

The Human Genome Project in Retrospect

Michael Abramowicz

Associate Professor of Law
George Mason University
Arlington, Virginia 22201

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I. INTRODUCTION

In his will dedicating funds to the prizes that would bear his name, Alfred Nobel specified that the prizes would reward the best research conducted in the preceding year.¹ The absence of delay may seem odd to those familiar with the modern operation of the Nobel Prizes,² but Nobel may have hoped that the prospect of an immediate prize would help stimulate investments in research. Even if that is so, though, why would Nobel seek to offer rewards at the end of each year instead of providing funding at the beginning of a year? Modern institutions funding scientists, after all, generally fund those with the strongest proposals for future research, not those whose work in the preceding year was the strongest. The answer may be that Nobel was not thinking about maximizing research, or he was recognizing that the Prizes' prestige would dwarf their dollar value. Regardless of Nobel's thoughts, however, I wish to use the topic of this symposium, the Human Genome Project, to advance and consider a proposal for the funding of science that is a variant of Nobel's approach. In particular, I will propose scientific grants that depend primarily or even exclusively on researchers' performance in the period immediately prior to awarding the grants.

Some have argued that scientific funding should have more of a retrospective element.³ After all, grant institutions do consider the reputation of scientists to some degree.⁴ To show how the approach that I consider would differ from proposals focusing on reputation, let us consider the design of this symposium. F. Scott Kieff and Charles McManis have done an admirable job of assembling a distinguished group of lawyers and scientists to discuss the Human Genome Project, and I am honored and delighted to be included. Presumably, the selections were made largely on the basis of the participants' reputation to the selectors, and we have all been wooed with honoraria, good company, and a swank hotel. A different approach might have been to issue a call for paper proposals and to select the best among them. But the approach that I am considering would be different from both of these, and it would work only if the symposium were a repeated game. Under this approach, the honorarium that I would receive for the next symposium would depend on my performance at this

¹ See Code of Statutes of the Nobel Foundation § 1 (June 29, 1900), reprinted in Elisabeth Crawford, *THE BEGINNINGS OF THE NOBEL INSTITUTION: THE SCIENCE PRIZES, 1901-1915*, at 221 (1984).

² The statutes implementing the Nobel Foundation interpreted Nobel's stipulation so liberally as to make it a nullity. *Id.* at 222-23.

³ See, e.g., Rustum Roy, *Peer Reviewed Productivity-Based Formula for Funding University Research*, 22 *MINERVA* 316 (1984) (offering a formula that would be used to determine funding of university departments, based on factors such as past publications).

⁴ For example, the National Science Foundation has long considered researcher competence as one of many factors in grant proposals. See, e.g., NATIONAL SCIENCE FOUNDATION, *GUIDE TO PROGRAMS FY 1991*, at ix (1991).

one, and my performance at that one would determine my funding for the one after that. The symposium, of course, is not a repeated game, but scientific research often is.

Perhaps the most obvious benefit of a retrospective grant institution is that it would save transactions costs; no proposals need to be prepared, and only those who have performed research will need to explain the significance of their accomplishments. While addressing this briefly, I will focus on a different point, that the proposal will increase innovation in scientific research. There are two foundations in the literature for my quest to identify an institution that can accomplish this task. First, the ongoing debate on the appropriateness of patenting the human genome,⁵ which is prominent in many of the discussions at this symposium, emphasizes the tension between the incentive benefits of intellectual property regimes and the provision of research for the public domain. Second, one legal scholar, Thomas McGarity, has offered a detailed administrative law assessment of agencies that award grants through peer review.⁶ The most significant problem that McGarity finds with peer review is that it often closes off funding to mavericks who wish to challenge accepted orthodoxies, thus limiting the possibility of significant scientific advances.⁷ The retrospective grant institution that I describe is a cross between a patent regime and a traditional grant regime, involving centralized provision of funds while providing incentives for original and risky approaches.

Perhaps the strongest rhetorical attack against my argument would be to note the danger of a slippery slope. If it is advisable to provide grants on the basis of past performance, why not switch all the way to a reward system?⁸ Indeed, my support for retrospective grants is based in part on the observation that grant money can be used as a way to reward private organizations for taking financial risks. Perhaps the strongest positive explanation for the absence of

⁵ Significant contributions to this literature include, Rebecca S. Eisenberg, *Patents and the Progress of Science: Exclusive Rights and Experimental Use*, 56 U. CHI. L. REV. 1017 (1989); F. Scott Kieff, *Facilitating Scientific Research: Intellectual Property Rights and the Norms of Science—A Response to Rai & Eisenberg*, 95 NW. U. L. REV. 691 (2001); Michael A. Heller & Rebecca S. Eisenberg, *Can Patents Deter Innovation? The Anticommons in Biomedical Research*, 280 SCIENCE 698 (1998); and Arti Kaur Rai, *Regulating Scientific Research: Intellectual Property Rights and the Norms of Science*, 94 NW. U. L. REV. 77 (1999).

⁶ Thomas O. McGarity, *Peer Review in Awarding Federal Grants in the Arts and Sciences*, 9 HIGH TECH. L.J. 1 (1994).

⁷ *Id.* at 39–42. McGarity considers what he terms “radical alternatives to peer review,” including review based on publication history. See *supra* note 3 and accompanying text.

⁸ Proposals for reward systems include Steve P. Calandrillo, *An Economic Analysis of Intellectual Property Rights: Justifications and Problems of Exclusive Rights, Incentives, and the Alternative of a Government-Run Reward System*, 9 FORDHAM INTELL. PROP. MEDIA & ENT. L.J. 301 (1998); Michael Kremer, *Patent Buyouts: A Mechanism for Encouraging Innovation*, 113 Q.J. ECON. 1137 (1998); Michael Polanvyi, *Patent Reform*, 11 REV. ECON. STUD. 61 (1944); and Steven Shavell & Tanguy van Ypersele, *Rewards Versus Intellectual Property Rights*, 44 J.L. & ECON. 525 (2001).

prize and retrospective grant institutions is an assumption that scientific organizations cannot be subject to risk, that they must know their funding in advance. This assumption is bizarre in a world in which private parties routinely invest resources without assurance of profit, and I will argue against the notion that science is unique in needing government to pick winners in advance. Given this argument, a prize system is just one obvious step beyond the retrospective grant institution, with money being used to reward past science with no requirement that it be used to further future science.

I would be willing to slide down the slippery slope. Indeed, I have argued in a separate paper that prize or reward systems might sometimes serve as complements to a patent system.⁹ One virtue of a reward system relative to the retrospective grant institution is that decisions on rewards need not be made at frequent intervals. Thus, a project can be judged at its completion or even later, with extra time serving to improve the prize givers' abilities to assess the significance of a particular contribution.¹⁰ A strike against a prize system is that it is quite different from any existing legal institution. The ultimate purpose of this paper is to show that several of the ideas underlying my advocacy of a prize system could be implemented without complete adoption of a retrospective prize system.

The essay will proceed as follows. Part I will recount the administrative history of the Human Genome Project, focusing on the debates about how the Project should be structured. My task is primarily descriptive, but my intent in offering this account is not to determine what the relevant questions are. To the contrary, I hope to show that those engaged in the debates considered a relatively narrow set of options and made questionable assumptions about what goals the administrative structure of the project should strive to promote. Part II sketches how a retrospective grant institution might work, considers objections, and assesses the extent to which this institution advances the postulated goals. Part III concludes.

II. THE HISTORY, STRUCTURE, AND FUNDING OF THE HUMAN GENOME PROJECT

At first glance, the Human Genome Project might appear to be the perfect model of "big science." This might seem so both in the sense that the Project was successful—the genome was sequenced, after all—and in the sense that the Project provides data on government involvement in big science generally. The

⁹ See Michael Abramowicz, *Perfecting Patent Prizes* (2002) (unpublished manuscript, on file with author).

¹⁰ *Id.* at 86–93.

ultimate success of the Project, however, remains unclear. The government's decision to invest in the Project reflected not just a belief that the gene sequence was intrinsically valuable, in the same way that any scientific or artistic project may increase our self-awareness, but also the view that the sequence ultimately would produce valuable remedies and medications. These are mostly in the future and even when they arrive, the question of how much, if at all, the Human Genome Project advanced them will remain. With legal institutions, in contrast to hard science, there are rarely controlled experiments.¹¹ As isolated data points go, however, the Human Genome Project has much to offer. Let us consider a brief history of the Project, with initial attention to occasions in which intellectual property approaches appeared to pose a challenge to the government-orchestrated Project.

A. A History of Private Challenges to the Human Genome Project

In 1987, as government agencies dithered over whether to undertake the major scientific endeavor of sequencing the human genome or to embark on a less ambitious gene mapping project, Dr. Walter Gilbert decided to break away from the debate.¹² He announced that he would form a private corporation that would sequence the genome, obtain a copyright on the data, and then sell it to researchers.¹³ This sparked debate over whether anyone could claim copyright protection for the sequence of the human genome. While his announcement created controversy over both intellectual property rights in genome research¹⁴ and the roles of private and public research,¹⁵ the fervor subsided when he failed to raise funds to launch his private effort.¹⁶ Many believed anyway that the federal government should fund and lead such a daunting exercise in basic scientific research,¹⁷ citing the potential public benefits and the prospect of

¹¹ Michael Dorf and Charles Sabel have urged a form of government that is inherently experimental. See, e.g., Michael C. Dorf & Charles F. Sabel, *A Constitution of Democratic Experimentalism*, 98 COLUM. L. REV. 267 (1998).

