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It's Not The Money, Stupid!

This guest editorial is by Allen J. Bard, a chemistry professor and director of the Center for Electrochemistry at the University of Texas, Austin.

THE CULTURE of academic research has shifted over the past 50 years from research evaluation based on teaching, creativity, and productivity to one based simply on the amount of money (often now called “resources”) raised. A number of factors have played a role in this change: the “business model” for universities, an increased willingness to accept greed as a virtue in our society and as a measure of success, and a desire for an easy “objective” measure of something that is otherwise difficult to quantify.

As a result, we have reached the point where faculty members are judged more by the amount of research funds they have raised, primarily from government agencies, than by the accomplishments that flow from the funding. Obtaining high levels of funding is considered not only desirable, but absolutely necessary, and I've been party to tenure discussions that have centered on this (for example, on the need for “scoring two major grants”) rather than on the quality of work.

It is possible to rationalize this attitude by saying that funds raised are a measure of how one is evaluated by one's peers. The fact is, however, that the final decision to fund really comes from project officers who have often become remote from the frontiers of research and often fall prey to the fad of the month. It is also true that the best grant-swingers are those who are willing shamelessly to hype their research and their field—truth and modesty be damned.

The result of this cultural shift, as we have heard over and over from colleagues, is that one spends 70% of one's working time writing proposals and seeking funding. Thus, not only do we operate under a model where highly trained scientists are almost immediately removed from direct hands-on research upon arriving at a faculty position, but now are also largely removed even from close research supervision of students.

As the system develops, the probability of being funded on any given project gets smaller and smaller, so one must keep

writing and sending in proposals that have to be processed and evaluated by a growing number of project officers, but, alas, by a fixed number of peers. The agencies, forever seeking more funding from the government, also keep inventing an alphabet soup of new programs. These come with an increasing bureaucratic burden of accepting the funds, thus guaranteeing an ever-increasing time commitment by investigators.

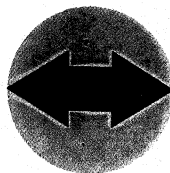
A more recent and potentially even more damaging trend is a growing expectation by universities that faculty should help fund operations not only through overhead from research grants, but also through the generation of intellectual property (IP). Faculty is heavily encouraged to generate patents and find partners to license them, or, even better, to nucleate new start-up companies.

To advance this agenda, universities are hiring highly paid administrators—with visions of Warfarin and Gatorade dancing in their heads—to head technology commercialization efforts. These are intended not only to generate additional funds for the university, but also to demonstrate to the public the “economic value” of the university to society. These kinds of activities have even been described as “critical to the mission of a university.” While such considerations have not yet become a major influence on promotion and tenure decisions, one has reason to fear for the future.

No wonder we have problems with attracting good young people to careers in academic science despite large outreach efforts. If working closely with students and doing long-term fundamental research is not the goal and money is the important thing, there are more lucrative professions than academic ones for them to pursue. I also fear that the perceived importance of money in science has led to a public backlash on issues like climate change and chemical toxicity, with the feeling that scientists are pushing these areas to get funding and not necessarily because they believe all that they report. This indeed is tragic.

Allen J. Bard

Views expressed on this page are those of the author and not necessarily those of ACS.



POINT / COUNTERPOINT

Academic Chemical Research

Divergent opinions on **WHAT SHOULD MOTIVATE CHEMISTS** in today's research universities



SHUTTERSTOCK/C&EN

C&EN PUBLISHED a guest editorial entitled "It's Not the Money, Stupid!" by University of Texas chemistry professor **Allen J. Bard** in its Oct. 11 issue. The editorial decried what Bard considers the undue influence of money and the creation of intellectual property on the culture of academic research in chemistry.

