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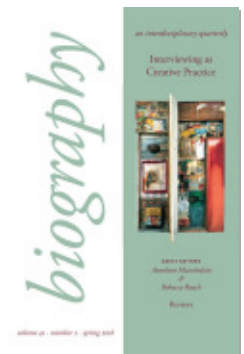
Emergent Conversations: Bronwyn Davies on the Transformation  
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## EMERGENT CONVERSATIONS

### BRONWYN DAVIES ON THE TRANSFORMATION OF INTERVIEW PRACTICES IN THE SOCIAL SCIENCES

INTERVIEWED BY ANNELEEN MASSCHELEIN AND REBECCA ROACH

Bronwyn Davies is an Australian pedagogue with an extremely broad horizon. Best known as a specialist of early childhood education (she received an honorary doctorate at the University of Uppsala in Sweden for her work in this domain), she has also worked on poststructuralist and gender theory and has, in the last decades, gained renown for her innovative methodological practices, especially her work on “collective biography.” Having been a professor of education at James Cook University and at the University of Western Sydney, she has been prolific as an independent scholar since 2009.

A constant throughout Davies’s career is interviewing as a methodology, from her early ethnographic study in 1982, *Life in the Classroom*, which was based on interviews with schoolchildren, through to her 2014 study *Listening to Children: Being and Becoming*. The subtitle of the latter book hints at the importance of the work of Gilles Deleuze and Félix Guattari, which not only provides the conceptual tools for interpretation but has also inspired her to reflect on and use cocreative and collaborative work, for instance in the co-written texts *Deleuze & Collaborative Writing: An Immanent Plane of Composition* (with Jonathan Wyatt, Ken Gale, and Susanne Gannon) and *Pedagogical Encounters* (with Gannon). On other occasions, both in her early work and in later publications, Davies has practiced more classical forms of interviewing with literary authors (*In(scribing) Body/Landscape Relations*), and her long conversation with Judith Butler was published as *Judith Butler in Conversation: Analyzing the Texts and Talk of Everyday Life*. In this book, the dialogical structure is multiplied: the conversation between Butler and Davies is put into a dialogue with divergent perspectives on and uses of Butler’s work.

In the opening pages of the seminal handbook *Doing Collective Biography*, Davies and Susanne Gannon explicitly position their project as an alternative to traditional social science approaches to interviewing:

An interview can be described as the interviewee's best attempt to describe or explain, in the particular dialogic context of the interview, what he or she remembers, based on a particular history of observation and experience. Similarly, our analysis of the interview transcript is our best attempt, based on what we remember having seen or heard or read, both "in" the data and outside the data, to make sense of what is said. (1)

Against what they see as the prevailing tendency to take qualitative data at face value, as self-evident truths, or to try to objectify them by different forms of coding, Davies and Gannon emphasize that the truth found in qualitative data is relative and situated, and sometimes even contains elements that could be considered fictional. Incorporating insights from feminist, poststructuralist, and postcolonial theories, their method of "collective biography" is part of a wider movement of innovation and creativity within qualitative research associated with scholars like Karen Barad, Norman Denzin, Carolyn Ellis, Maggie MacLure, and Laurel Richardson to name but a few. Collective biography, like autoethnography, tries to account for the living, embodied dimension of the interview situation.

For the project that became *Pedagogical Encounters*, during a week-long workshop, a group of researchers drew on their own memories and stories relevant to a topic, working through them collectively. When they were put into a text, they were integrated into an impersonal assemblage. The aim was not to come up with various individual truths or to unearth a hidden collective unconsciousness; rather, they wanted "to provide knowledge about the way in which individuals are made social, are discursively constituted in particular fleshy moments" (4). Although collective biography has many affinities with autoethnography, and Davies has written fiction for children, as a practice it is less literary than theoretical. The knowledge and texts that emerge from collective biography are open, rhizomatic, and transformative, even though they are created within a well-defined research context and method, and supported by a solid theoretical and ethical framework. While collective biography is based on conversation and interaction, it is neither a focus group nor a traditional interview.

The present interview was conducted via email. We sent Bronwyn an initial list of questions to act as prompts; she responded to those she found operative. In further correspondence, we asked Bronwyn to extrapolate on various points, added several questions, and made slight edits to the interview for clarity and to accord with *Biography's* editorial style.

