Clements-Foyster Lecture

The Clements-Foyster Lecture acknowledges an eminent mathematics education researcher from Australia, New Zealand, or a South East Asian rim country, who is invited to present a keynote address at the annual MERGA conference. This annual keynote address is named in honour of Ken Clements and John Foyster who initiated and organised the first Mathematics Education Research Group of Australia Conference at Monash University in 1977. This led to the establishment of the organisation now known as MERGA.

A Deep Dive Into Mathematics Education Research in Search of Significance

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Doing significant research is critical to building the quality of mathematics education research. But doing substantively significant research is inherently difficult because we are studying the unknown. The ability to clearly articulate or 'sell' the significance of our research often poses even a greater challenge to researchers. Nevertheless, without such statements of significance we are unlikely to win grants or have papers accepted for publication. In this presentation, I consider what it means when we say that research is significant. My reflections draw upon my own experiences and those of others as part of a 'deep dive' in search of significance in mathematics education research.

The annual Clements/Foyster Lecture at MERGA conferences provide an important reminder to us of the foresight John Foyster and McKenzie (Ken) Clements showed in founding an organisation that has played such a major role in the lives and careers of so many mathematics educators since its inception in 1977. I recall being in the audience when the inaugural Clements/Foyster lecture was given by David Clarke in 2005 and although I never had the opportunity to meet John Foyster, Ken Clements was one of the first people to greet me at my maiden MERGA conference in 1991 when I was still a doctoral student. The following year, I had the honour of receiving the MERGA Early Career award and being given a hand-written certificate signed by Ken Clements. The certificate is now a valued memento of not only the award but of Ken Clements as co-founder of MERGA.

In my letter of invitation to present this lecture, it was suggested that I "take an empirically rich and theoretically robust forward-thinking approach that celebrates and thinks about the future potential of our organisation and its members". With this request in mind, I felt that a presentation going beyond just my own research was needed. I wanted to focus on an aspect of research that impacts us all and is critical to strengthening the ongoing credibility and quality of mathematics education research not just in Australasia, but more widely. After assessing high stakes grant applications, research proposals for higher degree research students, and reviewing and writing manuscripts for publication over many years, I often find myself asking "So what?" and wondered why it challenges so many of us to clearly articulate the significance of our research. According to Cai et al. (2019b) about one-third of manuscripts rejected for publication in the *Journal of Research in Mathematics Education* (JRME) received feedback from reviewers that "So what?" had not been adequately addressed despite being a fundamental aspect of doing and reporting research. In keeping with the conference theme, *Surfing the waves of mathematics education*, I decided to take a 'deep dive' into the *significance* of mathematics education research. My aim in this presentation is to raise questions and start conversations about aspects of research that we should be discussing to help build some shared understandings about doing *significant* mathematics education research.

Speaking the Same Research Language

I started my dive into the significance of research by examining how I and other MERGA researchers describe the significance of our work. First, I searched for occurrences of the term and its derivatives in papers from the MERGA 2023 conference proceedings and scrutinised the context in which the terms were used. Excluding occurrences in references and short communications, the term significance or significant occurred 84 times but only 73 of those

(2024). In J. Višňovská, E. Ross, & S. Getenet (Eds.), *Surfing the waves of mathematics education. Proceedings of the 46th annual conference of the Mathematics Education Research Group of Australasia* (pp. 3–10). Gold Coast: MERGA.

were in research papers. I was only interested in references to the substantive significance of research so mentions of statistically significant results, and use of the term to indicate an issue was noteworthy or sizeable were discounted. Just five instances remained where the term was used to refer to the substantive significance of the research and only three of those (all written by early career researchers) offered elaborations as to why their studies were significant. Of course, the significance of a study can be conveyed in ways other than citing this exact term. Drawing upon papers from the MERGA 2023 proceedings, I created a list of terms that were most frequently used to potentially convey the significance of research. However, I now consider many of those terms to be distinctly different to the meaning intended by 'significance' of research.

Armed with this set of 'synonyms' for describing the significance of our research, I extended my inquiry to a relatively recent issue of the *Mathematics Education Research Journal* (MERJ). An examination of eleven articles from the same issue revealed that the term significance was frequently used in subheadings toward the end of papers (e.g., Significance of findings) but rather than elaborate on the significance in the ensuing paragraphs, authors spoke in terms of other characteristics that typically drew upon terms from the set of so-called synonyms. At the same time, I made a point of asking educational researcher colleagues from a range of fields such as philosophy, linguistics, educational psychology, and mathematics education what the significance of research meant to them during incidental conversations. Almost without exception, even the most experienced researchers used a range of terms and phrases to explain their thinking and all acknowledged that a succinct statement was challenging—especially when not having had time to prepare a considered response. The terms and phrases they most typically used were added to my list of synonyms. From this initial shallow dive, I realised that a fundamental issue surrounding how we convey the significance of our research exists—we are not all speaking the same research language.