¹² Leslie Roberts, *Controversial From the Start*, 291 SCIENCE 1182, 1184 (Feb. 16, 2001).

¹³ Leslie Roberts, *Who Owns the Human Genome*, 237 SCIENCE 358 (July 24, 1987).

¹⁴ *Id.* at 359. At a June 26, 1987, conference titled "Issues of Collaboration for Human Genome Projects," much of the discussion centered on whether Dr. Gilbert could legally copyright the human genome sequence. Those who agreed with Gilbert that copyright protection was appropriate analogized the genome sequence to a computer program or to a series of letters like those in a book. *Id.* Others argued that the copyright would be not only inappropriate, but also unconstitutional. See Ira H. Carmen, *Letters: Ownership of the Human Genome*, 237 SCIENCE 1555 (Sept. 25, 1987).

¹⁵ See Lorraine L. Greenlee, *Letters: Ownership of the Human Genome*, 237 SCIENCE 1555 (Sept. 25, 1987).

¹⁶ Roberts, *supra* note 12, at 1184.

¹⁷ OFFICE OF TECHNOLOGY ASSESSMENT, U. S. CONGRESS, *MAPPING OUR GENES—GENOME PROJECTS: HOW BIG? HOW FAST?* 93 (1988) [hereinafter OTA REPORT]; see also Greenlee, *supra* note 15.

underinvestment by the private sector.¹⁸ But the debate over whether genome research should be public or private did not end with the inception of the publicly funded Human Genome Project.¹⁹

Dr. Gilbert's early frustration with what would soon be called the Human Genome Project foreshadowed the later actions of J. Craig Venter, who left the National Institutes of Health (NIH) to head a private effort to sequence the human genome using an alternative approach.²⁰ Venter's exit from NIH spurred the public Human Genome Project to reevaluate its methods and compete with his private effort.²¹ This competition resulted in the release of a sequenced human genome by both the Human Genome Project and Venter's Celera Genomics in June, 2000. Though controversy remains over the extent to which Celera relied on public data,²² there is no doubt that competition led to completion of the draft sequence far ahead of the original schedule.²³ Part of Venter's motivation in breaking away and creating a private endeavor was the prospect of being able to patent the genes he sequenced and gain profits from those patents.²⁴ This abbreviated history suggests that competition and methodological innovation ultimately benefited the Human Genome Project, but the competition and innovation largely occurred outside of the project itself. That is because the Project itself offered no structural mechanisms to encourage competition and innovation, as we will now see.

B. Structural Options Considered at the Outset of the Human Genome Project

Ten years before the Human Genome Project officially began, academics, government researchers and private researchers entered into nationwide discussion about the possibility of mapping and sequencing the human genome.²⁵ As it

¹⁸ Lewis D. Soloman & Suzanne E. Schoch, *Developing Critical Technologies: A Legal and Policy Analysis*, 9 SANTA CLARA COMPUTER & HIGH TECH. L.J. 153 (1993).

¹⁹ See, e.g., Mark D. Adams & J. Craig Venter, *Should Non-Peer-Reviewed Raw DNA Sequence Data Release Be Forced on the Scientific Community*, 274 SCIENCE 534 (1996); Keith Aoki, *Authors, Inventors and Trademark Owners: Private Intellectual Property and the Public Domain*, 18 COLUM.-VLA J.L. & ARTS 191 (1994).

²⁰ Roberts, *supra* note 12, at 1186–88.

²¹ *Id.*

²² See, e.g., Scott Hensley & Antonio Regalado, *Scientists Publish Critique of Celera's Work—Rivals Charge Firm Recycled Public Data in Genome Map*. WALL ST. J., Mar. 5, 2002, at A2.

²³ Eliot Marshall, *Rival Genome Sequencers Celebrate a Milestone Together*, 288 SCIENCE 2294 (June 30, 2000).

²⁴ Venter has since departed from Celera as the company moves away from basic research provision to pharmaceutical development. Andrew Pollack, *Scientist Quits The Company He Led in Quest For Genome*, N.Y. TIMES, Jan. 23, 2002, at C1.

²⁵ Leslie Roberts *et. al.*, *A History of the Human Genome Project*, 291 SCIENCE 1195 (Feb. 16, 2001) (noting that the first proposal of a method to map the entire human genome was made in 1980 by scientists at the Massachusetts Institute of Technology).

became clear that mapping and sequencing the genome were possible, the debate shifted to how it should be done. Many advocated a traditional “big science” approach that the Department of Energy (DOE) would head.²⁶ There had never been such an approach to microbiology and many resisted it, pressing instead for a smaller and decentralized effort, similar to NIH’s ongoing support of investigator-initiated research in genome mapping.²⁷ Still others pressed for a cooperative effort between the DOE and the NIH.²⁸ The National Resource Council (NRC) and the Office of Technology Assessment (OTA) both presented reports to Congress regarding the structure and funding of a government project to sequence the human genome.²⁹ These reports recognized the ultimate goals of the Human Genome Project as rapid accumulation of knowledge about the genome, efficient storage and distribution of that information, and conversion of the information gained into both general theories and specific products.³⁰

The NRC and OTA together considered five basic types of structures for the Human Genome Project: one agency, single agency leadership, inter-agency agreement and consultation, interagency task force, and consortium.³¹ The creators of the Human Genome Project wanted a structure that would organize communication in the scientific community and would facilitate planning a multi-faceted research program, fostering effective partnerships and effectively allocating funding. OTA recommended that Congress choose a structure for the project based on “perceptions of necessary patterns of authority, of quality and scope of experience in research and development, and of fiscal and economic priorities.”³² As these priorities suggest, participants in the debate seemed to assume that an advantage of relatively more centralized structures is that they would facilitate the establishment of a consistent research agenda. Perhaps this emphasis on consistency makes sense for NASA; if two different scientific teams were to build two different parts of the spacecraft, it is important that the parts be compatible with each other. As Ronald Coase argued in *The*

²⁶ Leslie Roberts, *Agencies Vie over Human Genome Project*, 237 SCIENCE 486 (July 31, 1987).

²⁷ James D. Watson, *The Human Genome Project: Past, Present and Future*, 248 SCIENCE 44, 45 (Apr. 6, 1990). Although NIH came later to the game, there was competition between DOE and NIH for control of any government-funded human genome research. Both pursued their own genome-related projects during the debate over genome mapping. Roberts, *supra* note 26, at 487–88.

²⁸ Roberts, *supra* note 26, at 488.

²⁹ Roger Lewin, *Genome Projects Ready to Go*, 240 SCIENCE 602 (Apr. 29, 1988); Robert Mullan Cook-Deegan, *Origins of The Human Genome Project*. FRANKLIN PIERCE L. CTR. GENOME PAPERS, at <http://fplc.edu/risk/vol5/spring/cookdeeg.htm>, at 5.

³⁰ OTA REPORT, *supra* note 17, at 11; COMMITTEE ON MAPPING AND SEQUENCING THE HUMANE GENOME, NATIONAL RESEARCH COUNCIL, NATIONAL SCIENCE FOUNDATION, MAPPING AND SEQUENCING THE HUMAN GENOME (1988) [hereinafter NRC REPORT].

³¹ OTA REPORT, *supra* note 17, at 115.

³² *Id.*, at 116, NRC REPORT, *supra* note 30, at 93.

Nature of the Firm,³³ sometimes transactions costs are sufficiently high that it is cheapest to organize production in a single entity. But there is little reason to think that such transactions costs and compatibility issues are relevant for genomics research. The significance of the sequencing of one gene does not depend on the method used for sequencing another. And if different approaches may lead in quite different directions, including, for example, some DNA sequencing and some analysis of proteins, the project is likely to produce greater benefits than if it follows a linear path, because the most valuable work on a subsidiary task may be more valuable than the least valuable work on a principal task. Those arguing about different proposed structures, however, failed to take these considerations into account.

1. One Agency

In the one-agency model, a single expert administrative agency would organize all genomic research.³⁴ The agency would set all research policy and execute all of the research using government-employed scientists, either within the agency or at the national laboratories. Proponents maintained that this would promote a consistent research policy, without explaining why that would be valuable. A problem with the approach is that once the expert agency chose a method and specialized in it, there would be no incentive for it to find better methods or, if better methods were available, to abandon the original choice. The one agency model failed, however, not for these reasons, but because of significant political drawbacks. Several agencies had already developed expertise in specific areas of research. Projects such as the GenBank and DNA repositories were already inter-agency.³⁵ The one-agency approach would have eliminated the involvement of all but one of those agencies and consolidated the interagency projects. Because of these problems, NRC, OTA, and Congress never seriously considered the one-agency model. It was quickly “dismissed as unnecessary and politically unworkable.”³⁶

2. Single-Agency Leadership

Under a single-agency leadership model, Congress would dedicate one agency to act as the coordinating and monitoring agency for all genomic research.³⁷ Unlike the one-agency model, this structure would not restrict the funding to

³³ Ronald H. Coase, *The Nature of the Firm*, 4 *ECONOMICA* (n.s.) 386 (1937), reprinted in RONALD H. COASE, *THE FIRM, THE MARKET, AND THE LAW* 33 (1988).