**AM & RR:** Interviewing's privileged status as a qualitative research methodology has recently come into question with the so-called crisis in the social sciences being wrought by big data and social media. What is your sense of the future of the interview method? Does this vision of the future have implications for your own work?

**BD:** I have done so many interviews in so many different ways that at first I didn't know how to begin to answer you—as there were so many possible answers depending on which interviews, or what kind of interviews, I was thinking about. So I don't think there is a single “interview method,” at least not as I understand it.

In the days when qualitative research was struggling its way out of positivist thinking there were all sorts of rules that informed how one should interview. The interviewer should be as neutral as possible and not interfere or suggest anything. The questions were preconceived and had to be repeated exactly with each interviewee; and there had to be a large number of interviewees (that is, it was quantitative research, not qualitative). The interviewees' words were taken to represent the world that existed outside the interview.

In symbolic interactionist and ethnographic studies, you had to find reliable “informants” for your interview whom you could trust to give you reliable accounts of the aspect of the social world you were interested in. But even they could not be fully trusted, and so “triangulation” was necessary. Other informants who occupied different positions in the institution being studied had to be asked for their views, and other methods, such as observation of the group being studied, had to be used to verify the “reality” you had uncovered.

The belief, then, was that it was *possible* for human subjects to give accurate representational accounts of the worlds they lived in, and that those worlds existed quite separately from the interview itself. The interviewee's job was to represent a preexisting, separately existing world, and the interviewer's job was to document that which had been represented.

That set of assumptions started to shift with Garfinkel's 1967 development of ethnomethodology. Interest moved to the work that the “members” (rather than subjects or actors) engaged in to produce the reality they inhabited as it was lived in a particular moment. With this ethnomethodological shift, the preference then was for recorded observations rather than interviews, but, where interviews were undertaken, the interest was in the discursive constitution of reality within the interview itself. There was no longer a separation between the research and the researched. In that case, the interview itself (rather than the world it might reveal) was the object of interest, because

it contained the work that was undertaken by participants (or the members of the interview, being the interviewee and the interviewer) to constitute whatever it was they were interested in. Post Garfinkel, conversational analysis emerged as a rather structured set of analytic methods that extended ethnomethodology. Much later, in Honan et al., some colleagues and I showed how the same data might be analyzed from some of these theoretical frameworks, each approach producing very different insights into the same material.

For the most part, though, in any of that early qualitative research in the 1970s and 1980s you had to be able to “replicate” whatever you thought you had found, or the findings were not legitimate. I wrote some thoughts on the problem of legitimation in “Legitimation in Post-Critical, Post-Realist Times.” Experimental situations where participants were kept in total ignorance of what was being tested were in vogue. Experimenters assumed they had no impact on their findings despite the authority of their white coats and their stony silences and laboratory-style contexts of their work. Participants had to be bribed with money or Smarties to become researchers’ guinea pigs, since the experience was of no intrinsic benefit. I balked at that idea from the beginning. I did not want to engage in what seemed to me to be unethical research; the experience of the research had to be of value to the participants.

I began using interviews in the mid to late 1970s when positivist influences were still very strong in the social sciences and in education. I had undertaken a diploma in education when there was no research available that enabled me, as a teacher-to-be, to imagine how children thought about teachers, or what went on in classrooms. As part of my graduate studies I had undertaken two observation studies in classrooms that horrified me in terms of the lack of communication between children and some of their teachers.

Participant observation, conducted in a symbolic interactionist framework, went on evolving, leading to studies such as Goffman’s *Asylums* (1968) and *Presentation of Self in Everyday Life* (1971), and ethogeny, led by Harré and Secord (1972), with books such as *The Rules of Disorder* (1978), which asked the then-radical question, “why not ask them?” What was also emerging with researchers like Goffman and Harré was the use of theoretical concepts to make sense of what was going on. This was another radical intervention into the stranglehold that positivism had held until then.

