Moreover, there were **two research fora** on *Researching the Nature and Use of Tasks and Experiences for Effective Mathematics Teacher Education* (coordinated by Peter Sullivan and Orit Zaslavsky) and *Problem Posing in Mathematics Learning and Teaching: A Research Agenda* (coordinated by Florence Mihaela Singer, Nerida Ellerton, Jinfa Cai and Eddie Leung).

The following activities completed the offering:
161 research reports, nearly 200 short orals, nearly 100 poster presentations, 8 discussion groups and 5 working sessions.

An interesting national presentation was given by Turkey, with the coordination of Prof. Behiye Ubuz, about the doctoral programmes in mathematics education, mathematical thinking research and mathematics teacher education in Turkey.

Turkish mathematics educators gave the impression of a lively and competent community in the field.

"Solid findings" in mathematics education

Introduction

This series of articles is about the learning and teaching of mathematics. It has been prepared by the Education Committee of the European Mathematics Society (http://www.euro-math-soc.eu/comm-education.html). As part of its mission, this committee has decided to provide brief syntheses of research on topics of international importance. Each article aims to summarise an interesting and important "solid finding" of research on mathematics learning and teaching. As will be elaborated below, by solid finding we mean findings that:

- result from trustworthy, disciplined inquiry, thus being sound and convincing in shedding light on the question(s) they set out to answer.
- are generally recognised as important contributions that have significantly influenced and/or may significantly influence the research field.
- can be applied to circumstances and/or domains beyond those involved in this particular research.
- can be summarised in a brief and comprehensible way to an interested but critical audience of non-specialists (especially mathematicians and mathematics teachers).

The articles are, in an important way, different from (extensive and specialised) state-of-the-art papers and handbook chapters that review a particular topic in mathematics education, such as early childhood mathematics education, mathematical modelling and equity in mathematics education. Besides typically being (much) longer, most of these publications try to look at what seem to be the most important fundamental and unresolved questions, challenges and opportunities now facing research and development in mathematics education, to propose new conceptualizations of or perspectives on research problems and to suggest possible research programs to move the field forward. The primary goal of our summaries is rather to try to answer in a clear, straightforward and non-controversial way what we already do know from research about this topic or aspect of the teaching and learning of mathematics, what our research community learnt from approaching this theme with a lens that is different from how it was seen before and what this may tell us about how to improve the teaching and learning of mathematics.

Our audience

Our primary audience is, first of all, the group of mathematicians and researchers from adjacent disciplinary fields who are also directly or indirectly confronted with mathematics, mathematical thinking and mathematics education, such as psychologists, sociologists and anthropologists. Other important audiences are teacher educators, curriculum developers, policy-makers and test developers, as well as other people from outside the field who want to understand what mathematics education research is all about and what its relevance for those outside the immediate community might be.

For each article we will try to identify a significant and robust phenomenon, describe and explain it by making use of well-known and highly valued theoretical analysis and empirical research and then say something about what one might be able to do about this phenomenon from a mathematics educational point of view.

The overall message of our set of "solid findings" will not be that research in mathematics education has yielded unquestionable explanations and straightforward solutions of these phenomena. Instead, the main message is rather that things are more complex than one might think; but also that, particularly for the phenomena that have been selected as topics for our book, the community of mathematics educators has realised a serious and widely recognised breakthrough in identifying the complex nature of these phenomena, in understanding their impact on mathematics education and in suggesting potential solutions.