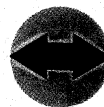
The editorial clearly hit a nerve. C&EN received a number of letters, both pro and con, addressing the issues that Bard raised. C&EN also received three unsolicited guest editorials from prominent chemists addressing the *raison d'être* of basic research in chemistry departments at major universities. Two of those editorials, one by University of Utah chemistry professors

Glenn D. Prestwich and **Charles A. Wight** and one by University of Texas chemical engineering professor **Adam Heller**, take issue with what they consider Bard's overly idealistic view of unfettered, curiosity-driven basic research. Another, by University of Wisconsin chemistry professor **Howard E. Zimmerman**, agrees with Bard's view and expands on it.

This conversation is an important one, and C&EN decided to stretch its "Point/Counterpoint" format to accommodate the viewpoints of Prestwich and Wight, Zimmerman, and Heller. What follows is a reprint of Bard's original guest editorial, three points, and a final comment from Bard.

BARD'S EDITORIAL

IT'S NOT THE MONEY, STUPID!



The culture of academic research has shifted over the past 50 years from research evaluation based on teaching, creativity, and productivity to one based simply on the amount of money (often now called "resources") raised. A number of factors have played a role in this change: the "business model" for universities, an increased willingness to accept greed as a virtue in our society and as a measure of success, and

a desire for an easy “objective” measure of something that is otherwise difficult to quantify.

As a result, we have reached the point where faculty members are judged more by the amount of research funds they have raised, primarily from government agencies, than by the accomplishments that flow from the funding. Obtaining high levels of funding is considered not only desirable, but absolutely necessary, and I’ve been party to tenure discussions that have centered on this (for example, on the need for “scoring two major grants”) rather than on the quality of work.

It is possible to rationalize this attitude by saying that funds raised are a measure of how one is evaluated by one’s peers. The fact is, however, that the final decision to fund really comes from project officers who have often become remote from the frontiers of research and often fall prey to the fad of the month. It is also true that the best grant-swingers are those who are willing shamelessly to hype their research and their field—truth and modesty be damned.

The result of this cultural shift, as we have heard over and over from colleagues, is that one spends 70% of one’s working time writing proposals and seeking funding. Thus, not only do we operate under a model where highly trained scientists are almost immediately removed from direct hands-on research upon arriving at a faculty position, but now are also largely removed even from close research supervision of students.

As the system develops, the probability of being funded on any given project gets smaller and smaller, so one must keep writing and sending in proposals that have to be processed and evaluated by a growing number of project officers, but, alas, a fixed number of peers. The agencies, forever seeking more funding from the government, also keep inventing an alphabet soup of new programs. These come with an increasing bureaucratic burden of accepting the funds, thus guaranteeing an ever-increasing time commitment by investigators.

A more recent and potentially even more damaging trend is a growing expectation by universities that faculty should help fund

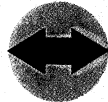
Innovation requires invention plus implementation, and academic science is critical to both.

operations not only through overhead from research grants, but also through the generation of intellectual property (IP). Faculty is heavily encouraged to generate patents and find partners to license them, or, even better, to nucleate new start-up companies.

To advance this agenda, universities are hiring highly paid administrators—with visions of Warfarin and Gatorade dancing in their heads—to head technology commercialization efforts. These are intended not only to generate additional funds for the university, but also to demonstrate to the public the “economic value” of the university to society. These kinds of activities have even been described as “critical to the mission of a university.” While such considerations have not yet become a major influence on promotion and tenure decisions, one has reason to fear for the future.

No wonder we have problems with attracting good young people to careers in academic science despite large outreach efforts. If working closely with students and doing long-term fundamental research is not the goal and money is the important thing, there are more lucrative professions than academic ones for them to pursue. I also fear that the perceived importance of money in science has led to a public backlash on issues like climate change and chemical toxicity, with the feeling that scientists are pushing these areas to get funding and not necessarily because they believe all that they report. This indeed is tragic.

