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Collaborative Research and the Emotions of *Over*statement: A Reply to Crow

In a recent article in this journal ('Collaborative Research and the Emotions of *Over*statement') Graham Crow argued that arguments for collaborative research were often overstated and sought to warn those who would adopt this position by offering 'four cautionary tales'. These focused upon (i) paying insufficient attention to history, (ii) conflating 'different' with 'better', (iii) over-simplifying social complexity and (iv) courting conflict and were themselves loosely tied to an emotional construct concerning hope and disappointment, pride and shame. 'A conversation between collaborative and other researchers about such issues' Crow concludes 'promises to be more constructive than hostilities, and is more in keeping with collaborative researchers' commitment to dialogue'. This response accepts Crow's entreaty to continue the conversation. First, it suggests that he (paradoxically) falls into the trap of his own making by *over*stating the extent to which collaborative scholars have sought to make the case for methodological supremacy in quite the manner he suggests. As such, the 'four cautionary tales' that Crow offers would not only be widely accepted as valid by the vast majority of collaborative scholars but are also of generic relevance across the full spectrum of social scientific methods. To seek to place such 'tales' so explicitly at the door of collaborative scholars is therefore arguably unfair or, at best, to make a curious case that remains largely unproven. Indeed, it is very difficult to read the piece without coming to the conclusion that Crow's focus is less upon the sphere of collaborative research and methods *per se* and more about the work of a specific group of scholars and a specific research programme.

Nevertheless, no matter how carefully constructed, strawmen (whether of the theoretical, empirical or common farmyard variety) are generally relatively easy to knock-down and having done this the second part of this response uses Crow's article as a launch-pad to identify four far broader and more fundamental issues that concern the interplay *between* emotions and collaborative research. It achieves this by locating this relationship within the shifting contemporary context through a multi-level lens which arguably offers a far more sophisticated and vibrant account of not only the need for collaborative modes of research (conducted 'with' not 'on') but also of the changing emotional pressures facing social scientists who seek to exist at the nexus or interface between academe, society and social change. As such, the second part of this article offers 'four cautionary tales' of its own that are not only very different to those raised by Crow but that are also generally *under*stated in existing debates (both professional and intellectual).

I. A CAUTIONARY TALE OF *OVER*STATING

The section evaluates Crow's core thesis concerning collaborative research in order to construct a reference point from which to open-up a far broader conversation about the interplay between emotions and collaborative research in the second section. My argument is that in seeking to expose what are perceived to be the narrow and unrealistic expectations associated with collaborative research Crow unwittingly slips into a trap of his own making by overstating the nature, dominance and relevance of those claims. 'Research methods and methodology is a well populated field in which proponents of an array of different approaches vie for the attention of researchers' Crow correctly notes. As a political scientist I am more than a little aware of the existence of deep rivalries, tensions and the destructive capability of methodological schisms. Putting the same point slightly differently, political science has always been 'a divided discipline' with various sects existing in different schools and sitting at different tables, 'each with its own conception of proper political science, but each protecting some secret island of vulnerability' as Gabriel Almond famously noted in (1988, see also 1990). This history of political science is a history of intense and ultimately futile contestation between 'hard' and 'soft', 'quants' versus 'qual', 'objectivity' against 'ideology', etc. Although more heterogeneous than is commonly recognised (see Eulau, 1963; 1968), the emergence of a behavioural movement led by scholars including David Easton (1953), David Truman (1955) and Robert Dahl (1961) in the middle of the twentieth century attacked the perceived backwardness of the discipline and was underpinned by the promotion of a very specific view of what 'excellence in research' looked like.

The 'behavioural-turn' was '[H]istorically speaking' Robert Dahl acknowledged 'was a protest movement within political science' (1961, p.766) that claimed new and innovative set of methods had the potential to break new ground. But although proponents argued in favour of a paradigmatic shift in the nature of the discipline not everyone was convinced. Works including Bernard Crick's *The American Science of Politics* (1959) sought to challenge many of the dominant assumptions that were too often veiled within exaggerated or exclusive claims for this new political science; an endeavour similarly undertaken in the same year, albeit with a more caustic intellectual tone and working across a more expansive disciplinary landscape, by C. Wright Mills' in his *The Sociological Imagination* (1959).

The subsequent disciplinary battles – whether the debates around 'real-world' or 'post-autistic' economics, the 'public sociology wars', or the 'Perestroika movement' in political science to mention just a few – have all revolved around the contestation of what constitutes credible 'science'. Different sects and tribes have attempted to promote and protect the innate value of 'their' methodological idiom against the competing intellectual demands of those who would advocate a very different perspective; or argue in favour of diversity and pluralism, and as part of this may have attempted to turn 'the methodological pyramid' on its side. The internal politics of academic life is therefore very often dominated by methodological squabbles that involve a variety of claims and counter-claims which are essentially political disputes concerning the distribution of resources (such as funding, appointments, prizes but also including notions of esteem, prestige and credibility).

This is not a new or particularly original argument. But it does help contextualise Crow's thesis concerning collaborative research. In essence, he suggests that collaborative researchers have adopted a somewhat triumphalist tone which seeks to highlight the distinctive benefits of inclusive and participatory methods while tacitly 'othering' all other approaches as by definition inferior. Exaggeration, hubris and the dangers of *over-promising* but *under-supplying* are linked by Crow (via. Becker, 1971) to the quasi-religious and emotionally-laden pathologies of 'proselytising', 'preaching', 'self-righteous assurance' and even 'salvation'. Heaven help those social scientists who promote 'one right way' (Becker, 1986). The emotional dimension therefore operates at, at least, two levels. There is an epistemological dimension which revolves around issues of objectivity and bias, and which flows into questions concerning whether it is either possible or desirable to remove emotions or feelings, values or normative ideals from the research process (i.e. often signalled through the language of 'science' as opposed to 'studies'). There is also a second related but also quite separate emotional dimension which is more professional in nature and simply highlights the emotional investment that many scholars inevitably invest in their research (i.e. the *emotional labour* involved in research practice). This is a rarely acknowledged dimension which may add tone and texture to Crow's arguments. The point I am trying to make is that the emotional investment made by a scholar in a specific approach can lead to a situation whereby 'to attack *the method* is to attack *the scholar* which is to attack *the individual*';' or (conversely) to promote '*your* method' or approach is to directly or indirectly do '*our* approach' or method down. This muddying of the waters between what might be termed 'the personal and the methodological' can often make it difficult to separate legitimate ontological, epistemological or technical questions or concerns from more dubious *ad hominem* critiques.

