Cellular and Molecular Biology™ **51**, 815-820
DOI 10.1170/T690

ISSN 1165-158X
2005 Cell. Mol. Biol.™

REVITALIZING SCIENCE IN A RISK-AVERSE CULTURE: REFLECTIONS ON THE SYNDROME AND PRESCRIPTIONS FOR ITS CURE

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Received March 4, 2005; Accepted June 3, 2005; Published December 16, 2005

Abstract - This paper considers problems with the scientific culture and granting systems, the most important of which is an aversion to risk. Grant awards tend to be "safe" rather than bold. This discourages the fresh approaches that may bring important breakthroughs. The paper then suggests remedies that could restore the scientific enterprise to one that is friendlier to fresh thinking.

Key words: Grants, reviewer conservatism, risk taking, scientific culture, remedies

PREFACE

The thoughts contained herein arise in part from my experience as a frequent dissenter from prevailing orthodoxy, and in part from my experiences attending workshops convened to address problems with granting systems. Inevitably, such experiences generate ideas. In this case, they have brought modest insights into how granting systems might better serve transformative approaches that challenge the *status quo*. At present, such approaches have little chance of success. Yet they are the very ones that could bring spectacular advances.

Here, I outline the problems as I see them with today's system of doing science, and their etiology. I also suggest remedies that could enhance scientists' natural proclivity to seek the truth. Some of these thoughts have been passed on to the funding agencies in the context of campaigns designed to make the peer-review system more responsive to highly innovative, "out of the box" approaches. Others are new.

HAS THE SCIENTIFIC ENTERPRISE GONE AWAY?

A half-century ago, breakthroughs were fairly common events that could be counted on to occur from time to time on an unpredictable but not infrequent basis. Pioneering such breakthroughs were scientific heroes – legendary figures such as Linus Pauling, Jonas Salk, Richard Feynman, James Watson, Francis Crick, and others, names familiar even to lay people.

But things have changed. While the past 30 years have brought a great outpouring of scientific results, breakthroughs are less common. Modern equivalents of Pauling, Salk, and Watson-Crick are not easy to identify. Considering the massive investment in science today, why is it that scientific heroes have become so scarce? Why so few conceptual breakthroughs? I refer to *realized* breakthroughs such as the biochemical nature of heredity or the polio vaccine, not incipient breakthroughs whose realization seems always just around the corner. Can you name more than a handful of realized breakthroughs that have come during the past three decades?

Some argue that this settling down is all but inevitable. After all, science today is far more complicated than it has been, often requiring teams of investigators and large groups to pursue effectively. Others argue that there is simply not much more to be discovered – that the breakthroughs have had their heyday and we need content ourselves with merely filling in the gaps. Thus, breakthroughs might not be expected to occur on an everyday basis.

Perhaps some of this is true – but a significant role may also be played by another factor: the growing aversion to risk taking. Although funding agencies have much to be proud of for past achievements, it is broadly perceived that they have become less agile in dealing with proposals that dissent from orthodoxy. Challengers of the *status quo*

rarely succeed in today's scientific climate. Hence, those approaches most apt to generate conceptual breakthroughs are throttled before they can emerge from the scientific womb.

The funding agencies worldwide are aware of this problem. Several agencies have held recent workshops to deal with the issue, and some measures have been taken over and above existing remedial programs. In the US, for example, the term "high risk" now permeates review guidelines. And, both the National Science Foundation and the National Institutes of Health have established special programs to encourage novel approaches.

These institutional responses acknowledge the problem. Yet, it is broadly felt that the responses are nominal. Few dissenters from orthodoxy report any more success than before. The reviewers are largely the same, and have not abruptly changed their well-honed views. Admonishing them to be "less conservative" comes with no guarantee that they will be. Thus, effective action has yet to be taken.

I am not alone in this view. A recent, highly praised book by Donald Braben, entitled *Pioneering Research: A Risk Worth Taking* (Wiley, 2004), concurs. Braben argues that limiting the ability of scientists to dissent from orthodoxy heralds a line of consequences leading ultimately to societal doom. Only a radical departure from the highly bureaucratic, top-down-managed approach to science, he argues, will solve the problem. In the absence of such a departure, progress will remain incremental, despite a vast pool of talent and an abundant pool of monetary resources. Freedom to dissent is essential.

