The field of outcomes research is devoted to a multidisciplinary effort to understand patient, provider, and health system characteristics associated with clinically important patient outcomes. Moreover, the field has a commitment to learning what works in medicine and supporting interventions to improve care and outcomes. Tools used by the outcomes research community have included the creation of performance measures and appropriate use criteria to accelerate translation of guidelines into practice. There has been great effort in the creation of registries that can provide feedback reports to providers so that they can improve their quality. There have also been efforts to create new innovations in healthcare delivery, such as decision aids and shared decision-making tools, to support the tailoring of treatment to risk and patient engagement in improving outcomes. Yet despite these efforts, our field has made only modest progress in improving the safety, timeliness, evidence-based nature, efficiency, equity, and patient-centeredness of healthcare.

I believe that we are, in part, limited by our perspective. Most outcomes researchers are academicians. We view the success of our efforts as our publications and grants. In fact, our job security and promotion are predicated on these successes, as if that is the endgame of our efforts. However, if we view changing and improving healthcare as the real benchmark of success, almost all of us would be found wanting. Consider a prototypical success story—you have created an intervention to improve evidence-based practice. You obtain a grant to prove that the innovation works and you find a significant improvement in the safety and outcomes of care. You publish the results in a prestigious journal and present it as a late-breaking clinical trial at a national meeting. After the congratulations from your colleagues and the media, what happens next? How do you sustain the program at the study sites? How do you support the dissemination of the intervention to other sites that were not included in the original grant? How do you further refine and improve the intervention? Although some may argue that you could write another grant to prove that the innovation works, I think that this is impractical, slow, ineffectual, and limited way to innovate in healthcare. Although it may help enhance one’s personal success and promotion, it will not have a meaningful effect on improving the deficiencies in health care that the intervention was developed to accomplish.

One strategy that is rarely pursued in most academic circles is to create a commercial company to carry forth the intervention: to improve it, to sustain it, and to disseminate it throughout the US healthcare system. Although there have been calls for academic medical centers to develop companies to improve health care, and some institutions have created formal programs to support such entrepreneurship, this is rare and has not been embraced by the broader outcomes community, who often view commercialization as an anathema to the academic mission. Although it is true that founding a company can introduce substantial economic biases in one’s commitment to the success of an intervention, there are also philosophical biases to one’s ideas that are present, even if there is no transparent financial gain from widespread acceptance of the innovation. We need to recognize that all investigators have strong commitments to their research and that potential conflicts, financial, or philosophical are inherent in all that we do. How our profession develops checks and balances to insure that only the highest quality work is published and replicated, without presuming ill intent among those who seek to disseminate their work through commercial avenues, is an issue that warrants widespread discussion.

In my personal experience, I was frustrated with the variability in health care; variability that was much more strongly associated with physician preferences than with patient benefit. We developed an intervention, ePRISM, to execute multivariable risk prediction models within the routine flow of clinical care to support the tailoring of treatment to risk. In our own healthcare system, and in a 9-center study, the use of ePRISM was associated with improved patient experiences with care, better use of bleeding avoidance strategies according to patients’ risks of bleeding with a 40% to 45% reduction in the odds of bleeding, and lower costs. Midway through our grants to test the tool, when I was concerned with how we would sustain and disseminate ePRISM, we formed a company to take over the tool. Our logic, consistent with recent calls for demonstrating the value of research, was that if there was real value in the tool, then hospitals would pay for it. Unexpectedly, I have been surprised by the negative reception of this effort to extend and improve our effort by journal reviewers and colleagues.
To me, this highlights the challenges confronting the outcomes research community. If we truly want our work to affect care, then we need to embrace a myriad of tactics, from public policy to entrepreneurship, as strategies to effect change. Limiting our efforts to publications and grants is slow and inefficient and limits our ability to make the changes in health care that drew us to this field in the first place. What we need is a dialogue within the academic outcomes community to debate how best to support healthcare improvement, to learn from each other what works and what does not, and embrace efforts to move our science from the pages of journals into routine clinical care.

Disclosures

Dr Spertus has an equity interest in Health Outcomes Sciences.

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