Collaborative research and the emotions of overstatement

Collaborative research and the emotions of overstatement: four cautionary tales but no funeral[1]

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Abstract

Collaborative research has grown remarkably in social science, bringing many benefits. Arguments for collaborative research are sometimes overstated, however, and this risks counterproductive consequences. In particular, the presentation of how collaborative research relates to other approaches to research may be problematic. Four issues are explored concerning potential consequences of paying insufficient attention to history; conflating different and better; advancing simplified accounts of complex issues; and courting conflict. These discussions relate to cautionary tales involving diverse emotions: hope and disappointment, pride and shame, love and anger, and hubris and humility. All are variations on the theme that exaggerating a case can unintentionally hamper rather than assist the achievement of high-quality, socially-useful research. The conclusion advances a methodological pluralist stance, treating collaborative research as one option available to researchers rather than an inherently superior approach, thereby encouraging mutual tolerance and constructive dialogue while reducing the risk of reigniting oppositional 'paradigm wars'.

Key words collaborative research, emotions, methodological pluralism

Introduction

Research methods and methodology is a well-populated field in which proponents of an array of different approaches vie for the attention of researchers. There are various familiar narratives by which this is done. The case for adopting long-established methods can be framed in terms of their tried-and-tested status and reliability, against which innovative methods may be presented as more imaginative and having the capacity to break new ground. In addition, long-established methods allow research findings to fit more readily into the model of knowledge as cumulative, as building on what has gone before, while innovators conversely argue for the value of fresh starts and new types of insights. Collaborative research has grown significantly in recent decades, a trajectory aided by the emphasis placed by proponents on its several strengths and the supposed shortcomings of other approaches. These claims relate to, amongst other things, 'quality, democracy and equity'. Although they do not necessarily treat collaborative research as 'one new truth, one new method, to replace all others' (Facer and Pahl 2017, 1-3), they do nevertheless tend to imply that collaborative research is better than those alternative approaches which by contrast have been critiqued variously as exclusionary, unaccountable, exploitative, unethical, invalid, unreliable, unimaginative, unproductive and lacking significance. (This mode of argument has parallels to the process through which heterogeneous entities are lumped together and re-framed as an inferior 'Other' (Said 1978, 1).) If Ian Craib was correct in his perception that the expression of emotion is 'intimately linked with hierarchy' such that 'the more hierarchical a social structure, the more important it seems to be to suppress the expression of emotion' (1994, 87), it follows that less hierarchical methods and methodologies will bring emotion to greater prominence in the research process. Emotion will also figure in how research is presented, because it is an important feature of processes of persuasion, including the use of rhetoric (Edmondson 1984; Walton 1992).

The use of exaggeration as a rhetorical device to help make a case is not new; indeed, it has featured prominently over the history of social science (Crow 2005). In the field of research methods and methodology exaggerated claims can come to resemble religious beliefs held by 'True Believers'

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[1] With thanks/apologies to the poet Hillaire Belloc and the film director Richard Curtis.

(Rugg and Petre 2007, 23-4). Long before collaborative research gained its current profile, Howard Becker warned of the dangers of 'proselytizing', that is, the 'very strong propensity of methodologists to preach a "right way" to do things, because of their desire to convert others to proper styles of work, because of their relative intolerance of "error" – all these exhibiting the same self-righteous assurance that "God is on our side" that we associate with proselytizing religions'. For Becker, it was anathema for researchers to be told 'what kinds of methods they should be using' and for those who did follow this guidance to be promised 'salvation' (1971, 4). As in other aspects of the research process, there are risks associated with the idea of there being 'One Right Way' (1986, ch.3). Regrettably, Becker's warnings were insufficiently heeded and the period of paradigm wars between rival groups of 'True Believers' stands as 'a pointless and damaging episode in social research' (Williams and Vogt 2011, 4). The intensity characteristic of these conflicts may have been left in the last century but paradigm wars were not conclusively ended (Bryman 2008a) and the potential remains for 'sectarian methodological fights' to re-emerge in new guises as people once more find themselves on the receiving end of adversarial attacks regarding 'who has the best method' (Lamont and Swidler 2014, 155). The purpose of the cautionary tales which follow is to discourage the proponents of collaborative research from following this path. Discussion of how emotions feature when research is approached collaboratively is an integral part of these considerations.

