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3. WHAT ARE THE SPECIFIC RESEARCH QUESTIONS OR *PROBLÉMATIQUES* OF RESEARCH IN MATHEMATICS EDUCATION?

Mathematics education lies at the crossroads of many well-established scientific domains such as mathematics, psychology, sociology, epistemology, cognitive science, semiotics, and economics, and it may be concerned with problems imported from these domains. But mathematics education certainly has its own specific *problématiques* that cannot be viewed as particular cases or applications of those from other domains. One question the ICMI study might address is that identifying and relating to each other the various *problématiques* specific to mathematics education.

There are certainly two distinct types of questions in mathematics education: those that stem directly or almost directly from the practice of teaching and those generated more by research. For example, the question of how to motivate students to learn a piece of mathematics (inventing interesting problems or didactical situations that generate a meaningful mathematical activity), or how to explain a piece of mathematics, belong to the first kind. The question of identifying students' difficulties in learning a specific piece of mathematics is also directly linked to practice. But questions of classifying difficulties, seeing how widespread a difficulty is, locating its sources, or constructing a theoretical framework to analyze it already belong among the research-generated questions. The problem is, however, that a difficulty may remain unnoticed or poorly understood without an effort to answer questions of the latter type; that is, without more fundamental research on students' understanding of a topic. Is it, therefore, possible to separate so-called practical problems from so-called research-generated problems?

Is it possible to admit the existence of two separate types of knowledge: the theoretical knowledge for the scientific community of researchers and the practical knowledge useful in applications for teachers and students? It might be helpful to reflect on the nature of these two types of knowledge, on relations between them, and on whether it would be possible to have a unified body of knowledge encompassing them both.

4. WHAT ARE THE RESULTS OF RESEARCH IN MATHEMATICS EDUCATION?

Any result is relative to a *problématique*, to the theoretical framework on which it is directly or indirectly based, and to the methodology through which it was obtained. This relativity of results, though commonplace in science, is often forgotten. One often interprets findings from biology, sociology, or mathematics education as if they were a kind of absolute truth. The reason

may be that in these domains we really want to know the truth and not simply whether, if one proposition is true, some other proposition is also true. Questions of biology, sociology, or mathematics education can be of vital importance and fundamental to survival and well-being.

Two types of “findings” can be distinguished in mathematics education: those based on long-term observation and experience and those founded on specially mounted studies. Are the former less “scientific” than the latter? Geoffrey Howson offers an example:

In the seventeenth century, Spinoza set out three levels of understanding of the rule of three (which, incidentally, can be viewed as an elaboration of the instrumental-relational model of Skemp and Mellin-Olsen expounded over three centuries later). This, like the well-known levels of the van Hieles, was based on observation and experience. On the other hand, for example, CSMS [Concepts in Secondary Mathematics and Science] used specially mounted classroom studies to develop and investigate similar hierarchies of understanding. Do we rule out the work of Spinoza as research in mathematics education? If we do, then we lose much valuable knowledge, especially that resulting from curriculum development. If we do not, then it becomes difficult to find a workable definition [of research in mathematics education].

Balacheff points out that it may be difficult to contrast, in this way, the hierarchies obtained by the van Hieles and the CSMS group. Besides the different ways in which these hierarchies were obtained, the van Hieles and the CSMS group may not have been asking the same kind of question. “What are these questions?” asks Balacheff. “What is the validity of the answers they provide? How is it possible to relate them?”

Can a new formulation of an old problem be a research result? Can a problem be a result? Or a questioning of the theory related to a problem, a methodology, or a whole *problématique*? Can a concept be a result? It might be useful to have a definite categorization of the things we do in mathematics education, and of the things we thereby “produce”.

Most people would probably agree that *making empirical investigations* is research. But is the *doing of practical things* research? Is *thinking* research? Can these activities be separated? Can a result be obtained without thinking and the doing of practical things? Should mathematics education be considered a science? Perhaps it is a vast domain of thought, research, and practice. What qualifies a domain of activity as scientific is the kind of validation and justification methods it uses. Proofs and experiments are considered scientific. But there are thoughts not validated in either of these ways that are valuable because they are filled with meaning.

What examples are there of what we consider results in mathematics education to be? What do we know today that we did not know before? What have we learned about the processes of learning and teaching? What do we know about mathematics that mathematicians were not aware of before?

Can we identify some categories of results? One category might be *economizers* of thought. Any facts, laws, methods, procedures, or theories that are general enough to direct our experience and predict its results will give us increased power over our teaching and learning. Another category might be *demolishers of illusions*. Results that undermine our beliefs and assumptions are always valuable contributions to the field. A third category might be *energizers of practice*. Teachers welcome research that helps them understand what they teach and provides them with ideas for teaching. The development of teaching materials, activities, and challenging problems belongs to this category. Other categories of results might emerge from epistemological, methodological, historical, and philosophical studies.

5. WHAT CRITERIA SHOULD BE USED TO EVALUATE THE RESULTS OF RESEARCH IN MATHEMATICS EDUCATION?

How do we assess the validity of research findings? How do we assess their worth? Should we use the criterion of relevance? What about objectivity? Or originality? Should we consider the influence research has had on the practice of teaching? What other criteria should we use?

The first problem is to clarify the meaning of terms such as *truth*, *validity*, and *relevance* in the context of mathematics education. A related issue is the question of what is knowledge as such. This is an even more fundamental question than that of validation. If we knew what kind of knowledge mathematics education aims at, we would be better equipped for answering the question of methods of validation.

It is also useful to understand the ways in which research results are used. How have the results of research in mathematics education been applied? How do teachers use the research? How do policy makers use it? By clarifying the uses to which research is put, can we develop better criteria for assessing its validity?

CALL FOR PAPERS

An ICMI Study on “What is Research in Mathematics Education, and What are Its Results?” will investigate the questions above, as well as others raised by various contributors, over the next year or so. The study will have two components: an invited *study conference* and a *publication* to appear in