

The Perverse Effects of Competition on Scientists' Work and Relationships

Melissa S. Anderson · Emily A. Ronning ·
Raymond De Vries · Brian C. Martinson

Received: 22 February 2006 / Accepted: 20 October 2007 / Published online: 21 November 2007
© Springer Science+Business Media B.V. 2007

Abstract Competition among scientists for funding, positions and prestige, among other things, is often seen as a salutary driving force in U.S. science. Its effects on scientists, their work and their relationships are seldom considered. Focus-group discussions with 51 mid- and early-career scientists, on which this study is based, reveal a dark side of competition in science. According to these scientists, competition contributes to strategic game-playing in science, a decline in free and open sharing of information and methods, sabotage of others' ability to use one's work, interference with peer-review processes, deformation of relationships, and careless or questionable research conduct. When competition is pervasive, such effects may jeopardize the progress, efficiency and integrity of science.

Keywords Competition · Misconduct · Research integrity · Ethics in science

When the actor Michael J. Fox was in the initial stages of creating his foundation for research on Parkinson's Disease, he came to recognize the negative impact that competition among scientific groups has on the overall progress of research on the disease. The director of one group actually said to him, "Well, if you don't help us, then, at least, don't help them" [1, p. 236]. Such was his introduction to the competitive world of U.S. science.

M. S. Anderson (✉) · E. A. Ronning
Department of Educational Policy and Administration, University of Minnesota, 330 Wulling Hall,
Minneapolis, MN 55455, USA
e-mail: mand@umn.edu

R. De Vries
University of Michigan, Ann Arbor, MI 48109, USA

B. C. Martinson
HealthPartners Research Foundation, Minneapolis, MN 55440-1524, USA

Competition is a fact of life for scientists in the U.S., and it is generally viewed in a positive light [2]. In principle, it undergirds a system of distribution of rewards that many view as superior to any alternative system. In practice, however, it can skew this system in unanticipated and perverse ways, with negative consequences for science as well as for the lives and careers of scientists.

This study examines what scientists themselves say about competition as it affects their work and relationships with others in their fields. It is based on six focus-group discussions with a total of 51 early- and mid-career scientists. These scientists had a great deal to say about the dark side of competition. Their discussions suggest clearly that the downside of competition has been underestimated and that it may have more prominent effects now than in past years. As reputation, respect and prestige are increasingly connected to resources and to success in the competitions that distribute those resources, scientists find more of their work and careers caught up in competitive arenas. The six categories of competition's effects that emerged in our analyses suggest reason for concern about the systemic incentives of the U.S. scientific enterprise and their implications for scientific integrity.

Background

Competition is sometimes confused with competitiveness. Competition is a process or condition underlying the distribution of resources and rewards. It is implicit in and between organizations as a result of individuals' and groups' common interest in resources that are scarce, or at least finite [3]. It is explicit when resources and rewards are distributed through systems designed to compare the work or ideas of candidates in a formal contest, such as a grant competition, a faculty hiring process or journal peer review. Competitiveness, on the other hand, assumes a process or condition of competition; it refers to a bidder's fitness for a contest or likelihood of winning. In a competitive environment, competitiveness is much to be desired. The analysis here, however, focuses on the prior issue of the nature and extent of competition and its effects on science.

The scientific enterprise is characterized by competition for priority, influence, prestige, faculty positions, funding, publications, and students [4]. In classic analyses of scientists' work, Robert Merton [5] and Warren O. Hagstrom [6–8] took competition as a fundamental condition and pervasive factor in science. More recently, the Institute of Medicine's report on integrity in scientific research notes this property of science: "Because science is a cumulative, interconnected, and competitive enterprise, with tensions among the various societies in which research is conducted, now more than ever researchers must balance collaboration and collegiality with competition and secrecy" [9, p. 25]. The report predicts that the role that competition plays in the scientific research community will increase with changes in the environment, notably the blurring of delineations between for-profit and not-for-profit organizations and the expanding role of industry in academic research. Increases in levels of competition in science are symptomatic of a more general hypercompetitive shift in organizations [10].

Competition in science has its bright side, which past analysts and commentators tended to emphasize and current writers often affirm. It has been credited with ensuring that ideas, work, proposals and qualifications of all interested parties are evaluated prior to the distribution of rewards, particularly funding and positions. From this perspective, competition promotes open examination and fair judgment. The norm of universalism [11] is supported when all qualified people have the opportunity to propose and defend their ideas and work in open competition [12]. After all, absent competition, cronyism is likely to flourish.

Competition has also been seen to advance innovation. Decades ago, some argued that research universities were more productive, creative and successful because of competition [13, 14]. Hagstrom [8] suggested that competition “that leads to anticipation and cases of independent multiple discovery” ensures that new “discoveries will be incorporated into the body of current scientific knowledge” (p. 15). Merton [15] noted that it provides incentives for extraordinary effort without which science might not progress as rapidly. In short, a focus on the bright side of competition has recognized its role in promoting fairness, right judgment, innovation and productivity. Hagstrom [8] typified this laudatory view over thirty years ago when he wrote, “compared to the positive functions of competition in science, the dysfunctions seem clearly less important” (p. 16).

There is a dark side, however, to competition and its effects on science. Discussions of the negative effects of competition in science often rest on anecdotal or sensational instances of personal injury: “Scholarly gossip is filled with the legend of rivalry—what senior scholar A has done most recently to competitor B, and what B is plotting by way of revenge” [16, p. 204]. There is empirical evidence, however, to show that perceptions of competition and competitive pressures have real, detrimental effects.

David Blumenthal and colleagues [17] found that university geneticists and other life scientists who perceive higher levels of competition in their fields are more likely to withhold data or results. Such withholding took the form of omitting information from a manuscript or delaying publication to protect one’s scientific lead, maintaining trade secrets, or delaying publication to protect commercial value or meet a sponsor’s requirements. John P. Walsh and Wei Hong [18] have reported similar findings.

The relationship between competition and academic misconduct is a serious concern. Empirical findings show a strong, positive relationship between the level of perceived competition in an academic department and the likelihood that departmental members will observe misconduct among their colleagues [19]. Melissa S. Anderson [20] furthermore found that a competitive departmental environment in science is positively correlated with exposure to misconduct, fears of retaliation for whistle-blowing, and conflict. It is negatively correlated with subscription to normative systems (either traditional or alternative) and sense of community. A subsequent analysis, which employed a hierarchical model to examine individual- and department-level effects, revealed that international students who are in science departments with high levels of competition are more likely to subscribe to counter-normative beliefs about scientific work (e.g., secrecy and self-interestedness) than are international students in less competitive environments [21]. A report from the

Institute of Medicine (IOM) [9] cites both organizational research indicating that “reward systems based on self-interest and commitment only to self rather than to coworkers and the organization are negatively associated with ethical conduct” (p. 58), and evidence that high levels of competition in organizations have a positive correlation with unethical behavior.

