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How to cite:

Hammersley, Martyn (2012). Am I now, or have I ever been, a Symbolic Interactionist? Autobiographical reflections. In: Denzin, Norman ed. Blue Ribbon Papers: Behind the Professional Mask: The Autobiographies of Leading Symbolic Interactionists. Studies in Symbolic Interaction (38). Bingley: Emerald Group Publishing Limited, pp. 153–174.

For guidance on citations see [FAQs](#).

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Version: Accepted Manuscript

Link(s) to article on publisher's website:

[http://dx.doi.org/doi:10.1108/S0163-2396\(2012\)0000038010](http://dx.doi.org/doi:10.1108/S0163-2396(2012)0000038010)

<http://www.emeraldinsight.com/books.htm?issn=0163-2396&volume=38&chapterid=17031017&show=abstract>

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Martyn Hammersley

AM I NOW, OR HAVE I EVER BEEN, A SYMBOLIC INTERACTIONIST?
AUTOBIOGRAPHICAL REFLECTIONS

Assigning or claiming identities can be a dangerous business, to varying degrees. Labels can carry conflicting meanings; and, even more importantly, what is a laudatory term to some will be grounds for condemnation by others. My immediate response to the invitation to write this piece, aside from the pleasure of being asked, was that I was not sure that I could claim, or even that I would want to claim, to be a symbolic interactionist. I have a visceral dislike of theoretical-cum-methodological camps, not least because over the years I have been accused of belonging to a variety of these, from positivism to postmodernism, always with disparaging intent. However, reflecting a little more on the invitation, I realized that I could not reasonably deny that in the past, particularly in the 1970s, I regarded myself and had been seen by others as an interactionist. Moreover, while my ideas about sociological work are now somewhat different from what they were then, and my direction of travel might be viewed as ‘un-interactionist’, in fact much of my work is still focused on issues coming out of the interactionist tradition: notably, Blumer’s ideas about methodology (Hammersley 1989 and 2011a), Becker’s arguments about ‘Whose side are we on?’ (Hammersley 2000:ch3; 2004a), and the notion of analytic induction (Hammersley 2011b, and 2011c, Hammersley and Cooper 2011).

Moreover, I still believe that interactionism, as a broad tradition, represents an important counter-trend to much that is currently wrong with sociology, and with the social sciences more generally.¹ For me, the list of failings it challenges would include: the prevalence of speculative theorizing in much qualitative research today as well as in ‘social theory’; demands for scientific rigour that try to reduce this to calculative or logical procedures, or require reliance upon what is purportedly quantifiable and/or observable; and rampant political partisanship. I also have a methodological preference for the kind of ethnographic studies that interactionists typically carry out. Yet, I probably see the character of the interactionist tradition rather differently from many of its current advocates, and I do not regard the boundaries around it as fixed and impermeable.²

My title alludes to a time in the UK when, in some quarters, ‘interactionist’ was a stigmatized identity. In the early 1970s interactionism had been a fashionable and influential position, notably in the study of deviance, education, and health. However, by the end of that decade it was less well-regarded. Indeed, in 1978 David Hargreaves was asking ‘Whatever happened to symbolic interactionism?’, suggesting that in the sociology of education, the field in which I was then working, it had been eclipsed by other kinds of work, notably those inspired by Marxism (Hargreaves 1978). In the circles in which I moved, from early on it was subject to ‘interrogation’ from a number of directions:

¹ For arguments along these lines, see Atkinson and Housley 2003; Martin and Dennis 2010.

² Atkinson and Housley (2003:chs2 and 6) have raised the general question of whether British sociology has ever contained an interactionist school, while also noting how interactionist ideas have had wide influence within it.

1. Ethnomethodologists and conversation analysts challenged interactionism's alleged theoretical assumption that the coordination of action requires participants to share the same meanings, its treatment of motives and institutions as factors shaping actions and their outcomes, and its alleged lack of rigour in using verbal data.
2. Marxists accused it of being 'macro-blind', neglecting and obscuring social structural forces and inequalities, so that it implied that the world could be transformed simply through changing how people see it. They also accused it of ignoring the way in which perceptions and beliefs are ideologically generated.
3. Feminists and anti-racists charged interactionist work with perpetuating patriarchal and racist attitudes, through the choice of topics for investigation, its 'voyeuristic' tendencies, and its 'failure' to adopt an activist stance towards the world.

