in the "publication" component of PCR, though having implications for the citation and reputation components as well. Given the pervasiveness of government funding of science, this cannot help but be seen as a potentially significant distorting element in the scientific order. Government funding of science is not separable from government regulation of science; the Baltimore Case reveals that the good (and not so good) intentions to uncover fraudulent scientists poses a strong risk to science of also inducing distorting elements in the scientific order.

7.2. Government Pyramids: Diet and Health

The relationship between health and diet has long been of interest to scientists, medical clinicians, policy makers, and the general public. Up until the early 1970s, medical researchers, dietary clinicians, a large body of empirical work, and long-standing reputable weight reduction programs stressed the connection between obesity and carbohydrates. Within a decade, however, a sea change within the medical establishment had occurred that recommended low-fat diets to reduce the risk of heart disease and for treating weight-control problems. As Taubes (2007, p. xv) notes: "This was one of the more remarkable conceptual shifts in the history of public health."

Our interest here is not, of course, to render a judgment about the merits of the science surrounding this shift. Clearly, doing so requires the kind of knowledge that only science can provide and it is this that finally is the essence of the question. Science will typically entail a spectrum of theories, data sets, and clinical approaches open for debate and critical assessment, and it is as a consequence of that process that we would expect to generate better understanding of the questions at hand. But scientific questions are not always analyzed through the standard filters of scientific discourse, and a closer look at this most interesting episode reveals that the emergence of what is now the consensus stance on diet and health involved a cast of players that included, perhaps decisively so, the U.S. Congress and government agencies. It is an illustration of how science may be affected when political bodies and regulatory authorities insinuate themselves into science in order to make policy.

This saga of the scientific understanding of the relation between diet and heart disease highlights the overall impact of events that singularly seemed of minor significance but which together had the effect of introducing the political process and policy initiatives into an already difficult scientific question. In particular, this episode illustrates the interplay arising from a directional shift in government heart disease funding in 1948, the role of cardiologist Dr. Ancel Keys in the American Heart Association (AHA),[21] the subsequent adoption by a Congressional committee and other government bodies of Keys' position on the correlation between dietary fat and heart disease, and the subsequent and ongoing developments in public policy toward diet and health.[22] The overall point we wish to make here is that the marriage of science and the political process illustrates a *distorting* effect of government on science.

In 1957, the AHA claimed that the evidence did not support the correlation between dietary fat and heart disease. But three years later, and without any new evidence, the AHA published a report that completely reversed its prior position. This report, issued by an ad hoc AHA committee, was written by Dr. Keys and a close ally. Leading up to this report and during the ensuing years, Keys campaigned tirelessly for his position. Shortly after the report's release, *Time* magazine brought Keys into the national spotlight by featuring him on its January 13, 1961 cover and related cover story. Despite the growing

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21. The AHA was established in 1924 as a private charitable organization of doctors and is not supported by government funds. In 2007 the AHA had revenues of nearly $800 million and expenses of $690 million, of which nearly 22% was used to support research and nearly 40% for public health education. Among its corporate sponsors, donations by 34 pharmaceutical and device manufacturing firms account for about 6% of its income. See American Heart Association (2007).

22. In one of many similar instances, in July 2008 the New York City Council implemented its ban on the use of non-naturally occurring trans fats in commercial food establishments. Earlier that June, the *New York Times* reported that other cities were studying New York's approach with an eye to installing similar bans in their own locales, with probably the most significant instance being California's adoption on July 26, 2007 of a state-wide ban on the use of trans fats by restaurants. See Hartocollis (2008) and Steinhauer (2008).

popular acceptance of Keys' and the AHA's increasingly trenchant low-fat dietary recommendations, the underlying science still remained subject to further research and would have been expected to produce over time further insights in the complex of questions pertaining to diet and health. Nonetheless, a tipping point was reached in 1977 when "the controversy was shifted irrevocably in favor of Keys' hypothesis" (Taubes 2007, p. 44). The occasion of this turning point was the publication of "Dietary Goals of the United States" by George McGovern's Senate Select Committee on Nutrition and Human Needs.[23] The report fully reflected Keys' views, arguing that various dietary guidelines as part of a "plan for the nation" be accepted.[24] It is interesting to note that important segments of the scientific community testifying before the Committee, including the AMA and the National Heart, Lung, and Blood Institute, disputed not only the Committee's dietary guidelines and the research used to arrive at those guidelines, but also the evidence regarding the underlying premise with respect to heart disease and fat.[25]