Of course, competition has always existed in science; see, e.g., Harriet Zuckerman’s *Scientific Elite*, Robert Kanigel’s *Apprentice to Genius*, or James Watson’s *The Double Helix* [22–24]. There are indications, however, that the nature of competition has changed in recent years. Goodstein [25] argues that this shift is linked to negative outcomes:

Throughout most of its history, science was constrained only by the limits of its participants’ imagination and creativity. In the past few decades, however, that state of affairs has changed dramatically. Science is now held back mainly by the number of research posts and the amount of research funds available. What had been a purely intellectual competition has become an intense struggle for scarce resources. In the long run, this change, which is permanent and irreversible, will probably have an undesirable effect on ethical behavior among scientists. Instances of scientific fraud will almost surely become more common, as will other forms of scientific misconduct (p. 31).

Analysts differ as to the reasons why competition has intensified. Some see the situation in terms of money. Tempering the effects of competition is not a prime impetus behind calls by the National Science Board [26] and by a recent coalition of 140 college presidents and other leaders [27] for more federal funding for scientific research; however, some scientists see such advocacy movements in terms of easing certain aspects of competition that are worsened by tight dollars. More money, more positions, and overall expansion of the research enterprise would improve the situation.

Another perspective sees competition as a function not just of funding, but of the balance between supply and demand of resources, particularly human resources. In the current competitive system, young scientists are pitted against one another for attractive career opportunities that are becoming increasingly scarce [28]. Researchers, feeling the pressure to be first to present findings in their fields, employ armies of graduate students and postdoctoral fellows and strive to make their laboratory groups the smartest and the fastest. The result is a “postdoc bottleneck” [29] where the supply for highly educated and trained researchers far exceeds the demand [30–33]. In concrete terms, Donald Kennedy and colleagues [34] have described the structural problem as a source of excess supply of human capital: “We’ve arranged to produce more knowledge workers than we can employ, creating a labor-excess economy that keeps labor costs down and productivity high” (p. 1105). The system produces, they claim, a “legion of the discontented” [34]. They argue that institutional and policy decisions about training scientists should be coupled to placement histories of recent graduates, numbers of intellectual offspring of faculty, and job markets for scientists. Roger L. Geiger [35] has suggested that the imbalance between supply and demand is due in part to deficiencies in graduate

education that make it ill-suited for both future professors and those beginning other kinds of scientific careers.

Finally, a third perspective suggests that structural properties of the academic research enterprise give rise to competition in ways that alignment of supply and demand could never address. Richard B. Freeman and colleagues [28] have characterized the problem as follows: "Research in the biosciences fits a tournament economic structure. A tournament offers participants the chance of winning a big prize—an independent research career, tenure, a named chair, scientific renown, awards—through competition.... It fosters intense competition by amplifying small differences in productivity into large differences in recognition and reward. Well-structured tournaments stimulate competition. Because the differences in rewards exceed the differences in output, there is a disproportionate incentive to 'win'" (p. 2293). Research environments in which only small numbers of scientists have the opportunity to gain significant attention increase the competitive stakes: playing the game may be a gamble, but the payoff for winning is significant [28, 36].

In short, there are many people (the oversupply factor) competing for prestigious, desirable and scarce rewards and resources (the funding factor), in a struggle that bestows those rewards disproportionately on those of marginally greater achievement (the tournament factor). This situation is supported to the detriment of that "legion of the discontented" and to the benefit of senior investigators, because it "generates good research by employing idealistic young graduate students and postdoctoral fellows at low cost" [26]. In other words, the benefits accrue to funding and employing institutions. This paper explores some of the costs that accompany these benefits.

Methods

The aim of this study is to analyze the effects of competition as reported by scientists themselves. The analysis is based on data from focus-group discussions held as part of a national study on integrity in science. The study, "Work Strain, Career Course and Research Integrity," was funded by a collaborative program between the federal Office of Research Integrity and the National Institutes of Health; the study was approved by the Institutional Review Boards of the HealthPartners Research Foundation and the University of Minnesota. It had two data-collection phases (focus groups and a national survey), and the present analysis is based on the focus groups.

Six focus-group sessions, involving a total of 51 early- and mid-career scientists at major research universities were held in the spring of 2002. The sampling plan for the national survey was based on data records of first-time, NIH-grant (R01) and postdoctoral (T32, F32) award recipients, and the focus-group participants were selected to reflect this population. Participants were recruited from departments in the biomedical, clinical, biological and behavioral sciences, using information available on the universities' public websites. Three of the focus groups involved mid-career scientists (associate professors), and the other three were early-career groups, including postdoctoral fellows, research fellows and assistant professors.

The members of each focus group were selected from different departments, to lower discussants' reticence to speak freely.

The design and administration of the focus-group sessions followed the protocols set forth by Richard A. Krueger and others [37, 38]. The sessions lasted between 1.5 and 2 hours. At least two project-team members were present at each session, one to facilitate the discussion and another to obtain consent signatures, tape record the session, distribute the lunches that the team provided, and otherwise support the session.

All discussions were transcribed verbatim by a professional transcription service. They were then coded independently by three investigators, who discussed and came to agreement on discrepancies, thereby enhancing the trustworthiness [39] of the data coding. This process yielded six major themes pertaining to competition, which likewise were subjected to discussion, comparison, and eventual agreement by the investigators.

The discussions were directed by a protocol of questions about scientists' work, ethical norms and rules, success and failure in science, and careers. In the discussions, the facilitator's role was to ask the protocol questions and to raise points for clarification or expansion. There were no questions explicitly about competition; however, the topic permeated the scientists' discussions of the broader issues that the protocol questions addressed. The discussions revealed their experiences with competition and its effects on their work, as well as their perspectives on competition. Analyses here are based on these aspects of the focus-group discussions.

Presented below are quotations from the transcriptions in a format that distinguishes speakers. The first speaker's contributions within a quoted exchange are noted by the numeral 1, wherever they occur, and likewise for second and subsequent speakers. The identification of speakers does not, however, carry over to subsequent quotations. The moderator's comments are so noted. The quotations have been edited only slightly, to improve readability; for example, noun–subject agreement has been imposed where needed for clarity. We have made no attempt to standardize the language of non-native English speakers, so here their own voices can be heard directly. Unrelated material has been deleted, as noted by standard ellipses within a speaker's contribution and by a full line of ellipses between different speakers. A few of the quotations presented here appeared in an earlier paper [40] that addressed different topics raised in the focus groups.

Results

The focus-group discussions covered a wide range of normative and behavioral issues in science, with attention to contextual and environmental forces affecting the everyday work of scientists. The idea of competition pervaded these discussions. Though competition is commonly understood to support positive contributions to science, participants' comments largely reveal the negative effects of competition.

This section presents participants' comments about competition in general as an aspect of their work lives, and then turns to six themes that together encompass the

focus groups' discussions about the effects of competition: strategic game-playing, decline of free and open sharing of information and methods, sabotage of others' ability to use one's work, interference with peer-review processes, deformation of relationships, and careless or questionable research conduct.

Competition in Science

As suggested above, in a context of scarce resources, competition is one means of encouraging and maintaining high levels of effort, high standards and valuable outcomes in research. Not every worthwhile research project can be supported, and competition in its best form serves a useful function in the allocation of resources to the people and projects most deserving of them in a particular instance. The focus-group data clearly indicate that scientists see competition as an inextricable part of the U.S. scientific enterprise, and most have the perception that it has increased over time. As one put it,

- 1 I think part of the problem today is it's so much more competitive than it used to be. When we were first starting out, it was more collegial. You gave reagents away freely. Now there's more at stake. There's patents at stake. There is getting yourself funded. They make it so difficult to get grant money these days. And all this stuff is coming into play. And people are more secretive. People are doing things like that more, to chop their competitors, to get a leg up on them. And it's, in a way, almost being forced to do it. Because it's just, it's too competitive. Especially if you're in a hot field. It's extremely competitive.