³⁴ OTA REPORT, *supra* note 17, at 115.

³⁵ *Id.*

³⁶ *Id.*; NRC REPORT, *supra* note 30, at 86, 88.

³⁷ OTA REPORT, *supra* note 17, at 116; NRC REPORT, *supra* note 30, at 94.

one agency, though it would designate one agency to administer the entire project,³⁸ with other agencies able to compete for funding.³⁹ The NRC proposed two modified versions of the pure single-agency model. In the one recommended by the majority of the NRC committee, a single agency would lead but a scientific advisory board would advise, appropriately.⁴⁰ In the second model, an interagency committee with a scientific advisory board would guide the project, but a single agency would retain control of the daily administration of the project.⁴¹ Those who backed the single-agency leadership models stressed their clear designation of authority and the ability of a single entity to determine a coherent theme and focus for all genome research.⁴² Detractors of this approach pointed out not that such consistency might be unnecessary, but that a single agency would not be able to marshal the resources of other agencies and might have a commitment limited to specific areas of genome research.⁴³

Perhaps the fatal problem, however, was once again that if this approach were adopted, a particular agency would have had to be selected to take the lead role: DOE or NIH.⁴⁴ Neither NRC nor OTA was willing to propose which particular agency should be the one chosen to lead the project.⁴⁵ The reports' assessments of the strengths of the two agencies help reveal the apparent priorities. DOE's greatest strength as a choice for lead agency was seen to be its ability to administer large, focused research programs and its demonstrated strong interest in genome research.⁴⁶ DOE's apparent weakness was its minimal experience in molecular biology, as its primary interest in genomic research had been limited to energy-related issues.⁴⁷ The reports thus seemed to assume that considerable expertise was necessary for an agency to conduct a project, ignoring the possibility that an agency could rely primarily on outside scientists. Meanwhile, although the reports applauded NIH's commitment to traditional

³⁸ NRC REPORT, *supra* note 30, at 94.

³⁹ *Id.*

⁴⁰ NRC REPORT, *supra* note 30, at 3, 93.

⁴¹ *Id.* at 97.

⁴² OTA REPORT, *supra* note 17, at 116.

⁴³ *Id.*; *see also* Lewin, *supra* note 29, at 603.

⁴⁴ Both OTA and NRC included the National Science Foundation (NSF) in their list of potential lead agencies, but neither gave much attention to the possibility. The NRC Report commented simply that the NSF was "involved in the development of technology and instrumentation relevant to the human genome project." NRC REPORT, *supra* note 30, at 94.

⁴⁵ OTA REPORT, *supra* note 17, at 118; NRC REPORT, *supra* note 30, at 94.

⁴⁶ OTA REPORT, *supra* note 17, at 118; NRC REPORT, *supra* note 30, at 94. DOE had been responsible for major projects in physics including the Manhattan Project. In addition, DOE had aggressively pursued its own human genome project while other agencies expressed less commitment to the idea of such a project.

⁴⁷ OTA REPORT, *supra* note 17, at 118. The genesis of DOE's genomic research came from administrators' desire to understand the effects of radiation from things like "dirty bombs" on humans, particularly on their genetic make-up. *Id.* at 6.

research through grants,⁴⁸ and although NIH was viewed as a leader in DNA research and had acted as overseer in some larger projects,⁴⁹ critics complained that because NIH focused on grants, it might not be able to support a focused effort as massive as the Human Genome Project.⁵⁰

3. Interagency Agreement and Consultation

This type of arrangement “eschews any formal creation of authority and relies on the good will of the participants to exchange information freely,” which reflects the “flexible, decentralized organization that are [the] strengths of American science.”⁵¹ Unlike the other options, this arrangement did not require new legislation.⁵² Each agency involved would theoretically work in its own “institutional area of interest,” and principals of the agency would communicate about research goals and developments.⁵³ Detractors warned that the “interests” of varying agencies would overlap and create duplication and that the potentially divergent priorities of various agencies would lead to a breakdown in communication.⁵⁴ There were also concerns that such an informal arrangement would not be appropriate to an expensive long-term effort such as genome mapping, which detractors argued yet again needed clear authority, accountability, and mechanisms to resolve disputes between agencies.⁵⁵

4. Interagency Task Force

The Interagency Task Force model, also called the Interagency Committee model, combined aspects of the single-agency leadership model and the informal agreement and consultation model.⁵⁶ A task force made up of principals from participating agencies would have the authority to design and direct the overall genome project.⁵⁷ It would also formally gather information, prepare reports, and formulate recommendations and future plans for the project. A task force would be the central point of contact for media and politicians. Detractors worried that the selection of the task force’s membership and chairperson might become lengthy political battles.⁵⁸ Delegation of research projects themselves

⁴⁸ *Id.* at 116–17; NRC REPORT, *supra* note 30, at 94.

⁴⁹ NRC REPORT, *supra* note 30, at 94.

⁵⁰ OTA REPORT, *supra* note 17, at 117.

⁵¹ *Id.* at 119.

⁵² *Id.* at 15.

⁵³ *Id.* at 118.

⁵⁴ *Id.* at 119.

⁵⁵ *Id.* at 118–19.

⁵⁶ *Id.* at 119; *see also* NRC REPORT, *supra* note 30, at 96; Lewin, *supra* note 29, at 603.

⁵⁷ OTA REPORT, *supra* note 17, at 119; *see also* NRC REPORT, *supra* note 30, at 96.

would lead to political infighting among the agencies over what distribution would be equitable. Some argued that a task force would create yet another level of meaningless bureaucracy, serving to do nothing more than further remove the administration of science from those performing the research.⁵⁹ Though this point comes close to recognizing that innovation is likely to come bottom-up rather than top-down, the focus seemed to be on the distance between the administrators and the scientists, not the locus of decision-making authority.

5. Consortium

The consortium model was the only model discussed that involved the private sector in the organization of the project.⁶⁰ Based on smaller consortia between states and their universities,⁶¹ the consortium model involved government research facilities and agencies as well as private companies who would be interested in the commercial applications of research. Generally, consortia involve government funding at the outset of basic research and private funding for development projects that stem from basic research.⁶² The theory is that government and universities allow basic research to move forward, while firms use the developing technologies to create new products.⁶³ The main problem with the consortium model was seen as preventing the setting of a research agenda.⁶⁴ Thus, while the debate did consider the possible use of the private sector, it did not consider that the private sector might advance basic research itself, and it assumed once again that project coherence was necessary.

6. The Solution

Ultimately, NRC recommended that Congress adopt the single-agency leadership model, and OTA recommend a collaborative effort between agencies.⁶⁵ Congress allocated funds to both NIH and DOE to participate in a coordinated

⁵⁸ Drafters of the NRC report seemed less bothered by this than drafters of the OTA report. The NRC report suggested that the committee would be made up of members from NIH, DOE, NSF and other interested agencies, and that the chairmanship would rotate among NIH, DOE, and NSF, NRC REPORT, *supra* note 30, at 96.

⁵⁹ OTA REPORT, *supra* note 17, at 121.

⁶⁰ *Id.*

⁶¹ An example is the Midwest Plant Biotechnology Consortium. Cited specifically by OTA, this consortium was created to increase America's position in agriculture by encouraging basic plant biotechnology research. *Id.* at 122.

⁶² *Id.* at 121.

⁶³ *Id.* at 122.

⁶⁴ *Id.* at 123.

⁶⁵ Cook-Deegan, *supra* note 29.

Human Genome Project.⁶⁶ In 1988, Congress appropriated twelve million dollars to DOE for its initiative and slightly over seventeen million dollars to the National Institutes of General Medical Sciences (NIGMS), a part of NIH, earmarked specifically for “genome studies.”⁶⁷ NIH promptly created an advisory committee to decide its genome research priorities.⁶⁸ Concerned about how their programs would relate to each other, DOE and NIH signed a “Memorandum of Understanding” that created a joint subcommittee whose members came from the genome research advisory committees of each agency.⁶⁹ The subcommittee set to work creating the first 5-year plan for the Human Genome Project. In the meantime, both NIH and DOE increased their 1990 budget requests for genome research. In response, Congress appropriated twenty-eight million dollars to DOE’s program and just under sixty million dollars to the newly created National Center for Human Genome Research (NCHGR), which later became the National Human Genome Research Institute (NHGRI) at NIH.⁷⁰ Congress recognized the interagency commitment to a Human Genome Project and promised three billion dollars over 15 years. Using this funding, DOE and NIH would finance research with the goal of finding all the human genes and sequencing the DNA base pairs.⁷¹ Although the amount of funding and the breadth of the Human Genome Project have changed over the years, NIH and DOE remain the principal agencies charged with heading the effort.⁷²

C. Funding Mechanisms

Of greater consequence than the allocation of authority among agencies are the mechanisms used to support scientific research. Both DOE and NIH support genome research through a combination of internal work, contracts with national laboratories and research centers, and grants to individual researchers, research institutions, and private companies.⁷³ NIH supports more extramural research than it does intramural research, while DOE uses most of its Human Genome Project budget to support intramural research at the national

⁶⁶ Watson, *supra* note 18.