Before discussing four ways in which emotions figure in exaggerating the case for collaborative research which may turn out to be counterproductive, it is worthwhile setting out core points about which broad agreement exists concerning the role of emotions in research, and some key controversies. Prominent among points of agreement is that research encounters frequently engage the emotions of researchers, research participants, and audiences. It has, for example, become established practice for researchers to record this in fieldwork settings: 'At least from the 1960s, most sociological ethnographers have advocated including accounts of personal feelings and emotional reactions in core fieldnotes'. This allows, amongst other things, the researcher 'to identify biases and prejudices as well as the changing attitude towards people and events' (Emerson *et al*. 2001, 360-1). Samantha Punch's account of the value of her Bolivian fieldwork diary is in this tradition, although she notes that the possible bias towards greater recording of 'negative emotions' (such as guilt) may be a reason for limiting the extent to which researchers draw directly on diaries in publications, along with the fact that 'participants are vulnerable when we ask them to reveal their emotions and open up to us' (2012, 91). Research conducted in the global South may be particularly prone to 'messy and emotionally challenging encounters' (Baillie Smith and Jenkins 2012, 76), but the presence of emotions in the research process for social scientists and humanities scholars is widely-recognised (Loughran and Mannay 2018a). If it is accepted that there is 'fundamental and pervasive relevance of emotions to all aspects of social life' (Holmes *et al*. 2019, 4) then researchers in these disciplines must engage with emotions. This recognition of emotions' ubiquity goes hand-in-hand with the observation that 'many methods are… available for exploring emotion' (Hochschild 2019, 9). As is appropriate for such a multi-faceted phenomenon, the methods available to research emotions include interviews, observation, linguistic analysis, visual research, documentary research and surveys (Flam and Kleres 2015).

Less of a consensus exists about some other matters. One of these concerns the relationship of emotions to the phenomenon of 'affect', the focus of a new field of study about which numerous competing perspectives are available (van Scheve 2018; Seyfert 2012; White 2017). The emphasis that writers on affect place on the importance of context militates against any single, generalizable theory of affect; indeed, it may well be a source of the scenario in which there are 'nearly as many different theories and definitions of affect… as there are thinkers who engage it' (Campbell 2017,

568). A degree of commonality exists in their concern to emphasise affect's collective rather than individual character, given its connection to the forces generated through encounters between people as well as between other kinds of entities (Halldórsson 2017). As a result the focus on affect directs attention away from an individual person and his or her emotions to collective phenomena, and away from rational deliberation to forces that Ian Skoggard and Alisse Waterston suggest include the 'collective unconscious' (2015, 112). If affect is taken to mean 'simply a capacity to affect or be affected' then it is not limited to the sphere of human agency, and people are merely one of many components in 'assemblages' in which micropolitics can lead to various outcomes, including 'unintended effects' (Fox and Alldred 2018, 193, 191). Emotions researchers who restrict their attention to people rather than extending it to non-human phenomena will nevertheless find here a shared rejection of 'the ideology of the rational, detached and objective researcher' (Loughran and Mannay 2018a, 11) and a shared commitment to pursue the question not of whether emotion matters but of 'how, when and where' (Loughran and Mannay 2018b, 262) it does. Whatever their differences, the study of emotions and the broader study of affect both look to the examination of particular cases for lessons about the workings of collaborative research. The particular concern of this paper is with what such cases reveal about what might be called the entanglements of emotions and exaggeration.

The danger of paying insufficient attention to history

There is much to be said for the exhortation to 'always begin with history', as both recommended and practised by the sociologist Ray Pahl (Crow and Takeda 2011). Pahl's celebrated study of the Isle of Sheppey in south-east England demonstrated that a researcher could not properly understand the community there without knowledge of the Royal Naval Dockyard at Sheerness that had for a long period in the past been the island's principal employer. Even though it had been closed in 1960, some two decades before Pahl's fieldwork, local people continued to remember the dockyard affectionately and its legacy remained powerful. Knowledge of this history helped to see later community relations in proper perspective, including people's emotional attachments to a place disregarded by outsiders but about which the local inhabitants exhibited a 'fierce pride' (1984, 189). Attention to history is also important in academia, if the risks of amnesia are to be avoided (Gans 1999 ch.18); a loss of comparative perspective follows any retreat into the present (Elias 1987). One consequence of losing sight of what has gone before is that exaggerated claims to novelty can result. This is common in the field of methodological innovation, where excited claims to novelty do not always stand up to scrutiny and sometimes turn out to be overblown (Wiles *et al*. 2013).

In the field of research involving communities, and on a grand scale, it has been claimed that the Connected Communities programme of research funded by UK research councils has in the decade since 2010 been doing nothing less than 'Creating a new knowledge landscape'. Collaborative research practice is presented as having the capacity 'to build new knowledge, address longstanding silences and exclusions, and pluralise the forms of knowledge used to inform common-sense understandings of the world' (Facer and McKay 2019, xii). These things are attributed to the so-called 'participatory turn' which involves people 'looking to experiment with new ways to create knowledge' (Facer and Enright 2016, 2). In formulations such as this, 'new' is necessarily contrasted favourably with 'old', or with 'traditional "academic" research' (Williamson 2007, 1), or 'academic-defined research' (Pahl and Ward 2017, 107), or work that is 'mainstream' (Kirby *et al*. 2010). Another expression of the case for collaborative research is found in the field of development studies, where the New Paradigm Research Group contrasted their approach with that of the 'old paradigm'. Their manifesto was critical of this old paradigm's 'conventional research' which was said to suffer from 'one-sided objectivity'; by contrast, it was claimed, 'new paradigm research involves a

much closer relationship than that which is usual between the researcher and the researched'. Provocatively, 'the right kind of research' was presented as superior to 'the wrong kind of research' (New Paradigm Research Group London 2011, 92-3). It is relevant to note here that this document dates back to the late 1970s. Claims that participatory research constitutes a 'new paradigm' more generally can be found in the early 1980s (Humphries *et al.* 2000, 4), and together these raise the question of just how 'new' things which are claimed to be novel turn out to be.