To guide a deeper dive into this topic, I did what all researchers do—I developed research questions. The first guiding question was: *What do we mean when we say research is significant?* Before moving deeper into my explorations of research significance, I urge you to pause reading and reflect on your own response and perhaps the responses of those around you to what we mean when we say that our research is significant.

Of interest, is the extent to which the terms and phrases you have just considered match those of other researchers. The most used terms and phrases in my list of synonyms for research significance included: importance, contribution to the field, benefits, impact, innovative solutions to a problem, implications, and extending or building new knowledge. There were others, but these were the terms and phrases most frequently mentioned by colleagues, authors of texts recommended for our higher degree research students, in papers from the MERGA 2023 conference proceedings, and in the *MERJ* issue examined in my initial investigations. From these exploratory beginnings, further questions were formulated to help me delve deeper into issues surrounding the identification and articulation of significance in mathematics education research. Questions included:

- Why is it so challenging for researchers to agree upon a definition of significant research?
- Is the significance of research different to its contribution or its implications? If so, how do they differ?
- How are significant research questions developed?
- How can we judge if a research question is significant?
- Why is it important that we *do* significant research?

The remainder of this paper is structured around responses to each of these guiding questions and draws upon examples from my own research experiences and those of others as part of a 'deep dive' in search of significance in mathematics education research.

Clarifying Key Terms

Why is it so challenging for researchers to agree upon a definition of significant research? Is the significance of research different to its contribution or its implications? If so, how do they differ? Before we can dive more deeply into what we mean by significant research, it is necessary to clarify a few key terms. The lack of consensus surrounding the meaning of key research terms is, according to Hiebert and colleagues (2023), a result of such terms being overused which has led to definitions gradually shifting. Meanwhile, Evans et al. (2014, p. 69) argue that confusion is "due to a looseness of expression" used among researchers. Conversations with established researchers from a range of disciplines, and readings of texts about doing educational research confirmed early in my dive that researchers often use a variety of terms interchangeably when talking about the significance of research. However, one term stood out from the rest—important. All colleagues and reference texts used the term 'important' at some point when discussing the significance of research. In their discussion of research agendas, Ertmer and Glazewski (2014) argue for the importance of research and do not mention significance at all. Although, when defining 'importance' they use almost the exact phrases and examples that Evans et al. (2014) use to define significance. Hiebert et al. (2023) suggest that the importance of our research is judged by its "significance, contributions, and implications" (p. 106). This statement implies that while they are connected, each of these terms refers to a different aspect of research that taken together, argue for the importance of our research.

Significance

From the literature and conversations, there seems to be consensus that significance is mostly associated with the research questions we pose and for them to be potentially significant they need to address important issues or problems in education. Arguing significance purely because there is a 'gap' in the literature is not sufficient. Perhaps the gap exists because it is just not important enough to know. Cai et al. (2019a) suggest that the significance of our research is drawn from the importance of the mathematics content or context in which the problem we are addressing is situated. They agree with Simon (2004), arguing that it is unlikely significant research will result from opportunistic research—studies undertaken purely because participants are conveniently available rather than those planned to address a recognisable problem of importance. This means that the likelihood our mathematics education research will be considered significant will depend on the degree to which it addresses a problem or issue of interest and perceived value by the community including other researchers and practitioners.

However, what is valued and considered to be an important problem in need of solving might not be the case in another place or time. Research significance is bound by context. What mathematics education research and the broader educational community perceive to be significant changes over time. Moreover, the significance of research might be viewed differently in different cultures. The implication of research significance being bound by context is that the significance of a study might not be realised for many years after it is published and might be more (or less) highly valued in another country. Consider the current research interest surrounding artificial intelligence (AI). The term AI was originally coined in the 1950's and made popular by Alan Turing when he posed the question "can machines think?" (Turing, 1950, p. 434). Interestingly, after posing this question, he questioned its significance, "Is this new question a worthy one to investigate?" (p. 434). He also made the prediction that by the end of the 20th century, the idea of machines thinking and learning will be without contradiction. Given the current pervasiveness of AI, the significance of his initial question is perhaps more important today than it was in 1950 when so many dismissed his claims as having little consequence for society. However, the contribution of Turing's paper to the field of AI was acknowledged from its release as being the first published paper to introduce the idea of testing machines for their capacity to exhibit acts of intelligence. From this example, we can surmise that unlike significance, the contribution of research remains the same regardless of time and place. This means that there is a clear distinction between the *significance* of research and its *contribution* to education research and education practice.

