

British Society for Research into Learning Mathematics Working Group

Induction for Secondary Mathematics ITE Tutors

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STARTING AS A RESEARCHER IN MATHEMATICS EDUCATION

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This paper reviews some of the issues related to beginning as a researcher in mathematics education. It looks at what helps, and at what might be some of the pitfalls. One key issue that emerges is the role of reflexivity in developing as a researcher - reflecting on how your own background, values, perceptions and behaviour can influence the research you carry out. Advice and illustrative stories are included which may be useful to those new to mathematics education research, especially those newly appointed as tutors in initial teacher education.

INTRODUCTION

Beginning as a researcher in mathematics education starts, according to Simpson (2000, p7), with questions:

Every time a teacher makes a decision to use a new textbook for some reason, they are getting involved in the beginnings of educational research. Once you start questioning why you are doing something, you can begin to articulate what you believe the problems are. Then you can investigate whether there are some possible and practicable solutions.

Beginning with questions is a start. Yet how does a beginner get to articulate their questions, and how can the questions be investigated? Of course, there is a multitude of books available on research methods, both for the social sciences in general and for education researchers in particular, some of which are suitable for the beginning researcher. The aim of this paper is not to distillate these various sources of advice much beyond the merest outline; as such a distillation cannot be achieved within the space available in this paper. Rather the intention is to look at some of the reflections of experienced researchers who, in looking back at what motivated them as a researcher, offer some thoughts that might help a beginning researcher. The paper begins by first considering what research in mathematics education might be, and, indeed, what it might not be.

RESEARCH IN MATHEMATICS EDUCATION: WHAT IS IT?

Researching mathematics education, for a beginner, may seem like something that other people do. So a first obstacle might be getting to grips with what such research is (and, importantly, what it is not). Selden (2002, p2) begins to approach the

question by considering what research in mathematics education *is not*. She ventures that research in mathematics education is *not*:

- curriculum development;
- descriptions of interesting courses one has developed (although, of course, mathematics education research may involve the description of some teaching);
- writing a new textbook, developing an online course, or implementing a new way of teaching;
- the development of novel assessment procedures;
- (local) evaluation studies that seek to answer questions like: Does our new algebra course really work?
- interesting mathematics for use with one's students *if* one only had the time.

Selden argues that while these are all useful, and even scholarly, activities, “they are not mathematics education research”.

Rather, Selden maintains, research in mathematics education is “*disciplined inquiry into the learning and teaching of mathematics*” (echoing Cronbach and Suppes, 1969), often involving close observations of students actively involved in mathematical tasks. It is conducted using a variety of methodologies and it is *domain specific* – it is about mathematics. Likewise, Niss (1999, p5) describes research in mathematics education as:

..the scientific and scholarly field of research and development which aims at *identifying, characterising, and understanding* phenomena and processes actually or potentially involved in the *teaching and learning of mathematics at any educational level*.

For mathematics education research, Selden (2002, p3) argues, one needs a solid understanding of the mathematics at, and beyond, the level at which the students being observed are working. In addition, Selden goes on, mathematics education researchers, just like mathematicians, put a great deal of thought into selecting their research questions. These questions, Selden suggests must be “new, nontrivial, non-obvious, and the potential answer(s) should be interesting”. An observation by Schoenfeld (1999a) may be useful here:

The hard part of being a mathematician is not solving problems; it's finding one that you can solve, and whose solution the mathematical community will deem sufficiently important to consider an advance. In any real research (in particular, education research), the bottleneck issue is that of problem identification – being able to focus on problems that are difficult and meaningful but on which progress can be made.

Thus questions may come first, but selecting which ones to address is crucial. After such questions have been carefully formulated, it becomes possible to move onto selecting appropriate data collection and analysis methods (at which point, texts on research methods become important). Yet researchers rarely write about how they

decide on which questions to address, whereas it is possible to learn more about their choice of data collection and analysis methods through the papers that do get published. The next section reveals some hints about what experienced researchers have said when reflecting on what motivated them to begin as a researcher.

REFLECTING ON STARTING AS A RESEARCHER

Schoenfeld (1999b, p4) observes:

While it may be a truism it is nonetheless true that much of what we do, individually and collectively, is shaped by our personal histories.

This observation is echoed in the writings of those, not necessarily mathematics education researchers but social science researchers more generally, who have published reflections on how they began as a researcher. Let's start with the reflections of Peter Townsend, until recently Professor of International Social Policy at the London School of Economics. Townsend (2004) writes:

Nowadays, I reflect a lot on the question of being an only child and what that means. It led, in part, to my enormous interest in family relations and extended family life, and the structure of families. Because I'm sure that single children have very different needs, and certainly very different experiences, and certainly a great deal of immaturity by virtue of not having siblings . . .

and, further, that:

I had, at the time [I was an undergraduate], and I have now, a kind of love/hate relationship to Cambridge . . . I suppose I learned a bit about sociological determinism there, and the way in which the construction of societies seeped into people's thoughts, and bones, even, and accounted for some of these extraordinary variations across the world. And that was a very exciting discovery, because it made me come back to thinking about how wartime Britain had created a different society, for example, or how post-war Britain was evolving. And that set a train of thought which has stayed with me forever

Here Townsend is demonstrating how even very early experiences in life, and reflections on them, can shape the research that one does. Stephen Billett (writing in Bloomer, Hodkinson & Billett, 2004) notes:

the interest that guides the direction of my inquiry into learning in workplaces and participatory work practices can be located in events in my life history.