As noted above, scientists compete on many fronts, for grants, publications in prestigious journals, faculty or other research positions, top students and support staff, and appointments to influential positions on boards and panels, among others. Still, among our early- and mid-career respondents, it is clearly the drive for publications and grants that fosters the greatest competitive pressure. One postdoctoral fellow resorted to hyperbole: "You've got to have a billion publications in my field. That is the bottom line. That's the only thing that counts. You can fail to do everything else as long as you have lots and lots of papers." Many others talked about the pressures they feel to bring in grant money to support not only their projects but also the people who work in their laboratories. One mid-career scientist noted incredulously that her institution computes person-dollars per square foot of lab space for each principal investigator and uses this figure in merit evaluations.

Participants discussed other ways in which institutions drive up competitive pressures on scientists. Institutions seek prestige and money (which are linked, of course), as this exchange among mid-career professors illustrates:

- 1 Well, we are in a situation where the university wants to raise its rank and standing as a research institution, and so you jump at all these appropriate grant opportunities. So [administrators say], "You work on it, and you are going to do

your contribution to raising us up by meeting this grant opportunity.... Because it is going to build our prestige, our name, and we will get it.”

- 2 It is basically how much money, how much federal money you are bringing in. So the whole thing—science is business, and so it is just all money. At one level, nobody cares what you are doing.... The whole intellectual content of—the whole idea of science as intellectual inquiry—becomes very secondary.
- 3 Yeah, trivial. It’s, “How much? How big is that project?”
- 1 So it doesn’t matter what, yeah.
- 2 In addition to that, the other thing that they focus on is science as celebrity.... So the standards are, “How much did it cost, and is it in the news?” And if it didn’t cost much and if it is not in the news, but it got a lot of behind-the-scenes talk within your discipline, they don’t know that, nor do they care.
.....
- 3 And I can see a new person, like yourself, in a tenure-track appointment, and the head of the department comes knocking at your door, saying, “Hey, I have these grants, and it is wonderful! Why don’t you do it?” And you read this study and say, “This is crazy. I mean, I don’t qualify to be in the PI position!” “Oh no, but this is going to put us on the map!” And this is on top of everything else you are doing, and you have to drop the ball for everything else and do this thing for the next month.

Other participants described their institutions’ pursuit of financial support in stark terms. An early-career academic said, “We’ve changed this university from the production of science ... to the production of dollars. And the need to create dollars ... has created the subculture of science, overshadowed by the larger issue of the entrepreneurial, avaricious, crave dollars. And that’s what this university is now come about to be.” A similar discussion in another focus group led to the following exchange:

- 1 The university now is trying to become a money-making machine. And that is exemplified by our new materials and transfer agreement folks over in legal, that you just—I mean, you’re asking somebody to give you a gift of some compound. And it takes 6 months to get the agreement so they can give you something to do a very basic project. So the drive for money, I think, is really changing how people think, and that is a big example.
- 2 The money is really changing people’s personality. (*Nervous laughter around the table*)
- (*Moderator*) And you have seen this? You are all early in your careers. Have you seen this already change?
- 3 Oh, definitely.
- 1 Oh, yes. Five years ago I came here, and it is completely different.
- 3 It is a revolution. It is! I mean the philosophy is, “Follow the money. Follow the money.” So, if you fail to do that, you run into trouble. So, what do you do?

Such pressure from institutions is only partially aligned with the priorities of individual scientists. At several points in the focus-group discussions, however,

scientists expressed a different view of what truly motivates them to compete. A group of postdoctoral fellows produced the following exchange in response to a question about what scientists must do to succeed:

- 1 The pressures to publish.
- 2 And to get more grants.
- 1 Yeah.
- 2 Which go hand-in-hand.
- 3 But what did you say, the pressure to ...?
- 4 To publish.
- 2 To publish. And sometimes publish in the right journals.... In my discipline ... there's just a few journals, and if you're not in that journal, then your publication doesn't really count.
- 3 That means that there's a citation. How many people reference you.
- 5 And the quality of those citations.
-
- 4 But also to get tenure or to get promoted, you also have to have letters of peers in the community, but [who] are not from the same school as you. So hopefully, it's not only what you publish, but also what other people—what you said—the peer pressure, what other people in your own scientific community think of you.

Though this exchange among early-career researchers started with mentions of obvious competitive pressures, it led to the matter of reputation. Among a group of more experienced, mid-career academics, a discussion about competition led beyond mere reputation to opportunities for influence, as follows:

- 1 How do you define competitor? I mean, compete for what? What is the competition?
- 2 For grants.
- 3 For grants. It's probably the most important thing.
-
- 3 ... And you're competing for, um—
- 1 Prestige?
- 3 Well, ... influence in the field, which is more important. I guess that translates into prestige, but—
- 2 It's getting into the old boys' network.
- 3 Yeah.
- 2 To get a name in the field so that you can give presentations when you go to the meetings. You're not relegated to giving just poster sessions. You get respect.
- 3 So you can get your point across, is the way I look at it. I'm not after their respect, I'm after the ability to get my point across—
- 2 Well, you don't have that much—you have their respect. Unless they think that you are a ...
- 3 A wacko?
- 2 No, no, no. If they think you're a wacko, they ignore you. But unless they think that you're somewhat of an expert, that you have some credibility in this field,

you can come up with the answer to the meaning of life and nobody's going to pay diddly-squat attention to it. So you have to get your nose in the door to get them to look at you as a colleague, as you are now part of that old boys' club, and then they'll likely listen to you.

Recognition, regard and inclusion by respected people in the field—these goals drive scientists' competitive behavior, over and above the pressures that institutions and the overall scientific enterprise impose on them. It is an individual-level, personal version of institutions' drive for prestige, but it can be at odds with institutional incentives.

Effects of Competition

As noted above, six themes capture the respondents' perspectives on competition. The first four themes relate to scientists' work. The fifth has to do with competition's effects on relationships within an institution or field, and the last addresses ways in which competition may compromise the integrity of science.

Strategic Game-playing

Competition for funding, publications, scientific priority and overall career success leads scientists to describe their work in terms of strategies one might use in a game. Focus-group participants revealed that, like it or not, working within the scientific community requires artful maneuvering and strategizing. While game-playing may be a distasteful reality for some scientists, it seems to be a source of perverse pleasure for others, those who cunningly orchestrate every professional move.

Perhaps the simplest form of strategic behavior is reflected in a scientist's desire to "look good," that is, to have a good reputation. The following quotation shows how valuable a good reputation, based on actual good work, is to a scientist. It also reveals that scientists need to be careful to get all the credit that their good work deserves:

- 1 There's no substitute for being smart and for slowly taking the time and getting a reputation as a smart person, because it's one of the few things that translates quickly. When your name comes up in a study section or a journal: "Oh yeah, they're smart." Boom, I mean, you're like halfway through the door at that point.

