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Going beyond validity criteria in mathematics education research: towards the generativity of a research study

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Context

This short essay is not a critique of Barallobres (2015) article, but is mainly aimed at a continuation on the ideas, as a complement, as an adjacent piece, to issues of validity of scientific research in mathematics education research. My central claim in this article is that research needs to be conceived on new grounds if a real call for change is to be taken up. Thus, beyond any validity criteria one can think of, I argue in this short essay that a fundamental aspect of a research study is its degree of generativity, that is, the ideas and the distinctions that it generates.

Preliminary remarks

Before I develop my argumentation, a quick word on my positioning as a researcher is in order. My orientation as a researcher is inspired by Maturana's (e.g. 1987, 1988) theory of the observer, where the observer is central to any *account* of any given phenomenon, for "everything said is said by an observer to another observer that could be himself or herself" (1988, p. 27). As I explain in Maheux and Proulx (2015) and in Proulx (2014a), even if as researchers we generally do not believe that "the phenomenon being observed" is a fact independent of the observer and that it can be decontextualized from the observational act, we often take this position implicitly by how we report our findings. As Barwell (2009) explains, even if we agree that we cannot account for what *really happens*, research is still being reported (and maybe even conceived) as if this were the case. Through being attentive to Barwell's critique, the issue of "accurate accounts" of data or findings is meaningless if the researcher holds an epistemological position where one does not *describe* what is being observed, but *constructs* (in Barwell's words) one's own account of one's own perceptions. The adequacy of research findings is not linked to some allegedly objective referent, but to the eye of the observer who *assesses* it on the basis of his/her own understanding of what research is about. It follows from this perspective that analyzing data and reporting on findings rest no longer in their truth or validity, but in what they offer to us and to others. This orientation transforms assertions about what are seen as research findings and what can be learned from them.

Introducing the issues

In their seminal work, Guba and Lincoln (e.g., Guba & Lincoln, 1982, 1985; Lincoln, 1995) argue for the insufficiency or inadequacy of validity criteria used in a rationalist/objectivist paradigm for assessing scientific research lodged in a naturalistic inquiry paradigm. They make the proposition to replace or rephrase these previous criteria with ones that would be more suited assessing properly the rigour and validity of naturalistic inquiry. Following this thread, and referring to the work of Guba and Lincoln, Kemp (2012) has recently raise similar issues of discontent with what she calls "traditional" scientific criteria coming from a positivist/objectivist epistemology (like applicability, external and internal validity, objectivity and

Chroniques
fondements et epistemologie de l'activite mathematique
foundations and epistemology of mathematical activity

reliability) for judging educational research under a constructivist paradigm. She suggests replacing each of them with a new set of criteria that represents better what is for her a constructivist epistemology. In a nutshell, here is how she explains them:

> The criterion of transferability relates to the traditional concept of external validity and is concerned with the applicability of the data and findings to different settings. [...] decisions about the extent to which the findings are transferable to other contexts may be more easily made by the reader of the research text. (p. 120)
>
> The criterion of "dependability" draws from the traditional notion of reliability, or the consistency of the study. "Reliability" refers to the idea that if the study were replicated under the same conditions the results would be the same. This meaning of reliability is more problematic in studies that are socially, culturally, and historically situated. "Dependability" relies on an adaptation of the notion of reliability and refers to the potential replicable nature of the study. (p. 121)
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> The criterion of "confirmability" is a replacement for the traditionally "neutral," or "objective," stance expected by researchers, in keeping with a realist philosophy where a researcher aims to understand a "real world" separate from the values and biases of the researcher. For an educational researcher, a transparent explication of the contextual features of the research and the "positionality" of the researcher is referred as an alternative approach [...] (p. 121)
>
> The criterion of "credibility" is derived from internal validity, the latter premised on a "correspondence" theory of truth and the ability of the data to match an external reality. "Member checks" [...] are often considered essential for establishing credibility. [...] One mechanism is that credibility could be contingent on ensuring that the researcher's own expressions of understandings and meanings are clearly distinguished from the expression of the participants in the study, an essential part of the quest to construct a coherent account of the research. (pp. 121-122)

One thing that is striking when reading these is that, ironically, Kemp's new criteria for judging the validity of an inquiry are referring directly to the traditional criteria. Thus, her new criteria are also grounded themselves in these traditional views linked to a positivist/realist epistemology that she aims to question! This leaves one with the sentiment that these traditional criteria are in fact "the" ones to satisfy, that they are the benchmark that scientists need to comply with, and that they represent the reference, the higher end of what scientific rigour is all about. However, paraphrasing Varela (1996; Varela & Poerksen, 2004), one could say that changing paradigms lead a number of previous issues to become irrelevant and unanswerable, because they are grounded in another view of the world that is being questioned by the new one.

