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Managing the Business of Science

This editorial picks up on a theme introduced in my editorial from October 2006, titled "NIH Funding of the Independent Investigator" (1). Thus, like the previous one, this editorial deals with science policy and mainly pertains to the United States. I apologize for being somewhat provincial, but the issues may resonate with some readers outside the USA.

I am concerned about two major and interrelated issues: *1*) the money available to perform investigator-initiated research and *2*) the management of resources at both universities and funding agencies such as the National Institutes of Health (NIH).

When I received my first NIH grant in around 1981, the payline (i.e., the percentile at or below which funding is assured) was in the mid-30s, and every faculty member in the department had a grant. Moreover, the Dean of the Yale School of Medicine (Robert W. Berliner) provided my department chair (Emile L. Boulpaep) with a sufficiently large budget that Emile could set aside funds for the occasional renovation of laboratories and hiring of new faculty. As an aside, I might point out that the NIH's funding of Big Science was modest, and the NIH used nearly all of the available money to fund applications in the order of their ranking by a panel ("study section") of senior investigators with considerable experience running successful laboratories. As a further aside, Bob gave Emile and the other chairs considerable freedom in developing programs . . . and aside from the occasional "center" (e.g., the Cancer Center), initiatives were departmentally based.

Much has changed. When I became chair at Yale (1989–1998), NIH paylines had fallen to the low-20s, and a few faculty members were temporarily between grants. By the end of my term as chair, the out-of-order funding of grant applications by the NIH based on programmatic need was becoming more common, and the move toward Big Science was in full swing. The Dean had taken tighter control of the departmental budget, so that I had to pre-negotiate renovations and new hires, for which the Dean paid directly, leaving little, if any, room for entrepreneurship. Even in the early 90s, the Dean retained "stars" by creating centers and other non-departmental structures.

Nationwide, the above trends have continued. At the NIH, the euphoria of the doubling of the NIH budget (between 1998 and 2003) gave way to harsh reality: The President and Congress—with other priorities—reduced (or reversed) the annual increases in the NIH budget, and, adjusted for inflation, we have returned to where we were in 2001–2002. To make matters worse, Congress and senior NIH administrators have steadily reduced the fraction of NIH dollars allocated to investigator-initiated grants (R01/R29), while relatively increasing support for other programs, including Big Science. Moreover, the number of applications for R01/R29 grants has increased substantially since 1998 (and has skyrocketed for non-R01/R29 grants)—a trend that is certainly not the fault of the NIH. Thus current R01 paylines, depending on the NIH institute in question, are in the range of 10–20 percentile (see http://www.aecom.yu.edu/ogs/NIHInfo/paylines.htm)—but too often the former. As a result, many previously successful principal investigators (PIs), are now permanently between grants. At many medical schools, another continuing trend is the growth of trendy interdisciplinary programs—at the expense of departments and their inherent entrepreneurial spirit.

Now, before I go on, I should pause to emphasize that I am not a disgruntled scientist. The NIH and my NIH program officers have been wonderful to me, and my deans have been highly supportive. However, I am concerned about the above trends because I believe that they will gradually stifle the independent investigator—the scientific entrepreneur who constantly pushes the boundaries, and in the process makes unexpected observations, the exploration of which is responsible for almost all breakthroughs in biomedical science. Moreover, the above trends will serve as a powerful disincentive for the best and the brightest to enter our field. Nevertheless, I am the eternal optimist, and I believe that we can right the ship if we return to our roots in the 1950s and 1960s. I suggest that we:

***1*) Increase and then stabilize future growth in NIH funding.** After making the sizeable investment in training and career development, society simply cannot afford to allow a productive scientist to fail in mid-career. One approach would be to de-politicize the funding mechanism by creating a national trust fund—perhaps by placing a tiny tax (e.g., 1%) on all expenditures on health care, from tooth paste to open-heart surgery.

***2*) Limit the funding of non-investigator-initiated projects until R01/R29 paylines reach the 25th percentile.**

***3*) Limit the number of investigators eligible to apply for R01 grants.** If we only implement *suggestion 1* above, institutions will respond by hiring more faculty, which will eventually lead to another cycle of low paylines and career failure. Somehow, we must limit the applicant pool by pre-selecting individuals of high promise. One approach would be to restrict R01 applications to those who either are in a tenure track (i.e., who have received demonstrable institutional commitment) or have previously received a competitive transitional grant (e.g., K01). I would not restrict the size of R01 grants or the number that one investigator may hold . . . we must reward success and invest in the successful.

***4*) Restrict the out-of-order funding of grant applications.** The National Institute of Allergy and Infectious Diseases, for example, has a payline of 12 percentile but a success rate of 24 percentile . . . half the grants are funded out of order by administrators rather than on the basis of peer review. On the other hand, the National Institute of Diabetes and Digestive and Kidney Diseases has a payline of 17 percentile and a success rate of 21 percentile—figures that I regard as more reasonable. If we could achieve *suggestions 1*, *2*, and *3*, the success rate would reach the 30s, and then the near elimination of out-of-order funding would raise the paylines to the 30th percentile—where nearly all fine science should be funded.

***5*) Stifle the urge of administrators to determine the direction of science.** Sometimes bureaucratic oversight can be a powerful and positive tool—as in mobilization of resources for studying the human genome or a nudge from the dean to ensure that department chairs play nicely together in the sandbox. At other times, however, interventions can be a disaster. Playing trends is easy, but dangerous. No one—not even the investigators making the discoveries, and certainly not senior NIH administrators or deans—knows where the next major discovery will come from. If we could predict

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it, it would not be major. Long ago, certain large countries with a penchant for a top-down style of management, developed a series of "Five-Year Plans" that led to the deaths of millions of their citizens and the stagnation of their economies. Biomedical administrators seem not to have learned from history the dangers of a top-down, Soviet-style of management.At my previous home, a decade ago, the administration decided to start a microbiology department (which they had closed two decades earlier) to chase the AIDs bandwagon. I remember shaking my head and saying that I could predict who would receive the Nobel Prize for AIDS, and it was not going to be anyone we could recruit. I was right. By the way, the mistake was closing the microbiology department in the first place.

6) **Restrict interdisciplinary initiatives to reside within disciplinary departments.** It is true that boundaries between disciplines can be fuzzy—and are invisible to some deans (my dean, Pamela Davis, who is an accomplished scientist, is a refreshing exception). However, I believe that we need professionals to guard our disciplines. Biochemistry, pharmacology, cell biology, and physiology all have something unique to offer . . . and we cannot have high-quality interdisciplinary research without disciplines. Otherwise, the literature will be littered with amateurs. ■

References

1. Boron WF. NIH funding of the independent investigator. *Physiology* 21: 300–301, 2006.