Moreover, recently, near the end of my career, interactionism, along with all kinds of qualitative research, has been subject to attack by advocates of revived forms of 'scientific research', notably those championing the randomized controlled trial as capable of determining which policies and practices 'work', treating this as the proper task for social research.³

A brief life history

I was born in Manchester UK, in the North of England, starting life in Moss Side, an inner-city area where my grandparents lived. My parents later moved to another part of Manchester, to a semi-detached rather than a terraced house, signaling a move 'up' in the world. They were part of the generation that experienced expanded opportunities for social mobility out of the working class into higher-income jobs (Goldthorpe 1980). And I benefitted not just from their good fortune and hard work but also from the fact that my generation's chances of entering university were much greater than theirs, as a result of expansion of the higher education system during the 1960s. I managed to enter university despite the fact that, at age 11, I had been consigned to a school for those deemed to be 'non-academic', destined for manual and clerical jobs, only later graduating to one of the early 'comprehensive' schools.

I became a sociologist largely as a result of taking a course at that school in 'The Use of English', which encouraged the development of arguments about social issues. The teacher, one of several who shaped my life, also engaged in a lengthy written dialogue with me where I defended cultural relativism. This experience turned me from the study of history, previously my primary subject, to an interest in understanding contemporary society. Around this time I took out Karl Mannheim's (1940) book *Man and Society in an Age of Reconstruction* from the local public library, incorporating some of his neologisms in a history essay, much to the disgust of my teacher. Later, I borrowed Merton's (1949) *Social Theory and Social Structure* from another teacher, who had done sociology at university. I also took an economics course at school, it was not possible to study sociology at that time, though I soon came to the conclusion that one could not understand even market behaviour and

³ See, for example, Oakley 2000; but see also Oakley 1998.

economic institutions without adopting a broader social perspective than that of the economists.

I became an interactionist when I was an undergraduate at the London School of Economics in the late 1960s. What the label meant to me then was very much shaped by the local context, and in particular by the dominant kinds of work done at LSE. The main theoretical perspectives at the time were various forms of 'conflict sociology'. This constituted a reaction against structural functionalism, drawing on the work of Marx and Weber but shorn of their philosophies and politics. As with functionalism, the focus was still on the social system, but there was an emphasis on structural contradictions and other sources of conflict within societies. Key influences were the work of David Lockwood (1956, 1964, see also 1992), Lewis Coser (1956), and Ralf Dahrendorf (1959; see also Collins 1975). As regards methodology, what was taught, at least at undergraduate level, was primarily the use of official statistics and survey research. There was a strong local tradition of 'political arithmetic' in the UK, and especially at LSE, this employing quantitative data to investigate social and educational inequalities, and being guided by 'ethical socialism' (see Dennis and Halsey 1988). While I didn't know it at the time, Paul Rock was already teaching at LSE, championing a more interactionist approach, but I didn't take any courses with him. He later went on to write what is, in my view, still among the best books on interactionism (Rock 1979).

I arrived as an undergraduate at the London School of Economics just in time for the student rebellion of 1968 – I took a minor part in occupying the building on the weekend of the Grosvenor Square demonstration against the Vietnam War. At this time there were vociferous reactions against current forms of sociology by those, mainly junior staff and postgraduate students, who championed Marxism – this time with the philosophy and the radical Leftist politics very much left in, albeit often twisted in an anarchist direction.⁴ The writings of Lukacs, and especially Gramsci, were promoted, but also the more recent work of Althusser and his students from across the Channel. Robin Blackburn, who was sacked from LSE for his political activities and who became editor of *New Left Review*, was one of my tutors – I recall his being shocked when we first-year undergraduate students complained that Heidegger's *Being and Time*, in the original German, was on the reading list for his seminar. Even had we been able to read German, and only one of us could, I doubt we would have been able to make much sense of it.

Around this time, I discovered the work of the symbolic interactionists, especially Blumer and Becker, and also phenomenological sociology. Blumer's *Symbolic Interactionism*, Berger and Luckmann's *The Social Construction of Reality*, and Schutz's *Collected Papers* became my guiding texts, along with Cicourel's book on *Method and Measurement in Sociology*. I read these books several times while still at LSE, even though they weren't on any of the reading lists for courses I took.⁵ Later, I explored the growing interactionist literature in the field of deviance, and of course

⁴ I remember the Socialist Society posting an open invitation to any member of the Sociology Department willing to attend a meeting to defend the idea that conflict has positive functions in current society, along the lines suggested by Coser.

