**The tyranny of research? Urban regeneration, ethnography, and the problem of unintended consequences**

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**Abstract**

Following a case study of community development in public housing in Liverpool, UK, we present in this article three potential tyrannies of research. We show the tyranny of the researcher, of the method selection, and of the data. In so doing, we identify the methodological challenges of conducting research that seeks to privilege the voices and perspectives of the subjects in a participatory project. We examine whether the dangers of tyranny are present in ethnographic and other participatory forms of research as much as in the practice of participatory development. We argue that, by acknowledging the potential tyranny of research, we are able to highlight the importance of a critical and reflexive research practice, particularly for ethnographers working in a participatory context.

**Keywords**: Affordable housing; ethics; qualitative research methodology; social capital;

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**Introduction**

Just as we might identify potential tyrannies in community participation, there are potential tyrannies in research. In the “developed” world of urban regeneration, participation has become a concept integral to the definition of problems and solutions, the management of interventions, and the evaluation of outcomes (Marris & Rein, 1972; Arnstein, 1969). We consider in this article an action research initiative that combined forms of ethnographic work in a case study of resident involvement in regeneration in an inner urban Liverpool community. Often, discussions about community participation focus on technique and method and fail to ask more fundamental questions about controlling the terms of discussions and the voices heard (Cooke & Kothari, 2001). We seek to reproduce such a critique through a reflection on our own research and suggest there are consequences often not considered in methodological design when conducting action research involving professionals and local people. We critically reflect upon the value of the ethnographic approach and its relationship with participatory modes of community development work and argue, by means of contribution, that there are three possible tyrannies of research: there is the tyranny of the researcher; of the method selection; and of the data.

We review an action research project that took place in the Anfield and Breckfield wards of North Liverpool and incorporated an ethnographic case study of community engagement. One of the authors was employed as a Research Associate in a Knowledge Transfer Partnership (KTP), a collaboration between the University, where the research team was hosted, and a local Housing Association[[1]](#endnote-1). The aim of the KTP was to examine the possibility of a Community Land Trust (CLT) as a means to achieve the transfer of housing stock to the community (Davis, 2010; Angotti & Jagu, 2007; Stein, 2010; Moore & McKee, 2012). The research considered what capacity the local community had to take control of housing assets and the support available to manage them effectively. While others have sought to understand the tyrannical aspects of the relationship between development or regeneration practitioners and the community (e.g. Pollock & Sharp, 2012), our reflection on the work has allowed us to think through tyrannical aspects of the research process. As the Research Associate adopted a dual role of researcher and community development worker, he was involved in the community participatory structures that would be central to the nascent CLT.

**Context of the case study research**

The ongoing relationship among local residents, the City Council, and the Housing Association, had been sensitive for a considerable time prior to the start of the research. By 1999, the area had been in decline for decades, as reflected in their position at the top of indices of deprivation and in an increasingly scarred and unloved urban environment. Housing associations and the private rented sector had established themselves as the major players in the local housing market as owner occupation declined, the retail sector failed, and basic community assets, such as the post office, closed. Crime, anti-social behavior, and poor environmental conditions began to undermine the community’s faith in public services (Bevington, 2008).

Local residents had accused the Housing Association and the City Council of neglecting the area. The Housing Association had responded by suggesting their tenants were difficult to manage, while a number of public and other organizations, including Liverpool Football Club, had met to discuss ways of regenerating the area. The football club’s ambitions to expand their stadium meant that they had a strong interest in plans for the area and, in September 1999, in collaboration with Liverpool City Council, they organized the “Breckenfield” exhibition and presented to the community a plan on how the stadium and the adjacent area should be regenerated. The plan suggested that a number of houses be demolished and, unsurprisingly, it failed to convince local residents. They felt excluded from the process. After a number of heated public meetings, an agreement was reached between the council and the local community, and the plans presented at the Breckenfield exhibition were dropped. The Anfield and Breckfield Community Steering Group was established to work up its own regeneration plans and to contribute to a Joint Steering Group, which included the City Council and the football club (see Southern, 2014).