⁶⁷ *Id.* at 46.

⁶⁸ *Id.*

⁶⁹ Leslie Roberts, *NIH and DOE Draft Genome Pact*, 241 SCIENCE 1596 (Sept. 23, 1988).

⁷⁰ Cook-Deegan, *supra* note 20, at 6.

⁷¹ U.S. Human Genome Project, *5-Year Research Goals 1998–2003: Time Table Accelerated on U.S. Human Genome Project*, available at http://www.ornl.gov/TechResources/Human_Genome/hg5yp/

⁷² In 2001, the United States Government spent \$394.8 million on Human Genome Project research. Human Genome Project Information, Human Genome Project Budget, at <http://www.ornl.gov/hgmis/project/budget.html>

⁷³ OTA REPORT, *supra* note 17, at 93.

labs.⁷⁴ Private companies conduct research in conjunction with or similar to the Human Genome Project with the hope of profiting from that research through the use of intellectual property protections including patents.⁷⁵

1. Direct Appropriations and Government Contracts

Both NIH and DOE receive funds for the Human Genome Project through congressional appropriations.⁷⁶ While the vast majority of these appropriations are earmarked only for the Human Genome Project, some appropriations carry more specific directions.⁷⁷ An appropriation may direct DOE to use a certain laboratory or may indicate a specific area within the Human Genome Project for which specific monies are meant to be used.⁷⁸ Such congressional earmarks are the least flexible mode of financing, offering no incentive by private parties or the government to innovate. Otherwise, NIH and DOE can use the appropriated funds to do intramural research or to employ contractors to perform specific research directed by the agency. NIH's intramural research is done by salaried government employees and recipients of NIH research fellowships.⁷⁹ Such funding generally is not conditional on any kind of performance review. When an agency contracts for specific research projects with national laboratories or other entities, these contracts are for specific research tasks or projects.⁸⁰ These contracts span a period of years and are not regularly subject to competitive bidding, as they are usually mere addenda to existing operational contracts.⁸¹

⁷⁴ OFFICE OF TECHNOLOGY ASSESSMENT, U.S. CONGRESS, FEDERAL TECHNOLOGY TRANSFER AND THE HUMAN GENOME PROJECT 3, 12 (1995) [hereinafter OTA TECH. TRANSFER REPORT].

⁷⁵ *Id.* at 3; see also Eliot Marshall, *The Company That Genome Researchers Love to Hate: Human Genome Services, Inc.*, 266 SCIENCE 1800 (Dec. 16, 1994); Eliot Marshall *et al.*, *In the Crossfire: Collins on Genomes, Patents, and Rivalry*, 287 SCIENCE 2396 (Mar. 31, 2000); Elizabeth Pennisi, *Genomics Comes of Age*, 290 SCIENCE 2220 (Dec. 22, 2000).

⁷⁶ See Watson, *supra* note 27, at 45–47; see also Jeffrey Mervis, *R&D Budget: Growth in Hard Times*, 263 SCIENCE 744 (Feb. 11, 1994).

⁷⁷ Often specific research programs receive funding based on either the agency's specific request or on the priorities of particular politicians. See Andrew Lawler, *Easing the Squeeze on R&D: Growth in the Economy Frees Money for Research*, 278 SCIENCE 1390 (Nov. 27, 1990); see also National Institutes of Health, *FY 2002 Budget Request*, Statement of Francis S. Collins available at http://www.nhgri.nih.gov/About_NHGR1/Od/Admin/Budget/fy02congStmnt.html

⁷⁸ See, e.g., 147 CONG. REC. S1139 (daily ed. Nov. 1, 2001) (statement of Sen. Boxer) (discussing the Department of Energy appropriations bill and correcting the name of the University of Southern California program to be supported with federal funds). Regardless of whether an appropriations bill actually spells out what lab funding should go to, members of Congress often know, at least in the case of intramural and contract research, who is likely to receive the funding.

⁷⁹ See Division of Intramural Research, *About the DIR*, at http://www.nhgri.nih.gov/Intramural_research/about_DIR.html

⁸⁰ For example, Lawrence Livermore, Lawrence Berkeley, and Los Alamos national laboratories have contracted with DOE to sequence chromosomes 5, 16, and 19. See The Joint Genome Institute, *Decoding the Human Genome*, at <http://www.llnl.gov/str/Branscomb.html>

2. Grants to Individuals and Institutions

When giving grants, the agencies conduct peer review on independent researchers' grant proposals, and committees decide whether to offer grants and how much money to give.⁸² The agencies award grants to both academic centers and commercial ventures, with private entities increasingly winning grants over academic centers.⁸³ Both DOE and NIH evaluate grant applications on a variety of factors, of which the competency of an applicant's personnel is one.⁸⁴ Although past performance will be considered at least in the DOE for those who seek to renew existing grants, the process does not give this consideration overriding significance. Grants thus provide for a type of competition, but the competition is primarily in the writing of grant proposals rather than in the performance of scientific research.

3. Cooperative Research and Development Agreements

Cooperative Research and Development Agreements (CRADA's), which Congress first created under the Federal Technology Transfer Act of 1986,⁸⁵ allow national laboratories to partner with private firms to develop products based on their research.⁸⁶ Each agreement can last for up to but no more than four years. Under a CRADA, "the government provides personnel, services, facilities, equipment or other resources (but not funds) and the nonfederal partner provides funds, personnel, services, equipment or other resources toward the conduct of specific research or development efforts consistent with the missions

⁸¹ See Lawrence Livermore, NATIONAL LABORATORY, INSTITUTIONAL PLAN FY 2001–2005 (2000) (noting that Livermore's work for the U.S. Government is performed under general contract W-7405-Eng-48).

⁸² See Center for Scientific Review, *A Straightforward Description of What Happens to Your Research Project Grant Application After It Is Received for Peer Review*, <http://www.csr.nih.gov/REVIEW/peerrev.htm> (Aug. 24, 2001).

⁸³ See Elliot Marshall, *Commercial Firms Win U.S. Sequencing Funds*, 285 SCIENCE 310 (1999) (reporting on \$15 million in grants to Genome Therapeutics Corp. and Incyte Pharmaceuticals, Inc.).

⁸⁴ U.S. Department of Energy, Office of Science, *Grant Application Guide*, at <http://www.er.doe.gov/production/grants/process.html> The National Human Genome Research Institute, *Guide for Applicants: Genome Research Resource Grants (P41)*, at http://www.nhgri.nih.gov/Grantinfo/Funding/Statements/p41_guide.html The factors considered by NIH for resource development grants include the potential for significant impact of the new technology on genome research, the scientific or technical significance and originality of the proposal, the appropriateness and adequacy of the experimental or engineering approaches to be used, documented community need for the resource, appropriateness and adequacy of plans for technical support, qualifications and experience of the principal investigator and the staff, reasonableness of budget and duration proposed and the adequacy of dissemination or commercialization plans.

⁸⁵ 15 U.S.C.A. § 3701 et seq.

⁸⁶ OTA TECH. TRANSFER REPORT, *supra* note 74, at 8, 47.

of the federal facility.”⁸⁷ Both the government and its partner provide resources for research and development that is “consistent with the missions” of the national lab.⁸⁸ In 1995, surveys of CRADA participants revealed views that these agreements had facilitated the sharing of resources and provided an economic benefit to the private entities, but not necessarily to the government agencies.⁸⁹ Although CRADAs offer a variant on grants, if anything they are less likely to invite competition and innovation, because partners do not have to compete regularly in offering new proposals.

D. Goals of Structure and Funding

So far I have argued that the Human Genome Project founders placed too much emphasis on consistency and developed mechanisms for distributing funds unlikely to promote innovation. I shall now offer a list of goals that an ideal system would meet. Each of these goals ultimately is subservient to a larger goal, maximization of the production of discoveries leading to technological progress that individuals would value. This is a broad goal, but it does rule out two possibilities. First, it rules out the possibility that the goal of governmental science should be to maximize knowledge irrespective of its usefulness. Knowledge may be intrinsically valuable,⁹⁰ but if we assume that the goal of science is knowledge for knowledge’s sake, then there is no obvious metric by which we can compare the success of one institution with that of another. Scientific experiments often turn out to be useful in unexpected ways,⁹¹ but this does not mean usefulness is an inappropriate criterion. Rather, it means that if some areas of scientific inquiry are more likely to lead to surprising byproducts than others, we should, all else being equal, seek to encourage more research in the former areas than in the latter. The focus on usefulness is, in any event, more important for the assessment of how the government implements its commitment to scientific advance generally than it is for the assessment of possible programs by which the government might seek to advance scientific knowledge in some area, whether that area is genomics or theoretical physics.

