

# PLENARIES

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## OPENING PLENARY

### STEPS TOWARDS A NEW EPISTEMOLOGY IN MATHEMATICS EDUCATION

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**Abstract.** *This talk will centre on a number of inadequacies that beset mathematics education in present-day societies. In the author's view, the basic obstacles lie in – or can be expressed in terms of – the epistemological “regime” of the knowledge imparted by most scholastic institutions, as well as in its main social, cultural, and political correlates. It is therefore an essential responsibility – though not a monopoly! – of researchers in didactics to contribute to the advent of a new school epistemology, more in tune with the needs of our time – a crucial pursuit on which this presentation simply aims to shed some light.*

Madam Chairman, Ladies and Gentlemen, dear colleagues, allow me to thank the programme committee for its kind invitation to deliver the opening lecture of the 4<sup>th</sup> congress of the European Society for Research in Mathematics Education.

Let me first say a few words about my usual occupation in Marseilles, France, where I live and work. For several decades now, I have been involved in, at first, in-service teacher training and, later on – over the last twelve years or so –, pre-service teacher training. Mathematics teacher training is one of my two main commitments to mathematics education. The other one is doing *research* in mathematics education; or, more exactly, in the *didactics* of mathematics – a notion I shall comment upon in what follows. It is essentially from these two vantage points that I'll try to consider the situation – which is, in my view, a perplexing and difficult situation – of mathematics education today.

To do so, I'll try to determine what *didactics* is; or, more prudently, what it can be construed to be. According to dictionary definitions, the *noun* “didactics” refers to the science, or art, or profession of teaching – dictionaries refrain from choosing. It derives from the Greek *didaktikos*, which means (or meant) “skilful at teaching”. And it is akin (through Latin) to such words as *docile*, *doctor*, and *disciple*. The idea behind *didactics* is that someone attempts to do something so that someone – generally, someone *else* – learns something. The *adjective* “didactic” refers to a cultural posture existing from time immemorial. It is a posture so vividly identified in our European cultures that “didactic” has come to caricature what it normally simply

intends to depict – as when it is applied to someone striving to instruct someone else “even, as a dictionary puts it, when it is not welcome or not needed”.

What I shall henceforth call a *didactic fact* is any fact that can in some way be regarded as the effect of a socially situated wish to cause someone to learn something. Let me add – this is a more difficult point, on which I shall not dwell any longer – that a didactic fact is considered to be so only to the extent that it is effective in influencing the learning process. However, the meaning I shall assign to the noun *didactics* will be a little more liberal, with a view to encompassing an even wider range of phenomena. Didactics should, in my view, be defined as *the science of the diffusion of knowledge* in any social group, such as a class of pupils, society at large, etc. This “definition” requires some comments. In the first place, let me emphasise that its referring to a *science* is no writing automatism. It points to the fact that research – in mathematics education, for example – *is not enough*. Science is both a *process* of gaining knowledge, and the *organised body of knowledge* gained by this process. (It happens that, in didactics, the knowledge gained and organised is about... the diffusion of knowledge!) Doing didactics is therefore not only just “doing research”, and, consequently, producing pieces of knowledge; it is also, inseparably, organising these pieces into a body of knowledge – *didactics* –, with an experimental (or clinical) basis *and* a theoretical superstructure endowed with a paradoxical capacity, that of strengthening its empirical foundation. The true social aim of research is to make new knowledge available to the world. This indeed is a lofty goal, but without it doing research would be almost entirely useless. In this respect, allow me to conjure up with gratitude and esteem the tall, elegant figure of that prominent mathematics educator, Hans-Georg Steiner, who passed away a few months ago, and who so aptly argued in support of a *theory* of mathematics education.

My second comment will be about the *nature* of the knowledge whose diffusion will be studied. The answer to this question can certainly be expressed in terms of “bodies” of knowledge: if we do so, didactics becomes the scientific study of how bodies of knowledge percolate through human groups. This is essentially the formulation I used, a quarter of a century ago, within the framework of the didactic transposition theory. In order to go further, however, one has to raise an almost puerile question, which is: the knowledge whose percolation is to be studied is the knowledge *of what*? In other words, what is the *object* of that knowledge? My answer will be formulated in terms of a key notion that I’ll have to describe to some extent: the notion of *praxeology*. Some dictionaries define praxeology as the study of human action and conduct. Up to a point, this is not foreign to the use I will make of that key word of the anthropological approach to didactics – provided we include in “praxeology” the study, not only of what people *do*, and *how* they do it, but also of what they *think*, and *how* they do so. In that sense, didactics includes praxeology, or at least some part of it, because the knowledge percolating through society is about human ways of doing and thinking: the didactics of mathematics, for example, is bound to accommodate a “praxeology of mathematics”, that is, a scientific

description and analysis of what we, human beings, do and think when we “do mathematics”. But what I shall call *a* praxeology is, in some way, the basic unit into which one can analyse human action at large. (The concept of *a* praxeology is therefore basic to praxeology as a science – in the dictionaries’ definition of the word.) What exactly is *a* praxeology? We can rely on etymology to guide us here – one can analyse any human doing into two main, interrelated components: *praxis*, i.e. the practical part, on the one hand, and *logos*, on the other hand. “*Logos*” is a Greek word which, from pre-Socratic times, has been used steadily to refer to human thinking and reasoning – particularly about the cosmos. Let me represent the “praxis” or practical part by *P*, and the “logos” or noetic or intellectual part by *L*, so that a praxeology can be represented by [*P/L*]. How are *P* and *L* interrelated within the praxeology [*P/L*], and how do they affect one another? The answer draws on one fundamental principle of ATD – the anthropological theory of the didactic –, according to which no human action can exist without being, at least partially, “explained”, made “intelligible”, “justified”, “accounted for”, in whatever style of “reasoning” such an explanation or justification may be cast. *Praxis* thus entails *logos* which in turn backs up *praxis*. For *praxis* needs support – just because, in the long run, no human doing goes unquestioned. Of course, a praxeology may be a *bad* one, with its “praxis” part being made of an inefficient technique – “technique” is here the official word for a “way of doing” –, and its “logos” component consisting almost entirely of sheer nonsense – at least from the praxeologist’s point of view!

Let me add here two or three more remarks. First, in the anthropological approach to which I have contributed for more than two decades, all human forms of activity are supposed to result from the bringing into play of praxeologies. When I blow my nose, for example, I draw upon some praxeology, which may vary according to the culture in which I was brought up. When I walk, I also put some praxeology to use, and this praxeology may well vary according to gender, milieu, and so on. Following the French anthropologist Marcel Mauss (1872-1950), I will say that a praxeology is a “social idiosyncrasy”, that is, an organised way of doing and thinking contrived within a given society – people don’t walk, let alone blow their nose, the same way around the world. My second remark builds on the first one: the concept of praxeology is a generalisation of the notion used previously, that of “body of knowledge”. Indeed, many or even most praxeologies of ordinary life are denied the status of “body of knowledge” – who would accept that blowing one’s nose or walking in a park means bringing some duly learnt “body of knowledge” into play? (Up to a point, I would!) In the following, I shall therefore stick to describing human action in terms of praxeologies, without inquiring whether people generally regard them as “true” bodies of knowledge or as simple know-how, or even as a “natural” endowment – most people think breathing is something natural, for example. Third remark: human praxeologies are open to change, adaptation, and improvement. If I have to write the number

$$A = \left( \frac{2}{1 + \sqrt{3}} \right)^4$$

in standard form (i.e.  $a + b\sqrt{3}$ , where  $a, b$  are rational numbers), I can know *that*  $x = 1 + \sqrt{3}$  is a non-zero root of a quadratic equation, and I can know *how* to generate this equation, which is  $(x - 1)^2 = 3$ , or  $x^2 - 2x = 2$ . It then follows that

$$\frac{2}{x^2} = 1 - \frac{2}{x} = 1 - (x - 2) = 3 - x,$$

and therefore that

$$\frac{4}{x^4} = 9 - 6x + x^2 = 9 - 4x + 2 = 11 - 4(1 + \sqrt{3}) = 7 - 4\sqrt{3},$$

so that  $A = \frac{16}{x^4} = 4(7 - 4\sqrt{3}) = 28 - 16\sqrt{3}$ .

Let me indulge in one more example. Marta's car uses 35 litres of unleaded petrol to drive 320 kilometres; how many litres will she use to drive 640 kilometres? In this case, the answer is easy – Marta's car will use *2 times* 35 litres, that is to say 70 litres. But what about driving 950 kilometres? Most non-mathematical people, I suspect, would recoil at the idea of confronting such a problem! Now, the following, very simple technique will do the trick: to do a number  $x$  of times 320 kilometres, the car uses  $x$  times 35 litres of petrol; so, to do 950 kilometres, that is to say  $\frac{950}{320}$  “*times*”

320 kilometres, the car will use  $\frac{950}{320}$  times 35 litres, which, according to the calculator in my cell phone, equals 103.90625 litres, or approximately 104 litres of (unleaded) petrol!

The preceding technique is the key component of the “praxis” part of an arithmetical praxeology useful whenever proportionality is involved, and which, therefore, will make life more pleasant, closer to “good life”. Making life more pleasant is, since classical antiquity at least, the main pursuit of all peoples and cultures. (Everyone knows, I presume, the assertion in the preamble to the “*unanimous Declaration of the thirteen united States of America*”, adopted on the 4<sup>th</sup> of July, 1776, that all men are endowed “with certain unalienable Rights”, and that, “among these are Life, Liberty and the pursuit of Happiness”. The preamble to the “Treaty establishing a Constitution for Europe” less boldly puts forward that “The Union's aim is to promote peace, its values and the well-being of its peoples”...) Now it is worthy of note that, on the subject of proportionality, the ancient Greeks could never reach – if I may say so – the technique I presented above, because their *logos* could not accept the metaphor that makes a fraction into a full-fledged number and allows one to speak of a *fractional* number of *times* exactly as if it were a *whole* number of times. In this respect, the ancient Greeks never found the “North West passage” leading from whole “natural” numbers to “artificial” numbers.