The Joint Steering Group published its plans in June 2002 (GVA Grimley, 2002). This advocated the demolition of a number of homes and the refurbishment of others, but the focus remained on accommodating the expansion of the football stadium. In that same year, the national Housing Market Renewal Initiative (HMRI) was launched to intervene in areas where the housing market had “failed.” The Newheartlands HMRI Pathfinder for Liverpool, which included Anfield and Breckfield in its scope, called for 50,000 homes to be demolished in the Liverpool city region, with over 2,000 in the research area (Allen, 2008). In response to the Steering Group plans and in the context of HRMI, community representatives in the Anfield Breckfield Community Partnership (ABCP) – a development trust – were asked to consider the future ownership of assets by the community.

It was during this period that the Housing Association began to consider the idea of using their housing as a resource for the area’s regeneration. Ongoing discussion with community representative began to prompt ideas, such as a community based housing organization that would sit as a subsidiary of the main body (Bevington, 2008). By 2003, the term “community endowment” was used to describe the Housing Association’s commitment to secure resources for the community, and it was at this point that work with the University led to a joint initiative resulting in the employment of the Research Associate. The KTP was designed so the Associate would have a community development role to assess the feasibility of community ownership of assets in the area, either as a resident led Housing Association or as a CLT. In large part, these ideas emerged from the statutory agencies and through a top down process and were then proposed to the community, who were represented by the two local resident associations for Anfield and Breckfield. A commissioned legal paper had already explored the possibility of a CLT being formed, although the community were short of those technocratic skills needed to drive the idea (Engelsman, Rowe, & Southern, 2016) and were reliant on both the Housing Association and City Council to recognize and explain the merits of the CLT idea.

By the time this case study research began in 2006, Anfield had become of interest on a regional and national level. Academics, politicians, and journalists became regular visitors to the area, and local residents were accustomed to guiding visitors and answering questions. While residents were happy to welcome visitors, they remained concerned that their community’s transformation, through the demolition of dilapidated, mostly vacant housing, and building new affordable homes, was delayed. When the Associate began working in the field, local residents appreciated that this project was different due to its action research form, which fostered relationships between the researcher and key local activists. In effect, research and the presence of outsiders in the area had become normalized, and the context of this work was grounded in the many important practicalities facing local residents, such as dealing with the maintenance of housing and the environment. On occasion, residents would be skeptical about our work, commenting that it was “just another project that would probably fail,” although there was never any open hostility or negativity towards the Research Associate and the aims of the research.

**Methods**

The research was a case study concerned with the capacity of the local community to take control of the housing stock and to manage it effectively. It had some of the characteristics of action research, or what Schein (1987) might call “clinical research,” but, for the purposes of this article, we are concerned with the ethnographic form the case study took, and draw upon the observations and other evidence gathered by one author, the Research Associate, deployed as both a researcher and a community development worker. The fieldwork sought to explore the form and culture of the nascent CLT and its participatory structures in the local community. We were able to pose questions such as, who was involved? What form did involvement take? How were decisions taken and by whom? Could we identify processes of knowledge exchange, and if not, why not? How did the community understand their role in the CLT? And what were the dynamics of community participation and decision making?

While not ethnography of a classic form, a number of different data collection techniques were employed. Semi-structured interviews were conducted with 17 staff and local residents, and more than 15 informal interviews were conducted with a number of other stakeholders. Observation, and participant observation, was conducted in the homes of residents and the offices of the Housing Association in the course of full-time work in the local regeneration center between 2007 and 2010. This work included attending regular meetings, discussions with staff and residents, as well as informal interactions with both staff and residents on a day-to-day basis. A research diary was kept to record data. The Research Associate did not seek to explore the routines, rituals, and meaning systems of a distinct and remote indigenous community. Rather, the research was focused on understanding these questions in the organizational setting of the nascent CLT. Whether we think of this as paraethnography (Holmes & Marcus, 2003 & 2005; Marcus & Fischer, 1999), or qualify it in some other manner, as a multisited (Marcus, 1998 & 2008) or organizational ethnography (Brannan, Rowe, & Worthington, 2012), it shares a form that is common to ethnographic work in aid and in participatory development projects (e.g. Escobar, 1995; Ferguson, 1990; Mosse, 2004a, 2004b; Wedel, 2001).

