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ALICE THROUGH THE TELESCOPE: A CRITICAL AUTOETHNOGRAPHY
OF AN (ALMOST) PARTICIPATORY RESEARCH PROCESS

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Thank you to Anna, Louise, Sarah and Phoebe for allowing me to turn you into characters in this story and to all the anonymous people who were part of the project in Parkhood and Urbanwood.

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We sat together on the cheap plastic bench, taking stock, taking a breath. I am terrible at small talk, but she didn’t seem to mind my sitting down with her. We watched the children running about. Some looked very young to me, not more than five years old and they were showing off; showing us that they were grown up, that they could look after themselves. They were shouting and swearing, offering a challenge to our adult authority. They stole glances at us; we ignored them. They didn’t shock us.

She runs youth groups, she told me. Often young children come on their own, not more than six or seven, without the money they should bring to pay for the session. She doesn’t turn them away, she knows that they would have nowhere else to go and doesn’t like to think of them wandering the streets. The policy is that she shouldn’t release a child until an adult, someone on the list, has come to claim them. But she knows that if she didn’t let some children go by themselves, nobody would claim them. They would be hers forever if she followed that rule. So she lets them walk away into the evening, by themselves.

She looks like anyone else in the neighbourhood, is anyone else, really. She grew up not far from here, is a single mum. But her daughter wasn’t allowed the freedom to wander the streets that the children we are watching have. It was funny, she said, that here we are judging their parents for letting them out to buy energy drinks from the shop and run wild, swearing at people, and yet her neighbours judged her because, they said, she wasn’t letting her daughter get streetwise like a good parent should. I said that was funny, because if someone let their child out to wander for hours without adult supervision where I lived, someone would phone social services. Our ideas of good parenting are totally the opposite of what they think round here, she said. We could agree on this, everyone believed their way was for the best.
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Abstract

Social marketing is a technique for behavioural change that has been around since the 1960s, when prominent academics argued that the tools and techniques of marketing could be used for social as well as commercial ends. More recently, the orthodox approach to social marketing - based on the marketing management paradigm - has been challenged and new approaches are emerging. One such development has been characterised loosely as ‘co-creation’, which in a social marketing context is understood to mean behavioural change interventions that are developed collaboratively with the target audience, rather than by remote experts.

I present here an autoethnographic study of an 18-month ‘co-created’ social marketing project that sought to reduce risky drinking in two deprived neighbourhoods. Locating myself epistemologically within the post-structural approach articulated by critical sociologists (e.g. Laurel Richardson and Norman Denzin), I have written two analytical stories about the project based upon field notes, project documents, emails and recollections. One story is akin to a thick description, the second organised around four emergent themes: negative space, legitimacy, resistance and performativity.

Drawing upon literature from participatory research, international development and activist scholarship, I present a contribution in three parts. First, a detailed ethical and epistemological critique of social marketing’s claims to legitimacy as a methodology of social change; second, the development and theoretical justification of autoethnographic writing as a method for analysing participatory and action research projects; and finally, an exploration of the relationship between identity (internally cultivated and externally imposed), social inequality and social activism via evocative writing as “the very possibility of change” (Cixous, 1976, p. 879).
Chapter 1: In which I begin

“A research presentation...is a discourse in which you expose yourself, you take risks... The more you expose yourself, the greater your chances of benefiting from the discussion and the more constructive and good willed, I am sure, the criticisms and advice you will receive” (Bourdieu and Wacquant, 1992, p. 218).

Lewis Carroll’s subversive critique of 19th century language and social structure was accomplished via the medium of nonsense poetry and prose (Farrell, 2007). The Alice stories are thought to generate unease in the reader’s mind by exposing an individual’s struggle to “conform to cultural systems to which they are not especially well suited” (Lane, 2011, p. 1030). This thesis wrestles with such a problem; I am such a problem. I must have changed the title more than 50 times. This final version, the joined metaphors of a telescope and Alice in Wonderland as a unifying concept for my contribution, came from my realisation that my writing had changed in focus. Rather than looking outward to the worlds that surrounded me, I had turned the focus inwards, to myself, in a version of Foucault’s ‘Parrhesia’ (1983) the act of “telling the truth without either embellishment or concealment for the purpose of criticizing oneself or another” (Robinson, 2014, p. 4b). When you look through one end of a telescope, you can see a long way into the distance but you can only see a small part of the picture. When you look through the telescope the wrong way scale and perspective are inverted, everything looks... different.

When I was first inspired by the Alice stories as a way of conceptualising what this PhD is about, I was thinking of my struggle to negotiate the tensions inherent in using participatory methods within a powerfully positivistic public health commissioning framework; explored via a detailed study of one particular project. But as I rewrite this introduction now, near to the end of the process, I realise that there was a great deal more to struggle against. Consequently, as Bourdieu and Wacquant portray, I have exposed a great deal in this work; I have been critical of myself, of my discipline and of objectivist notions of research. I expose my
struggle to be more critical still, yet more reflective and more honest, to navigate a path between an inclination towards experimentation and unconventionality in style and structure and desire to actually, well, get a PhD.

The real starting point for this work as it stands today, the point at which I really felt I stood a chance of getting somewhere, was when I read Julie Davis’ (2004, p. 4) account of writing her own thesis, during which she discovered that “writing, like acquiring data, is also a ‘way of knowing’ …a method of discovery and analysis, and that form and content are inseparable”. Writing as a way of knowing. This was rather a revolutionary idea and one that I was keen to explore, mostly because it resonated so strongly with the way that I seem to know. If this really was a thing, a proper academic framework within which to work, then I had found the paradigm for me.

This chapter begins with an explanation of how I came to locate my work as autoethnographic, which began as a pragmatic navigation of various opportunities and constraints and ended in the feeling that I had found the paradigm and method that naturally suited my talents and worldview. Next, I explain how the project that forms the basis of this study came into being, as well as providing some information about the place in which the project was enacted: a deprived neighbourhood. Following this, I offer a brief introduction to the two literatures upon which I draw primarily, social marketing and participatory research, as well as explaining how I perceive that these two approaches to social change are linked. Following this, I articulate the aims of this thesis, which are primarily methodological. I conclude this chapter with an overview of the rest of the document.

1.1. Evolution of the study: Alice and a telescope

Writing as a way of knowing, a creative and generative process with epistemological validity in and of itself (Richardson and Adams St. Pierre, 2005), is indeed a thing. But it is the oasis at end of this story, which started in the wilderness. For a long time I didn't know what I was doing, in the literal as well as
the figurative sense; I didn’t even have a clear idea of my topic or research question. This section explains the thought processes, informed by methodological reading, which led to the selection of autoethnography for this work. I begin with an explanation of some of the constraints that shaped my decisions, followed by a brief explanation of the alternatives that were explored before reaching my current position. In essence, this section tells the story of my discovery of autoethnography.

I have always felt myself to be slightly on the outside in academia, because I am a distracted, only partially present colleague. Like Blake (2007, p. 416) I am an “embodied researcher who must balance work, home and community responsibilities”. I am not an unattached student working full time on their PhD in the traditional academic apprenticeship model, I started part-time study in 2004, when my first child was just under 12 months old. My PhD has dogged my footsteps through the baby and toddlerhood of two children and more recently, it has been balanced with a demanding research post 4 days a week. It has changed shape countless times and been neglected too often. I realised that I would have to do something drastic to salvage it in the summer of 2012 and that drastic something ended up being changing direction (yet again) and making the focus of the PhD a participatory project I had recently finished about risky drinking in two deprived neighbourhoods. I thought this would be wise partly because I had received positive feedback from a range of people that the project was important work, but also I had become fascinated with the whole philosophy of participatory research (see Cahill, 2007a; Cameron and Gibson, 2005; Fine, 2006; Fine and Torre, 2006) and wanted the luxury of exploring it. And in purely functional terms, it made sense because I had access to all the data.

I am not explaining all this in the hope that the reader will judge the work less rigorously because I have struggled to produce it. I am telling this story because the thesis is that struggle; it is me and I am it. One cannot be understood without the other. It is a slice of my ethnographic self (Richardson, 2013; Richardson, 2000; Coffey, 1999). So with the encouragement of my new lead supervisor Tim, I began to write in earnest. I started with the familiar, a social marketing chapter, which is also where I started as an academic. I sketched out other chapters on
participatory research and on social inequality (the latter since integrated into the current structure), but I still didn’t really feel I knew where I was going. Then I attempted the methodology chapter, and stalled. Participatory research is supposed to be a method of action but the action had already happened, some of it had happened more than a year ago. And there was no participation now; there was just me and a big pile of field notes about drinking. I was stymied. Participatory research was not my methodology. I felt that the conventional methodology chapter Tim was encouraging me to write should by rights be my results chapter, because with participatory research, planning, action and results are entwined, inseparable (Davis, 2004; Ozanne and Saatcioglu, 2008). And yes, I could have used my memories and notes from the project to write it as if it was happening right then. But that wasn’t what had happened, and I felt uncomfortable perpetrating what would have seemed to me a deception. I wanted something that felt like authentic scholarship. Not simply to produce something that looked like a PhD ought to look.

At first my reading and writing about action research, the methodology that underpinned the participatory project, seemed to offer a solution. Action research is not a linear approach (Wadsworth, 1998). Instead, the metaphor of a spiral (Kemmis, 2001; O'Brien and Moules, 2007) is often used to explain the process of combining action with research in pursuit of change, but the literature does not proscribe or even describe the spiral as a mechanical series of steps (Kemmis and McTaggart, 2005). Rather, it represents a self-reflective cycle of planning, action, observation and reflection, followed by reassessment of plans, more action and yet more observation and reflection. The reality of action research is messier still, more fluid and responsive, than the neat spiral depicted in the literature (Davis, 2004); initiatives take an unexpected path or end in abject failure and chance meetings lead to unexpected opportunities. Thus, rather than judging the research upon whether it has followed faithfully a set process, success should be determined by whether the researcher has “a strong and authentic sense of development and evolution in their practices, their understandings of their practices, and the situations in which they practice” (Kemmis, 2001, p. 595). This notion of myself as a reflective practitioner seemed
apposite. The project had indeed followed a messy spiral of planning, action and reflection. There had been failure and serendipity. I did feel as though I had evolved in my practice as a researcher.

In the educational action research literature (Leitch and Day, 2000), ‘reflection’ appears to be synonymous with ‘thinking’ in the sense that it relates to acts of cognition that occur during learning (e.g. Csikszentmihalyi and Sawyers, 1995, on creativity and insight). Reflective practice on the other hand can be conceptualised as an “epistemology of practice” (Schön, 1983, p. 49) that consists of three types of reflection: reflection-in-action, reflection-on-action and reflection-about-action. The first encompasses the implicit thought processes that accompany and continually modify and reframe (Loughran, 2002) ongoing practice. The second type relates to a retrospective analysis of past practice so as to gain knowledge about those experiences, engaging in a deliberate and systematic process of analysing one’s actions in context (Moran, 2007; Samaras and Gismondi, 1998) The third type of reflective practice has roots in Critical Theory (Leitch and Day, 2000) and is intended to stimulate reflection upon the social, economic and political aspects of one’s practice (Zeichner, 1993).

Adopting this critical stance, reflection directed outwards to the social system of public health commissioning as well as directed inwards to individual practice would link, I thought, this thesis to ideas of emancipatory action research (Kemmis, 2001).