Second, the criterion of maximization of useful scientific advances assumes that more science is better than less. Maximization of science, however, need not necessarily improve human well-being. One reason is that science

⁸⁷ *Id.* at 14.

⁸⁸ *Id.* at 14.

⁸⁹ *Id.* at 3, 20.

⁹⁰ For a discussion of the distinction between pure and applied science, and the claim that scientists make to autonomy from outside regulation, see Barry R. Furrow, *Governing Science: Public Risks and Private Remedies*, 131 U. PA. L. REV. 1403, 1415–17 (1983).

⁹¹ The classic example is Alexander Fleming’s fortuitous discovery of penicillin. See, e.g., L. Ludovici, FLEMING: DISCOVERER OF PENICILLIN 131–34 (1952).

could produce discoveries with negative consequences, perhaps catastrophic ones.⁹² Another is that society conceivably could spend too much on science, at the expense of satisfying other human wants, from immediate health care needs to national security to entertainment.⁹³ Nonetheless, I assume away these issues for analytical simplicity. If there is too much government science, a remedy is not to create a government program, and if there is too much private science, a remedy is to shorten patent terms.⁹⁴ My interest is in how government should design an administrative program to promote science, given the assumption that more science is better than less.⁹⁵

Given the ultimate goal of maximizing scientific advances, I will offer five criteria: flexibility, competition, nonexclusivity, nonredundancy, and cost minimization. My purpose for now will be to explain the criteria, without detailed comparison of different institutions' performance against the criteria, a project that I will turn to later in this essay.⁹⁶

Flexibility. Organizations often change their goals and the means by which they seek to meet those goals in response to new information. The stock market is perhaps the institution in our society that is most conducive to flexible decisionmaking. Equity flows to corporations that seem to have the best prospects, and if new information contradicts high expectations for a company, its activities will be contracted. Recent events like the telecommunications crash and the Enron fiasco may fairly be used to point out that stock markets are

⁹² Cf. David Whitehouse, *Not the End of the World*, BBC NEWS (July 22, 1999), available at <http://news.bbc.co.uk/1/hi/english/sci/tech/newsid399000/399513.stm> (discussing controversy over whether a certain experiment might cause the creation of a black hole that would devour the earth). While we often dismiss such possibilities, perhaps finding them paranoid or even amusing, that may be a reflection of the human cognitive tendency to pay too little attention to small probability, high magnitude costs.

⁹³ See Matthew Erramouspe, Comment, *Staking Patent Claims on the Human Blueprint: Rewards and Rent-Dissipating Races*, 43 UCLA L. REV. 961 (1996) (considering this possibility). Government funding of science also could lead to reduced advancement in science, if inefficient government science crowds out efficient private sector science. See Terence Kealey, *THE ECONOMIC LAWS OF SCIENTIFIC RESEARCH* (1996) (arguing that government investments in science have generally been welfare-reducing).

⁹⁴ An alternative remedy might be to make patent rights less certain. See Ian Ayres & Paul Klemperer, *Limiting Patentees' Market Power Without Reducing Innovation Incentives: The Perverse Benefits of Uncertainty and Non-Injunctive Remedies*, 97 MICH. L. REV. 985 (1999) (exploring the tradeoff between patent certainty and term length).

⁹⁵ My own instinct is that the benefits probably are worth the cost for anything the government reasonably might be expected to spend in the genomics area. Whether the space program receives excessive funding is a more difficult question. Cf. Bonnie E. Fought, Comment, *Legal Aspects of the Commercialization of Space Transportation Systems*, 3 HIGH TECH L.J. 99, 107-10 (1988) (discussing the cost efficiency of NASA).

⁹⁶ See *infra* Part II.D.

not perfect, but they also show that once poor prospects become manifest, the market responds. The flexibility of the stock market allows shareholders to cut their losses. When a project is to be funded outside of traditional capital markets, some alternative way of ensuring flexibility is desirable. A flexible project would be one that first, could be expanded or contracted as the project turns out to be more or less successful than imagined, and second, can move research in more profitable directions. On the former score, the Human Genome Project had no explicit mechanism that would lead to contraction if the project proved to be wasteful. In theory, Congress might have stopped funding, but the creation of a government bureaucracy devoted to the program makes such a decision less likely. The latter aspect of flexibility presents the more serious problem for the Human Genome Project. The premise of the project was that sequencing of the entire genome would be useful. There is some evidence that this premise has turned out to be correct, as even the sequencing of “junk” DNA has led to important discoveries.⁹⁷ But the Project lacked any mechanism that would have redirected funds to other research projects if sequencing junk itself turned out to be junk.

Competition. Just as competition in markets for goods and services is likely to lead to the highest quality of such goods and services, so too is competition in approaches likely to improve science. The possibility of private gains—whether reputational or financial—is the strongest inducement to innovation. The preliminary evidence from the Human Genome Project supports this observation, as the competition from the Celera group led to an earlier-than-expected completion of the initial sequencing. The competition that occurred, however, was more happenstance than a product of any specific structural mechanism intended to produce competition. Moreover, there is no guarantee that private competition will materialize for future big science projects, given the uncertain state of intellectual property rights in the area. Perhaps the absence of any specific structural mechanism can be attributed to a view that sequencing seemed to be a methodical project, requiring more brute force than innovation. That fact that the innovation turned out to be important even for such a project highlights the need for encouraging competition.

Nonexclusivity. If the results of scientific research are made available to all, the number of future innovations will be greater. Availability of data was certainly a goal of the Human Genome Project, as the resulting sequences

⁹⁷ See, e.g., Kristen Philipkoski, *DNA Junkyard Yielding Gold*, WIRED NEWS (Feb. 12, 2001), available at <http://www.wired.com/news/technology/0.1282.41750,00.html> Some scientists, however, argue that even if junk DNA has some function that sequencing helps elucidate, it is still not the best use of resources to sequence the complete genome. *Id.* (“[Bill] Haseltine believes studying the whole genome is a waste. ‘It’s clear we should focus on genes and not the genome,’ he said.”).

were to be placed and in fact were placed in the public domain.⁹⁸ The designers of the Project, however, gave little consideration to whether private researchers could obtain patents on sequences. Perhaps that absence of attention ultimately proved beneficial, by providing a means by which private parties were able to compete. This highlights that the possibility of property rights may provide a useful incentive for private innovation. My point is simply that if we can produce innovation without exclusive property rights, that is likely to be better than producing innovation with them. An important caveat is that property rights may help encourage commercialization of the fruits of government research;⁹⁹ such property rights, however, need not necessarily be on the government research itself.

Nonredundancy. If different sets of scientists work on the same problem at the same time, some of the work they produce might be redundant, and there will be fewer scientific advances than if scientists worked on different projects. There may be some situations in which redundancy is worthwhile if the successful completion of one goal is so much more important than the marginal project that the additional competition outweighs the redundancy. Perhaps one virtue of the Human Genome Project as originally designed was that it was nonredundant. Instead of having different teams of scientists sequencing the same genes simultaneously, scientists would sequence each gene once. Because the Project did not prevent separate private research, however, it failed to stop redundancy. Once again, Celera's entry may have been socially worthwhile on the whole, but science would be advanced further if it were possible to harness the benefits of competition without redundancy.

Cost minimization. This consideration includes not only the cost of the administrative apparatus itself, but also costs incurred by private parties in connection with the administrative scheme. For example, if a particular design for big science, such as reliance on government contracts with ambiguous terms, will lead to litigation, that litigation is a social cost that offsets the benefits of the project. The Human Genome Project should be credited with inducing relatively little litigation,¹⁰⁰ but the involvement of multiple agencies may have increased more traditional types of costs. One type of cost that is particularly significant is political rent-seeking costs,¹⁰¹ such as the costs associated with lobbying government officials. These are

⁹⁸ See <http://genome.ucsc.edu/>

⁹⁹ See F. Scott Kieff, *Property Rights and Property Rules for Commercializing Inventions*, 85 MINN. L. REV. 697, 717–36 (2001).

¹⁰⁰ None of the nine published cases mentioning the Human Genome Project involves litigation over the Project itself. Search of Westlaw, ALLCASES database (Mar. 13, 2002).

¹⁰¹ See Dennis C. Mueller, PUBLIC CHOICE II at 229–38 (1989) (providing an overview of rent seeking).

likely to be particularly high to the extent that Congress appropriates money for particular organizations, or to the extent that there is a chance that Congress might do so. These costs are of concern not only directly, but also because they may distort government decision-making.