The Research Associate led extended participant observation of both professionals, working to empower the community, and of those members of the community engaged in the process. Thus, the research shares features we would associate with a case study. We draw upon multiple sources of data and different data collection methods to develop our understanding of a complex and multifaceted phenomenon (Yin, 2014). Our focus was not solely on the voiceless, familiar to traditional forms of anthropology, but included those with power and resources in the local state. This interest in both the subaltern and those in authority, with whom a more collaborative (Marcus, 1998) relationship might be more appropriate, presented particular problems and conflicts upon which we will reflect throughout the rest of this article.

**Magical powers, unintended consequences**

Our research drew upon Bourdieu’s (1991) concept of symbolic violence. This helps us to analyze the relationships between stakeholders, specifically local residents and regeneration professionals, as they worked together to develop the CLT. Symbolic violence is defined as:

…an almost magical power which enables one to obtain the equivalent of what is obtained through force (whether physical or economic), by virtue of the specific effect of mobilization – [symbolic violence] is a power that can be exercised only if it is *recognised,* that is, misrecognised as arbitrary (Bourdieu, 1991, p. 170).

Bourdieu argues that symbolic violence occurs when it is covert, unintentional, and never recognized as violence. It is enigmatic by nature:

the gentle, invisible form of violence… is not so much undergone as chosen, the violence of credit, confidence, obligation, personal loyalty, hospitality, gifts, gratitude, piety - in short all virtues honoured by the code of honour - cannot fail to be seen as the most economical mode of domination (Bourdieu, 1977, p. 192).

This concept helped us explore the nature of the unintended consequences of professional practices and the complicity of “victims” because of their “misrecognition,” and we found examples of the magical power of symbolic violence in aspects of the participation process.

The regeneration office, where meetings took place, was an old police station with an intimidating look and layout. It had barbed wire around its outer walls as well as frosted windows with metal grates. Inside, one was met with a glass barrier separating the reception area from the public. To reach the meeting room, residents had to walk up a narrow staircase and through six doors to participate with those from the Housing Association and City Council. Meetings were often presented with dense documents that residents had been expected to read prior to attending and would then involve presentations through PowerPoint and the use of professional jargon. These examples may not seem dramatic, but it is exactly their subconscious and unintentional impact that makes them examples of symbolic violence. While the relationships between stakeholders cannot be reduced to the magical power of symbolic violence, we can draw this idea from the research and understand how residents too were often complicit in the construction of symbolic violence through their acquiescence to what they assumed was regular practice. This becomes a simple but subtle form of the tyranny of participation (Cooke & Kothari, 2001).

On reflection it led us to consider the potential for symbolic violence as a result of the ethnographer’s actions in the particular context of the community. Others have written about the ethical challenges of conducting ethnographic research (e.g. Hammersley & Atkinson, 2007; Ferdinand, Pearson, Rowe, & Worthington, 2007), of the evolving relationships between the researcher and the researched (Beech, Hibbert, Macintosh, & McInnes, 2009), and of the problems of power in organizational ethnography (Fine & Shulman, 2009). Reflexivity in research, just as much as in participatory practice, is necessary because of the potentially tyrannical relationships. As Bourdieu explains:

How can we not feel anxious about making *private* words *public,* revealing confidential statements made in the context of a relationship based on trust that can only be established between two individuals? True, everyone we talked to agreed to let us use their statements as we saw fit. But no contract carries as many unspoken conditions as one based on trust (Bourdieu, 1999, p. 1).

This points to the need for reflexivity and the sensitive nature of conducting research, and echoes the themes and observations of Cooke and Kothari (2001) on the practices of participatory development. At the same time as our research was concerned with evidence of symbolic violence and of the tyranny of participation, the process of leaving the field and of beginning the desk and head work of ethnography (van Maanen, 2011) prompted further reflection. For this, we frame the nature of the relationship between the ethnographer and the subjects of the research as the three tyrannies of research.