Although the McGovern report had turned a scientific question into a political one, it received further support when other government agencies joined in. In March of 1977, the U.S. Department of Agriculture (USDA), under Carter appointee and consumer advocate Carol Foreman, decided to get involved in the McGovern Committee's controversial report. The job of drafting the dietary guidelines fell to the USDA and its successful solicitation of the Surgeon General's Office.[26] When the Food and Nutrition Board of the National Academy of Sciences released its own version of dietary "guidelines" in *Toward Healthful Diets* it aligned itself in opposition to the McGovern Committee, the USDA, and AHA by suggesting that weight control was the only sort of reliable and safe dietary advice that could be scientifically recommended. This brought forth a

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23. The motives for the Committee's work in this area have been the subject of speculation; its work on dietary issues was not closely connected to its original 1968 charge to address malnutrition. Taubes (2007, p. 45) suggests that the real driving force was "the committee staff, composed of lawyers and ex-journalists." He quotes staff director Marshall Matz: "we were totally naïve" and believed we "should do something on this subject before we go out of business." However, based on William Broad's 1979 article in *Science*, Taubes notes that the McGovern Committee believed it was about to be downgraded to a subcommittee (i.e., effectively put out of existence) and that the Dietary Goals was "a last-ditch effort to save the Committee" (p. 45).

24. These guidelines referred to the recommended dietary percentages of carbohydrate calories (55-60%) and fat calories (30%). See U.S. Senate Select Committee on Nutrition and Human Needs (1977).

25. Taubes (2007, p. 47) notes that reputable scientists were sandwiched between speakers representing the dairy, cattle, and egg industries, thereby tainting the testimony of scientists.

26. The USDA failed to solicit the assistance of other government agencies in generating the dietary guidelines: first, the National Academy of Sciences, whose President, Philip Handler, told Foreman that *Dietary Goals* was "nonsense" (as quoted in Taubes, p. 49). In turn, entreaties to the National Institutes of Health and the Food and Drug Administration were rebuffed.

firestorm of criticism from its aforementioned adversaries, and also from Henry Waxman's House Subcommittee on Health, and from the press, which impugned the professional ethics of Board members as being in "the pocket of the industries being hurt" (Jane Brody, *New York Times*, as quoted in Taubes 2007, p. 50). In this political battle, the McGovern guidelines became the de facto position of the government.

Needless to say, what emerged from this episode was a quasi-official government stance and popularly accepted belief that legitimated the basic contention of a controversial scientific and clinical take on dietary issues. By the early 1980's, the policy debate effectively ended, despite the fact that a scientific consensus proved elusive. When the NIH simply announced that such a consensus did exist, "the controversy over dietary fat appeared to be over" (Taubes, p. 60).

This brief overview suggests that the McGovern Committee's insinuation into a controversial question of science turned that question into a political issue that was resolved by political means and not scientific ones. With the eventual marshaling of government resources and agencies, it is easy to recognize that the government was able to override and dominate all comers opposed to it and, as a result, able to impose its view on the public and "conventional wisdom." This episode reveals the potency of the government when it chooses to act as a Big Player.

But what does this episode have to say about the effect on the scientific order and its PCR processes? As noted above, government funding in medical research took a directional change in 1948 when Congress passed the National Heart & Lung Institute National Heart Act creating the National Heart Institute and the National Heart Council, ostensibly to increase public awareness of heart disease. Funding for heart research passed through the Institute; within a year it had allocated $9 million, by 1960 $54 million, and $2.5 billion in 2008.[27] The magnitude of these amounts gives credence to the assertion that government was the dominant funder for heart research during the period this episode covers. Of particular interest is the effect on the disbursement of these monies once the government's political process established its basic posture. Taubes' (2007) discussion of conflicts between science and industry is worth quoting at length:

> Scientists were believed to be free of conflicts if their only source of funding was a federal agency, but all nutritionists knew if their research failed to support the government position on a particular subject, the funding would go instead to someone whose research did. 'To be a dissenter was to be unfunded because the peer-review system rewards conformity and excluded criticism,' George Mann has written in *The New England Journal of Medicine* in 1977 … David

27. The National Heart Institute was redesignated as the National Heart & Lung Institute in 1969 and as the National Heart, Lung, and Blood Institute in 1976.