Mathematics education as a research field

People from outside our research community may be surprised that we consider mathematics education as an independent discipline. Against the background of the radical changes in psychology from the late 1950s onward, research in mathematics education has started to emerge as a field of study in its own right. Bishop (1992) proposed that there were three identifiable traditions reflecting the state of research in mathematics education around 1970: the empirical-scientist tradition, the pedagogue tradition and the scholastic-philosopher tradition. To a considerable extent, research in mathematics education was conducted within the empirical-scientific tradition, relying heavily on psychological theory and methodology. However, the limited perspective of such research, wherein all aspects of interest are treated as "variables" that can be defined and measured and wherein teaching is taken as "treatment" and learning as "effect", has always been open to criticism from the perspectives of the other two traditions. Relying on a deep knowledge of mathematics and/or a rich experience of how to teach it, leading to well-elaborated and strongly held views on the nature of mathematics and/or how it should be taught, scholars from these two other traditions reacted especially against the assumption that a theory of mathematics education could be derived from a domain-independent theory of cognition, learning or education. Increasing interactions between researchers and scholars working within different perspectives led to the first intimations of the idea that mathematics education could be delineated as a field of study in its own right while retaining strong links with other disciplines (Vergnaud, 1982). Of these other disciplines, psychology and mathematics itself remained, of course, pre-eminent but, increasingly, interest is being taken in the work of sociologists, linguists, anthropologists and historians, reflecting the increasing recognition of mathematics education's "situatedness" in social, cultural and historical contexts (De Corte, Greer & Verschaffel, 1996).

Important contributions to (and, at the same time, external signs of) the emergence of an identifiable community of mathematics education researchers were: firstly, the organisation of the first International Congress of Mathematical Education (ICME) in 1969 and the International Group for the Psychology of Mathematics Education (PME) in 1977; secondly, the formation of important research institutes in mathematics education in many countries (such as the Shell Centre for Mathematics Education at Nottingham University, the Freudenthal Institute at Utrecht University and the Institut für Didaktik der Mathematik in Bielefeld, or the Department of Mathematics Education at the University of Georgia in Athens); and, finally, the establishing of several domain-specific journals (such as *Educational Studies in Mathematics, For the Learning of Mathematics, International Journal for Science and Mathematics Learning, Journal of Mathematical Behavior, Journal of Mathematics Teacher Education, Journal for Research in Mathematics Education, Mathematical Thinking and Learning, Recherches en Didactique des Mathematiques* and *Zeitschrift für Didaktik der Mathematik)* and book series (e.g. Kluwer's *Mathematics Education Library)*. Over the last few decades, the task of self-definition of mathematics education as a research field on its own has largely been accomplished (Sierpinska & Kilpatrick, 1997).

So, there now exists a recognisable body of research within an identifiable community of mathematics education researchers – which means not only that this research is conducted within the realm of mathematical cognition, learning and teaching but also that the specific and unique nature of the mathematics domain has been seriously taken into account in all aspects of the work: framing research questions, choosing a mode of investigation, designing instruments, collecting data, interpreting results and suggesting implications (Grouws, 1992, p. ix). Certainly not all studies that are classified as mathematics education research give the mathematics involved the same attention nor is each study equally sensitive to each of the aspects of investigation mentioned. However, in our series, we will primarily look for "solid findings" that result from research that has put the specificity and integrity of the domain at the centre of the work rather than simply applying models and theories from other fields.

Criteria for "solid findings"

Our choice of solid findings was necessarily eclectic. Major criteria for the choice of solid findings were those that have been put forward by Schoenfeld in his chapter on "Method" in Lester's *Second Handbook of Research on Mathematics Teaching and Learning*. According to Schoenfeld (2007), good research in mathematics education must be examined along three somewhat independent dimensions.

The first – trustworthiness – refers to the degree of believability of the claims made in a single piece of research or a group of studies about a particular topic. There are various criteria for the trustworthiness of (empirical) research: its descriptive and explanatory power, its possibility to predict and falsify, its rigour and specificity, its replicability and whether the research makes use of multiple sources of evidence (so-called triangulation). Of course, not every criterion from the above list is relevant for every phenomenon to be studied and for every piece of research but trustworthiness is anyhow an essential criterion for good research. However, it is not enough. A study may be trustworthy but trivial, along one or both of the other dimensions of generality or importance.