Such emotive hermeneutic questions are clearly problematic but they matter because Crow arguably fails to offer a critique of collaborative research *per se* but instead focuses almost exclusively on a very specific sub-strand of this broad field of scholarship, and one that I would suggest is quite exceptional (for a review of the breadth of the field see Olson and Jason, 2015). Even the most cursory review of the references used to identify Crow's methodological miscreants highlights the way in which he reads 'collaborative research' as for some reason almost synonymous with the work of Keri Facer, George McKay and Kate Pahl (Facer and Pahl, 2017; Facer and McKay, 2019; Facer and Enright, 2019; Pahl and Ward, 2017). As such, the narrowness of the reference profile suggests that Crow's critique is not really with 'collaborative research' in the broad and mainstream sense of the term (discussed below) but of the research emanating from the 'Connected Communities' research programme which ran in the UK from 2014-2019. Led by the Arts and Humanities Research Council but co-funded by a range of organisations this project sought to understand the changing nature of communities in their contexts, and the role of communities in sustaining and enhancing the quality of life. As the Director of Research at the AHRC underlined at the time, the programme was designed to be unique in terms of testing innovations in the co-design of research and closing the traditional distance between academic research and community engagement (AHRC, 2014). It

also had social as well as scholarly ambitions which, in turn, reflected the co-funded nature of the programme and were reflected in its core aim:

> To mobilise the potential for increasingly inter-connected, culturally diverse, communities to enhance participation, prosperity, sustainability, health & well-being by better connecting research, stakeholders and communities.

Mobilising communities and enhancing participation are explicitly political ambitions and there will always be risks 'associated with engendering expectations that new solutions to old problems may be in prospect' as Crow suggests. However, this was intended to be uniquely risky research of a very specific manner (i.e. embedded, immersive, participatory action-research) which Facer and Pahl underlined was *not* offered as providing 'one new truth, one new method, to replace all others' (Facer and Pahl, 2017, 1-3). It therefore seems somewhat odd to locate this body of work at the core of a thesis regarding 'the emotions of *over*-statement' and leaves Crow suggesting rather tenuously that,

> [T]hey 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 [emphasis added].

'Guilt by implication' is always a tricky charge to uphold but there is little doubt that Kate Pahl and other members of the 'Connected Communities' team were willing to make a very personal and undoubtedly emotional investment in the quality and ambitions of their work which, in turn, leads to the selection of methodologies which from a mainstream perspective might appear as unbounded as they do messy. In this regard they do reject the ideology of the rational, detached and objective researcher' (Loughran and Mannay, 2018a). However, in the twenty-first century my sense is that the vast majority of social scientists also reject this ideology in its purest form for the simple reason that the entanglements of emotions can never be denied. What Facer, Pahl and others have arguably decided to do is to expose and engage with those emotional entanglements as a central feature of their research. Instead of seeking to *depoliticize* their research through a *faux* attempt to deny the existence of underpinning values and emotions they seek to explicitly *politicise* their scholarship in terms of process, affect and outcomes. Once again, the history of political science provides a useful vantage point. In the United States the dominance of behaviourism in the 1950s and 1960s led to widespread concerns not just about its claimed emotional objectivity and its perceived hegemony within the discipline but also about the manner in which it demanded that a clear separation should be maintained between political science, on the one hand, and political activism, on the other. The creation of the Caucus for a New Political Science in 1967 challenged this stance and encouraged methodological pluralism and political activism within the discipline (see Barrow, 2008). Social science without some form of progressive social ambition was not an attractive prospect for many scholars and the official mission of the Caucus was 'to make the study of politics relevant to the struggle for a better world' and in many ways the more recent 'Perestroika movement' was simply the latest instalment of the ongoing ebb-and-flow in the battle about the most scientifically credible and socially valuable way of studying socio-political phenomena (Renwick Monroe, 2005). The notion of 'ebb-and-flow' brings us back to Crow's focus on *over*statement (and therefore *under*statement) and his 'four cautionary tales'.

Tale 1: The Danger of Paying Insufficient Attention to History

Can there be any credible social scientists that would disagree with the simple adage that 'history matters'? The suggestion that any scholar or research project needs to be warned about 'the risks of amnesia' – especially when working in an embedded community context – is tantamount to an accusation that lies somewhere between amateurism and incompetence. The fact that Ray Pahl's (1984) scholarship is explicitly discussed as an exemplar of someone who understood how history casts a shadow over communities adds a strange emotional twist to the critique. This is because Crow proceeds to highlight the 'Connected Communities' programme, in which Kate Pahl (i.e. Ray's daughter) was a leading figure, almost as an exemplar of a project that 'exaggerated claims to novelty' to the detriment historical awareness. Claims that this programme played a role in 'creating a new knowledge landscape' or that it developed a capacity 'to build new knowledge, addressing silences and exclusions, and pluralise the forms of knowledge used to

inform common-sense understandings of the world' (Facer and McKay, 2019, xii) is critiqued on the basis that the claims of collaborative or participatory methods being 'new' (or 'better') than 'older' (or more 'established') approaches is contestable. '[C]laims to novelty may turn out to be harder to sustain once history is explored in more detail' Crow suggests '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 of the research contexts).'