It is also possible to discover longer histories than claims to novelty imply in other areas. In community development, the contrast between 'traditional' and 'radical' approaches can be traced back to the United Kingdom's Community Development Projects of the 1970s (Crow *et al.* 2019). These projects employed action researchers alongside more conventional academic researchers, and their endeavours soon made apparent the challenges that result from working in this fashion. One CDP worker cautioned that it was 'simply not the case that elected representatives, administrators, project leaders, field-workers, local residents and research-oriented social scientists will necessarily see things in the same way' (Lees, 1973, 247-8). The references cited in support of the claim that '"co-produced research" draws on a long tradition of participatory and action-oriented research, inspired by radical movements concerned to democratise knowledge production' (Banks *et al.* 2019, 23) also stretch back to the 1970s; others have traced them back further still (Scott and Bromley 2013, 97). The point being made here is simply that claims to novelty may turn out to be harder to sustain once history is explored in more detail and supposedly new methods and methodologies turn out to have had a longer period in which to prove themselves (and possibly to reveal that they may also have limitations in at least some research contexts).

There is a cautionary tale associated with the fact that Ferguslie Park in Paisley, the location of the sole Community Development Project funded in Scotland in the 1970s, remains the most multiply-deprived area in the country some four decades after the original five-year intervention, which was the first of several initiatives intended to tackle local disadvantage (Crow *et al.* 2019). Based on instances such as this of research projects that achieved their objectives to only a limited extent, the risks of funders and researchers over-promising what research has the potential to deliver community partners are appropriate to bear in mind. Raising hopes about what researchers might deliver long pre-dates modern collaborative research in the field of community research (Crow 2013). Oscar Lewis's story of how during his re-study of the Mexican village of Tepoztlán he witnessed an event during which 'One dignified, elderly Tepoztecan rose and said, "Many people have come here to study us, but not one of them has helped us"' (1963, xv) serves as a telling illustration. In the light of such stories some caution regarding claims about the benefits to be brought by methodological innovation would appear prudent, if similarly disappointed expectations are to be avoided in the future. History has valuable lessons to teach those who are prepared to search for them, and one of these concerns the importance of disappointment (Craib 1994), an emotion that it is wise not to discount. Excitement about the prospects of a new approach to research may (intentionally or otherwise) foster hopes of more rapid and extensive benefits than eventually are delivered, and experience teaches us that the scale of disappointment will be proportional to preceding levels of expectations.

The danger of conflating different and better.

The previous section raised the question of just how new collaborative research turns out to be, and noted the risk associated with engendering expectations that new solutions to old problems may be in prospect. Compelling records exist of disappointment and frustration among research participants (e.g. Clark 2008), but the case for collaborative research is that it involves 'doing research differently', especially in fields where previous modes of enquiry have left them 'ripe for new

approaches to research' (Hart *et al.* 2007, 107). The field of refugee studies has been presented as an example where the participatory approach can go beyond previous research that treated people as passive objects, by operating in an inclusive way that allows them the opportunity of 'finding a voice' (Morrice *et al.*, 2007, 108; see also Temple and Moran 2006). A similar case has been made for migrants, among whom the ability of researchers using innovative creative methods such as poetry to access emotions has been found to provide 'the opportunity to articulate feelings not expressed or shared before' (Rasool 2018, 116), and this has been presented as another example of the benefits that a participatory approach can bring. Many further examples could be cited, and the more that such cases accumulate, the greater the potential exists for the implication to be drawn that doing research differently equates to doing research better.

Before the conclusion that doing research differently is an advance on previous practice can be supported, however, consideration should be given to the phenomenon of publication bias. The interests of researchers, funders, publishers and other stakeholders lean towards the reporting of success stories over reports that might in one sense or another be deemed failures; as a result, 'all the research which is published is not a representative sample of all the research that has been conducted' (Gomm 2008, 348). There is a parallel here with the finding of Norbert Elias and John Scotson's (1965) classic community study *The Established and the Outsiders* in which the image that members of a group 'have of themselves and communicate to others tends to be modelled on the "minority of the best"' and contrasted with the image that they project of others, which 'tends to be modelled on the "minority of the worst"' (1965, 159). Authors of research methods textbooks have long recognised the dangers of promoting the latest innovations at the expense of more established approaches. John Madge's mid-twentieth-century textbook expressed the pluralist wisdom in its statement that 'no single technique is adequate for the grasp of a situation of any depth and complexity' (1953, 20). Its sentiment was echoed in the recommendation that 'For most sorts of social research… more than one method must be used' (Stacey 1969: 101). The saying 'no one has a key to fit all locks' (Rodinson 1977, viii) captures neatly the rationale for consideration of a plurality of approaches and their treatment with equal respect.