And thus, my problem of finding an authentic mode of scholarship seemed as though it was answered by the notion that action researchers ‘freeze’ moments of their practice for later reflection, analysis and interpretation (Davis, 2004). These ‘frozen’ moments of practice can be found in the field notes and project documents. And I had all the emails as well, documenting the ebb and flow of our communication and our relations as we negotiated our way through the project. So, I began to reconstruct my researcher self in this vein. In the summer of 2013, I wrote that I wanted the thesis to be judged as a reflective account of practice. But I was still unclear as to the analytical processes I ought to be following. I had put all the data, including the project documents and emails, into NVivo and gone
through the rather laborious process of coding it all. But I found this dissatisfying, unenlightening; even tortuous (Cahill, 2007b, p. 182). In my diary, I wrote:

*Open coding of all this data seems a bit pointless at the moment. Is this activity worthwhile or am I just sticking with it because every researcher starts their training in positivism? (Extract from research diary, 5th November 2013).*

I continued my reading in the reflective enquiry literature, learning that the actual enquiry itself can be carried out in a variety of ways, including autobiography, dialogical conversations, stories (Evans, 1994), reflective writing and journals (Holly, 1989). And here, the trail leads back to Laurel Richardson and writing as a way of knowing.

Investing in that weighty doorstop of a tome the Sage Handbook of Qualitative Research in order to have access to Richardson and St. Pierre’s chapter (2005) opened up a rich world of qualitative enquiry with research conceptualised as a site of toil and transformation, critique and compliance, of play and of serious work. I realised that I was not really that interested in myself as a reflective practitioner of action research, I was much more interested in the research process itself, particularly its potential for challenging convention, for stirring up social action. With some irony though, later this interest spiralled back to the personal: my writing and my ethnographic self. And thus the thesis in its current form was born: a critical, creative and somewhat unconventional autoethnography of an (almost) participatory research process. In this tradition, I felt I could belong.

Therefore, Wonderland exists in layers. At the most intimate is discipline and institution, followed by academia in general: the business of knowledge production and dissemination. Then come the various cultures I navigated during the course of this work: the project itself, a transient world the team created that has passed out of existence save for memory and record; the deprived neighbourhoods in which the project took place and the system of public health commissioning in the region. Finally, the cultures that have influenced my ethnographic self, including what has been described as the ‘fourth wave’ of feminism online (Cochrane, 2013) and the new (to me) world of feminist post-
structuralist qualitative research. Wonderland in its full complexity (or at least the parts of it I discovered during the writing of this thesis) was only revealed towards the end of the writing process, so in this introductory chapter I will only introduce the fragments I saw when I started: social marketing, the project and, briefly, deprived neighbourhoods. The next chapter is a critical review of social marketing with particular attention paid to the context of deprived and socially excluded target groups, the one after that a review of participatory research. For reflections on the wonderlands of academia, feminism, post-structuralist qualitative research and my reconstructed ethnographic self, the reader will have to constrain their curiosity until the final two chapters. I don’t want to spoil the ending.

1.2. We merry band of sisters

This story has a huge supporting cast, all of whom have been kept anonymous in this thesis to preserve their privacy. This section introduces the main characters in the story and their roles, before explaining how the project itself was conceived. Following this, I define some significant terms relating to the project that will appear throughout this thesis: I explain why I have chosen to refer to Parkhood and Urbanwood, the locations for the project, as neighbourhoods rather than communities and I provide a definition of deprivation in the context of UK public health policy.

Only six main characters are named (though pseudonyms for people and places are used to preserve confidentiality), all of whom are females in their late twenties and early thirties. Our similarity in age, gender and professional status was a coincidence, probably, though our commonality of experience and perspective doubtless influenced the project in ways that will be explored in the results chapters. Anna was a Research Associate on a fixed term contract, hers was a field-based role and she became the face of the project in Parkhood and Urbanwood. Louise was Communications Manager for the Primary Care Trust (PCT), the organisation that commissioned the project; her role was to develop and implement communications strategy (including social marketing, but she also
managed all public relations for the PCT) for her patch. Louise has since moved on from this role, though we are still in touch. Sarah was (and is) the local counsellor for Urbanwood and a passionate advocate for the people who had elected her. Phoebe was (and is) co-founder of social impact consultancy ‘Codesigns’, a trained designer and specialist in collaborative engagement and co-design. Codesign Associates are members of Phoebe’s team who supported the project at varying stages in various ways; their skills and roles are explained within the chapter, as they are relevant to the story. Finally me; at the time of the project I was a Senior Research Fellow at a university in a neighbouring city and the principal investigator of the project. The story is always told from my perspective. Before we began working on this project together, Louise, Phoebe and I had joined forces on a collaborative project about breast cancer and before this, in 2010, Phoebe and I had worked together on yet another collaborative project on the topic of lung cancer among men in deprived areas. None of us knew Sarah personally and Anna was brand new.

The project began its life as dialogue in early 2011 between Louise, the director of my research centre and I. The meeting was intended to be a general discussion; Louise had seen my name at a conference and was aware of my project on lung cancer, a topic Louise herself was about to embark upon. In this meeting, we discovered a mutual frustration with the ‘issue-led’ approach to intervention development that appeared to us to be prevalent at the time; an approach that seemed to result in the same few wards being targeted by several health interventions, seemingly created in isolation from one another. Smoking, alcohol, healthy eating and exercise, all aimed at the same few neighbourhoods, all developed independently by different people. We noted that what these neighbourhoods had in common in most cases was deprivation with concurrent high levels of health inequality (Marmot and Wilkinson, 2006).

Frequently in policy documents, reports and academic literature (e.g. see Hastings, Bramley, Bailey and Watkins, 2012; Camina, 2004) as well as in the discourses of public health, deprived areas are written and spoken about as deprived communities, as opposed to neighbourhoods. What do I mean by a deprived neighbourhood and I why do I avoid the term community (which is more
common parlance) throughout this thesis? The construct of community, with its rich and abundant intellectual heritage, is integral to the history of social thought. The word itself can be traced to the 14th century, with origins in the French word comuneté and the Latin word communitatem (Sichling, 2008). However, the word is, according to Paddison (2001), one of the more ‘slippery’ in the social sciences. Much of the discourse on community is concerned with its fate in the wake of industrialisation, urbanisation, migration, market capitalism, technological advances and consumer culture (Muniz and O’Guinn, 2001); what Wellman characterises as “the community question” (1979, p. 1201). It is the legacy of this community question that seems to obfuscate more recent discourses around communities as units of social action. Sentimentality about an old-fashioned folk community, in which relationships were founded in ties of kinship, friendship and the sharing of a common identity (Chaskin, 1997) is important in understanding modern interpretations and is thought to stem from Toennies’ (1887) codification of community versus society (Sichling, 2008). Thus, the term ‘community’, defined as oppositional to individuality, brings with it a raft of associations that can hamper its theoretical and political value as a unit of social analysis. For this reason, the more neutral and uncomplicated term ‘neighbourhood’ has been adopted for this thesis. Neighbourhood is related to community; indeed the two are often used as synonyms. However, a neighbourhood typically relates to the area around a residence where people engage in sets of informal, face-to-face interactions based upon proximity to their homes (Davies and Herbert, 1993). Thus, it is more delineated in spatial dimensions than a community, which can encompass wide geographical distributions of members in, for example, a community of interest like a brand community (Muniz and O’Guinn, 2001).

In contrast, the term ‘deprived’ is relatively straightforward to define. The UK’s Department of Communities and Local Government defines deprivation as “unmet needs caused by a lack of resources of all kinds, not just financial” (p. 2). Resources are grouped into: income, employment, health, access to housing and other services, freedom from crime, quality of living environment and access to education, skills and training (2010). The relationship between deprivation and neighbourhood in this thesis is founded in the observation that there are a
growing number of spaces where the majority of people are disadvantaged (Meegan and Mitchell, 2001); according to Dowler (2001), the UK has more extreme levels of poverty and greater inequality than elsewhere in Europe. As well as a lack of resources, the way in which disadvantage is experienced in these spaces is thought to be qualitatively different: it is more difficult to access services, both public and private; transport facilities are relatively poor; the environment is degraded, for example properties are allowed to fall into poor repair or deliberately vandalised (Meegan and Mitchell, 2001). Not least, there is significant stigma, both social and economic, associated with living in these deprived neighbourhoods.

At the time of our very first discussions, we wanted to explore the possibilities presented by viewing such a neighbourhood as a social ecology (Collins, Tapp and Pressley, 2010) and considering what factors made people appear more likely to need advice about smoking and drinking less, eating a healthier diet, taking more exercise, being more alert to early symptoms of cancer and so on. Interventions based upon these holistic insights could be aimed less overtly at specific health behaviours and more towards tackling the underlying causes of poor health, we speculated. We talked enthusiastically about a community garden, which would provide exercise, social interaction and increase wellbeing, perhaps even increase consumption of fruit and vegetables. This insight isn’t new (see for example Prochaska, Spring and Nigg, 2008), and we knew that community developers have worked this way for many years. But we wanted to use this approach to show that concentrating on each health issue in isolation without acknowledging the underlying structural factors was an inefficient and probably ineffective way to do social marketing. The next section provides a brief introduction to this particular approach to social change.

1.3. Citizenship and soap

Marketing has rather an unfortunate image tainted with a whiff of corporate greed, a touch of artificially inflated consumer desires and a Machiavellian willingness to manipulate in pursuit of economic success. Running contrary to
this unsavoury reputation, fierce debate has raged within the discipline for at least sixty years about marketing’s potential to make a positive contribution to society, accelerated by an article in the Journal of Marketing by Phillip Kotler and Sidney J Levy (1969). Their thesis held that marketing was a pervasive societal activity that could in fact sell citizenship like it could sell soap (Weibe, 1951). Despite attracting censure at the time, Kotler and Levy’s ideas won through and inspired a discipline widely practiced today as social marketing i.e. the application of marketing principles and tools to achieving socially desirable goals (Kotler and Zaltman, 1971; Donovan, 2011). This section provides a brief explanation of social marketing, before clarifying the relationship I perceive between it and participatory research, an orientation to enquiry that aims to combine knowledge and action to stimulate social change (Heron and Reason, 1997).

Two elements of the depiction of social marketing provided in the previous paragraph probably require further explanation: first, marketing itself; and second, the notion of a socially desirable goal. Jack Trout, an eminent self styled ‘Marketing Guru’, writes that marketing is “simply figuring out what you have to do to sell your product or service for a profit” (Trout, 2003, p. 9). The Chartered Institute of Marketing in the UK defines marketing in similar terms, as “the management process responsible for identifying, anticipating and satisfying customer requirements profitably” (CIM, 2009, p. 2). At its root then, marketing is said to be about ability to influence human behaviour (Andreasen, 1994) and, as Gerard Hastings (2007) points out, this ability offers enormous power to anyone equipped with the relevant skills and knowledge to be a marketer. But rather than using this power for profit, as Trout and the CIM describe, social marketers aim to use it to benefit society. In social marketing therefore, the term ‘customer requirements’ becomes rather more metaphorical than literal, as ‘customers’ are the targets of a social intervention to influence behaviour; and a customer’s ‘requirements’ translates to an understanding of why they behave in a way that is deemed undesirable (for example, not eating enough fruit and vegetables) combined with an understanding of what might encourage them to make different choices. Some recent examples of using marketing in pursuit of socially desirable goals include challenging homophobic attitudes (Hull et al., 2013); reducing the
risk of carbon monoxide poisoning (Damon et al., 2013); encouraging children to eat healthily (Stead, Arnott and Dempsey, 2013) and reducing risky drinking among students (Thompson et al., 2013).

