Communications

The voices and the echoes

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A distinguir me paro las voces de los ecos
[I halt to distinguish the voices from the echoes]
Antonio Machado (1997) *Retrato*

A few years ago, I read an article that discussed the Piagetian concepts of *assimilation* and *accommodation*, but none of the works from Piaget were cited; instead a recent work from another author was cited. This is not an isolated example; unfortunately it represents a practice that is all too common. Perhaps examples like this are frequent because there are many pressures for authors to highlight what is recent. We want our reference lists to be up to date. However, we need to pause and ask ourselves whether, in our quest to cite recent work, we are relying too much on the echoes and not paying enough attention to the voices. What do we lose as a profession by listening only to the echoes and not the voices? What are some of the problems of not going to the original sources?

I will discuss three benefits of paying more attention to the voices. The first is avoiding the dangers of relying on secondary sources, such as repetition of erroneous facts, oversimplification, misinterpretation, and nuances or issues that are lost in translation. The second is gaining a sense of history. For instance, we can look at how technical terms in mathematics education used today are related to other terms in the past that address similar issues, and gain a better understanding of what the new terms contribute. The third is the opportunity to learn from the masters, both present and past.

Dangers of relying too much on secondary sources

Repetition of factual errors. When using only secondary sources erroneous information can be repeated, and after many repetitions the field is no longer aware of the error. A story often repeated in books on the history of mathematics is that the ancient Egyptians used a rope with knots marking intervals of lengths 3, 4, and 5 to construct right angles (for example, Turnbull, 1961, p. 2; Dunham, 1990, p. 2; Reid, 2004, p. 2). When I first read about this method, I took a piece or rope and made knots as described to form a right triangle. In my enthusiasm, I did not ask what evidence there was that indeed the Egyptians used such a method. The story has been repeated without evidence for over a century. How did this story originate? According to Van der Waerden (1961) it is based on two facts, and a conjecture by Moritz Cantor. The two facts are that most Egyptian temples and pyramids had pretty accurate right angles, and that rope-stretchers participated in laying out an Egyptian temple. About the use of the 3-4-5 triangle, Cantor (1880) wrote:

> Let us conceive, at present yet without any substantiation, that the Egyptians knew that the three sides of length 3, 4, 5, joined as a triangle form one such with a right angle between the two shorter sides. (p. 56, my translation)

While Cantor was cautious to point out that his conjecture did not have any basis, other authors do not mention the lack of evidence. For example, Eves (1969) states "There are reports that ancient Egyptian surveyors laid out right angles by constructing 3, 4, 5 triangles with a rope divided into 12 equal parts by 11 knots" (p. 47). Eves is careful to mention that there is no evidence that the Egyptians knew even a particular case of the Pythagorean theorem. Other authors are less careful and use the story about the 3-4-5 triangle with the Pythagorean theorem. After many repetitions the story is told as if it were a fact, even though some authors have pointed out that there is no evidence for such assertions (Kline, 1972, p. 20; Fey, 1989, p. 68).

Oversimplification. Given that we usually frame our studies in relation to previous work in the field we need to synthesize and summarize what others have written. It is almost unavoidable that some oversimplifications will creep in. For example, two authors [1], in their work on problem solving state that Pólya "described the problem-solving process as a linear progression from one phase to the next". Pólya (1957), however, does not state that the process of solving a problem happens in a linear progression. Quite the contrary, he states, "Trying to find the solution, we may repeatedly change our point of view, our way of looking at the problem. We have to shift our position again and again" (p. 5). According to Pólya (1968), "Solving a problem is an extremely complex process. No description or theory of this process can exhaust its manifold aspects, any description or theory of it is bound to be incomplete, schematic, highly simplified" (p. 145).

