EDUCATIONAL PRACTICE AND EDUCATIONAL RESEARCH IN ENGINEERING: PARTNERS, ANTAGONISTS, OR SHIPS PASSING IN THE NIGHT?*

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For most of the 20th century, engineering education research mainly consisted of using student satisfaction surveys and instructors' impressions to assess the effectiveness of teaching methods, courses and curricula. In the 1980s and 1990s the emphasis shifted to less anecdotal methods involving statistical comparisons between experimental and control groups (Wankat *et al.*, 2002). Starting early in the new millennium, a movement arose to make engineering education research more "rigorous" by using methods and philosophies drawn from the social sciences.

A precise definition of rigorous research in engineering education was formulated in a three-year NSF-funded project, "Conducting Rigorous Research in Engineering Education: Creating a Community of Practice," commonly known as the RREE project (Streveler, Borrego, & Smith, 2007). The RREE Committee proposed four levels of rigor in inquiry about teaching and learning:

Level 1–Excellent teaching. Good content and methods but no formal inquiry intended to improve teaching quality.

Level 2–Scholarly teaching. Classroom assessment but no testable and replicable scholarship.

Level 3–Scholarship of teaching. Inquiries into teaching and learning and presentation of results in a public forum where they can be critiqued, evaluated, and built on by others.

Level 4–Rigorous research. Inquiries that meet the Level 3 standards and three more criteria:

- Begins with a *research question* (focuses on why and how learning occurs) rather than an *assessment question* (what and how much is learned);
- Ties the research question to learning, pedagogical, or social theory and interprets the results in light of the theory;
- Pays careful attention to the study design and methods, adding validity, reliability, and impact to the findings.

While the rigorous research movement has made valuable contributions to engineering education, it has also given rise to a concern. The engineering education research community has begun to split into two divergent and sometimes antagonistic groups: the *theoreticians,* who seek

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to understand the learning process at a fundamental level, and the *practitioners*, who continue to focus their research on improving teaching structures and methods. Those descriptions represent extremes, with many researchers occupying intermediate positions, but the existence of the two different camps and the danger of a widening schism between them are real.

Part of the problem is that the theoreticians have used a hierarchical model to sort inquiry about teaching and learning into different levels, with their preferred approach clearly occupying the superior position. The RREE Committee conveyed that attitude when they applied the term "rigorous" to Level 4, suggesting a belief that practice-oriented studies that do not meet the Level 4 criteria cannot be rigorous and should not even be called research. (Rather, they are "assessment.") Not surprisingly, many practitioners have felt discounted by such statements and mutual antagonism has been the consequence.

In fact, many prominent leaders of the rigorous research movement fully appreciate applied research: they understand its importance in the development of a discipline and some have conducted significant applied studies themselves. They are simply making the reasonable claim that applied research alone will not lead to impactful educational reform—it will also take more fundamental scholarship. [For example, see Watson (2009).] Unfortunately, not all theoreticians take that balanced view: some argue instead that to be fully acceptable, engineering education research should focus exclusively on the process of learning and should always be grounded in learning theory. In the balance of this essay, when we speak of the theoreticians and their position we will be referring to that point of view.

The theoreticians have enjoyed considerable success in setting a research agenda for engineering education in the United States. The National Science Foundation has taken the position that engineering education reform efforts failed to produce significant changes in recent decades, and what is needed is more theory-grounded research on learning and less on program and instructional development (Gabriele, 2005). The proposal submission guidelines for the 2013 NSF Research in Engineering Education program (National Science Foundation, website) include the statement "Competitive proposals advance understanding in engineering education by grounding the proposed work in theory." Guidelines for manuscripts submitted to the *Journal of Engineering Education* include the question "What conceptual or theoretical framework informs the study?"and the statement "The relevant theories should be presented."

While applied research has continued to be funded by the NSF—such as in its *Transforming Undergraduate Education in Science* (TUES) and *STEM Talent Expansion* (STEP) programs—and reported in JEE papers, the existence of the guidelines still has potentially negative consequences. Manuscripts of proposals and articles that meet all traditional standards for quality, clarity, and potential impact could be rejected if most reviewers take the Level 4 requirement literally, a possibility that makes many researchers nervous. With growing frequency, learning theories are treated like passwords to gain entrance to journals and funding agencies: they are cited in manuscripts as frameworks for research studies and then play little or no role in the studies. If talented engineering educators start to believe that basic research with a theory attached to it is the only kind of work NSF and top engineering education journals will accept in the future, it could discourage them from undertaking applied research, which could have a serious impact on the future development of the discipline.

Given those concerns, one would think that the decision to give priority to basic theory-grounded studies in funding and publication decisions must have a firm foundation in research and experience. Our purpose in this paper is to suggest that no such foundation has been advanced; rather, the current case for rigorous research rests on several hypotheses that have been accepted without critical examination or plausible theoretical or empirical support. In the remainder of this essay, we will state those hypotheses, question them, and challenge the engineering education research community to test them rigorously and use the results to formulate a truly research-based agenda for engineering education research and reform.