All praxeologies, and among them those intended to make life better for all, are “artificial”, that is, products of human cultures. The age-old, ever recurring bias against things contrived “by art” seems to be a permanent cultural trait across the world and throughout mankind’s history. Human societies take fright at humanly creations, as if these anthropic additions to nature were offences against the orderly cosmos, and a misdeed for which societies should punish themselves by decrying and, eventually, rejecting the artefacts they bring to life as steadfastly as they repress them. Surprising though it may sound, this repression is, in my view, basic to what happens in our societies as concerns the diffusion of knowledge, that is, the diffusion of praxeologies, which are the very substance human action is made of. Praxeologies are artefacts, and, conversely, all artefacts are praxeologies or *ingredients* of praxeologies invented to give praxeologies flesh and bones. (A city, or a theatre, are ingredients of praxeologies which allow us, respectively, to live together “urbanely”, and to flock together for leisure or instruction...) Now the ambivalence towards artefacts engenders a kind of splitting of artefacts, and therefore of praxeologies, each of them being split into a “good” object and a “bad” one. This is a crucial point. The *bad* one is the praxeology as such, that is, an organised body of notions, ideas, statements, justifications and explanations – the *logos* part – and of ways of performing a certain *type of tasks* (solving quadratic equations, blowing one’s nose, composing a fugue, or achieving no matter what), which make up the “praxis” part. The *good* object is the praxeology once we have blanked out what it was here for, that is the praxeology deprived of its inbuilt uses. In this fashion, praxeologies are soon turned into *monuments*, that is, things notable or great, fine or distinguished, but which, paradoxically, are effective in helping us to forget what they stand for – what exactly was the thing “monumentalised”. Everyone has heard, I suppose, the urban legend about the student who, having to solve a quadratic equation whose discriminant was 7, said: “If 7 is less than zero, then the equation has non-real, complex conjugate roots; if 7 equals zero, the equation has one real, double root; and if 7 is greater than zero, the equation has two real, different roots.” This indeed expresses something genuine about the epistemological regime of mathematical praxeologies diffused at school. These praxeologies are hardly instruments devised to gain insight into types of mathematical situations and to operate efficiently in those situations. They are rather bodies of knowledge that the student has to “visit”, and, if possible, honour and praise. The urban legend “If  $7 = 0$ ” is a gross distortion of the naked truth. Still, the prevailing mode of turning a taught praxeology into a school monument consists indeed in cutting it off, more or less surreptitiously, from the authentic mathematical situations whose treatment might reasonably call forth the praxeology in question. In this way, school propagates a relation to knowledge close to fetishism. Praxeologies are accordingly studied not for what they would allow us to do or to think, but for themselves. It’s knowledge for the sake of knowledge, and even, if I dare say so, know-how for know-how’s sake!

There is an easy way to make the current, “monumentalistic” school epistemology visible – by asking for the *reasons* why such and such praxeology or such and such

praxeological “ingredient” exists. Why do mathematicians seem so attracted to *triangles* for example? Why does geometry tell us about *angles*, *lines* and *rays*, or about *crossing lines* and *parallel lines*? Why does geometry make room for the notions of *acute angle*, *obtuse angle*, and *reflex angle*? If you are tempted to answer: “Mathematicians are interested in all these entities simply because *there do exist* crossing lines, rays, acute angles, reflex angles, etc., that is, just because these ‘things’ are *out there*, in the natural world, waiting for us to study them”, then you have been infected with the evil “monumentalistic” doctrine that pervades contemporary school epistemology. If indeed you accept such a poor, unspecific reply, it is more than likely that you have secretly espoused a naturalistic view of the human world – including the mathematical world –, forgetting that almost everything out there, as well as everything in our minds, is socially contrived. A straight line is a concept, not a reality outside us. It is something created in order to make sense of the outside world and to allow us to think and act more in tune with that reality. When dissonance grows too much, we invent – well, *some* people invent – a renewed *logos* and a changed *praxis*. Fractal geometry, for example, speaks differently about the same “given” world, for it goes far beyond the concept where Euclidean geometry stopped – the fictitious straight line. For every praxeology or praxeological ingredient chosen to be taught, the new epistemology should in the first place make clear that this ingredient is in no way a given, or a pure echo of something out there, but a purposeful human construct. And it should consequently bring to the fore what its *raisons d’être* are, that is, what its reasons are to be here, in front of us, waiting to be studied, mastered, and rightly utilised for the purpose it was created to serve. These are two necessary conditions for the diffusion of praxeologies to be meaningful. Why do we simplify fractions? What are the reasons for the seemingly irresistible urge to reduce them to lowest terms? Likewise, what are the reasons that, in some situations, make us speak in *percents*, which is almost the opposite of expressing a fraction in lowest terms? And, to crown it all, why do we spend so much time visiting that impressive and apparently inescapable monument called “Converting Fractions, Decimals, and Percents”? All these questions will have to be duly answered. However, there is more to it than that.

To take a global view of the problem school is faced with, we must consider a four-character play. The first character is society, the second is good life (or happiness, or well-being), the third is the bulk of praxeologies already existing or still waiting to be created, and the fourth is school. Society endeavours to achieve conditions of well-being for its members, and especially for its younger generations, through the creation and subsequent diffusion of praxeologies, thereby trying to put the right knowledge into the right place. There are mainly two ways to do that. The first has been amply criticised: it proceeds by diffusing praxeologies deprived of their *raisons d’être*, as if praxeologies were meaningful by themselves. The second way will sound familiar to whoever has once really called upon some determined piece of knowledge or know-how to achieve something, be it in scientific research or in ordinary life. Praxeologies travel through society because they are necessitated to solve problems,



or, as I shall put it, to *answer questions*. The basic situation in this respect can be summed up like this: a *question*  $Q$  is raised, and an *answer*  $A$  is searched for. The question may be for example “How can we live together peacefully?” or “How can we work with large numbers?”, that is, numbers that the calculator in my cell phone refuses to take care of. These questions are “practical” questions, because answering them amounts to providing a “technique”, in the first case allowing people to live peacefully together, and, in the second case, allowing people to work effectively with large numbers. However, an answer cannot be reduced to just a *praxis*. We know *praxis* ( $P$ ) eventually calls for some form of *logos* ( $L$ ), so that any answer is to be thought of as a part of a whole praxeology. Roughly speaking, an answer *is* a praxeology of a sort. And the migration of praxeologies through society can be explained in terms of questions and answers. In a given institution  $I$ , a question  $Q$  emerges; people in the institution seek an answer  $A$  to  $Q$ , that is, an adequate praxeology  $A = [P/L]$ . Generally, some supposed variant of the wanted praxeology exists somewhere within society. The people in the institution therefore have to locate that praxeology, and then make a copy of it. “Copying” is not the right word here. What happens is a *reconstruction* process that I called – years ago – a process of *transposition*. The original praxeology, let me call it  $[\Pi/\Lambda]$ , is transposed into a new praxeology,  $[P/L] = [\Pi^*/\Lambda^*]$ , supposed to be better at surviving the constraints imposed on both its “praxis” part  $\Pi^*$  and its “logos” part  $\Lambda^*$  by its new habitat,  $I$ . The *raison d’être* of praxeology  $[P/L]$ , the reason why this praxeology is now present in  $I$ , becomes clear: it has been brought into this institution because it was expected to solve a problem, to answer a question. It was wanted for just that reason – not for itself, however sophisticated it is.

The two questions I took as examples show that an answer  $A$  to a question  $Q$  does not always exist, and that, when it does, uniqueness is not sure. There certainly exist many “ways of life” to ensure that people will *not* live in peace, but we know of no way of life that would be a complete and perfect answer to the first question raised. Likewise, we do not doubt, I suppose, that there are several ways, not all easy, of working with large numbers. It is now time for me to introduce the third character, school. School is a manifold concept. As you probably know, the words “school” in English, “escola” in Catalan, “escuela” in Spanish, “école” in French, etc., all go back via classical Latin *schola* to Greek *skholè*. Originally, *skholè* meant “leisure” and gradually developed through “leisure used for intellectual argument” to “studious leisure, study”. The idea I would like to put forward is that, following in the wake of the ancient Greeks, European societies and their many institutions equipped themselves with different forms of *skholè*, that is, with institutions designed to allow for that specific need of human groups, finding answers to questions that beset them. A *skholè*, if I may say so, is therefore organised around the study of a number of questions  $Q$  to which the *skholè*’s students seek to give answers  $A$ . Two aspects have to be emphasised here. First, for that “scholastic” process of study not to be imposed on the students, it is necessary for the questions  $Q$  to be regarded – by the students,

by their teachers, and, so to speak, by the *skhole*'s "board of trustees" – as *crucial* to a better understanding and mastery of their lived world. Second, in studying questions  $Q$ , students will have to investigate many other, *derived* questions  $Q'$ , dynamically raised by the study of  $Q$ . Addressing these derived questions will lead to the transposition of many praxeologies  $A'$ , that will answer many unintended questions. In the long run, this basic phenomenon will turn the students (and their teachers) into "scholars" of a sort. For example, in studying some naïve question of ecology,  $Q$ , supposedly crucial to the well-being of their region, students may be led to learn a little bit of difference equations and a lot about photosynthesis, and something about many other subjects. However, a third point must be made clear. A major principle in trying to establish a new epistemology at school is that one should *not* go directly to the questions  $Q'$ , let alone the questions  $Q''$  generated by the study of  $Q'$  – unless questions  $Q'$  or  $Q''$  seem crucial to the *skhole*'s students and teachers. To go straight to a question  $Q'$  without being motivated to do so by the study of a previous, crucial question  $Q$  generally means that the study process is going adrift and will soon be replaced by the mere inspection of a succession of official monuments of knowledge, that is, the monumentalised praxeologies normally called forth by the study of question  $Q'$ .

This description of what should take place at school according to the new epistemology I refer to leaves the question of how to implement such a new epistemological and didactic "regime" open. One vital point here is that questions  $Q$  should be taken *seriously*, not as mere opportunities, soon forgotten, to bring up the study of some predetermined mathematical monuments, the way an illusionist conjures a rabbit out of a top hat. For several reasons, among them the wish not to surrender to the opportunistic spirit which pervades the old school epistemology, I have propounded a type of didactic structure that I tentatively propose to call, in English, a *study & research programme*. The French name is actually "*parcours d'étude et de recherche*", "study & research course", where course is to be taken in the sense, say, of a golf-course. A S&R programme is to be thought of as a part of the curriculum, together with several other such programmes. It is essentially determined essentially by the will to bring an answer,  $A$ , to some *generating question*,  $Q$ , but it is also determined by constraints imposed upon the study & research to be done by the existing curriculum – which, for example, will not allow the class to draw upon such and such advanced mathematical praxeology. Up to a point, then, it can be said that a S&R programme is underdetermined, or that it is a context-bound scheme. However, such a situation is not at all peculiar to the school management of knowledge: in any research lab, all over the world, what is going on does not depend only on the question studied, but also on all sorts of resources – including intellectual resources – that the lab can obtain. The relative lack of determination inherent in the notion of a S&R programme turns the study & research process which question  $Q$  generates (and not only starts up) into what study & research should be – an intellectual, human, and institutional *adventure*, which may develop along different routes, within the territory bounded by the curriculum.

