INTRODUCTION

In this debate, in arguing for the position that ‘Research should not inform teaching.’ we present three lines of reasoning. First, we observe that despite many years of research, research has had very little impact on teaching and begin our argument by presenting scientific evidence and other sound reasons as to why this is so. Hence given the fact that research does not inform teaching we should, as a community, now accept that research clearly should not inform teaching. Our second argument rests in the observation if research is to influence teaching then it must be accessible to teachers and that the current publishing practices militate against this. Finally, much research is now funded by policy-initiated programmes, with research agendas being tacitly steered towards policy directives. Thus rather than researchers being free to follow interesting lines of thought and design independent research studies that identify and address the crucial questions regarding teaching and learning, funding research ends up conforming to policy-formed questions that are rooted in current knowledge. This results in maintenance of the status quo in teaching rather than radically changing it.

FIRST ARGUMENT – RESEARCH HAS NOT CONTRIBUTED TO SIGNIFICANT CHANGES IN MATHEMATICS TEACHING, BECAUSE IT CANNOT

Visiting a typical classroom, it is obvious that teaching has changed very little over time, with students and teachers coming together to teach and learn mathematics in ways that would be familiar to anyone schooled 50 or more years ago. The artefacts may look different, with chalk and blackboard being replaced by pens and whiteboard (or an electronic smartboard version of these) and paper textbooks may have given way to printed work sheets or computer tablets, but the substantial content of what is on the boards, or in the students’ hands, has not greatly changed. The teacher will present

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1 Readers should note that the opinions expressed here are not necessarily those of the authors.
something (a theory, an example, a task or problem to be solved) and students subsequently engage in activity. But surely, one might ask, there is more interaction in classrooms now and less teacher authoritarian? Our response is, yes, there is likely more interaction between the teacher and the students than might have been observed many years ago, but the dominant form of this interaction is still closed questions and answers, with a focus on getting correct answers. The climate may be less authoritarian but the teacher is still the primary authority.

Given such lack of changes in teaching, research clearly has not informed teaching, so we should stop pretending that it does or that it should. For instance, the way mathematical problem solving is taught in schools has not been much improved as evidenced by the large body of research on problem solving revealing that teachers still struggle to teach students problem solving strategies and to develop collaborative problem solving within classrooms (see, for instance, Lesh & Zawojewski, 2007). Similarly, despite the extensive research on the importance of talk and collaborative work in mathematics (see for example, Stein et al., 2008) most classrooms are still characterised by a dominance of teacher exposition followed by students working individually on tasks (Peña-Lópes, 2009). Why has research had so little impact on teaching? We argue that two of the reasons why research has not informed teaching are, first, the nature of the research itself and second in how research outcomes are made public.

The bedrock of scientific research leading to scientific advancement is experimentation, yet few true experiments can be found within our academic field. Indeed, even where there are experimental studies, the cumulative effect of these is slow. To take a concept like scaffolding, which many would agree is key to successful teaching, Bakker, Smit and Wegerif (2015, p. 1056) wrote ‘We predict it may well take a decade before there are enough experimental studies of sufficient quality to quantify the gains of various scaffolding approaches compared to regular teaching.’ Yes, some studies do have designs close to the scientific experiment (pre- and post-test designs, design experiments) but without randomised allocation to intervention groups and control over multiple variables it is not possible to calculate the effect of the implemented innovations on mathematics teaching and so make strong comparisons with other innovations, or even with the ‘normal’ or untreated classrooms. The key issue here is that the contexts of classrooms are far removed from the science laboratory – control of variables is much closer to being achievable in the latter than in the former. As Mason (2013) points out, all teaching practices are highly contextualised, so there are no generalizable practices, only generalizable principles. Since principles are based in local, ethical and moral considerations of what constitutes a good education, how can research provide global or even local recommendations for practice? Following Bakker, Smit, Wegerif and Mason, we might conclude that at best mathematics education research may only be able to produce ‘fuzzy’ generalisations that cannot provide concrete insights that easily can be applied in teaching.
Compounding the lack of experimental studies, the dominance of research involving small scale case-studies limits the generalizability of findings. Although Yin (2014) for instance argues that analytic generalisations might be valid inferences drawn from case studies, this is highly debated. For example, is there such a thing as a ‘neutral’ or context-general case study where the situatedness of the case-study be disregarded? As noted classrooms are highly contextual and patterns observed in one classroom might be differently composed in other classrooms. Small case studies might be fine if they built on each other’s findings but there is little evidence of that in the research literature (Mitchell & Charmaz 1996). We would agree with Nietzsche in his observation that in doing science ‘one should not wish to divest existence of its rich ambiguity’ (1974, p 335). In addition too many case studies are descriptive – they tell us what is happening, and in many cases why current practices are not effective (see for example, Ensor et al, 2009) but without recommending ways to improve practice. At best, case-studies may go further and theorise about the practices observed but the findings then are explanatory – researching why teaching has not changed much does not necessarily provide empirical evidence that informs how to bring about change.

