

Editorial

Research Worth Publishing

As editor, one of my main responsibilities (and probably the most interesting) is reading articles submitted for publishing in this journal and then providing advice to the author(s). The message I send most frequently explains why an article is not suitable for publication, and I usually make suggestions for making changes that should improve its chance of being accepted in the future. These recommendations include comments from reviewers, because every article is vetted by at least two peer reviewers. There is also a piece of general advice I find I have to give repeatedly, and it applies regardless of the specific topic of the research being reported. I found myself giving this same advice recently to students in my online course on Research in Distance Education. As I thought about it, I decided the problem I am concerned about isn't going away, but indeed is becoming a bigger problem in these days of Internet publishing than before. The problem I refer to is the propensity of many authors as well as students to underestimate the importance of the literature review that *must* precede any presentation of data. To get to the point, I will reproduce part of my reply to a would-be author who wrote to me recently to ask if an article she had in mind had any chance of being published. Here is what I said:

There is a fairly high non-acceptance rate; the main reasons for non-acceptance (apart from the fact that we have far more supply of articles than space to accommodate them) is that people gather data that answers a question that is *not grounded* in previous research and/or is so specific to a particular program that it is of little value beyond that program, which together contribute to a disconnection between the empirical part of the research and the theoretical. A good article includes not only good data, gathered by a technically sound method and well analyzed, but a rationale for the research that explains in a persuasive way why, in terms of the existing state of knowledge as reported in the literature, there is meaning and significance in that data. In other words, to be publishable, the question about which you have gathered data has to be grounded in a good review of previous research and then have conclusions that show how it fills a hole in that previous state of knowledge.

It ought to be a matter of surprise that many academics and graduate students either do not understand this basic relationship between empirical research and theory or chose to ignore it—and (before anyone writes to me) let me emphasize that this is not an issue about qualitative research. I am as enthusiastic about the appropriate use of qualitative methods as anyone, but the research question to be answered by qualitative methods has to be justified in terms of existing knowledge as much as a question to be answered in other ways. Authors who toil over analyzing data to answer a question with little or no revealed connection to previous research are wasting their time—if they want to be published in a quality scholarly journal—as well as showing incompetence. Is “incompetent” too harsh? Perhaps, but how else can one describe a researcher who reports data and conclusions about a question when the answer to the question was already known? This happens quite frequently, and the sad part of it is that if the researcher reviewed previous research, it would become apparent that there were numerous *related* questions that would justify the investment of time and effort.

We could conjecture at length on reasons why this problem of disconnect between what is known and what is researched is worsening. Whatever explanations we came up with, what would link many of them, I believe, is the accelerating trend to rely on the Web as the sole means of doing the literature review. In particular, using key words in a literature search is a good way to scoop up a large basket of apparently relevant sources. There are several problems with this, though. One is that there can be a huge number of even more relevant sources, or sources of better quality, that are not located by the key words that have been chosen. Conversely, what is scooped up by the key word ends up in the basket merely as a result of the author having chosen that key word, a choice of word that is often not valid. In effect, putting trust in key words is to take a risk with the safety of one’s research by gambling on the good judgment and integrity of strangers. Most material on the Web is unvetted, and even those online journals that have some form of refereeing are so desperate to feed an ever-hungry information outlet that the quality of what is available ranges widely, and most ranges from the mediocre to appalling. For the student, the big problem is that before one can safely use Internet search tools, one has to know the field well enough to first manipulate key words judiciously and then, even more important, to be able to distinguish the occasional high-quality item from the vast undergrowth of vanity-publishing spawned by the Web. There is no easy solution to this dilemma; one might be to take a graduate course in the academic study of distance education from a reputable university, but in this technol-

ogy-obsessed climate even that is not as easy as it first seems. One suggestion I would like to make is that potential authors (and students writing term papers) give precedence in their literature reviews to the longer established, and therefore more reputable and authoritative journals. These, as all readers of this journal should know, are, in addition to the *American Journal of Distance Education*, the following: *Journal of Distance Education* (http://www.cade-aced.ca/en_pub.php), *Open Learning* (<http://www.tandf.co.uk/journals/titles/02680513.asp>), and *Distance Education* (<http://www.tandf.co.uk/journals/titles/01587919.asp>). In summary, my advice to potential authors is to be a very cautious user of free material distributed on the Internet, and certainly not to rely too much on that source of material when undertaking a literature review that aspires to lay the foundation for quality research.

What Research Is Worth Reading?

If the advice above is good for potential authors of articles, that is, the “suppliers” of research information, it should also be relevant for “consumers,” that is, readers. As a consumer, one of the first tests to apply when deciding if a piece of research is worth using is to evaluate the quality and source of references and/or bibliography at the end of the article or book. As an editor, within the first five minutes of beginning to review an article, I look at the references. If I do not see recognizable authorities in our field, seminal publications, established refereed journals, among other things, I take a suspicious stance when I turn to consider the quality of the research question and the data that address it. To illustrate, can you imagine an article that claims to deal with a question about self-direction in learning at a distance that had no reference to Garrison, or one on faculty rewards that did not refer to Wolcott, cost-effectiveness with no mention of Rumble, or transactional distance without Saba? None of these are impossibilities certainly, but I would wonder if the researcher did, in fact, know what he or she was writing about. What I would expect to see in such articles would be, at least, an explanation of how the research of these leading scholars points to a new question, that is, the question to be examined, or an explanation of what is missing in their work that justifies further research. What I would *not* expect is that authorities are simply ignored. Similarly, if an article contains *no* references to *any* work published in *any* of the leading journals of distance education, I find it hard to believe these journals have been reviewed and that no precedents for the reported research were found there.

To cite articles and explain what is missing is good; to show no awareness of previous research is not.

Case Studies

Before closing, I would like to offer one other piece of advice to both “suppliers” and “consumers” of research literature in our field. This concerns so-called “case studies.” As editor, I receive a deluge of so-called case studies. However, just because someone online (where anything can get published) describes a set of data as a “case study” doesn’t make it so. Like all other research, a review of previous research is necessary to explain the need for a proposed research question. But it also has to explain how the state of knowledge regarding that question is at such an immature stage as to justify investigating it through the data regarding a single case, as compared with methods that study a larger number of cases. (And just because it seems easy is not a good reason!) Another way of testing whether a case study is worth reading is to ask if there have already been case studies of that phenomenon, and if so, what the new study adds to what was known before; all research should move from the single case, from which little can be generalized, to larger populations. If the phenomenon has *not* been studied, a case study may be justified, but if it has, then we should be testing the conjectures and hypotheses thrown up in the case study in other ways, probably by surveys, and ultimately by experiments. It remains a matter of great concern that distance education has matured so little beyond anecdotal reports and case studies. In the *American Journal of Distance Education* I give high priority to publishing well-executed survey research, and above all any good quality experimental research, and these methods have precedence over case studies—provided, of course, the question is reasonably well-grounded in a review of previous research!

Michael Grahame Moore
Editor