Thus, the scientific enterprise appears to have gone at least somewhat astray. It has for sure generated a massive number of hugely productive enterprises whose outpouring of results seems to come ever closer to generating the hoped-for conceptual breakthroughs. But those breakthroughs don't always materialize; often, it seems, their realization remains just around the corner. A reason for this mixed track record is that the very approaches that could lead to breakthroughs – those that challenge the current line of thinking with fresh alternatives – have virtually no chance of advancing in today's scientific enterprise. This is a serious impediment.

The remainder of this piece is divided into two sections. First I offer my view of the underlying cause(s) of the problem. I consider both the grant system and the scientific culture. In the second section I suggest possible remedies for these problems, based on my experience with the review process and my experience dealing with proposals that dissent from orthodoxy.

SOURCE OF THE PROBLEM

If the situation described above seems to you to be as antithetical to science as it does to me, how could it have arisen? I consider two sources: the scientific culture and the grant system.

A. The scientific culture

Scientists since the time of the ancient Greeks have been dedicated to the pursuit of truth, and the same dedication applies today. We want to find out how the world really works. On the other hand, in response to societal demands for finding cures to diseases and developing technologies to compete effectively in a world growing ever more complex, science has grown from the small cottage industry it once was, into a big business. It consumes money and generates products. Whether the business-like culture spawned by this transformation has brought complicating features that might compromise the noble goal of science is an issue that needs to be considered.

Formerly, science was a modest endeavor. Principals were largely known to one another, and they enjoyed support either from a benefactor, or from having been born into a family of wealth. Scientific pursuit was largely unfettered by the exigencies of everyday life, because it was practiced by a talented and fortunate few.

Today, science has grown to a massive enterprise, not unlike a big business. The business is supported by tax-paying investors, who channel their investments through government-granting organizations. In return, the business generates useful products. These products are mainly conceptual frameworks describing how the universe operates. They are belief systems – theories and hypotheses emerging out of available evidence. Thus, money is invested and belief systems are generated. The presumption is that such belief systems will eventually be useful for advancing technologies, curing afflictions, accruing national prestige, etc.

Investors are patient. Especially with the promise of incipient breakthroughs, the public is remarkably willing to continue its investment, recollecting the incredible scientific and medical breakthroughs of the past and anticipating even more in the future. At least for now, it is axiomatic that scientific research will enjoy continuing (albeit fluctuating levels of) support. Investors will not pull the plug.

It is this implicit confidence that may be the first reason for the existing problem. The scenario is akin to a business that does not need to compete. Ordinary businesses must innovate to beat the competition, but the scientific business suffers little such concern, for so long as the promise of incipient breakthroughs continues to be as well advertised as it is now, the public will continue to invest.

Safe and secure, the scientific endeavor presses on. Truth seeking certainly remains high on its agenda – but who is checking to see what fresh and insightful truths have

been uncovered? Which investor will demand a list of last year's conceptual breakthroughs? Such breakthroughs are anyway presumed to be beyond the investors' ability to understand, so why even bother. Scientists have become largely insulated from public scrutiny, hidden within the confines of the huge business complex that is today's science. Breakthroughs are welcome, but certainly not demanded or even expected. Making incremental progress is sufficient.

To illustrate the point, consider the expectations implicit in the typical government-grant proposal. Within many grant systems, applicants are obliged to project annual milestones: What will be accomplished by the end of year 01? Year 02? Even, year 05. Such requirements amount to implicit admission that no breakthroughs are to be anticipated, for any unexpected finding will immediately change the direction of the research and render earlier projections irrelevant. Scientific society has institutionalized its expectation that even modest breakthroughs will not happen, and this expectation is passed onto its scientists.

If not satisfying the expectations of its patrons, what then motivates today's scientists? Is it the pure, noble and unfettered goal of seeking truth, or do extraneous factors come into play? My take is that the most pressing motivational factor comes not from outside the system, but from within: the competition to survive.

To survive, all scientists need funding. The pressure is especially acute when one's salary is at risk, and it is also acute in larger laboratories, where long-term staff akin to family must be supported uninterruptedly. Host universities turn up the pressure further. They count on the indirect costs that come with grants to help pay their bills, and remind you of this when promotion and tenure considerations come around. Thus, obtaining funding has become not just a means of paying for the costs of experimentation, but a source of relentless pressure in a fiercely competitive arena, which rarely abates. By no means has the motivation to seek for truth been abandoned; but Darwinian survival must evidently be an additional motivator of some significance.