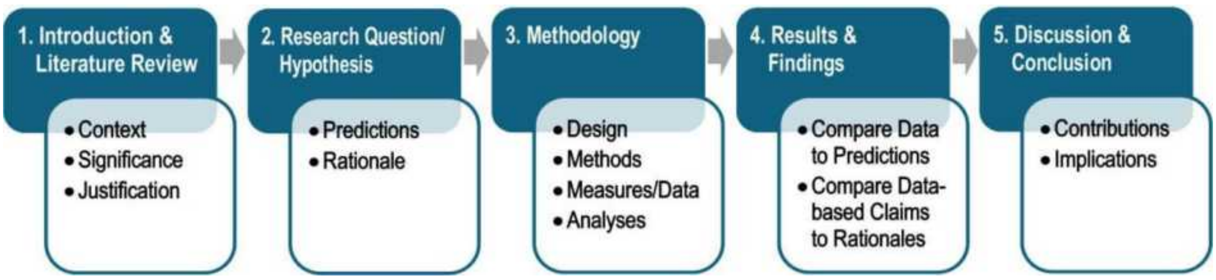
Although nowhere near the level of importance as Turing's seminal work on AI, the realisation that the significance of research is bound by context, but its contribution is not, caused me to reflect on my early experiences as a doctoral student and why my initial ideas for research were rejected by my supervisor but eventually gained traction in mathematics curricula. Emerging directly from a part-time research master's degree and full-time primary teaching to my PhD in the late 1980's I was prepared with a specific mathematics-related problem from the classroom that I wanted to explore. My proposal involved investigating the impact of a teaching intervention designed to facilitate the development of visualisation strategies in young children that would assist mental computation strategies. I had already conducted a pilot study with my kindergarten class from the previous year to refine my instructional activities. To my dismay, my supervisor quickly rejected my proposal during our first meeting on the grounds that he could not see any potential for it to be significant research. I revised my proposal to align with my supervisor's expectations and completed my PhD. As a very early career researcher, I did not question the rejection of my initial proposal. When I finished my PhD, I returned to my pilot data and decided to present it at the 1993 MERGA conference—the year after I had won the Early Career Researcher award with a paper based on my PhD research. The new paper was titled, 'Visualisation and the development of mental computation' (Bobis, 1993) and was one of two papers concerned with visualisation strategies at the conference. I recall a very large audience and feeling intimidated by some influential researchers, such as Professor Bob Wright and Alistair McIntosh sitting in the front row. As far as I was aware, no one in the audience seemed familiar with ten frames or the potential benefits of using subitising strategies to assist the noticing of structure in dot pattern arrangements or the potential for students' early number knowledge. Immediately following my presentation, Bob Wright congratulated me on such an important piece of research. Less than a year later, he had incorporated subitising into his *Learning Framework of Number*, eventually including subitising and ten frames into the New South Wales Department of Education's *Count Me In Too* (CMIT) numeracy program learning and assessment materials (Wright, 1998). Although ten frames and subitising were familiar to primary teachers involved in CMIT since 1996, they were not popularised until they were included in the 2002 *NSW Mathematics Syllabus K–6* (NSW Board of Studies, 2002). Following the conference, I was also invited to expand on my paper for a chapter in a book edited by Joanne Mulligan and Mike Mitchelmore (see Bobis, 1996). This chapter is still one of my top ten cited publications.

Reflecting on this experience caused me to question: Why was my initial proposal not judged as significant research if only a few years later it could be acknowledged as such and impact curriculum documents? I consider that there are at least two reasons for this delayed acknowledgement of significance. First, is the context in which my initial proposal was delivered. In the late 1980's, research surrounding number sense and mental computation was surging, but that of visualisation and visual imagery was still mostly confined to geometry. Its potential for improving young students' number knowledge was relatively undervalued. Additionally, my supervisor was not an expert in early years mathematics learning issues, so was not familiar with this area of research. The second major reason for its rejection was probably my own inexperience as an early career researcher to articulate a convincing argument for the potential significance of the proposed research to my supervisor—something that I was much better at doing in publications post-PhD (Bobis, 1993, 1996). Reflecting on this experience, the importance of establishing a robust case for significance early in the evolution of a research project and of clearly articulating it as such were reinforced. Without the ability to clearly articulate a case for significance, our research may never come to fruition.