Research on the didactic technology (and theory) of S&R programmes (or courses) is currently one of my main concerns as a researcher in didactics. This pursuit leads almost immediately to another key problem in the anthropological approach to didactics – a problem I have labelled the *dialectic of media and milieus*. Before I explain this terminology, let me say a few words about the joint use of “study” and “research” when it comes to labelling S&R programmes. “Study” is used here as a comprehensive term, meant to include research; but study at school often precludes research, if one understands this word in the sense I shall now try to make clear to you. The editor of a book entitled *The pleasure of finding things out*, gathering the “best short works” of Richard Feynman (1965 Nobel Prize in physics), writes in his introduction: “Another of the most exciting events, if not in my life, at least in my publishing career, was finding the long-buried, never-before-published transcript of three lectures Feynman gave at the University of Washington in the early 1960s, which became the book *The Meaning of It All*; but that was more the pleasure of finding things than the pleasure of finding things *out*.” Now it seems that most school study consists in *findings things* – as is the case with “documentary research” for example –, that is, finding works that can, equally well, either provide the needed praxeologies (in the framework of a S&R programme) or be turned into monuments that one will visit without even trying to find *out* what they are here for. The mention of “research” in “S&R programme” is intended to convey the idea that such a programme is designed to allow students to do research, that is, to “find things out”. This is where I can take up the “dialectic of media and milieus”. Students – and all of us indeed – are surrounded by *media*, a word I use here in a generalised sense, calling “medium” *any* social system pretending to inform some segment of the population or some group of people about the natural or social world. In such a comprehensive view, a course of lectures, for example, is a medium, and so is a textbook, and the same can be said of urban legends passed on by word of mouth... The problem that arises here can provocatively be formulated thus: how can a student ascertain that his/her teacher’s claims are not a sheer succession of fallacies and pieces of misinformation? This is where the notion of “milieu” comes in. “Milieu” should be understood here, not in its sociological sense, but in the sense given in Guy Brousseau’s theory of didactic situations to the concept of “a-didactic milieu”. To make a long story short, let me say that I call “milieu” any system that, as far as the question that you put to it is concerned, is devoid of intentions and therefore behaves like a fragment of nature – a system that intends neither to please or to displease you nor to defeat you of your hopes. In mathematics, of course, proofs are the chief traditional milieu in that sense – a deductive system does not try to comply with the mathematician’s wish... The dialectic of media and milieus is in my view the central problem of our time, at school *and elsewhere*, in building a *democratic* epistemological regime. Such a new social relation to knowledge should at long last eliminate the ubiquitous remnants of the social epistemology that flourished in what is known, in the history of my country, as the “*Ancien Régime*” (“Old regime”), that is, the regime that prevailed before the French Revolution, in which there existed, in

the classroom and elsewhere, almost no other milieu, at least for the general public, than the “master”, regarded as an authoritative source that, in retrospect, appears to be a poorly a-didactic milieu!

For a S&R programme to be effective as a means to get the younger generations to tackle a number of questions of interest to them and to society, a generating question  $Q$  must not only be “crucial” (and therefore legitimate). It must also have sufficient “generative power” to engender *many* questions open to study and research. In that respect, choosing generating questions is a crucial step. Who will take that step? It should be clear that this is where politics comes in. Choosing the generating questions and the main routes of study and research is at the same time a curricular and political matter, so that it is not up to researchers and educators alone to decide, even if they can voice their own views as experts and as citizens. However, I would like to conclude by adding a very short note on a too often misunderstood political concept, without which, in my view, it remains impossible to fully achieve democracy, namely the so-called “French” notion of *laïcité*, a concept that cannot be reduced to secularism, but that must on the contrary be *extended* much beyond religious considerations. According to that political principle, no vision of “good life” may be *imposed* – however surreptitiously – *on anyone*. A vision of “good life” usually includes views about religious matters; but it also includes views about “earthly” matters – literature, music, mathematics, and so on. The principle of *laïcité*, when applied rigorously, implies that, even in mathematics, when questions  $Q$  are raised and answers  $A$  are obtained, while it is legitimate to require of the students that they “know” the corresponding praxeologies and their *raison d’être* (and to be able to bring them into play relevantly when asked to do so), it would be *utterly illegitimate* to urge them to regard questions  $Q$  and, even more, answers  $A$  as, respectively, the right questions and the right answers to make life better – including mathematical life! Mathematics educators, in particular, are not asked to make students *love* mathematics, nor to hate it of course, but to *know* mathematics, which is quite a different and demanding task! Love, hate and indifference reside in each and every one of us. To ignore that principle would be in my view *utterly undemocratic* – not exactly what school is supposed to achieve.

## CLOSING PLENARY

### THE ROLE OF MATHEMATICS EDUCATION RESEARCH IN INFLUENCING EDUCATIONAL POLICY

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**Abstract.** *The paper first considers the aims of research in mathematics education in relation to educational policy and notes that the relation between the criteria for research quality and systemic impact is not straightforward. Specific examples are presented of basic, strategic and applied/practice-based research which have influenced policy in single countries or more widely. Finally some of the problems of being involved in policy-related research and development and of membership of policymaking bodies are discussed, with some recommendations for action.*

#### 1. WHAT IS EDUCATIONAL RESEARCH FOR?

There are many selfish reasons why we all engage in educational research, from intellectual curiosity to professional advancement. If we were funding our own research in our leisure time (as some of us effectively are) there can be no need to question our motives as long as we do not interfere with the rights and freedoms of others. However for those of us who are funded by public, charitable or private money, and/or who make demands of pupils, teachers and others in the course of our work, it is difficult to argue that a major purpose of mathematics education research should be to increase our knowledge and understanding in order to:

- improve the teaching, learning and assessment of mathematics;
- help build positive attitudes to mathematics and to its application.

The resulting contribution can be directly at the level of local or national policy, or may be at the level of the single or small group of classrooms, teachers or schools. But even in these cases there is normally the expectation that the influence of the work will spread so as to affect policy more widely.

#### 2. WHAT ARE THE CRITERIA FOR QUALITY IN EDUCATIONAL RESEARCH?

As in many other countries, educational research in the UK is funded from several sources. We are free to bid for limited funds to carry out specific research and development projects, sometimes in response to a public call for bids on a particular theme. The money may come from publicly funded research councils, from central or local government, from other public bodies, or from privately funded charities. The

charities are often associated with successful businesses or business families who may take an interest in particular aspects of education.

It used to be the case that all UK universities were publicly funded to carry out both research and teaching. However with the expansion of universities the funding of research activity by university staff is no longer automatic. For each university the amount per department is based on past research performance, as well as size and subject costs. The research performance of departments is judged every 4-7 years by a system-wide Research Assessment Exercise which provides an accountability measure.

I am personally very much involved in the question of the criteria we should use for judging quality in educational research, as I have the unenviable task of chairing the Education panel in the next UK Research Assessment Exercise (RAE), which takes place in 2008. Each panel is required to publish in advance the criteria and the working methods it will use to make the judgements, which are mainly based on the quality of published outputs, although research environment and individual and collective esteem are also taken into account. Each university is allowed to submit up to four outputs for each active researcher, mainly in the form of articles, chapters and books, each of which will be judged on a 4-point scale. On the basis of the resulting departmental quality profiles, departmental research funding will be decided for the next period of 4-7 years. In the case of education we are anticipating the need to assess about 8000 outputs from 2000 researchers (although relatively few of these will be in mathematics education). Draft criteria for judging research quality in each subject were issued in Summer 2005, and after consultation the final versions will be available in January 2006. (For details see [www.rae.ac.uk](http://www.rae.ac.uk)).

Generic guidelines require all types of output and all types of research to be valued equally and to be judged by the same three criteria of *originality*, *rigour* and *significance*. The Education panel has declined the opportunity of making a tight classification of types of educational research in the 2008 exercise and setting different criteria for quality, because we recognise that much research crosses any boundaries we might define. However we do on occasion refer to the possible characterisation of research as *basic*, *strategic* or *applied* which was used generically in the previous (2001) research assessment exercise. Broadly these categories differ according to purpose, although in practice they are likely to be clearly distinguishable:

- *Basic* research—aims to develop knowledge and understanding without any immediate practical outcome’ (‘blue skies research’);
- *Strategic* research—aims to inform practice and policy but not necessarily at a detailed level of implementation;
- *Applied/practice-based* research—aims to develop products/ artefacts/processes which have an immediate use.

In the 2008 RAE we have agreed to pay particular attention to *applied and practice-based* research which occupies:

‘an area situated between academia-led theoretical pursuits and research-informed practice, as consisting of a multitude of models in research explicitly conducted in, with and/or for practice’. (Furlong & Oancea, 2005, p.9)

Because of the need to ensure that applied and practice-based research is appropriately valued we have appointed five ‘user members’ to the panel to join the 16 university-based educationists.

Although we have adopted a single set of criteria which relate to *originality, rigour* and *significance*, we do note that the idea of *significance* can be differently interpreted according to its position on the basic/applied spectrum. I will quote the relevant passage in full as it relates to the different ways in which different types of research can influence policy, which is my concern in this paper:

‘*Significance* can be judged in different ways according to whether the research is basic, strategic or applied. Research has, or has the potential to have, considerable significance if it breaks new theoretical or methodological ground, provides new social science knowledge or tackles important practical, current, problems, and provides trustworthy results in some field of education. These results might be empirical or analytical and theoretical, providing new (and sometimes challenging) conceptualisations, and evidence for audiences ranging from academics to policymakers and practitioners. Ways of evaluating the significance of research include judging its effects or potential effect on the development of the field, examining contributions to existing debates, and assessing its impact or potential impact on policy and practice. The nature and degree of immediate impact on policymakers or practitioners will provide some useful indication of significance in terms of ‘value for use’. However, there may be reasons for high impact that are not dependent on research quality; and, equally, in many cases the observable impact of high quality research is achieved only over the longer term. Theoretical and more analytical research can also be of high significance if it takes forward the state of current international knowledge in its field, and has influenced, or has the potential to influence the work of other theoreticians. In education it is possible that such significant theoretical advances also influence practitioners and/or policymakers...’. (RAE2008, 2006)

This quotation suggests that the relation between high quality research and the development of policy is not straightforward. I will return to this in a later section when considering ‘evidence-based policy’.

### 3. HOW MATHEMATICS EDUCATION RESEARCH HAS INFLUENCED POLICY

In order to consider the ways that different types of mathematics education research have influenced policies for teaching, learning and assessing mathematics, I will discuss some examples in relation to the three categories of *basic*, *strategic* and *applied/practice-based* research. I will mainly use examples of policy from England and the United States, for which I apologise, largely because I am not familiar enough with other countries to understand the key factors underlying reform. However I believe that there are many common trends.

It is unusual perhaps for *basic research* to have a strong and identifiable effect on policy but it is clearly the case that Piagetian research directly influenced primary mathematics teaching and the primary curriculum in England from about 1950 onwards, as it did in many other countries. It is perhaps stretching a point to claim Piaget's work as mathematics education research rather than investigations in genetic epistemology, but parts of the research certainly dealt specifically with the way children understood and learned mathematical ideas.

The work of Polya on problem-solving is more clearly characterised as mathematics education research although less empirically based than that of Piaget, reflecting the contrast between Piaget's scientific background and Polya's mathematical/logical orientation. Polya's work probably qualifies as *basic* in relation to his formulation of mathematical heuristic processes although this seems likely to also be broadly *strategic*; certainly the films of Polya interacting with university students to support their problem-solving capabilities suggest an intention to affect policy and practice. The moves in the US and UK in the 1970s and 1980s towards teaching problem-solving rather than just tackling standard word problems in secondary mathematics were based on a new awareness of and emphasis on mathematical heuristics derived mainly from Polya's research.