Misinterpretation. Concepts and ideas in mathematics education are complex and multifaceted. Scholars in the field coin new terms and expressions to define different constructs or they use old words with new meanings. It is often hard in mathematics education to pinpoint exactly what we mean because it is not possible to use precise formulas like the physical sciences. Scholars before Galileo struggled to disentangle the concepts of impetus, momentum, force, and energy. It took centuries, and the capability to measure time precisely, to distinguish the momentum from the kinetic energy of a body in motion. In the two cases both velocity v and mass m of the body are important. Both the momentum and the kinetic energy of a body increase if the velocity increases. For two bodies moving at the same velocity, both momentum and kinetic energy are bigger for the body with the bigger mass. With such descriptions it is hard to understand why momentum and kinetic energy are different physical concepts. To understand and distinguish the two concepts, we need precise quantitative definitions: momentum is mv, and kinetic energy $\frac{1}{2}mv^2$. In most cases, it is not possible to quantify concepts related to mathematics education in such precise terms. Our explanations of what the new terms mean, and of the new meanings we want to attach to old words, are likely to be misinterpreted. In a secondary source, it is possible that we are not reading the meaning intended by the original author but a misinterpretation from the secondary source author.

Lost in translation. Not everything that is valuable in mathematics education is written in English nor is it eventually translated into English. Furthermore, in many cases, when the work is finally translated, the delay could amount to decades, as was the case with Piaget and Vygotsky. There are more recent examples where seminal work enriches the practice and theoretical perspectives of researchers in several countries years before that work is translated into English. Researchers who only read sources in one language will miss important and relevant work. On the other hand, it would be impossible to learn all the languages in which important ideas in mathematics education are written, and necessarily we rely on translations. However, we need to be especially cautious when doing so. Some terms are challenging to translate from one language or culture to another, not just in mathematics education (Cassin, 2014). Words that represent new constructs or ideas are often especially difficult to translate.

A sense of history

By tracing how new technical terms used by researchers relate to older terms that address similar issues in mathematics education we can develop a better sense of the field's development. Joe Crosswhite, a great practitioner, valued research to the extent that it informs practice. Crosswhite (1987) made this request: "I ask researchers to meet us where we are. Show an awareness of what has gone before. When you bring new terms into the lexicon, tell us how they are similar to or different from the terms with which we are familiar" (p. 269). Following this advice will not only benefit practice but also research. For example, it can be very illuminating for researchers to understand the similarities and differences among constructs such as profound understanding of fundamental mathematics (Ma, 1999), pedagogical content knowledge in mathematics (Shulman, 1986), and mathematical knowledge for teaching (Ball, Thames & Phelps, 2008). By contrasting the new terms with the old ones we can also develop a better feeling for whether the field is actually moving forward, or whether we are rediscovering again and again the same issues under different names.

Tracking back the origin of ideas can also give us a better sense of the complexities of giving proper credit for discoveries or new ideas. There are many instances in the history of mathematics where a theorem is associated with the name of a mathematician who was not the first one to state or prove the theorem. In some cases the theorem was published but ignored by the community for years or even decades until the theorem was later rediscovered or used by a better known or more influential mathematician. Misattribution in mathematics is well documented by Boyer (1968) where some 30 cases are reported for a period of about 150 years (late seventeenth to beginning nineteenth centuries).

Likewise, in mathematics education there are instances where the universal dissemination of concepts is not due to the originator of the terms. For example, thanks to Skemp, the concepts of *relational understanding* and *instrumental understanding* are widely recognized and used by mathematics educators in many countries. The terms will be linked to Skemp's name, and rightly so, even though he did not invent the terms. Skemp (1987, p. 153) attributes the origin of the terms to Mellin-Olsen.

Learn from the masters

The brilliant mathematician Abel stressed the importance of studying the masters: "It appears to me that if one wants to make progress in mathematics, one should study the masters and not the pupils" (marginal observation in Abel's mathematical notebook, as quoted by Ore, 1974, p. 138). I recommend to my doctoral students, when they are interested in a topic, to identify the masters who have contributed to the topic. Of course, I emphasize the need to include the masters from the past, not only from the present.

Another way to give students the opportunity to meet ideas from the masters is, of course, to include writings from the masters in doctoral courses. For example, in one inquiry course for doctoral students in mathematics education, we read the chapter "Re-invention" by Freudenthal (1973). Students are frequently surprised how insightful and relevant they find the ideas of such classics. They also realize that proposing great ideas to the education field will not automatically have an impact on the teaching and learning of mathematics in a systemic way.