⁵ One of my tutors at LSE, Gabriel Newfield, set me an essay on Kuhn's *The Structure of Scientific Revolutions*, and this too became a very important source for me.

the work of Goffman. David Matza's *Becoming Deviant*, with his championing of what he called 'naturalism', also became extremely influential for me.

When it came to thinking what I wanted to do for a PhD I had already decided that this was the sort of approach I wanted to adopt. It appealed for a number of reasons:

1. *The emphasis on agency rather than on systems and structures.* I saw this as an essential complement to conflict sociology's stress on dissensus and diversity, as against the presumption of structural coherence and value consensus that was central to functionalism. The forms of Marxism that were becoming influential at the time were rather ambiguous on this issue, it seemed to me. In many ways, Marx's epigram '[people] make history, but not in circumstances of their own choosing' captures the spirit of interactionism rather better than it does that of much Marxism. For Marx and many Marxists, agency and action are located within a teleological conception of History derived from Hegel (Prokopczyk 1980), or alternatively within a system theory that is not very different in *form* from that assumed by functionalism (see Cohen 1978).
2. *Agency and action were approached from a sociological rather than from an entirely psychological perspective,* with an emphasis on how people collectively interpret and respond to situations, the cultural resources that they use to do this, and so on. At the same time, this was not done through reliance on the notion of a collective subject, with Humanity treated as the agent of History, and this also seemed to me to be an advance over Marxism.
3. *The diversity that was recognised extended beyond commitment to different values to include variation in ways of experiencing and interpreting the world.* The fact that people could and did on goingly 'construct' reality in different ways, through interpretation and action, was perhaps the central feature of what I took interactionism to stand for at this time.
4. *Interactionist work seemed to involve a cosmopolitan perspective on the world rather than a dogmatic, partisan one.* I was committed to the same sort of Leftist politics as many of my peers but I had also been interested in existentialism and had read the work of Albert Camus; and, as a result, it seemed to me that utopian forms of politics needed to be resisted. I felt that Camus' social and political thought represented a more realistic, tolerant, open-minded perspective on the world even than the kind of Marxism adopted by Sartre, where the idea of what is good and what is bad, how things ought to be, and so on are taken for granted, much as they were by many Leftist sociologists in the 1970s, and still are today. I regarded the view of the world implicit in Becker's work, especially, as very similar to that of Camus.⁶

⁶ I didn't meet Howie Becker until quite recently, despite our having made a radio programme together (recorded in separate parts on each side of the Atlantic) in the late 1970s, and being in correspondence on a few occasions.

5. *The insistence that we must not assume that the world is how some theory claims it to be, that we must explore and inspect it and try to describe and explain it on its own terms.* This sort of ‘empiricism’, the label often applied to it, seemed to me to be of great value. Equally important was the argument that very different methods were required from those characteristic of much mainstream sociology, namely kind form of ethnography.

6. *Interactionism also avoided the naïve romanticism that treats the views, experience, and feelings of the marginalized or oppressed as genuinely authentic, and their culture as superior to that of any elite.* While the importance of understanding the perspectives of all parties involved in a situation was emphasized, so that any hierarchy of credibility that operated was to be resisted, at the same time there was no pretence that those at the bottom of the hierarchy have any epistemic privilege (see Becker 1967).

7. *There was a desire for knowledge but also a recognition of the limits operating on the sorts and degrees of knowledge that are possible.* In its original form, interactionism combined recognition of the constructed and plural character of perspectives with an insistence on the importance, and the possibility, of discovering what people’s attitudes *actually* are and what is *really* going on. While this represents an unstable, perhaps even contradictory, orientation, it nevertheless seemed productive in sociological terms.

I went to the University of Manchester to do my PhD, largely because I had discovered that there were people in that department who were interested in interactionism and did interactionist work. However, when I got there I found that what had previously been a predominantly interactionist department, headed by Peter Worsley an anthropologist whose work was strongly influenced by Marxism, was now split, with a strong contingent of converts to ethnomethodological conversation analysis. The influence of Garfinkel and Sacks had begun just before I arrived in the Department, and grew very strong while I was there. Initially I knew little about their writings, and while I became sympathetic to this kind of work, I never abandoned the more broadly interactionist perspective I had already acquired. It seemed to me that conversation analysis, in particular, had much to contribute to interactionist ethnography, but that its guiding assumptions were open to serious question.⁷

While, my supervisor, Isabel Emmett, was neither a Marxist nor an ethnomethodologist, she was not really an interactionist either. She had carried out an anthropological community study near Blaenau Ffestiniog in Wales, and later returned to study the town itself, but when I was at Manchester she was carrying out a survey on youth and leisure in the Manchester area (Emmett 1964, 1971, 1982a and b). With hindsight, I greatly regret that at the time I paid little attention to her work, or to the whole community study tradition; something for which my infatuation with interactionism, phenomenology, and conversation analysis was largely responsible, along with her intellectual modesty.