This pluralist viewpoint does have exponents in the field of collaborative research. Thus, collaborative ethnography 'is not the one method for all types of ethnographic research' (Lassiter 2000, 611). Although there are 'those who dismiss collaborative research outright' and others who believe that it is 'always appropriate for all types of research' (Lassiter 2005, xi), researchers are not obliged to come down on one or other side of this stark divide. This is unsurprising when it is remembered that research has several facets to it. Researchers aspire to produce findings that are significant and that are of sufficient robustness and quality to meet the needs and stand up to the scrutiny of various audiences, through a process that is both practicable and ethical. These are high standards to live up to, and instances of research falling short of one or more of them are not limited to any single approach to research. Research that took short-cuts with regard to ethics is a domain in which 'infamous cases' (Bryman 2008b, ch.5) are particularly well-documented, with instances ranging from ethnography to experiments and all points in between. Publication bias notwithstanding, it would be easy enough to locate cases of research across the range of methodological approaches that produced only trivial findings, that did not meet its audiences' needs, that had flaws in its research design that compromised the robustness of its findings, that involved modes of analysis that cast doubt on the quality of interpretation offered, and that posed unanswerable questions. The concern being raised here is simply this: how fair is it for all cases of a phenomenon (in this case non-collaborative research) to be judged by 'the minority of the worst'?

Among research projects conducted in non-collaborative ways there have been many rightly criticised for their shortcomings. From a long list of research practices about which concerns have been raised, excluding particular groups of people from research is widely regarded as problematic because they are as a result denied a voice. For example, the exclusion of African-American residents of the city of Muncie, Indiana, by successive researchers was made all the more troubling by the success of publications about 'Middletown', yet it took many decades for this to be corrected (Lassiter 2012). Alongside non-representation stands the related problem of misrepresentation of research participants. Researchers returning to communities previously studied by others have reported local people's frustration at stereotypical misrepresentation, for example misleading impressions given of women's lives by male investigators. The complaint that 'outsiders continually get the place and its people all wrong' (Warwick and Littlejohn 1992, 33) is a familiar one (Crow 2013), although the issue did prompt one researcher to try to dampen down expectations of research outcomes by noting that 'no analysis of community life can be completely pleasing and still be honest' (Gallaher 1961, xiv). Rachel Breunlin and Helen Regis's collaborative ethnography in New Orleans echoes this sentiment about the legitimate place of critical perspectives through the statement, 'Your reader isn't going to trust you if everything is presented as perfect all the time' (2009, 139). These authors go on to suggest that the problem is tempered by participants who may feel misrepresented having rights to participate and to reply. Another common criticism made of non-collaborative research relates to ethics, and the exploitation of participants that occurs when what is taken from them by researchers happens without their informed consent. The sense of betrayal felt by the people of 'Springfield' (Vidich and Bensman 2000) remained a reference point in discussions of research ethics long after the study's original publication.

Many other cases of shortcomings associated with non-collaborative research could be cited, but it does not follow that only collaborative research is worth undertaking, remembering, and building on. Alongside the 'minority of the worst' of non-collaborative research there is also its 'minority of the best'. Thus, Ray Pahl's (1978) Sheppey research employed the method of imagined futures essay writing that bears comparison with Zanib Rasool's method of using poetry to encourage girls in Rotherham to articulate their thoughts 'about their imagined futures, about the women that they wanted to become one day and the world they wanted to live in' (2018, 121). Elias and Scotson's (1965) study provides another interesting case of what researchers outwith the collaborative approach have been able to achieve. The argument that the book advances about stigmatisation of subordinate groups has been remarkably influential, and its analysis of the emotional dimensions of group identification continue to be insightful more than half a century on from its publication, but the study was not undertaken collaboratively and its message about prejudice was an uncomfortable one for the members of the community being studied (Crow and Laidlaw 2019). Whether a more collaborative approach would have allowed the criticism to be made of community leaders who 'spoke as if the "village" were in fact, as they thought it ought to be, a harmonious, wholly united and wholly good community' (Elias and Scotson 1965, 164) must be open to doubt, not least because of the hostility experienced by other researchers whose descriptions of communities were less flattering that the dominant images that those communities' members had of themselves (Crow 2018).

