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Editorial

Troubling Methods in Qualitative Inquiry and Beyond

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Abstract

This present paper troubles and literally 'shakes' the idea of methods as the founding ground of qualitative inquiry. It does so by addressing the real-time messiness of research and the retrospective character of research reports. While the paper is not as such opposed to methods, it does suggest that many actual research practices do not follow defined and regular plans as the terminology of methods inclines. However, rather than seeing the messiness as a bias to be eliminated, a more constructive approach is suggested. With the intention of inviting more creative and thought-provoking research within qualitative inquiry, three specific 'messy' research strategies are suggested in the paper. These are: 1) Searching for associations between actors, of both human and non-human kinds, 2) following the traces of many kinds of actors and 3) doing a theoretical re-working of materials. The overall suggestion is that these open-ended and flexible strategies allow for an innovative approach to the development of a qualitative psychology while also serving to trouble (at least for a moment) the current popularity of methods in research.

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"I cannot explain how deconstruction happens, but it does if one has read enough and puts it to work"
(St. Pierre, 2011, p. 620)

Introduction

The aim of the present paper is to trouble and literally 'shake' the idea of methods as the founding ground of qualitative inquiry. In these days, qualitative inquiry usually involves rather fixed and taken for granted concepts like 'data', 'coding', 'analysis' and 'methods'. In courses on qualitative work, we teach students what methods to use for collecting data, how to code data and how to do an analysis. However, each of these concepts can or must sometimes be troubled, questioned, critiqued or even replaced with a more fitting terminology if this is what is needed to keep the field developing. In the present context, I intend to do so with the concept of methods, generally defined as: "a way of doing anything, esp. according to a defined and regular plan; a mode of procedure in any activity, business, etc." (*The Oxford English Dictionary*, 2008, p. 963).

My trouble is that I have never myself conducted research according to a defined and regular plan – or rather my plans and the realized research are seldom identical constructs and any description of methods in my writing is at best retrospective and a kind of subsequent rationalization of what was done. Consequently, even if I do have a research-plan (as I carry a map or a GPS when travelling); unexpected things always happen, newly constructed roads turn up and research paths which could not be foreseen surface. And why is this so? Well, as Bent Flyvbjerg notes, following his qualitative case-based research on mega-projects (such as building a new infrastructure in

modern day cities): “*Predictive theories and universals cannot be found in the study of human affairs. Concrete, context-dependent knowledge is, therefore, more valuable than the vain search for predictive theories and universals*” (Flyvbjerg, 2006, p. 224). Likewise: predictive methods cannot be found in researchers’ studies of human and social affairs which are what most qualitative researchers are up to. The rules of research, often formulated in many research-guide books, are good to know, especially for the novice research, but they are not identical with the concrete, realized research. The latter requires experience-based researcher skills (observing, describing, and listening, reading, writing) which are accumulated and embodied over the years. The above are the main reasons why my research reports are always a backward reading of how my empirical material came into being rather than a (blind) result of following the prescribed rules of a method (Ingold & Hallam, 2007). In an attempt to take this seriously, this presentation aims at exploring possible alternative approaches to do qualitative research which are, to my knowledge, not very frequently described in qualitative methods textbooks. Accordingly, I have found three kinds of research practices:

1. Searching for associations between actors, of both human and non-human kinds
2. Following the traces of many kinds of actors
3. Doing a theoretical re-working of materials.

If the above may not be themes of general interest, they have at least come to represent what I tend to do when conducting qualitative research and I hope they can serve as inspiration for other researchers alike. However, before explaining these concepts in more detail, we will have to study why methods became so popular in the first place.

Why Did Research Methods Become so Popular? A Short Story

As a university teacher in psychology for the last 15 years, I have noticed that we tend to teach students research methods more intensively than we did just a few years ago. As part of our local psychology education at Aalborg University, in Denmark, students are obliged to conduct two major research studies already while at the bachelor level. That is: one major qualitative project and one major quantitative project, each lasting one semester. They are taught how to do so in separate methods courses, and they carry out their empirical projects in groups, with a supervisor helping them. Only five or six years ago, they also wrote projects, but they did so without having to conduct empirical work and without being examined as intensively in research methods as they are these days. There are a number of reasons, I believe, why this increased focus on teaching research methods is occurring.