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Hypothesis 1. *Innovations in science and engineering have all been grounded in theory, so the same must be true of educational innovations.*

Alternative. *Most innovations in science and engineering (and every other discipline) have begun with observations and experimental data. Theories arose later in an effort to make sense of the observations and data.*

Induction (starting with observations and generalizing laws and theories from them) and deduction (starting with laws and theories and predicting and verifying consequences) are both fundamental components of the scientific method, with oscillations between the two leading to increasingly general theories. Every scientific field has developed starting with empiricism and induction, however: observing natural phenomena and trying to explain them, or trying something new and observing the consequences. For example, a theoretical understanding of the principles of statics did not emerge until the 17th century through the work of Newton and Hooke, even though humans had been building remarkable structures such as the Pyramids, the Pantheon, and medieval cathedrals for the previous 5,000 years. Similarly, modern thermodynamics theory had its origins in 1824 with Carnot's work on the efficiency of the steam engine, while steam had been used to produce mechanical motion for more than 2000 years before Carnot, and James Watt patented his steam engine in 1781. If theory-based educational research is ever shown to be a necessary precursor of educational innovation, it cannot be on the basis of an analogy with science and engineering innovation.

Hypothesis 2. *An educational research study cannot be rigorous and impactful if it lacks an underlying learning theory framework.*

Alternative. *An educational research study can be perfectly rigorous and have a powerful impact even if no learning theory provides a framework for it.*

Attitudes about theory may be the single greatest source of division between STEM and social science researchers and the most difficult aspect of social science research for engineers to understand (Borrego, 2007). Pat Hutchings of the Carnegie Foundation for the Advancement of Teaching has called theory "the elephant in the scholarship of teaching and learning room" (Hutchings, 2007).

Many important questions facing engineering education could be studied perfectly well without introducing a learning theory as a research framework.

- *What attributes of entering college students and features of college instruction and learning environment have significant impacts on students' performance, retention to graduation, and attitudes about their education experience?*
- *What is the nature of current and future engineering practice and what cognitive and professional skills are required to succeed in it?*
- *How can engineering curricula (which have traditionally focused on content and a limited number of technical abilities) be redesigned to effectively equip students with those skills?*
- *What incentives and rewards can be offered to motivate engineering faculty at research universities to adopt proven teaching practices, and how can they best be equipped to do so successfully?*

And so on. In fact, the first of those questions was addressed in Alexander Astin's monumental work *What Matters in College* (Astin, 1993), a report of a research study with a huge impact in which learning theory played no discernible role. To be sure, good learning theories can contribute significantly to studies of such questions; our point is that they are not necessary conditions for the validity and impact of educational research. Making their presence mandatory could conceivably keep researchers from exploring pathways that might lead to significant breakthroughs and motivate them to concentrate on projects that yield little improvement in student or teacher performance.

A common justification for Hypothesis 2 is an argument that learning theories are to education as scientific theories are to the physical, chemical, biological, and engineering sciences. The analogy is false. Scientific theories are testable and refutable: if a single observation contradicts the predictions of a theory, once the contradiction has been satisfactorily replicated, the theory is either overthrown or its domain of applicability is severely reduced. Learning theories are not at all like that. They are much more like engineering *models*—empirical, approximate, and almost always coexisting with alternative representations. While they (like engineering models) can be valuable frameworks for research design and data analysis, there is again nothing fundamental about them that should make them essential components of all engineering education research studies. Their absence or presence should therefore not be a deciding factor in judgments about accepting or rejecting manuscripts.

Finally, even researchers in the social sciences do not insist on basing all research studies on pre-existing theories. Many well-regarded types of research use a *grounded theory* approach, in which experimental data are collected and organized into categories and eventually refined into a theory that explains the data. Instead of the experimental research being grounded in a theory, the theory is grounded in the experimental data, as it always is in the physical sciences.

Hypothesis 3. *Efforts to reform engineering education that are not based on rigorous research are unlikely to succeed.*

Alternative. *Engineering education reforms not based on rigorous research have succeeded brilliantly in the past, and there is no reason to believe they will not do so in the future.*

A highly successful major reform started in the 1950s and culminated in the 1960s, when the empirical correlations, rules of thumb, and laboratory and plant procedures that had previously dominated engineering courses were largely replaced by “engineering science” built on a foundation of basic science and mathematics. The reform took place with no theoretical research to support it, and its success at profoundly changing engineering education is undeniable. (Some say that too much engineering practice was sacrificed in the reform, but that’s a different debate.) Other successful reforms include the change from basing engineering program accreditation on counting credits and resources to basing it on students’ attainment of learning outcomes; a renewed emphasis on design throughout the engineering curriculum; and dramatic advances in the applications of technology in both traditional classroom and online instruction [Froyd *et al.*, 2012]. While learning theories might support those reforms, most engineering administrators and faculty members who have adopted the changes were not influenced by the theories and probably didn’t even know they existed.