For my doctoral research, then, in the late 1970s, I gained access to a new kind of school that had recently begun to emerge, called an Open Plan School. In theory, those schools gave children much more responsibility than they had in traditional “teacher-directed” schools. I struck a deal with the principal that I could use the small room known as the library (though it didn’t yet have any books) as my interview room, and the children in his classroom could

come and talk to me any time they wanted. What was startling back then was how much the children wanted to talk, and how gratifying it was for them to be listened to, their words taken so seriously that they were recorded and transcribed to be used for the book I would write. There were always small groups of children clamoring at my door to gain entry. Mostly they wanted to talk to me about what was going on in the classroom—what injustices, what conflict, what didn't make sense. The teacher, who in the beginning was also the principal, told me the children always came back to the classroom much calmer than when they left and much readier to participate in the activities in the classroom. He found it hard to believe that all I did was listen to them. Listening had not yet been established as something teachers might engage in. Their job was to discipline and to tell and to explain; their questions were only for testing understanding of the teacher's knowledge.

The deputy head also found that hard to believe. He was a much more authoritarian teacher than the principal, and the children were often outraged by his betrayal of what they understood the open schooling philosophy to be about. So horrified was the deputy at the thought that children were being given a voice that might be critical of him that he forbade me to include anything they said about him in my analysis. That forbidding, incidentally, was accompanied by a massive tantrum on his part that included a lot of yelling and throwing of books around his study, behind its shut door, and after everyone else had gone home.

So even then, for me, the interviews were never just interviews. There were no preconceived questions. Our conversations were emergent, rising out of their desire to be heard and to use talk to clarify their thoughts, and out of my desire to understand that talk.

**AM & RR:** That description, “emergent” conversation, seems telling when applied to literary interviews for the tensions it exposes around purpose: are such interviews the product of the writer's desire to be heard—for promotional or political reasons—or a desire to clarify thought? Or indeed, do they waste creativity in the demand for talk? And what is the interviewer or reader's desire?

**BD:** Yes. Literary interviews are sometimes a superficial, promotional exercise. And they are often dogged by the clichéd assumption, on the part of the interviewer, that digging into the writer's identity will unearth vital keys to open up the literary text.

In my interviews with the children, my task was to put to one side my restricted beliefs and assumptions and to open myself up to their world in all

its difference from my own. The idea, then, that primary-school children were more than imperfectly formed or yet-to-be-formed people was integral to a much larger event in which new forms of education were being experimented with and new ideas about childhood were emerging. It was very new for children to be listened to, and for their ways of making sense of the world to be valued. The interviews themselves, as well as the book that came out of that study, contributed to the unfolding of something that was largely un-thought of before that. Others had thought it made sense to interview teenagers but were shocked at the idea that primary-school children might have anything worth listening to.

The early reaction to that study was, in retrospect, also quite shocking—in a different way. My “supervisors” were at a loss when they read my chapters, the sole contribution of one of them being to neatly cross out “I” with a red pen wherever I had written it, replacing it with a neatly written “the researcher.” That was his sole contribution. Another refused to read any of it, as there were no numbers, and another because he thought he wouldn’t understand it. When I had completed the thesis, my head of department refused to allow me to submit it for examination, as he had no way of knowing if it was rubbish. It was soon after published by Routledge and Kegan Paul as *Life in the Classroom and Playground* (1982) and has recently been reissued by Routledge as one of their “classics.” It was also, despite my head of department’s resistance, accepted as a legitimate PhD.

It’s hard to believe now that qualitative research had such a tough beginning and that it was so recent.

What emerged for me in that study were at least two important insights into interviewing. One was that the quality of listening mattered—an idea I have more recently expanded in my book *Listening to Children*, where I write about emergent listening as a form of what Henri Bergson called “creative evolution.” That is, the interview is not a fixed object or method but an entangled, creative unfolding of new ways of being and seeing for both interviewer and interviewee.

The second early insight was that the raw transcripts of the interviews were impossible for anyone to simply read and comprehend. In those early days of interviewing, researchers toyed with the idea that one should simply present all the hundreds of pages of the transcripts as “the findings,” as anything else involved undue interference with the “truth” they contained. But people simply couldn’t read them and make any sense of them. How one might call on concepts and work with the material to enable the crucial interpretive work to be done was a vital part of what my thesis and the 1982 book set out to explore.



**AM & RR:** For researchers, the interview is of course something that has to be interpreted. Do you feel that concepts and theory are necessary to create a distance, an interspace in which meaning can unfold in between the immediacy of the conversation and the position of the researcher who is “looking” for something? Or are concepts necessary for other reasons? Are these concepts from social science, or are they borrowed from other hermeneutic traditions like literature?