The benefits of identifying masters from the past are not restricted to doctoral students. From time to time, some in the field make efforts to bring to the attention of new generations the work of some of the giants on whose shoulders we all stand. Sometimes it is by republishing some of their seminal papers in collections (for example, Carpenter, Dossey & Koehler, 2004), or by republishing longer contributions (for instance, Fawcett, 1995/1938). Other times it is by discussing the work and impact as a whole of some of the masters (Lindquist, 1995; Noddings, 1994; Cooney, 1995).

Final comments

Through the benefits described above I have made the case that scholars in mathematics education would benefit by following Machado's example and pause to distinguish the voices from the echoes. Going back to the original sources can certainly help put things in perspective. Listening to the voices is almost always illuminating and inspiring, and quite often also a sobering experience.

Notes

[1] "de cuyo[s] nombre[s] no quiero acordarme" (Cervantes, 1608, f. 1).

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Mathematics education research as study

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I recently participated in a conference organized by the education doctoral network of the Université du Québec (Réseau-UQ) about the grounding of educational research in society. This conference brought back to me memories of Kilpatrick's (1981) article in FLM, where he pleads for the *reasonable ineffectiveness of research in mathematics education.* Through deconstructing the dichotomy of basic-fundamental and applied-pragmatic research, he argues that this dichotomy rests mostly in the eye of the observer who reads and is inspired by the research studies conducted. As well, he presents the following (strong) position:

> Too many mathematics educators have the wrong idea about research. They give most of their attention to the results. They think it is primarily important for teachers to know the results of the research on a given topic. They give a high priority to summarizing and disseminating research results so that teachers can understand them. In a nontrivial sense, however, the results are the least important aspect of a research study [...] A researcher makes a contribution to our field by providing us with alternative constructs to work with that illuminate our world in a new way, and not simply by piling up a mass of data and results. (p. 27)

Kilpatrick's perspective on mathematics education research is quite appealing to me, and participating in similar debates in the conference, I was drawn into developing a view of research strongly grounded in society and practice, but not along the usual lines of offering answers to society's questions or problems. In the following, I unfold some of these (very personal) ideas.

My argument, in a nutshell

> For a long time research outcomes have influenced the reality of mathematics instruction and mathematical learning on a very small scale only. Research has followed the need of school practice rather than hurrying on ahead. (Bauersfeld, 1977)

Influenced by Bauersfeld's above proposal, it is my contention that the role of mathematics education researchers is not to follow and answer society's or practice's problems and needs, but to attempt to bring them forward by hurrying ahead, by aiming to participate in and push the continually evolving dynamic of society and practices. In my view, the role of the researcher is at its core (1) to conduct thought experiments on ideas, through (2) developing distinctions for thinking about and understanding mathematics teaching, learning, and practices, and thus (3) to generate ideas to bring practices and society forward. The researcher's role is to generate, using Bateson's (2000) words, differences that make a difference or, following St-Exupéry, to "mettre des forces en mouvement". In short, research is not geared toward truth-seeking, but the generation of ideas.

Research and researcher's activity

My view of the role of research and researchers in mathematics education is not reduced to providing answers or information for practice or society, in similar vein to Charlot's (1995) comments about educational research:

> [...] les sciences de l'éducation ne sont ni un ensemble de savoirs réflexions sur les pratiques d'éducation et de formation, ni un ensemble de techniques permettant d'améliorer l'efficacité de ces pratiques. Non que de telles pratiques et techniques soient sans intérêt, mais parce qu'une "science" vise à produire de l'intelligibilité (et non pas, en tout cas pas directement, de l'efficacité), ce qui implique une rupture avec le sens commun que la simple réflexion sur les pratiques ne peut suffire à produire. (p. 23)

Studies that attempt to "tell" the right ways to teach algebra or operations on functions, or to illustrate precisely what students learn through this and that activity about fractions, are interesting in themselves; but their contributions to the field are sporadic and limited. In addition, as Biesta (2007, 2010) explains, studies focused on "evidences" of "what works" are doomed to failure since education is value-based and not information or evidence-based. But, some will ask, what use is research then? Two caricatures are possible in answer to this question. One is to assert that research is at the service of society and practice, that this is its goal, that this is how taxpayers' money is to be used, that research needs to be and *should* be useful to offer paths to solving problems of practice: a perspective that reminds us of Schön's (1983) technical-rationality model where research offers pre-determined tools to solve already known problems. A second possible caricature is to say that research in mathematics education is useless, that we *should not* pay attention to finding practical use, that we will see later on if any outcomes can be generated: this is what some people call fundamental research.