In addition to case studies, other frequently used research methods are observation, questionnaires and interviews. Generalisation might not be the issue when questionnaires or large-scale assessments are used to study teaching and learning, as large, representative samples might be applied (see, for instance, Schleicher, 2012). However, large-scale studies mean simplifying the educational context or leaving out “the friction” in order to isolate and enable study of the phenomena into which we want to gain insights. The PISA and TIMSS studies for instance, use student questionnaires to study instructional quality (Mullis & Martin, 2013; OECD, 2016), but how do we know either what the relationship is between student reports on instructional quality and the actual quality or even if what students think is good is actually effective? What is researched is perceived instructional quality and what is perceived as effective is not necessarily so.

Even if large-scale studies were to make recommendations for teaching, then the unintended consequences may outweigh the intended ones. For instance, aims such as sharing ideas on best practices in teaching has been lost in the promotion of international league tables by policy makers (Auld & Morris, 2016). Rather than raising standards of teaching or bringing best practices into view, the league table mentality and the jockeying for position within ranks has resulted in a narrowing of practices – a reduction in taking risks and trying out new pedagogies for fear of reduced ‘standards’ (Broadfoot, 2000). The ultimate effect is a lowering of spaces for innovation but innovation rests on risk taking and learning from failure.

Across both small- and large-scale studies, a lack of unified theories and agreed ‘best practice’ methods further limits the applicability of research findings to teaching. A good example of a situation where findings ‘do not add up’ is the manipulatives debate
Askew and Nortvedt

(McNeil & Jarvin, 2007). While some researchers found that manipulatives help students with mathematics learning disabilities (MLD) learn mathematics, other researchers found that the manipulatives had a negative effect on the learning of MLD students. McNeil and Jarvin (2007) in their review concluded this is connected to how the research was carried out.

The key issue is that in most, if not all, mathematics education research the phenomenon under investigation is complex and rich and contextual and intertwined. Research necessarily simplifies the object of investigation, can never account for all the variables treating the complex as complicated (in the sense of ordered and predictable (see, for instance, Leder & Grootenboer, 2005). Consequently, research findings rarely, if ever, transfer to other contexts. Take the case of the PISA study and Finland. Finland was among the top countries in the early PISA studies (that is, in 2000 and 2003, see OECD, 2004) and, not surprisingly, many, researchers and policy makers wanted to learn from Finland. However, Finish researchers themselves, thought that this could not easily be done and they themselves struggled to understand these outcomes, stating that there was:

‘no one single explanation for the result. Rather, the successful performance of Finnish students seems to be attributable to a web of interrelated factors having to do with comprehensive pedagogy, students’ own interests and leisure activities, the structure of the education system, teacher education, school practices and, in the end, Finnish culture.’ (Välijärvi, Linnakylä, Kupari, Reinikainen, & Arffman, 2002, p. 4)

Similarly, there is much interest in what is being referred to as ‘Singapore mathematics’ but as B. Kaur from Singapore pointed out in her IMCE17 plenary, there is not agreement within Singapore on what constitutes best practice (Kaur, 2016).

SECOND ARGUMENT: DISSEMINATION OF RESEARCH FINDINGS PREVENTS RESEARCH FROM INFORMING TEACHING

Not only are research methods problematic, but also the publication of research outcomes is problematic on account of what findings are published and also how findings are communicated to teachers. The first stumbling block lies in what might be accepted for publication – typically only original research is published. To many, this is conceived as ‘research with findings’. This leads to research being re-invented since pedagogies tried out that yield no changes in learning, have not been reported on and so neither the research nor the teaching field gets to know what has been demonstrated as not working. In the climate of accountability in Universities researchers have to be original, rather than test out or replicate previous findings. In addition, research not meeting standards for publication does not get into the public domain. For instance, review guidelines often state that researchers need to embed their empirical studies in current research traditions and theoretical paradigms to (see for instance our own PME guidelines for Research Reports). Novel research that represents a clear break with current framework and traditions or is very creative might not be considered nor
accepted for publication. There may be much research carried out that could make a
difference but we know little about the potential effect of the proposals that come from
such research.