Our argument is consistent with the idea that community participation in economic and other development projects can be tyrannical (Cooke & Kothari, 2001, pp. 7-8; see also Bell, 1994). In this case, we are using the idea of tyranny to refer to the “illegitimate and/or unjust” (p. 4) power that researchers can hold over the research process and research subjects. We take the three questions posed by Cooke and Kothari: Does participation override pre-existing legitimate decision making processes (their “tyranny of decision making and control”)? Do participatory group dynamics reinforce power (their “tyranny of the group”)? Finally, does participation displace other methods with other merits (their “tyranny of method”)? We reframe these and raise the possibility of a further dimension. We ask whether research, specifically in community development and urban regeneration, may pose similar problems of tyranny: of the researcher; of method; and of data.

**The tyranny of the researcher**

This form of tyranny concerns itself with Bourdieu’s (2010) notion of capital and particularly the way interviews are affected by unequal levels of capital between researcher and researched. We show the power relations that arise and argue that there are aspects of the tyranny of the researcher that can be found in research in general.

***The intrusion inherent in social exchange***

Intrusion was evident in the ways in which people came to visit the Anfield and Breckfield communities. Neighborhoods undergoing extensive regeneration work are often sites of academic study and political interest, and often visitors came to the area, took photographs, conducted a few interviews, and then left. When the Research Associate applied to work on the project, part of the interview process involved a bus tour around the regeneration zone. The bus did not stop for the candidates or interview panel to walk around and speak to people. It was as if the candidates were on a safari tour to experience a neighborhood that was totally out of the ordinary, one that was too dangerous to actually walk around. To take the analogy further, the regeneration area became a spectacle, a place to come to see how “other people” live (Glynn, 2009). A psychological or cultural divide was created between the visitors and locals, with the regeneration office sitting behind barbed wire and a large fence to separate it from the street. Many of the employees in the office would not consider living in the area and they drove to and from the office without spending any time in the neighborhood. The Research Associate (educated, middle class, and an outsider from London) was introduced to this alien world on his first day with stories of shootings and the advice that he should keep to himself when out on the street.

The Associate also visited the homes of local residents to attend meetings and conduct interviews, often in streets designated to be demolished, containing blocks of vacant properties. Residents were skeptical of the project, classifying it as yet another attempt to regenerate the area that was likely to fail, although taking the opportunity to vent their concerns and frustrations. It was logical then that the Research Associate and researched would want the visit to be used to record the extremes of the situation that people were living in. Yet, in holding a dual role, as an employee of the University and a community worker on behalf of the landlord, the Housing Association, the authority of the Research Associate was not clear. Access to record such poor housing conditions was easily facilitated, but the purpose for gathering evidence was sometimes ambiguous. Residents raised questions about practical problems as if expecting the Research Associate to resolve them, unable to make the finer distinctions of the role. For the Research Associate, this would increase the feeling of intrusion, as if the reason that entry was granted was his status as an employee of the Housing Association and he was, therefore, able to deal with complaints.

***Symbolic capital and violence***

Bourdieu (2010) writes that social proximity and familiarity are two of the conditions which may enable nonviolent communication. It is understandable why these two conditions bring the researcher and researched to a more equal footing where the aim is to have a research process as near as possible to a conversation between equals. However, in our case, this was a challenge for a number of reasons. Liverpool, as a city, has a reputation for being unique, and locals often refer to it as a separate region. An outsider researcher, arriving from London, automatically creates a wedge of social distance rather than proximity. While the longitudinal nature of the case study did enable relationships to develop, there was always a sense of distance due to cultural and social differences. The nature of the interaction between researcher and researched was dependent on the levels of capital exchanged. Linguistic and symbolic capital were especially relevant in this instance and the Associate, having a different regional accent to the locals, felt like a stranger as cultural differences added to the social adjustment and interpretation of the field of study (Schuetz, 1944; Kearney, 2003).

Familiarity did improve as the research evolved, enabling more easy interaction with the community, in spite of this unequal capital. Typical of this was the way one of the local residents made comments that indicated her awareness of the imbalance in symbolic capital. As with the examples of symbolic violence in meetings we note above, this resident wanted to make it clear that she was still to be taken seriously, even though she might lack formal higher education qualifications. She mentioned, on a number of occasions, that “although I may not have been to university, I still…” and would then complete the sentence with phrases such as “understand the report” or “follow what’s ‘really’ going on,” thereby, at the very least, illustrating how power between the research subjects and the researcher became operationalized.