As will be explained in the next chapter, social marketing is probably best understood as a method of change rather than a theory or discipline; social marketers are typically conceptualised as “hired guns” (Dann, 2007, p. 54) rather than people with their own political aims (though I challenge this idea later in the thesis). As such, it draws upon other disciplines for technique and underlying theory. Tools and techniques are transferred and adapted from commercial marketing see Peattie and Peattie (2003) for a review. Theory taken primarily from psychology (Andreasen, 1994; Hastings and Saren, 2003; Kotler, Roberto and Lee, 2002), but also and more recently, sociology (Spotswood and Tapp, 2013) typically underpins a social marketing strategy.

The study I present here explores another alternative to the conventional assumption of cognitive, self-reflexive behavioural triggers and to the cultural lens offered by Spotswood and Tapp: participatory or collaborative approaches (Bryant et al., 2007). Collaboration with ordinary people has long been advocated by public health practitioners (NICE, 2008) and in the health promotion literature (Glanz, 2008; Lefebvre and Flora, 1988; Israel et al., 1998). Ideas like community engagement, which assumes public services that involve their users are likely to be of higher quality and more relevant to the communities they serve (SCDC, 2010). Others include co-production, which in the context of public services posits that “people who use services contribute to the production of services”, based on the insight that service users bring expertise and assets which can help improve those services (Needham and Carr 2009, p. 4). Foundations for these methods include community organisation, community participation or the planned approach to community health model (Stead et al., 2007). There is also interest in what has been termed the Assets Based or Community Capacities approach (El-Askari et al., 1998; Kretzman and McKnight, 1996), which focuses on a community’s resources, skills, talents and ideas for generating change, i.e. their potential for social production (Arvidsson, 2008) rather than on their needs and deficits (Sharpe et al., 2000).
A rich and instructive heritage for participatory approaches like these is woven through a variety of disciplines in the social and health sciences, such as education (Kemmis and McTaggart, 2005; Freire, 2000), community development (Fals-Borda and Rahman, 1991), theology (Berryman, 1987) and post-colonial international development (Chambers, 1997; Hickey and Mohan, 2005). The approach that appears to have most in common with recent innovations in social marketing practice (Collins, 2014; Collins, Spotswood and Manning, 2012; Stead, Arnott and Dempsey, 2013; Domegan et al., 2013; Bryant et al., 2007) has been categorised by Hickey and Mohan as “populist” (2005, p. 242) and is influenced by the World Bank Participation Learning Group and UN agencies (Rahman, 1995). This method emphasises the role of the ‘target audience’ - in social marketing parlance - as knowledgeable and capable and repositions agents as facilitators rather than as experts leading change from the top.

Like many methods with very broad application, participatory approaches have many different applications and underlying perspectives. One such perspective that is particularly relevant to this thesis is participatory research, which is not so much a method as a family of methodologies that have coalesced loosely around the dual notions that firstly, knowledge should lead to worthwhile social action (Fals-Borda and Rahman, 1991); and secondly, that there are good reasons to involve research ‘subjects’ as collaborating partners (Kindon, Pain and Kesby, 2007; Cahill, Sultana and Pain, 2007). Thus, divisions between researcher and researched are dismantled (Gaventa, 1988). These approaches are “concerned with developing practical knowing in the pursuit of worthwhile human purposes” (Reason and Bradbury, 2001, p. 1), which might include education, development of consciousness and creativity or expression through projects, acts and struggles (Somekh, 2006).

At core this “orientation to enquiry” (Kindon, Pain and Kesby, 2007, p. 13; Kesby, 2005), which emphasises a constructed reality in which there are multiple perspectives, is founded on these two concepts of action and participation (Davis, 2004). But things aren’t as straightforward as a reliance on an understanding of the words ‘action’ and ‘participation’ might suggest. Perhaps because there is no clear agreement in the literature about what ‘action’ is and its
relationship to social change (Reid, Tom, and Frisby, 2006); and probably more significantly, what change it is intended to stimulate and why (and for whose benefit, Whitehead, 2009). Hotly contested also is the idea that participation is in and of itself a beneficial, and indeed neutral, approach (Cooke and Kothari, 2001; Cooke, 2004; Cleaver, 1999; Hickey and Mohan, 2005; Tandon, 2008). The next section explains the relevance of these concerns and the intended contribution of this work.

1.4. Meat and potatoes

Previous sections in this chapter have included statements about what I have wanted to achieve at various stages, as this study evolved into its current form. These have included a desire to ‘sort out’ the different literatures that touch on participatory as opposed to top-down methods; a desire to explore the culture clash of participatory working within a positivistic framework; the development of a critical, even Parrhesic stance towards my practice and that of other social change practitioners; and the rather more general and philosophical question of how the action happens, how the change that we all speak and write about actually occurs. But in essence, despite the rather complicated journey my ethnographic self has taken, this PhD is about methodology. The remainder of this short section explains the methodological contribution I offer, which can be applied to social marketing research as well as to intervention design.

In the previous section, I explained that collaborative or participatory methods for designing interventions present an alternative to the psychologically or sociologically grounded approaches that have typically underpinned social marketing in the past. Participatory methods might seem particularly attractive in light of some of the criticism that has been levelled at social marketing more recently. For instance, Brenkert’s (2002) critique seems to point directly to participatory methods: he judges social marketing to bypasses the usual democratic processes of grassroots action and political discourse, stripping the targets of behavioural change of their voice and rights to self-determination. Accusations that an overemphasis upon an assumption of rational decision
making at the expense of a full consideration of context can lead to “victim blaming” (Hoek and Jones, 2011, p. 33) and stripping of voice is another relevant criticism, upon which I will expand in Chapter 2.

This is not to say that social marketers are unconcerned with ethics (for a summary, see Dann, 2007; Andreasen, 2001 and Chapter 7). But I argue that the use of participatory methods in themselves do not solve these problems. While it has been suggested that such methods offer a promising way to empower “surplus” (Hickey and Mohan, 2005, p. 239) or “at-risk” populations (Pechmann et al., 2011, p. 23), like the people living in deprived neighbourhoods who are often the target for social marketing interventions (Hastings, 2007), this premise is not uncontested. Indeed, criticism that participation has failed to achieve meaningful change has been mounting over the last decade (Cooke and Kothari, 2001). It has been suggested that mainstream participatory methods may be hampered by inattention to issues of power and politics (Hickey and Mohan, 2005) exacerbated by the problem that such methods may be underpinned by an unsophisticated understanding of the mechanism and constitution of power (Mosse, 1994; Kothari, 2001). Another criticism concerns an overemphasis on local concerns to the detriment of pervasive problems of inequality (Mohan and Stokke, 2000) and a conceptualisation of the relative functions of structure and agency that is inadequate (Cleaver, 1999). Finally, it has been argued that mainstream participatory approaches may be too voluntaristic in regarding any form of participation as superior to non-participatory practices (Chambers, 1997) without considering the risk that those with disempowering agendas may adopt (or co-opt) initiatives that serve their purposes (Rahman, 1995). The issue of co-option in particular adds an interesting dimension to the ongoing debate about the role of commercial organisations in social change initiatives (Lefebvre, 2012).

Drawing upon literature from participatory research, international development and activist scholarship, I locate my contribution to knowledge primarily in the realm of ethics. Specifically, I point to the danger in adopting the radical discourses of participation without challenging some of the foundations of social marketing. I aim, through a critical reflective autoethnographic practice, to expose, “to contest and to reconstitute unproductive, unjust and alienating”
language, discourses and the ways in which power relations are not typically acknowledged in standard modes of commissioning, implementing and evaluating social marketing initiatives, particularly those that claim to be participatory. Problematic assumptions include a tacit consensus of social good, which may draw authority from the perceived legitimacy of the speaker; aided by an assumption of neutral positionality. I synthesise these points into the literature on social marketing ethics, specifically the idea that increasing participation can be a solution to the ethical dilemma of how to articulate a definition of the social good; proposing instead of total reliance on uniform codes, social marketers consider reflexive praxis, which holds ethics as locally negotiated, dialogic and political. I also offer some practical suggestions for policymakers and social marketers wishing to embrace participatory modes of intervention design ethically. The next section explains how my contribution is set out in the remainder of the thesis.

1.5. Overview of thesis

In Chapter 2, I offer a critical review of the social marketing literature organised into three broad sections: the first presents an explanation of what I have termed the orthodoxy for social marketing, this is a depiction you would find in any mainstream social marketing textbook and serves as a basis for the next section, which summarises the critiques that have been offered of this perspective. The final section of Chapter 2 presents three significant influences on the most recent literature, which I have organised into those that coalesce around a systems or ecological perspective, those who favour community-led approaches and those interested in co-creation and collaboration.

Chapter 3 tackles participatory research. I begin with a brief attempt to set the scene before launching into the two main root systems; traditions that appear geographically founded in either the northern or the southern hemispheres, the latter the birthplace of the more radical and emancipatory interpretations of participatory research, from which I take much of my own inspiration. Following this brief history, I explain the ontological and epistemological foundations of
participatory research today, an orientation to enquiry founded on the notion that oppressed and marginalised people’s knowledge can be suppressed by conventional modes of knowledge production. Following this, I present a couple of critiques: firstly, that largely it has ignored other emancipatory research traditions like feminism and secondly, that participatory research is in itself a source of power and therefore should be resisted. Taking up the theme of resistance, I summarise the ways in which institutional bureaucracy can hamper participatory researchers, followed by a brief foray into the territory of activist research. I conclude with an exploration of an important concept, one that has unfortunately received relatively little attention in the literature: the role of space, both metaphorical and physical, in participation.

Chapter 4 segues from one methodology to another, outlining the approach I took to creating the knowledge presented in this thesis. I begin by outlining the ontological and epistemological positions I have adopted, then introduce autoethnography as a methodology. Before explaining in detail the interwoven stories that comprise my research method, I outline and respond to some of the main critiques, of which there are several that cut to the very core of the legitimacy of autoethnography as a valid method. I conclude the chapter with a review of the ethical issues I encountered and how they were dealt with.

Chapters 5 and 6 tell the story of my participatory research project, serving both to communicate and to create the findings of this study. In Chapter 7, I synthesise my findings with the social marketing literature in order to generate a contribution to knowledge in this area. In so doing, I highlight the problem of an assumed consensus that there is a universal social good, drawing upon the critiques presented in Chapter 3 to show how such an assumption can further marginalise people who lack social power. With reference to the importance in participatory research of reflexivity and positionality, I underline a concern that social marketers are speaking themselves out of existence and thus allowing important questions about power, politics and ideology to remain comfortably unasked. Finally, I highlight the danger in an uncritical adoption of the language and process of participation, arguing instead for reconceptualisation of the role of ethical frameworks in social marketing, drawing upon ideas from participatory
and feminist research. In the final chapter, I present a “text of illegitimacy” (Richardson, 1997, p. 137) that arose from my desire to experience more fully the epistemological significance of writing as a way of knowing.
Chapter 2: In which I say where I came from

“How cheerfully he seems to grin, 
How neatly spreads his claws, 
And welcomes little fishes in, 
With gently smiling jaws!”