PRESTWICH & WIGHT’S POINT



Bard characterizes a shift in the academic culture over the past 50 years from “teaching, creativity, and productivity to one based simply on ... money.” While the time-consuming and often frustrating quest for

research funding is a concern, the growing interest of universities in entrepreneurial activities is neither tragic nor greedy. Rather, it is a hopeful sign that universities have begun to recognize their true mission—to educate, motivate, and innovate. In the traditional model, faculty earn the respect of their peers by publishing in top journals, by obtaining research funding, and by winning awards. In the evolving new model, faculty will earn the respect of everyone when their scholarship has a positive impact on other people.

Faculty, whether at public or private institutions, are public servants. If we provide a clear connection between taxpayer dollars and the research they fund, the public will realize the value that science delivers. To this end, a new generation of faculty entrepreneurs has emerged: We identify and solve real-world problems, translate basic research into applied technology, and create products as well as publications. We understand the importance of market pull, and we have learned the business of science. Rather than teaching only to the 5% who will become professors and physicians, we provide real-world skills to the 95% who will go on to become politicians, business owners, writers, and company scientists.

In a recent commentary entitled “The Bayh-Dole Act Turns 30,” in *Science Translational Medicine* (DOI: 10.1126/scitranslmed.3001481), Vicki Loise and Ashley J. Stevens document how the Bayh-Dole Act of 1980, which allows universities to claim title to inventions made with federal funding, has rejuvenated the U.S. economic system. Moving technologies from academic labs to companies transformed our economy from a manufacturing base to an innovation base. This is what makes the evolving model so exciting: Innovation requires invention plus implementation, and academic science is critical to both. The world needs science graduates and educated consumers like never before.

Leading academic economists report that only 6% of faculty members are frequent inventors, and two-thirds never disclose at all. No shift from basic to applied research has occurred despite a 10-fold

I ... fear that the perceived importance of money in science has led to a public backlash on issues like climate change and chemical toxicity.

increase in disclosures. Importantly, faculty members with commercially funded research published more, and were more involved with administrative activities, than those without. In addition, professors who started companies raised more money to support their academic labs and published more five years pre-start-up and five years post-start-up than those without start-ups. Faculty entrepreneurs frequently report that participating in commercialization expands their research portfolios and that understanding deliverables brings tighter focus to each research area.

Most important, the motivation of entrepreneurial faculty is not greed. Life sciences and engineering professors who patented a lot did so because they were passionate in their conviction that their work would improve people's lives. A start-up company is neither a trivial undertaking nor the path to riches; it is hard work and not to be embarked on casually. Faculty entrepreneurs deserve commendation, not denigration.

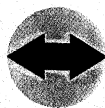
Universities spend more on technology commercialization than they receive in income from this activity. Barely 16% earn enough to cover costs; this is not a good business model for adding to the funding base. It's expensive, long term, and high risk. So no, it's not about the money. Recipients of public research funds have an obligation to bring the benefits of science back to the public.

Academic science is at a tipping point. We are evolving from a culture that the public sees as irrelevant, to an open culture based on impact as the unifying metric. We now recognize that no technology reaches the public unless it is commercialized. Translational research consists of basic research to create new knowledge plus its implementation to address real-world needs. Faculty pursuit of this goal is truly a scholarly activity. Participation in translational and transdisciplinary team activities engages and excites students in a way no lecture or lab course possibly could—we call this translational teaching. The ultimate deliverable of entrepreneurial scholars is translational service. Through recognition that science makes life better and creates jobs, we serve the public. This is how we help the public appreciate and admire chemists.

No, it's not about the money. It's about impact.

It is critical to emphasize the original NSF mandate to support basic research rather than considering immediate utility.

ZIMMERMAN'S POINT



I first had contact with the National Science Foundation in 1956. I was a new assistant professor at Northwestern University, having just arrived from a postdoc with R. B. Woodward. I knew that NSF's mandate was to support basic research.