It should however be noted that the work of one researcher alone rarely shifts national practice; the need for change and a direction for change is identified by leading figures in mathematics education and needs to be widely communicated and supported both within the mathematics education community and among policymakers. It is thus the influences on the people leading the change which are important; in many cases these are multiple and may not be clearly traceable. The main influences may not be from research or recognised theory, and even if they are may not be based on work by mathematics education researchers, but, for example, on that of sociologists. However the role of researchers can be critical in providing either a theory or empirical evidence to undermine current practice and to express the theoretical power or empirical efficacy of an alternative. The strength of the work of both Piaget and Polya is the wide theoretical and conceptual basis which allowed others to express these powerful new ideas, together with a level of practical detail which provided clear messages for implementation.



Before leaving the field of *basic* research it is worth considering the potential of neuroscience researchers such as Dehaene to strongly influence future mathematics education policy. Some leaders in the field of dyscalculia are already interpreting the basic results on areas of brain activity into courses of action, and it may not be long before this impinges on mainstream policy.

Moving into more *strategic* research which has influenced curricula, we can see strong influences of much of the post-Piagetian work, like that of CSMS (Concepts in Secondary Mathematics and Science) in the UK, Herb Ginsberg and Tom Carpenter in the US, and Gerard Vergnaud in France. For example, curricula in many different countries (e.g. Cyprus) now reflect the work of Carpenter and Vergnaud (Carpenter, Moser & Romberg (Eds.), 1982) on the characterisation of different models of the basic number operations. Research by Ginsberg (1977) and CSMS (Hart (Ed.), 1981) helped to shift the teaching of arithmetic away from standard algorithms and towards students' own methods, leading to changes which have underpinned the reform movement in the US and the National Numeracy Strategy in the UK. Teaching styles have also changed in response to research on grouping and on whole class teaching by researchers like Thomas Good and Doug Grouws. For example evidence presented to support a recent move towards whole class teaching in England actually depended crucially on the results of one much earlier study in the US by Good, Grouws & Ebmeier (1983).

A further group of studies which could broadly be classified as *strategic* are the various international surveys of achievement, in particular those organised by the IEA (FIMS, SIMS, TIMSS) and OECD (PISA). These have been probably more influential in policy terms than any others, and eventually also in some countries have had a clear effect on practice. It might be possible to argue that these properly constitute *basic* research since no specific outcome is intended, but even from the initial FIMS study it is clear that there were other motives behind the work, in that case to consider the evidence for non-selective secondary education (Husen, 1967). More recently the process of international comparison has been increasingly political, as the world has become more globalised and more driven by statistical indicators. This means that the selection of who designs the surveys is critical; for example the first PISA survey which used mainly complex modelling problems produced a rather different ranking of countries from TIMSS. (However in the PISA and TIMSS 2003 surveys rankings are more closely aligned, perhaps because PISA this time incorporated some of the TIMSS items.) National surveys and more local evaluations of attainment have also provided good examples of influential strategic research which has in the US, and to a lesser effect in the UK, been used by both sides in the 'Math Wars'.

The difficulty of categorisation into *basic* or *strategic* has already been demonstrated, since strategic aims may drive what appears to be basic research. Equally there are many educators whose research starts off as *strategic* but who later become involved in *applied/practice-based* research by translating their findings into curriculum

specification, classroom resources or teacher development, and continuing to research the impact of these. Aspects of this are known as ‘design research’ or ‘didactical engineering’. Tom Carpenter’s work already referred to illustrates this trend; he translated his studies of pupils’ development into teacher education and then into curriculum development in the influential CGI (Cognitively Guided Instruction) project. Herb Ginsberg has followed a similar trajectory in relation to early years’ mathematics.

In Europe Hans Freudenthal’s work was mainly analytical rather than empirical but at a *strategic* level definitely oriented to achieve changes in policy and practice. Later in the IOWO and Freudenthal Institute work on RME (Realistic Mathematics Education) his inspiration led into development of curriculum, materials, assessment and into teacher development, all of which constitutes *applied/practice-based* research. This work has again been hugely influential on national policies worldwide, and has now permeated most countries in one way or another. Many which have not adopted ideas from the teaching materials are now looking at a curriculum structure influenced by PISA, which also heavily relies on the RME work.

Another highly influential *applied/practice-based* line of research which has derived from Europe is that of Colette and Jean-Marie Laborde in dynamic geometry based on CABRI, an artefact that has become uniquely globalised, perhaps because of the high dependence on spatial and logical aspects and low dependence on language use which has meant little need for translation.

Thus although some of the *applied* research we have considered has its roots in changes to practice developed through small-scale trials rather than via a broad national policy route, it is clear that such developments in practice in turn often influence developments in policy, and not just local practice.

We have thus seen that policy, in some cases national but often with common elements across many countries, can be strongly influenced by mathematics education research, and that this can either be research which is *basic*, *strategic* or *applied/practice-based* in its nature. Commonly lines of research which prove influential are not confined to one of these three characterisations but move across two or more of them. This relates to an earlier point that for successful translation into policy a powerful conceptual basis and related detailed work at an applied classroom level are a powerful combination.

So far we have characterised a typical trajectory as moving from *basic* into *strategic* and then *applied research*, but often the *applied* also feeds the *strategic* or even the *basic*, in raising questions which cannot be answered on a simple empirical level. Thus many researchers, like myself, move back and forwards across these approaches through their career.

#### 4. WHAT ARE THE PROBLEMS IN POLICY-RELATED RESEARCH?

Some research is carried out without specific regard for its policy consequences. But in a world controlled by the need for audit and ‘value-for-money’, most public and private research funding requires some prior reassurance that the results are likely to ‘make a difference’ in policy terms. In some cases this has led to researchers making extravagant and quite unrealistic claims for the likely significance of modest research.

Some research, often financed by government or government agencies, is more tightly related to policy, and is thus likely to be tightly monitored to ensure that it delivers. I have been involved on some occasions with this type of project, and continue to be so currently. It brings many advantages, such as a close personal relation with policymakers and an opportunity to influence them informally as well as in formal meetings, together with insights into how they work and hence how we can be more effective in gaining influence. If the results are accepted on both sides then there is a strong possibility of significant change as a result without waiting for the normal long delays before research translates into practice.

But there are also hazards:

*a) The threat to academic integrity caused by sudden changes of direction.* At the outset the researchers are likely to negotiate a project which they feel is feasible and professionally justifiable. However my experience is that over the course of any policy-related project, political circumstances are likely to change rapidly—policies, officials, ministers and even governments may be replaced, and almost certainly timetables will change. Government lawyers cleverly build this contingency into the contracts. Lawyers from my university and from a commercial company with which we are collaborating have recently spent 9 months on an 18 month project arguing with lawyers of a Government agency in order to obtain more reasonable terms for coping with possible variations which would allow us to sign the contract; during this period no-one working on the project could be paid. Although the final contract is fairer to us, we were not able to obtain an agreement which prevented the agency from varying its required outcomes with very little warning. This has indeed happened three times within the first 11 months.

Such changes mean that having formulated a careful research design to obtain specific information you can be asked to vary the design, or to provide the results before the investigation is complete, or for other information which you did not built into the design. In each case the research team have to decide whether to comply, whether to comply only if additional funds are made available to pay for additional work, whether to propose a compromise or whether to refuse the request on the grounds that it is unreasonable. Refusing a request, even on grounds of professional integrity, has the possible consequences that not only future work but also completed work which has not yet been billed will not be paid for, and thus people may lose their jobs. Where information is needed rapidly your refusal will probably mean that

others with even less research or knowledge may be only too happy to fill the gap. Thus maintaining your own professional standards may result in the school system, and the pupils, suffering, which makes the decision very difficult. It is also likely that you, and even possibly others in your institution, will not be regarded favourably in future funding bids if you fail to comply.

Clearly where possible some compromise is preferable so as to keep the project alive and the procedures as rigorous as possible, but I have sometimes made such compromises only very reluctantly. In our current project all three requests were for results a year earlier than originally agreed; we twice negotiated a compromise where we delivered a relevant alternative and on the third occasion offered a part-compliance only.

This tale of constantly changing requirements is not in my experience unusual. In the early years of the English national curriculum there were a lot of initial problems because the subject working groups were not given enough time and had to work in an environment where ideas about the structure and the assessment were in constant turmoil (I know this as I sat on the mathematics group). King's College, working with colleagues in Birmingham and Cambridge, won a 3-year contract for evaluation of the new curriculum. However the government decided at the beginning of this that they would amend the curriculum after the first year of our contract, so that our first year of data referred to the original format and our second to the new version. By the end of the third year they were planning to change it once again so they were no longer interested in our results and asked us to rewrite our report to relate it to their proposals rather than to our research questions. We achieved a compromise of presenting our full report as planned but including any information we happened to have gathered that was relevant to their proposals as one chapter.

Another such project was extremely influential in that it was one where we were in a London University-based consortium developing the first national secondary school (age 14) assessment in mathematics, science, English and ICT/design technology. The mathematics and science elements were based at King's College. It had been agreed following a report of the National Task Group on Assessment and Testing that the tests would be authentic, integrated into one or more themes and would provide material for classroom work over 2-3 weeks. After three years of development and two years of piloting and development in which the pupils, teachers and advisory groups of experts and civil servants all expressed considerable satisfaction with the product (some pupils even said these were the best maths lessons they'd ever had), the minister changed. Advised by right wing lobbyists, the new minister with no educational knowledge or experience requested the pilot assessment materials and dismissed them as 'elaborate nonsense'. The contracts were immediately cancelled and were revised on the basis of developing short traditional skills tests.

After considerable debate we decided to bid for the new contracts, although we knew that educationally they would be much less satisfactory, both in terms of their validity and their classroom consequences.

We retained the mathematics contract only, losing the other subjects to organisations which were prepared to undercut our price by doing less piloting. However retaining the contract was a mixed blessing since there were constant pressures to make changes requested by civil servants and politicians, and maintain a quality which was not reflected in the funding level which we had been forced to agree. This led to unacceptably long hours being worked, causing problems for the staff and long-term illness of the administrator. However we were proud that the tests we piloted set a very high standard. (The format has not been changed over more than 12 years.)

We were asked to bid for the next contract to develop and run the first three years of the operational tests and were told informally that we should not give notice to our staff as we would be sure to win the new contract. After the interviews and the meeting we were told that we had indeed been selected, but before it could be confirmed in writing there was again political interference by a right wing adviser which resulted in a reversal of the decision, on the basis that we were a group of radicals who would subvert the tests. Luckily the director of the second choice group, which now were told they had the contract, was an ex-student of mine and had no team in post. This meant that we could transfer most of our team to his institution, no-one lost their job and the design of the eventual national tests continued unchanged. Although we lost the new money and the College had to meet some expenses of closing the project early, we also avoided the continual pressure and hassle of test writing which still continues for those who are involved with it.