⁷ It was thirty years before I finally completed my assessment of ethnomethodology and conversation analysis: see Hammersley 2003.

My doctoral work at Manchester involved carrying out what I initially thought was entirely novel: the application of interactionist and phenomenological ideas, along with some of the resources provided by conversation analysis, to the sociological study of education, in particular classroom interaction in secondary schools. In this I also drew on the fundamental questioning of schooling characteristic of the radical education and deschooling movements of the late 1960s. In fact, this was very much my starting point and my source of motivation. At the time, I regarded dominant forms of education as reproducing an unequal and alienating society. I produced a small number of articles from this work, though it took me nearly ten years to finish my thesis – not a good model for students today (Hammersley 1974, 1976, 1977, 1980, 1989b; see also Hammersley 1984).

Early on in my doctoral work, I discovered that there were others pursuing similar agendas. In 1970 a book entitled *Knowledge and Control* was published, and soon after that an Open University course appeared called *E202 School and Society*, these coming to be treated as representing a ‘new sociology of education’ (Young 1970; Open University 1971). This movement had started at the London Institute of Education, under the influence of Basil Bernstein and Michael F. D. Young. However, I discovered that, elsewhere, other researchers were also going in new directions (see Atkinson et al 1988; Atkinson and Housley 2003:90-101 and *passim*). At Manchester, there were regular departmental seminars, which were very lively, with speakers being vigorously challenged by both ethnomethodologists and Marxists. One speaker in my early years there was Paul Atkinson, and talking with him afterwards we discovered that we shared very similar views about sociology and methodology. I also met Sara Delamont around this time, who was almost single-handedly challenging the quantitative approach that then dominated the British Educational Research Association. I also became aware of what was going on in the sociology of deviance in the UK, the work of people like Stan Cohen, Jock Young, Paul Rock and others, by attending the National Deviancy Conferences (Cohen 1971; Taylor and Taylor 1973; see also Atkinson and Housley 2003:46-7).

In 1975 I was invited to a conference organized by Peter Woods, who was working at the Open University, and in that year I too obtained a job there. I found that Woods was also using interactionism to rethink the sociology of education (Woods 1979), and he subsequently played an important role in coordinating a community of interactionists studying educational processes in the UK, notably through organizing what came to be called the St Hilda’s conference, named after the Oxford college where the first conferences were held.⁸ He also ran several influential research teams that applied an interactionist approach in the sociology of education (see Hammersley 1999a). We published several books together that showcased interactionist work on education (Hammersley and Woods 1976; Woods and Hammersley 1977 and 1993).

Schooling and Society, the course at the Open University for which I had been recruited, was a re-make of the earlier course that had already been very influential in spreading interactionist ideas. However, just as the situation at Manchester was not what I expected when I got there, so too things had moved on at the OU, this time in

⁸ These conferences are still going today though the name was later changed to the Oxford Ethnography Conference: see <http://www.ethnographyandeducation.org/conferences.html>

the direction of Marxism. The new course team was split, one side working within interactionism while others promoted an approach influenced by, for example, Althusser's (1971) article 'Ideology and ideological state apparatuses' and the work of Bowles and Gintis (1976). The Marxist character of the early parts of the new course attracted national attention, and began to be publicly criticized in newspapers and on the radio even before we had even completed writing the whole course. The Course Team were accused of 'Marxist bias', and there was an internal inquiry within the University.⁹ Questions were raised in the UK parliament about the issue, and it was said that the Secretary of State for Education was reading the available course materials himself. The interactionists on the Course Team were caught in the cross-fire, having to defend the Course against external attack by forces from the political Right.