Similarly, Martin Bloomer (writing in Bloomer, Hodkinson & Billett, 2004) realises:

it is apparent that my research career was shaped partly through formal instruction, with certain individuals assuming particular importance in that. But that formal instruction was responsible for only a part of the researcher I was to become. There have been many transformations in my life and the impact of all the teaching I ever received was reciprocally linked with these: what I learned contributed to the transformations but the transformations whose origins often seemed to have lain elsewhere gave shape to what I

learned. I have drawn upon a number of my life ‘strands’ here: my orientations to knowledge and learning, my music and my politics. I could have mentioned others, more profound, including the mysterious processes through which I became a husband, father and teacher. The point is that my becoming a researcher has been inextricably bound up in all other aspects of my becoming a person. It has been a learning career.

Overall, Bloomer, Hodgkinson & Billett (2004) observe that:

Our respective stories demonstrate that we each approached our research careers from complex positions, rooted in our past lives—particularly our early lives. Throughout our lives, we developed dispositions—to life, to education and, eventually, to research, as part of our personal habitus. What is striking in all [our] three stories is the way in which our wider social background, our early experiences of education, and deep personal convictions about injustice influence our thinking to this day. Though we cannot fully bottom it out, the early latching on to a particular theoretical or disciplinary perspective also contributed to who we are, and framed our developing research interests.

This “early latching” is present in the reflections of the influential theorist Zoltan Dienes (responsible for Dienes blocks, amongst many other things). He writes (Dienes, 1999, p237-8):

I knew that most people had difficulty with mathematics. For the most part it was an unpopular subject at school. And how come I did not encounter such great difficulties? Difficulties there were, to be sure, but they formed part of the challenge of doing mathematics. People enjoyed other challenges, why not mathematics?

As the years rolled by, I kept wondering about the possibilities. I had an idea that some people had perhaps a closed idea of mathematics while other preferred to leave things open..... I was so fascinated by the problem that I wrote a paper on it.....

This is how it began for Dienes, recognising an issue and keeping on wondering about the possibilities. Then beginning to investigate and write. More examples of reflexivity in the research process, with researchers acknowledging the impact of their own life history, experiences, beliefs and culture on the processes and outcomes of their research, can be found in Etherington (2004) and McCotter (2001).

ON THE GETTING (AND REFINING) OF RESEARCH IDEA

The illustrative extracts in the previous section provide some insight into how people have made sense of their own research journey. To finish this paper we look at some practical ways in which initial ideas of some area research can be developed. As we said above, this is a process of articulating questions, and thinking about how such questions can be investigated. One way to do this is to start to become an active reader and listener. This might entail making the transition from a passive approach to a more active and critical style so that whenever articles are read, or a research talk is heard, the following sorts of questions are asked:

- From where did the author (or authors) seem to draw their ideas?

- What exactly was accomplished by the piece of work?
- How does it seem to relate to other work in the field?
- What would be the reasonable next step to build upon the work?
- What ideas from related fields might be brought to bear upon the subject?

One technique that can be helpful is to keep a written log of research items read and heard. Such a log can be reviewed periodically to see whether some of the ideas are beginning to fit together.

Through reading and attending research talks, you are exposing yourself to research. Through this exposure it becomes possible to set aside some time, periodically, to try to generate research ideas. Some possible catalysts for this are:

- Making regular trips to a suitable library to read at least the abstracts from some leading journals (like *ESM* or *JRME*). Maybe choose one or two articles to read in depth and critique.
- Attending suitable research events in order to listen and critique. *BSRLM* day conferences are designed to support the beginning researcher with various ways of contributing, from more informal working groups to short (and longer) presentations.

Once a topic has been identified that looks feasible, it is advisable to become aware of the significant literature in the area. This involves keeping reading and listening, and keeping distinct in your mind what is different between your own perspective and the work of others.

One pitfall to try to avoid is spending almost all of your time in literature review and seminars. It is easy to become convinced that by doing this you are working hard and accomplishing something. In fact it is possible that little will come of this unless you are an active reader and listener and unless you assign yourself time to develop your own ideas too. It is nigh on impossible to "finish a literature review and then start research." New literature is always appearing, and as your expertise and knowledge increases, you will continually see new connections and related areas that entice you to further work. Active listening and reading has to be viewed as "continuing education" that will involve you for the rest of your research career.

Another pitfall is thinking that all the reading and listening must be finished before you can begin to research. Try making a list of open problems and possible projects that are of interest to you, and discuss these with colleagues and people you meet at research events. Even after you have decided on your initial focus, it is important to continue a routine of reading new journals and technical reports and attending seminars. All of these sources can contribute to the development of your idea.

CONCLUDING COMMENTS

Starting as a researcher can be a daunting task. This paper is intended to illustrate that there is advice and support available. Children deserve good teaching. Research is part of this, and, as Pope, Haggarty and Jones (2003) argue, there are many opportunities to get involved and make a contribution.

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