(Alice, Alice’s Adventures in Wonderland, p. 9)

2.1. Introduction

As an academic ‘discipline’ social marketing is rather an oddity. The term was coined during a fierce debate between prominent academics, sparked by a paper in the Journal of Marketing in 1969 by Philip Kotler and Sidney J Levy entitled ‘Broadening the Concept of Marketing’. Kotler and Levy’s thesis was that marketing was “a pervasive societal activity that goes considerably beyond the selling of toothpaste, soap, and steel” (Kotler and Levy, 1969, p. 10); rather more than the business activity of finding buyers for a firm’s output. “Too far!” retorted David Luck, writing in the same journal in 1969 (p. 53). Luck’s concern wasn’t that the non-profit examples given by Kotler and Levy were dissimilar to commercial applications or were invalid; rather he was uneasy about the implications of broadening the definition of marketing because he couldn’t identify suitable new boundaries. His concern was that “If a task is performed, anywhere by anybody, that has some resemblance to a task performed in marketing, that would be marketing” (Luck, 1969, p. 53). Thus, any organisation planning its services, any individual considering how best to get their point across would be a marketer. In Luck’s opinion, this would not do. Marketers should concern themselves with markets, “of course, and markets must be characterized by buying-and-selling” (Luck, 1969, p. 54). But despite the censure their ideas attracted at the time, Kotler and his colleagues’ ideas won through and inspired the discipline widely practiced today as social marketing (i.e. the application of marketing principles and tools to the achievement of socially desirable goals, Kotler and Zaltman, 1971). Building on the definitions provided in Chapter 1, in
this section I introduce social marketing as an academic discipline in preparation for the review of the social marketing literature that is the substance of this chapter.

According to Andreasen (2002), social marketing has met all reasonable criteria for validity as a field: it has its own textbooks and chapters in generic marketing texts; it has a dedicated journal (and now another, launched in 2011 by Emerald Publishing Group), conferences and research centres. But he acknowledges that it lacks “academic stature” (2002, p. 4), pointing to the absence of any formal social marketing degree. To this I would add that both social marketing journals are of low ranking and my own experience, which has been that social marketing does not have the same disciplinary status as, say, psychology or sociology. I once attended a National Centre for Research Methods summer school and, following the typical introductory small talk about who was from what discipline I was told by an astounded human geographer: “Really? I’m surprised they let you in”.

According to the Higher Education Statistics Agency (2012) social marketing doesn’t even exist; only marketing, which is defined as “techniques involved in the management of an organisation’s relationship with its customers and the world at large” merits a classification. A scan of academic research centres backs up this impression that social marketing is an adjunct of commercial marketing and therefore belongs in a business school. In the UK there are two research centres with an explicit social marketing focus: the Institute of Social Marketing in the University of Stirling’s School of Management and the Bristol Social Marketing Centre in the Faculty of Business and Law at the University of the West of England. Internationally, there is a Social Marketing Institute at the McDonough School of Business of Georgetown University, USA and the Centre for Social Marketing Research in the Faculty of Business at the University of Wollongong in Australia; suggesting social marketing belongs with its commercial cousins in the eyes of most university executives. The only exception seems to be the Florida Prevention Research Center at the University of South Florida, which resides in College of Public Health.
So if social marketing isn’t really a proper academic discipline, what is it? In Andreasen’s terms, social marketing is actually more akin to a methodology, a technique, a hammer with which to hit the nails of social problems (Dann, 2007), rather than a discipline. Thus, it is in “competition” (2002, p. 5) with other methods of social change such as those practiced by psychologists, social workers, educators, community mobilisers, sociologists, media advocates and lobbyists. Often, this technique that social marketers practice is referred to as an ‘intervention’ (Andreasen, 2002; Brennan, Voros and Brady, 2011). According to the Oxford English Dictionary, the term can relate to interference by a state in another’s affairs (for instance, a military intervention); an action taken to improve a medical disorder (a surgical intervention); or an occasion upon which a group of friends or family members elect to confront a person with an addiction or other behavioural problem, in an attempt to persuade them to address the issue (2013).

Health and social care ‘interventions’ might occur in a primary care (Solberg, Maciosek and Edwards, 2008; Babor and Higgins-Biddle, 2000) or family setting (Moran, Jacobs, Bunn and Bifulco, 2007) and are typically delivered by professionals or people who have received training in the intervention technique (Lewin et al., 2006). That social marketing appears to have adopted with relatively little reflection the neutral, medicalised interpretation of the ‘intervention’ is not surprising given that term’s roots in public health (Lefebvre and Flora, 1988), but is interesting given that in the language of participatory research, the term means an aspiration for radical changes in society. Changes in areas like social and economic structures, employment rights and rights more broadly: to property, education and health and conversely also “to the reactionary position whose aim is to immobilize history and maintain an unjust socio-economic and cultural order” (Freire, 2001, p. 6). Nevertheless, it is the relatively value neutral interpretation of the term that has become prevalent to describe social marketing projects: as attempts to intervene in a particular situation in order to change individual behaviour.

The review of social marketing literature in this chapter is organised around three broad themes. I begin in part one, with a relatively brief explanation of the core
principles of social marketing as advocated by the most influential scholars in the field, which I regard as the current orthodoxy in the sense that these are the principles most likely to underpin introductory training courses in social marketing and to form the backbone of social marketing texts written for practitioners. Part two summarises the foremost criticisms of this orthodoxy, namely that it is founded upon an overly simplistic construction of a rational individual motivated by self-interest, that it fails to account for social and health inequality in a meaningful way and that the customer orientation metaphor can be obfuscatory. Part three presents what appear to be the primary contenders for developing the field, which can be grouped into two categories: systems or macro-social perspectives and theories of participation and co-creation.

2.2. Part 1: Orthodoxy

As well as delineating social marketing as a sub-discipline of marketing, as explained in Chapter 1, proponents have been faced with the task of explaining to the wider field of behavioural and social change what social marketing is and what differentiates it from other approaches. This section explains the key principles of social marketing as set out in the literature, with particular attention to the concept of exchange.

In attempting to demarcate territory for social marketing in the wider behavioural and social change space, scholars have proposed various typologies and markers of identification that have come to be accepted as an orthodoxy for many social marketers. In the most simple terms, Andreasen (2006) explains that social marketing has three elements: firstly, a customer orientation (which he describes as “slavish attention to target audiences”, p. 94); secondly, a sensible process for organising and implementing social marketing campaigns; and thirdly, the application of relevant concepts and tools, examples being the Stages of Change model (Prochaska and DiClemente, 1984) or the Model of Goal Directed Behaviour (Ajzen and Madden, 1986). Earlier work by the same author (2002) identifies six benchmarks of social marketing that have been widely adopted in other literature (see for example McDermott, Stead and Hastings,
2005; Stead et al., 2007): (i) that achieving a change in people’s behaviour is the ultimate goal against which interventions should be judged (Lefebvre, 2012; Andreasen, 1994; Stead et al., 2007; Blair-Stevens, Reynolds and Christopoulos, 2010); (ii) that projects consistently use audience research throughout the intervention process (Maibach, 2003; French and Blair-Stevens, 2010); (iii) that target audiences are segmented (i.e. that a heterogeneous population is split into smaller, identifiable groups that share relevant geo-demographic or psychographic characteristics, so that they may be treated differently); (iv) that the creation of attractive exchanges (Bagozzi, 1975) is a central element of any strategy (see also Noble et al., 2007; Andreasen, 2002; Peattie and Peattie, 2003; Laczniak, Lusch and Murphy, 1979; Pels, 1999); (v) that all four “Ps” (Product, Price, Place and Promotion, which in social marketing might be analogous or literal) of the marketing mix are considered in the development of any strategy (Maibach, 2003; Grier and Bryant, 2005); (vi) that factors that might constitute ‘competition’ for the desired behaviour change are accounted for (Peattie and Peattie, 2003; Hastings, 2003a; Grier and Bryant, 2005; Kotler, Roberto and Lee, 2002; Peattie and Peattie, 2003; Peattie, Peattie and Clarke, 2001; Valentino, Beckmann and Buhr, 2001; McKenzie-Mohr and Smith, 1999). Yet another principle that has featured in the social marketing literature is the notion that any behaviour change must be voluntary (see Grier and Bryant, 2005). This way of working has been called the “marketing-to” approach (Lusch, 2007, p. 261) because the process tends to be managed by “experts” who attempt to act upon a target audience that is assumed to be passive.

The notion of exchange is foundational to conventional conceptualisations of social marketing. Economic exchange is a straightforward idea: two parties each possess something that may be of value to the other (for example, one has food, the other currency) and they agree to exchange one for the other; a process Peattie and Peattie term “a mutual and dependent transfer of value” (2003, p. 369). The exchange paradigm emerged early in the marketing literature as a helpful framework (Bagozzi, 1975) and in Bagozzi’s view, exchange was the crux of contemporaneous debates on broadening the concept of marketing. Those in opposition to the broadening movement argued that marketers should concern
themselves with the most fundamental forms of exchange, that found within markets, “of course, and markets must be characterized by buying-and-selling” (Luck, 1969, p. 54). Though more recent work on markets themselves would suggest that they are anything but simple, as will be explored in section 2.4.

Exchange is assumed to function via the medium of self-interest, whether conscious or unconscious (Nord, 1973; Homans, 1958). Bagozzi (1975) identified three types of exchange: restricted, generalised (a distinction he attributes to anthropologist Claude Levi-Strauss, 1969), and complex exchange. Restricted exchange is dyadic and, at the time, was thought to underpin much of the marketing literature. Two principles of this type of exchange are identified: first, that it is characterised by an attempt to maintain a perception of equity between the two participants. This is primarily for reasons of self-interest on the part of the marketer, as they are assumed to “know that they will not obtain repeat purchases if the consumer is taken advantage of and deceived” (Bagozzi, 1975) p. 33). The second principle is that of reciprocity; a concept Bagozzi describes as the “quid pro quo” (ibid.), i.e. the knowledge that something of value must be exchanged for something else of value. Conversely, generalised exchange does not conform to a typical understanding of quid pro quo; instead it represents a system within which each actor imparts but instead of exchanging directly, they receive from someone “other than to whom he [sic] gave” (p. 33). Bagozzi uses an example of a commercial organisation making a charitable donation: if a potential customer notices that the donation has been made and then does business with the donor, a generalised exchange would have occurred. Complex exchanges are defined as a system of relationships within which multiple exchanges occur. Bagozzi’s example here is the complex circular exchange that is theorised to occur when a viewer watches a television programme. This theory rests upon the validity of an intangible exchange of value, such as the viewer’s time and attention in exchange for entertainment or information. The complex system comes into play with the involvement of the advertiser and advertising agency that place advertisements and potentially attract new business as a result of the viewer’s willingness to exchange entertainment for their attention.
Many social marketers argue that their work involves the provision of “meaningful benefits” (Noble et al., 2007, p. 395), which might include products, information, a compelling argument or even an incentive; in exchange, the target audience changes their behaviour (Andreasen, 2002). Described thus, this theory of exchange is closest to restricted exchange, a private transaction between social marketer and behaviour change target. Such approaches are thought to appeal because they parallel simple theories of commercial exchange (Peattie and Peattie, 2003), a vote in exchange for a promise of a particular political action (Laczniak, Lusch and Murphy, 1979) for instance. Other social marketers argue for a conceptualisation that is closer to complex exchange as it acknowledges the existence of differing exchange paradigms (Pels, 1999) exemplified in social marketing in the relationship between ‘upstream’ and ‘downstream’ targets (see for example Hastings, 2003b; Hastings, MacFadyen and Anderson, 2000). Thus exchange appears to have been accepted, with relatively little critical reflection, as a core principle of social marketing (French and Blair-Stevens, 2010). It has been suggested that this acceptance has come to pass more on the basis of habit and adherence to marketing management dogma rather than the creation of a compelling argument (Peattie and Peattie, 2003).