Unequal levels of capital shape the nature of research interactions and translate explicitly into power relations. This relationship was complex, having at least three dimensions: between the Research Associate (and the wider research team) as researcher and researched (the residents); the residents (researched) and professionals (researched); and the professionals (researched) and the Research Associate as researcher. Further complexity is added through the hierarchical structures of the Housing Association, the presence of the local state, and the politics inherent in community organizing. In terms of local residents, the perception of (relative) power held by the Research Associate within the organization made locals view him as coming from a position of power where he could arrange for things to “get done,” as we have noted. The fact that the Research Associate had a university education and was viewed as middle class meant that he was affecting those researched through the use of words and the way they were spoken. To distinguish the Research Associate from the researched, and then to discern the research from the management of housing, meant different levels of symbolic and cultural capital came into play, and this made it difficult for the research team to unravel the processes of power.

***Egocentric questioning***

When research is underway and during dialogue, the researcher is often thinking of the next question to ask in order to keep the conversation flowing. The result is that, whilst the next question may be quick to hand, there is a degree of manipulation taking place and, although interviews may be termed conversational or semi-structured, the element of planning ahead and directing the conversation remains. Symbolic violence is inherent in the ebb and flow of dialogue and can obscure the views and perspectives of the research subject. The researcher’s position enables him/her to influence, to set the terms of the debate, possibly to the detriment of those researched. Interviewing can prove tyrannical, even when conducted with full ethical review intended to obviate that possibility. We employed ethnographic and action research to enable the voice of the community to come through clearly but we did not fully anticipate some of the complexities.

In our reflections, during the work and after, we felt that tyranny could be seen in the limits of participation and in the way the research developed. Residents and regeneration staff were asked to define community empowerment and then to discuss whether the community was sufficiently empowered. Some professionals responded that, in their opinion, the community was sufficiently empowered through their inclusion in certain meetings and community consultations. On occasion, the Research Associate would take a contrary position to the professionals in meetings, supported too by the research team when they were present. Yet, this rarely translated into greater levels of community empowerment and, more often than not, sought to reinforce rhetoric about involvement. As we reviewed our role in an attempt to contain these adverse consequences, egocentric questioning was never eradicated and there were other ways in which the methods had the potential to lead to tyrannical outcomes.

**The tyranny of method selection**

There are aspects of tyranny when the researcher’s methods and manner affect the field as she/he interacts with people. It is seldom heard of for researchers to approach their research subjects and ask how they would prefer to be studied, or what method would be most suitable for the subject of the research. Furthermore, when research is planned, the research team determines a list of research aims which drive the research and directs it towards certain types of conclusions and ways of presenting findings. As the research develops, these research questions then influence the way the researcher interacts with the field. One of the benefits of ethnographic research is the way theories of knowledge construction play a role not only in research design, but also in research outcomes. This is no less rigorous than any other form of research and helps us to realize that the methods deployed will be influenced by the researcher’s previous experience and their assumptions. In the design of action research, an approach fraught with potential conflicts and tensions, the notion of tyranny may become more poignant.

***Spying/eavesdropping?***

At the start of the project, all research subjects gave permission to be part of the research, but this was a general and one off consent (Ferdinand, Pearson, Rowe, & Worthington,2007, p. 533). The Associate was fully aware, throughout the research, that all interactions and conversations could be used as data, while those researched were not always so. On the other hand, throughout, there was awareness that, although the Research Associate was working in the office, he was from the “university.” In the office, there was a clear distinction between people, employed solely by the Housing Association or the council, and the Research Associate, who was carrying out slightly different work and who would have not normally been placed in this particular office.

In principle, the duality of the research and practitioner roles combined in a logical way. They sat comfortably with the action research design and appeared consistent with the objectives of both the University and the Housing Association. In practice, the researched, the residents in particular, did not always recognize the subtle differences between the two roles. Constant reference to the distinction could be made, although, immersed in the field, the Research Associate could also lose sight of the significance. He felt that some of the methods used to gather data could be described as tyrannical. For example, participating in conversations in the corridor, at the water cooler, or in the kitchen were all potentially part of the data gathering process. Data collection had the potential to turn the Research Associate into an eavesdropper with control over what was/was not considered data and when/where he adopted the role of researcher or employee.