My position is that both caricatures are untenable and unbearable. As a researcher interested in questions of mathematics teaching, learning, and practices, I think that the key resides neither in the technical-useful nor the fundamental approaches. To reuse Maturana and Varela's (1987) ideas, one can think of walking on the razor's edge that separates the Scylla of practice (the partial but calculated survival) and the Charybdis of fundamental research (the random all-or-nothing future).

The researcher, the studies and the practice

At the core of research activity, on the razor's edge, there is for me this fundamental importance of deepening ideas, concepts: to push them, to explore them, to extend them. The more I invest myself in research, the more it is akin to the notion of studies that one finds in the arts: to conduct studies, to study ideas, to test ideas, to offer and try distinctions, to make attempts, to see what it gives, to mingle and intermingle, to dig deeper: in short, continually to conduct studies and to be studying.

One can think of the *nature mortes*/still lives realized by painters like Van Gogh, Cézanne, Renoir, and others. Or think of Monet's series of haystack paintings. I do not believe that these paintings, these *œuvres*, were meant to show us what apples, onions, knifes or tables (should) look like! My understanding is that they were aimed at studying, attempting perspectives, techniques, ideas, to work them out, to texturize, to imagine ways of doing, and so forth; and often to continue, push, or develop a movement, an ideology, an understanding. I suggest borrowing this as a metaphor to conceptualize mathematics education research and the researcher's role (in society and for practice). The researcher in mathematics education, through his/her studies, is also attempting ideas, testing them, offering and creating distinctions, generating ideas, discarding them; directing attention to these proposed distinctions/differences that he/she as an observer considers as distinctions/differences worth paying attention to. Through conducting studies, he/she offers a vision for practice, some tests, some possibilities and impossibilities. Research studies aim to provoke thinking, to make people reflect, to offer ways of seeing (thinking). In their writing, their texts, researchers offer textures, ideas, ways of seeing, in similar or different ways.

Through the researchers' suggestions, attempts, explorations and proposals, he/she becomes someone who, as Bauersfeld (1977) suggests, hurries ahead, brings forward, throws ideas, with the intention of provoking thinking. And, as Wolcott (1994) reminds us, research is geared toward inspiring, not answering:

> There is simply no way one can get from a descriptive account of what is to a prescriptive account of what should be done about it. Those are value judgments. Granted, such judgments are critical to the work of practicing educators. It is appropriate for them to seek whatever help they can, and for us to be prepared to offer help, but we need to clearly mark the boundaries where research stops and reform begins [...] The big value judgments are easy to spot because words like *should* and *ought* abound in sentences containing them. (p. 132)

This is in line with what Standish (2005) explains: profound changes in educational practices do not come out of empirical research illustrating some efficiency or interest for this or that technique, but arise from paradigm changes that orient practices and actions. It is these paradigm changes, coming from conceptualizations or theorizing, that provoke changes: the entire behaviourist or constructivist wave in education is a case in point. Another aspect that Standish highlights, and others like Piaget have said the same, is that a theory, as good as it can be, is not produced to be applied, but mainly to inspire, to help better understand situations and phenomena, and thus to aim at working better or improving practices. Again, research is not geared toward seeking truth, but toward the generation of ideas.

The role of research is to inspire, to generate thoughts, to make us think of/about practice. It is indeed through these conceptual bundles, these ideas, these propositions, that research brings practice and society forward. These exercises, these studies, are very *practical*, because they are oriented by and for mathematics teaching, learning, and practices: not in the utilitarian sense of what does or does not work (remember Biesta's, 2007, 2010, work), but to provoke ideas, to make us think, to "mettre des forces en mouvement," as St-Exupéry says.