There are, however, both internal and external dimensions to how any social research project might seek to engage with historical factors. What might be termed the *external dimension* refers to the degree to which any study seeks to locate itself within an appreciation of the broader and longstanding seam of scholarship of which it now forms an element; the *internal dimension*, by contrast, revolves around the degree to which the basic aims and ambitions of a project are themselves concerned with facilitating a deeper understanding of why history matters. It would therefore be possible for a world-class collaborative project to excel in relation to the *internal* dimension without necessarily or explicitly spending a huge amount of time seeking to wrap itself in a cloak of credibility through endless references to previous studies. Take, for example, Cathrine Thorleifsson's (2016) ethnographic work in the South Yorkshire town of Doncaster and the manner in which it revealed why the history of a former railway town in a coal mining region mattered. It mattered because a populist party, in this case UKIP, was able to tap into the town's 'deep story' by promising to reinvest in the extractive industries. The emotional links between history, hope and nostalgia were revealed through the stories local people told but the quality and relevance of this research was not affected by its failure to acknowledge the collected works of previous generations of ethnographers. As Sara Ahmed (2017) has argued, to construct an argument through the citations of previous scholars also risks seriously delimiting the boundaries of any discussion, which in turn, suggests that when it comes to learning from the past through a scholarly lens there is also a danger of paying *too much attention to history*.

With this in mind it becomes necessary to assess the degree to which a detailed review of both the *internal* and *external* dimensions of the 'Connected Communities' programme actually validates Crow's implied critique concerning 'historical amnesia'. From an internal perspective the critique seems weak for the simple reason that a large number of projects within this programme were explicitly concerned with revealing and understanding why history matters by utilising participatory tools and collaborative methods to allow specific and generally hidden communities to find their voice. In this sense a core objective of the programme was to ensure that sufficient attention *was* paid to the historical narratives and interpretations held by different sections of society (in order 'to uncover knowledge and imagine better futures') (see, for example, Graham and Vergunst, 2019; Campbell, Pahl, Pente and Rasool, 2018). The omission by Crow of any apparent awareness of the leading role played by the noted historian Paul Ward in the programme – with analyses entitled 'How can historical knowledge help us to make sense of communities like Rotherham (2018, with Elizabeth Pente) – is a major oversight. From an external perspective it is also true that the books arising out of the programme are generally more engaged with the intellectual heritage *vis-à-vis* collaborative research and 'do not lose sight of what has gone before' in quite the way that Crow suggests. The projects that Sarah Banks led, for example, within the 'Connected Communities' programme revolved around revisiting the Community Development projects of the 1970s (see Banks, Hart, Pahl and Ward, 2018). It's also possible to suggest that the claims to novelty and newness that were actually being made by the 'Connected Communities' programme were not in fact about *collaborative research or community engagement being new* but were actually more concerned with the cultivation of new participatory methods or tools, with that research being conducted within a social context that was to some extent inevitably 'new' and, as such, potentially created 'new' opportunities. To link this programme with the danger of failing to pay sufficient attention to history is therefore possibly something of an *over*-statement. Which brings the discussion to the second cautionary tale.

Tale 2: The Danger of Conflating 'Different' with 'Better'

The British sociologist Michael Young, later Lord Young of Dartington, was an exceptional person. A great believer in social reform, he was responsible for drafting the Labour Party's manifesto for the 1945 General Election and he went on to found or help establish a number of organisations - including the Open University, the Consumer's Association, the National Extension College, the Citizens Advice Bureau

network, the Open College of Arts, Language Line, the Social Science Research Council, and the University of the Third Age – which all shared a focus on social inclusion, active citizenship and community participation. In 1953 Young established the Institute for Community Studies (ICS) in the East London district of Bethnal Green with the stated purpose of examining the interaction of the family, the community and the social services. As such, the ICS sought to understand how ordinary men and women were interacting with the new expanded social service sector and whether the tentacles of the state operating in cooperation or conflict with established patterns of family support and mutual aid? In 1957 Young published *Family and Kinship in East London* (co-authored with Peter Wilmott) which engaged with this question through a focus on the transfer of large working class communities from the East End to a new housing estate in Essex called Debden. They suggested that extended family ties and kinship networks provided an essential support structure for the residents of the East End and that those who had moved away were more likely to experience a decline in their quality of life due to elements of social and cultural isolation. Long before the concept became popular Young and Wilmott had essentially focused attention on the significance of social capital. Dense networks of mutual support, rooted in the notion of the family and the neighbourhood provided individuals with a sense of shared identity and sources of practical day-to-day support, Young and Wilmott argued, that tended to be lost as individuals and families were 'uprooted' and moved to Essex.

The reason for offering this brief account of Michael Young and the work of the ICS in the 1950s is because it provides something of a link or bridge between Crow's first 'cautionary tale' about not forgetting the past and his second warning that 'different' is not always 'better'. More importantly, however, it also provides a powerful example of *not* forgetting the past and *why* 'doing research differently' may, from some perspectives, equate with 'doing it better'.

The 'different' element of *Family and Kinship in East London* was the manner in which it was rooted in a critique of the dominant methods of social research at the time. This is a book that has been acknowledged as one of the most influential sociological studies of the twentieth century and a large part of that intellectual heritage is based upon its use of deep, immersive, ethnographic and participatory methods. These methods were in stark contrast to the highly statistical and 'scientific' approaches that were dominant in the 1950s (and which were themselves linked to the availability of new mass social data sets and technological advances). The *raison d'être* of the ICS was that it undertook social research '*with'* local communities not '*on'* local communities and through this developed a deep understanding of the aspirations, worries, doubts and insecurities held be different people, families and communities. It was therefore both an intellectual and political project but its methodological originality rested on the use of collaborative research methods and participatory techniques. In this regard it might be relevant to Crow's thesis that none of the ICS's initial researchers (i.e. Michael Young, Peter Wilmott, Peter Townsend) were at the time the Institute was established experienced academics. They were 'outsiders' or as Lise Butler suggests 'a collection of amateurs with cavalier attitudes towards academic sociology' (2015, 213). Nevertheless, the work of the ICS was 'different' in the sense that it worked beyond the social scientific mainstream at the time, and it offered what might be described as a 'better' account of the politics of kinship due to the manner in which it exposed *the emotional foundations* of working class life and their day-to-day manifestation in the existence and role of extended families. It was for this reason that the *Family and Kinship in East London* was republished fourteen times between 1961 and 1980 and has influenced several generations of town planners, sociologists and social historians.