One consequence of collaborative researchers 'doing research differently' is that alternative criteria for evaluating the quality of their research may be employed. Specifying what these criteria might be is a process that is only just beginning to be worked out systematically and in detail, and lags behind claims that working collaboratively 'creates better research outcomes' (Facer and Pahl 2017). As a result, a second cautionary tale may be derived from the experience of collaborative approaches involving co-production through the use of peer community researchers which led to the conclusion

that 'community involvement and "better" data are not necessarily linked'. Among the reasons for this is the point that an insider of a group being researched may have certain advantages compared to researchers who have come from outside, notably familiarity with the group and its practices, but research by insiders has its own challenges. Thus, 'peer researchers may tend to access people who they feel comfortable with', and may also 'present a positive, rather than a potentially damaging, picture of their community'. Put another way, the very human emotions of pride in one's community, and of shame about its less attractive aspects, may come into play. Recognising that research undertaken by insiders is different to that conducted by outsiders does not require that one or other is adjudged inherently superior; there are advantages and drawbacks to each, and the consequent trade-offs mean that when comparing accounts collected by community insiders and by outsiders 'one version is not necessarily "better" or more illuminating than another' (Edwards and Alexander 2011, 272, 274, 276). There may, after all, be benefits to drawing on the insights of 'individuals who are socially marginal in the society being studied' (Vidich and Bensman 2000, 354), even if the resultant accounts risk unpopularity with the more mainstream members of that society by presenting things that they would prefer not to hear (Crow 2018). Studies which 'seem so full of *nice* people' (Payne 1996, 18, emphasis in original) will be suspected of being too good to be true, whatever the methodological approach adopted. Products of wishful rather than critical thinking lack plausibility (at least in the wider world beyond 'True Believers').

The danger of advancing simplified accounts of complex issues

The moral of the previous section is that in the comparison of alternative approaches to research, collaborative and other, it is easier to distinguish the different ways of working than it is to demonstrate the inherent superiority of one over another. Because outputs from the different approaches 'are aimed at very specific audiences, which have different priorities and expectations and are likely to struggle in finding value in the other's traditions of knowledge production' (Campbell 2018, 3), questions of how far it is appropriate to adopt a critical stance, and of how much authors should aim for accessibility and relevance, remain contentious. Facer and Enright's comment about the publication of academic articles as 'another step on the treadmill of banal academic over-production' (2016, 126) indicates their personal preference, and is made in the context of noting the wide range of other forms that the 'products' of research may also take in the new knowledge landscape that they posit (see also Pahl and Ward 2017, 103). Research outputs with a clear, immediate practical application may be valued more highly by community partners than discussion of abstract, analytical 'blue skies' thinking. A related distinction may be drawn between research that is '*for* research participants' and research that is '*on* research participants' (Humphries *et al*. 2000, emphases in original), or between '"learning with" rather than doing things "on" people' (Facer and Pahl 2017, 15), and these distinctions have the potential to be re-cast as a debate about taking sides, famously framed as the deceptively-simple question asked of researchers, 'Whose side are we on?' (Becker 1967).

Analysis of the detail of Becker's article reveals several layers of complexity (Hammersley 2000, ch.3). The tradition of speaking truth to power may predispose researchers to align themselves with society's underdogs against dominant groups, but Ray Pahl's experience of being regarded by some as a champion of 'underdogs' while simultaneously being suspected by others as having been co-opted by 'the top dogs' (1977, 130) indicates an immediate complication regarding the identification of sides. It is not always clear where the dividing line between sides lies. This conundrum was familiar to the Paisley Community Development Project team in the 1970s who had to contend with the fact that, although almost all of the residents in Ferguslie Park were tenants of the local authority, the social deprivation on the estate was concentrated in particular streets and

enumeration districts. The local culture in Paisley and in other CDP areas reflected this through the distinction between the 'respectable' part of the working class that could afford the rents charged for the better-quality properties and 'the poorest families in real housing need' who could not. The CDP teams had greater sympathy for the latter, prioritising work 'with tenants who are the most stigmatised and isolated not only from the community in general but even from other council tenants'; it was amongst groups 'stigmatised by officials and other tenants' (Community Development Project 1976, 14, 10-11, 15) that need was greatest. In Paisley the CDP team had to acknowledge that 'it is unrealistic to expect instant unity in a population of around 10,000' (Paisley CDP 1978a, 3), that is, across Ferguslie Park as a whole. Communities, including disadvantaged communities, are not homogeneous, and on some issues they may comprise competing 'sides' whose interests cannot easily be reconciled.