Hypothesis 4. Doing more rigorous research is the key to achieving significant engineering education reform in the future.

Alternative. Achieving significant engineering education reform will require (a) establishing effective discipline-based instructional development programs for current and future engineering faculty members; (b) providing meaningful incentives for faculty members to participate in those programs and adopt the practices the programs recommend, and (c) providing additional incentives for departments to integrate the practices into their core curricula rather than counting on individual courses and instructors to be the sole vehicles of change (Graham, 2012).

Even though an immense, decades-long body of research has demonstrated the effectiveness of learner-centered teaching methods, engineering professors have not rushed *en masse* to adopt them. When surveyed about why not, very few argue that the research was not rigorous enough; instead, they talk about their institution’s reward structure discouraging efforts to improve teaching (Fairweather, 2008). Moreover, the type of scholarship being advocated by the rigorous research movement has been practiced for many years in the social sciences. If Hypothesis 4 were valid, higher education in at least some social science fields should be well ahead of engineering education in terms of teaching quality. We have not seen indications of this outcome in any social science discipline—including education, which despite decades of scholarship devoted to understanding learning at a fundamental level, continues to use traditional, ineffective teaching practices.

We believe that if engineering education research were stopped completely right now (which we are in no way advocating), and engineering faculties could be induced to put into practice everything we currently know about teaching and learning from past research, cognitive science, and experience, we would achieve innovation with impact to an extent beyond the wildest dreams of the most idealistic reformers. The question then becomes, how can we do that?

Signposts to the answer are raised in the final report of a recent three-year study of engineering education led by Leah Jamieson and Jack Lohmann (2012) with contributions from over 100 authorities in the field. Two of the study’s recommendations are:

- *Value and expect career-long professional development programs in teaching, learning, and education innovation for engineering faculty and administrators, beginning with pre-career preparation for future faculty*.
- *Raise awareness of the proven principles and effective practices of teaching, learning, and educational innovation, and raise awareness of the scholarship of engineering education.*

Those two suggestions address the reasonable proposition that faculty members cannot be expected to implement new and effective teaching strategies if they don't know what those strategies are. Effective instructional development programs (including teaching workshops, learning communities, and mentorships) in engineering and other STEM disciplines have been shown to motivate faculty members to adopt new teaching practices (Felder, Brent, & Prince, 2011).

Another key recommendation in the Jamieson-Lohmann report is: *Increase, leverage, and diversify resources in support of engineering teaching, learning, and educational innovation*. Just putting instructional development programs in place and inviting faculty members to attend them is not likely to have much of an impact on teaching practices. Unless faculty members are convinced that their institution truly values teaching quality as opposed to simply paying it lip service, most are likely to continue viewing teaching workshops and experimenting with new teaching methods as wastes of time. When institutions start providing the kinds of incentives and rewards for innovation and performance in teaching that they now provide for productivity in research, the reforms will happen—at least that is what the alternative to Hypothesis 4 presumes. It is possible that more rigorous research will also be required, but a plausible case for that requirement has not yet been made.

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What, finally, are we saying? Are we opposed to using social science methods in engineering education research? Not at all! We particularly welcome qualitative and mixed research methods, which provide insights into why teaching methods and structures work as they do that cannot be obtained in any other way. Are we criticizing using learning theories in engineering education research? Absolutely not, any more than we would criticize using process models in engineering research. Both can be valuable guides to organizing and interpreting data and suggesting research questions and methods.

What we *are* doing is questioning four hypotheses whose uncritical acceptance has potentially serious negative consequences: *(1) Innovations in science and engineering have all been grounded in theory, so the same must be true of educational innovations; (2) An educational research study cannot be rigorous and impactful if it lacks an underlying learning theory framework; (3) Past efforts to reform engineering education have failed because they were not based on rigorous research; (4) Doing more rigorous research is the key to achieving significant engineering education reform in the future*. We suggest that these hypotheses have been advanced without empirical or logical support, and propose that until such support is provided, the hypotheses should not be used to exclude research studies from being funded, published in any journal, or presented at any conference.

We offer in conclusion this challenge to the engineering education research community: *Conduct rigorous studies that will validate or negate those hypotheses.* Once the results are in, use them to formulate a rational basis for decisions about funding proposals and accepting submitted manuscripts. Until the results are in, do not impose standards of rigor that have not themselves been subjected to rigorous evaluation, but continue to use the criteria that have long been the basis of funding and publication decisions.

We have no illusion that this is an easy challenge, but it is hard to imagine a more important one. Among other benefits, meeting it will definitively support engineering education's claim to being a truly scholarly discipline.

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