Participants were regularly updated about the research, mostly through informal chats often instigated by the participants as they inquired about the university work. Nevertheless, while conversations were recorded in field notes with the formal consent of those researched, such as when disparaging remarks were made by one group of subjects about the other, it became a contextual problem of tyranny. Where informed consent is obtained and there is an understanding that the researcher could record or note all, there remains a question about the obligation that the researcher has to reveal what has been captured. Even though all names and identities were anonymized, comments and remarks might cause tensions or affect partnerships on the ground, a consequence which neither residents not professionals would wish for.

Ellis (2007) attempts to deal with this issue using her concept of relational ethics, explaining how difficult it would be to publish any of her research without potentially offending some participants:

When I returned to Fishneck, my friends there confronted me with the words I had written; they reacted strongly to my descriptions of their smelling like fish, taking infrequent baths, being overweight, making little money, wearing mismatched clothing, having sex at an early age, and being uneducated. The Fishneckers had little difficulty deciphering the identities of my characters. My strategy of inventing pseudonyms starting with the same letters as the double names of the Fishneckers had made it easy for them. However, even without these clues, they recognized the stories they had told me and themselves as the characters. Although they knew I was writing about them, some said they thought we ‘were friends, just talkin’,’ and never thought I’d write down the things they told me. (Ellis, 2007, p. 11).

Ellis concludes that she would have trouble “doing research on anyone, though I would be happy doing research with any number of people and communities in an egalitarian participative relationship” (p. 13). Ethical difficulties may arise, even when the ethnographic research may appear “egalitarian.” There were constraints on our research design, as organizational objectives took precedence, even as we sought to support resident empowerment. Concerns surrounding the tyranny of research do not only occur when research is conducted “on people” but are just as likely to occur when research is conducted “with people.”

The ethnographic method enabled a nuanced and considered analysis of the regeneration project to emerge which would have been impossible through simply conducting interviews or briefly visiting the regeneration site. Nevertheless, decisions about what to include in and what to exclude from any written output, however distant the field may be, is an exercise of power over subjects. This is where our research differs from Holmes and Marcus’s (2003) notion of paraethnography. They turn towards a collaborative ethnographic method, whereas we are pointing to what might be termed the betrayal of the participant (whether those with resources or the subaltern) that arises in research characterized by the conflicts between professional and researcher roles of the kind we are concerned with here. This tyranny of method is particularly difficult to design out of action research or of research within communities.

***Research or work?***

In this work, the Research Associate had to combine and balance the role of employee and researcher, as would be the case in many participant observer and action research projects. A conflict of interest arose when the Research Associate had to decide which role to hold during meetings and conversations. He would find himself thinking of the usefulness of a certain exchange and recording it, instead of thinking about how the exchange might be relevant to the practice of regeneration. As an employee of the University and located in the local offices of the Housing Association, the Research Associate had to move between roles, as researcher and as community development worker. While the two roles seemed complementary, it was not always clear what influence was being exercised in what role.

Aspects of research reactivity and reflexivity are common in qualitative research, although not always debated. One of the questions asked of regeneration professionals in interviews was whether they think local residents should have more control over the future of the area and whether they should be more involved in decision making. Questions such as these may be seen as exercising a hidden power and influence. They may make interviewees think about issues they may not have otherwise spent time pondering. The objective of the work was to prioritize some change within a low income community and, by so doing, the research team hoped to disturb the status quo. Laudable as this may seem, by making people aware of possible tensions and asking people for opinions about other stakeholders in the regeneration program, our interventions could be considered tyrannical if they sow the seeds of animosities. Change was a desired outcome, but the research team could not always anticipate the consequences of the process of research.

On a number of occasions the Research Associate went to a resident’s home to conduct an interview and ended up hearing specific complaints about the broken promises of Housing Association staff. In one example, a resident complained that the office manager was “too nice,” and how he always promised to deal with problems, but they rarely got resolved. Here the researcher is potentially compromised because, at this point, the resident expected the Research Associate, there to conduct an interview, to approach the manager, as a Housing Association employee, and pursue the service complaints. On the other hand, the Research Associate also heard officers’ verbal abuse of residents. One office worker referred to a community member as a “cream covered razor blade,” often pleasant to her when face-to-face, but “bitchy” and “backstabbing” at other times. Another council worker claimed that some activists were “wielding far too much power and influence and, unfortunately, the city is having to take heed of them.”