How does one ensure survival in so competitive an arena?

Obviously, attention needs to be paid to those determining your fate, and high among those are your grant reviewers. Impressing them is important. Being productive, motivated, and highly professional at all times will go a long way. But the peers reviewing your application are also your competitors, seeking to fortify their own positions, and proposals that threaten to undercut those positions are not likely to be embraced with warmth. So, one must be ever cautious. The safest bet is to avoid even hints of any dissent from orthodoxy. Keep it safe, and survive. Most everyone knows it.

In short, the culture has deflected scientists from their singularly noble goal of pursuing truth. Just keep it safe, and get your funding. We have evolved into a culture of obedient sycophants, bowing politely to the high priests of orthodoxy.

As you might imagine, this culture generates some unwelcome side effects. Among them are the following:

– *Truth plays a subsidiary role*: In determining which belief systems prevail, survival-related issues may play a dominant role. Truth may not necessarily be the singular factor.

– *Crowd power*: Those on review boards have commanding power, which can be subject to group reinforcement. "Yes, this unorthodox proposal contains brilliant ideas, but, unfortunately…." Colleagues around the review table are relieved, for any threat to the prevailing belief system impacts them, too. The reviewer is applauded for his/her critical insight, and the establishment is sustained.

– *Narrowness*: Science is broadly conceived as a growing tree of knowledge: The trunk, limbs, and major branches are thought solid and well defined; it is only the most peripheral twigs that remain to be elaborated – and that is the task of today's scientists. Few seem to be paying attention to whether the foundational limbs are really solid. We are all too narrowly preoccupied dealing with the explosion of information to step back from details to rethink the fundamentals. We have defaulted into becoming a culture of believers.

– *Aggression toward interlopers*: The inner voice repeats: "My colleagues have bestowed ample funding upon me; they seem to think my ideas are all right. Therefore, probably my ideas *are* all right. *Ergo,* challengers are likely to be cranks bent on making unnecessary trouble." Challengers are too often arrogantly ridiculed. Even scientists unfamiliar with the challengers' work seem to take pleasure in demeaning it – many colleagues report this experience.

– *Misplaced values*: With facts growing at an exponential rate, the growing density of peripheral twigs seems more and more to obscure the core paradigms. From the outside, science looks like a thicket of complexity. The notion of complexity is reinforced by the culture: grant programs provide ample support for large-scale computer models thought to be necessary to make sense out of the seemingly impenetrable minutiae. But, what happened to Occam's Razor? The time-honored approach of replacing complicated belief systems with simpler belief systems that explain more facts, has given way to a resignation that science is too complicated for any one person to comprehend. A sense of futility pervades – a sense that formidable problems can no longer be solved through bold approaches and fresh ideas.

In sum, the culture of science is beset with a number of

problems. It has produced scientists whose drive to survive can sometimes overshadow their drive to pursue fundamental advances. It accepts growing complexity as a given, and thereby offers limited incentive for scientists to find simplifying truths; those few scientists reckless enough to stray onto paths less traveled are marginalized by the majority of scientists, who are deeply and unquestioningly acculturated in their fields' orthodoxy. Dwelling within this orthodoxy, by contrast, promotes ample rewards. Understandably, this is a culture that is not likely to produce much in the way of breakthrough science.

B. The Grant System – Perpetuator of Orthodoxy

Shortly after most government-grant systems were created a half-century ago, Thomas Kuhn published his now-classic book entitled "The Structure of Scientific Revolutions," arguing that science advances less by accretion of knowledge than by a series of scientific revolutions. These revolutions, as we well know, were referred to as "paradigm shifts." Often, normal science is completely overthrown by the work of some creative scientist who comes upon a finding so completely out of accord with the prevailing view that the latter becomes untenable. Such paradigm shifts, Kuhn argues and many though not all scientists agree, make for the real advances in science.

Most grant systems were not designed to deal with proposals aimed at promulgating paradigm shifts. Because they were created before Kuhn's ideas became broadly known, the systems were implicitly designed around the earlier common-sense principle of accretion of knowledge: Add flesh to the bones of the existing framework, and eventually the universe will be better understood. As knowledge grows, so will understanding. From such a perspective, peer review makes good sense and has produced much new knowledge – for who better than peers can judge whether a scientific proposal seems sound?