This 'historical' element arises from the fact that in 2019 a new Institute for Community Studies was established by the Young Foundation to 'bridge the chasm between the voices of people and those in power. To shift the rhetoric of listening towards genuine forms of engaging and co-creating; and to bring the sharp insights and knowledge from communities in to where they can be heard in their own words, in robust and real ways.' Crow may well remind those that promote the 'new' ICS that these stated ambitions echo those that were set out 65 years ago and that this history should not be forgotten. Nevertheless, acknowledging the past need not limit bold ambitions for the future. The institute seeks to 'build on the long track record of deep insights and detail-driven research with communities started by Michael Young in the 1950s'. Yet there is a clear sense within the institute's ambitions of the need for doing things differently. 'Our mission is to build bridges between communities and institutions' its website states 'finding *new* ways in which academics and communities can work together, and by doing so, bring together *very*

different kinds of knowledge [emphasis added]'. The new ICS is arguably one of the most significant long-term investments in research infrastructure around collaborative methods and social change to occur in the UK for several decades but plans and statements lack the triumphalist and exaggerated tones that Crow seeks to link collaborative endeavours.

In this regard Crow seeks to draw upon Norbert Elias and John Scotson's classic community study *The Established and the Outsiders* (1965, p.159) by suggesting that collaborative scholars constantly self-define as 'the minority of the best' while defining all other approaches through a focus on 'the minority of the worst'. This is a fascinating argument but its strength is to some extent weakened by Crow's own admission that the '[methodologically] pluralist viewpoint does have exponents in the field of collaborative research'. The specific studies he cites in support of his argument are also underwhelming and lead him to make the rather unconvincing claim that '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 [emphasis added]'. As already argued, 'guilt by implication' is never a firm foundation on which to launch a rather strong intellectual and professional critique in the manner Crow seeks to promote. Without the great weight of these 'further examples' being provided the reader is arguably left with the distinct feeling that it is actually Crow who may be exaggerating the case that 'different is better' rather than the collaborative scholars to whom he seeks to make the accusation stick. My strong sense is that the vast majority of collaborative research is located within an epistemological argument that 'different *is* different' rather than 'different *is* better'. Take, for example, the ICS' *Looking Local* report of November 2019 and the manner in which researchers working with local communities through 'deep' participatory and discursive methods revealed a very different set of key social concerns to those that national polling companies were producing through 'thin' survey methods.

Crow's argument that although non-collaborative methods are not perfect 'it does not follow that only collaborative research is worth undertaking, remembering and building on' appears to be an argument that any credible social scientist would accept (and collaborative scholars would also gladly concede that their methods are far from perfect). There are two other slightly odd elements to Crow's thesis. The first is that he highlights the work of Ray Pahl in Sheppey (1978) and Zanib Rasool in Rotherham (2018) as existing within the 'minority of the best' of *non*-collaborative research which is odd as both studies utilise collaborative approaches and participatory tools. The former was traditional old-fashioned deep ethnography that involved Pahl living on the largely boggy and barren Isle of Sheppey off the Kent coast; Rasool's work could hardly have been more collaborative given that it involved working with a poet and groups of local people. The second odd element of Crow's argument is that despite the fact that Rasool's work was part of the broader 'Connected Communities' programme Crow returns to a critique of this initiative and to a very specific comment by Keri Facer and Kate Pahl (2017) that working collaboratively 'creates better research outcomes'. On its own this simple four-word statement would seem to place Facer and Pahl very much within the sphere of over-statement and possibly even methodological supremacy that Crow seeks to critique. The full sentence in which these four words appear, however, is more a statement of fact than over-statement or hyperbole: 'Universities are increasingly being asked to take an active role as research collaborators with citizens, public bodies and community organisations which, *it is claimed*, makes them more accountable, *creates better research outcomes* and enhances the knowledge base [emphasis added].' Facer and Pahl are in this sentence contextualising a debate about valuing inter-disciplinary collaborative research and in doing so are highlighting the increasing emphasis that research funders are placing on modes of co-production and co-design with potential research users. The manner in which they insert the phrase 'it is claimed' into this statement before listing the rationale on which this shift has undoubtedly been based hardly seems to fit with concerns regarding *over*-statement. Facer and Pahl are not themselves claiming anything but stating what has *been claimed* by others; Crow, on the other hand, risks offering a simplified account of complex issues which brings us neatly to the next cautionary tale.

Tale 3: The Danger of Advancing Simplified Accounts of Complex Issues

'The moral of the previous section' Crow states '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'. There is very little on which to question this statement for the simple reason that very few scholars would want to throw themselves into the methodological maelstrom that would come with seeking to demonstrate 'the inherent superiority of one over another', (as noted above, 'different *is* different, different *is not* better'.) Crow (correctly) notes that outputs from different approaches can be aimed at very specific audiences and that questions concerning how appropriate it is to adopt a critical stance, and how much authors should aim for accessibility and relevance 'remain contentious'. It is also true that approaches to scholarship that exist beyond the mainstream may well struggle in finding value in other's tradition of knowledge production. Although this is the specific focus of Facer and Pahl's (2019) *Valuing Inter-Disciplinary Collaborative Research*, Crow immediately focuses his critique on the work of Facer and Enright (2016, 126) and their comment about the publication of academic articles as 'another step on the treadmill of banal academic over-production' before placing this in the context of other outputs from the 'Connected Communities' programme (specifically Pahl and Ward, 2017).