However, some argue that there are fundamental differences between commercial and social marketing that make direct and uncritical transfer of theories and techniques problematic (Peattie and Peattie, 2003), particularly as social marketing is regarded as the trickier “game to master” (Bloom and Novelli, 1981, p. 87; Goldberg, 1995; Andreasen, 2012). Bloom and Novelli (ibid.) identified a number of mainly practical challenges, such as the difficulty of accessing reliable data with which to segment. Andreasen (2000) goes further, pointing out elemental obstacles faced by social but not commercial marketers, including non-existent or even negative demand for changes that frequently ask for a great deal of effort and involvement from ‘customers’ of the change programmes; in return for which they receive benefits that are deferred, invisible (i.e. the benefit is in the avoidance of a possible negative outcome, rather than accruing a positive one) or that are to the advantage of third parties. In addition to the challenge of social marketing’s propositions being difficult to ‘sell’, Hastings
(2003a) notes that social marketers and public service providers are frequently tasked with targeting the most at risk (Pechmann, et al., 2011), disempowered, base of the pyramid (Prahalad, 2005) ‘customers’ whose choices are greatly restricted by a lack of wealth, opportunity, literacy, political power and access to markets (Santos and Laczniak, 2009; De Soto, 2000). Conversely, commercial marketers have greater freedom (Heskett, 1986) to ignore such ‘customers’ with impunity.

The earliest debates in social marketing focused on whether it was appropriate to expand definitions of ‘the marketing concept’ beyond commerce (Luck, 1969; Tucker, 1974). Contemporary issues appear to be less concerned with whether marketing can be used for social aims, but how best to apply it to that purpose. This debate is being conducted at several levels, with the most attention being paid once again to issues of definition and scope: we are still arguing amongst ourselves about what social marketing is and how it should be applied (Spotswood, 2013). Part two of this chapter, which follows this section, is organised thematically around these points of contention, starting in section 2.3 with the least controversial criticism of Kotler’s original vision: that social marketing is excessively concerned with psychological drivers of individual behaviour and therefore pays too little attention to context.

2.3. Part 2: Heresies

Context is all

The principles of social marketing outlined in the first section are myopic, according to a thought provoking and precognisant article by Thomas Tucker (1974). In his opinion, by viewing the consumer at a micro level Kotler, Levy and Zaltman (1969; 1971) were creating a concept of marketing based solely upon the viewpoint of the marketing manager without consideration of the wider societal context. This concern is borne out by more recent criticisms, as this and subsequent sections will argue.
It can be argued that models of behaviour change, enacted by individuals who are free to make intelligent and informed decisions (Hoek and Jones, 2011; Kennedy and Parsons, 2012), via the medium of simple exchange and predicated upon an assumption of rationality as a primary driver of behaviour (Homans, 1958), have underpinned a significant portion of theory and practice in social marketing. Popular behavioural theories include the Theory of Planned Behaviour (Ajzen, 1991; Holdershaw, Gendall and Wright, 2011; Stead et al., 2005); the Health Belief Model (Rosenstock, 1974; D'Souza et al., 2011; Earp, 2002; Ross et al., 2011) and Social Cognitive Theory (Bandura, 1997; Stead, Gordon and Angus, 2007; Baranowski et al., 2003; Beech et al., 2003). Further, it can be said that reliance upon the individualistic paradigm encourages an overemphasis on cognitive decision making based upon a conception of people as rational utility maximisers (Shanker, Cherrier and Canniford, 2006) and thus neglects the emotional and even sub-conscious habitual drivers of behaviour. According to Goldberg (1995), this psychological paradigm invites the bias of treating the behaviour as the ‘figure’ at the expense of the ‘ground’ (Winett, 1995). This may have the consequence of overemphasising individual agency (i.e. the capacity of individuals to act, individually or collectively, as agents on their own behalf, Ritzer, 2008; Ritzer and Gindoff, 1994) over structural factors like education, social class, customs and norms (Angel, 2011). Thus calling into question the notion that behaviour change can always be ‘voluntary’ and indeed whether it is fair to hold the individual responsible for behaviour change (Raftopoulou and Hogg, 2010; Brenkert, 2002).

An alternative perspective recognises the complexity of human behaviour, emphasising sociality and acknowledging that people are consciously and subconsciously shaped by others (Franzoi, 2000; Deutsch and Gerard, 1955) and by their environment (see for example Dahlgren and Whitehead, 2007). This acknowledges that behaviour is often influenced by contextual factors beyond an individual’s control (Goldberg, 1995; Hastings, MacFadyen and Anderson, 2000; Peattie and Peattie, 2003). Calls to recognise the role of context are perhaps the least contentious critique of mainstream social marketing theory presented here and this perspective is gaining considerable momentum in the literature.
(Andreasen, 2006; Lefebvre, 2012; Hastings and Donovan, 2002). However, models like Social Ecology (Collins, Tapp and Pressley, 2010), which is founded on the ecological paradigm (McLeroy, Bibeau, Steckler and Glanz, 1988) and offers a systematic way of studying behaviour in context (Dresler-Hawke and Veer, 2006; Elder *et al*., 2007), have yet to become as popular in social marketing as psychological or psychosocial behaviour change models. A different ecological metaphor for social marketers, the construct of a market, has also surfaced recently in the social marketing literature (Lefebvre, 2012) and will be discussed in subsequent sections.

This section has summarised the arguments in favour of a conceptualisation of social marketing that accounts for structural drivers of behaviour as well as psychological motivations. The next section expands upon the issue of inequality as relates to social marketing.

**All being equal...**

“We all know the problems of our poorest neighbourhoods - decaying housing, unemployment, street crime and drugs. People who can, move out. Nightmare neighbours move in. Shops, banks and other vital services close” Prime Minister Tony Blair (Social Exclusion Unit, 1998, p. 7).

It is acknowledged by many social marketers that behaviour is influenced by context as well as cognition and, as explained in the previous section, often the people social marketers are asked to target with their behaviour change interventions suffer restrictions upon their ability to exercise agency. This section expands upon a contextual factor that is particularly relevant to the project in Parkhood and Urbanwood that forms the setting for this study: living in relative poverty in an urban deprived neighbourhood.

There is widespread consensus in the literature that the worse someone’s socio-economic position, the worse their health is likely to be (Graham, 2007); numerous factors contribute to this health inequality which may not be in themselves spatial (Glennerster *et al*., 1999), rather they are shaped by broad social, political and economic forces (CSDH, 2008) such as housing policy and access to health and social care (see Bambra *et al*., 2010, for a review). For
example, Townsend, writing in 1979, points out that regardless of the way deprived areas are defined, “unless we include over half the areas in the country, there will be more poor persons or poor children living outside them than in them” (p. 560). Nevertheless, despite strong arguments for macro causes of poverty and social exclusion, there is thought to be compelling evidence of a relationship between space and inequality. Societal increases in prosperity, for example, do not necessarily trickle down to the poorest areas (Glennerster et al., 1999) and it has been observed that certain geographic areas are particularly associated with health inequality (Atkinson and Kintrea, 2001). One explanation for this can be found in the lack of power that results from social exclusion; people in deprived areas have limited access to whatever is driving the increased prosperity for the more fortunate groups. Another factor is thought to be the free operation of land and property markets that reinforce the social and spatial segregation of rich and poor people (Madanipour, 1998). Theories of the free market hold that this inequality in urban areas is cyclical: as the urban landscape changes, property values rise and fall naturally (Glennerster et al., 1999).

As well as lower life expectancy (Shaw et al., 2005), residents of so called “underprivileged” (Meegan and Mitchell, 2001, p. 2176) neighbourhoods have a low sense of belonging, fear for their personal security, low expectations of mutual support from neighbours and little sense of belonging (ibid.) as well as low self-worth (Kawachi, 2000). The way this exclusion can be concentrated spatially, allied to the stigma associated with living in such a neighbourhood, is believed to exacerbate powerlessness because spaces themselves exist to function as units of state organisation and power (Madanipour, 1998).

This stigma is said to contribute to the phenomenon of social exclusion, which piles a lack of access to public resources and decision-making (Madanipour, 1998) onto a plate already full with economic disadvantage, resulting in disempowerment. Further, the way this exclusion is concentrated spatially is believed to exacerbate powerlessness because ‘underprivileged’ people are less likely to be able to access the services they need outside their neighbourhood or by accessing commercially available solutions (Thomas, 1991). This powerlessness has a number of documented psychological, sociological and
cultural effects, such as increased aggression and feelings of stress (Abrams, Hogg and Marques, 2005). For these reasons, it can be argued that it is both unfair and counterproductive implicitly to blame individuals experiencing this inequality for their ‘bad’ lifestyle choices (Marmot, 2010; Green, 1984) by targeting them with broad brush social marketing strategies founded upon a metaphor of exchange between equals and thus predicated upon high levels of individual agency.

Having set out the particular challenges faced by people who live in deprived neighbourhoods, the next section examines critically the customer orientation metaphor that underpins orthodox social marketing thought, with particular emphasis on its implications for marginalised groups, like those living in deprived neighbourhoods.

**All hail the customer**

Earlier, I suggested that in social marketing the notion of a ‘customer’ is rather more metaphorical than literal and in the previous section I argued that not all of social marketing’s customers are created equal. In this section I subject the customer orientation metaphor to critique on the basis that it constructs a situation whereby voluntary exchange is assumed to occur between parties of equal standing and agency; which may be unhelpful, and indeed unfair, in some situations.

Metaphor invokes similarity as well as contrast between disparate concepts (Cornelissen, 2003), as with the customer orientation metaphor, which conceptualises people in the role of a ‘customer’ for interactions ranging from the delivery of public services to the design of unwelcome interventions to change behaviour. The notion of a customer orientation is thought to resonate well with public services (and more recently social marketing, Russell-Bennett, 2012), because the idea that ‘customers’ should be ‘served’ corresponds with an assumed desire of public servants to help the public (Fountain, 2001). The special characteristics of services – intangibility, inseparability (which has been linked to co-production and heterogeneity, Zeithaml, Parasuraman and Berry, 1985) – familiar as they are to services marketers, bring the role of the customer
as the ultimate arbiter of successful delivery to the fore in ways that other conceptualisations, like citizenship, may not. It is evident that some public service delivery has been improved through the adoption of services thinking. Russell-Bennett (2012), for example, describes a case in which applying the principles of service marketing improved blood donation rates by increasing flexibility of appointments and improving communication.

However, the emphasis upon customer satisfaction associated with this metaphor carries an implicit notion that the customer is being privileged, even empowered in some way, by their role as the focus of the social marketer’s efforts. This observation appears increasingly relevant given the trend in the marketing and consumer research literature to assume that power is shifting from producers to consumers (Shanker, Cherrier and Canniford, 2006). In the case of interventions targeted at people who may not actually want to change their behaviour (as was found to be the case with people living in deprived neighbourhoods in relation to physical activity, Spotswood, 2011) the customer orientation metaphor stretches further still, particularly when the rhetoric of voluntary change is maintained (Albrecht, 1996).

