

Editorial

Reviewing Research Manuscripts — Something Like Jury Duty?

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The papers published in each edition of MERJ have made the bar — they have undergone peer reviews and been accepted for publication. Almost certainly suggestions and criticisms from reviewers and editors have helped shape the final paper, with varying degrees of appreciation or resistance from the authors. Providing feedback to MERJ authors on research manuscripts is essential for encouraging and supporting high quality research in mathematics education in our region. But how is reviewing a manuscript experienced from the reviewer's perspective?

When editors and reviewers of MERJ receive a manuscript to assess we are presented with both opportunities and challenges. Reviewing and critiquing a work of research offers opportunities to engage with research, which may be in an area different from our usual engagement, even outside our comfort zones. We are asked to evaluate the quality of the research and to communicate criticisms and suggestions to the authors in a constructive manner. Reviewing a paper is an opportunity to participate in a learning experience.

One of the challenges for reviewers is to make a recommendation as to whether the paper should be published in MERJ (with possible amendments) or should not be published (including rewrite and resubmit). Reviewers are often consistent in their recommendations about whether to publish, suggesting that experience leads to 'I know what good research is when I see it'. However, some reviewers give the editors conflicting views and so present a dilemma about how to report back to the authors.

So what are the criteria for a decision to recommend publication? What could make research in mathematics education worthy of publication? I offer some ideas that may be helpful to MERJ members when reviewing papers or, more importantly, as a reflective process for self-review. I will confine my remarks to empirical research, that is, research based on investigations.

There are standard criteria for quality common among major research journals in mathematics education. These include evaluating whether the literature review is comprehensive and whether the paper is well written, cohesive, and scholarly. Are the results analysed critically rather than simply listed? Some themes make a useful checklist for MERJ peer reviewers and self-reviewers.

- Does the research fit with the journal's area of focus?
- Does the paper integrate and build on published research?
- Are the research questions open-ended and thoughtful, allowing for alternative perspectives on the phenomena being researched? Are the research questions clearly articulated?
- Are the methods of data collection and analysis systematic and clearly outlined while flexible enough to allow unexpected and interesting data to

emerge (Eybe & Schmidt, 2001).

- Are the results discussed critically and alternative interpretations explored?
- Have ethical issues been given thought and consideration?
- Does the paper meet technical criteria about length, formatting, and ways of referencing?
- Are arguments presented logically? Do the conclusions follow from the evidence?
- Is use of language at a high level?

More fundamental questions about quality in empirical research are these:

- Is the research significant?
- Is the methodology appropriate?

What may be overlooked about these two questions is the relationship between them. The terms 'significant' and 'appropriate' are problematic and both depend on the paradigm in which the research is situated, for example, a positivist or an interpretative paradigm.

'Significant' research in mathematics education usually denotes research that furthers knowledge in a worthwhile way. The terms 'knowledge' and 'worthwhile' are developing constructs rather than static or absolute. They are historically and socially generated and so complex and ambiguous. One idea about significant research is that the research should be useful to improve teaching practice. Wardekker (2000) points out that usefulness of research cannot be a criterion for good research. This would require the researcher to know in advance that positive changes in teaching and learning will occur as a result of the research. Even the relevance of research questions cannot be guaranteed in advance as aspects may become significant to the researcher as they emerge.

Another idea is that research produces generalisable results (Eybe & Schmidt, 2001). This idea has its basis in the experimental paradigm where the environment is largely controlled. This assumes that research in mathematics education can be decontextualised—lifted from the arena in which it arose and transferred to another context where the actors have different needs and goals. In an interpretative paradigm individuals construct their own meanings and a researcher cannot persuade practitioners by logical arguments that his or her story about the world is better and should be used. Criteria of quality of a research report in interpretative paradigms are based on the "heuristic quality" of the report, "its power to enlighten people by making them really understand the narratives that were the objects of study" (Wardekker, 2000, p. 266). The communicative validity of the paper seems to me to be one of the useful gauges of qualitative research. From a socio-cultural perspective, research is co-developed by the researchers and participants. Research is dynamic; the project is shaped and transformed as it progresses.

To evaluate significance and methodology in mathematics education research I suggest reviewers could think about questions like these:

- Does the research signal researchers and practitioners to look at our own research and practice in a new light—to re-interpret personal teaching and learning experiences?
- Does the research alert us to a view of knowledge as complex, incomplete, dynamic, and multifaceted?

- Does the paper have the potential to generate or develop theory or theoretical constructs?
- What are the appropriate criteria for rigour in the paradigm in which the research is situated? Are the methods rigorous?

As an aside to this last point, the use of the word 'significant' when 'statistically significant' is meant is not forgivable, in my book. Statistical significance has a precise, mathematical meaning and may have more to do with sample size than educational significance.

The peer review process is something like jury duty. We can all think of many reasons why this is a good system, albeit with limitations. However, serving on a jury is not necessarily welcomed as an opportunity. Reviewing a research paper could be seen primarily as a chore. It could, however, be viewed as a way of connecting with other researchers in mathematics education. It could be an opportunity to guide and encourage less experienced researchers, to help enculturate authors into the community of researchers. It also enables reviewers to receive feedback, through editorial summaries and other reviewers' comments about the manuscript, and so develop our abilities to evaluate and critique research. We could think about reviewing a research paper as playing a small but essential part in co-developing the paper. It depends on how we interpret the task.

References

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