Generality (or scope) refers to the question: how widely does this finding, this idea, this theory apply? In this respect, Schoenfeld distinguishes between claimed, implied, potential and warranted generality as ways to think about the scope or generality of a piece of research. So, although researchers mostly tend to study learning within clearly specified content domains, age groups and cultural and educational settings, some pieces of research have offered a number of important constructs for thinking more broadly about mathematical thinking and learning that extend beyond the bounds of the individual studies in which the constructs were expounded. A complicating factor in this respect is the cultural dependency of findings and recommendations concerning mathematical thinking, learning and teaching: what is working in China or Finland might fail in the U.S. or Japan. Where the mathematical statement $2 * 2 = 4$ may be true everywhere, this is not the case in mathematics education.

Thirdly, there is the importance criterion, which addresses the question: does it matter? What is the (actual or potential) contribution of this piece of research to theory and practice, and how important is this contribution? Of course, importance is to a large extent a value judgment. As in any other field of study, beliefs about what is essential and what is peripheral are not static but change over decades, reflecting trends both within and beyond the discipline.

From our perspective, we would like to emphasise that Schoenfeld's three major criteria for the solidity of a finding do not necessarily (and maybe not even primarily) rely on one particularly important and general piece of research but also, and even preferably, rely on a research line consisting of a larger set of related studies that together yield such a "solid finding". So, the term "solid" also includes an aspect of "robustness" in the sense that the finding should be repeatedly observed or confirmed in many studies reporting the same or similar results leading to the same (general) conclusions.

To these three criteria proposed by Schoenfeld (2007), we have added a fourth, which has to do with the specific aims of our series and its primary audience, namely the need for addressing phenomena, findings and insights that (a) have the potential to attract or surprise mathematicians and mathematics teachers and to be considered by them as (directly) useful to their teaching practice, and (b) can be clearly described and illustrated in a very brief way without reliance on technical language that is incomprehensible to people from outside the field.

In summary, in our search for "solid findings", we have looked for and ultimately selected lines of research that have converged on a particular and clear point and fit well within a larger line of "disciplined inquiry", thus being sound and convincing in shedding light on the questions they set out to answer. Moreover, the solid finding should be generalizable in that it can be applied to circumstances and/or domains beyond those studies themselves and it should address issues that do matter both to people inside and outside the community. Finally, it should be summarisable in a brief and comprehensible way to an audience of non-specialists.

A framework for conceiving and presenting "solid findings"

In the description of each solid finding, an attempt will be made to address its mathematical-epistemological, its psychological, its didactical and its institutional dimensions. Moreover, we will follow the same overall structure in describing these different solid findings, addressing the following five (sets of) questions:

(1) What is the *genesis* of the *issue or problem* being addressed by the solid finding? What is the motivation to consider the issue or problem? Why is it relevant and to whom?

(2) What is the *genesis* of the *solid finding* as an answer to the issue or problem addressed? What exactly is the finding? What is its nature (e.g. is it essentially a theoretical construct, an empirical or experimental result, an aggregation of several subfindings?). What were the first seminal publications?

(3) What makes the solid finding *solid*, and what is the *evidence* for its solidity? What is the nature and state of this evidence (e.g. theoretical, empirical, experiential, a mixture)? What are the conditions for the solid finding to hold, and hence what are its limitations? This includes cultural, societal, institutional and organisational aspects. The key references for the solid finding will be given here.

(4) What is the actual or potential *impact* of the solid finding on mathematics education practice (e.g. on curriculum design, teaching, assessment, teacher education)? For whom is the solid finding significant and in what ways?

(5) What are the *open questions* around and beyond the solid finding? What is not yet known in relation to the solid finding and are there promising and accessible ways that could provide new knowledge and insight in this respect?

Authorship

Even though certain authors have taken the lead in each article, all publications in the series are published by the Education Committee of the European Mathematics Society, with all committee members listed inside each publication.

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