In the perspective of studies that I offer here (coherently, this communication is also a study in itself, aiming to offer distinctions to make us think), progress is not conceived through obtaining solutions and answers to problems, but through how things go forward, how these forces have been brought into play and the new questions that are now available or developed following these studies. This is what Dewey (1910) meant about scientific progress more than 100 years ago:

> The conviction persists, though history shows it to be a hallucination, that all the questions that the human mind has asked are questions that can be answered in terms of the alternatives that the questions themselves

present. But, in fact, intellectual progress usually occurs through sheer abandonment of questions together with both of the alternatives they assume—an abandonment that results from their decreasing vitality and a change of urgent interest. We do not solve them: we get over them. Old questions are solved by disappearing, evaporating, while new questions corresponding to the changed attitude of endeavor and preference take their place. (pp. 18-19)

Thus, through their studies, researchers are not lagging behind and answering society's or practice's questions. They are grounded in society and practices by attempting to push them forward, to go beyond practice's and society's questions and problems. This is how researchers participate in the continual evolving dynamic of society. Doing otherwise would be attempting to keep society and practice stagnant and fixed, interested only in questions of the moment. Hence research is not there to answer society's and practice's problems, but maybe, following Dewey, to help society and practice perceive that their current problems are to be addressed for them to be surpassed.

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From the archives

Editor's note: *The following remarks are extracted (and slightly edited) from an article by Jeremy Kilpatrick (1981), published in FLM2(2), to which Proulx refers in the preceding communication.*

Why is lack of attention to theory such a serious problem? I contend that it is only through a theoretical context that empirical research procedures and findings can be applied. Each empirical research study in mathematics education deals with a unique, limited, multi-dimensional situation, and any attempt to link the situation considered in the study with one's own "practical" situation requires an act of extrapolation. Extrapolation requires, however, that one embed the two situations in a common theoretical framework so that one can judge their similarity in various respects. As the old adage has it. "There is nothing so practical as a good theory." Kerlinger (1977) has argued that "the basic purpose of scientific research is theory" (p. 5) and that "there is little direct connection between research and educational practice" (p. 5). The effect of research on educational practice is *indirect*; it is mediated through theory. As Kerlinger points out, two factors that in the long run hinder the effectiveness of educational research are the twin demands for payoff and relevance. Such demands short-circuit the theory-building process.

Let us consider some examples of how theory has, or has not, affected practice in mathematics education. A frequently cited example (Cronbach & Suppes 1979; Resnick & Ford, 1981) is E. L. Thorndike's influence on the teaching of arithmetic during the early years of this century. There is no doubt that Thorndike, through his research, his teaching, and, most especially, his analysis of the psychology of arithmetic, substantially influenced the teaching of school arithmetic in the United States. He was one of the few educational theorists to be actively concerned with the nuts and bolts of curriculum building. His theoretical idea had an impact in the classroom largely because he himself (and his students) analysed textbooks in the light of his theory and made concrete suggestions for changes. His theory was his hammer; he looked around and saw the arithmetic curriculum as something to pound. One should perhaps note that he did not have much competition at the time and that he was extremely energetic in his efforts to apply his theoretical ideas. He was not, strictly speaking, a mathematics educator, and his research, strictly speaking, was not research in mathematics education, but we put it there quite happily. He is a notable exception to the charge that researchers do not influence practice in our field.

A second example is Piaget, also—needless to say—not a mathematics educator. Groen (1978) has assessed the impact of Piaget's theoretical ideas on educational practice. And he devotes one section of his assessment to mathematics. Goren begins by noting that "the hard core of Piagetian theory is replete with mathematical analogies" (p. 299), and consequently, "it is not surprising that there are many parallels between mathematics education and Piaget's own ideas" (p. 299). Groen contends that, usually, rather than Piaget influencing the teaching of mathematics, it was the other way round—mathematics influenced Piaget's thinking. Groen then raises the issue of discovery learning and argues—with considerable justification—that on this issue the influential theorist has been not Piaget, but Pólya. Further, he argues that with respect to "the notions of mathematical competence underlying the 'new math' curricula" (p. 300), the applied research done under the Piagetian influence dealt with highly specific problems and was difficult to generalize from. Groen concludes with an analysis of Copeland's book for elementary school teachers on the teaching of mathematics. He claims Copeland gives a one-sided view of Piagetian theory that emphasizes its static aspects and that tends to confuse mathematical structure with Piaget's more dynamic view of structure.