> Kritchevsky, a member of the Food and Nutrition Board when it released *Toward Healthful Diets*, put it this way: 'The U.S. government is as big of a pusher as industry. If you say what the government says, then it's okay. If you say something that isn't what the government says, or that be parallel to what industry says, that makes you suspect' (p. 52).

The admissions in the quote above actually go further than simply highlighting questions about the objectivity of industry versus government funded research, as important as that might be. They also speak to the effect of government funding on PCR by virtue of its providing an implicit entitlement to those whose scientific reputations are enhanced because they agree with "correct" science as deemed by the government. If funding success helps to engender further funding, reputation-seeking scientists have an incentive to align research questions, if only marginally, to those of the funder, while providing additional incentive to those whose views are consistent with those of the funder to seek such funding.

In addition, the reputational costs of dissent can be significant. As the quote above suggests, dissenters have an incentive to seek funding from non-government sources in order to carry out their research. But what if privately supported research is routinely vilified, thereby inducing a reduction in dissent as scientists adapt to the prospect of incurring reputational costs? Such costs are not generated because the science is sloppy or incompetent; rather, they arise as a result of who the funder is. This constitutes a mechanism whereby reputation is conferred on scientists by criteria distinct from criteria irrelevant to science; it is "science" by other means. This is illustrated by the reaction to critics of the McGovern Committee's *Dietary Guidelines*, several of whom served on the Food and Nutrition Board, whose criticisms were dismissed because they had received industry funding support earlier in their careers.[28]

Funders are able to nudge research in desired directions while attracting self-selected funding-seekers. And the larger the share of funds provided by a single funder, the more significant is the likely effect of that funder on the kind of work that gets published and hence on the reputation status of researchers. The political dimensions to science funding suggest, as a consequence, that government funding enjoys the possibility of imposing on PCR an additional and distorting element to science as a consequence of its Big Player status.

28. Taubes (2007, ch. 3) provides a detailed treatment of this episode and rightly points out the obvious inconsistency of the underlying "guilt by association" premise, which typically is invoked because the accuser disagrees with the accused. Despite their logical incoherence, such arguments are often seen in the political and policy arenas.

8. Conclusion

Funding of science is not, and cannot be, neutral with respect to its effects on the activity of scientists and the knowledge this activity generates. The question arises, then, as to what sorts of effects can be anticipated from different regimes of funding. In order to clarify the issues involved, we have introduced a taxonomy of possible effects – a taxonomy which has its basis in a model of the scientific enterprise as an adaptive social system.

We have identified three distinct types of effect: directional effects on the choices of research activity, destabilizing effects (both long-run and short-run) on physical arrangements and resources supporting science and on the employment of scientists themselves, and distorting effects on the procedures scientists use to generate and validate scientific knowledge. While some or all of these effects are present under all systems of funding, they are likely to be much more noticeable when the funding regime is dominated by a small number of very large funders, and the most serious of them – distortion – is a distinct possibility when the funding agencies exercise regulatory oversight. The current system of funding in the U.S., characterized by the involvement of several large and influential government-sponsored agencies, is certainly an environment conducive to generating all of these effects, and we have provided case studies for illustration.

While a full discussion of current science funding would require due attention to the political agendas, mechanisms, and institutional arrangements in play, we hope to have shown that studying factors bearing upon the direction, destabilization, and distortion of science provide a useful set of analytical categories. The general story that emerges from such an analysis is that concentration of funding sources is not conducive to the stability of the scientific order, and that a regime dominated by government funding and the sheer magnitude of its funding leverage may be particularly prone to generating unanticipated effects that decision makers themselves find undesirable or which may threaten science itself as a knowledge-generating order.

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