**BD:** Yes, these are important questions. Transcribing and then analyzing the interviews is quite different again from the process of listening during the interview. At this point, the transcripts present the researcher with conversational fragments that can be interpreted in clichéd ways, confirming the already known, or new questions can be asked of those fragments. This stage of the research is often quite painful, reading and rereading the transcripts, and reading philosophical and literary work that might provide a way into reading the texts differently. The new concepts do not create a distance between me and the text, quite the reverse, but they do disrupt the hold of those familiar modes of enunciation that make no room for new thought of any consequence.

The antithesis of such analytic work is the movement to coding interview data, taking chunks of it out of context and allocating them to preconceived categories. It is a laborious and generally fruitless exercise, though it satisfies those still yearning for positivist legitimations.

In relation to your question as to whether interviewing is still relevant in the face of “big data,” I can’t imagine that it would ever be irrelevant to listen to people. What format that listening takes, and what interpretive work is done with what they have to say is, to me anyway, endlessly fascinating.

**AM & RR:** You have found the “assemblage” a useful model for understanding collaborative work. Is an interview an assemblage?

**BD:** I think the concept of assemblage is really useful in thinking about any research project based on interviews or textual analysis or collective biography. Even where I don’t specifically mention it as the concept I am using, I find the insights Deleuze and Guattari offer in their discussion of assemblage to be invaluable in bringing together the entangled threads and flows in any aspect of life I am exploring. The place I most conscientiously set out my understanding of assemblage was in a paper I wrote with Peter Bansel on Oscar Pistorius and the shooting of Reeva Steenkamp.

In that paper we explored how the concept of assemblage draws attention to the connections and flows through which an event (and the larger historical and cultural context in which it is embedded) is simultaneously brought into being, stabilized, dissolved, reassembled, and “carried away” (Deleuze and Guattari 98). Assemblages are not objects or things, but qualities, speeds, flows, and lines of force. They are interesting not because of what they are but by what they do or become. They involve multiple encounters and emergent multiplicities.

To study an assemblage, you begin with a concrete case that enables you to come close to the movement of thought as it unfolds: “the starting point required by Deleuze’s method is always a concrete case . . . you place yourself where thought has already started, as close as possible to a singular case and to the movement of thought” (Badiou 13). And that singular case is made out of the “generative flux of forces and relations that work to produce particular realities” (Deleuze and Guattari 7). What the interview provides you with, potentially, is that particular case.

Deleuze and Guattari call assemblages machinic in order to draw attention to the materiality of bodies, and the forms, actions, passions, desires, and substances, and the complex, always mobile relations among them (97–98). Interviews are much more than the transcribed words on the page.

Deleuze and Guattari are also interested in modes of enunciation and what is made possible by them. Collective assemblages of enunciation make some statements possible and others not. They account for the social character of utterances and constitute possibilities for, and limits to, the sayable. A significant task for the researcher is to figure what modes of enunciation the interviewee is mobilizing.

An assemblage territorializes or reterritorializes or deterritorializes a particular space-time as it touches up against other assemblages. The analytic work of the interviewer/researcher is to bring the interviewee’s (and their own) modes of enunciation up against concepts that enable them to see, for example, the workings of power, of gender, race, or class, in the text of the interview even if they are not necessarily part of the interviewee’s conscious frame of reference.

The interview and its analysis is mobile, emergent, a flight into the not-yet-known. In Deleuzian terms, it is a becoming, a process of change, flight, or movement within an assemblage; it is the movement in which one piece of an assemblage is drawn into the territory of another piece, changing it in the process. This movement is one of deterritorialization in which the properties of the constituent elements disappear and are replaced by the new properties

of the (reterritorialized) assemblage. Such an idea would be anathema to a positivist interviewer/researcher!

That takes us then to the process of writing, which inevitably takes the analysis further, while at the same time coming closer and closer to, more intimate with, the text of the interview. The writing, if it works, enables the reader to see how the words of the interview, and the conceptual lines of flight, can open up into that reterritorialized assemblage.

Although I didn't think of the interviews for *Life in the Classroom and Playground* as assemblages, since I didn't have that concept then, it is interesting to think about how differently I might have undertaken the analysis if I had. I would have explored the modes of enunciation that had burst onto the global scene in May 1968, primarily in universities, and then eventually making their way into primary schools a decade later. I would have had more of a sense of the historical event the children were part of as education was being radically rethought. I would have looked more closely at the entangled modes of enunciation that the teachers brought with them from their pre-open-schooling days and at the intra-action between the material reconfiguration of the classroom, the ways teaching was being done, and the betrayals that upset the children so much.