The Performative Role of Research Methods

One consequence of the local prioritization of research methods in teaching is that there is now less ‘innocence’ concerning research methods among students than just a few years ago. They learn to define phenomenology, grounded theory and analysis already from the second semester, and empirical work is valued to a higher extent than ever before, which is a relief for those who believe in empirical work as that which ensures progress of the discipline. On the other hand, theoretical work is facing a hard time, and while students’ projects formerly tended to concentrate on the particular subject matter of, say, social and/or personality psychology, some of them are now more obsessed with the methods than with the actual subject of their research. Furthermore, methods are increasingly seen as more or less content-independent tools with which to handle almost everything. Consequently, many of our students conduct an IPA (Interpretative Phenomenological Analysis) of almost everything ranging from childhood suffering to elderly joy and happiness. They do so because we teach them to do it, and IPA is indeed

very instructive in its delimitation of the possible concrete steps to follow in a phenomenological-interpretative type of analysis. Accordingly, IPA is really popular among our students as it represents an easily applicable and also very useful analytical approach, but the problem might be that it is over-inclusive and seems to be perceived by the students (and also the teachers!) as immediately useful almost everywhere regardless of the content of the project, even if this is actually not always the case. At least, this is what we discuss at the lunch-table in the teacher's room, without really having found ways to tackle this issue other than encouraging students to also do something else.

One may speculate why research methods have taken such a superordinate position. At an analytical level, Lyotard's (1984) analysis of the performative role of knowledge as a circulating and easily exchangeable entity in a post-modern world would be one way to look for an explanation. "Be performative or die" is the dictum of the postmodern condition, and methods are surely playing a performative role when they show their applicability almost everywhere, independent of particular content. According to the analysis of Lyotard, knowledge is increasingly seen as a circulating entity and not as embodied, connected to expertise, and context-bounded. That is: Instead of spending years as an apprentice among experienced researchers, scholars today often learn the tools of research from research methods courses, not integrated into particular fields, as researchers did years ago. The former practice carried the risk of being misguided or suffered from the exploitation of 'master' figure, but also offered students the change to learn from experts in the field rather than from the cookbook of science as embodied by many research books (Kvale, 1999). That is, along with the apparent democratization of research practices, with methods books and courses offering a relatively easy, fast-food approach to the world of science, there is also a real risk of compartmentalization of research into easy digestible bit(e)s, not lasting long. Nevertheless, being unsure about the value of 'truth' in knowledge construction in the postmodern condition, research methods seem to be one of the more secure anchors when faced with the postmodern flood. Meanwhile, the content, or the craft of research, tends to slip away, critics would claim.

Legitimization Crisis?

Another way to understand the popularity of research methods today, not just generally, but also in relation to qualitative inquiry, is related to an overall legitimization crisis within research and science. In the moment of performance, when knowledge does not seem to be an end in itself, deeply ingrained in realization of the IDEA or the empowerment of people, control slips away from all, including the researcher. Accordingly, the whole idea of a research method, by which data can be handled with reference to specific guidelines and procedures, represents an attempt of systematization, formalization and maybe a kind of semi-control of the whole research process which can be a relief for a discipline trying to legitimize itself as a science and still recognizing the value of qualitative work. For example, Giorgi's (1975) description of the phenomenological approach to the analysis of interview data did play a major role for psychologists striving to turn psychology into a human science. It represented a systematics with which to make sense of often complex qualitative data. The initial formulation of Grounded Theory by Glaser & Strauss (1967) did a similar job in the social sciences. Furthermore, the growing number of books concerning qualitative methods is also an indication of the apparent need for reference-points and typologies of data-handling in order to make research publishable (or whatever the goal of the researcher might be). On the other hand, this practice risks locking the researcher into using only ascribed methods while more creative thinking, digressions and the simple following of the object of the research at the expense of procedure are not happening and/or are seen as something to hide. The reader is thus made to believe that data were analyzed using the prescribed and familiar steps.