For applications that dissent from orthodoxy, however, the merit of this system is less clear. A fresh idea commonly challenges the *status quo*. Sent to those for whom the *status quo* is a central belief system, the review outcome is more-or-less predictable: Self-interest will commonly block the application. I do not propose to demean the value of experts, or of knowledge over ignorance, only of the *de facto* power conferred on those who have risen to prominence within today's establishment to dictate the direction of scientific inquiry. Perhaps understandably, the establishment will deflect challenges, retain convenient assumptions, and snuff out proposals that appear too threatening.

Compare this review system with that of a court. The "court" is set up here to adjudicate the fate of a grant application. A plaintiff makes the case. The case is against the defendant (establishment) – who then proceeds to act as judge. The party making judgment is the very party who stands to lose most if the application prevails. Imagine a court system in which your suit against a drug company is to be judged by that drug company. It happens, for sure – but is this the fairest mechanism to judge the merit of the plaintiff's case?

It is difficult to escape the conclusion that the peer review system virtually guarantees that the most far reaching of potential scientific advances will not be supported. Genuine challenges of the *status quo* have little chance, and indeed, scientists broadly acknowledge this situation; submitting proposals that challenge orthodox thinking are considered to be suicidal.

…

In sum, the scientific endeavor is beset with problems originating from both the culture and the grant system. One reinforces the other. The result is a proclivity to favor what is safe, and to shun what seems "risky." Einstein's challenge of orthodoxy would probably fail in today's grant system, as would Galileo's – not to mention Watson and Crick, whose coffin of failure would likely be sealed by the absence of requisite background in nucleic acid biochemistry and by the absence of preliminary data. Would they be worth the risk?

It is the focus on safe science that in my estimate is responsible the dearth of conceptual breakthroughs and the dearth of real (not promised) solutions to medical problems. Left unchecked, such systems all but guarantee that the future will not be any more fruitful than the past.

TOWARD A SOLUTION

The proposed solution has three components: i) a program designed to support credible approaches that challenge the prevailing orthodoxy; ii) a program designed to launch promising schools of thought into the mainstream; and iii) a program designed to free the scientific culture from the yoke of excessive conservatism.

i) A New grant-award system for ideas that challenge conventional views

I propose a new scheme that works in parallel with existing schemes. To work effectively, the power of experts to quash dissenting or challenging proposals in their field should be curtailed. Inadequate attention to this issue may well have been what compromised previous attempts to promote transformative, paradigm-shifting proposals. This is not an argument against knowledge, but against impediments to change. Experts need not be eliminated from the review process, but the system design *must* ensure that their self-interest does not dominate.

Experience suggests several additional features: short proposals that focus on background, rationale and potential significance, rather than on details of proposed

experimental protocols; evaluation by open-minded "generalists" who have no stake in the outcome, much as in the judicial system; evaluation criteria based not on details of protocols, but on three factors: the magnitude of the question – how much will the earth shake if validated?; the evidence that it has a chance of being validated; and whether the applicant has the determination to pull it off. Finally, paradigm-shifting proposals should compete against one another, and not against standard proposals. This levels the playing field.

ii) Bootstrapping promising schools of thought

While program outlined above is designed to support individuals who challenge the *status quo* with potentially better alternatives, something more is needed to support evolving schools of thought – ways of thinking that have gathered strength and now pose a tangible challenge to orthodox thinking. I am referring to so-called "minority" views.

Many of us are aware of minority views in our respective fields. Fewer of us have taken the time to evaluate whether they are as flaky and ill conceived as the establishment would make them out to be, or whether they might actually be supported by solid evidence. Cogent minority views do not necessarily rise to the top like the cream in the coffee. Even if they have gained a following, alternative schools of thought are rarely welcomed in today's competitive scientific environment. They are most commonly shunned – either left to wither on the vine for want of financial sustenance, or hammered so mercilessly by the orthodox establishment that the perpetrators are often left dazed and beaten. Even the most far-reaching of these alternative schools of thought commonly languish in obscurity.

For me, this is the single most significant obstacle to scientific progress. It virtually precludes Kuhnian paradigm shifts.

Mechanisms are needed to propel the strongest of these alternative schools of thought into mainstream consideration, so that they can compete side-by-side with the prevailing schools of thought. Competition is healthy. It can sharpen of both points of view, setting the stage for the emergence of a winner. If the winner is the prevailing school of thought, so be it; if the winner is the challenger, then, what might follow are bold advances beyond anyone's imagination.