Thus, I applied to NSF for my first research grant. It was promptly rejected. I received the rejection letter from Walter R. Kirner, then NSF's Chemistry Division

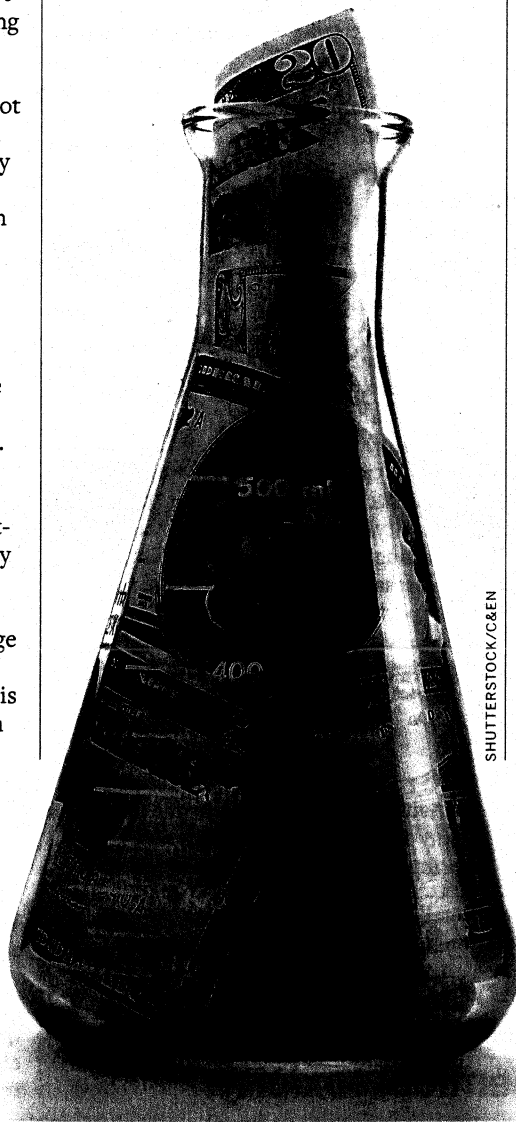
director. It stated, "It is well known that enols are the intermediates in the conversion of cis to trans isomers. Thus how can it be suggested that ketonization of enols will preferentially afford the less stable stereoisomeric ketones." But this was the work for which I obtained tenure at Northwestern and a paradigm for which I am known.

I finally obtained my first NSF grant in 1960 when I moved to the University of Wisconsin, Madison. For almost 46 years, I was able to get NSF support for my basic research. With that help I was able to do research on my favorite projects—the stereochemistry of enolate ketonization (which had not fared well earlier), the Möbius-Hückel concept for pericyclic reactions and molecules, the correct mechanism of the Birch reduction, the understanding and prediction of photochemical reactions on the basis of excited-state structure and computations, the Traxler transition state stereochemistry for aldolization, the di- π -methane rearrangement, the synthesis of barrelene and its photochemical conversion to semibullvalene with its rapid valence tautomerism, the delta-density matrix method of predicting organic reactivity, and a variety of other things.

My approach as a basic researcher had been to select problems that promised to be difficult to understand but were important to advancing knowledge of chemical transformations. For a long time there was still no pressure to stress items that were of immediate consequence; that is, of the type noted 10 years ago by the late F. Albert Cotton of Texas A&M University in his C&EN editorial "Endangered Basic Research" (Dec. 4, 2000, page 5). But times change.

Because of the current emphasis on nano-, materials-, bio-, polymer-, collaborative-, societal-, social benefits, impact, educational benefits, and more, it is critical for NSF to, nevertheless, continue to support basic research.

Many think the best of the Chemistry Division directors was Kenneth G. Hancock, who was one of my 87 former students to go into academia and get tenure. In his editorial, Bard notes that project officers "have often become remote from



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the frontiers of research." This is a polite circumlocution that also applies to many Chemistry Division directors. I hope that the new director, Matthew S. Platz, will continue with Hancock's philosophy and buffer basic research from omnipresent financial pressures.