According to Valsiner (2012), one consequence of the streamlining of methodology, taking the discipline of psychology as an example, is that psychology has been in a conceptual limbo over the last century:

“As an aspiring science, it has found its methodology to resemble that of ‘the real sciences’, in which theories are accepted as finished and immutable conglomerates of thoughts. Thus, much of psychological discourse has been devoted to issues of the following key thinkers of the past – Jean Piaget, Lev Vygotsky, B. F. Skinner, to mention a few, rather than transcending their legacies in new directions. Following in science indicates an inferiority complex” (Valsiner, 2012, vii).

This critique is interesting in light of the fact that psychology has been an extremely productive discipline with new journals, peer-reviewed articles and books coming up in a fast accelerant tempo, but without remarkable breakthroughs in the understanding of the psyche. And the streamlining tendency is not only a phenomenon within psychology. Considering the popularity of methods books, it might be the time to look at them in a more critical vein. As stated by Elizabeth St. Pierre:

“My critique is not that qualitative inquiry is unscientific, my critique is that, to a great extent, it has been so disciplined, so normalized, so centered – especially because of recent assaults by SBR – that it has become conventional, reductionist, hegemonic, and sometimes oppressive and has lost its radical possibilities to produce knowledge differently” (St. Pierre, 2011, p. 613).

According to St. Pierre, the many textbooks advising researchers to organize their empirical material in categories like research design, data, data collection, data analysis, interviews, observations and representations convince researchers that these are suitable for organizing and getting a hold on the material. The failure is that this is maybe not always true and that new inventions and the discipline of science in itself risks being under threat because these categories may work restrictively at the expense of the invention of new categories with which to handle empirical material more in line with the material at hand. St. Pierre’s inspiration comes from Derrida’s deconstructionism, meaning “*the overturning and displacement of a structure so that something(s) different can be thought/done*”. Without seeing myself as a deconstructivist per se, I will, in the following, trouble the concept of methods when this implies the following of a linear and regular plan which can, beforehand, determine the ways of the researcher in the empirical field. I think something else can and is actually done within qualitative inquiry. I do so by introducing three different concepts I have found useful for describing my own empirical work and which can maybe not overturn the popularity of methods, but at least trouble our conceptions of what qualitative inquiry is in the 21st century.

Qualitative Inquiry as Searching for Associations

One of the claims and points of critique voiced by St. Pierre in the above is that qualitative inquiry is presently locked in the image of the humanistic ‘I’ taking control over the research process, with the appropriate coding-tools (methods) in hand. Is qualitative inquiry a matter of the ‘I’ of the researcher conducting research according to a regular and linear plan, asks St. Pierre? My claim in the following is that this is seldom the case, and that the role of the qualitative researcher is much more one of searching for associations between humans and between humans and things revealing themselves as part of the inquiry. Let me develop this point a bit further.

One of the assumptions in St. Pierre’s text cited above is that deconstruction concerns the challenging of taken for granted concepts. In qualitative research, these concern for example many concepts based on the basic assumption of a humanistic ‘I’, e.g. the participant, the self, voice, lens, subjectivity, objectivity, dialogue, reflexivity

or identity. Working with the idea of entanglement inspired from deconstructionism and quantum physics, St. Pierre believes that everything is entangled with everything else and with everyone and that our individuality is, rather than a stable and defined unity, more akin to a chain of events and more about becoming than about a defined beginning and end. Accordingly, it is indeed hard to determine exactly where the 'I' of the researcher begins and ends. Am I for example the same 'I' after having conducted a piece of empirical research? Was the piece of research done according to the plan of the 'I' of the researcher or were there many other I's (eyes) present? The changeability of the field and the entanglement of everything with everything else make these distinctions hard to find out there.