As well as the potential for constructing a false perception of the ‘customer’s’ role outlined above, a metaphor founded upon customer orientation may serve to obscure the complexity and dynamics of power that surround behaviour by creating the impression of a dyadic relationship between the service provider and their customer when in fact, in both public services and social marketing, a network of customers and clients – otherwise described as stakeholders – can be found upstream, midstream (Lagarde, 2012) and downstream in the form of funders, policymakers and influential groups, as well as the eponymous customer of the behaviour change effort. Similarly, a recognition that historically, theoretical approaches to market orientation privileged one stakeholder – the customer – above others (Ferrell, 2004); allied to the an observation that stakeholder relationships are both complex and worthy of attention (Maignan, Ferrell and Ferrell, 2005) has been accepted for some time in the commercial marketing literature (for a review, see Hult et al., 2011).
Yet another implication of a customer orientation metaphor is that it can imply an assumption of rational, free choice and thus can invite those that have little knowledge of the realities of being part of a marginalised group to fall into a counterproductive ‘victim blaming’ ideology (Green, 1984; Hoek and Jones, 2011). It can be tempting to wonder, for example, if people know that risky drinking has poor health and social outcomes, why they do not reduce their consumption. Do they not care about their health or their family? Are they too foolish to understand the implications of their actions or too lazy to change? This way of thinking is not only unrealistic because it assumes that a ‘free choice’ exists in most circumstances, but it is undesirable because it hampers understanding of the link between behaviour and context. A related problem may be that while the customer service metaphor may help to create operational improvements in the delivery of public services, it is thought likely to exacerbate inequalities by obscuring outcomes that render some customers less powerful than others (Fountain, 2001). One reason for this concern is the potential for public servants to serve those who are easier to serve (see for example Bohte and Meier, 2000, on goal displacement). Similarly, a core principle of social marketing, segmentation, is thought to place public service providers on a “slippery slope” (Fountain, 2001, p. 63) of ambiguity in decision making about resource distribution among segments. These issues may also risk encouraging social marketers towards too narrow a focus upon their ‘customers’ at the expense of wider, structural problems.

So far in this chapter, I have outlined what I term orthodoxy for social marketing, predicated upon stimulating voluntary change in individual behaviour via the medium of exchange. I have presented three distinct but interlinked critiques of this orthodoxy, namely that it fails to account adequately for social and environmental context; that it assumes high levels of individual agency when structural inequalities and social exclusion may limit the ability of people to exercise it; and that conceptualising the targets of a behaviour change effort as customers may obscure the dynamics of power and invite an overly simplistic perspective on how change can be stimulated. In the particular context of this thesis - deprived urban neighbourhoods - these critiques invite the conclusion
that, at best, social marketing is rather pointless and at worst, it has the potential
to do harm. By this I mean that in some circumstances, as well as documented
unintended consequences such as stigmatisation of obesity (Carvalho and
Mazzon, 2013), an uncritical application of orthodox social marketing in deprived
areas could allow those in power implicitly to blame people for their behaviour
and for their failure to accept what could be regarded as an attractive package of
benefits in exchange for behaviour modification. Taking this thought to its ultimate
conclusion leads to the worrying possibility that individualistic perspectives on
behaviour change could lead to explicit blame and ultimately withdrawal of state
support from socially excluded and disadvantaged groups on the basis that they
have snubbed all reasonable offers of assistance. But this line of thinking is
sailing close to personal ideology rather than academic literature, so while I will
return to political and ethical considerations in subsequent chapters, I will cease
these musings for the time being. Part three offers a critical review of the most
recent thinking on how social marketing should develop as an approach to
planned social change.

2.4. Part 3: Revolutionaries

The holists

The previous sections laid out a number of criticisms of orthodox social
marketing; which all point, somewhat uncomfortably, to the possibility that social
marketing may exacerbate inequalities rather than help to resolve them. These
concerns are not new, and revolutionary perspectives have been gaining traction
in the literature for several years. I have grouped these broadly into two
categories: those arguing in favour of an holistic approach to planned social
change, which is the topic of the remainder of this section; and those advocating
more open, participatory and communitarian approaches like that employed in
the project in Parkhood and Urbanwood. The latter is reviewed subsequently.
These orthodoxy-challenging approaches are at an early stage of development in
social marketing, though they draw upon other disciplines where the theory is
more mature. Consequently, the literature is fragmented and typically associated with a particular individual or group’s perspective.

The holistic approach is exemplified in the Total Market Approach (TMA), which dispenses with the market as a metaphor and engages head on with complexity via systems thinking (Domegan and Hastings, 2012), suggesting that the role of social marketing may be to provide a framework for a planned approach to social (rather than commercial) innovation (Lefebvre, 2012). The TMA, which Lefebvre has adopted as a sort of ‘brand’ for systems thinking, actually emerged in low-income countries, where social marketing programmes typically offer subsidised products, such as contraceptives (Drake et al., 2010; Drake, 2011). Thus, the TMA aimed to bridge “gaps between the public, NGO and private sectors” (Lefebvre, 2011, p. 66) in situations where vulnerable or at risk consumers need protection from market failures. ‘At risk’ consumers are defined as “marketplace participants who, because of historical or personal circumstances or disabilities, may be harmed by marketers’ practices or may be unable or unwilling to take full advantage of marketplace opportunities” (ibid, p. 23), i.e. consumers that have been excluded from full participation in the market.

However, Lefebvre’s (2012) more general articulation of the TMA perspective acknowledges that it is helpful to view behaviour as multifaceted and context laden rather than rational and driven by relatively simple causes, and this means the range of actors and their interrelationships under consideration grows in scale and complexity. Lefebvre lists the private and third sectors, government and policymakers, formal organisations and informal networks who choose to participate (or not) in a complex network (Lusch, Vargo and Tanniru, 2010), comprised of groups of micro institutions and their societal context (Bartels and Jenkins, 1977). All this is conceptualised as a ‘marketing system’ (Lefebvre, 2012; Dowling, 1983; Duhaime, McTavish and Ross, 1985).

Inspired in part by the work of the Stanford Center for Social Innovation (Phils, Deiglmeier and Miller, 2008), the TMA is defined as “the application of marketing principles to shape markets that are more effective, efficient, sustainable and just in advancing people’s well-being and social welfare” (Lefebvre, 2012, p. 120;
Lefebvre, 2011; Hastings and Domegan, 2014). In harmony with the House of Lords enquiry into behaviour change (2012), which noted the need for more research on the subject of multi-disciplinary approaches, TMA appears to be a way of operating in response to complexity that may call upon communication, regulation, finance or other strategies, including collaboration with commercial participants. In fact, any technique that might advance social welfare “through the application of marketing principles” (Lefebvre, 2012, p. 120) is fair game. It could be argued that this approach conceptualises social marketing as integral to the policy process, rather than a mechanism for behavioural change brought in at the end.

It seems relevant here to offer an observation about the difference between the way commercial and social marketers position themselves in the hierarchy of their respective spheres: On the one hand, much social marketing theory is positioned as a tool to be wielded by heavyweights like epidemiologists and policymakers. Commercial marketers, on the other hand, demand a seat at the boardroom table (the Chartered Institute of Marketing calls it a “key management discipline”, 2009, p. 2). Lefebvre appears to be proposing that social marketers ape their commercial counterparts in asking to be promoted to conductor of the orchestra of disciplines whose aim is to improve people’s lives. If social marketers were in this strategic position, integrated with the entire policy process, then hypothetically they could ask for changes to housing policy, large-scale investment in adult education programmes, better support of young parents and people struggling with mental and physical health problems in deprived areas. They could develop efficient, low-cost public transport and invest in organisations working to build community capacity, which would probably improve the situation with regards to the sorts of behaviours that social marketers are often commissioned to tackle, such as smoking and drinking. But it seems unlikely that social marketers will be given such a remit.

Lefebvre suggests that the TMA will encourage social marketers to think less about individuals and more about the marketplace in their analysis and implementation. Given this shift in focus from individual to system, it seems helpful to explore the idea of a market, the core construct that social marketers
are advised to use in their planning, in a bit more depth. There is a vast literature on markets, spanning the social theory of Max Weber (Swedberg, 2000); economic sociology (Smith, 2007); morality and market failure (Zak, 2011); political science and foreign policy (Francois and Wooton, 2010) and financial economics (Ikenberry, Lakonishok and Vermaelen, 1995) to name just a few examples. This breadth and depth of theory suggests that a market may not be as simple a framework as Lefebvre’s early work might suggest. In a neo-classical interpretation, markets are thought to exist in a state of perpetual evolution driven by changes in the entities and the relationships between them; rather like Heraclitus’ doctrine of perpetual flux “nothing ever is, everything is becoming” (Russell, 2004, p. 52). Unlike Heraclitus though, who is thought to have believed in the strife of opposites coming together, Storbacka and Nenonen (2011) suggest that markets fall naturally into harmony, consistency and “fit” (p. 243) between interdependent elements (Meyer, Tsui and Hinings, 1993; Miller, 1996). Thus, as Lefebvre pointed out, markets can be conceptualised as a form of self-correcting ‘business ecosystem’ (see also Vargo, Maglio and Akaka, 2008) that always returns to stasis (i.e. punctuated equilibrium, Gould and Eldredge, 1993).

These hints at a neoliberal belief in “the magic of the marketplace” (Nitsch, 2001, p. 445) to establish fairness and balance seem juxtaposed conceptually with Lefebvre’s thesis that the market itself (its rules and practices) needs to be changed so that it functions not simply for the profit of powerful stakeholders, but “to better serve the needs of individuals and society through the application of marketing principles” (p. 120). Thus, advocates of TMA appear to propose a market driving, as opposed to the more typical market driven philosophy (the former characterised as attempting to influence the structure of a given market in a way that enhances one’s ‘competitive position’, or in this case, social welfare and the latter the more traditional approach of understanding and reacting to the existing structure of a given market, Jaworski, Kohli, and Sahay, 2000). This is important because of its relationship with the problem of inequality and social exclusion identified earlier in this chapter: if an ideology underpinned by the modernist axiom of freedom through choice (Shanker, Cherrier and Canniford, 2006) holds that markets themselves are an efficient mechanism with which to
allocate resources, without taking into consideration that structurally some groups are excluded from participation, then the TMA will be no better at dealing with inequalities than orthodox conceptualisations of social marketing.

Kennedy and Parsons (2012) tie systems approaches like the TMA explicitly to politics, suggesting that when a government undertakes marketing as part of a long-term strategy of behaviour change, in concert with policy and regulatory changes, taxation, community mobilisation, research or training and education, it is actually engaging in positive social engineering. Social engineering is a term that has negative associations, with the societal and behavioural modification that occurred under totalitarian regimes in Soviet Russia and Maoist China (McMahon, 2002). However, Kennedy and Parsons seek to redefine it as a process that involves “arranging and channelling environmental and social forces to create a high probability that effective social action will occur” (Alexander and Schmidt, 1996, p. 1). In other words, an approach that attempts to address enabling conditions (such as political or economic forces); precipitating circumstances (factors that might predispose people to accepting a new idea or to innovate, Duhaime, McTavish and Ross, 1985); societal motivation (that even if there is strong pressure for change, it will only occur if there is willingness and energy in society). Finally, actions are the proposed fourth element in the systems approach, which could include a social marketing intervention, as well as other government actions. It is interesting to note that these four factors that underpin Kennedy and Parsons’ positive social engineering approach arose from research into societal change and modernisation projects in developing countries in the 1960s and 1970s (Dube, 1971). Social engineering, thus defined, has moved a considerable distance away from the creation of attractive propositions to offer to rational beings in exchange for voluntary behaviour change. Further, it appears to deal with the issue of complexity decisively, by embracing and indeed exploiting it, rather than seeking to reduce it to measurable variables.

All of the ideas covered in this section seem to point towards the creation of a new, albeit many would argue better, structure within which individual choice is shaped (Thaler and Sunstein, 2008). This may not necessarily be a problem in itself, however it remains somewhat paternalistic in the sense that an unidentified
‘other’ is empowered to decide upon how individual wellbeing and social good is defined (in the case of social engineering as understood here, this would be the government, but the arbiter of social good isn’t specified in the literature on systems thinking and TMA), and then to enact these, potentially without recourse to public scrutiny or any sort of democratic process. An alternative perspective claims directly to engage with issues of choice and empowerment, seeking to collaborate openly with the ‘target audience’ of behaviour change programmes to create, with them, solutions that increase personal and social welfare. This will be discussed in the next section.