However, perhaps the most significant event in my intellectual career was that, immediately after finishing work on *Schooling and Society*, I was invited to join the team working on a new course on research methods. As a result of the influence of interactionism, I had become interested in the relationship between theoretical ideas about society and the methods used to investigate it. I'd also read the then fairly small literature on qualitative method. But I was relatively ignorant about quantitative research, having previously largely rejected it, using the stock arguments common among qualitative researchers. Now, as the only practising qualitative researcher on the course team, I had to engage with people committed to various kinds of quantitative work, and I found that the issues dividing us were more complex and uncertain than I had previously realised. Jeff Evans, who was responsible for bringing me on to the course in the first place, proved an important guide in this process, then and subsequently (Evans 1983).

As coordinator for the units in the course dealing with qualitative method, I commissioned Paul Atkinson to write two of these, while I wrote the others. We also made a couple of television programmes together, with Paul taking the lead role. Subsequent to this, we decided that we could build on what we'd written to produce an introductory text on ethnography (Hammersley and Atkinson 1983). In doing this, we found that we agreed on many matters, and as far as I can remember there was little debate between us about the issues. He proved a stimulating intellectual collaborator and provided essential support in thinking more deeply about methodology, especially given my isolation on the Course Team. While, subsequently, our views have diverged somewhat, this has not been a problem in preparing later editions of our book: pragmatism has long been the order of the day for both of us (Hammersley and Atkinson 2006).

Another significant contact for me, a little later, was Phil Strong, one of the most important interactionist researchers in the UK, who worked in the field of health and drew particularly on the work of Goffman. He obtained a post at the OU, within another faculty, and we were able to meet up and share ideas. His broad interests and thoughtfulness, and marvelous essays, were illuminating, and his death at a young age was an intellectual tragedy as well as a personal one for those who knew him (see Bloor 1996; Strong 2006). His essay on sociological imperialism, in particular,

⁹ See <http://www.open.ac.uk/blogs/History-of-the-OU/?p=20> and Harris 1978.

resonated with growing concerns I had about the ‘grand conception’ of sociology (Hammersley 1999c).

Working on the Open University research methods course (Open University 1979), and being forced to take the arguments behind quantitative work more seriously, led me to question some of the assumptions that I had initially adopted from interactionism and qualitative methodology. The differences between approaches were not as clear-cut as I had thought, nor were the positions I had adopted unproblematic. For example, I discovered that in the 1930s Blumer had been arguing against a form of positivism, represented for example by Lundberg, whose character bore some striking resemblances to the constructionist stream of interactionism, arising from a nominalist reading of labeling theory and other sources, that I found appealing. Blumer countered this with a form of realism, but his position itself came to be criticized as turning Mead’s stance in a nominalistic direction (Lewis and Smith 1980). Around this time I began to read primary and secondary material about pragmatist philosophy, coming under the influence of Peirce. While his work is open to divergent interpretations, from his being seen as a proto-logical positivist to being viewed as an objective idealist, what was most important for me was his account of the nature of scientific inquiry, and the epistemological and ontological arguments he deploys in explicating this. Later, I discovered the work of Susan Haack, which develops this aspect of Peirce’s thinking, and this too has been a very important influence on me (Haack 1999, 2003, 2009).

At this time, I came to the conclusion that a major problem with the growing body of qualitative work, in education and other fields too, was a general failure systematically to build and test theories. I explored what this required in several articles (Hammersley 1985 and 1987a). This led to empirical research that I did with two colleagues – John Scarth and Sue Webb – looking at the influence of variations in assessment regime on modes of teaching in secondary schools. We realized that this focus required us to use quantitative as well as qualitative data, and to find ways of relating the two; and in the course of this we became very critical of both sides of the quantitative-qualitative divide (Hammersley et al 1985; Hammersley and Scarth 1986a and b, 1987a). This experience led to articles on quantitative method – dealing with reliability and validity, internal and external validity – and also to a series of methodological assessments of qualitative studies, resulting in a book on reading and assessing ethnographic work (Hammersley 1987b, 1991, 1997).