As a way of example, a rapid critique of the new set of criteria offered by Kemp could be as follows, following the perspective of the observer above-cited. About *transferability*: because research is lodged in different contexts, which are at the core of the results and knowledge developed (see e.g., Lave, 1988), transfer is impossible and only inspiration or extensions for future research, to paraphrase Mason (2009), can be seen to be possible. About *dependability*: this assumes that any other researcher, given the same "tools", can do the same job and find the same results, whereas research results are researcher dependant and assuredly from one researcher to another the results and meaning making practices vary. About *confirmability*: validity is an observer dependant issue and explications are all but transparent because they are coloured by the researcher who brings his/her noise within it. About *credibility*: why would a researcher make the exercise of separating

Chroniques
fondements et epistemologie de l'activite mathematique
foundations and epistemology of mathematical activity

his/her voice from the one of the participant if in any case the "data" is never pure and always noised by the researcher him/herself when developing the research (see e.g., Geertz, 1973, Chap. 1)?

Kuhn's (1962) and Bakker's (1995) work on paradigms also raises these same issues about ideas becoming irrelevant from an old paradigm to the next. As an example, traditional notions, like the one of "truth", have been amply questioned and are often seen as irrelevant in post-modern inspired epistemologies. Changing epistemologies brings forth criteria that offer something else, and not only an improved version of the ones the new epistemology attempts to question. This is what Schmidt explained to Poerksen in a recent interview (in Schmidt & Poerksen 2004, p. 149).

> *Poerksen*: The approximation of an absolute reality can, if I follow you, no longer be a criterion for the evaluation of research results. What then?
>
> *Schmidt*: It is the quality of the procedure that supplies the criterion. It is the controllable care in the production and interpretation of facts. Facts are only as good as the methods of their fabrication, and as significant as the procedure of their interpretation. And we must remember that the hardest empirical results turn soft at the moment of interpretation, at the latest: their contingency then appears ineluctable because, for any collection of facts, I can – as is well known from the interpretation of statistical data – generate differing interpretative stories.

Thus, new paradigms bring forth new issues, which does not mean that the previous issues from previous paradigms are trivialized, settled, or answered: they are simply stepped over, often in disinterest, out of irrelevance for the new paradigm. Dewey raised a similar concern about intellectual progress:

> 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 (Dewey, 1910, pp. 18-19)

Taking this to heart, the same can be thought about for scientific criteria originating from positivist/objectivist epistemology: those criteria are not to be answered, replaced, improved, rephrased or rethought by parallel ones in the new paradigm; they need to get over with, to be set aside in order for new criteria to be established (which could also mean to question the very notion of having criteria in the first place, see e.g., Schwandt, 1996). The above exercise on Kemp's (2012) work was an example of such. And, letting go of the very notion of criteria, in the following I discuss what I see as a central element of a research study, that is, its generative character.

Generativity of research studies

In my sense, the most important aspect of a research study is its degree of generativity, that is, the ideas and the distinctions it generates. Let me state this bluntly: *the goal of research is to generate new ideas, ways of thinking and distinctions, and it is not to offer answers*. This sentence may trigger provocation, but it is for me what a non-objectivist perspective, like the one of the observer, offers for understanding the research process and its results. I intend, in the following, to explicate this understanding.

Through questioning the concept of "truth" and "existence", and offering instead concepts like "viability", "relativity", "situativeness", post-modern inspired discourses raise significant issues to be

Chroniques
fondements et epistemologie de l'activite mathematique
foundations and epistemology of mathematical activity

pondered upon concerning the goal of research studies in mathematics education research (see my paper in Bednarz & Proulx, 2011, concerning the impact of various concepts like "viability" on research studies in mathematics education research). To some extent, the intention is not anymore to offer what *is* or how it *is*, but it is to offer views into what *can happen*. Research is not an endeavour for finding answers and solutions, or for explicating what *is* and how it *is*: it is about generating ideas rather than proving things. Issues of what *is* belong to an objectivist viewpoint paradigm, in which the intention becomes to gather sufficiently enough data for making such claims, for offering generalisations.

This idea of generalisation of results, or of transferability that Kemp (2012) discusses in her article, is a trap. It is an old trap grounded in vestigial objectivist's views of research. It is a trap that non-objectivist orientations, or post-modern ones some would say, avoid or simply get over with to use Dewey's terms. Research studies do not aim to generalize, but rather to generate ideas.

> Instead of evaluating a study only in terms of its generalizability, which is connected to external validity, we may consider its generative capacity as an important criterion. Generativity can be taken as the extent to which a study originates new research objects for study and alternative research methodologies as well as produces new outcomes. (Valero & Vithal 1998, p. 158)

This view of generativity is maybe not easy to grasp, as we are mostly all used to think of research studies as endeavours that offer what is and the state of ideas, that dictates the good from the bad, that separates the "what works" from the what "does not work", etc. It is in fact a view of research that is linear, that aims at closing all the doors, one after the other, in order to answer all problems that exist, in the quest for the ultimate truth or solutions (for fixing problems).