Second, research findings are not made easily available to teachers so they do not know
what might be important. For example, a study conducted at Durham University (See,
Gorard, & Siddiqui, 2016) examined how teachers made use of research findings
showing the impact on learning that enhanced feedback can have (Hattie, &
Timperley, 2007). The teachers worked with the published research findings, but could
not put them into practice. Two main reasons for the lack of change were noted. First,
that the published findings did not provide sufficient examples of what sort of feedback
was envisaged, so the teachers could not identify what changes to practice to make.
Second, the style of writing was a barrier to engaging with the research. One teacher
was noted as saying ‘I need a translator to understand what this article is saying. I just
cannot understand what [Hattie] means and what he wants us to do.’

THIRD ARGUMENT – RESEARCH IS POLICY STEARED TOWARD
MAINTAINING EXISTING PRACTICES

Educational research is mostly funded by national or international authorities that often
want research to have an applicable outcome that is innovative or can improves some
aspect of society (see for instance European Union, 2017, 01.04). In many ways, policy
makers want to influence teaching more than research due to a desire to cater for
effective and high performing educational systems. One means to achieve this, is to ask
for educational research directed toward national educational policies, e.g. assessment
for learning (Baird et al., 2016).

Given this politicising of education, with policy makers resistant to taking risk and so
seeking evidence from large, statistical studies often conducted by economists, whilst
the concerns of many in the field for social justice and equity, which are important and
may intersect with teaching but not influence it directly, are seen as less important than
research aimed at raising standards, but in the absence of any real debate about what
standards are being set. Teaching is, inevitably, goal directed, but in the world of
standardized testing, of targets, and of the ‘no child being left behind’ policy, surely it
is the role of the researcher to be challenging such discourses of accountability, not
feeding into them (see, for instance, Berliner, 2011)?

Restricting our research to what policy makers and funding authorities see as
worthwhile means research being directed towards an agenda set from outside, not
from within the research community. As such, this might not help us as a research
community to identify the important issues that need to be addressed (Linden, 2008). A
more substantial issue is related to the need to grow as a research community, and to
arrive at a place where we do manage to direct and coordinate our research so that we
do manage to develop substantial theories and findings (Burkhardt & Schoenfeld, 2003).

**OUR ARGUMENT IN SHORT – LINKING THE ARGUMENTS**

We have argued that mathematics education research has not moved teaching practices forward, and may, perhaps, in some instances have moved teaching backwards. Let us stop deceiving ourselves that our research should inform teaching – it does not, it cannot, it should not.

As researchers, many of us come from a background of teaching and it is natural that we should want to improve the practices that we previously were members of. It is notable that researchers coming from non-teaching backgrounds are more willing to be openly critical of schooling, to point to its inadequacies but not position themselves as having answers (a classic example here is Stephen Balls’ work, see for example Ball, 1984). Those of us moving from the school to the academy would do well to recognise that our roles as researchers are very different and not mix up these roles with previous ones.

This is not to argue that work is not needed to improve teaching – it is, but that is the work of curriculum developers, who may or may not choose to research their developments, but should not be required to. We need to be clear about curriculum development and research. Curriculum development IS about informing teaching but it is not necessarily research. Research, of necessity, involves looking back – re-searching for answers rather than forward, pro-specting for solutions (Burkhardt & Schoenfeld, 2003). Creative and novel research must take place in contexts where risk taking and the potential of failure are allowed (Linden, 2008), but current funding does not encourage this and that position is unlikely to change in the near future given the fragility of global economics and lack of funding for blue-skies research.

The pressure to publish means researchers have to present their findings as new (Billig, 2013) and many researchers, working within the field of mathematics education are forced to direct their interests to classroom studies so that they can argue that their work has an ‘impact’. Acknowledging that mathematics education research should not need to inform teaching would free up researchers to pursue genuine interests, interests that may ultimately have a greater impact on practice through widening the breadth of research. Research should be free and researchers should be free to investigate what really matters – restricting our attention to research that informs teaching would limit that freedom.

With regard to findings being disseminated more widely, as researchers we are too reluctant to be prescriptive. Research findings are hedged with qualifications – too easily interpreted by teachers as a lack of confidence in the findings – and that, together with the dominant discourse of ‘reflective practitioners’ suggests that teachers have to make up their own minds about what good practice comprises. For research to have an
impact on teaching, researchers need to be less tentative in their results, but that is rare in the discourse. Perhaps those more drawn to living with certainties (even if these have to change) are drawn towards policy work, while researchers prefer to keep things open.

In summary, the lack of experiments, the risks of innovation and the heavy emphasis on ‘standards’ and difficulties in getting research findings into the hands of teachers means that research largely builds on current practices rather than proposes anything radically different. Mathematics education research must raise its game; move the gaze from small scale, non-cumulative studies to larger scale work that of necessity can then say less about actual teaching practices. And it must not buy into the discourses of policy makers that are only concerned with raising standards. And it must ‘speak’ to teachers. Only then it might begin to have an impact on teaching.

References