As Badiou says, using Deleuze's concept of assemblage means, though, that you have a concrete case that enables you to "place yourself where thought has already started, as close as possible to a singular case and to the movement of thought" (13). And that is what our conversations, and the subsequent analysis and writing, were doing.

**AM & RR:** What is the importance of aesthetics in your practice of collective biography and (re)assembly and collage? When do you know the text is done?

**BD:** So, first, in what sense might we say that collective biography can be called interviewing? I wouldn't have thought to call it that myself, but I can see how for me it flows out of my experience of interviewing. In collective biography, the group of researchers (and they are all researchers and all research subjects) works with a topic of shared interest, and each draws on their own memories to explore the topic in hand. Each of the participants (usually five or six of them) engages in emergent listening, in listening together to each other's memories. Unlike in "interviews," though, the participants have very specific instructions on how to present their memories both in talk and in writing. They are asked not to use clichés or explanations and not to engage in moral judgment of themselves, the people in the stories, or each other. They are to work with the memory in such a way that it is communicated as an embodied

event, which the listeners are able to imagine as if they, too, had lived it. They tell their stories, write them, and read them out loud. The effect of being listened to collectively in this setting can be very emotional, and goose bumps and tears are common. It is both embodied and moving, and each comes to understand their own experience differently through listening to the others over a period of several days.

This process multiplies what for me are the positive aspects of interviewing and makes them less vulnerable to happenchance.

The process of writing together out of the stories adds another dimension of value for the participants, enabling them to find in what ways drawing on theoretical concepts can enhance what happened in the collective biography workshop.

So how does this relate to aesthetics and to knowing when a text is done? Knowing when a collective biography memory story is complete is partly an embodied sense of satisfaction that the remembered experience exists in the text in a way that others can know what it was to live in that moment. Sometimes we ask each other “does it work” and that “working” is about being carried into the place-time of the memory and to being carried away by it.

And aesthetics? For me, the completion of a text is a negotiated event, even when I am sole author. I need my readers to tell me how the text draws them in (or not) and what it enables them to think. So its aesthetics begin with its power to communicate, and what is being communicated is not some repeated boring platitude but has power to open up new thought. In shaping a text, word by word and sentence by sentence, a different dynamic is at work where I am seeking to maximize its clarity, its capacity to puncture old ways of seeing, and to take the reader with me into that punctured space-time. I learn a lot from novelists like Virginia Woolf about the aesthetics of text. It was fashionable, when I began writing, to write totally incomprehensible sentences, and I didn’t ever want to be so ludicrous. I wanted to write as if I were speaking to my readers and to do that in such a way that they could open up their thinking, in a pleasurable way, into the idea that was unfolding. The aesthetics were very much to do with the accomplishment of that communicative act and with not falling back into old platitudes and modes of enunciation.

**AM & RR:** In literary interviews, the positions of interviewee and interviewer are associated with a curious shifting hierarchy: the interviewed author may ultimately be regarded as the author, but in actual practice the interviewer (and editor) is often in control of the final text. How does this work in collective biography (or the social sciences)?

**BD:** First, to literary interviews or interviews like this one, with a known interviewee, the process is very different, as the answers are linked indelibly to the interviewee. I did some interviews with authors of fiction for my book *(In)scribing Body/Landscape Relations*. I was interested in the way their novels had introduced new ways of thinking about body/landscape relations, so the interviews were focussed on the details of their texts rather than on them. They incidentally found it enormously gratifying to have their texts focused on in that kind of detail. I also interviewed Judith Butler for the book *Judith Butler in Conversation*. That interview was about her thinking, and so she had total control of the interview text. In the body/landscape conversations, I quoted some of what the authors said about their texts, but I retained control of the interview text, subject to their agreement and approval. In neither case would I write about a named interviewee without their agreement and approval. I see that as both to do with ethics and as a means of expanding and checking what the text is trying to do.

In collective biography, the names of the participants are usually detached from their memories on the understanding that they have become collective and also that to give them a particular author is to tie them to dodgy humanist assumptions that might get in the way of what we are trying to do. Our embodied memories are not worked with to tell “who we really are,” or in an orgy of narcissistic pleasure, but as material that takes us inside the aspect of the human condition that we are interested in exploring.