If the significance of research is mostly associated with the research problems and questions we pose early in the life of a study, this means that significance can be determined before data collection. Authors of researcher guides and thesis-writing handbooks (e.g., Evans et al., 2014; Hiebert, et al., 2023; Paltridge & Starfield, 2020) invariably discuss research significance when describing the contents of the introductory chapter or opening paragraphs of research work. A colleague described research significance and contributions as the 'bookends' in her writing—a case for significance is built at the start of her work and the contributions of findings are presented at the end. The important aspect of the metaphor for my colleague was the connecting thread between the bookends. In her view, this connection ensures consistency between the aims of the study and the conclusions, providing a robust argument for the importance of the research which starts with the case for significance and ends with a discussion emphasising the value of its contributions and implications. This connecting thread is akin to Simon's (2004, p. 160) "line of reasoning" and to the "coherent chain of reasoning" that Hiebert and colleagues (2023, p. 116) identify as running through all parts of a research study. The visual representation (Figure 1), adapted from Hiebert et al.'s chain, conveys the point that there must be a clear flow between the initial phases of research and the contributions and implications discussed at the end. Methods are viewed as an enabling bridge—selecting the appropriate methods is needed to ensure a strong connection between significance and contributions. The authors warn that contributions are also constrained by the original research questions and hypotheses or revisions to these in the light of findings. A takeaway message of this chain of coherence is that if the significance of the study is established early, and if the methods are appropriate, the likelihood that the contributions will be important is increased.

Figure 1

The Chain of Coherence That Runs Through a Research Study (Adapted from Hiebert et al., 2023)



Contributions

Contributions are determined by the degree to which the research has moved the field forward, what we now understand better, and that can only be determined at the end of the study. The greater the perceived importance or value of the problem or issue investigated, the greater the perceived contribution of the research. An important characteristic of research contribution is that it is most often cumulative. We are regularly led to believe every study we conduct must include the Holy Grail as its contribution! Yet, one of the major criticisms made by reviewers of *JRME* is that authors' claims of contribution are often not justified by the data (Hiebert et al., 2023). The reality is that the merits of the contributions to a field of research by most researchers are judged on their contributions over a series of studies. Consider the contributions of prominent mathematics education researcher Professor Emeritus Peter Sullivan. A driving force behind his work throughout his career has been the identification of and desire to address barriers to learning mathematics particularly for students from disadvantaged backgrounds. Throughout his career, Sullivan has addressed this theme from multiple perspectives including good questioning (Sullivan & Clarke, 1991); open-ended tasks (Sullivan et al., 2001); enabling and extending prompts (Sullivan et al., 2006); lesson structures (Sullivan et al., 2015); challenging tasks (Sullivan et al., 2016); and sequences of challenging lessons (Sullivan et al., 2023). Examining even this narrow representation of his work reveals how Sullivan's underlying research agenda to identify and address barriers to learning mathematics provided direction to his research. More importantly, the linked work yielded a far greater contribution over time to advance our understanding of how to design tasks and lessons that increase the likelihood of challenging and engaging all students in learning mathematics than just one study could possibly achieve.

The critical point derived from the notion of cumulative contribution is that important advancements of knowledge in mathematics education rarely come from a single research study but from the linked contributions made by many. This is a major reason why researchers develop research agendas and base their studies on prior research conducted by themselves and by colleagues. Look at any chapter in *Research in mathematics education Australasia 2020–2023* (Mesiti et al., 2024) for the evidence of this cumulative contribution!

Implications

In the mathematics education research field, implications are most popularly presented in terms of suggestions that are *reasonably* derived from the findings for improving educational practices. Implications for educational policy, curricula development, theory, and research methods are also common. Additionally, implications expressed as recommendations for future research are usually expected in reports of research. Contrary to these popular views regarding the nature of implications, Hiebert et al. (2023) argue that most implications can be considered as research contributions. They propose that only conclusions about the efficacy of certain methods for generating a study's data align with their definition of research implications. Although I can understand this perspective given that there is often a fine line between contributions and implications, I prefer a targeted discussion of implications, especially for educational practice and future research. A thorough discussion of practical implications is valued by MERGA and is a reason why we offer the Beth Southwell Practical Implications Award. Unfortunately, some researchers encounter difficulties writing about implications of their studies. One common difficulty relates to the definition that implications should be *reasonably* derived from data. In their analysis of *JRME* reviewer comments, Cai et al. (2019b) report that nearly 30% of reviews included concerns about implication claims that were unsupported by the findings.