The slightly curious element of Crow's article is that he then proceeds to highlight that research outputs with immediate practical application may be valued more by community partners, just as more academic outputs are likely to be more valued by scholars; just as distinction may be drawn between research that is '*for* research participants' and research that is '*on* research participants'; or as Facer and Pahl (2017, 15) have previously acknowledged between '"learning with' rather than doing things 'on' people". Highlighting these valid and well-known distinctions is not in itself distinctive but Crow's subsequent statement that "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 you on?' (Becker, 1967)." Why does Crow need to highlight the potential of fairly conventional methodological distinctions to be 're-cast as a debate about taking sides?' Having written extensively on issues including 'the tyranny of impact' (see, for example, Flinders, 2013a), the politics of engaged scholarship (see, for example, Flinders 2013b) the moral foundations of public engagement (see, for example, Flinders 2019), 'the politics of co-production' (see, for example, Flinders, Wood and Cunningham, 2015) and most recently on the emotional underpinnings of the growth of populism (see, for example, Flinders , 2019) By adopting this 'whose side are you on?' approach it is Crow that is in danger of advancing a simplified account of a complex issue. Social and political scientists must learn the skills, or as C. Wright Mills (1959) might say 'the craft', of navigating a series of often blurred, over-lapping and shifting socio-political boundaries. 'The ethics of such issues' are as Crow notes 'complicated' but the cautionary tale he offers to collaborative scholars regarding the need to keep emotions in check or that 'it remains prudent to ask of these would-be [community] representatives who the 'we' is that they are seeking to represent' seems as banal as it is obvious.

If, at base, Crow's article represents a critique of collaborative scholarship on the basis that it too often advances simplified accounts of complex issues then what it lacks is a clear and credible evidence base to support this argument. In fact, two issues appear highly problematic; the first from an intellectual perspective, the second from a professional perspective. The first issue is that each of Crow's cautionary tales seeks to impose a fairly simple binary dynamic which generally fails when stress-tested against either the existing research base or the simple complexity of the social phenomena under analysis. Sufficient *or* insufficient attention is paid to history, it's different *or* better, simplicity *or* complexity, insiders *or* outsiders, 'minority of the worst' *or* 'minority of the best'; all generally viewed in oppositional terms and as representing mono-dimensional zero-sum games that offer very little in terms of developing the analytical traction of each of these lenses. The irony, as has already been stated, is that this in itself represents an example of both *over*-statement alongside the advancement of a simplified account of a set of complex issues which is exactly the critique that Crow seeks to lay at the door of collaborative researchers. In many ways he does not seek to critique the full sphere of collaborative scholarship but a very specific sub-set of scholars that played a role in an explicitly innovative, risky and socially-orientated programme of work that was always intended to exist at the nexus or intersection between academe and society. Exactly why the 'Connected Communities' programme provides the lightning-rod for so much attempted critique is never really clear; to the extent that there is a sense that an unspoken emotional undercurrent lurks beneath the surface of the text. For example, Crow mentions Facer and Enright's (2016, 126) comment about 'the treadmill of academic over-production' but not the fact that given the pressures of the UK's Research Excellence Framework similar comments have been made by scholars from a vast range of disciplines and methodological standpoints (see, for example, Sayer 2015). By forging a very broad and not unproblematic

critique and then consistently relating it to one specific piece of research Crow appears almost captured by a desire to 'court conflict'. A point that brings us neatly (but ironically) to his final cautionary tale.

Tale 4: The Danger of Courting Conflict

'At this point' Crow notes as he introduces his fourth and final cautionary tale 'an interim conclusion may be drawn that there are dangers in exaggeration'. This is a conclusion that the vast majority of readers of his article are by this point likely to have already come to, but possibly not in quite the way Crow predicts. Once again and somewhat working against the tide of his own argument Crow proceeds to underline that a significant number of scholars who work within a broadly collaborative idiom have been very open and explicit about the risks that such approaches inevitably bring with them. These include ethical concerns, the risk of harm to communities or individuals, the creation of unrealistic expectations, the existence of power imbalances, the need to constantly (re)negotiate relationships, the possibility of conflict over timescales, processes or outputs, the potential for competing agendas to come into tension, and even the creation of almost parasitical co-dependency relationships that can, on occasion, become pathological to all parties (citing Salway, 2011; O'Neill, 2012; Nind, 2014; Hacker, 2013; Robson, 2002). What Crow's review suggests is the existence of a range of balanced accounts that highlight the benefits alongside the challenges of collaborative research undertaken with the community through participatory methods. No one is suggesting that collaborative research offers a panacea to the challenges of contemporary social or political analysis which leads Crow to draw-upon McLennan (1995) and state 'a degree of humility is in order about one's own way of knowing relative to others that are available'. A cautionary tale no doubt but hardly a new or distinctive revelation.

The sting in the tale, however, emerges when Crow suggests that not all scholars have shown the required degree of humility. Trumpets have been blown, bold statements have been made and some scholars have dared to make the case for collaborative research within far broader debates concerning (*inter alia*) the social role of scholarship, the marketization and commodification of academe and the essential necessity of creative thinking, questioning and criticality within higher education. The fact that all three of these issues form central components of what C. Wright Mills sought to both capture and promote in his notion of 'the sociological imagination' deserves to be recognised. With apparent affront, Crow notes that '[n]ot all cases made for collaborative research adopt this pluralist position' and he returns to a (by now common) target when seeking to set this particular hook. "For example, one recent contribution to the debate' he notes 'advanced the view that 'We should fight for academia as a space in which to co-produce"' as he once again focuses the critique upon the 'Connected Communities' programme and again on Kate Pahl (2018, with David Bell). Crow notes,

> 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"').

Crow's interpretation of Pahl and Bell's arguments lead him to suggest that they are not just in danger of courting conflict but of possibly even sparking a new set of 'paradigm wars'. The rationale for this is that a key characteristic of these 'wars' was the 'intolerance, indeed bigotry, towards styles of research which do not accord with their own view of proper procedure' (Crow citing Bulmer, 1977, x). The implicit implication appears to be that 'courting conflict' is for some reason a 'bad thing' which somehow appears almost as quaint as it is naïve when assessed against the real-world grinding and grating, struggles and skirmishes that define intellectual debate and indeed progress. To seek to associate a specific piece of scholarship with allusions of 'intolerance' and 'bigotry' is a far more serious statement to make, and possibly a major *over*-statement that brings with it a danger of creating unnecessary conflict. The problem for Crow is that his presentation and interpretation of Pahl and Bell's specific 2018 article which he uses as the hook on which to hang his argument could in itself provide the basis of its own 'cautionary tale'. This is because, in essence, Crow decontextualizes Pahl and Bell's arguments in a way that leaves them devoid of tone and texture and therefore susceptible to what could be seen as a slightly unfair critique. More specifically, Crow seeks to

locate the combative style and language of Pahl and Bell's as symptomatic of previous 'paradigm wars' without at the same time acknowledging that their arguments were, in fact, based within an explicitly radical and utopian account of co-production that offered a highly distinctive interpretation of change to the extent that it is positioned "'within, against and beyond current configurations of power in academia and society more broadly"' (2018, 105).