**The communitarians**

"Relearning some old lessons about fairness and participation," the UN says, "is the only way to eventually overcome the crisis and pursue a path of sustainable economic development" (UNCTAD, 2012, p. 167). One such lesson appears to be the value of working with communities. According to Raco and Flint (2001), this idea has underpinned local reorganisation by the UK government over the last 30 years. The UK Coalition Government, which came into power on 11th May 2010, emphasised its Big Society policy, “working to help people take action to solve their own problems and create strong, attractive and thriving neighbourhoods” (DCLG, 2013). This section begins with a brief introduction to the wider policy context of community-based approaches, before homing in on how methods inspired by community organising and community development have emerged in social marketing.

Policy thinking that emphasises the role of the active citizen and community as partners in local service delivery is by no means new (DETR, 1998). An active citizen is one who is defined as a democratic agent, empowering themselves by offering a challenge to the organisations and institutions that shape their lives; their power is not located in actual or metaphoric consumerism (Raco and Flint, 2001). This appreciation for geographically targeted community or neighbourhood-based interventions (Chaskin, 1997) does not appear to have slowed in the 15 years. Rather it has evolved further, with approaches like the Asset Based or Community Capacities approach (Mathie and Cunningham,
2003; El-Askari et al., 1998; Kretzmann and McKnight, 1996), which emphasise a community’s resources, skills, talents and ideas as opposed to their needs and deficits (Sharpe et al., 2000), growing in popularity. The term ‘community’ appears to be a cornerstone of these approaches and consequently, despite my preference for the term neighbourhood, as explained in section 1.2, it seems sensible to provide some elaboration of this ubiquitous idea.

According to (Camina, 2004), analysis of communities is frequently undertaken dichotomously from either a predominantly socio-political perspective or one informed by a spatial or physical understanding. Sichling, (2008) however, suggests that the term ‘community’ can have five distinct purposes: to signify a unit of social organisation, to distinguish among social classes, to locate people geographically, to denote shared interest and to indicate shared identity and characteristics. Meegan and Mitchell (2001) suggest a similar range of factors by which communities can be identified: geographical proximity, common interest, conduct or social relations and physical or social character.

Another stance is found in the tension between “the producers of space and the makers of place” (Taylor, 1999, p. 12). It is argued that ‘spaces’ are units of state organisation; artificial entities created to deliver services efficiently and assert governmental power. Conversely, ‘places’ are constructed through human interaction, engagement and sociality “as the simultaneous coexistence of social interrelations and interactions at all spatial scales” (Massey, 1994, p. 168). Thus, they mediate interactions between individuals and within communities, functioning as a locus of being (Raco and Flint, 2001) for social relations, part of the “routine, taken-for-granted nature of everyday life” (Giddens, 1995, p. 64). Even within relatively small areas geographically speaking, places can adopt a variety of form and size. Political conflict can play a role in constructing community identity, which can be produced and reproduced, not necessarily in the same form as before (Dalby and Mackensie, 1997). Along side this fluidity of relationships that is assumed to be associated with the construct of place is the question of whether communities are constructed because all the members are known to one another socially or whether the community is in some aspects imagined by the members. As Anderson explains in the context of nationalism, in
many communities members do not know most of their fellows personally, yet “in the minds of each lives the image of their communion” (1985, p. 49). A further complication is the observation that even communities that may appear homogeneous at first glance are very likely to have internal structures. For instance, Massey (1994) points to the different senses of place experienced by men and women in many communities.

In very simple terms then, the word ‘community’ encompasses a variety of groupings, whether the distinguishing criteria are geographical or comprised of social, cultural or political factors. But, whatever lens one elects to view communities (of which there are several), it is apparent from the literature that there are many forms of community, including those that exist in the absence of a shared place or space (i.e. communities of interest). Equally, one place hosting a single community “is probably quite rare” (Camina, 2004, p. 3).

The rationale for working to tackle issues at community or neighbourhood level rather than with individuals or on a societal scale derives in part from the observation that the complex and interrelated circumstances that underlie an identified ‘problem’ (such as an unusually high level of alcohol related hospital admissions) can usefully be delineated spatially (Chaskin, 1997) as well as by more general characteristics like age, gender or occupation. As well as practical arguments in favour of community or neighbourhood level interventions, there is also a powerful discourse spanning disciplinary boundaries that posits community interventions as ‘a Good Thing’. Certainly, current political rhetoric in the UK seems to have subscribed to this perspective, as outlined above. It can be argued that this discourse stems from what Chaskin calls the “decline of community thesis” (p. 522), i.e. that urbanisation and an associated breakdown of the traditional bonds that held people together has led to a decline in ‘community’ and that the consequences are broadly undesirable. While this construction of community has been criticised for its overly romanticised conception of a village life (a conception that current research suggests never really existed, Camina, 2004) the policy discourse at present links community interventions, as opposed to those targeted at individuals or at society as a whole, with a desire to build citizenship and community capacity; with a desire to empower through

Community Based Prevention Marketing (CBPM) and Community-Based Social Marketing (CBSM) are two methods labelled explicitly as ‘social marketing’ that appear to favour community engagement methods. These ‘branded’ methods have much in common with one another: both aim to inform intervention design, originated in North America and were spearheaded largely by a single academic (Professor Carol Bryant and Dr. Doug McKenzie-Mohr respectively). Both appear to have been developed in response to particular issues: health promotion and disease prevention in the case of CBPM and pro-environmental behaviours with CBSM. CBPM draws explicitly upon community organisation methods (Bryant et al., 1999) and involves working in partnership with communities to apply a social marketing framework based upon exchange theory and the 4Ps (Kotler, Roberto and Lee, 2002). Formative research and segmentation techniques are used to identify perceived costs and benefits of the ‘product’ and to isolate distinct markets to receive the intervention (Parsons and McCormack Brown, 2004; Forthofer and Bryant, 2000). A marketing plan is created with the community; and messages, materials and strategies pre-tested (Salazar, 2004; Salazar, Bryant and Kent, 1997). Conversely, the intention of CBSM is to merge expertise from social marketing with psychological knowledge (McKenzie-Mohr, 2000; McKenzie-Mohr and Smith, 1999), and in fact it does not emphasise collaboration with target communities per se. CBSM is a process that encompasses four or five steps: first, a practitioner of this approach should seek to undercover barriers to desirable outcomes and then, based upon these insights, choose what specific behaviours to promote. Next, a programme is designed to overcome barriers and promote desirable behaviour. Then, the programme should be piloted and finally, evaluated. In their book, McKenzie-Mohr and Smith (1999) include a fifth stage: rolling out the successful pilot to the whole community. Other than regarding geographical communities as a target for behaviour change programmes, it is not clear why the term ‘community’ is so strongly associated with this approach as its philosophy appears to be routed primarily in the psychology of the individual,
though some suggested strategies do cover such topics as challenging social norms.

Aside from the specific ‘brands’ of CBPM and CBSM, a more general approach to collaborative intervention development is emerging in social marketing practice (Collins, Forthcoming; Collins, Spotswood and Manning, 2012; Domegan *et al.*, 2013; Stead, Arnott and Dempsey, 2013) that draws upon methods developed by community organisers (Minkler and Wallerstein, 2008; Stead, Arnott and Dempsey, 2013). Whilst these participatory methods offer thought-provoking new directions for social marketing, they also have an association with community and international development, as well as participatory research (Fals-Borda and Rahman, 1991; Hickey and Mohan, 2005; Rahman, 1995). Ostensibly, participatory methods offer a promising way to empower at-risk populations like people living in deprived neighbourhoods (Lefebvre, 2012); this is the crux of the overlap I perceive between social marketing interventions and participatory research and in this apparent commonality I locate some of the contribution of this work. I will elaborate on these connections throughout the remainder of this thesis. These methods also chime with the ‘consumer empowerment’ movement in the commercial marketing literature and, on the face of it, with increasing momentum of the theoretical approach known as co-creation of value and the Service-Dominant (SD) Logic (Vargo and Lusch, 2004), which will be explored in the next section.

**The co-creationists**

Since 2004, a significant stream of work in marketing has centred on co-creation as part of the discourse on S-D Logic. In fact I intend to argue here that an association between the S-D Logic and community or participatory approaches is largely an unfortunate misconception created in part by their linguistic similarity and in part by the level at which the ‘co’ (used to denote an interaction of some kind) is located, theoretically. In very straightforward terms, I contend that in S-D Logic, the theoretical interaction is between resources and in community and participatory approaches the interaction is between people. In order to make this
case however, first I need to lay out my interpretation of the S-D Logic, based upon a review of the principal literature.

Value co-creation theory is founded in the recognition of what proponents call a more comprehensive and inclusive logic for marketing, a “service-dominant view, in which intangibility, exchange processes, and relationships are central” (Ferreira and Proenca, 2009, p. 2). The S-D logic is based upon the insight that consumers do not buy ‘goods’ or ‘services’, they buy offerings, which in turn render services that create value (Gummesson, 1994); thus, the idea of a resource takes on a new meaning. Vargo and Lusch (2004) explain that, where resources were once conceptualised as static items that humans could capture and use, now they are regarded as neutral entities that only become active once humans engage with them. Yoda like, “resources are not, they become” (Vargo and Lusch, 2004, p. 2). Resources can be categorised as operand and operant resources. Operand resources are entities upon which an act must be performed in order to realise value, such as land or minerals; and operant resources are resources that act upon operand or other operand resources, such as creativity or technology (Constantin and Lusch, 1994). Goods-Dominant (G-D) logic privileges the operand resources (customers are conceptualised as falling to this category) and identifies value as embedded in physical products. In S-D logic, operant resources are primary because they produce effects, and both consumers and firms possess them. Further, customers perceive and determine, indeed they co-create, value in use (Vargo and Lusch, 2004).

So, when the term ‘co-creation of value’ is used in S-D logic, it refers to an acknowledgement that both producers and consumers possess resources, and that both parties must act to integrate their resources to enable value-in-use to be co-created. Thus, this way of thinking about co-creation of value can be seen primarily as a way of constructing a theory of the process of value realisation, referring to the processes by which both consumers and producers collaborate, or otherwise participate, in creating value (Prahalad and Ramaswamy, 2004; Pongsakornrungsilp and Schroeder, 2011). Consequently, while the term ‘co-creation’ may have been conflated with the notion of participatory approaches to intervention design (Domegan et al., 2013), for the purposes of this thesis they
are regarded as separate entities, with S-D logic having limited relevance, despite the illusion of similarity created by the terminology.

Another perspective that has more relevance draws upon an observation that the last two decades of marketing thought have been characterised by a subtle shift in the way consumers are conceptualised. From passive entities that sit patiently at the end of the value chain (Pongsakornrungsilp and Schroeder, 2011), consumers have been recast as active agents that resist efforts to “constrain, control and manipulate them” (Gabriel and Lang, 2008, p. 334). This has been described a profound transformation in the producer-consumer relationship (Arvidsson, 2005; Pettinger, 2004) from producer-recipient to collaborating partner (Wikström, 1996). Thus, co-creation (same term, somewhat different interpretation) is also thought to be a necessary response to the challenges of sophisticated and demanding customers who resist categorisation and control (Holt, 2002; Fırat and Dholakia, 2006). In extending this argument, some scholars have assigned to co-creation an ideological mantle; by characterising consumers as creative collaborators, they become empowered to seek self-fulfilment through their labour and to create value that would otherwise go unrealised (Bonsu and Darmody, 2008). This interpretation of co-creation is less about a theory of how value is realised via resource integration, and more about an assumption that the traditional balance of power between an organisation and its consumers is shifting. Thus, this version of ‘co-creation’ bears more similarity to participatory research, as will become apparent in Chapter 3.