*b) The threat to academic freedom caused by loss of intellectual property rights*

Government agencies usually require academic staff to surrender to them intellectual property rights over any publication associated with the research they commission; quite often they also include in the contract the need for their approval of research instruments like interview schedules and questionnaires. This is not unreasonable, given that the agency name is associated with the work, but it can lead to unanticipated delays in the research schedule.

However I have once been in the situation where a government agency would not allow one of our findings to be included in the final report since it contradicted one of their policies. We refused to change it and were then threatened by a senior agency official that our department would never receive any more government money if we did not comply with their wishes. The report was delayed by more than 6 months as we argued. Happily the research director of the organisation, a right-wing professor of philosophy, eventually supported us and the report was published with the finding intact. The difficult official was later moved out of the post.

In another case the Government would neither publish the national curriculum feasibility report we had undertaken nor give us the right to circulate it, as a new minister wanted to bring in a new policy which was in conflict with the research results. But all our steering committee had copies of our report and I was not surprised when photocopies eventually found their way to the major newspapers in

anonymous brown envelopes. I made sure that as project director I was able to claim honestly that I had no idea who had leaked it, although in retrospect I am fairly sure it was the work of a very eminent mathematician.

We have found though that in most cases government agencies are willing to change the intellectual property rights clause of the contract to allow the researchers to publish in academic outlets, and they may be willing to allow this without prior approval if a delay clause is included.

*c) The requirement for results which are unambiguous and acceptable*

This factor has already been referred to in the two examples of agencies which were not prepared to publish findings they found unacceptable arising from projects they had funded. However it also arises even on projects funded by independent charities. I have twice been asked by government advisers to deny newspaper reports of results of independently funded research which I have directed (and refused on both occasions). In one case the finding was that homework in primary schools did not improve standards of mathematical attainment and on the second that the hugely expensive National Numeracy Strategy had made only a very small average improvement in Year 4 attainment, with standards of low attaining pupils deteriorating. In the case of the homework claim I the minister of education publicly responded, saying that this result could not be trusted as it came from irresponsible and politically motivated researchers. This is the usual response and it had happened to me before in relation to the national curriculum feasibility study, when the editor of the newspaper which published the report of our work advised me I would win if I decided to sue the minister concerned for libel! On the last time I was asked to deny reported results (about two years ago), a ‘spy’ in the ministry told me that the only reason I didn’t receive the usual public defamation was that she strongly advised the ministry that I would get a lot of support from teachers and researchers and would be likely to win any legal case. Since it arose in a paper given at an American academic conference it was difficult to accuse me of trying to subvert government policies by spreading scepticism among British teachers.

An occasion where a government adviser was ‘economical with the truth’ following research which gave the ‘wrong results’ occurred when I was a member of the Government Numeracy Task Force which recommended the National Numeracy Strategy. It was clear that we were expected by ministers to support more whole class teaching and no calculators. The Chairman, a leading education professor and government adviser, asked for further investigation of the 1995 TIMSS results for English primary schools assuming that the data would demonstrate the superiority of whole class teaching. Unfortunately in Year 4 the classes which worked individually directly from textbooks or worksheets most of the time performed significantly better than those which were taught as a class for most of the time, although in Year 5 the results were equivocal. Similarly those Year 4 classes using calculators for solving problems significantly outperformed those which never used calculators. The task force was sent this data along with many other papers—when I mischievously inquired

whether we were going to include reference to it in our report I was told that we could safely ignore it since it was a very small sample (although actually TIMSS used one of the largest samples in any research on English primary school mathematics). No final report of the work was ever sent to us or published—when I inquired of the research organisation when it would be published I was told that it wouldn't be, and that the researcher who did it had retired so had no further interest in publishing. Although I have referred to the data in articles (e.g. Brown, 1999) to try to quietly draw people's attention to it I cannot publish it since it isn't my data and I don't have all the methodological details.

The Task Force did of course in the end do as expected and recommend a substantial period of whole class teaching in every lesson; I went along with this as I didn't believe it would do any harm and teachers in the pilot project seemed to be enthusiastic about this change. However I pointed out to the group and in print that having done a review of the literature (Brown et al., 1998) I didn't think the existing research was other than ambiguous so I suspected it wouldn't have much effect. In fact our research funded independently by the Leverhulme Trust in the following two years demonstrated that there was virtually no difference in gains made by classes who had a lot of whole class teaching from those who had very little. The study showed that the curriculum had a significant effect but there was very little effect from the resources or method of delivery.

This is just one example of a number of studies financed by government or others with the aim of proving that one way of teaching is better than any other. Most end up with no clear answer, e.g. a very recent EC-funded report notes:

‘The data from PISA 2000 and 2003 thus contradict a hypothesis that there are specific and clearly identifiable advantages related to specific learning strategies or learning situations.’ (Haahr, 2005, p.18)

However this finding is very difficult for governments to accept as it gives them no support for changes they want to bring about. For example I am currently part of a five subject study on effective practice in adult basic skills teaching which is not being well received by the ministry because it confirms this conclusion. (However at least the adult numeracy classes in the study made an average gain over a year; the mean reading results fell slightly!)

Incidentally on calculators the Numeracy Task Force came to a compromise – the final wording, which referred to judicious use of calculators (and which could therefore be used to justify many different practices), took many hours to draft so that everyone could agree. But it didn't matter as the Ministry in its summary of our report for the press included the fact that the minister congratulated us for banning calculators. Teachers, who are more likely to read newspapers rather than government reports, therefore stopped using calculators and it took several years to convince them that there was no national law against it!

## **5. WHAT ARE THE PROBLEMS OF MEMBERSHIP OF POLICY-MAKING BODIES?**

While I have been discussing the problems of undertaking policy-related research, I have also touched on difficulties of membership of policy-related bodies. In particular there are many agonising decisions, as on any committee, of knowing when to compromise and when to hold out for what you believe to be the best solution. However these decisions become more critical when you remember that if the outcome goes wrong you will as a member of the group be blamed for damaging the nation's mathematical standards.

You also need to be confident that you can support the conclusions of the group as you will often be asked to defend them to teachers. Sometimes there have been decisions made by the policy group which I really do not agree with but have decided to accept as part of a larger package which I can support. I have sometimes found it necessary to be frank about such issues when talking to groups of teachers (provided neither officials nor press reporters are present), especially when my views are already publicly available in articles I have published. However this is only really possible where you are able to be enthusiastic about most other recommendations.

One other aspect I should perhaps note is my observation that the views of mathematics educationists on such policy committees tend to be given much lower status than those of mathematicians. There may be different reasons for this, for example gender could be a factor since I have obviously only experienced situations in which the educationist is female and the mathematicians male. There is however I think the problem that the mathematics educator cannot often produce hard-edged views, since as noted above research tends to give ambivalent results about learning methods and situations. Mathematicians who may be guided only by their own educational experience, and their experiences of teaching only the cleverest pupils, often find it easier to come to hard-edged views and to express them strongly. This puts onus on mathematics educationists to present a well-grounded argument backed wherever possible by relevant research evidence.

## **6. SOME THOUGHTS ABOUT EVIDENCE-BASED POLICY**

There is a move in some countries, especially Anglophone regimes, for politicians to espouse evidence-based policy, claiming their support for a pragmatic view of 'what works' rather than for political ideology. Unfortunately in practice this sometimes becomes distorted, as reported above, into selective use of evidence to support already agreed government agendas, determined by other means, often on the advice of policy advisers and lobbyists. Thus 'evidence-based policy' can easily become down-graded into 'policy-based evidence'.

However on the positive side it means that in education in the US and UK considerable funds have been given to systematic reviews of the research literature, on the Cochrane collaboration model used by the medical establishment. In fact in the



UK one of the leading consultants in the educational agency has been recruited from the equivalent medical review agency.

I strongly support the notion of reviewing the research literature before making educational policy decisions but I am clear that this needs to be done with great care because of the problems of interpreting empirical educational research studies, especially in an international context where it is not easy to understand the nature of the contexts, political and educational, in which the work was carried out. It is necessary therefore that the group undertaking the review is expert and able to draw on their own often rather subjective knowledge. What concerns me is the attempt to reduce systematic research reviewing to an almost algorithmic process which can apparently be carried out by anyone in the field, and the resulting discounting of professional opinion. The American Educational Research Association have set a good precedent in their arguments with the US government that methodologies like randomised controlled experiments are good for tackling some educational problems but not others, and that methodology needs to be determined by the research questions not by some algorithmic status-order. I believe that other countries will need to engage more with pointing out the strengths and weaknesses of particular processes of systematic review for policy determination.

## **7. IMPLICATIONS FOR MATHEMATICS EDUCATION RESEARCH**

It is obviously important that mathematics education research and researchers be involved in policy determination as far as is possible, for otherwise there must be questions over why our research should be funded at all. It is clear that some research has had a huge impact on educational policy, often internationally, and that the most powerful research has been that which has combined theoretical strength with translation into classroom detail for implementation. However getting involved in either applied research which has a clear potential to affect policy, or in policymaking bodies, is not without risks, as I have illustrated here.

I would nevertheless urge mathematics education researchers to engage in such arenas rather than playing safe with small pieces of their own research over which they have control but which may have very little impact on the system. This can mean undertaking policy-related work funded by government and government agencies. It may mean engaging directly with policymakers and 'brokers'. I believe that it is irresponsible to criticise from the sidelines without attempting to both listen to and educate those whose job is in policymaking. We can learn from them about practical constraints and they have much to learn from us about what we know about educational effects of change and professional learning. At the least we can ensure that our research has the potential to affect policy by publicising it in the national media as well as to other researchers and to professionals; it is difficult to complain that no-one listens to us if researchers choose only communicate with each other in obscure journals and select conferences.

Finally when caught up in the policy maelstrom it is sometimes easy to get entangled in issues of status and celebrity, with the rewards appearing to go to those who offer governments simple solutions. It is not always easy to hang onto our professional and personal integrity and our academic values, but it is essential to do so as that in the end is all we have to offer.

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## PANEL: HISTORY AND THEORY OF MATHEMATICS EDUCATION

### AN INTRODUCTION TO THE TOPIC OF THE PANEL

#### REMEMBERING HANS-GEORG STEINER: THE THEORY OF MATHEMATICS EDUCATION PROGRAM

**Juan D. Godino**, *University of Granada, Spain*

When Barbara Jaworsky asked me to assume the responsibility to organize and chair this panel on the “History and Theory of the Mathematics Education” I thought that the best person for this work was professor Hans-Georg Steiner, who devoted much of his professional life to promote the reflection about the theoretical foundations of Mathematics Education. More than 20 years ago, during the V ICME Congress held in Adelaida, Steiner organized a working group on the Theory of Mathematics Education and later he organised the celebration of several international conferences on this topic. These initiatives, his talks and articles established the bases and the interest to reflect about the paradigms, theories and methodologies that serve as foundations to research in Mathematics Education.

I am sure you agree that our dear professor Hans-Georg Steiner would have been the best qualified chair for this panel, but unfortunately he died on January the 14<sup>th</sup>. As the Chair of the panel, I proposed to specially dedicate this CERME 4 panel to the memory of professor Hans-Georg Steiner, as a sign of admiration and respect to his academic contribution and its great humanity.