Taking entanglement as the ontological point of departure, it does indeed become problematic to assume that we can then still stand on the shoulders of the 'I' invented by humanistic psychology when we do qualitative inquiry. The 'I' as a concept or category in the world is for St. Pierre closely coupled with the ideal of the transparent researcher who is able to make distinctions between the I and the world, the subjective versus the objective, the researcher versus the researched, but these distinctions do become blurred when the field is moving you, the researcher, in unexpected directions and when the groupings of participants in the field encountered yesterday turn into something else the next day. Even if it is hard to write oneself out of the 'I' or the above distinctions, a more moveable, entanglement approach would be preferable in these cases. It would furthermore help accounting for the experience mentioned in the introduction of the present text, namely that the actual research practice is so moveable and dependent on the events in the fields studied that it can be hard to determine beforehand what is going to happen and even harder to explain what guided the concrete steps along the way. The field can literally move the researcher in unexpected directions.

What I (sic) suggest instead is that qualitative inquiry is a matter of searching for associations between humans (the many participants and also the researcher) and between humans and things in the field, and that it is the change in associations between these entities that shows us what the field is made of, what happens and what changes are interesting. Rather than beginning with pre-determined concepts and looking for assembles out there, 'in the real world', we do, co-construct, associate and make empirical material as we go along. For example, within a field study approach, it is very often the reactions among participants towards the researcher and vice versa that show what the values, the norms and the habits of the field are, e.g. how they use to welcome strangers, what they emphasize, leave out or don't focus upon. And as the participants guide the researcher around, he or she learns what to look for, where to go and what there is to know. And as the traveling goes along, it becomes evident how abstract most theoretical constructs can be, and how they can actually be redefined along the way as associations are found and (re)changed. One example of this can be found in my descriptions of how I had to change my conception of learning confronted with apprentices in vocational education who objected to the researcher (me) trying to describe them during research interviews as learning subjects –subjects of learning (Tanggaard, 2007). Instead they wanted to be seen as becoming professional mechanics and they believed that the term learning and its associated educational discourse made it difficult for them to describe themselves as craftsmen and that it furthermore, at least metaphorically, keeping them on the school bench, something they did not want. Indeed, in research interviews, the interview itself is the site of empirical production, a space of co-production and a social practice in which the field is spoken and re-spoken – to and about – in new ways (see also Tanggaard, 2009; Tanggaard & Szulevicz, in press).

Finding a way to describe this co-production approach to research, which radically critiques the idea of the researcher as the sole I or knowledge-producer in research, I have not only consulted St. Pierre, but also the ANT

approach of Latour. In the words of Latour: “*For the sociologists of associations, any study of any group by any social scientist is part and parcel of what makes the group exist, last, decay, or disappear*” (Latour, 2005, p. 33). Accordingly, it is no longer enough to limit actors to the role of informers offering cases of well-known types. We have to grant them back the ability to make up their own theories of what the social is made of, and we have to follow the actors themselves. As such, the researcher is not following a method understood as a linear and regular plan, but must follow the actors themselves and learn from their objections. Indeed, we should not impose metanarratives on the stories of the actors themselves, but let the movements, the words, the events and the actors speak for themselves, as much as possible. In this way, you are constantly searching for associations, between actors you did not know of before, stories which you did not know could be told, but which show themselves as important. That is, we cannot begin with more or less empty concepts such as the ‘social’ and the ‘psyche’ and then expect to find these categories out there. Again, in the words of Latour:

“The word social cannot replace anything, cannot express anything better, cannot be substituted – in any form or guise – for anything else. It is not the common measure of all things, like a credit card widely accepted everywhere. It is only a movement that can be seized indirectly when there is a slight change in one older association mutating into a slightly newer or different one” (Latour, 2005, p. 37).