III. A PROPOSAL FOR ADMINISTRATION OF SCIENCE

I have argued that competition and innovation in genomics research happened in spite of the Human Genome Project, rather than because of it. One might argue that this is unfair, because the developers of the Human Genome Project created it against a background of legal institutions, and the creators of the structure for the Human Genome Project cannot be faulted for implicitly taking advantage of those institutions' strengths, whether consciously or not. It would be a mistake, however, to assume that we have achieved just the proper combination of policies so that the strengths of one will diminish the weaknesses of another. Moreover, it would be foolish to assume that a multipronged approach is necessarily the best strategy. A multipronged approach might be worse than a single-minded one if the negative aspects of a particular funding strategy manifest themselves even when that strategy is inducing only a relatively small amount of investment. For example, if patent law induces a relatively small percentage of sequencing work, its benefits may be negligible, yet the anticommons problem suggests that its costs might be quite high.¹⁰²

Thus the combination of patents, grants, contracts, and intramural research that the Human Genome Project exemplifies may not be optimal. The most obvious alternative would be to choose one of the above. We might well be better off, for example, if we left further genomics work (and if we had left past genomics work) entirely to the patent process, perhaps a modified version with either stronger property rights or a fair use doctrine to weaken them.¹⁰³ Although there might still be holdup problems, economies of scale might lead to the establishment of a relatively small number of dominant players, and thus the conditions for an effective private rights-management organization might have formed.¹⁰⁴ And similar arguments might be offered for the primacy of either of the other solutions. I recognize the importance of the

¹⁰² See Heller & Eisenberg, *supra* note 5 (discussing the problem); see also Michael A. Heller, *The Tragedy of the Anticommons: Property in the Transition from Marx to Markets*, 111 HARV. L. REV. 621 (1998) (introducing the anticommons problem in a general property context).

¹⁰³ See Maureen A. O'Rourke, *Toward a Doctrine of Fair Use in Patent Law*, 100 COLUM. L. REV. 1177 (2000).

¹⁰⁴ See Robert P. Merges, *Contracting into Liability Rules: Intellectual Property Rights and Collective Rights Organizations*, 84 CAL. L. REV. 1293 (1996) (emphasizing that private organizations often emerge to manage and license rights).

debate over which of the approaches would be the best if we were forced to select one, because perhaps we should be forced to make just such a choice. Nonetheless, none alone will be able to meet all of the goals we would like to achieve in funding big science, and thus the choice among these alternatives must be made while holding one's nose.

Instead of advocating for any of these, I will sketch out an alternative model. The model adheres more closely to the grant process than to its alternatives, in part based on a practical recognition that any conscious further government effort to encourage innovation in big science will probably take that form. The approach, however, draws on the virtues of all three strategies, seeking to create a regime that involves some centralized government control but considerable private competition. I recognize that one should address the task of building a better structure with some trepidation, both because what works on paper may not work in practice, and because what sounds radical on paper will never make it into practice. While the overall institution that I aim to describe would be different from what we now have, it uses elements that are familiar. Where I offer new twists to old forms, I seek to do so in ways that the old institutions are still visible in the new.

A. A Sketch of a Retrospective Grant Institution

The essence of my proposal is that grants should be provided based on the success of previous research results on the same project rather than on the basis of future plans. Peer review, of course, typically does involve consideration of an applicant's past efforts, at least implicitly, except in situations in which peer review is blind.¹⁰⁵ Ordinarily, however, reviewers select grants on the basis of anticipated future performance, rather than past performance, even when taking into account the success of past results or the reputation of the applicants. This practice presumably reflects the common sense intuition that it is easier to predict how successful a project will be with knowledge of what the project will entail. More information, it might seem, is better than less information, and if the government is to select those projects that have the greatest likelihood of success, common sense suggests that its decision makers are far better off if they can assess the project itself.

Common sense, however, misses that the question is not *which* information will be considered, but *who* will consider it. There are, in essence, two decisionmakers: the grant giver and the scientist (a shorthand that I will use to encompass also private, academic, and other organizations pursuing scientific research). Even if the grant giver does not directly consider what project is more

¹⁰⁵ See generally *How Blind is Blind Review?* AM. J. PUB. HEALTH. July 1991, at 843 (discussing blind review and the contention that it eliminates bias).

likely to be successful, a scientist provided with a grant but concerned about receiving a grant in the next period as well will make an assessment among alternative projects. Individual scientists are likely to be better situated than government grant givers to make this decision. In part, this is true because scientists are likely to confront practical research questions most directly and thus are most likely to have the knowledge and information needed to answer those questions. Equally importantly, unlike government decisionmakers, individual researchers have a stake, professional if not financial, in the quality of the decision and thus strong incentives to make it well.

That scientists rather than reviewers will make the fundamental choices in a retrospective grant system points to several advantages of such a regime. First, it encourages scientists to pursue those research strategies that they themselves believe are most likely to be productive. In a more typical grant regime, the focus of scientists will be on preparing a proposal likely to appeal to those making grants, such as members of the administrative agency, and subsequently will be on completing the grant as proposed. Retrospective grants do not eliminate the scientist's incentive to cater to others' tastes, because scientists will still seek to achieve results that grant givers are likely to find important. Nonetheless, a scientist who believes that her approach is more likely to produce a desired result than an alternative approach will be able to prove it.

Second, the system will encourage risk-taking. A scientist who otherwise would not expect to receive large grants might adopt an unconventional approach even if this approach is unlikely to succeed. If the project fails, the scientist will expect to receive less than if the scientist had taken an unconventional approach, though perhaps not zero, as the demonstration that a particular approach is ineffective itself can be a valuable contribution. And if a project thought unlikely to be productive does succeed, the scientist will receive considerably larger grants. The possibility that upstarts will take risks in turn may encourage leaders, scientists, or laboratories receiving relatively large grants to do the same.¹⁰⁶ Of course, more risk-taking is not always better, as sometimes it might be undesirable for scientists to pursue a low expected value, high upside project. The experience of the Human Genome Project, however, suggests that risk-taking may lead to substantial advances. Moreover, even with retrospective decision making, scientists are likely to accept less risk than is socially desirable because individuals are generally more risk-averse than society at large.¹⁰⁷

¹⁰⁶ An absence of risk-takers is similarly likely to dampen innovation in corporate law. See Marcel Kahan & Ehud Kamar, *The Myth of State Competition in Corporate Law* (2002) (unpublished manuscript, on file with author). (arguing that there is not much competition among state corporate laws, thus relieving Delaware of much of the burden of competition).

¹⁰⁷ See Kenneth J. Arrow & Robert C. Lind, *Uncertainty and the Evaluation of Public Investment Decisions*, 60 AM. ECON. REV. 364 (1970). (demonstrating that if the number of taxpayers is large, then public planners can ignore the risk of any particular project).

Third, the system will make science more flexible and better able to respond to new information about the approaches that are likely to produce useful advances. This is true both within a period in which grants are being performed and across periods. Within a research period, a scientist who believes that a project is not going well or who recognizes that an abrupt change in plans would be desirable will have an incentive to switch gears. With traditional grant institutions, a scientist might prefer to finish a project that is unlikely to be useful, and thus complete the grant, than switch to a new project. Sometimes, a scientist might conclude that completion of an unsuccessful project will provide more social value (and thus a larger future grant) than a late switch to a new project, but at least the scientist has the incentive to make the appropriate comparison. If one scientist's efforts change other scientists' assessments about which approaches are most likely to produce useful results, the other scientists will have an incentive to follow the first scientist's lead.

The most serious potential disadvantage to grants that are based on *ex post* performance is that they might lead to redundant research, as with patent races. Suppose, for example, that the benefits of one research project seem likely to be more than twice as great as those of the next research project. Then two different firms might perform the same research, even if they expect that the ultimate grant determinations would give each only half credit for discoveries. Even with less of a gap, failure among firms to coordinate with each other or a game of chicken in which neither party wants to give up the preferred research project might lead to redundancy. A virtue of *ex ante* grants is that decision makers ordinarily will pick no more than one party to perform a particular project, thus eliminating the possibility of redundancy.

An agency providing grants on the basis of *ex post* performance, however, easily could perform the same function. Just because grant amounts are retrospective does not mean that the agency cannot make prospective assignments. If the agency dictates what a particular scientist should do, then the benefits discussed above would disappear, but there would be no such harm from the agency's designation of priorities on certain research projects. Doing so would still involve less work than screening proposals for ultimate merit, especially if the agency examines only proposals from institutions whose past work entitles them to funding. For example, if two different teams were sequencing genes, the agency could assign them different genes. Indeed, the agency ought to do so in the ordinary case even if the teams are using different techniques, unless there is reason to believe that any redundancy is worthwhile because the particular research project is so much more important than the marginal research project that would be ignored. Much of the overlap between Celera's work and that of the Human Genome Project could have been eliminated through the assignment of property rights to research different sections of the genome. (These property rights would not be patents, of course, but merely

rights to research in a particular area, valid over other potential grant recipients.) The enforcement of such property rights could occur in the next grant cycle, with appropriate deductions made if a grant applicant had strayed onto another's jurisdiction.¹⁰⁸

B. Refinements

I have as yet provided only the vaguest description of how a retrospective grant-giving institution would operate. While I do not intend to fill in regulatory minutiae, it may be worthwhile to consider some of the specific issues that likely would develop in the design of such an institution. The most obvious question is how the agency should handle the first and last periods in which grant money is being distributed. A simple solution for the first period would be to use prospective grants for that alone. Thus, the seed money would depend on an assessment of future plans, as well as factors like reputation and infrastructure, but performance in the first period would determine funding in the second period, so the benefits of retrospective decision making would not be lost. The last period, meanwhile, might be rewarded with a simple distribution of cash, or an open-ended grant. Taken together, this approach would lead to one more set of checks being issued than the number of periods, but this additional expenditure simply increases the incentive that scientists will have to participate in the process and engage in the desired research. If this were deemed undesirable, researchers might be required to put up their own seed money in the first period.