Developing some of those same prejudices through the processes of research would have been tyrannical. In seeking to avoid this danger, great care was taken to question and challenge emerging assumptions in discussion with the wider research team. The ethnographic process also enabled the Research Associate to build long term relationships with both residents and regeneration professionals. Negative and abusive comments could then be placed in the context of those relationships spanning a number of years. Spending every day in the regeneration office enabled the Research Associate to see people working in difficult circumstances, facing people from a working class community with day-to-day problems they were trying to overcome. The ethnographic process led to a more authentic account, but the process of turning data into published material could equally be seen as tyrannical.

**The tyranny of data**

Mosse (2006) points to the power inherent in research and in the process of presenting findings. His co-workers and informants claimed that his work would harm “the professional reputation of individuals and institutions, and would harm work among poor tribals in India” (p. 935). Mosse problematizes the exit from the field and the writing up of ethnographic texts. His representation of research conducted is admittedly positioned, but the fact that his research caused such controversy shows how “academic discourse itself demonstrated considerable power, even against quite determined objectors” (p. 951). In this way, both the researcher and the text itself “are active agents in the worlds they describe” (p. 952) and ethnography itself should be described as a situated intervention, or “clinical research” (Schein, 1987), as opposed to a passive or descriptive process.

***The tyranny of research output***

Bourdieu(2010) points out that writing includes the projection of oneself onto the other. Penning research findings may be viewed as tyrannical if they represent the views of the author more than those researched. Furthermore, turning hearsay and recordings into data is a process that transforms the data through the act of writing and the research from private to public. Much of the research data here was collected during working hours as a participant observer and included field trips in the UK and US, with the University research team and with community and Housing Association representatives. The duality of roles within the research process was always explicit, although access to office conversations, telephone calls, meetings, and emails had a less than clear research and subject distinction. Evidence was collected in the field without the explicit knowledge of how the data were going to be interpreted and, importantly, that it could be published, which had the potential to create tensions between those involved.

Published reports can be viewed as tyrannical when they obscure the “reality” on the ground, or give an impression that is misleading. Evaluation reports for national regeneration or development programs are often partial and misleading in a number of different ways. They misrepresent and do violence to neighborhoods by exaggerating positives or emphasizing negatives, often neglecting the fact that, overall, many of the interventions have not significantly changed the area. Strategic reports of this kind show a tyranny of publication when they are written with political goals. They are tyrannical to those who have a contrary view, particularly if alternate channels to express opinions are closed off. There was a worry, because of the dual role of the Research Associate, that some commissioned reports might result in a negative effect for the local community.

Throughout the research process, part of the development role was to write strategic reports. After a research trip to the US to learn about Community Land Trusts and their potential applicability to the Anfield and Breckfield regeneration plan, a number of research reports were commissioned. A regeneration consultant was commissioned by the Housing Association to write a report on learning from the visit and how transferable the principles of the CLT movement could be. A complementary report, written by the Research Associate, began to articulate a model for developing a CLT in the Anfield and Breckfield wards. Both reports considered the capacity of the local community and provided an argument for development, although, ultimately, they were written in a manner that served the interests of the Housing Association. Essentially, the tyranny from the research would be to assume that the Housing Association managers and local tenants and residents shared the same objectives, and for this to be implicit in report writing (Engelsman & Southern, 2010).

**Reflections on the tyranny of research**

In researching urban regeneration, there are unintended consequences that require critical ongoing consideration by the researchers involved. The consequences arising from our research were, on further reflection, more subtle than perhaps we had fully understood at the outset. The community with whom we became involved had been subject to many common processes of regeneration. Our involvement with a local Housing Association enabled us to adopt an ethnographic approach with the aim of stimulating the community to organize for change. We considered whether this could pose problems of tyranny in three specific ways: the tyranny of the researcher; the tyranny of the method selection; and the tyranny of the data. By reflecting on the tyranny of research, the importance of a critical and reflexive research practice is emphasized, particularly relevant for ethnographers working in a participatory context.