#### PANEL ORGANIZATION

We spent two 1 hour and 45 minutes sessions to the panel. In the first session, we devoted approximately 1 hour to the panelists’ intervention, where they explained their points of views about some key questions that I posed them about the evolution and current state of the Theory of the Mathematics Education. The 45 remaining minutes as well as the second session were spent to questions and reactions by the audience.

Some general questions to stimulate initial thinking in the area of the topic were as follows.

#### THE PROGRAM OF THEORY OF MATHEMATICS EDUCATION

In several writing, Steiner (1987) presented the Theory of Mathematics Education as a developmental program with three interrelated components:

1. *The identification and elaboration of basic problems in the orientation, foundation, methodology, and organization of mathematics education as a discipline.*

2. *The development of a comprehensive approach to mathematics education in its totality when viewed as an interactive system comprising research, development, and practice.*
3. *Self-referent research and meta-research related to mathematics education that provides information about the state of the art –the situation, problems, and needs of the discipline- while respecting national and regional differences.*

These three items will serve to organise the questions to debate in the panel.

### **I. Variety of paradigms and agendas**

Regarding the first point of Steiner's program, I consider that research methods have changed from the prevalence in the seventies and part of the eighties, of a psycho-statistical approach based on statistical tests and reliability, to a large variety of methods, the opening of research agendas and the adoption of eclectic positions. There is neither accepted universal framework nor relative consensus among thought schools, investigation paradigms, methods, or quality standards.

- Do you consider that this situation, similar to a Babel tower, confuse the diverse communities and often makes the research efforts unproductive?
- By the contrary, is this situation beneficial to develop the field, given the partiality of each approach?
- Is progress in the field subordinated to elaborating a “unified approach to mathematical knowledge and instruction”?

### **II. Divorce theory-practice**

Mathematics education as an academic discipline has been progressively consolidated, in the international scene, in the past 30 years. However, this development has been unequal in its different facets, in particular in the articulation between research, development and practice.

We should recognize the scarce and frequently null connections between innovative activities and research developments. There is a big gap between scientific academic investigation and its practical application to improve the teaching and learning of mathematics. Didactic problems are frequently decomposed in such a way that they lose their initial character and become epistemological, psychological, sociological, political... problems. The problem of what mathematics to teach and how is rarely approached.

- What are the reasons that could explain the present divorce between theory and practice in mathematics education?
- What type of actions would be necessary to improve the situation?

### **III. Regional and national differences**

The third item in the Steiner's program is self-referent research and meta-research related to mathematics education that provides information about the state of the art –

the situation, problems, and needs of the discipline- while respecting national and regional differences.

This component suggest me the following questions:

*Schools of Mathematics Education*

- Are there an Anglo-Saxon didactics and a continental European didactics? Which are their characteristic features?
- Are there national (regional) schools of mathematics education? Which are their characteristic features?
- Can we say there is (or there has been) mathematics education schools centred around particular authors? (Freudenthal; Brousseau, etc.) Which are their characteristic features?

*PME and CERME objectives:*

- Is there a different vision of mathematics education (didactics of mathematics) among the “PME and the “CERME communities?”
- To what extent is there a complementary/opposed vision about the objectives of mathematics education research?
- To what extent is CERME a reaction to compensate a possible psychological bias in PME?

**SOME QUESTIONS POSED BY THE AUDIENCE**

1. Teaching practices are different in the different countries (for instance, Northern Countries, GB, Italy, France, just to cite some of them). This must have an influence on the research and the theoretical elaborations.
  - Do we have an idea of the importance of this influence?
  - Could this lead to theories which it would be impossible to integrate?
2. In Italy they have teachers – researchers. A situation opposite to what we have in France for instance.
  - How does this possibility of teachers – researches influence the links between theory ad practice?

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# HISTORY AND THEORY OF MATHEMATICS EDUCATION

*Michèle Artigue, Université Paris 7, France*

As my two colleagues Fulvia Furinghetti and Paul Ernest, I have been asked to contribute to this panel by presenting a personal view on the themes under discussion: the variety of paradigms and agendas, the divorce between theory and practice, regional and national differences.

In this contribution, I will focus on the two first ones. The third one will be present but, in some way «under the line», through the references I will make to my didactic culture and to different experiences I have been exposed to in my professional life.

In an e-mail exchange we had preparing this panel, Juan Diaz Godino wrote that «mathematics education as an academic discipline had been progressively consolidated in the international scene, in the past 30 years». I will begin by commenting this assertion. My personal vision is that, in spite of an evident institutional consolidation, at both national and international levels, the situation of the field remains fragile.

## **I. The institutional situation of the field**

There is no doubt that, from an institutional point of view, research in mathematics education has gained in status. Master and doctorate programs have developed and they exist now in many countries, both developed and developing countries. New journals are regularly created, as well as national and international associations. The number of conferences offered every year exponentially increases. And some didacticians have even been given here and there important educational responsibilities.

Nevertheless, I cannot escape a feeling of institutional fragility. The field develops but its image, even in the restricted sphere of the educational world, does not progress in the same way, not to mention the relationships with the community of mathematicians or the general population. Having been a member of the national commission in charge of the qualification and career of mathematicians and didacticians in France for 12 years, serving in the ICMI executive committee since 1998, I have regularly to face the fact that the gains we regularly achieve have to be re-negotiated each time responsibilities change and that still today our relationships with educational institutions and scientific communities are too much dependent on personal images and relationships. We can of course lament on that and react with some feeling of injustice but we have certainly also to question the objective reasons for such a persisting fragility. I will come back to this point later on when discussing the relationships between theory and practice but wanted to stress the point from the beginning.

## II. The diversity of the research paradigms and agendas

In the e-mail already mentioned, Juan Diaz Godino evoked the Babel Tower for imaging the current state of the field and the diversity of its research paradigms and agendas; he also asked us to give our personal view about the positive and negative sides of this diversity. I will enter the discussion about diversity by rephrasing his question in the following way: Is the Babel Tower image an accurate image for representing our field?

At a first sight, this image seems a rather good image, due to:

- the multiplicity of the existing theoretical frames for approaching the same or nearly the same issues,
- the exponential increase of theoretical constructs and the increasing diversity of their native fields,
- the local character of most of these constructs which makes attempts for achieving some global coherence more and more difficult. I could myself argue in favour of it by referring to two recent personal research experiences.
- The first one is the meta-study on research about the integration of Information and Communication Technology into mathematics education I have been involved in, some years ago, with other French colleagues in the frame of a national project founded by the Ministry of Research (Lagrange & al., 2003). For this meta-study, we analyzed more than 600 publications dealing with technology, which were published between 1995 and 1998. We were struck by the multiplicity of the theoretical frames involved in these publications and by the local character of many of these frames. Beyond that, a question emerged linked to the role given to theoretical frames in these publications. They seemed more invoked than operationally used, as if theory was something that had to be there for some reason of didactic contract more than an effective tool for the research work.
- The second experience is the experience I currently live inside the European network of excellence Kaleidoscope<sup>1</sup>. In this network, six teams from England, Greece, Italy and France work more specifically on mathematics education within the TELMA European Research Team – TELMA means technology enhanced learning in mathematics – It soon appeared to us that, to be able to work together, we must try to share our respective theoretical frames, understand how these shape our vision of technology, our conception and design of technological educational tools, the experiments and scenarios for their use that we develop. We are only six partners coming from only four countries, not so distant, but we have to appropriate and connect six main frames, not to mention the secondary ones. Even if this is our common project,

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<sup>1</sup> For more information about this network, the reader can consult its website : [www.kaleidoscope-noe](http://www.kaleidoscope-noe).

this is not easy at all as has been evidenced at CERME4 by the contribution of some Italian members of the TELMA team (Cerulli, 2005).

Nevertheless, due to the idea of increasing incommunicability it conveys, I don't think that the image of the Babel Tower accurately reflects the evolution of the field. Neither I want to necessarily see in the diversity of theoretical frames just a symptom of the immaturity of the field as is often argued in reference of the ideas developed by Kuhn (Kuhn, 1962) as regard scientific normality.

What seems to me more important to point out is the convergence in theoretical evolutions. At the end of the eighties and even in the early nineties, as a French researcher, I felt much more isolated than today. I had been raised in didactics in a culture where the social dimension of learning processes was something like an axiom. In the theory of didactic situations (see Brousseau (1997) for a synthetic view) that accompanied my first steps in the field, the essential object is a social construct: the situation. Moreover, soon after, thanks to Yves Chevallard (1985, 1992), an institutional perspective also entered the scene. The way such perspectives shaped my research work, the way they determined for me acceptable levels of analysis, influenced my interpretative frames and the kind of results I was looking for, was not easy to convey and share. Today the situation is quite different. The international world of mathematics education is a world where social and cultural perspectives are more and more influential as already noticed in 1996 by Lerman and Sierpinska in their synthetic chapter about epistemologies in mathematics and mathematics education (Lerman & Sierpinska, 1996). Communication becomes easier, the building of bridges between our different didactic cultures does no longer seem something out of range, and up to some point diversity instead of being seen as an obstacle can be seen as a source of mutual enrichment.

Nevertheless, even if I consider that diversity can be a source of enrichment, and is not something we should necessary try to escape, I do not consider the current situation as an ideal one. Common trends, similar dynamics are evident but I have also the feeling that taking this opportunity for going a step further is not a priority in most research agendas as if the current state were the best we could hope.

Is the cultural dimension of educational issues often invoked the essential reason? I seriously doubt. We could share theoretical frames and concepts while cultural and context differences would express in the didactic strategies we would derive from fundamental research for improving practice. This is the case in other scientific fields, not in ours. Why?

Exchanging, collaborating with persons of good will but from different didactic cultures, I am more and more sensitive to the difficulty one meets at understanding constructs that have developed outside his or her didactic culture, and at making these operational. Most often, it is easier to adapt, to complement the constructs we are more familiar with than to invest in the understanding of concepts built outside. All the more as the problem is not the problem of getting familiarity with one specific



concept. As scientific concepts, didactic concepts function in systems and cannot be approached in an isolated way. I think that, from this point of view, the theory of didactic situations is a good example. It is certainly not too difficult to access a cultural vision of it and to become able to label some phenomena as didactic contract phenomena for instance. But developing the kind of knowledge required for having this theory an operational tool and for understanding what it can offer you is not easy at all. The fact that this theory has been growing for more than thirty years, that it is regularly reworked and enriched increases the difficulty. If you can, in line with your own theoretical evolution, built something that seems to take into account what this theory takes in charge, the cognitive cost is not comparable. In that sense, the existence of common trends does not necessarily help unification. And the current situation shows that the different didactic cultures, facing similar problems and similar limitations in their constructs, accommodate in the most economical form for them. This economy tends to lead more to parallel than convergent developments.