In the 1990s my work took another turn, being shaped by one of my PhD students, Peter Foster. He carried out a study of a school that had a reputation for its anti-racist policy, arguing that there was little evidence of racial discrimination on the part of teachers in the school. He suggested, instead, that the underachievement of black students at the regional, and perhaps also at a national, level stemmed in large part from differences *between* schools rather than from processes *within* them (Foster 1990). His argument went against the conclusions of much research on racism in English schools at the time, which implied that most teachers were racist and that this was what produced national ethnic inequalities in educational achievement. Subsequently, Foster criticized some of that research, and a major dispute arose that was frequently acrimonious (see Hammersley 2000:ch4). A colleague, Roger Gomm, and I wrote some articles in support of his position, and all three of us were accused of racism. This experience made me reflect further upon the naïve political

assumptions that seemed to shape a lot of social research, not just that concerned with racism. Around this time, I examined the claims made about feminist methodology, challenging amongst other things its commitment to a political framework of analysis, and later I extended my critique to 'critical' approaches more generally (Hammersley 1992, 1995, 2000b). All this forced me to re-think my experience back to the politics of the 1960s, and the way I too had been ready to build political assumptions into my work. Working with Peter Foster and Roger Gomm, I moved towards a more Weberian position (Foster et al 1996). However, Peter Foster's suicide in 1999 cast a shadow over subsequent work.

As is perhaps clear, I was becoming increasingly critical of dominant trends within the fields in which I worked, and this included methodology. Of course, that field was not standing still but in fact being pushed in directions even further away from my position, symbolized for example by the sorts of approach represented in later editions of the *Handbook of Qualitative Research* (Denzin and Lincoln 2000 and 2005). From quite early on I had become involved in an informal trans-Atlantic correspondence with John K. Smith about criteria for assessing qualitative work. This resulted in some publications but generated little agreement between us (Hammersley 2004b, 2009; Smith and Hodkinson 2009). Later, I tried to spell out the grounds for my disagreement with these increasingly influential trends within qualitative research methodology (Hammersley 1999b and 2008).

Around the same time, qualitative research started to come under attack from those who argued that the function of research is to facilitate evidence-based practice. I was critical of this notion (Hammersley 2001, 2004, 2006), and I became a target of criticism in turn, with Sir Iain Chalmers apparently suggesting in various talks that my attitude would change if I were seriously ill and viewed matters from the perspective of a patient on a trolley in a hospital needing the best treatment possible.¹⁰ The impact of the evidence-based practice movement generated a major crisis in educational research in the UK in the late 1990s, and subsequently in the US. Interestingly, one of the major figures on the other side of this debate, David Hargreaves, had originally championed interactionism within the sociology of education, and had made important contributions in that field (Hargreaves 1996; Hammersley 2000a). I wrote a response to his influential call for research that demonstrates 'what works', albeit a response which found more common ground with him than most other reactions to his lecture (Hammersley 1997b).

These developments not only stimulated me to think more deeply about the relationship between research and practice (Hammersley 2002) but also to finish an empirical study that I had started earlier. This looked at how research is represented in the mass media, one of the main routes through which it is likely to have any major impact on policy and practice. I examined the ways in which a review of research on ethnic inequalities in education had been covered by the media, exploring the complex relations between research as a source of knowledge and media representation of the findings and their implications. My focus was on possible bias media bias, given that researchers frequently complain about media distortion of their work (Hammersley 2006).

¹⁰ For his published critique, see Chalmers 2003; see also Hammersley 2005.

In the debates about research and evidence-based practice, as with the Marxist bias controversy, I found myself somewhere in the middle between warring parties – seeking to resist what seemed to me to be very undesirable developments on both sides. I became increasingly concerned by the reaction of many advocates of qualitative research, especially in the US, against arguments for ‘more scientific’ forms of social research. The new emphasis on quantitative method and applied research had to be resisted, but my view was that this must be done on methodological not political grounds, and that we needed to take proper account of the complexities of the issues involved (Hammersley 2008). This was not an easy or popular position to take, and was seen by some interactionists as a kind of betrayal (see for example Denzin 2009a and 2010). But, for me, that reaction is symptomatic of the ideological mindset that now prevails (Hammersley 2010).

Looking back on my career now, my concern is that academic social science is in decline, in the face of politicized and arts-based approaches to qualitative research, on one side, and of increasing ‘strategic management’ of research, within universities and beyond, that is designed to make it serve policymaking and practice in direct ways (Hammersley 2011), on the other. It is hard not to despair at the prospects, at least in the short and medium-term.

Conclusion

So, am I still an interactionist? In some ways, writing this piece has convinced me that I am, even more strongly than I thought before. But what I draw from the interactionist tradition is seriously at odds with the positions now adopted by many researchers who call themselves interactionists. While I do not believe that battling over the label is worthwhile, the underlying issues are of great importance. Much hangs on how they are resolved.

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