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On priorities of research in mathematics education

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The purpose of this article is to provoke discussion on the priorities of research in mathematics education Mathematics education is a vast field, and there is always a possibility that no one direction stands out in its importance. Each researcher has some list of priorities and 'pet topics' I venture to propose a research program that could be the most fruitful research direction Actually, candidates for the role of the most important research direction can be found in the literature. For example, the late Professor Amitsur suggested the goal of understanding the nature of mathematical thinking (see Sfard's talk with Amitsur in Sfard, 1998) and Schoenfeld (2000) stated at the beginning of his paper that, "without a deep understanding of thinking, teaching, and learning, no sustained progress [in improving mathematics instruction] is possible" I respectfully disagree. We really do not know how the mind works Cognitive processes involved in mathematical learning are of astonishing complexity (Niss, 1999). A demand to understand the thinking processes, as a preliminary condition to working on improving mathematics instruction, is like saying that it is impossible to develop physics prior to understanding the nature of matter This aim might be compared with a Holy Grail: the ultimate goal being to provide an everlasting inspiration to generations of researchers This search can yield important results However, one cannot bank on its ultimate success, and so it is not of the highest priority from the practical point of view

Freudenthal (1981) formulated several problems in mathematics education. The first was, "Why can Jennifer not do arithmetic?" (p 135). Later, Freudenthal's questions were repeated in a way similar to the famous Hilbert unsolved problems of mathematics (Adda, 1998). In his later book, *Revisiting mathematics education*, Freudenthal (1991) writes, "[it] is not a question anymore, because today Jennifer is eleven and excels in arithmetic" (p. 174). So, mechanisms of learning are certainly interesting from a theoretical point of view Better understanding can facilitate learning of children in general, and especially of children with difficulties. But the fact that Jennifer (i e just a regular child for this matter) can do arithmetic without relying too much on research in mathematics education is crucial. It is not the most important problem, not Problem One. I once heard a remark attributed to Otto von Bismarck, that the Franco-German war of 1870 was won by a school teacher, meaning that the German soldiers had a higher level of literacy (basic skills in reading, writing and arithmetic) Since then, long experience of developed countries has shown that if the government supplies enough competent teachers, they succeed in implanting these skills in most children, no matter what the (reasonable) methodology. The basic method is sufficient practice in a supportive atmosphere. As far as arithmetic learning is concerned, wars will not be won by educational researchers.

Niss (1999) gave the following definition:

> The didactics of mathematics, alias the science of mathematics education, is the scientific and scholarly field of research and development which aims at *identifying*, *characterising*, and *understanding* phenomena and processes actually or potentially involved in the *teaching* and *learning of mathematics at any educational level* (p 5)

This assumes that the subject of learning (i e mathematics) is taken for granted, and this is exactly what I want to question. The scope of mathematics is in perpetual change. For example, in the Pythagorean tradition, music is a part of mathematics while logic is not! In medieval groupings of subjects for educational purposes into the *trivium* and the *quadrivium*, geometry and arithmetic composed the *qua-drivium* together with music (harmonics) and astronomy The *trivium* was formed of logic (dialectic) together with grammar and rhetoric Indeed, prior to the middle of the nineteenth century, logic was not recognized as a part of mathematics

Ours is a dynamic society For the educational context it creates new needs, unexplored by previous generations of educators Formulation of these needs is not a simple task, and even more so supplying these needs As an illustration let us consider the junction between secondary and tertiary education on the one, and between mathematics and computer science education on the other hand. A typical student I have in mind is an individual, not naturally inclined to study mathematics, learning it exclusively for practical purposes (e g to find a decent job). Most of these students will work in computer-related technologies, that is, their needs in mathematics are closely connected to various computer applications. From the point of view of a mathematics educator there *never* was so large a population that needs a solid mathematical background just to earn their living.

> *The sort of mathematics that arises in a computing context is not necessarily what most people would consider to be mathematics at all.* Its character may seem more like that of 'mere' organization, symbol management, or data manipulation. (Truss, 1999, p v; *emphasis added*)

There is no tradition in teaching this kind of mathematics to such a population: many topics of mathematics needed for work with computers used to be purely mathematical research areas before the dawn of the computer age and were taught exclusively to the mathematically-inclined minority.

Most teachers of these subjects are unaware of the problems of the newly-arrived population and take for granted various abilities of their students. Their assumptions are wrong: the students do not have these abilities So here is the problem: the new type of students, "regular individuals", have to learn material previously supposed to be taught to elites only This demands a novel approach to teaching, not just teaching new material with traditional tools

The new character of the task is essential here There is a chronic lack of qualified hi-tech specialists in most developed countries. The reason is that the required skills are unnatural to most humans and there is no tradition of preparing such specialists So, this is the chance for researchers in mathematics education, and if they deliver, future bismarcks could thank them (hopefully in a peaceful context).

I believe that the following three-step research program is the most important from a practical point of view and has the best potential to make a substantial contribution of research in mathematics education to society.

1. Discerning the mathematical needs of our students: this includes goal-orientation analysis of various professional activities as well as the mathematical needs of an 'average' individual in general context (see Khait, in press, for a discussion)

2. Formulation of the teaching goals according to these needs.

3. Suggestions concerning proper educational environments (teaching methods) that facilitate achievement of these goals

Concerning point 3, let us compare learning mathematics to the question of how humans learn to talk The latter is one of the great riddles; it is of vast scientific importance Cognitive processes associated with learning to talk are certainly of great complexity. However, the skill at talking is acquired by most children without a need for specialists, by practicing with their families. Opportunities for practice naturally arise in everyday life. This is not the case with complex mathematics. Appropriate learning opportunities have to be created, after deciding on the learning goals. So the stress should be put on *what* to learn, and then, *how* to construct a supportive atmosphere for sufficient practicing

To pinpoint the most important field for educational research, I introduce a scale of learning tasks beginning from the basics The first two items in this scale could be:

1. Most basic skills like speech, typically acquired without the need for specialists. In practical terms, research in speech education is needed only in pathological cases.

2. Literacy (reading, writing, arithmetic). These skills demand explicit teaching. However, most children, in reasonably good schools, master them without any extraordinary effort. There is a minority of students with learning problems, who could be helped by researchers

On the other end of the scale there is advanced mathematics at research level, intractable for all except a tiny minority. There is no need to teach these topics to anyone else, so educational research is not too important here.

There is an intermediate interval on the scale, where the supply of adequate experts does not meet the demand. Preparation of such specialists involves inculcation of abilities that do not come naturally. *This is the point where educational research is most needed.*

There is no contradiction between the research direction I am emphasizing and those formulated by Niss (1999) in his definition quoted above. Moreover, the search for adequate methods of teaching non-traditional mathematics contributes to the understanding of cognitive processes. Creating new teaching methods is inseparable from *"identifying, characterising,* and *understanding* phenomena [...] involved in the *teaching and learning of mathematics*", thereby providing new specific contexts for study.

The ideas presented above have been developed while meditating on the nature of the core of mathematics education, as presented by Wittmann (1995), who advocated seeing mathematics education as a 'design science'. I want to add to his proposal that consideration of new non-traditional areas is potentially the most promising direction of research in mathematics education

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What would your suggestions be for the "research problems whose solutions would make a substantial contribution to mathematics education"? You will find an earlier set of responses to this request in FLM 4(1), 40-47, from 1983. You will also find a pdf file of that article on the website at http://flm.math.ca. Would some or all of the problems be the same today? Would you suggest changes? Significant additions? Send your contribution to this discussion to flm-editor@bris.ac.uk (eds).