Changing this dynamics will be costly. My personal feeling is that nothing will happen if we let the task to the sole responsibility of individual researchers. It needs a collective will, agenda and scientific policy. A European association such as ERME is certainly a structure which could allow the efficient development of such a research agenda. If it engages in that direction, I think that it must take seriously into account the lessons of anthropological and cultural approaches: knowledge emerges from institutional and cultural practices. These practices cannot be accessed just through the reading and comparative analysis of the literature. They have to be experienced.

### **III. The divorce between theory and practice**

I evoked earlier the deficit of image of our field. In my opinion, this deficit is not independent on the ways we situate as regard practice, and more globally on what we consider as our research priorities.

I would like to come back once more to my first steps in the field in the seventies at the apex of the new math reform disillusion. At that time, the idea that it was necessary to take some distance from action on educational systems and to develop deep understanding of didactic phenomena through fundamental research in order to avoid such disillusion in the future, imposed to everyone. Of course, this did not prevent didacticians from being involved in curricular commissions, in innovative projects, in the development of educational resources which they tried to diffuse through teacher training and other channels, but it certainly influenced our vision of the relationships between theory and practice. The radical character of the new math reform in France, and the force of its rejection certainly reinforced the theory-practice divide in this country if compared with other educational cultures.

This situation had as a consequence that we certainly did not pay enough attention to the specificity and the cost of the work, including research work, necessary for developing more efficient links between theory and practice. In my country, some attempts were made through the idea of didactic engineering. But the initial and

necessary distinction introduced by Chevallard (Chevallard, 1982) between phenomenotechnics and didactic engineering was soon forgotten. As a result, if the idea of didactic engineering played an efficient and important role in supporting classroom research, it did not really supported an efficient vision of the relationships between theory and practice (Artigue, 1988, 2002). Hence my personal feeling is that we have to question our vision of the relations between theory and practice, a vision influenced by a heritage which progressively naturalized.

I have evoked here the French situation, pointing out some of its specificities but my international experience shows me that, even if the stories are different, the theory-practice divide is not the apanage of my country. It does not just result from what is often seen from outside as the particular and, for some of you perhaps abusive, attraction towards theoretical concerns of the French didacticians. This was clearly evidenced by the report that Anna Sfard presented at ICME-10, in July 2004 (Sfard, 2005).

Once more I have the feeling that improving substantially the current state will be costly and deserve more than good will and individual actions. It needs to introduce some change in our research agendas. I serve as a reviewer for several journals and am often worried by the evolution in the submissions I receive. These tend to be longer and longer, to scrutinize in a more and more detailed way, through more and more sophisticated theoretical lens, smaller and smaller pieces of transcripts or data. Let us me be a bit provocative. Sometimes I have the impression that our theoretical developments allow nearly everyone of us to write 50 pages about nearly anything, but that when one tries to figure out what is really learnt from such 50 pages, there is very little to consider.

The predominant role given to qualitative and bottom-up methodologies, the desire to take into consideration the multiplicity of existing determinants of didactic phenomena, make this evolution understandable and up to some point legitimate, but we need today to develop and value also other forms of research, as also stressed by Even and Ball in (Even & Ball, 2003). We need to find better balance between on the one hand the fine grained analysis and deep understanding allowed by short term and small scale experiments, and on the other hand the long term and large scale research that is necessary if we want to seriously address the issue of practice. Such an evolution will of course impact our methodologies and oblige us to find a new balance between the quantitative and the qualitative.

I dream today about ambitious projects where theory and practice would develop in a rather harmonious way. This can be seen an utopia but, this summer, at ICME-10, listening to Jo Boaler who was presenting the impressive work which has been developed by her team with minority students and teachers (Boaler, to appear), I was thinking that such an ambition is not necessarily out of range. This cannot be of course the research of an individual, it needs teams, collaborative work involving researchers, teachers, and institutional partners; it needs substantial means and funding, but it would better evidence that didactic research has something important

to offer. I am personally convinced that this is the case and I hope that, thanks to international associations like ERME, we will be able to join our forces and succeed.

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# HISTORY AND THEORY OF MATHEMATICS EDUCATION

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What defines an *area of knowledge* – *field of study* – *specialism* – or *discipline* – such as mathematics education or indeed any science or discipline? My provisional answer is that four things are required: 1. An object of study, 2. A specialist literature, 3. A body of committed specialist researchers, and lastly, 4. An academic identity for the specialism. Below I consider at each of these briefly, in turn.

## I. The Object of Study of Mathematics Education

The primary object of study of mathematics education is the practice of *teaching and learning mathematics*. Needless to say, the study of the practice of teaching and learning of mathematics must be cognizant of the social context in which it takes place and the persons or ‘actors’ involved. There is also a secondary object of study that arises from the reflexivity of all mature discursive practices, namely mathematics education as a social institution and domain of knowledge itself (Ernest 1988). However, the primary object of study, the ‘problematique’ of the field is the research questions and problems that arise from a institutionalized social practice, the teaching and learning of mathematics in schools and colleges.

Taking a historical perspective is illuminating for the study of mathematics education as a domain of knowledge. First of all, mathematics itself is the original source of mathematics education practices. Mathematics has a 5000+ year old tradition involving teachers of mathematics, schools, and mathematical texts (including papyrus scrolls, cuneiform tablets, books, etc.). These texts have a dual role in mathematics: an *epistemological* role involving the systematization of mathematical knowledge, and a *curricular* role involving the structuring of mathematics (problems, examples, skills) for instruction. These functions first emerged in recorded history in the ancient civilizations of Mesopotamia and Egypt (Høyrup 1980). They are also evident in more recent texts from Euclid’s *Elements* right up to modern monographs in advanced mathematical topics.

Second, in the 19th century there was a rapid growth of organised schooling in many countries due to increased economic demands for a literate and numerate workforce. This led to three significant developments:

1. Institutionalised systems for the assessment of learning providing reports on mathematical achievement (as well as serving as a tool for selection);
2. Teacher training and teacher colleges, including teacher trainers in mathematics; and
3. Growth of interest in pedagogy, resulting in the publication of educational and teaching methods texts including mathematics pedagogy.

Each of these developments contributed to the development of mathematics education as a field of specialist knowledge, if not yet a field of research.

## II. Specialist Literature in Maths Education

Another crucial development in the history of mathematics education was the growth of a specialist literature in mathematics education in the 19<sup>th</sup> and early 20<sup>th</sup> century. Initially four types of early specialist research literature were evident:

1. The mathematical analysis of mathematics itself and the curriculum for educational purposes, such as the works of Felix Klein (1907–8) and the Mathematical Association (1925) report on the teaching of geometry in schools which offered a theory of the stages in learning geometry.
2. Theories of mathematical learning, which included the very influential biogenetic law of Haeckel ('ontogenesis recapitulates phylogenesis') which was applied to mathematics by Branford and others (Fauvel 1991).
3. Psychological research on learning of mathematics, for example: Thorndike's research on the transfer of mathematical learning in the early 20<sup>th</sup> century (Kilpatrick (1992), researches on error patterns in tests of arithmetic, and later, from 1920/1930s, onwards, the growing impact of Piaget's theories of cognitive development.
4. There were also philosophers' contributions on the aims of the teaching and learning of mathematics, such as those of Dewey and Whitehead.

These types of literature were initially seen as belong to the domains of mathematics, education, psychology and philosophy, respectively, but through their specialisation on the problems of the teaching and learning of mathematics created converging subspecialisms of their parent fields of study.

Another important development around the turn of the century was the establishment of early journals addressing the problems of mathematics teaching, including *Enseignement Mathématiques* (France), *Mathematical Gazette* (UK), *Mathematics Teacher* (USA). Although primarily addressed to mathematics teachers, their contributors and audience also included mathematicians and mathematics teacher trainers.

This development was followed by the establishment of 'mature' research journals in the late 1960s, including *Educational Studies in Mathematics* (Holland), *Zentralblatt für Didaktik der Mathematik* (Germany), and *Journal for Research in Mathematics Education* (USA). These journals were addressed to and contributed to by researchers in mathematics education, which included mathematics teachers researching for higher degrees as well academics in teacher training colleges and university education and mathematics departments.

### III. Body of Committed Specialist Researchers

A body of specialist researchers began to emerge at the turn of the 19<sup>th</sup> and 20<sup>th</sup> centuries, with the founding of national associations, such as the *Mathematical Association* in the UK (originally 'The Association for the Improvement of Geometrical Teaching'), and international organisations, such as the *International Commission on Mathematical Instruction* (ICMI) established at the *International Congress of Mathematicians* held in Rome in 1908. These groups provided both a social structure (professional bodies with membership, conferences and congresses) as well as a platform for the publication of journals, reports and other texts. By the mid 20<sup>th</sup> century there was a small band of mathematicians specializing in mathematics education research, such as William Brownell. After World War 2 there was a growing body of mathematics education specialists leaders, such as Begle, Davis, Dienes, Freudenthal, Gattegno, Papy, Servais. Some of these leaders were more research orientated than others. During this period there emerged the training of mathematics education researchers, although the supervisors were, in the first instance, mathematicians, psychologists, philosophers. Consequently the very small number of PhDs in mathematics education prior to World War 2 grew and suddenly mushroomed in the 1960s. This was the era when mathematics education emerged as an identifiable if rather young discipline.

Later in the 20<sup>th</sup> century a number of further developments created and consolidated the place and status of the field, including:

1. Large scale funded research and development projects in mathematics education from late 1950s onwards (given an extra push in the US by the shock of Sputnik);
2. The move of mathematics teacher training from colleges to universities with a research mission;
3. The existence of a mathematics education sub-specialism in education, mathematics, or psychology departments in universities (there were also some specialist mathematics education departments);
4. The opening up of specialist mathematics education research centres, including IDM (Germany), IREM (France), Shell Centres (UK);
5. The regular scheduling of specialist conferences in mathematics education with a research component (ICME, CIEAM, HPM, IOWME) or an exclusive focus on research in mathematics education (PME, CERME).

### IV. Academic Identity for Specialism

Finally the academic identity for mathematics education as a specialist area of research and knowledge was developed through 1. Its recognition by academic institutions including university departments, specialist associations, national and international bodies (ICMI comes under the International Mathematical Union, which is funded and overseen by UNESCO) as well as government bodies. 2. There is a

networked international community of specialist researchers in the field. 3. There is an extensive research literature. 4. Perhaps most importantly for the academic identity of mathematics education as a field of study, a name for the specialism. In English this is ‘mathematics education’ (or ‘mathematical education’), although in many European countries it is translated as the ‘didactics of mathematics’. Having a title for a field reifies it into a unique and quasi-tangible entity, that is, something that subsists on its own.

Of course ‘mathematics education’ is an ambiguous term, with two main meanings. First of all, it refers to the *practice* of the teaching and learning of mathematics. Secondly, it refers to a *research specialism* (the didactics of mathematics), with its relatively recently formed academic identity. But rather than being a problem, this ambiguity may be useful; for the emphasis of the first meaning is more practical and utilitarian. By funding ‘mathematics education’, it sounds like this is supporting the practice of teaching and learning mathematics. Politicians often prefer the funding of activities with an immediate payoff, whereas research is always one step removed from practice.