Nevertheless, even within this fundamental assessment of modes of co-production there remains a clear sense of balance, '[o]ur focus is primarily on the potential (*and dangers*) of co-production's utopian power to transform academia [emphasis added] (2018, 105).' The style of the specific piece Crow cites to support his position is explicitly and intentionally agonistic, as befits a provocative analysis from a utopian perspective. The boldness of the arguments and the combative style of writing is therefore unsurprising in a piece that seeks to explore the potential of co-production to 'destabilize' both academia and capitalism. What is interesting given Crow's purported focus on the link between collaborative research and the emotions of overstatement is that Pahl and Bell's work draws-upon Ernst Bloch's *The Principle of Hope* (1986) in order to tease-apart the relationships between a variety of emotions in the context of co-production. The omission of any mention of the existence or value of such insights is an odd oversight from Crow's article but it should not be used to distract attention from two more fundamental shortcomings. The first is that Crow offers no real explanation for why collaborative scholars should avoid 'courting conflict'. Intellectual and social progress is dependent upon the existence of debate, argument and contestation; and even though collaborative scholars may seek to broaden the main currency within academe away from a focus on peer-reviewed publications they do not deny the existence of a middle-ground that adopts a more varied approach to 'what counts' as valid research outputs. Following on from this, the second shortcoming focuses on the lack of evidence that Crow is able to coral around the arguments he appears so committed to making. Pahl and Bell's 2018 article cannot sensibly be presented as the basis of an argument that seeks to suggest that such radical approaches are at all common within the field of collaborative scholarship, in general, or within Kate Pahl's extensive body of writing, in particular. This is not to deny the passion with which some scholars advocate the use of collaborative methods but simply to temper Crow's position and suggest that his own position appears somewhat infused with the emotions of *over*-statement.

II. A CAUTIONARY TALE OF *UNDER*STATING

There is no doubt that Crow's 'four cautionary tales' offer significant insight and guidance to all social and political scientists. What is, however, missing is any clear rationale for why he feels these warnings are particularly appropriate for those who favour collaborative approaches and participatory methods. The clear and demonstrable evidence base for this core argument is simply not provided and, as such, the evidence base that collaborative scholars have been more wayward in terms of paying insufficient attention to history, conflating 'different' with 'better', over-simplifying complex issues or courting conflict is weak. Why then make an argument about the potential emergence of 'paradigm wars' and 'sectarian methodological fights'? With this in mind one of the most curious elements of Crow's proposition is its constant focus on one specific programme of research and a small group of scholars rather than upon collaborative research *per se*. Even in relation to this very specific body of work the charges with regard to historical awareness, methodological supremacy and over-simplifying complexity are not really supported by a detailed and rounded review of the work. If there is a cautionary tale of *over*-statement to be heeded, then it is possibly one unwittingly provided by Crow's article rather than in the wider body of collaborative research. The reasons for this are unclear, especially in a piece that warns against 'courting conflict', but my sense was that the article was itself driven and defined by a powerful emotional dynamic that is never quite allowed to surface.

This notion of surfacing emotional under-currents matters because it is possible to argue that the link between collaborative research and emotions has never been more important. This is a bold point to make. It is also one that resonates with many of the themes regarding social shifts and academic antagonisms that Crow raises within his work. But whereas he *over*-states a number of issues concerning the link between collaborative research and emotions this section seeks to continue and broaden the conversation by (very briefly) offering four very different 'cautionary tales' about issues that are general *under*-stated within and beyond academe but actually demand far more attention. In doing so it offers a multi-levelled analysis that

draws-upon Pahl and Bell's (2018) positioning of '*within*, *against*, and *beyond*' as a useful heuristic but adds a fourth dimension '*between*' as a further way of understanding both the emotional dynamics of collaborative research.

Tale 1: The Danger Beyond: Fearful Societies and Emotional Intelligence

If the work of the 'Connected Communities' programme provides the recurring focus of Crow's case, then the work of C. Wright Mills appears to provide the intellectual touchstone for this response. The central element of which is Mills' focus on what he termed 'the promise' and 'the trap' in his (in)famous work *The Sociological Imagination* (1959). 'Nowadays men often feel that their private lives are a series of traps' Mills noted in the opening line '[t]hey sense that within their everyday worlds, they cannot overcome their troubles, and in this feeling, they are often quite correct…In what period have so many men been so totally exposed at so fast a pace to such earthquakes of change?' What Mills' labelled 'the trap' was therefore the risk that social change was outpacing the public's capacity to make sense of the world; 'the promise', by contrast, related to the capacity of the social sciences to promote public understanding and to help individuals and communities understand their position in society and through this to assume some sense of control (i.e. to free them from the trap). The role of the scholar was, through this interpretation, concerned with far more than the accumulation of knowledge but in helping individuals make sense of the changing world and their capacity for action within it.

Mills was a large, brusque, Texan cowboy – a 'radical nomad' (Hayden, 2006; see also Horowitz, 1983). He believed in 'taking it big' (Aronowitz, 2012) and was certainly not afraid of 'courting conflict' in the manner Crow suggests should be avoided. And yet Mills was acutely aware of not only the changing emotional landscape within American society but also the changing nature of academe. 'The very shaping of history now outpaces the ability of men to orientate themselves in accordance with cherished values. And which values? Even when they do not panic, men often sense that *older ways of feeling and thinking have collapsed* and that newer beginnings are ambiguous to the point of moral stasis (1959 [2000], p.4)[emphasis added]'. It is not difficult to take from Mills' focus on 'the misery of vague uneasiness' and to recognise its direct contemporary resonance in debates concerning 'the cultural backlash' (Norris and Inglehart, 2019), 'the left behind' (Wuthnow, 2018) or 'le peripherique' (Guilluy, 2019)? To be trapped is therefore to feel confused, fearful, anxious and adrift.