Accordingly, searching for associations implies letting the stories, the words and the concepts of the actors themselves stand out. There is no ‘on top’ researcher defining what is wrong or right or whose voice is to determine the conclusions. Rather, the aim is to follow, describe and be part of the associations of the field.

Qualitative Inquiry as Following the Traces of Many Kinds of Actors

My second claim in this paper is that qualitative researchers must follow the traces of many kinds of actors. Much qualitative research, and not least interview projects, has as its aim the study of the subjective experience of something (Kvale & Brinkmann, 2009). There is nothing wrong per se with subjective experiences, but what we may tend to forget, if action is limited a priori to what is subjective, intentional and human, are the many other things that may modify the state of affairs in a given situation. Even if we are still interviewing persons and not things, we may risk counting out these other materials in our stories. In a recent study of creative activity this became very evident to me. Many psychological treatments of the phenomena of creativity define it as the ability to think in novel and valuable ways (Craft, 2005; Glăveanu, 2010, 2011). A central component of this is often perceived to be the divergent thinking of the individual person. However, what became evident while interviewing 25 persons about their creative practices was that they continuously referred to things, events and materials in their stories. One of the interview persons which did this was the Russian born painter Andreas Golder. As I was interviewing him, we were wandering through one of his exhibits. At one point, a number of particularly lovely works catch our eyes: miniature people on miniature easels on a shelf. We asked Andreas how he got this idea in particular and his story of the miniature easels brilliantly captures the importance of materials and non-human actors in the process of creative painting:

“Well, I was walking around in a big store with painting supplies and that sort of things. Then, I saw these little easels, and I thought, well, I need to make some miniature works. Because all works of art are getting bigger and bigger, so why not go the other way and make something really little?” Andreas continues, “I have around 20 books of sketches or photos I’ve taken with my mobile phone. It could be anything I come across. Before, I sometimes just got straight to work on the painting, but this caused a lot of mistakes, which meant that I had to paint the whole thing over again. Now, I test at least three or four colours before

I get started in earnest on the actual canvas. Other times, I just go around searching. For example, I walked around my friend's workshop. There were all sorts of junk— modern things and things from the Middle Ages. So I went there and got a lot of ideas” (Tanggaard & Stadil, 2012, p. 80, my translation).

It seems that inspiration for Andreas' work processes comes from many sources and that Andreas samples ideas from all of the materials and sources of inspiration that he has at hand.

“I once did a show for the White Cube. It was a really compact show, and all of the paintings were closely linked. It was very dark and *memento mori*, Catholic pictures and so on, but it was more of a hodgepodge of all sorts of things” (Tanggaard & Stadil, 2012, p. 80, my translation).

Which methodological implications follow from the above examples? Well, they point to other elements than intentional, human actors, showing an interest in what might turn up; the cellar room, the notes blocks, the mobile phone, the canvas. As such, we follow the actors' own ways and begin our travel by the traces left behind by their activity of forming and dismantling everyday life. The researcher needs to describe these movements, lines, traces and must engage in a kind of backward reading of how the final product came into the world. Similarly, we are not beginning with pre-determined and frozen concepts like social class, gender, ethnicity, personality trait or other known and rather abstract concepts taken almost for granted in the human and social sciences, but we are searching for the traces leaving boundary markers which can re-materialize in new forms as soon as the researcher leaves.

Qualitative Inquiry as Theoretical Re-Working of Materials

The inspiration for the last point in my paper comes from having considered how I myself conduct the analysis of a given empirical material and how my students do this. Consulting the literature, the inspiration came from the writings of St. Pierre (2011), Kvale and Brinkmann (2009), and Brinkmann (2012). It concerns the work researchers do when they are writing up their empirical material in research papers and books. While we sometimes distinguish empirical and theoretical work, my point is that both kinds of activities are ordering materials, either on paper, in notes-books or whatever media is used, very often in words, pictures or figures. This is, in my viewpoint, not done to present a better ordering than what the actors in a given field may establish themselves, but it is done to give examples of a particular reading of a field, which may, considering the point of departure, either inform, propose a critical reading, de-stabilize or help re-form, rebuild or just simply do something to a given practice. Theoretical work is definitely a kind of re-working and re-collection of a material, done from a particular standpoint, which literally informs the reading of a field.