Perhaps the most important issue is who should make the decisions on grants. One concern might be that if the preferences of the decision makers is known, much of the value of a retrospective process might be lost. This objection can be overblown, as even a decision maker whose belief in a certain strategy is known may be willing to concede in making retrospective assessments, should research prove a different strategy to be effective. It should not, however, be underestimated. The problem may be particularly severe when the ultimate objective is far off. Suppose, for example, that a number of intermediate steps, each taking a research period, are necessary before any tangible product of research emerges, such as development and testing of a pharmaceutical drug. Then the retrospective grant institution might be a little different from a prospective one, because decision makers will assess the success of past work by their estimates of future success. Only if a scientist believes that success in an early step will sway decision makers will she be willing to take risks and pursue

¹⁰⁸ An alternative, though more speculative possibility, would be for the agency to auction off the right to pursue particularly attractive research prospects. Cf. Dean Lueck. *The Rule of First Possession and the Design of the Law*, 38 J.L. & ECON. 393, 403 (1995). (noting that auctions are the most common alternative to first possession rules in assigning property rights).

the path that she believes is most likely to be successful in the long term. Sensible steps are meaningful only if they are perceptible.

The key to finding a solution to this problem is the recognition that the problem is not so much the impossibility of proving the success of a stage of a research project, but the danger of bias in the officials deciding grants. If a biased decisionmaker decides grants for each period, then the decisionmaker might refuse to recognize that a daring project has exceeded expectations. If a decisionmaker is open-minded, however, the multi-stage nature of a research project is of little consequence. Such a decisionmaker should be willing to make honest assessments of progress and should take into account that some risky projects should be rewarded because of the possibility that they will lead to substantial advances. The problem is that even for honest and disinterested individuals, it may be impossible to place aside one's expectations of success from one round to the next, as decisionmakers may interpret equivocal evidence as reinforcing prior beliefs. Thus, even a grant-giver who believes he is willing to admit error may tend to believe that equivocal results reinforce his initial preferences.

Rotating the decisionmakers who dictate grant amounts may be sufficient to overcome this hurdle. If the individual who chooses a grant amount one period is different from the one who does so the next, then initial conceptions of the likely success of a project will not be eternal determinants of grant funding. Rather than rely on internal staff to make decisions about grants, an agency should rely on outside scientific experts, as in many peer review programs, with those consulted one period ineligible for at least several subsequent periods. One danger to be guarded against is that the choices of decisionmakers might be reflections of the preferences of agency decisionmakers, for then scientist will still cater to some particular decisionmaker's agenda. To avoid this problem, the agency's responsibility should not be to select the "best" decisionmaker, but to assess all applicants against a relatively objective set of minimum qualifying criteria. The decisionmakers in a particular period could then be chosen by lottery from among all those qualified. Although there is some chance that subjective considerations would influence whom the agency would deem sufficiently qualified, and thus who scientists would anticipate might set their grants, such an approach would be considerably more objective than the alternative.

This approach cannot eliminate the possibility that some decisionmakers will approach the grant task with preconceptions, and it may increase the risk that a decision on a particular grant will reflect an idiosyncratic preference. The important point, however, is that errors in grant decisions are of relatively little consequence, leading at worst to the occasional provision of resources to scientists who are not necessarily in the best position to make progress. The decisions that matter most are the decisions of scientists about what to research, made in anticipation of subsequent decisions on grants. And these decisions will be made not in the shadow of any particular decisionmaker,

but in the shadow of a hypothetical average decisionmaker. Thus, even if decisions on grants are idiosyncratic—with a particular research team receiving more than enough to cover expenses one period, less the next—rotation ensures that decisions on what to research will not be. As long as a scientist believes that she can establish promise in an unconventional approach and that an average decisionmaker will recognize the value of diversified approaches, she will be willing to take that path.

Even with a rotation of decisionmakers, a grant could be based on some average of performance in previous periods rather than on the evaluation of performance in the immediately preceding period. There is a tradeoff here. On one hand, incentives for potential grant recipients will be greatest if the performance in a particular period is the sole determinant of the size of the grant. The difference appears primarily only in the latest periods, because in those periods performance will have only a small effect on future grant receipts. On the other hand, it may be problematic for scientists to have research support increase and decrease dramatically from period to period. I believe that this problem is not serious, for reasons I will explain below,¹⁰⁹ but at least this objection might make a system without some form of averaging politically infeasible. Perhaps an even more politically palatable system would be one in which performance grades would determine only whether there would be relatively small increases or decreases in grants from one period to the next. With such a system, a grant applicant's original reputation or proposal might have a substantial effect on subsequent grant payments, but there is still some incentive for innovation.

In discussing how the grant-making process should be structured, I have made one notable omission: I have not addressed the issues that dominated discussion at the onset of the Human Genome Project, how the agencies themselves should be structured. These decisions are of relatively little importance if research is funded through grants rather than being accomplished by internal agency scientists. Perhaps it is best to have just one agency running the project to minimize transactions costs, or perhaps it would be better for different agencies to control distribution to ensure that grants reflect a diversity of views. The difference will not likely be large. I do not expect that these questions will disappear, as government officials with an interest in increasing their own portfolio can be expected to seek jurisdiction over new science projects.¹¹⁰ Of course, this recognition that government bureaucracies can be self-perpetuating only increases the normative case for a system in which a relatively small government agency would disburse money, because a small agency is less likely

¹⁰⁹ See *infra* Part II.C.

¹¹⁰ See W. Niskanen, BUREAUCRACY AND REPRESENTATIVE GOVERNMENT 36–42 (1971). (noting the tendencies of members of bureaucracies to seek to increase their size.)

to be able to ensure its own preservation should a scientific project no longer be worth finding.

Perhaps the strongest objection to a system in which the agency officials' roles are administrative, coordinating the selection of grant decision-makers and disbursement of funds according to some formula, is that agencies might have specialized knowledge or expertise that could benefit the scientific process. Sometimes government scientists might be better than their private or academic counterparts, and a system in which grants are only made to non-governmental organizations may neglect them. This argument, however, is ultimately circular, for it fails to explain why the scientists work for the government rather than in the private or academic sectors. If they have developed expertise, they presumably can leave government work and compete for grants with private organizations. Perhaps there are occasions in which national security interests demand that the government rather than the private sector be responsible for scientific experimentation. Ordinarily, however, the benefits of having competitive forces stimulate research advances seem likely to be greater than any benefits of governmental organization. Indeed, my principal point is that government funding of science need not deprive us of vigorous competition.

C. Objections

Let me now consider two possible objections to a retrospective grants regime. The first objection points to my observation that even though the government agency will not consider prospective plans, individual scientists will have an incentive to do so. Perhaps agency officials are better able to develop objective assessments of scientists' ideas than are the scientists themselves. Such an argument might cite the cognitive psychology literature on the self-serving bias,¹¹¹ which here translates into individuals thinking more highly of their work than others do. This argument, however, could be made about any competitive regime. Perhaps we could have a government agency determine whether opening an ice cream parlor is a good idea, because individual entrepreneurs may overestimate the quality of their vanilla. Moreover, the retrospective grant system provides scientists incentives to pick the strongest project from among the various projects on which they might work, and there is little reason to think that the self-serving bias should distort an individual's choice of projects. Perhaps a scientist is more likely to favor an idea that he came up with than someone else's idea, but the ultimate quality rating at least will force a scientist to consider carefully various options.

¹¹¹ See, e.g., David Messick & Keith Sentis, *Fairness and Preference*, 15 J. EXPERIMENTAL SOC. PSYCHOL. 418 (1979).

A second objection is that retrospective financing would be too chaotic. Perhaps scientists are most comfortable when their future financing is settled. The argument is analogous to a claim often made about academic tenure, that it provides professors with the freedom necessary for high-quality scholarship. Freedom of speech, however, is hardly relevant in most science projects,¹¹² and freedom to pursue projects is more likely to produce indifference to others' views about the usefulness of projects than to stimulate useful innovation. More fundamentally, freedom and security are products of the organizations that employ scientists, except in the case of the lone scientist. An academic institution or private organization can promise to provide salary and funding to a scientist regardless of whether an agency gives a grant at the expected level. That private organizations often do not provide complete security and freedom to scientists reflects that those who run such organizations believe that the costs of such provision would exceed the benefits and that the employees of such organizations prefer to work with such pressure than to work with less pressure but lower salary. A retrospective grant system would allow for such tradeoffs to be made by private actors, with academic institutions and for-profit companies presumably taking different approaches.

