

## Editorial: Scholarship in Chemical Education

Is scholarship in chemical education equivalent to scholarship in chemical research? Could it be? Should it be? These are questions that it is reasonable to ask, because we are beginning to be able to discuss and partially answer them rationally and empirically.

In his book *Scholarship Reconsidered*, Ernest Boyer (1) argued that three new forms of scholarship ought to be added to the scholarship of basic research on which modern research universities are based. (Boyer calls basic research the “scholarship of discovery”, and states that it “has come to be viewed as the first and most essential form of scholarship, with other functions flowing from it.”) Boyer’s three new scholarships are

- Scholarship of integration: putting isolated facts into perspective, making connections across disciplines, placing specialized areas into larger context, revealing the meaning within data.
- Scholarship of application: bringing knowledge to bear on consequential problems and making scholarship useful to individuals.
- Scholarship of teaching: transmitting knowledge and, in addition, transforming and extending it.

There has not been a stampede to embrace Boyer’s ideas, despite the fact that his new forms of scholarship exemplify teaching and service, two of the three areas university faculty are supposed to contribute to (and be evaluated on). For example, it does not appear that scholarship in chemical education is considered equivalent to, say, scholarship in organic chemistry in terms of its rewards and perquisites within most universities.

In a paper in *Change*, Donald Schön (2) suggests that in order for any kind of scholarship to be considered appropriate and worthy, it must “produce knowledge that is testably valid.” Rigorous criteria must be applied to decide on validity, and scholarship must lend itself to intellectual debate within academic and other communities. But the kinds of knowledge, rigorous criteria, and claims to validity may differ from those of the traditional scholarship of discovery. That is, the epistemology of the new scholarships may differ from the epistemology of the scholarship of discovery, and the new scholarships may not be accepted without a shift in beliefs about how we come to know what we think we know.

### Rigor versus Relevance

Schön likens the epistemology of the research university to hard, high ground overlooking a swamp. On the high ground researchers solve manageable problems using theories and techniques based on prior research. In the swamp lie problems that are confusing, intractable, ill defined, and perhaps incapable of technical solution. However, almost by definition, the well-defined, tractable problems of the high ground are often of little or no interest to society at large or to individuals other than those directly involved. The problems of greatest human concern lie in the swamp. Should one opt for the rigor of the high ground or the relevance of the swamp? The epistemology of the research university clearly favors the former. Those who are passionately involved in teaching have chosen the latter.

This was brought home to me a week or so ago in a discussion with colleagues regarding an experiment in chemical education. (Some would have put quotes around “experiment”, I think.) We wanted to compare two different teaching paradigms applied to the same course, trying to reveal whether one was clearly superior to the other. The epistemological question was, can such an experiment be done rigorously enough to convince others of its validity? And for most of us discussing the question, the answer was no. Even given the best of circumstances, we did not believe that we could adequately control the very many, very messy variables inherent in comparing two classes of about 300 students each.

For some this lack of rigor argued conclusively that the experiment was not worth doing, but for others it did not. My view is that our ability to carry out experiments in chemical education is far less well developed than our ability to carry out experiments in chemistry, but that ought not be an argument against chemical education research—the opposite. We need to learn all we can about how to carry out experiments in chemical education, because such experiments can provide us with a great deal of useful, practical, even crucial, information. And we are unlikely to learn how to do something better if we don’t even attempt it.

Schön argues that there is a lot to be learned from observing competent practitioners in any area of applied knowledge. Further, such practitioners may well have their own epistemologies that differ from the norm and provide new ways of knowing. Those who immerse themselves in the swamp of relevance develop tacit knowledge, implicit in their patterns of action. Even they may not be aware of what they know and how they know it. Schön’s fundamental position is that the “new categories of scholarly activity must take the form of action research.” That means reflecting on, studying, and learning from what we, as successful practitioners of chemical education, do every day in our classrooms and laboratories. It also means observing successful practitioners, or observing ourselves, to see what we did in a given situation and how we could do it better. It is not based solely on collecting data or statistics but may be augmented by them.

It seems to me that we are on the threshold of significant developments in action research in chemical education. A great many teachers of chemistry are interested in experimenting with how they can help students to learn. We are beginning to develop ways to evaluate successful innovations according to our existing epistemology, even though we are far from being able to provide evidence that would convince sceptics. And all of us can contribute by reflecting on what we do as we are doing it. In the past, much of that reflection has appeared in this *Journal*, and I hope that it will continue to appear here.



### Literature Cited

1. Boyer, E. L. *Scholarship Reconsidered: Priorities of the Professoriate*; Carnegie Foundation for the Advancement of Teaching; Princeton, NJ, 1990.
2. Schön, D. A. *Change* **1995**, November/December, 27.