It is critical to emphasize the original NSF mandate to support basic research rather than considering immediate utility as a criterion. The results of basic research, said to be curiosity-inspired, are invariably found to permeate and be the foundation of virtually all applied research. It might be termed the rate-limiting step.

One of my friends commented that this type of discussion is unlikely to be stated publicly by anyone expecting continued research support. In my own situation I can afford to state these views, since I now support my own research.

HELLER'S POINT



Unlike Bard, I consider the pursuance of patent-protected, people-serving products and processes by professors not as a sad sign of our times, but as a reawakening in the proud history of academic chemistry and chemical engineering.

Adolf Baeyer of the University of Munich synthesized indigo in 1885, winning the 1905 Nobel Prize in Chemistry for "the advancement of organic chemistry and the chemical industry, through his work on organic dyes and hydroaromatic compounds." Fritz Haber, while at the University of Karlsruhe, codeveloped with Carl Bosch of BASF the high-pressure catalytic process to synthesize ammonia from nitrogen and hydrogen, for which Haber won the 1918 Nobel Prize. Karl Ziegler, working at the Max Planck Institute for Coal Research, and Giulio Natta of the Polytechnic University of Milan won the 1963 Nobel Prize "for their discoveries in the field of the chemistry and technology of high polymers."

The 2005 Nobel Prize was awarded to Yves Chauvin of the French Petroleum Institute, Robert H. Grubbs of California Institute of Technology, and Richard R. Schrock of Massachusetts Institute of Technology "for the development of the metathesis method in organic synthesis." Chauvin developed the nickel-based gasoline and isooctene production Dimersol processes and the titanium-based Alphabutol process for ethylene dimerization

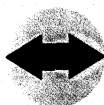
There is excellence in uncovering new truths, and there is excellence in the creation of people-serving products and processes.

to 1-butane. In his view, which was also Michael Faraday's view, "There is no difference between fundamental research and applied research."

I share this view. Academic excellence exists in all human endeavors. There is excellence in the propagation of knowledge through teaching, there is excellence in uncovering new truths, and there is excellence in the creation of people-serving products and processes.

Intellectual property, whether academic or corporate, is of essence because in its absence, funds required for the realization of products and processes are just not committed. Unlike in the times of Faraday, Baeyer, and Haber, when secrets were kept, the academic environment of today is open. Metathesis could not have become an industrial reality if it were not protected by patents. Grubbs was issued 77 metathesis-related U.S. patents; Schrock, 12; and Chauvin, five.

BARD'S RESPONSE



I'm happy that my editorial has promoted needed discussion of the issues raised there. I might mention that in addition to the responses published here, I received over 20 messages strongly supporting the views expressed.

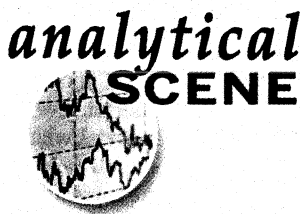
My colleague Adam Heller misses the

point of my editorial. Nowhere did I take sides on the fundamental versus applied research question, nor did I question the desirability of faculty getting patents. (I have over 25 myself, some of which even made money!) The issue is whether this should become a key factor (along with overhead raised) in evaluating faculty and whether universities should become venture capital organizations.

I respect the views of Prestwich and Wight and the belief that with the "evolving new model, faculty will earn the respect of everyone." I hope that this respect extends to the 94% who are not frequent inventors. It's also hard for me to believe that in tough economic times universities are willing to run money-losing "expensive, long term, and high risk" technology commercialization efforts simply to benefit the public. Most governing boards that encourage this are sold on the possibility of the big one that will pay off for the university.

I don't believe that we ever had "a culture [of academic science] that the public sees as irrelevant." Even now surveys show that the public holds scientists in high esteem and are smart enough to understand that curiosity-driven research sometimes leads to great new things. If the "tipping point" tips over too far—encouraging only short-term returns—we will have lost the future. ■

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