One might reasonably conclude from Groen's assessment that Piaget's ideas had not had much influence upon mathematics teachers. A more valid conclusion is that Piaget's ideas, as the teachers understand them, have had a profound impact, but this impact is often difficult to discern clearly. In countless classrooms today, mathematics teachers are dealing with children and teaching their subject matter in the light of what they believe to be Piaget's ideas. It is part of the professional baggage they picked up in college that is still with them, and it is heavily reinforced by the processional culture in which they live. Although Groen apparently could not find much of an overt Piagetian influence on mathematics education, the influence has been substantial, but largely covert and indirect.

Let us consider a final example of the influence of theory on practice in mathematics education. Several years ago, Stake and Easley directed a series of case studies of science and mathematics teaching for the National Science Foundation (see Fey, 1979). They found a number of secondary mathematics teachers who offered, as justification for teaching their subject, the argument that the study of mathematics improves one's ability to think logically:

> "I can teach them to think logically about real problems in their lives today."
>
> "Mathematics can teach the student how to think logically and that process can carry over to anything. To be able to start with a set of facts and reason through to a conclusion is a powerful skill to have." (Quoted in Fey, 1979, p. 498)

These teachers had clearly rejected Thorndike's findings concerning the lack of transfer of the disciplines—if indeed they had ever heard of these findings—and had adopted a view that has echoes of faculty psychology. Presumably this view was not dominant in their preservice education program, which doubtless gave them much sounder, more scientific justifications for the teaching of mathematics. These justifications either had not survived or had never been accepted. The educational psychology textbooks are fairly clear on this issue: one cannot train logical reasoning ability through specific school subjects like mathematics. This is part of the received wisdom of the school-of-education culture, and these teachers must have been taught it. We have here a case in which current theories have not had much impact on teachers' thinking, and presumably their practice.

These three examples are intended to illustrate some of the various and perhaps perverse ways in which theory influences practice in mathematics education. As Kerlinger and others have noted, the influence is primarily indirect. Unless someone forceful and dominant such as Thorndike acts on the system, one must look hard to detect how the influence is occurring. A common procedure is for the theorist to set forth his views and then for a transmitter, such as Copeland, to provide a simplified, and perhaps somewhat garbled, version for a larger public of teachers. The transmission network, however, is complex. A Piaget introduces a new idea, which resonates for someone else. who incorporates it into a talk, paper, or book, and other mathematics educators begin to use it in their speaking or writing. Gradually, the idea comes into the culture of mathematics education and is picked up by teachers in practice. Sometimes the idea is banned from colleges of education—like faculty psychology—but lurks in the culture like a virus to strike down the receptive practitioner.

Sometimes the force of theory is felt merely by providing a name for a construct that people have been grappling with but have not articulated. Attribution theory and expectation theory seem thus far to have contributed to mathematics education in this fashion; researchers in mathematics education are intrigued by the constructs, but they have not been much concerned with following out the ramifications of the theories. Naming, however, is a powerful force, as Adam must have discovered. Hadamard (1947), in discussing Newton's contributions to the calculus, said it aptly:

> The creation of a word or a notation for a class of ideas may be, and often is, a scientific fact of very great importance, because it means connecting these ideas together in our subsequent thought. (p. 38)

Pimm (1981, p. 48) quotes Higginson's anagram, "renameing is remeaning", from which it follows that "nam(e)ing is meaning". We need the constructs and networks of theory to help us think about things—about the phenomena we confront as mathematics educators. We ought to be giving more serious attention to the theoretical underpinnings of our work, and we need to make more explicit and coherent the assumptions we are making, the point of view we are adopting, and the frame of reference that surrounds the picture we are trying to paint. As long as we ignore the theoretical contexts of our research work in mathematics education, it will remain lifeless and ineffective.

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