



Do We Really Value What Our Faculty Do?

Our academic promotion process is out of alignment with the faculty jobs of today

BY PAGE S. MORAHAN, PhD, and JANET FLEETWOOD, PhD

Since 1990, when the idea of “scholarship” was broadened in *Scholarship Reconsidered*¹ to include the pivotal concepts of scholarship of application, discovery, and integration, individual medical schools have initiated numerous efforts to evaluate the clinical and educational activities and scholarship of medical school faculty. We assert that medical schools have focused on changing policies and procedures for promotion and tenure, and that we now need to focus on *changing our deep-rooted academic cultural belief system*.

The entrenched promotion process persists, despite the fact that academic medical centers are increasingly being asked to focus on what Donald Schon called “in the swamp problems”—those messy, confusing problems that defy logical technical solutions.²

There is a growing divide between what the academic promotion system rewards and what faculty and medical schools are now being asked to do, reflecting a deeply ingrained and unquestioned value system that underpins the academic promotion system. In our view, this value system is obsolete for today’s academic medical centers. The new challenges require greater understanding of the science of interdisciplinarity, collaboration, and engaged scholarship.³

Challenging Entrenched Assumptions

What are some of the unquestioned assumptions about what is valued in the academic promotion process? How are they outdated? What are the unintended consequences? What is being done to change them?

❖ *National/international reputation as an independent investigator is essential for academic promotion.* The unintended consequence of this deeply ingrained belief is that junior faculty focus on narrow fields in which they can be primary authors and garner numerous publications. Faculty members must choose—either focus

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narrowly to accumulate publications for promotion, or focus broadly and risk perishing due to lack of sufficient, timely publications.

- ❖ *Deep expertise in one discipline or subdiscipline is more valued than multiple expertise of moderate depth.* This belief leads to the unintended consequence of faculty not risking changing fields, such as from basic science or clinical research to medical education, or even from one area of clinical or basic science to another.
- ❖ *Primary author (or last, senior author) and principal investigator are the important, valued roles in publications and grants.* Middle author and co-investigator slots are less valued. The unintended consequence is greater difficulty in achieving successful scientific collaborations, because contributions such as expert statistical experimental design and data analysis are not valued sufficiently, even though they are essential to success of the research.
- ❖ *Disciplinary research efforts are of higher quality than collaborative interdisciplinary efforts.* This belief is fostered by the difficulty that traditional disciplinary-based study sections have had in evaluating truly

interdisciplinary and transdisciplinary proposals, such as those for clinical translational studies.

- ❖ *Hard-science research designs, such as those epitomized by randomized clinical trials, are more rigorous than social or educational research.* A related unquestioned assumption is that descriptive studies are less rigorous than hypothesis-driven studies, although these are essential at the beginning of a field.
- ❖ *Investigator-designed research is more rigorous and objective than is engaged scholarship or participatory-based research.* The first unquestioned assumption is that the experience and perspective of the trained investigator can be completely independent from the research being performed. The second is that the investigator knows best what and how to investigate, and participant involvement contaminates study quality.
- ❖ *Publications in traditional peer-reviewed journals are of higher quality, and have more impact, than other scholarship.* This assumption has several flaws. First, citation impact calculations are flawed in that they are biased toward large-circulation journals, and thus do not accurately measure the impact that specialty journals may have on that particular field. Second, citation impact calculations do not measure many important types of impact, such as educating patients to be more effective in their disease prevention behaviors and more educated about health care needs. Finally, is traditional peer review really better than editorial group decision about publications, whether print, e-publication, wikis, or blogs—especially where readers can respond? How are we incorporating the new scholarship distribution methods in the promotion process, and assuring that the scholarship meets the three

“P’s”—peer review, public, and providing a platform for others to build on?⁴

- ❖ *Discovery research is more creative than applied, outcomes-based, or educational research.* This unquestioned assumption comes from overvaluing whatever is “new” and “innovative.” NIH, NSF, and other funders also promulgate this unquestioned assumption; they look for and are more likely to fund the “novel” and “innovative” rather than “systematic” and “long-term implementation” research required to establish if a procedure or intervention is really effective.
- ❖ *The amount, source, and duration of grants are measures of academic success.* Academic institutions have lost their way in using big federal grants as a marker of individual academic success, confusing institutional advancement with advancement of science. The ingrained belief is that big grants are more valuable than

small (even if fields differ in funding needed or available); federal money is worth more than private foundation or industry funding; and renewed funding more than funding for a single conclusive project.

How have these beliefs helped academic medical centers reach their goals of quality and responsiveness to societal needs? How may these beliefs now be hindering these goals? What experiments can we design for change, and then assess if that change warrants being systematized within a medical school, university, scientific discipline, or the country?

Let us remember that we are all scholars and academicians, and use our scientific process to surface these unwarranted beliefs and to question the embedded assumptions and unintended consequences. Only by examining our presuppositions can we hope to respond rationally to the questions we pose while creating a promotion and tenure

system where what we truly value is what we actually reward. ❖

References

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