The flip side of this attention to getting credit is an unwillingness to give others credit for their work. One respondent mentioned "people purposely not giving credit, because you don't get as far if you give other people credit." In another focus group, the issue of credit turned on whether one's reputation is improved more by greater numbers of publications or higher-caliber publications. One remarked, "I think the pressure to publish can cause people to sort of cave in and publish a lesser study in a lesser journal than a better study in a better journal, just because they need the numbers. I mean, somebody said that in their system they get evaluated on what

journals they publish in, but I don't feel that's true for me. It's just basically the numbers."

More explicit attention to game-like strategies appeared in discussions about the grants system. In all three early-career focus groups, participants expressed concern about competition's effects on scientists' willingness to align their research agendas with funding agencies' priorities, as in the following:

- 1 I was going to possibly collaborate with somebody who I think I've decided not to, at this point. We were sitting around at a meeting discussing how we were going to write this grant. And at one point in the discussion he said, "We're talking about different things. You're talking about the experiments you really want to do and I'm talking about the experiments that are going to get us the money." Now, (*laughter*) and this is just a very successful person....

- 1 But he's the tenured professor, doing really well, and I'm the soft-money assistant professor ...
- 2 He has more leverage than you do.
- 1 Well, no, I'm not sure that I meant that he had more leverage. I was thinking maybe he understands better how to play the game.
- 3 He knows how to be successful.
- 1 Right. That's exactly what I mean.

In the other early-career groups, concern turned to dismay as these scientists wondered aloud about other scientists who would portray their areas of expertise in ways that match calls for proposals. One conversation began with a participant talking about scientists who change the whole focus of their research from one field to another in order to get funded, thereby abandoning interesting problems. Others chimed in:

- 1 In the grant system, unluckily, there are waves, you know. Like, this thing is good to study for the past few years, and you have [requests for proposals], you know—that now everybody studies prostate cancer, because it's where the money is. Five years ago it was breast cancer, because it's where the money was.... There's a whole bunch of studies that could be very important—that are the basis for the others—that are not done, because you know it's not going to be funded. So it's done in some universities, some small labs, but it's not going as fast as what it could. Because people tend—I mean, you know you're not going to be funded if you do what really, you know, motivates you...
- 2 ...They're really a breast cancer researcher, but they go after the prostate money because that's where it's at. And so they don't have the theoretical knowledge and skills, but they end up going down that path.

The discussants in the mid-career groups showed greater acceptance of the gaming aspect of the grant and publication systems. One group revealed three layers of competitive strategy that they employ. The first had to do with overlapping effort on successive grants.

- 1 Oh come on, everybody that writes a grant, ... half the grant is done before you write it up, right? I mean, that's a given.
- 2 Right, exactly.
- 1 Exactly. Everybody understands that too....
- (*Moderator*) And is that, like, funded by your previous grant?
- 3 Yes.
- 1 Absolutely. So you purposeful...—I mean, you have written a grant for XYZ. X and most of the Y is done. And then you take that money and you do with it—you're doing ABC.
- 3 But you have to, because if you don't have the preliminary data, the reviewers won't even look at it. So, you know, you have to have it done before you do it.

Second, the scientists discussed the strategic benefits of taking this approach to funded research.

- 1 Well this is the best one, ... my favorite grant scenario, which has happened at least three or four times in my career. You submit the first grant, you propose the novel thing. You know damn well any study section that's even mildly conservative is going give you, "Well, it sounds promising." They might give you a good score, you hope for a good score, but it's not going to get funded, because it's too novel, it's too risky, it's too blah blah. But you already have the damn data. You know on the second resubmit, you're going to say, "Good point! We took that to heart. Oh, what a wonderful suggestion! We will worry about this too. Guess what? Here's the data!" Shove it down their throat. And then it's funded. Because, wow, you flagged them, you sucker-punched them. They said, "This is really novel, blah, blah. Boy if you could only do that, that would be a great grant." Well, you already *did* do it, and that's the point. And you basically sucker-punch the study section into giving you the money by default. They have to at that point. They don't have a choice.
- 2 That's right. Except, what if you get a different study section?
- 1 Well, then you're screwed. (*Laughs loudly*)

Finally, the discussion turned to strategic behavior in publishing. The primary speaker in the previous exchange began:

- 1 I really hate to admit this, but you do the same thing with your competitors as you do with grant agencies. You sucker-punch them. You might have—when I submit a paper, I already have the next two or three papers' worth of data. I mean, I know what I'm going to be publishing a year from now, mostly. But the paper that comes out of my lab is Part A. Parts B and C are mostly on my desk. And I've put things in part A to basically entice my competitors into making an ass out of themselves, or to second guess, or say, "Oh that must be wrong because of that, or something." You try to do those things purposely, a little bit. I mean, I don't know—
- 2 Strategic behavior.

- 1 Strategic. Yeah, this is strategy, because it is a battle. It is a game. It is all these things that you keep saying it is. And if you're going to play it, and win it—I mean, you know, read *The Art of Warfare*. I mean, it's a very helpful manual.

Such sanguinity about the thrill of competition for grants is clearly not universal among scientists. In fact, such behavior has costs in terms of careers abandoned. Some of our early-career scientists talked about colleagues who left science because, as one put it, “They just didn't want to play the game anymore.” Another early-career scientist said,

- 1 I know a large number of people in that category, in my own experience, who ... opted out because they didn't want to play. They didn't want to play the kind of games that have to be played to be successful, and in bringing in money and getting the papers out. There's so much more than just doing good science that comes into it. There's so much communication and there's salesmanship that has to go on.

Decline of Free and Open Sharing of Information and Methods

As scientists increasingly perceive that they are involved in a professional gambit, they are less likely to share information that could compromise their competitive advantage. They are likely to pause to consider the consequences of sharing whenever they are asked to provide data, materials or unpublished results to another scientist or group. On the other hand, however, scientists rely on each others' work to advance their own, so it is in everyone's best interests that free and open sharing be maintained. The challenge is to balance the competitive drive to protect one's own advantage with the competitive need to be part of an open network of scientists:

- 1 Difficult balance.... Your success is determined by your contributions to knowledge, and that means you have to share.... Personally, sometimes I am not sure how often I should be [sharing]. And I would like to be open, and that actually helps you to have good networking. But at the same time, there is always the fear—depending upon where you get training, or the context that you get trained in—I mean, if you have had experience having someone steal your ideas, you get real frightened to share anything. And then you cannot really have a good network. And it is so difficult.

One scientist noted that research done in industry is often not shared with academics, and others expressed a related concern about the U.S. federal government's interest in encouraging more privately-funded research. Most seemed to understand and accept reasonable differences in standards for sharing between academic and commercial organizations.