A second potential reason why some authors experience difficulties making a strong case for implications is linked to the positioning of implications in the concluding sections of research text. Word limitations along with an afterthought that implications should be included can result in the generation of meaningless sentences, such as:

> The results have *important implications* for theory and practice.
>
> Findings from this study *contribute significantly* to the development of recommendations for teacher professional learning.
>
> The study's findings will *impact* student learning and have *important implications* for pedagogy.

Regularly these types of sentences comprising sweeping statements appear with little or no details as to what the significance, contributions and implications are.

Inevitably there will be some variation in the way different researchers interpret the meaning of key terms used to argue the significance of their research. Nonetheless, it is still important that we have a clear understanding of what we mean when we use them in our work and that we can differentiate between them. Without this clarity or differentiation, we risk using these terms interchangeably, which can cause confusion for readers. The important point is that although we are aware of the necessity of claiming the significance, contribution, and implications of our work to argue its importance, without clarifying what these terms mean to ourselves, it is almost impossible to present a convincing case to others.

Developing Significant Research Questions

Earlier I stated that significance is mostly associated with the research questions we pose. This statement is in accordance with the National Research Council's (NRC, 2002) six guiding principles for scientific inquiry. The first of these principles is to "pose significant questions that can be investigated empirically" (p. 52). The NRC maintains that such questions in education pertain to "pressing problems of practice and policy" (p. 8) and that the formulation of significant questions can be more important than the solutions. This advice is still not very helpful for developing significant research questions because it is not always obvious what pressing problems of practice or policy exist in education. In fact, Evans et al. (2014) consider that using the term 'problem' may be too strong and suggest that "motivation for the study" (p. 63) might be more appropriate in some contexts. Either way, a major challenge for researchers is often how to develop significant research questions. Moreover, as reviewers of research manuscripts and proposals, how can we judge if a research question is significant? Or at least, has the potential to be significant?

Helpful advice for developing potentially significant research questions in mathematics education is provided by Cai et al. (2019a). They assert that research questions are more likely to be significant if they arise from teachers' problems of practice and should "aim to directly impact practice" (p. 115). They advocate increased interaction between teachers and researchers so that authentic problems of practice can guide research question development. Although Cai et al. focus on teachers' instructional problems as inspiration for research questions, it is important to also consider the problems of learners as a source. Perhaps the most significant questions in mathematics education are derived from our aspirations to improve students' learning of mathematics. This improvement should occur by addressing problems of instruction *and* problems of learning.

Once a question is selected, it will usually go through refinements to ensure it is unambiguous and meaningful. I use the Goldilocks principle as a guide for judging the meaningfulness of a research question. Namely, the question should not be too broad, not too narrow, but just right. Being 'just right' means that it will be focused, address an important problem or issue, and not require only a yes/no answer but seek to understand how and why.

Finally, simply because we consider a research question to be significant does not ensure that readers will also view it as such. It is unlikely anyone can judge the significance of a research proposal simply by reading the research questions. Presenting a strong case or 'selling' the significance of a research question starts early by establishing the research context. The research question(s) and the problem it addresses must be contextualised in terms of existing research, providing a justification as to how addressing the research question will deepen our understanding of an important phenomenon in mathematics education and extend our knowledge in the field. As so eloquently argued by Hiebert et al. (2023) and represented in Figure 1, contextualising the research question based on prior research is only the first stage in a chain of justification that should be coherently woven through each subsequent section of writing; thus, continuously strengthening the case for significance.

Conclusion

An aim of this paper was to raise questions and spark conversations about aspects of doing and reporting significant mathematics education research. These are conversations that we should regularly revisit not only with our research colleagues but also with potential end-users whether they be practitioners or policymakers. I hope that my comments have made a positive contribution to individual researchers and to the MERGA community by raising awareness of how critical it is for researchers to not only *do* significant research, but to think more deeply about how to communicate a coherent case for its importance via the significance, contributions, and implications. Not doing so, runs the risk that the quality of mathematics education research will be questioned. Refining our skills for arguing the case for the importance of research impacts us all—only together can we strengthen the ongoing credibility and quality of mathematics education research and have a truly significant impact on students' learning internationally.

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