Describing qualitative inquiry in everyday life, Brinkmann (2012) proposes six steps of research, e.g. 1) choose a topic, 2) collect materials, 3) consult the literature, 4) continue collecting materials, 5) do analytic writing and 6) publish your text. Considering qualitative inquiry as a theoretical re-working of materials, it is phases three and five which are presently most interesting. Consulting the literature means considering if, for example, a given theoretical framework like Butler's gender analytic may inform a material concerning gender issues or if something else might be relevant. It is my own experience that writing blocks often result from having read too little or not knowing how to infuse energy into the text. Categorizing or condensing empirical material is certainly one way to go, but it is often described in methods books as a rather technical affair of 'cut and paste'. However it can be theoretical concepts which help unpack a given material, because theories represent a way of looking at a phenomenon, which can serve as inspiration.

Furthermore, as stated by Brinkmann: “*what brings rigor and scientific quality to small-scale projects is a disciplined and analytic awareness informed by theory*” (Brinkmann, 2012, p. 4). Informing this view of theories as helping tools in the writing process is the pragmatic standpoint that theories are tool with which we make sense of and grasp the world. As such, it is theories which may help us moving from a personal to a social analysis using what Mills (1959) famously called social imagination. All in all, this calls for images of scientific craftsmanship which destroy distinctions between theory and methods and Brinkmann cites Ingold stating “*that there is no division, in practice, between work and life. [An intellectual craft] is a practice that involves the whole person, continually drawing on past experience as it is projected into the future*” Ingold, 2001, p. 240, as cited in Brinkmann, 2012, p. 4). This echoes Elizabeth St. Pierre arguing that: “*data are collected during thinking and, for me, especially during writing*” (St. Pierre, 2011, p. 621). And what St. Pierre furthermore suggests is that analytical writing is not only informed by theory, dry as it may be, but also by all kinds of transgressive data, emotional data, dream data, sensual data, that is data which might not be visible and which might challenge our usual distinctions between mind and body, reflection and action, and so forth. However, until one begins to think, one cannot know what one will think with. I guess there is no other proper way to summarize this argument, for the moment,, than to cite St. Pierre again on writing before moving on to the conclusion:

“That work about subjectivity (an inadequate concept) required a simultaneity of living, reading, and writing. I needed living (experience is inadequate) for which humanist individuations no longer worked (me with the woman in space-time simultaneously), theories that provided language to think living differently (the ‘posts’ and theories of space-time and memory), and the setting-to-work of writing that forced the rupture and demanded I move on. When writing the next word and the next sentence and then the next is more than one can manage, when one must bring to bear on writing, in writing, what one has read and lived, that is thinking that cannot be taught. *That is analysis*” (St. Pierre, 2011, p. 621).

Conclusion

This paper has been devoted to troubling methods. I (or the paper) did so by suggesting three kinds of research practices:

1. Searching for associations between actors, of both human and non-human kinds
2. Following the traces of many kinds of actors
3. Doing a theoretical re-working of materials.

All three research practices were described and serve to trouble the idea of the (rational) researcher conducting research according to a defined research plan predicting each research step. Rather than following plans, I argue that plans are often re-written during research and that many descriptions of research practices are of a retrospective kind being a post-rationalization of what was actually done during the process. To avoid the often boring and superficial character of such descriptions, I argue for the insertion of more flesh and blood in research reports, using the words of everyday actors, re-working and engaging in dialogue with them. The three kinds of research practices or procedures are based on the idea of scientific craftsmanship challenging distinctions between mind and body, human and non-human, theory and method. Instead, research practice is conceived of as something we live everyday day and research is understood as an activity in which we follow the traces of many kinds of actors and order material in new ways as we write it up.

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