> Efficiency, facts and replicable experiments are all artefacts of the machine metaphor spawned by the industrial revolution. They are no longer appropriate in a world which recognizes feelings and perceptions as well as behaviour. (Mason 1984, p. 25)

Through believing in generalisation or transferability, one often thinks that one's research will be seen as more valid, more applicable and legitimate, more acceptable and "classifiable". But this appears hardly to be (mathematics education) research studies' goal. Research aims at generating. It aims at generating more research, generating questions, new ideas, new studies, new understandings, new distinctions, etc., that push meaning making processes about the phenomena under study. As I argue in Hardy, Maheux & Proulx (2014) and Proulx (2015), research aims to provoke thinking, to generate and transform ways of making sense, to generate new distinctions, to push and explore ideas. Research provokes thought, it keeps it alive, in movement, it generates it. It is not fixed once-and-for-all, classified. The results obtained in a study are there to generate, they are not there to be maintained and fixed. As Châtelet (1987) explains, fixing is killing. When something is fixed, it is finished, I can go to bed, it is done, there is nothing more to do with it, all is said and done. But, somehow, nothing is said. To take form, to live, ideas have to be alive and dynamic; research is keeping ideas alive. The world that surrounds us is in constant motion and thus all questions and problems evolve and change; they get transformed. Research studies contribute to help understand (and even create) these new problems, these new dynamics and new questions and then to generate some anew. Research studies in mathematics education generate new distinctions, new ideas, that lead to new or/and different perceptions of mathematics education phenomena under study, which helps meaning making processes, in order to contribute to their evolution/transformation; which in return provokes or makes emerge new research studies, new questions, new phenomena to consider, and so on. It is what Varela (1984) calls a creative circle.

However, this creative circle is not the same as the old "circular" evolution present in objectivist thinking that the more we know, the more we know what we don't know, and how to know it. This circularity is transformational, its cycles are recursive: the phenomenon under study is transformed through research, through the distinctions produced about it. It is not cumulative, it changes how we think of the phenomenon and this changes the phenomenon in return, as Piaget (1979) explains: "l'objet se transforme pendant que la connaissance s'en rapproche" (p. 411).

This is why research leads to consider what can happen, what is plausible some would say, and not what is "exact" and fixed "real" state of affairs, or what *is* exactly. Research that is generative triggers reflection, pushes on new ideas, new ways of thinking and of understanding. In a word, as mentioned, it transforms. It transforms research of others, it transforms the researcher itself, the practitioner, the teacher educator, the domain of study, the ideas, and so forth. And, it is not only the studies' results that are generative: all from a research study, all parts can contribute and generate ideas for transforming understandings. From the most simple and technical issues to the more complex ones: the way the research question is addressed and formulated, the entry on the theoretical issues, the methodology developed, the way the data is gathered and analysed, and so forth. All in a research study has potential to generate new understandings on the phenomena under study. Mason offered a similar view:

> If it is either impossible or not necessary to be able to replicate the conditions of a study, what is it that we are gaining by reporting on our studies? My radical response to such a question is that what matters most is *educating awareness* by alerting me to something worth noticing because it then opens the way to choosing to respond rather than react with a more creative action than would otherwise be the case. I don't need all sorts of detailed data, because the more precise and fine-grained the detail, the less likely I am to pay attention to the over all phenomenon being instantiated, and so the less likely I am to recognise it again in the future and so choose to act differently. (Mason 2009, p.12)

This entry on a research study being generative impacts on the researcher's attitude when dealing with his/her research. One is not focused on obtaining a representative data sample, on making sure that what is offered helps for other cases or that it resonates in other contexts, etc. The intention becomes one of grasping new ideas, new occasions for understanding, new entries and avenues that arise, and even taking advantage of unpredicted events that happen. The intention becomes one of generating new ideas through and with the research, for advancing on new grounds, to develop new alternatives, and so forth, to make people think.

Final remarks

In his article, Barallobres (2015) questions notions of scientificity in mathematics education research. To some extent, what I offer here in this adjacent piece, this short essay, does the same and offers a different way of conceiving studies in mathematics education research. In a nutshell, maintaining criteria for assessing the validity of a research study reflects a view of research, of the world, that needs to be set aside because it maintains us in a similar rationalist paradigm. To make a step beyond, I contend that the central role of research studies in mathematics education is to be generative, not as a criterion for judging the quality of a research study (since generativity is deeply observer-specific), but as a way to think about what research in mathematics education is and aims to be/do.

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