The ethics of such issues are complicated. In Paisley, ethical arguments could be found on different sides of the issue of the allocation of local authority housing. From one standpoint, households in the greatest material need were deserving of priority, though they included people from out of the area and in consequence were regarded in some quarters as 'undeserving' (Paisley CDP 1978b, 16) of priority assistance, relative to the claims of people on the housing waiting list who had local roots and connections. Representations of a community as 'one big happy family' (Dempsey 1990, ch.4) abound, but Elias and Scotson's (1965) discovery that such rhetoric masked significant lines of social cleavage has been replicated many times over. Numerous researchers have reported experiencing ethical dilemmas relating to conflicting loyalties to different groups in a community. Elias and Scotson's response to this was to seek to keep their personal values and their research separate; as they expressed it, 'It is one thing to make confession of one's political faith, another to make a sociological enquiry' (1965, 170). Elsewhere, Elias argued for the need to curb 'the personal feelings and ideals of the researcher' (1983, 28). In doing so he followed Max Weber's view on value freedom that scientific impartiality requires clarity regarding '*where* it is no longer the reasoning scholar who speaks, but the striving human being who takes over – *where* the arguments are aimed at our reason, and *where* they appeal to our feelings' (Weber 2012, 106, emphases in original). This point of view, that researchers need to keep their emotions in check, is fundamentally at odds with the position of those collaborative researchers who are committed to 'blurring the boundaries between… academic and activist' and to 'promoting social justice and developing caring relationships' (Banks *et al*. 2013, 267, 275), and who may also regard 'engaging with emotions' (Hardill and Mills 2013, 329) as a feature that enhances research.

It is not in question that communities generate emotions of various hues, but there are divided opinions among researchers about what to do with them. Alison Gilchrist's comment that 'The emotions that flow through community networks are an important dimension of organising that has too often been denigrated, but cannot easily be dismissed from real life' acknowledges their significance. Gilchrist goes on to note that these emotions may be more or less attractive: 'Feelings of loyalty, admiration, love even, are often the driving force for many community and voluntary activities, but so too are the less positive emotions of pity, resentment, anger and fear' (2004, 45). The exploration of emotions in community settings has an instructive history, including as it does Arlie Hochschild's account of relations between members of an old-age community (comprising primarily widows) in which 'the haves experience different emotions toward the have-nots than the have-nots experience towards the haves' (1978, 59). The emotions of pity, scorn and envy figure in this analysis, as does the observation that political activists within the community were not necessarily respected by other community members, nor regarded by them as either their superiors or representative of them. This all complicated the workings of hierarchies within the community.

Many subsequent studies have revisited the profile of people who take on the role of community representatives or community leaders; they have been described as 'often self-appointed but sometimes elected' (Jan-Khan 2006, 97), and distinguished by their commitment of time, energy and emotion to community building framed as service of the common good. The community leaders in Gerd Baumann's study of Southall in west London fit this profile, although the community towards which their actions were oriented had the character of a 'useful fiction' (1996, 204), masking older lines of ethnic and other divisions in which they were necessarily implicated. Helen Regis's (2019) analysis of competing claims to belong in New Orleans (another context in which race and ethnicity figure prominently) highlights the key role of cultural activists in the politicised contestations of what it means to be 'local'. Where activists who claim to be community representatives turn out not to be representative in the sense of being typical of the wider community, but rather are unevenly connected and engaged in 'vigorous promotion of particular value positions, or advocacy of the supposed interests of particular groups' (Seale 1999, 10), uncertainties may emerge about who speaks for the community, and why. This cautionary tale suggests that in the context of research involving community activists, it remains prudent to ask of these would-be representatives who the 'we' is that they are seeking to represent, and to note that the simplified understandings that activists may hold of community interests and ethics will not necessarily be shared by ordinary community members. Although styles of collaborative working between action researchers and local communities have evolved markedly in the interim, the lessons from the Community Development Projects regarding the channelling of anger and frustration (Loney 1983) remain pertinent.

The danger of courting conflict

At this point, an interim conclusion may be drawn that there are dangers in exaggeration. The observation that 'there is a tendency in this field to over-claim the benefits of partnership working and to avoid acknowledging the complexities of the field' (Frankham, 2009, 16) may have been made a decade ago, but it is a theme that has continued to be echoed in a number of related contexts. Thus Sarah Salway and her colleagues have expressed ethical concerns about the way that community researchers 'could become indebted to local people as they sought opportunities to gain information and negotiate access' (2011, 147). Maggie O'Neill's remarks on 'the tensions and problems of participatory approaches' include the potential for 'risk of harm to communities, groups and individuals' (2012, 169) and concerns that were noted above about raising expectations that are then disappointed. Melanie Nind's (2014, ch.4) review of the field of inclusive research makes the case for the adoption of this approach but also acknowledges the existence of criticisms on several counts, questioning whether it necessarily leads to more authentic or better knowledge, is more ethical, addresses power imbalances between researchers and people being researched, is more inclusive, is empowering, and solves problems. Further, Karen Hacker's (2013, 16-19) account of community-based participatory research is similarly balanced in its account of the challenges that sit alongside the benefits of the approach, including complications concerning the time dimension of the relationship between researchers and community partners and the issue of generalizing from specific cases, which has long been recognised as a potential difficulty. Colin Robson's discussion of action research notes that formalised statements of how to proceed in this field are available but that they are no exception to the general observation about accounts of research that any such 'idealization' is 'in practice unlikely to be as neat as suggested' (2002, 216). Methodological pluralists draw the conclusion from this that a degree of humility is in order about one's own way of knowing relative to others that are available (McLennan 1995).