The core point being made, however, is that - notwithstanding the work of scholars including Sara Ahmed, Ernst Bloch, Martha Nussbaum, Raia Provhovnik, Paul Hoggett and Emma Hutchinson - the role of emotions and particularly *why feelings matter* and how they affect political attitudes and political behaviour in a context defined by the emergence of anti-political sentiment and populism remains under-studied, under-acknowledged and ultimately *under*-stated. This case has been powerfully made by Laura Jenkins (2018) but the link back into Crow's thesis about collaborative scholarship, and particularly the debate concerning 'different *is* different' as opposed to 'different *is* better', is that an argument can be made that collaborative approaches provide specific value in terms of identifying and understanding the emotional dynamics of communities. As the 2016 (p.64) Casey Review into social fragmentation argued in its critique of much of the existing social science research base, 'specific' in the sense of being better able to 'capture the mood of communities' than more top-down quantitative or survey-based methodologies. Hochschild's (2016) research *with* (not *on*) is mentioned by Crow and arguably provides a powerful example of the capacity of collaborative approaches to facilitate a deeper understanding of why emotions matter and how they might be understood. The great value offered by Hochschild's analysis of what she terms an 'empathy wall', the 'great paradox' and the existence of a 'deep story' is that it focuses attention on the significance of historical and arguably *emotional* anchorage. The potential links with Zygmunt Bauman's work on 'liquid modernity' are obvious but the temptation to engage with them must be resisted in order to make two simple points: first, this mode of research is *different* and generally understood as such; and secondly, the links between collaborative research, emotions and social change go far wider than Crow's 'cautionary tales' seem able to acknowledge. These leads us to consider a second challenge that has generally been *under*-stated: the expectations trap.

Tale 2: The Danger of Going Against: The Emotions of the Expectations Trap

One of the most interesting and yet under-developed elements of Crow's article is the manner in which it focuses attention on what might be termed 'the politics and management of expectations'. History therefore provides examples of over-promising and under-supplying that might prevent the raising of unrealistic expectations in the future (i.e. Crow #1). Understanding that 'different' is not necessarily 'better' serves to dampen one's claims (i.e. Crow #2); whereas advancing over-simplified accounts of complex issues (i.e. Crow #3) is also likely to underplay the probability that different actors may have very different priorities and expectations about the aims and ambitions research. There is little doubt that the challenge of managing various research-related expectations is an issue that has not received a significant amount of attention. It remains under-stated. I cannot help but wonder if when dealing with this topic Crow places too much emphasis on *supply*-side variables and too little emphasis on *demand*-side variables. Take, for a moment, the example of a politician who seeks to secure elected office. To some extent he or she is trapped within a professional framework that requires them to exaggerate and inflate the public's expectations in order to secure office. On occasion, the pressures of party competition can lead to a situation where the public's expectations are raised to such an extent that failure becomes to some extent inevitable (see Flinders 2012). The politician who considers adopting a very different strategy of *under*-promising and then *over*-supplying is also 'destined to disappoint' (Stoker, 2006) for the simple reason that they are highly unlikely to be elected in the first place. The danger of paying insufficient attention to the issue of expectations therefore has practical and emotional components. From a practical perspective, politicians who want to secure office have little choice but to 'play the game'; and beyond this, part of the emotional essence of politics revolves around its capacity to inspire hope, belief and passion (i.e. *'Yes we can!).*

It would at this point be possible to engage with Arlie Russell Hochschild's *The Managed Heart* (2003) and her suggestion that by reducing our expectations we may protect ourselves from later disappointment but the main point this sub-section seeks to make is that the *demand*-side pressures on academics have increased considerably in recent years. The traditional expectations surrounding the need for research grant applications to demonstrate originality, significance and rigor have now been overlaid with a set of more externally-facing expectations regarding social impact, relevance and public. The slight irony in terms of scholarly curiosity is that academics are increasingly expected to present a very clear narrative, or set of expectations about *what* will be achieved ('reach' and 'depth'), *how* they will be achieved ('pathways to impact' and *why* it matters ('link to recognised societal challenge') *before* the research is even undertaken. It would at this point be possible to highlight the perceived tension between 'blue-skies', 'discovery' or 'pure' research, on the one hand, and 'applied', 'goal orientated' or 'mission-driven' research, on the other, but to do so would be to slip back towards Crow's dependence on binary distinctions and zero-sum games which, as Sir Paul Nurse has underlined, are rarely 'completely distinct' (2015, 5). The main points being made in this sub-section are: (1) academics are arguably under increased pressure to make exaggerated claims about what their research might or has achieved; (2) those academics who *lean against* this pressure and make their case in a more modest but arguably honest manner risk rejection; and (3) the politics and management of expectations is a particular but rarely acknowledged challenge for collaborative scholars due to the manner in which they must persuade external actors (communities, charities, communities, service providers, etc.) to actually collaborate with them in the first place, and then to sustain that engagement. In this context 'the use of exaggeration as a rhetorical device to make the case', as Crow suggests, could be significant in terms of creating a sense of belief or value in a research project. This could create tensions as the project unfolds but I cannot help but wonder if the changing funding and audit framework within academe is almost demanding that researchers engage in the sort of exaggerated over-statement that Crow cautions against (for a discussion see Thomas, 2018). How academics cope with the emotional pressures of either accepting or going against this pressure leads to a focus on the emotional dynamics of modern academic life.

Tale 3: The Danger of Being Within: The Emotional Dynamics of Modern Academic Life

Although rarely discussed, it is actually far easier to craft an argument about the link between the politics and management of expectations (the topic of the previous sub-section) and the changing emotional dynamics of modern academic life than might have been expected. The work of Jennifer Chubb, Richard Watermeyer and Paul Wakeling, for example, on 'Fear and Loathing in the Academy' (2017) explores the

complexity of the varying moral dispositions, experiences and heightened emotionalism experienced by academics as they seek to navigate an increasingly pressurised and uncertain professional sector. The emergence of an 'impact agenda' in many advanced liberal democracies (for a review see Bandola Gill, Flinders and Anderson, 2019) is very often perceived by academics as the latest phase of the imposition of a neo-liberal market-based culture within higher education with negative effects for traditional notions of professional autonomy and intellectual freedom (see Wilsdon *et al.* 2019). As such, the emotional state of academic labour is frequently set against 'crisis accounts' that are said to make it increasingly difficult for scholars to distil a 'true' sense of academic identity.