What bothered respondents more, however, were situations in which they expected to be able to share and found that they should not. One early-career scientist's story resonated with the others in the focus group:

- 1 I presented my dissertation at an international conference, and the topic was on ethical decision-making by nurses. And a number of famous nurse-ethicists came up to me afterwards and asked if they could have a copy of my paper, which wasn't published yet. So I took their cards and I came back. And I was in a postdoc at the time. And I talked to my postdoc group and our mentor about my experience and that they had requested the manuscript. And they said, "You're not going to send it, are you?" And I said, "These are nurse-ethicists! Why couldn't I trust that they wouldn't steal [my] paper?" And they all said, "You're a fool if you do." And I didn't, I didn't send them my paper. But I remember thinking, I was so shocked, that I couldn't trust nurse-ethicists not to steal my paper.
- 2 Well another example is when you go in for a conference. I know that when we go to present, my PI is, like, "By the time you go present at a conference, you have to have your manuscript pretty much done." When you go there, people are after your ideas.... We're a nice-sized lab, but we are not a huge lab. He says, you know, "There are other people that have huge labs. They come in and they see what you did. They send, like, five or six postdocs to do it, and they publish it before you did."
- 3 Yes.
- 4 That happens a lot.
- 5 I present, like, posters for an academy meeting. And then, like, a few months later, I saw an article, like, almost the same. Like you know, I just gave a case report. Almost the same thing. And I said, "What is going on?" So I look at the time the paper was submitted. It's almost the same time, you know? I don't like to say anything, but it was almost identical.
- 6 And you thought they were just taking pictures.

For most of the focus-group participants, competitive concerns about sharing do not focus on sharing versus not sharing; rather, they involve careful consideration of the group with whom it is appropriate to share. One scientist talked about a group of theoretical physicists who know each other well and regularly share pre-print drafts with each other, in "their little club." As he put it, "I was very surprised, because I feel that the physicists are really careful with each other. But they have this consensus about acknowledging creative ideas, but also they are very good to share those things right after they come up with it. They put it up on the net, and everybody knows about it the next day." This quotation illustrates free and open sharing, but in the context of what the speaker takes to be a broader competitive environment. When results are shared only with insiders, the insiders mutually increase their competitive advantages at outsiders' expense.

Sabotage of Others' Ability to Use One's Work

A more deliberate form of not sharing is the omission of critical details in presentations, papers and grant proposals so that others will have difficulty replicating and extending one's own research. The scientist quoted above, who

omitted information when submitting a paper for publication, was doing so to increase his success rate in the grants system. By contrast, others use it in the publication process solely to maintain their competitive bid for priority in a line of research inquiry, as a way to sabotage others' progress. A scientist in an early-career group acknowledged the need to make results reproducible by telling people "the whole recipe of the whole method" if asked directly, but then she talked about the "little trick" of not including all the details in a publication or presentation. Like the scientists in a different group quoted above, she mentioned others' practice of taking photographs of poster presentations in order then to publish the results first. She said that people, in defense, "omit tiny little details": "But sometimes in the publication, people, just to protect themselves, will not give all the details. It's always right, but maybe it's not totally complete—to protect themselves. Because your ideas get stolen constantly, and it's so competitive if you're a small lab."

A discussion in another early-career group followed a similar line. Here, though, even reproducibility was seen as compromised:

- 1 The other thing that I found is that a lot of people will not tell you their protocol, exactly the way they did it.
- 2 Real briefly.
- 1 Yeah. Real briefly, how they did it. Even [if] you talk to them on the phone and they will tell you—but they left out something, and that's critical ... So you never can repeat it.

Without being able to reproduce experiments, scientists cannot either verify findings or begin their analyses where others have left off. The former calls the integrity of research into question, and the latter introduces inefficiencies in terms of wasteful, fruitless efforts to replicate results and de facto restrictions on who can undertake subsequent work.

Interference With Peer-review Processes

Competition affects the ways in which scientists approach the peer review system. When they submit proposals for grants or manuscripts for publication, their drive for competitive advantage leads to the strategic behaviors discussed above, namely, game-playing, withholding of information and sabotage. In these ways scientists reveal that they are mindful of their competitors and their mutual struggle for priority.

The focus-group discussions showed, however, that scientists see peer review as affording a unique, even protected opportunity for competitors to take advantage of them. In this sense, competition infects the peer review process, not only through scientists' competition with other applicants, but also through scientists' distrust of the reviewers themselves, as competitors. The following exchange among mid-career discussants shows their sense of vulnerability:

- 1 I have a question. What do you think about submitting a manuscript that you have or an idea—a new idea for a grant that’s really different than what’s been out there? And that it’s out there, in the public places, in this peer-review scenario? Are you nervous about putting good ideas out there?
- 2 Very. Very.
- 3 Sure. Oh yeah.
- 1 I am too. And that means something’s wrong.
.....
- 4 It takes about ten years to figure out that you don’t even show all your preliminary data.
- 2 That’s right, exactly.
- 4 You probably leave the best things out.
- 2 That’s right. You only show them enough to get it funded. And you talk about plagiarism with the grants—I mean, that makes me nervous. I’m always wary of submitting grants to study sections, because those people who sit on the study sections, it’s not unknown for them to take your ideas, kill your grant, and then take and do it. And I think all of us have either had that happen to them or know somebody who had that happen to them.

Indeed, assumptions that improprieties occur in peer-review processes appeared to be common among discussants. What seemed particularly to frustrate them was the role of the power differential between reviewers and grant applicants in these cases.

- 1 Normally there is a professional courtesy..., but ...I have an example: somebody submitted a grant, and suddenly that guy called him to ask him about – can he give him some of his proof? And that has not been published. It’s only in the grant. So that person has to have been reading the grant to know about it. So that happens.
- 2 And the review committees have privilege to–
- 1 And the person is a big person. What do you do? You send your proof to that person.

An early-career scientist talked about fields in which a small group of people exert great influence over what gets published: “If they don’t like it, you’ll never get published. You proved them wrong? ... You better have tenure or something!” He then noted the common solution (mentioned in several focus groups) of scientists’ requesting, upon submission of a proposal or manuscript, that certain individuals not be assigned as reviewers. He went on: “But usually my experience is, a couple days later, this guy has my manuscript on his desk. He didn’t officially review it. But he can tell someone if that should be published. Unfortunately, sometimes it won’t get published.” Another hedge against the power of reviewers was expressed by a mid-career scientist:

- 1 It’s usually the people who have been around a long time, who have the names, and who sit on the study sections as far as your grant goes. You know ...

whenever you submit a grant, you always go looking to see who's on that study section first, and you try to work some of their papers somehow into your grant. Because if you don't, they come back and say, "Well, you obviously don't know the deal," meaning you didn't include *me* in your database. So, basically, you have to know who's there, and you have to try to write a little bit towards it so that they will appreciate what you're doing. Because it's an old boys' club. Very much an old boys' club. And you have [to] work hard to get into it.

One mid-career scientist told a story of how he and others in his lab counteracted an abuse of power by his mentor, a senior scientist, while he was in training. His mentor received a manuscript to review that was authored by a "quasi-competitor." It presented results of experiments similar to those that were going on in the mentor's lab. The scientist continued, "That paper ... basically would have beat us to the punch. They would have published these results before us, and they would have gotten credit, and not us. And my mentor, God bless him, sat on the paper." The mentor not only delayed writing the review but asked someone working in the lab to write it (a move of questionable ethicality in itself). That lab person and our respondent decided, in response, to stall their own work, so that their lab would not have an unfair advantage over the group who submitted the paper for review. In the end, the original group got credit for the findings, while the respondent's lab was also able to publish their slightly different findings. He ended his story with, "Sometimes you're in an awkward position, and you try to do the best thing you can under the circumstances, within your own internal ethical clock or whatever. And sometimes it's ugly and it's imperfect, but it's the only thing you can do. If we had gone to the mentor and voiced this objection, our careers would have been over. If we had approached the journal—God forbid, forget it." The speaker qualified this story by saying that it made him sound much more ethical than he actually is.

