Review

What is good research?

Göran Wallén
Department for the theory of science and research,
Göteborg University, Sweden

Criteria for Scientific Quality and Relevance in the Didactics of Mathematics. Report from a symposium held in Gilleleje, Denmark, 1992. Editors: Gunhild Nissen and Morten Blomhøj. Danish Research Council for the Humanities. Roskilde University, IMFUFA. 1993.
ISBN 87-7349-178-0, ISSN 0906-0103.

This is a report from a Scandinavian course for young researchers in the didactics of mathematics. This field of research is complex and multi-disciplinary, and there are different research trends. It is therefore of interest to bring researchers from different traditions and countries together. This is moreover emphasized by the invitation of some internationally recognized researchers. The report has values beyond the course in the critical reviews of research, the discussions of criteria, and the examples of research approaches. Although not in focus, there are a lot of critical remarks that reveal a part of the underlying crisis in the whole field of research in didactics. Before I dig further into that, I will present a summary of the papers.

Jeremy Kilpatrick (Georgia, USA) and *Anna Sierpinska* (Montreal, Canada) discuss criterias like relevance, validity, objectivity, orginality, rigor and precision, predictability, reproducibility, and relatedness to mathematics and mathematics education. Kilpatrick notices that these criteria seem outdated. This is largely due to phenomenological and especially constructivist approaches. But in his view they could be useful if interpreted appropriately. He makes some harsh comments; for instance, that much research in mathematics education has been ineffective in changing practice and that it lacks theoretical underpinnings.

Anna Sierpinska clarifies the above criteria and their context of application by using them in examining some research reports. Like Kilpatrick, she does not accept the radical constructivism that leads to subjectivity: Beside their own constructs the pupil /teachers/researchers must also be aware of others' concepts and constructs. This will also

lead to a more reflexive apprehension of one's own view. The discussion of criteria (like predictability) leads to some more fundamental problem such as: It is not at all clear what a succcessful teaching is.

Willibald Dörfler (Klagenfurt, Austria) presents himself as a social constructivist but points out that this does not mean that "anything goes". In didactics there are mutually exclusive paradigms, for instance, an orientation to the mathematical content and to constructivism. This diversity poses problems for journals in the field. He discusses requirements for acceptance of a paper. Among these are an explicit formulation of the central research questions and of the research paradigm. The paper must be embedded in existing research and literature.

Celia Hoyles (London, England) presents a study of interconnections between teachers´ attitudes and practices. Anna Sierpinska (above) in a short summary wrote: "Phenomenological descriptions do not seem to lead to relevant knowledge". Parallell to this view Hoyles writes: "The quantity and diversity of this data provided a rich ethnography of beliefs and practices but posed serious problems of analysis". The solution presented in the paper was to develop five "caricatures" of teachers as a kind of synthesis and to serve as paradigmatic cases for the issues of research analysis.

Bengt Molander (Uppsala, Sweden) presents a philosophical perspective on practical knowledge. One of his starting points is a research project "Education for the Application of Statistics". He puts forward some very profound questions (with some answers): Is practice applied theory? In textbooks there are no genuine questions and problems of real life and human action but only "the technical machinery" – how to calculate correctly. These and other findings lead to a model with two aspects of knowledge: the "technical knowledge" as mappings and mastery of a technique and the important but neglected "directive knowledge" of meanings, functions, norms, critique, and overview.

Jette Fog (Aarhus, Denmark) discusses ethical and methodological problems in qualitative interviews. There are no simple criteria but some conditions for quality lie in the researcher's competence: An emphatic understanding of the other person and a cognitive ability to get an overview of the theme in its context. The ethical and methodological problems are connected. You cannot be an objective observer, as the information you get depends on involvement of both parts. But the researcher's trust could be misused and your interpretation could diverge from the person's self-understanding.

The last paper is from *Steinar Kvale* (Arhus, Denmark) about standard objections to qualitative research interviews. Especially interesting is when Kvale turns the arguments upside-down: Leading questions could be used for obtaining information being withheld and for checking the reliability of the interviewees' answers. Contrary to demands for the one and only meaning, different interpretations could be comprehensible if different perspectives are made explicit. Further on, he argues that criteria such as validity are central also in qualitative research although measurement oriented criteria such as predictive validity could not be fulfilled. Broad conceptions should reflect the phenomena of interest and a validation could be an examination of sources of faults.

I have read this report with great interest and also with some frustration. My global interpretation of it is that didactics in mathematics is in a crisis – partly parallel to a crisis in my own subject: theory of science and research. There are four main difficulties which are not in the focus of the report but which are often pointed out in introductions, remarks, and discussions of examples of didactic research. In short passages between the conference themes, there is a questioning of this field of research.

1. Research in mathematics education is of quite another character than mathematics itself. For instance, are mathematicians constructivists? There is also a gap between research in mathematics and what is taught in schools.
2. Constructivism is widespread in didactics, but in this report there are several indications of its limits. This leads to reintroducing criteria like objectivity and claims that the pupils' understanding also must include others' "constructions". A radical sociological constructivism leads to "anything goes" – in this report different criteria are discussed and there are some pleads for realism.

   From my own experiences of sociologists of science and their constructivism ravaging the field of theory of research, I think a return to a pragmatic realism is necessary. There are a lot of theoretical constructs in research but also real things, and these ought to be connected. Findings are bound to a context, but this will not lead to total relativism if the context is specified.
3. Much didactic research has little relevance for teachers. Relevance of research is of course discussed in the report, but there is perhaps nothing wrong with the research. Wrong is the claim that research findings ought to be applied in teaching. Teaching is perhaps not

applying research findings. Then teacher training must be done on quite different premises. Molander's paper could be redirected from applied statistics to teaching.

4. Research in didactics has not developed a theory of what makes teaching successful (more is known about constraints), and there are no good methods for evaluating teaching quality and of pupils' understanding (and especially of more complex heuristic and problem-solving abilities). The last two points makes it difficult to make a pragmatic evaluation of different teaching methods.

The question of what is good research is as difficult as what is good teaching. I think that different criteria of research work and of reports are necessary but not sufficient. I miss something of what has been named a "theoretical evaluation." This is an evaluation based on a model explaining/predicting the context, factors, and processes that makes good teaching. But without a theory there could be no theoretical evaluation. (Let me say that the theoretical base of what research is, is as bad as that of what teaching is.)

As a small comfort to my conclusions, the situation is quite similar in my own field, and perhaps even more in fields like architecture (architects neglect research in architecture) and nursing (which is definitely not applied medical knowledge, and nursing research has a lot of difficulties).

Criteria for Scientific Quality and Relevance in the Didactics of Mathematics, is a very interesting reading. A disussion like this of methods and research approaches on an international basis is necessary for the development of a research field. The report also points out some crucial problems that will stimulate further discussions.

Göran Wallén, associate professor
Department for the theory of science and research,
Göteborg University, 412 98 Göteborg, Sweden