This response might seem to invite a variant of the objection, that uncertainty in funding may wreak havoc on the organizations that employ scientists. It is not always possible, the theory states, for an organization simply to scale up and down a project. Large-scale endeavours sometimes require organizational commitments to persevere through unexpected obstacles, but such perseverance may not be possible if the obstacles are an absence of funding. This argument seems to reflect a view that organizations producing scientific research are incapable of bearing risk. The patent system, though, proves that risk and science are not inherently incompatible.¹¹³ Even academic and non-profit organizations can withstand some risk, taking losses on some projects while earning profits on others. Indeed, there is no reason that the government necessarily must require scientists to spend grants on further research. Grants used to reward past research will be more valuable the fewer strings attached, and so the government will maximize private investments in science the more flexibility scientists have in spending grants. Some organizations may respond by investing their own funds in early periods to build up a foundation for later, cheaper research. Grants used to reward such research ought to consider the quality of the results, not the investment in any given period.

¹¹² Such issues are potentially of greater significance for agencies like the National Endowment for the Humanities. See Alvaro Ignacio Anillo, Note, *The National Endowment for the Humanities: Control of Funding Versus Academic Freedom*, 45 VAND. L. REV. 455 (1992).

¹¹³ See, e.g., John F. Niblack, Ph.D., *Why Are Drug Development Programs Growing in Size and Cost? A View from Industry*, 52 FOOD & DRUG L.J. 151. (discussing the costs and risks associated with drug development.)

D. Assessment of the Proposal

Although I have provided only a sketch of a retrospective grant institution, no more is required to convey the theoretical point, and I have offered enough to allow for a preliminary consideration of the extent to which the institution meets the goals that I have set forth. First, consider flexibility. When scientists know that the compensation that they receive in the future depends on an *ex post* analysis of their work, the incentives to innovate and to change course in midstream will be greater than in cases in which compensation is tied only indirectly to performance. Thus the retrospective grant institution that I have described is certain to be more flexible than a traditional grant institution. It will be as flexible as a patent regime, but scientists in the two regimes will have flexibility to pursue different goals. With retrospective grants, scientists will have flexibility in the means they use to reach the goal of impressing subsequent government decisionmakers that they have contributed social value. With patents, scientists will have flexibility in the means they use to reach the goal of impressing customers or downstream users that their products will have private value to them. The advantage of the retrospective grant goal is that it emphasizes social value, which might be different from private value. The advantage of the patent goal is that private value is ultimately measured through revealed preferences rather than through a third-party assessment.¹¹⁴

The second criterion, competition, also recommends the retrospective grant institution over a more traditional alternative. The comparison is particularly favorable when the alternative is a government-provided intramural science, for the same reason that government generally has weak incentives when it is the provider of goods and services. Even against a traditional grant institution, the retrospective grant institution performs well. While both institutions involve competition, the traditional emphasizes competition in proposals, while the retrospective institution emphasizes competition in results. A potential disadvantage of the retrospective institution emerges in comparison to patent law. Competition in the retrospective grant institution is over a fixed sum, allocated by Congress or an administrative agency, and changeable only through the exercise of political power. The competition in patent law is scalable, so the number and size of the competitors will be proportional to the size of the projected rewards. The difference, however, will be of less significance if a broad approach is taken to defining the problems on which grant recipients might work. Perhaps defining the task of the scientific project as “sequencing” would entail a risk that more funds than necessary might be provided, but if the project

¹¹⁴ “Patents at least let the market decide.” H. I. Dutton, *THE PATENT SYSTEM AND INVENTIVE ACTIVITY DURING THE INDUSTRIAL REVOLUTION, 1750–1852*, at 26 (1984). (discussing nineteenth-century debates on patent protection.)

is genomics, there is a strong likelihood that there will be many projects producing positive social returns.

How the grant institutions, both traditional and retrospective, meet the third goal, which is nonexclusivity, depends on whether scientists are allowed to obtain patents on their discoveries. Some commentators have criticized policies that allow researchers to benefit from government largesse and then patent their discoveries as well,¹¹⁵ but economically it is conceivable that a regime in which research is awarded from both patents and grants may be superior to one in which grant recipients cannot obtain patents.¹¹⁶ Awarding grants shifts out the supply of innovation, but attaching a restriction preventing grant recipients from obtaining patents will not shift out the supply as much, and if patents are eliminated or restricted in the relevant field of endeavor,¹¹⁷ whether net supply increases or decreases depends on the relative size of the grant and patent incentives. The question thus becomes whether we are better off with patent protection in a particular area in which the government promotes research through grants, and general arguments about the costs and benefits of the patent system apply.¹¹⁸ At least, however, the grant institutions, unlike patents, may reduce the need for legal protections guaranteeing exclusivity. Neither the traditional nor the retrospective variety seems more likely than the other to promote this goal.

The grant institutions are also likely to be superior to the patent system in promoting nonredundancy. Patent races lead to redundant work before a patent is issued,¹¹⁹ and inventing leads to redundant work after a patent is issued.¹²⁰ Grants can limit the number of races to develop a particular technology, without necessarily limiting races to make progress on parallel technology tracks. In addition, to the extent that grants allow for the promotion of

¹¹⁵ See, e.g., Rebecca S. Eisenberg, *Public Research and Private Development: Patents and Technology Transfer in Government-Sponsored Research*, 82 VA. L. REV. 1663 (1996).

¹¹⁶ See, e.g., Richard E. Romano, *Aspects of R&D Subsidization*, 104 Q.J. ECON. 863 (1989).

¹¹⁷ The Patent and Trademark Office has signaled that it will restrict genomics patents by explaining how it will consider whether the patent application meets the utility requirement. See *Utility Examination Guidelines*, 66 Fed. Reg. 1092, 1098 (2001).

¹¹⁸ See, e.g., Fritz Machlup, AN ECONOMIC REVIEW OF THE PATENT SYSTEM, in SUB-COMM. ON PATENTS TRADEMARK AND COPYRIGHT OF THE SENATE COMM. ON THE JUDICIARY, 85th Cong. 44–45 (Comm. Print 1958). (summarizing such arguments.)

¹¹⁹ For discussions of patent races, see Partha Dasgupta & Joseph Stiglitz, *Industrial Structure and the Nature of Innovative Activity*, 90 ECON. J. 266, 284–87 (1980); Donald G. McFetridge & Douglas A. Smith, *Patents, Prospects, and Economic Surplus: A Comment*, 23 J.L. & ECON. 197 (1980); and Gideon Parchomovsky, *Publish or Perish*, 98 MICH. L. REV. 926 (2000).

¹²⁰ See, e.g., Louis Kaplow, *The Patent-Antitrust Intersection: A Reappraisal*, 97 HARV. L. REV. 1813, 1869 (1984); Donald F. Turner, *The Patent System and Competitive Policy*, 44 N.Y.U. L. REV. 450, 455 (1969).

nonexclusivity, they eliminate the need for inventing around—engaging in research solely to avoid collision with others' intellectual property rights—thus freeing resources from tasks that would offer only incidental social benefit. The traditional grant institution offers more natural protections against redundancy than the retrospective grant institution, for it is straightforward to select proposals that are not overlapping. We have seen, however, that it is possible to design a retrospective institution in which participants have property rights to particular areas of research.¹²¹

The retrospective grant institution also seems likely to reduce administrative costs relative to either of the alternatives. It may be easier (and thus cost less) to evaluate past achievements than to project achievements onto the future. More significantly, the retrospective grant institution greatly reduces the costs of seeking a grant, because prospective plans need be shown only in enough detail to allow the agency to prevent redundancy. Scientists presumably will write up the results of their experiments eventually with considerable care anyway, and drafting proposals may be a considerable burden.¹²² Both grant institutions may help avoid the administrative costs associated with the patent process, particularly the costs of prosecuting patents and of patent litigation. These costs will be reduced as long as grant recipients are required to place any discoveries in the public domain. Meanwhile, with appropriate protections, all of these systems can be insulated from the most blatant of rent-seeking abuses,¹²³ which are most dangerous when Congress earmarks funds for particular purposes.

IV. CONCLUSION

Grant institutions may be sufficiently established to make it unlikely that Congress would adopt the change suggested here. Let me thus break up the argument that I have offered into three distinct points, none of which needs to be taken to an extreme to improve science policymaking. First, the more that grant decisions are based on immediate past performance rather than on general reputation or on anticipation of future performance, the better. Second, there is little need for a large administrative apparatus to run even so large a project as the Human Genome Project. Just because the government sponsors research

¹²¹ See *supra* text accompanying note 108.

¹²² For an anecdotal example of how investment in grant proposals can dissipate rents that otherwise might be used productively, see Roy, *supra* note 3, at 317. In Roy's example, 2200 proposals were submitted for grants, and Roy estimates that the process diverted "nearly 200 years of scientific work" from research. *Id.*

¹²³ Some simple steps would be a requirement that interested decisionmakers recuse themselves and a ban on *ex parte* contacts.

does not mean that the government itself must engage in research. And third, decisionmaking in assessing grants may be better if there is rotation of decision-makers instead of a fixed group. Except in a project that needs a high degree of coherence because different parts must fit together, consistency of vision is more a vice than a virtue. The goal of science is innovation, and its demands are sufficiently pressing that they cannot wait for natural selection to do all the work.

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