Not all cases made for collaborative research adopt this pluralist position. For example, one recent contribution to the debate advanced the view that 'We should fight for academia as a space in which

to co-produce'. The supporting argument is full of echoes of the paradigm wars that were fought between advocates of positions characterised more by hubris than humility, both in tone ('against', 'hostile', 'attack', 'parasitical') and content (the need to 'challenge established ways of doing things', including pursuing 'challenges to academic modes of knowledge dissemination', the need to address 'the demands of (overly-) research (ed) subjects', and to 'avoid working with groups who will not "rock the boat"') (Bell and Pahl 2018, 107-114). One of the key characteristics of the paradigm wars was the difficulty of establishing constructive dialogue, since the tone was set by the warring parties of 'intolerance, indeed bigotry, towards styles of research which do not accord with their own view of proper procedure' (Bulmer 1977, x). Rapprochement was made all the more difficult by overstatement, including likening aspects of the research process to rape which, as Ann Oakley (1998) notes, its proponents later regretted. The paradigm wars thus provide a fourth cautionary tale for collaborative researchers, illustrating what can happen when other approaches are treated merely as embodiments of wrongness, with a view to reinforcing set ideas about the correct way to proceed.

The term 'paradigm wars' is frequently associated with the prolonged period of hostilities between advocates of quantitative and qualitative research in the last part of the twentieth century, but the potential exists for paradigm disputes to occur in other contexts as well (Bryman 2008a). Colin Bell and Howard Newby characterised paradigm wars as the outcome of rival beliefs in there being 'only one style of social research with *one* method that is to be *the* method'. They contrasted this 'methodological exclusivism' with 'methodological pluralism' (1977, 10, emphases in original); others have associated paradigm wars with 'methodological closure' (McLennan 1995, 68) and 'methodological tribalism' (Lamont and Swidler 2014, 154). This history rewards researchers paying attention to it. Norman Long's assessment of the consequences of knowledge being treated as a 'battlefield' is worth quoting at length: 'It would be wrong… to expect the rise and fall of paradigms to conform to a neat "stages theory" of intellectual development whereby new conceptions and findings lead to progressively more sophisticated modes of theoretical understanding. In fact one might even turn the argument around and say that dramatic shifts in theory and paradigm often signal the introduction of new simplifying conceptions or gimmicky ideas that close off certain existing areas of inquiry in favour of new ones. While this sometimes results in stimulating new insights, it may also produce increasingly sterile and inward-looking research' (1992, 39). Advocates of the 'participatory turn' who present it as progress might usefully reflect on this warning from history.

If the key lesson of the paradigm wars is that conflicts are easier to start than they are to bring to a conclusion, then dialogue would appear to have a vital role to play in reducing the risk of hostilities. In turn, dialogue requires common ground around an agenda of things to discuss. In the discussion of collaborative researchers' relationships to more conventional approaches to research, there is already common ground in that there are several research methods in use by collaborative and other researchers alike, such as interviewing, focus groups, and ethnography. The observation that 'Ethnography has always depended, at least to some degree, on collaboration' (Campbell and Lassiter 2015, 4) could also be made about other research methods, paving the way to the identification of commonalities. Conversely, dialogue will be all the harder if common ground is denied, as it is in the claim that 'collaborative interdisciplinary work cannot be understood through the traditional prism of social science evaluation tools' and needs to be seen through 'a different lens' (Pahl and Ward 2017, 106). By challenging 'the assumption that there is one toolkit/process/method' for evaluating research, the prospect of collaborative researchers engaging in a constructive dialogue with researchers who use other approaches is made harder by the insistence on the need to value aspects of collaborative research that are unfamiliar: 'messy,

contingent on practice, uncertain, embedded in stories and histories that could be dismissed as "anecdotal", and located in events and practices that are themselves ephemeral and lacking disciplinary anchorage' (Facer and Pahl 2017, 18, 11). The paradigm wars between quantitative and qualitative research were rooted in and prolonged by oppositional thinking that denied the possibility of middle ground on which they could meet (Oakley 1998, 725), and the oppositional thinking associated with collaborative research carries with it similar risks. Reflecting on a career carved out against the background of paradigm wars, Andrew Abbott notes that oppositional thinking predisposes participants in debates 'to treat others' positions with…. contempt' (2001, 1), and argues that the open-mindedness associated with the intellectual curiosity of the eclectic has much more constructive emotions in its favour. Arguments framed in terms of a paradigm shifts continue to be made (von Scheve 2018, 52) but the idea that 'loyalty to one paradigm is both unnecessary and undesirable' (Blaikie and Priest 2017, 9) has come to be more widely-held among methodologists.