Strategies to counteract the effects of competition on the peer-review system are not always available or pursued. Some of our respondents discussed the pressures exerted on the system by scientists' need to publish frequently. As a mid-career scientist put it, "The system is crashing. I think that I myself—I have, like, five or six papers on my desk now waiting to be reviewed.... I don't think we are able to review the papers accordingly." In the face of such pressures, measures to control the effects of competition may be ineffective. As another scientist noted, editors are "dying to get some reviewers anyway, because everybody's so overloaded." As a result, he said, those who want to ensure that their work is reviewed by a particular scientist have only to cite that person several times. Such gaming of the system takes advantage of the system's overload.

Deformation of Relationships

Competition obviously affects interactions between competing scientists, but it also shapes relationships among collaborators. Our discussants talked about scientists, including themselves sometimes, whose relationships with graduate students, fellow researchers or lab employees were strained because of competition.

The most negative, most distressing effects of competition mentioned in our focus groups involved graduate students or postdoctoral fellows. One scientist freely admitted that he exploits students in order to get work done in his lab:

- 1 I mean, there's lots of dead bodies, out of my lab anyway. Ethically, this is a very tough thing to learn, a brutal thing to learn. People have sayings that graduate students are reagents and things like this, and they half joke about it. But, ethically, if you're a teacher and a researcher, it's part of your job to teach people and to nurture young lives and all that stuff. And that's a very high ethical standard that I take very, very seriously. But on the other hand, I also take the success of my lab very seriously. And there's going to be dead bodies out of my lab. There are going to be kids that aren't going to make it. I know they're not going to make it, but I'm going to lie to them. I'm going to say, "Well, you might get a Ph.D." And I know that their chances are probably one in three. But—
- 2 So why do you bring them into your lab, if they're not going—
- 1 For whatever reasons. I might need that work, those pair of hands at that particular point and time or something like this.

There were stories as well about Ph.D. advisors who publish their student's dissertations without giving the students credit. One told about a prize-winning scientist "who will take two postdoctoral fellows, after their Ph.D.'s, put them on exactly the same project, and one person gets the paper, period, and the other one gets zero." Another researcher told about a "very famous scientist" who had a reputation for keeping his best people in his lab: "If he liked you—if you were really good—he wrote you a lousy letter of recommendation so you would stay in his lab forever. If you got a good recommendation from this guy, you don't want to hire this person, because he really wanted to get rid of them." A mid-career discussant told about her own postdoc supervisor: "When I left my postdoc, I was told, 'Don't compete with me. You won't win.' And, you know, it was a given that you wouldn't—you wouldn't win."

Peer collaborators are also not immune to the effects of competition—that is, when such collaborators exist: one mid-career scientist described solitary researchers who turn off their office lights and close their doors so that no one will bother them. In a different focus group, a mid-career scientist talked about collaboration:

- 1 Well the route to success doesn't encourage collegiality so much. I mean there's more, these days, about interdisciplinary work and all that sort of stuff. But from a pure stereotypical way, at this kind of an institution, it's to build your own silo and be the star and have all these people under you, working for you, and be the star.

When collaboration does occur, it may succeed up to the point of allocating credit for interesting findings. An early-career scientist related her story:

- 1 I moved and I started some work. And the principal investigator says, "This is your paper. You are doing this work, so you are the first author." Okay, we get

some good data, and it looks interesting. Then somebody else wants to be first author. And then the person claims, "Oh, I did most of the writing." And different people have different claims you know. And somebody can grab that paper and write and rewrite and rewrite and rewrite, and keep it, you know, for six months or one year, just to be able to make some claim that they did a lot of work on it.

In another case, a collaboration fell apart when the team realized that they had important results. The high stakes doomed the joint project: "And literally, on the day something was found, it just started to crumble. And ... people just don't speak to each other anymore, or trying to block publications, just sort of a mess."

Finally, competition infects relationships across levels of academic rank. Discussants talked about difficulties related to assignment of authorship that arise when someone of a lower rank feels pressured to yield the coveted first-authorship designation to another of higher rank. Some in an early-career group said that, as students or postdocs, they avoided working with assistant professors, who were particularly eager to be first authors. Another told about a patent application which was based on her work but which appeared under the names of more-senior scientists. She took the following lesson from the experience: "If you still want the application to go ahead, you don't want to cause trouble or make it difficult, la la la. So you wait until it's—you're the PI, and then you can do the same thing as they did."

Laboratory technicians and others with specific, technical knowledge are also caught up in the competitive environment. The following exchange in an early-career group suggests how these people try to maintain their competitive value:

- 1 Within a group, they could be in charge of particular jobs, like a particular setup of instrument and so on. And because they need to keep their job, they kind of monopolize that. They will teach as little of it as possible, because they have to be expert, *the* expert in the group as far as that instrument is concerned.

- 2 There's really not anybody else there that knows what to do apart from myself. And, you know, I've been asked to train other people... You never totally want to give 100%, because then you are making someone else more equal and you're making a competitor for yourself.

Such expertise is highly prized. A mid-career focus group revealed what scientists do to ensure that their own labs get the right people to maintain the lab's competitive edge. The exchange gives an indication of how competition shapes scientists' behavior in general.

- 1 This is why I never advertise. Usually I get people that I know, some colleagues recommend. So I actually—rarely—I don't have, like, an interview. I just call somebody, say "Do you have somebody good?" And they say, "Yes." They send them to me. So this is how I work, I hardly advertise.
- 2 That's what the old boy network is. That—

- 3 We aren't allowed to do that. That's unethical. Can't do that. It's illegal.
- 1 Is it though?
- 3 Yes!
- 4 You have to make believe that you at least interviewed three or four people or—
- 1 For the postdoc position?
- 3 For all positions in our department, we must have an open marketplace.
- 5 Really?
- 3 Sure.
- 5 That's asinine. What if somebody calls you up, "I'm about to finish my Ph.D...."
- 3 You're not allowed to just give them the job. We can't do it. It's against the law.
- 5 That's asinine.
-
- 4 What you do is, you tailor the job description so it fits only that person. We interviewed five or six people, but only that person fit the job description that we wrote up. That's how you get around this.
- 5 The person has to be five-foot-two high, because the lab is small...
- 4 Just about, just about.

Careless or Questionable Research Conduct

The previous exchange indicates behavior that some would call ethically questionable. Focus-group participants linked other questionable or careless behaviors to the competitive environment in which they work. One mid-career scientist expressed wariness about postdoctoral fellows, because "dishonesty occurs more with the postdoc. Because they want to get the data—whereas if they don't publish, they don't move on. And they, I think, are more likely to sort of fudge a little bit here and there if they need to get the data done. Unless, like you say, you watch them." Another mid-career researcher similarly talked about the pressures facing more-junior colleagues:

- 1 It is the pressure of the tenure process. This pressure to publish or perish, which in some cases might lead to cutting some corners, because the pressure is real and you actually are going to perish. And so, you know, there is a lot of pressure for people to come out with things in a very short time-frame. The likelihood that corners are cut, is real.... And that is precisely for the people who have the least experience. That is a funny thing, you know: the less experience you have, the more pressure you get, in terms of producing all these things that are supposed to astonish everybody else.