These tyrannical possibilities are more likely to occur when certain conditions are present. In our work, the research design was an action research project, where the researcher seeks to influence change as part of the research process. Our research was conducted in an excluded, vulnerable, and marginal community with a number of agencies active in regeneration processes. As a consequence, this made the work more susceptible to issues of tyranny, to the “illegitimate and/or unjust exercise of power” (Cooke & Kothari, 2001, p. 4). It is possible that ethnographies of vulnerable groups in general will lead to an exercise of inequality between researcher and researched, thereby increasing the potential for unintended consequences. We do not claim that the tyranny of research is inevitable or that it applies to all ethnography. Rather, it is more likely to occur under certain circumstances, which we argue are common enough to merit this exploration.

A central element of ethnographic research is to be reflexive, to understand the impact of our presence in the field, the way we shape, interpret, act, and observe (Hammersley & Atkinson, 2007; Schein, 1987; van Maanen, 2011). This piece of research was work with a community to develop the capacity to control housing assets, and we are struck by the parallels with the debates and issues raised by Cooke and Kothari (2001). At the same time, we draw parallels between this debate and Bourdieu’s (1991) concept of symbolic violence. In each case, while the language of tyranny and violence might appear to exaggerate the nature of the harms done, they draw attention to those harms and demand that we treat them seriously.

Participatory practices and ethnographic modes of research should seek to give a voice to the subject of action or of research, more so when the research is concerned with stimulating change that involves the researched. Cooke and Kothari (2001) suggested that tyranny might arise when practice fails to reflect on the impact of workers on their subjects and communities. They cautioned against the tyrannies of decision-making and control, of the group and of method. Their concern was not to return to some *status quo ante*, of passive communities waiting anxiously for the trickledown effect of national policy. Rather, they suggest the need for greater reflexivity in the work of practitioners seeking to genuinely hear and respond to voices traditionally unheard. Similarly, ethnographic work, in these participatory contexts requires an awareness of the nature of power in a particular community, and of the ethical and moral dilemmas involved in conducting research in the field. In drawing attention to the dilemmas we encountered, we seek to begin a discussion that will inform future researchers of the meaning of these dilemmas for their practice. The tyranny of research can arise in an ethnographic research setting, particularly where the work has some of the character of action research (Mosse, 2004a) amidst an excluded and potentially vulnerable community. In reflecting on these questions, we must also accept that our efforts to raise the standards of reflexivity and of ethical awareness in ethnographic research of this nature require “a sincere acceptance of the possibility that it should not be saved” (Cooke & Kothari, 2001, p. 15).

In drawing these parallels, and reflecting at length on the experience of the research collaboration, we are not seeking to suggest ethnography, as a methodology and a practice, is flawed or always tyrannical. We wish to clarify some of the questions about the potential for tyranny in research that need consideration in the design stage and space for ongoing reflection in the field. We are also explicitly responding to the call for authors to reflect on methodology in submissions to *Community Development* (Green & Phillips, 2013). We present both an analysis of our methods and, at the same time, share something of our action research design, and specifically our ethnographic case study. Much as Cooke and Kothari were not arguing for a return to bureaucratic top down programs and projects in international development, we do not suggest we should return to more distant and “objective” modes of evaluative inquiry. Indeed, the value of research such as ours is most apparent in those communities where it might also be most problematic. In this instance, the community had been studied, analyzed, and evaluated many times in the years before the research began and since (Ellis & Henderson, 2013). It is the poorest communities who receive the most attention from government agencies and social researchers. Yet their voices remain largely unheard and their experiences much misunderstood, and we suggest that the obligation to reflect on practices, whether of participatory development or of ethnographic research, is critical.

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1. The co-authors of this article were the academic supervisors of the project and were involved in formal meetings involving the housing association and residents and met regularly with other groups involved in the regeneration of the area. Throughout the article, when we refer to the authors collectively, we use “we.” When we refer specifically to the actions of the researcher in the field, we refer to “he” or “the Research Associate.” [↑](#endnote-ref-1)