Conclusion

The paradigm wars did not conclude with the death or surrender of one of the contending parties, and there is little reason to believe that such an outcome is any more likely for other epistemological assaults that may be underway. Alan Bryman's use of the term 'détente' (2008a, 23) points to the acceptance of the methodological pluralist position, as does Malcolm Williams and W Paul Vogt's term 'methodological ecumenism', used in their discussion of how the 'fundamental quarrel about how we can know the social world' (2011, 4-6) may have lost some of its former intensity but nevertheless retains the potential to re-emerge. The term 'ecumenism' is significant for its religious connotations, and reminds us that to some extent the positions we adopt about the relative merits of different types of knowledge are matters of faith. As Marja Alastalo has noted about research methods histories, partisan 'origin myths' (2008, 27) should be treated with caution. Collaborative research is a broad church, but within it much depends on action researchers' claims that this approach leads 'to "better" research because the practical and theoretical outcomes of the research are grounded in the perspective and interests of those immediately concerned, and not filtered through an outside researcher's preconceptions and interests', and on their related claims that they are in tune with the move beyond modernism to postmodernism and the related move from positivism to 'a participatory worldview' (Reason and Bradbury 2001, 4). These remain claims that have not been demonstrated, at least not to everyone's satisfaction. Rather, the case is being advanced by people who are 'advocates for a new knowledge landscape' and 'actively committed to fighting for what they see as more inclusive and democratic research practices' (Facer and Enright 2016, 50). That is to say, the debate is being driven by values relating to research procedures as much as it is by evidence of outcomes. As a result, considerable doubt has been cast on claims that 'participatory methods are the gold standard' (Hammersley 2017, 113) for researchers; a less exaggerated proposition would be more sustainable.

This paper has put the case for caution when advancing arguments for collaborative research. It has noted that there are dangers in exaggeration. Four areas on concern were identified. First, there are risks in exaggerating the novelty of collaborative research, for although there are developments taking place all of the time, the fundamental principles have been appreciated for some time and knowledge of the history of debate about them has the potential to be instructive. Secondly, there are difficulties associated with the conflation of different and better; collaborative research may operate according to different principles, but demonstration of its superiority to other approaches remains, at best, a work in progress. Thirdly, the exaggeration involved in oversimplification brings with it the possibility that ethical and practical challenges will not be appreciated; the issue of who

speaks for communities was discussed as an example. And fourthly, exaggerated language framing debates about alternative approaches to research in terms of conflict has the potential to polarise opinion and make it harder to find common ground on which constructive dialogue might be fostered. For the generation of researchers too young to have had first-hand experience of the intensity of the paradigm wars it would be prudent to heed the advice of Michèle Lamont and Ann Swidler that 'building bridges, and not digging deep moats, is the key to fostering a strong field', and that engaging in 'sectarian methodological fights' (2014, 167, 153) has little to recommend it.

The presence of strong emotions may make such advice easier to give than to receive. Collaborative researchers are motivated by a desire to change the world in ways that matter to people (Halldórsson 2017) through the pursuit of 'opportunities that range from filling in historical and representational gaps, to redressing past wrongs, to developing new collaboratively based activisms'; it is unsurprising to find their 'ethical and moral commitments' (Campbell *et al*. 2018, 97) associated with emotions such as hope, pride, anger and scorn. These are, moreover, emotions that are active in 'collaborative encounters' (Campbell 2017, 568) between people conscious of their differences as well as their commonalities, a context in which participants' 'relationships, histories, ethical and moral choices and commitments, personal sensibilities and aspirations, ideas and assumptions inevitably collide'. In the Middletown project which informed this comment there were 'well over 75 people' involved in a variety of capacities and their relationships were characterised by both 'affinity, rapport or agreement' and 'discord, resistance, or antagonism' (Campbell *et al*. 2018, 98, 95). The force and momentum generated in such contexts may be understood as affect (White 2017) and although the precise ways in which such affective dynamics play out will vary from case to case, there will be scope for drawing broader conclusions through comparison of cases that have common denominators (Haldórsson 2017). Kate Hardy's (2012) research project on sex work with two collaborating organizations proceeded well with one but saw the other discontinue involvement, an outcome she attributes to the spatial expressions of collective emotions in organizations. The routes by which individuals leave collaborative projects have also been noted (for example Breunlin and Regis 2009, 132; Campbell *et al*. 2018, 103), and in the micropolitics of such processes there will be commonalities and uniqueness to be explored regarding what may be a fine line between success and failure. A conversation between collaborative and other researchers about such issues promises to be more constructive than hostilities, and is more in keeping with collaborative researchers' commitment to dialogue.

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<u>Key messages</u>

- Collaborative research is distinctive from but not necessarily superior to other research approaches
- Arguments for collaborative research are sometimes overstated and this risks unproductive conflicts
- Acknowledging the role of emotions in research is quite compatible with methodological pluralism