Indeed, one of our early-career discussants expressed exactly this connection between pressure and misbehavior when he said, "If you need one more grant to solidify your rank and tenure package, you may violate your personal integrity." Another early-career scientist attributed such problems to universities' emphasis on productivity:

- 1 I think the other thing is that what's driving a lot of this, you know, sort of dishonesty or, you know, edging around things, is the expectations of the universities of productivity. You know, numbers mean everything at [my institution]. You know, the quality of your work—they don't even read your papers. In my field, all they care about is how many papers you publish per year. They don't care whether it's an excellent study or a study that people don't really believe.

Some of the questionable behaviors mentioned by discussants are derived from the need noted above to “look good”:

- 1 Yeah, but you've always gotta put your best foot forward. So there's where the temptation comes in to exaggerate how good you are, or how good your work is, or not talk about the failed experiment, or whatever.
- 2 Right.
- 1 So sometimes you can make yourself look better on a poster that's only going to be shown once. Even though you wouldn't dare fabricate data for a paper, you can still maybe make yourself look more impressive in a temporary situation, in the things that you say to people.

This exchange among early-career scientists shows that the nature of the public forum has a bearing on what scientists are willing to do to promote their own work. Another early-career discussant explained that published graphs and photos need to represent all the data, but that for visual displays (a gel or a Western blot, for example), one generally selects the best to show; she concluded, “and that's a bend.” Others described situations that went farther, beyond a “bend” of the rules:

- 1 But there's, I think there is a question of how you interpret the data, even ... if the experiments are very well designed. And, in terms of advice—not that I'm going to say that it's shocking—but one of my mentors, whom I very much respect as a scientist—I think he's extraordinarily good—advised me to always put the most positive spin you can on your data. And if you try to present, like, present your data objectively, like in a job seminar, you're guaranteed to *not* get the job.
- 2 Sure. You're expected to be an advocate for your work once it's done.
- 1 You are. And you know what the problems are in doing the experiments. And if you, in your mind, think that there should be one more control—because you know this stuff better than anybody else because you're doing it, you know—you decided not to do that, not to bring up what the potential difficulties are, you have a better chance of getting that paper published. But it's—I don't think it's the right thing to do.

Competition is also manifested in scientists' pressured haste, leading to carelessness, which can verge on questionable behavior. One discussant talked about scientists “cutting a little corner” in order to get a paper out before others or to get a larger grant, and another said that she once published a result that she got three times in one week but could not replicate the following week, just because her

chair told her she needed more publications that year. An early-career researcher said,

- 1 You also get a hit-and-run type of strategy ... where people go for the quick paper. You can do—you know, in [my field]—you can do some really neat stuff pretty fast, so you can make a nice observation. But then, you don't stick around to fill out the details and follow it up all the way through, because the next big fad is somewhere else. And it seems like some whole labs operate that way. They never follow anything in depth. They ... score one big one after another.

Another was talking about doing an experiment with one set of controls and then deciding whether or not to do it again with a different set, to back up the result:

- 1 You're not going to invest a lot of time in that second series, because that would take you another 3 months. That means it's another 6 months or longer before your material is published. And by that time, some bigger lab's going to come along and publish your stuff, and you get nothing, even though—because you're being more careful. There's a fine line with actually having enough ... data to support your idea, and then going that extra half-meter to really send it home. You don't have that sort of time, because if you don't get it published in a timely fashion, someone else will—without that data.

Discussion

The results presented here suggest that competition among researchers has pronounced effects on the way science is done. It affects the progress of science through secrecy and sabotage and interferes with peer review and other universalistic merit-review systems. It twists relationships within a field and can increase the likelihood of a scientist engaging in misconduct. None of the focus-group participants made reference to positive effects of competition on their work, despite the fact that the focus-group questions dealt in a general way with scientists' work and the norms of conduct that govern that work. If the protocol questions had asked explicitly about competition, doubtless there would have been some discussion about the positive aspects of science. In the context of the general questions, though, the scientists referred to competition as a constant and negative force that interferes with the way science is done. It is disconcerting to ponder the consequences of competition, such as mistrust and defensive posturing, for a community that has long been committed—in principle—to shared ideas and collegiality.

Over the past 30 years, academic science in the United States has come to resemble more closely its counterpart in the corporate sector. During this period of time, science has increasingly come to reflect the assumptions, tools, methods and products of modern-day market capitalism, a political and economic system driven by competition. Reasonable thinkers who extol the benefits and virtues of market

capitalism still recognize, however, that markets are not perfect; markets sometimes fail. Many of the troubling aspects of competition identified by the scientists who participated in this study may well be related to aspects of market failures in how the scientific enterprise currently operates, as evidenced by imbalances in human resource supply and demand.

It is widely believed that competition in science leads to optimal innovation and discovery, but it has recently been observed that current levels of competition may well be damaging to innovation, and that the system itself reflects structures and processes that stem more from unguided, evolutionary forces than from rational planning [41]. The system is also highly inefficient, in that scientists invest many years in advanced training and then face high probabilities of failure in attaining secure research positions to match their training [29, 42, 43]. T.V. Rajan [44] has recently characterized biomedical science, in particular, as a pyramid scheme, with an expanding base of newcomers providing inexpensive, highly skilled labor for a much smaller number of scientists at the top of the hierarchy. High levels of competition and low success rates for acceptance of papers at conferences and journals, for funding of research proposals by federal agencies, and for acceptance into prestigious academic positions ensure that much effort will be rewarded by failure.

Faced with this bleak view of the dynamics and environment of science, researchers respond with self-protective and self-promoting behaviors. If these behaviors were aligned with the progress of science and with the public trust that is embodied in public funding of research and universities, competition would prove salutary. Our findings suggest that it is not.

To date, scant attention has been paid to the connection between research integrity and competitive pressures in science. Findings presented here signal a need for analysis of how heightened competition, either directly or through its effects on fear or perceptions of injustice, compromises the integrity of the research enterprise [45]. The present analysis suggests that those who fund, manage and regulate the enterprise have underestimated the extent to which competitive pressures on scientists induce behaviors that can only be described as perverse, counter-normative and counter-productive. This connection suggests a need for greater attention to preparation for professional survival in the competitive environment without compromise of ethical standards. Competition's bright side as a driving force behind U.S. dominance in scientific thought and innovation needs to be viewed along with its dark side. When competition is chosen reflexively as the default sorting mechanism, scant attention is paid to unintended consequences that run counter to norms like trust, sharing, collaboration and the public good. Over-reliance on competition must be tempered by a realization of the harm that unfettered competition can produce.

It is difficult to envision a solution to this dilemma, given the inexorable demands of a system in which competition has been activated as the driving force [46]. In considering what would improve the situation, Kennedy [16] concludes, "Surely a less competitive academic universe would help.... Because originality and priority hold such high reputational value, it is difficult to see how the situation is going to change very much. Thus the best hope is for a set of standards—cultural norms—

that recognize that even in a highly competitive environment departures from fairness simply cannot be tolerated” (p. 208). We suggest, however, that the fundamental premises of a system whose incentives, intentional or not, lead to natural and rational choices by individuals in the system that then compromise the integrity of the system must be subjected to intense scrutiny.

