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Effect sizes, epistemological issues, and identity of mathematics education research: a commentary on editorial 102(1)

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1 Stepping back

In the last editorial (Bakker et al., 2019), various editors of journals in mathematics education research address some issues related to effect sizes in quantitative studies for mathematics education and their usually accompanying analyses and meta-analyses. These issues appear timely, as discussions on effect sizes are becoming overtly present in our field, but also, and in some places mainly, in the discourse of various practitioners in school milieus.

Being acquainted with this kind of work as commentators (Proulx, 2017, 2019), we take notice of the relevance of the points raised by the editors. In this, we saw an opportunity to step back even further and raise some issues of importance about our entire research field. In essence, we felt that in addition to the technical, methodological, and empirical questions raised by the editors in their 12 points, there were a number of epistemological matters worth underlining in order to stimulate and engage fruitful reflections in our research field.

On page 6, the editors claim that "One important aspect of situating the research is realizing that the role and interpretation of effect size will depend on the goal of the study." Although they follow this with explanations related to another kind of studies interested in effect size (causal ones where "X influences Y"), we see this claim as fundamental to our entire field in relation to considering these effect size–related-prone studies. In most work that aims for effect sizes, the intention is to investigate the effectiveness of an intervention or a causal relation between two variables in relation to effect. Obviously, asserting through research that intervention A makes students learn mathematics is very attractive to school practitioners. Along Hattie's (2009) words, practitioners might then be under the impression that these researchers have indeed found the Holy Grail!

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Not denying the value of these studies, as researchers, we all know that there is much more to "educational studies in mathematics" than answering such so-called pragmatic concerns of school milieus. This raises significant questions about the goals and legitimacy of our research field, and its relation to schooling practices. To address these issues, in the following section, we highlight the importance of also discussing why, in the greater scheme of mathematics education research, one conducts and presents a given study. That is, while Bakker et al. discuss the importance of making explicit *how* a given measure matters, we believe that it is also essential to engage seriously the question of what the study itself hopes to contribute to go beyond the narrowed, specified significance usually articulated in a research question.

2 Some epistemological reflections about our research field

A first reflection about the goals of our research work in relation to school practice can be triggered by the words of Heinrich Bauersfeld. He boldly asserted that research has very limited practical effects on already formulated concerns and should instead aim to identify new issues, new needs, and new concerns. In his words:

> 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, p. 231)

In this sense, one could even say that the main goal of research is to formulate questions that are not even yet thought of (in schools or elsewhere) about mathematics education, in order to participate in the wider advancement of mathematics education itself (see some of our thoughts on this in Proulx, 2015). Here is an opportunity to reflect on how one's research questions are actually rooted in some "existing" needs, and how much they contribute to highlight new concerns beyond the need of school practices. In relation with effect size, one could think in terms of what a given "size" talks to us about, what kind of "needs" it might answer, and, most importantly, according to Bauersfeld, what new questions it raises. In this sense, it becomes more than knowing or having "information" about what effect size and how, but mostly of foresee(k)ing new understandings and orientations through it, and to gain from/through it.

Another reflection is related to how the search for effect size seems to relate to the hope for "effective" interventions that provoke mathematical learning or other desired outcomes. The quest for "what works," as Biesta (2007, 2010) calls it, can also derail us at times from *why* these interventions allegedly work. There is a danger in these "what works" of reinforcing a utilitarian side of our field. Thus, in this quest, one might wish not to let go of the scientific *why* question in favor of only a pragmatic *what* one (as is said to have happened in the so-called *Dark Ages* in the history of mathematics itself, see Struik, 1987). At the same time, we might want to be careful not to oppose too emphatically the *what* and the *why* questions, and fall into an unfortunate dichotomy of applied/pure research that has plagued our mathematician colleagues for years, and still does in some places. For us, sternly engaging in the question of explaining observed results (good, bad, or inconclusive) can be another way to nuance the impression that research is merely geared toward the identification of "conclusive-evidence-based" practices and the like. Appreciating to what extent a phenomenon might be observed and looking for those that are widespread does not mean that it could or should be controlled or turn into a tool for ones' end (no matter how noble), reducing all far-reaching empirical

research to technocratic endeavors. As a research field, taking time to discuss how concerns about effect size within a study might serve more than practical interventions appears fundamental to gain from such studies, stimulate supplemental inquiries, and succeed in going beyond merely accumulating "information."

Finally, a reflection that is in our view the most significant is related to the improvement of kids' mathematical learning in schools. The story has it that, supposedly around the 1990s, Kath Hart gave a lecture at the *Psychology of Mathematics Education* (PME) conference and at some point said something like "We all agree that our main goal is to aim for having kids learn and understand mathematics better in schools." It is said that the reaction in the audience was somewhat mitigated, some agreeing and some strongly disagreeing (and aiming for the field to focus instead on studying a phenomenon for itself, or that issues happening outside schooling practices also be considered). The complementary story is that of Yves Chevallard, who in one of his conference addresses is said to have asserted the opposite: "Our goal is not oriented toward improving schoolkids' success in mathematics but on studying phenomena related to *didactique des mathématiques*." Supposedly, again, the audience appeared divided.

That these two almost mythical Hart/Chevallard stories really happened is not as important as the fact that they *do* circulate in discussions between researchers in our field. The fact that they frequently circulate highlights that this issue of what could be called the epistemological foundations of our field is not so simple or reducible to an easy answer, as obvious or well-traced as one would like it to be. As the stories show, we need to acknowledge that numerous researchers in our field work on issues that are simply not related to schooling or classroom phenomena. Sadly, their work would become obsolete if we were to gear ourselves only to school improvement or kids' learning. So we see here an occasion for researchers to explore through discussions of their research design (including the question of effect size), what is meant by/to be "improved."

As a field, we are well-advised to pay attention to, and even to conduct, these meta-analyses and gain knowledge from these effect sizes. At the same time, we also acknowledge that these studies are aligned toward and driven by particular research agendas, and these agendas are not representative of our entire research field. Moreover, through their possibly convincing and even seductive nature, can these studies about effect size run the risk of overshadowing what our research field, in all its variety, is all about? This might be a question worth pondering.

3 So then what? So then why?

We believe that epistemological issues related to the foundations of our field are important and need to continue to be addressed. These epistemological discussions, of course, can happen in a variety of ways: in more formal venues like journals and conference papers, in informal hallway discussions and email conversations, in the media (social, written, oral), as well as inside the school walls themselves where some of us frequently engage (through various research goals).

The resurgence of effect sizes and of the place of quantitative studies for our field is salutary for that matter. At first sight, it might seem that the question of effect size and the sturdiness of given result according to scale is mostly a matter of how these can effectively be applied in schools. This is, of course, a limited and somewhat debatable view. Discussing why effect size matters regarding new questions we might ask, like what is being gained, why the effect takes place, or what it tells us about mathematics education, are only a few examples of alternative

purposes. We believe that discussions about these issues share the potential to force us to make these matters explicit and make them salient objects of interaction in and around our field. They represent an occasion to understand (ourselves) better, and for others to appreciate better what we do as a research community.

Our field has gained maturity over the years. We might not be where we were some 25 years ago around the publication of Sierpinska and Kilpatrick's (1998) edited volume. Let us not forget, however, the importance of these discussions to the identity of our research field and the fruitfulness of it for all of us, and for those with whom we interact widely.

It is, yet again, an invitation.

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