What I have offered here is an account of the history and formation of mathematics education as a discipline or field of study, organized by some theoretical ideas about how its identity has emerged. Not surprisingly, reflection on the formation of mathematics education as a specialism also raises further questions about its nature and scope, which I offer as provocations to further thought.

### **QUESTIONS FOR THE THEORY OF MATHEMATICS EDUCATION**

1. Is global theory of mathematics education possible? Is it desirable?
2. Is mathematics education a science, social science, one of the humanities, or none or all of these? Is it cross-disciplinary, inter-disciplinary, or a single discipline itself?
3. Is mathematics education a unified area of study or has it split into sub-specialisms with different research traditions? (psychology, sociology, philosophy, etc)
4. Does mathematics education have unique research methods and methodologies or are they imports, e.g., from psychology, social sciences or educational research in general?
5. Does mathematics education build on unique ‘homegrown’ theories and concepts, or imports from other disciplines?
6. Are agreed answers to these and other global questions about the field possible? Or is mathematics education a multiplicity of different epistemologies and research practices which only share the same name and a myth of a unique identity?

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# NOT OUT OF THE BLUE: HISTORICAL ROOTS OF MATHEMATICS EDUCATION IN ITALY

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## I. The past

Though the development of mathematics education as a discipline is affected by many factors I deem that the national policy of the system of instruction plays a major role. On its turn this policy is strictly linked to the history of the country and to the academic world (in the case of mathematics the world of mathematical research.) As an example I briefly outline the events that preceded the birth of the Italian community of mathematics education research to catch a glimpse of links between the present settlement and the past.

Italy became a unified country in 1861, before it was composed by little states which had different systems of instruction or no system at all; to create a national system was one of the main concern of the new government. It is remarkable that the concern about public instruction was already present before the unification, as evidenced by the proceedings of the annual meetings of scientists held from 1839 to 1847 in the future Italian territory: these scientists called themselves “Italian” before Italy existed as a political entity and planned the survey of the situation of the instruction in the Italian territory. It is said that the motto chosen for the proceedings of their meeting in 1846 was “The educator and not the weapon will be in the future the arbiter of world’s destiny” and that this sentence was ink-cancelled by order of the governor in almost all the already printed copies. Strong ideals were present in the scientific community: in particular, some important mathematicians participated personally to the independence wars and, when the process of unification was achieved, were involved in political activities (also as members of the parliament) concerning instruction. The evolution of the political situation in the following century made the initial ideal position changing: the motto proposed to school children in the 1930s (Fascist period) was “Book and musket”.

In the pioneering period after the unification the Italian community the relation of mathematics school teachers with professional mathematicians was sometimes difficult, as evidenced by the well known episode of the controversy around the teaching of elementary geometry. This episode parallels an analogous episode happened in England and shows how similar situations may lead to different outputs in different countries. These are the facts. In Italy before the unification there was no tradition in mathematics education and textbooks were mainly imported from abroad. The first significant act of the new born nation was to publish an Italian mathematics textbook for secondary school. This happened in 1868 and the book was the edition of Euclid’s *Elements* edited by two outstanding mathematicians (Enrico Betti and Francesco Brioschi). The Ministry of Education proposed it as a textbook to be

adopted in Italian schools. The content was good from the mathematical point of view, but not suitable for secondary students. Teachers and mathematicians with some feeling of what mathematics education should be expressed a strong disappointment against the use of this book as a school text: a hot controversy was hosted in one of the two journals of mathematical research existing in Italy in those times (*Giornale di Matematiche*) from 1868 to 1871, see (Furinghetti and Somaglia, 2005). In one side of the duel there were the two editors of the *Elements* and Luigi Cremona, an important mathematician author of the official national programs for mathematics, in the other side there was a second rank mathematician who was caring of the pedagogical point of view and of school teachers' opinions. At those times the ordinary teachers had no voices, since professional journals did not exist, nor associations of mathematics teachers.

In England since many years the admission examinations to Cambridge, London and Oxford universities were based on rote exercises of Euclidean geometry. Many people were complaining about that, among them outstanding mathematicians such as Augustus De Morgan and James Sylvester. Books based on new syllabi were produced from 1868 onwards. In 1871 the A.I.G.T. (*Association for the Improvement of Geometrical Teaching*) was founded; it was the mother of the *Mathematical Association* founded in 1894. John Perry's address on 'The teaching of mathematics' delivered to the new 'Education' section of the British Association (1901) opened new perspectives to this problem: the educators were pushed to hear the voices of those students who will not become mathematicians and needed of a kind of mathematical education close to the requirements of the changing society. Perry's ideas were clearly expressed in the article 'The teaching of mathematics' (*Nature*, 1900, 317-320), see Howson (1982, pp. 147-148):

The young applier of physics, the engineer, needs a teaching of mathematics which will make his mathematical knowledge part of his mental machinery, which he shall use [...] readily and certainly [...]

[This] method is one which may be adopted in every school in the country, and adopted even with the one or two boys in a thousand who are likely to become able mathematicians.

In Italy things evolved in a different way. The academic power of mathematicians choked the timid attempts of rebellion to the use of the *Elements*. A sentence in the mathematics programs issued after Italian unification epitomises the official attitude towards mathematics in school: "mathematics is a gymnastic of the mind." This view was not unanimously accepted (especially by school teachers) and ironic references to this expression are present in papers appeared in the following years.

Many factors affected the different evolution in the two countries. Not only authors such as Herbart influenced the view of certain mathematics educators in England, but also the different level of industrialisation which called for a different role of

education in society. This latter fact is evidenced by Godfrey's passage as reported in (Howson, 1982, p. 158):

In England we have a ruling class whose interests are sporting, athletic and literary. They do not know, or if they know do not realise, that this western civilisation on which they are parasitic is based on applied mathematics. This defect will lead to difficulties, it is curable and the place for curing it is school.

A relevant factor in the different development was mathematicians' attitude about rigour. In Italy at the beginning of the twentieth century the concept of rigour was shifted from the Euclidean rigour to the Hilbert's and Peano's rigour, but still remained the main concern of university professors when discussing mathematics teaching in school. This strong concern is epitomised by the important report on the various types of rigour in textbooks at the first big international meeting of I.C.M.I. in Milan, see (Castelnuovo, 1911).

In the meanwhile teachers were growing up professionally, in 1874 the first Italian journal devoted to mathematics teaching was founded. After its death a journal was founded, which was the cradle of the Italian association of mathematics teachers born in 1895 (*Mathesis*). These journals were concerned with discussing details of mathematical subjects taught in school rather than on pedagogical issues. In principle the association of mathematics teachers should have been the right place to discuss educational issues, but this did not happen: most energies were devoted to decide if university professors could be admitted as members. The association had various deaths and resurrections until it acquired a rather stable status in 1921 under the chair of Federigo Enriques, one of the greatest Italian mathematicians of the twentieth century. He was researcher in algebraic geometry, and also author of textbooks and books for teachers translated into foreign languages. The first half of twentieth century was dominated by this relevant personage, who had to face events important for the Italian system of instruction, such as the reform promoted by the philosopher Giovanni Gentile. Unfortunately, in accordance with the idealistic philosophical theory of Gentile, scientific culture (including mathematics) was relegated to a second rank position. Other Italian mathematicians were contributing to the discussion on mathematics teaching and had contacts with the international milieu of I.C.M.I.: besides Enriques, Guido Castelnuovo and Gino Loria were among the nine persons awarded by I.C.M.I. with the special acknowledgement FOR their work in the field of mathematics instruction at the world Congress of mathematicians in Oslo (1936).

We see that, as it happened in the pioneering period of the nineteenth century, the chief characters in mathematics education of the first half of twentieth century were mainly university mathematicians. In summarising their attitudes towards mathematics teaching we may say that Enriques and Loria were interested in the dynamic of mathematics (its history, the psychology of the great mathematicians, the relationship of mathematics with painting, music...). As a historian Loria was a pioneer in facing the problem of the use of history in mathematics teaching,

especially in teacher education. Castelnuovo stressed the importance of modelling and application of mathematics; already at the beginning of the twentieth century he proposed the introduction of probability in mathematical programs. A singular position was that of Giuseppe Peano, who tried to apply directly the object of his research (logic) to school practice. According to him the language of logic, which is clear and not ambiguous, should make accessible mathematical knowledge to all students. Peano's project was utopian, but his enthusiasm and good willingness attracted secondary teachers who collaborated with him. His environment constitutes an early example of a mixed group of university professors and school teachers working on didactic problems.

## II. The present

The international panorama is changed since the period I have considered before. The second after world war saw the raise of important initiatives, which slowly lead the community of mathematics educators to become a community of researchers in the new discipline of mathematics education, see (Bishop, 1992; Dreyfus and Paola, 2004; Freudenthal, 1968-1969; Kaufman, B.A. and Steiner, 1968-1969; Niss, 1999; Sierpiska and Kilpatrick, 1998). The wrench with the past was marked by the creation of the journal *Educational Studies in Mathematics* in 1968, which initially gathered the contributions of mathematics teachers and university mathematicians. This was the time of the birth of the ICME conferences. In this international movement Italy was represented by few persons. One of them, the secondary teacher Emma Castelnuovo, daughter of Guido, was member of the first editorial board of *Educational Studies in Mathematics*. The impact inside the country of what was happening abroad was confined to a few groups of researchers in some Italian university. Some good projects for renewing the mathematics teaching were carried out under the guidance of mathematicians, who were interested in mathematics teaching. Until ICME 5 in Berkeley (1984) the Italian participants to ICME conferences were very few. As a consequence also the involvement in the activities of the affiliated Study Group (HPM and PME) created in 1976 was very poor. Initially the conferences of the commission for improving the mathematics teaching CIEAEM were the main bridge of Italians with the international community. The sudden increasing of the number of Italian participants at ICME 6 (1988 in Budapest) may be taken as a mark in the internationalisation of our community.

Important aspects of the development of mathematics education research in Italy until the 1990s are outlined in (Arzarello and Bartolini, 1998). Moreover, since ICME 6 (Québec, 1992) the national community of mathematics educators has issued special books containing summaries of papers authored by Italian researchers and surveys of the Italian streams of research.

I feel that the Italian community has developed its own identity and independence from the mother-community of mathematicians, nevertheless I observe remarkable elements of continuity. Firstly, though our attitude towards rigour is strongly changed, still the interest for the approach to proof in secondary school is central in

our research as for all the stages (exploring, conjecturing, proving) and for all mediators (paper and pencil, computer, mathematical instruments, language), see (Boero, 2002). Secondly, in Italy many groups of research are characterised by close collaboration of teachers and researchers in planning and carrying out educational studies. This contributes to make the relation between theory and practice less problematic than in other countries: our research has always in mind the classroom. Unfortunately the position of teachers as researchers is not officially acknowledged by the Ministry of Education and the involvement of teachers is voluntary and without official rewards. At the end, as chair of the HPM Study Group in the years 2000-2004, I can not forget the historical flavour present in many Italian works, which is a direct heritage of Enriques's and Loria's style of approaching mathematics education problems.

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