Acknowledgements This research was supported by the Research on Research Integrity Program, a collaborative program between the Office of Research Integrity and the National Institutes of Health, grant #R01-NR08090. Raymond De Vries’ work was also supported by grant #K01-AT000054-01 (NIH, National Center for Complementary and Alternative Medicine).

References

1. Fox, M. (2002). *Lucky man*. New York: Hyperion.
2. Feller, I. (1996). The determinants of research competitiveness among universities In: A. H. Teich (Ed.), *Competitiveness in academic research* (pp. 35–72). Washington: American Association for the Advancement of Science.
3. Pfeffer, J. (1992). *Managing with power: Politics and influence in organizations*. Boston: Harvard Business School Press.
4. Bok, D. (2003). *Universities in the marketplace: The commercialization of higher education*. Princeton: Princeton University Press.
5. Merton, R. K. (1957). Priorities in scientific discovery. *American Sociological Review*, 22, 635–659.
6. Hagstrom, W. O. (1965). *The scientific community*. New York: Basic Books.
7. Hagstrom, W. O. (1970). Factors related to the use of different modes of publishing research in four scientific fields In: C. E. Nelson & D. K. Pollack (Eds.), *Communication among scientists and engineers*. Lexington: Heath Lexington Books .
8. Hagstrom, W. O. (1974). Competition in science. *American Sociological Review*, 39, 1–18.
9. Institute of Medicine, National Research Council. (2002). *Integrity in scientific research: Creating an environment that promotes responsible conduct*. Washington: National Academy of Sciences.
10. Thomas, L. G. III (1996). The two faces of competition: Dynamic resourcefulness and the hyper-competitive shift. *Organization Science*, 7, 221–242.
11. Merton, R. K. (1942). Science and technology in a democratic order. *Journal of Legal and Political Sociology*, 1, 115–126.
12. Ziman, J. (2000). *Real science: What it is and what it means*. Cambridge: Cambridge University Press.
13. Ben-David, J. (1960). Scientific productivity and academic organization in nineteenth-century medicine. *American Sociological Review*, 25, 828–843.
14. Ben-David, J., & Zloczower, A. (1962). Universities and academic systems in modern societies. *European Journal of Sociology*, 3, 45–84.
15. Merton, R. K. (1968). Behavior patterns of scientists. *American Scientist*, 58, 1–23.
16. Kennedy, D. (1997). *Academic duty*. Cambridge: Harvard University Press.
17. Blumenthal, D., Campbell, E. G., Gokhale, M., Yucel, R., Clarridge, B., Hilgartner, S., & Holtzman, N. A. (2006). Data withholding in genetics and other life sciences: Prevalences and predictors. *Academic Medicine*, 81, 137–145.
18. Walsh, J. P., & Hong, W. (2003). Secrecy is increasing in step with competition. *Nature*, 422, 801–802.
19. Louis, K. S., Anderson, M. S., & Rosenberg, L. (1995). Academic misconduct and values: The department’s influence. *The Review of Higher Education*, 18, 393–422.
20. Anderson, M. S. (1996). Misconduct and departmental context: Evidence from the Acadia Institute’s graduate education project. *Journal of Information Ethics*, 5, 15–33.
21. Anderson, M. S. (2000). Normative orientations of university faculty and doctoral students. *Science and Engineering Ethics*, 7, 487–503.
22. Zuckerman, H. (1977). *Scientific elite: Nobel laureates in the United States*. New York: Free Press.
23. Kanigel, R. (1986). *Apprentice to genius: The making of a scientific dynasty*. New York: Macmillan.

24. Watson, J. D. (1997). *The double helix: A personal account of the discovery of the structure of DNA*. London: Weidenfeld & Nicolson.
25. Goodstein, D. (2002). Scientific misconduct. *Academe*, 88, 28–31.
26. Field, K. (2004). U.S. is said to produce too few scientists. *The Chronicle of Higher Education*, 50, A28.
27. Wasley, P. (2006). College presidents join ad campaign to increase spending on scientific research and education. *The Chronicle of Higher Education* <http://chronicle.com/daily/2006/02/2006020804n.htm>.
28. Freeman, R., Weinstein, E., Marincola, E., Rosenbaum, J., & Solomon, F. (2001). Competition and careers in biosciences. *Science*, 294, 2293–2294.
29. Russo, E. (2003). Victims of success. *Nature*, 422, 354–355.
30. Juliano, R. L. (2003). A shortage of Ph.D.s? *Science*, 301, 763.
31. Kaiser, J. (2005). NIH funding: Success rates squeezed as budget growth slows. *Science*, 307, 1023.
32. Butz, W. P., Bloom, G. A., Gross, M. E., Kelly, T. K., Kofner, A., & Rippen, H. E. (2003). Is there a shortage of scientists and engineers? How would we know? RAND Issue Paper—Science and Technology. Santa Monica, CA, pp. 1–7.
33. Teitelbaum, M. S. (2003). Do we need more scientists? *The Public Interest*, 153, 40–53.
34. Kennedy, D., Austin, J., Urquhart, K., & Taylor, C. (2004). Supply without demand. *Science*, 303, 1105.
35. Geiger, R. (1997). Doctoral education: The short-term crisis vs. long-term challenge. *The Review of Higher Education*, 20, 239–251.
36. Juliano, R. L., & Oxford, G. S. (2001). Critical issues in PhD training for biomedical scientists. *Academic Medicine*, 76, 1005–1012.
37. Krueger, R. A. (2000). *Focus groups: A practical guide for applied research, third edition*. Thousand Oaks: Sage Publications.
38. Morgan, D. L., & Krueger, R. A. (1998). *The focus group Kit, vol. 1–6*. Thousand Oaks: Sage Publications.
39. Lincoln, Y., & Guba, E. (1985). *Naturalistic inquiry*. Newbury Park: Sage Publications.
40. De Vries, R., Anderson, M. S., & Martinson, B. C. (2006). Normal misbehavior: Scientists talk about the ethics of research. *Journal of Empirical Research on Human Research Ethics*, 1, 43–50.
41. Cech, T. R. (2005). Fostering innovation and discovery in biomedical research. *The Journal of the American Medical Association*, 294, 1390–1393.
42. Committee on Bridges to Independence. (2005). *Bridges to independence: Fostering the independence of new investigators in biomedical research*. Washington: National Research Council.
43. Goldman, C. A., & Massy, W. F. (2001). *The PhD factory: Training and employment of science and engineering doctorates in the United States*. Bolton, MA: Anker Publishing.
44. Rajan, T. V. (2005). Biomedical scientists are engaged in a pyramid scheme. *The Chronicle of Higher Education*, 51, B16.
45. Martinson, B. C., Anderson, M. S., De Vries, R. (2006). Scientists' perceptions of organizational justice and self-reported misbehaviors. *Journal of Empirical Research on Human Research Ethics*, 1, 51–66.
46. Lloyd, A. L. (1995). Computing bouts of the prisoner's dilemma. *Scientific American*, 272, 110–113.