An effective way of bootstrapping the most promising of these alternatives is to offer them programmatic support. For example, suppose an alternative view of cancer etiology has gained support from a substantial body of evidence, and has accrued some following. Its potential seems evident. While one might naively think that such potential would be recognized and supported, the reality is that it most likely will not: competing enterprises will work assiduously to quash what is perceived as "competition." An effective way of bootstrapping these emerging alternatives is to open them to broader experimental investigation, so that real momentum can be developed. This can be achieved by setting aside pools of money for supporting *multiple* grants on selected schools of thought.

Programs to set aside pools of money for promising areas are hardly radical; granting agencies do it all the time. Here, I propose similar programs, except that selection is made by scientists in consultation with grant administrators, not by grant administrators in consultation with scientists. I propose a competition in which proponents of alternative schools of thought argue their case against the prevailing orthodoxy in front of panels of impartial scientists. Only those judged to be the most far reaching are selected. Once such a program is set up, it would be opened to any scientists who might wish to apply.

With such a program in place, promising challenge paradigms would be quickly elevated to competitive status. Challenge and prevailing paradigms would become rivals, debated on equal footing. Both sides' arguments would be sharpened by the ensuing debate, with a winner emerging in reasonably short order. This could perhaps be the single most effective program to give breakthrough science a head of steam.

iii) Removing the yoke of conservatism from the scientific enterprise

Programs (i) and (ii) deal with the practicalities of launching promising alternatives. It does not deal with the scientific culture, which has become conservative.

At present, members of the scientific community are obliged to profess adherence to some widely accepted frame of interpretation in order to prosper. The burden of forced acquiescence has become a habit. For many, it has evolved into a natural way of life. Young scientists must learn that the ultimate objective is not to get funded (although that is obviously important), but to pursue truth even if it means challenging some prevailing orthodoxy.

Through training grants, as well as research grants that come with an obligatory training component, the granting agencies could leverage educational programs designed to reinforce students' natural sense of curiosity.

While specific educational mandates are not a tradition of most granting agencies, they are a consistent feature at the US National Science Foundation, and could be easily implemented elsewhere. Training grants could mandate programs designed to promote cultural change. Such programs might include the following: Student-presented seminars designed to critically explore how conclusive is the evidence underlying some broadly accepted paradigm; invited seminars on paradigm-shifting topics; courses on paradigm-shifting topics; a required course on the history / philosophy of science; and, workshops designed to stage debates between competing paradigms.

By targeting such educational programs primarily to incipient scientists, the cultural change would be imparted to the next generation, where the likelihood of taking hold is highest. It is the young who will ultimately carry the flag of innovation. Young scientists need to learn something of the purity and value of truth seeking, and this is best achieved through educational programs designed expressly for this purpose.

CONCLUSION

Scientific progress has dwindled during recent years because of cultural and structural reasons. In order to survive at present, scientists must tow the line; they must implicitly profess allegiance to a prevailing belief system, lest their supply line get cut. This pressure has bred a culture of conservatism, where out-of-the-box ideas that challenge currently accepted frameworks are unwelcome. Likewise, the grant system has been unable to cope with such proposals. Those proposals with the highest potential for creating breakthrough science seem to have the lowest chance to succeed.

If the resulting scientific paralysis is to disappear, measures will need to be taken to deal with cultural and structural impediments. This will not be a band-aid fix. Addressing the issue of paradigm-shifting grants alone without addressing the underlying cultural issues will not work effectively. The suggestions outlined above address all of these issues. I believe they have the capacity to restore science to the truth-seeking venture it once was, a restoration that cannot help but generate breakthroughs of unimaginable power and significance.

By adopting measures such as these, the granting agencies have an opportunity to restore their vaunted roles as the drivers of cutting-edge science. But they need to proceed with caution. Previous programs designed to bolster proposals that dissent from orthodoxy have not been as successful as hoped, and if any one is to succeed, inadvertent design flaws need to be avoided. I believe that this is best achieved through cooperative effort between grant-system administrators and those scientists who have been impacted most by system conservatism. These scientists know the obstacles very well. They stand to gain the most from a grant system that works effectively, and are therefore motivated to ensure that any such design is successful. Absent their input, I predict that future remedial measures may be as unsuccessful as those of the past.