**AM & RR:** Does interviewing presuppose a humanistic subject or subjects at all? Does this create limitations or problems?

**BD:** Interviewees do sometimes approach the interview as a chance to reveal the essential truth of themselves, and when they do that, they sometimes position the interviewer as a therapist. That can make for awkward entanglements and dependencies that are not appropriate to the particular research situation. When the interviewee brings a humanist self to their answers or their conversational ambits, they do provide gorgeous data on how such essentialisms are created and maintained in talk. In a way, whichever way the talk goes, it is going to provide interesting material, even when it is not addressing the thing you wanted to know. Depending on the interviewee and what they already know, I might draw their attention to the production of themselves as essential humanist subjects so that their talk becomes the thing to be explored between us.

**AM & RR:** You have spoken of the usefulness of working with a diffractive rather than a representational methodology when approaching texts, writers, and readers. Can interviews support such an approach?

**BD:** Yes, of course. You can do a diffractive analysis of any material, including interview transcripts, even if the interviewee understood themselves to be representing a truth of the world that was independent of their account of it.

**AM & RR:** Recently some scholars of queer theory have spoken of the “apparent incommensurability of the phrase ‘queer methods’” (Brim and Ghaziani): on the one hand, might the introduction of “queer” to “method” provoke anxieties around scientific reliability? On the other, might attaching “method” to “queer” neutralize the latter’s resistive potential? Do you agree? Does this terminology have implications for the way we think about interviewing?

**BD:** The problem here seems to be the concept of method—a positivist concept through which validity itself is established through a rigorous following of method. In the social sciences we talk about *methodology*, which is the intersection between method and theory, which is more flexible. Even so, I resist wherever I can the idea of method with its authoritarian over- and undertones. In *Doing Collective Biography*, which was to be published as a method book, we compromised by talking about methodic practices and by insisting that we were not providing rules to be followed but some ideas for doing collective biography, which should vary depending on the participants, the question being asked, the context, and so on. But to answer your question, I guess my interviewing has always been queer in the sense queer is being used here, and obviously I don’t see that as a negative. I couldn’t have done what I’ve done if I’d been bound by positivist obsessions with method.

**AM & RR:** We are interested in your thoughts about the perceived tension between theory and the interview conversation. Norms of criticism (quoting and citing others for example) don’t seem to apply in this context, yet much “French theory” has been mediated via interviews.

**BD:** I don’t see them as in tension with each other. My interviewees engage in theorizing all the time, even if they don’t realize that’s what they are doing. I love some of the interviews with French theorists because they put their theories into a more accessible form when talking about them in interaction with the interviewer. As you might have guessed already, I’m not interested in norms—I’m much more interested in creating conditions where they can be deterritorialized.

**AM & RR:** An important element in your collaborations and assemblages seems to be love and trust, but what about distrust and power in dialogues? How do you integrate love and trust, or distrust and power, in collaborative writing?

**BD:** Yes. Love and trust. The interview and the work with the text can't happen without them. I'm reminded of a famous lover in history—was it Casanova?—who discovered the libidinal surge he aroused in women when he listened attentively to what they had to say. There is no doubt a libidinal surge both in listening and being listened to. The connection the encounter creates is embodied. The desire to be known is intense. The gift of the other's trust in you, to know them, to hear them, is an extraordinary gift. One of the ten-year-old boys I interviewed for *Life in the Classroom and Playground* said into the tape recorder, when I was called away to the phone, "Bronwyn, I want head now."

The interviewee, including Roddie who spoke those words, must be able to trust absolutely that the libidinal energy that flows between the interviewee and the interviewer will be held safely within the bounds of the research relationship and never exploited. That adults can and do savagely exploit that trust has been shown over and over again in inquiries such as the recent Royal Commission in Australia into child sexual abuse. It is a trust that must be honored, in my view, in every aspect of our professional work as researchers. Which brings us to distrust and power. The best work I have done on that is I think in my 2008 paper titled "Re-thinking 'Behaviour' in Terms of Positioning and the Ethics of Responsibility." In a way, I could describe my life's work as deterritorializing those situations that are saturated with distrust and oppressive forms of power (not least authoritative ideas of method). So many of our life's situations are saturated with them, and my life work has been to search for ways of doing human life differently.

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