> Words such as; 'scary', 'threat', 'nervousness' and 'worry' littered the transcripts as many spoke of their 'frustrations', 'suspiciousness' and even 'resentment' of the impact agenda. Participants reported feeling 'sad', 'unhappy', 'jealous', 'anxious', 'demoralised', 'disillusioned' 'confused', 'hopeless', 'paranoid', 'vulnerable', 'scared', 'nervous' and 'depressed' about the impact agenda (Chubb, Watermeyer and Wakeling, 2017, 561).

The 'decline of donnish dominion' – to paraphrase the title of A. H. Halsey's 1995 book on the British academic professions in the twentieth century – is a well-worn and possibly over-stated narrative but what is arguably *under*-stated is the impact that increasing professional pressures are having on the mental health and wellbeing of academics. It is neither possible nor necessary to engage in a detailed discussion on this topic apart from saying that if there is a 'cautionary tale' that really needs to be told about the link between research and emotions in the contemporary professional context then it is one concerning what Morrish (2019) has recently labelled 'the epidemic of poor mental health among higher education staff'. Excessive workloads and workload models that under-count the time involved in completing tasks, the failure to include many roles and responsibilities within the definition of 'what counts', the emergence of 'excellence;' as the default expectation across an increasing range of work strands, the pressure and visibility associated with external research audit systems, the emergence of precarious employment models within higher education, and the existence of a short-term performance management model in many universities are all held to be responsible for declining levels of mental health and wellbeing. Research conducted by Paul Gorczynski, Denise Hill and Shanaya Rathod (2017), for example, found that 43% of academic staff exhibited symptoms of at least a mild mental disorder (i.e. nearly twice the prevalence of mental disorders compared with the general population). A 2017 RAND report – 'Understanding Mental Health in a Research Environment' - also found that higher education staff reported worse mental wellbeing than those in other types of employment but also found that academics were generally reluctant to disclose or discuss their mental health challenges to employers (with only around six per cent of academics disclosing a mental health condition to their employer). It would at this point be possible to develop this argument into a discussion of Maggie Berg and Barbara Seeber's *The Slow Professor* (2016) or to engage in more existential debates about the manner in which academic work is by its very nature rarely completed. The basic point is that the emotional dynamics of modern academic life, and especially the need to promote mental health and wellbeing, is an area in urgent need of open conversation. However, are the emotional dynamics and pressures of contemporary academic life particularly pronounced for collaborative researchers? Is there a cautionary tale of danger of existing *between* that needs to be told?

Tale 4: The Danger of Being Between: Emotional Labour and Collaborative Research

Crow's article on 'collaborative research and the emotions of overstatement' offers 'four cautionary tales' about paying insufficient attention to history, assuming that 'different' is necessarily 'better', over-simplifying complex issues and courting conflict that are for reasons that remain unclear laid squarely at the door of collaborative researchers. They are, in fact, generic and largely accepted truisms that would pertain to any methodological approach and if forced to identify a 'repeat offender' in the sense of a methodological tribe that had, in my opinion, frequently failed to heed these warnings it would *not* be to collaborative researchers that I would turn. Nevertheless, generally hidden beneath the link between collaborative research and its inevitable emotional content there is a critical but generally *under*-stated issue that Crow's analysis unwittingly brings to the fore: the danger of being *between*.

What I mean by this is that collaborative scholars often exist not as 'insiders' or 'outsiders' but somewhere in the intersection or nexus between academe and society. This reflects both their belief in the capacity of social science and their commitment to supporting social change. The risk, however, is that by adopting this position they exist as neither-one-nor-the-other, in the sense of not being a 'mainstream' academic but also not being a 'conventional' social campaigner or activist. Existing *between* these spheres is arguably akin to operating in a professional hinterland. In this regard the recent work by Kate Pahl, Milton Brown, Zanib Rasool and Paul Ward (2020) in this journal is as insightful as it is possibly misleading. They suggest that,

> [C]community partners working with academics have to bear the emotional labour, by 'standing in the gap' they are having to move between community and university. We also recognise the power of community co-writing as a form that can open-up opportunity to speak differently, outside the constraining spaces of academia.

The focus on community partners is undoubtedly correct but I would argue that it is also the collaborative researchers that must have to bear a high degree of 'emotional labour' as they also 'stand in the gap' - or as I prefer to suggest *between* – society and academe. The danger of standing between or in any gap, irrespective of whether it is inter-sectoral or inter-disciplinary (or both), is that you are never fully accepted by any side and therefore face a stark choice of either bearing the emotional costs of operating in that space or 'taking a side' by committing to a specific sect, sector, tribe or discipline. Being an inter-disciplinary academic or being a collaborative researcher demands – as Catherine Lyall (2019) has demonstrated – not only resilience but the ability to craft a very distinctive research trajectory in a professional environment that while rhetorically promoting the mobility of people, ideas and talent struggles in reality to recognise those scholars who 'stand in the gap'. Crow might respond that history is littered with scholars who have survived in this space, the liminal zone. To some extent Sir Bernard Crick provides an example of a scholar who certainly did try and operate *between* but who ultimately took early retirement due to a sense that many of his skills and contributions were no longer valued when set against an increasingly narrow definition of 'professionalism'. C. Wright Mills arguably provides an ever starker example of the emotional burdens placed on those scholars who seek to row against 'the drift' of dominant professional expectations. He certainly paid a high price for what Brewer (2005) described as his 'outsider mentality' and by the time he died at just 45 years of age he was very much an academic outcast. 'Standing in the gap' Pahl and her colleagues argue 'is a lonely place' and maybe this is the 'cautionary tale' that really needs to be told more often.

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