With this reasoning, the concept of co-creation appears to invite a revolution in the way markets are conceptualised, classing them as democratic spaces within which firms strive to engage creative and resourceful consumers in joyful and empowering collaboration for mutual benefit (Bonsu and Darmody, 2008; Schau, Muñiz and Arnould, 2009). Rather like Lefebvre’s vision for markets that are more effective, sustainable and just, value co-creation has been linked to an “evolution of capitalism” (Bonsu and Darmody, 2008, p. 356) towards creationist capitalism (Ruckenstein, 2011; Boellstorff, 2008) or an ethical economy (Arvidsson, 2008) made possible through consumer empowerment (Kozinets, Hemetsberger and Schau, 2008).
These interpretations are interesting for two reasons: firstly, that once again they speak to issues of power, choice and freedom in theories of consumption and the market; and secondly it links to Lefebvre’s ideas about redefining, indeed recreating, our understanding of markets. Indeed, according to recent work by Vargo (2011), S-D logic represents a pre-theoretic lens that invites a different perspective upon the economic and social world. Vargo suggests that the first step in building a theory of S-D logic lies in constructing a new theory of markets. He suggests that S-D logic is a model, or “service ecosystem” (p. 220), that conceptualises all actors as resource integrators, connected in shared systems of exchange, i.e. markets, governed by socially constructed convention. As explained earlier, these suggestions for market definition (Storbacka and Nenonen, 2011; Layton, 2011) appear to chime with neo-classical economic ideas of markets as business ecosystems that fall naturally to consistency and equilibrium, understood primarily in financial terms.

Conversely, the ethical economy (Arvidsson, 2008) is thought to consist of emergent, grassroots innovation, typically by non-commercial participants for whom financial gains are not the primary concern; rather, value is realised through making a social contribution of some kind (von Hippel, 2006); an example can be found in the Time Banking movement (Lasker et al., 2011). Arvidsson (2008) terms this social production (Benkler, 2006) an alternative “value logic” (Arvidsson, 2008, p. 326) within which different ways of participating, such as those facilitated by social media or consumption (Muniz and O’Guinn, 2001) are becoming a source of value (e.g. Toffler and Toffler’s ‘Revolutionary Wealth’, 2006). The ‘value’ that is sought in the ethical economy is not monetary or financial capital, it is realised through improving one’s social standing in one’s community, through networks and respect, or ‘ethical capital’ (Arvidsson, 2008). Arvidsson attributes these developments to three factors: firstly, the increasingly coherent consumer culture following World War II, facilitated by media and technology. The second antecedent is claimed to be an assumed loss of connection to traditional community structures with the growth of the service and knowledge economy (i.e. what has been termed elsewhere the “decline of community thesis”, Chaskin, 1997, p. 522). A final cause is believed to be an
increasing emphasis upon higher education, which created an increasing number of ‘knowledge workers’ who have been taught to value self-realisation through their labour. These are the so called ‘empowered’ consumers (Shanker, Cherrier and Canniford, 2006). The term ‘empowerment’ is quite significant I contend, and some of the consequences of this idea are reviewed critically in section 3.8 in the next chapter. In conclusion, this section has put forward the suggestion that co-creation of value from the S-D logic is conceptually distinct from other types of co-creation, collaboration and participation found in consumer research and more widely. Furthermore this section has explored contemporary ideas on the conceptualisation of the market and the empowered consumer that are impacting on the discourse on social marketing.

2.5. Concluding comments

In this chapter, I have attempted to provide an overview of social marketing theory as it currently stands. I have divided this into what I term an orthodoxy, underpinned by exchange theory and the 4Ps of the marketing mix; followed by an articulation of the various revolutionary approaches founded in systems theory, community participatory approaches and co-creation. I have attempted to show how the relationships between social marketing orthodoxy and those that plot their revolutions against its doctrine are complex and interlinked, founded partly in the relatively uncontentious observation that orthodox approaches are overly concerned, both theoretically and metaphorically, with individual psychology at the expense of context, which is thrown into sharp relief when social and economic inequality and exclusion are brought into the equation. Some would argue, and they are in august company with Professors Alan Andreasen and Philip Kotler (Dibb and Carrigan, 2013), that social marketing is a “brand of individual behaviour change” (Andreasen, 2002, p. 8) and as such, should only be employed in situations where individual, rather than structural or community change, is an appropriate goal. But other influential social marketers are working towards embedding social marketing into the “DNA and mind-set” of “government, state, regional or local public institutions”, seeking to position social
marketing as “part of the solution that senior managers and politicians can use to help them solve their problems” (French, 2012, p. 1, 2). Given this aim, these questions about social structure seem significant.

What social marketers seem less keen to do, however, is advocate systemic social and cultural change, preferring to work towards incremental improvements within the current system. Where this observation becomes particularly relevant is when it is hinted that social marketing is emancipatory and empowering; to quote Professor Jeff French once again, ideologically, social marketing can be positioned as “a reflection of a citizen centric approach to social programme delivery and one that emphasises mutual responsibility as well as social cohesion” (2012, p. 3). But as I outlined in section 1.4, I share the concerns of a few voices (Brenkert, 2002; Raftopoulou and Hogg, 2010) that social marketing intervention may in fact bypasses the usual democratic processes of grassroots action and political discourse, and thus the people social marketers believe they are helping may in actuality be stripped of their voice and rights to self-determination and participation. Thus, to draw upon discourses of citizenship and participation seems to me to be a co-option of these ideas, rather than engagement with them. The next chapter seeks to explore these discourses of participation and emancipatory knowledge, in the context of which I aim later to explore some of the questions that this chapter has posed about social marketing.
Chapter 3: In which I say where I thought I was going

3.1. Foreword

“Do not monopolize your knowledge nor impose arrogantly your techniques but respect and combine your skills with the knowledge of the researched or grassroots communities, taking them as full partners and co-researchers” (Fals-Borda, 1995, p. 1).

I wasn’t satisfied with my work on this chapter so I rewrote it, but only after I had finished Chapters 5 and 6. When I went back to the literature, it read very differently to the way it did when I was trying to understand it the first time. Now, I can nod along to the dilemmas and frustrations that participatory researchers write about. For example, the way Caitlin Cahill writes of the young women she worked with in New York...

“The report featured a hypothetical profile of a young woman of color which was a composite of ‘at risk’ stereotypes... The research team developed their project Makes Me Mad: Stereotypes of young urban womyn of color [sic], as a way to ‘speak back’” (Cahill, 2007c, p. 271).

…could have been written about the way people in Parkhood and Urbanwood feel about being identified as ‘problem neighbourhoods’.

3.2. Introduction

There is a great deal of literature that deals with the notion that researchers might collaborate with their ‘subjects’ rather than treating them as passive sources of information. Collaborative methods are woven through a variety of fields in the social and health sciences, such as education (Kemmis and McTaggart, 2005); community development (Fals-Borda and Rahman, 1991); theology (Berryman, 1987); post-colonial international development (Chambers, 1997; Hickey and Mohan, 2005); social psychology (Fine, 2006; Fine and Torre, 2006); criminology
(O'Neill, 2012; O'Neill, Woods and Webster, 2004); geography (Cahill, 2007a; Cahill, Sultana and Pain, 2007; Kesby, 2005; Kindon, Pain and Kesby, 2007) and health (Israel et al., 1998; Cornwall and Jewkes, 1995; Khanlou, 2010). Some are more radical than others.

Often, ideas of participation are combined with ideas of social action or action research. But while there are similarities between participatory research and action research, the differences in their origin and some of their assumptions about power and politics means that it is helpful to draw a distinction between these interwoven traditions, as much as possible given the cross fertilisation that has occurred (Taylor et al., 2004). Therefore, while acknowledging that boundaries are blurred, in this Chapter I identify the root of the difference as the explicitly critical emphasis of participatory research with its end goal of social transformation, which is described in the Southern story in section 3.3; in contrast to the more reflective focus of action research, which developed in industrialised countries (Whyte, 1995) and typically espouses an end goal of improved practice (Whitehead, 2009; Brown and Tandon, 1983). This is outlined in the Northern story in section 3.3.

This distinction is my partly my own line in shifting sands (though others have drawn similar conclusions, see Gouin, Cocq and McGavin, 2011); where things become confusing is when participatory researchers seek social transformation through improved practice (Cornwall and Jewkes, 1995) and action researchers seek to improve their practice by tackling social injustice (Kemmis and McTaggart, 2005). Nevertheless, this imperfect distinction helps delimit the field and enables me to locate this thesis and myself in the realm of explicitly critical and emancipatory participatory researchers, the inheritors of the Southern tradition, rather than with the less radical forms of action research more typically found in management studies, health promotion or the study of classroom teaching. Hadfield (2012) draws the same distinction, classifying the two types as critical and pragmatic.

In the two sections following the Southern and Northern stories, I elaborate upon two important characteristics of contemporary participatory research: firstly, that it
is cultivated ‘on the spikes of injustice’. By this I mean that participatory researchers seek to give voice to those who are typically marginalised, both generally by the society they live in and by the research process. And secondly, in section 3.6, an exploration of the epistemological foundations of participatory research, which hinge upon a rejection of the objective distance between researcher and researched. Having located my own sympathies with the emerging feminist post-structuralist perspective on participatory research, next I present some feminist critiques of mainstream participatory and action research literature, which is followed by two further areas of critique: the first in section 3.8 links to the role of post-colonial development (which is defined as an expert-led, top down transfer of ideals and techniques from ‘developed’ to ‘developing’ countries, predicated on the assumption that ‘developing’ countries should naturally wish to emulate Western nations), with the concern that participatory methods are not as empowering as the rhetoric might suggest. The second is not a critique of participatory methods themselves, rather it homes in on the bureaucratic difficulties academic participatory researchers have experienced because the method is so different from conventional research techniques. The penultimate section of this chapter explores the boundaries between participatory research and activism and the final section explores the issue of space, both metaphorical and physical.

3.3. A story of the northern hemisphere

In terms of a named methodology, Action Research appears to have developed before participatory research. Social psychologist Kurt Lewin is frequently credited with coining the term in 1946 (Kemmis and McTaggart, 2005; Bargal, 2006; Kindon, Pain and Kesby, 2007), describing a process in which “theory would be developed and tested by practical interventions and action” and that “ends and means were grounded in guidelines established by the host community” (Stull and Schensul, 1987, p. 88). This section charts the early roots of action research, before showing how it has intermingled with various traditions, including those outlined in detail in the next section.
The work of the Tavistock Institute of Human Relations in the UK is thought to have given expression to many of Lewin’s ideas, generating through work in industry in what has been termed an early “British tradition” for an action research methodology (Kemmis and McTaggart, 2005, p. 560). Kemmis and McTaggart identify further developmental stages that have contributed to the current position: a second generation, The Ford Teaching Project (Maw, 2006) is thought to have evolved from the work of the Tavistock Institute. Whilst recognising the value of this approach, it was considered by some to be overly practical (Carr and Kemmis, 1986) at the expense of critical consciousness. Thus emerged the third generation of more explicitly emancipatory action research in education (Kemmis and McTaggart, 2005); one of those that blurs the boundary between critical and practice focused traditions (Adelman, 2010). A similar movement was gathering pace in sociology at around the same time, with William Foote Whyte and Sol Tax’s concern with the crisis of representation (Denzin and Lincoln, 2005; Richardson, 1997); seeking to enable people to speak about their lives in research without the mediating voice of an outside expert (Reed-Danahay, 1997).

What Kemmis and McTaggart identify as a fourth generation of action research - but others argue is the first generation of participatory research (Kindon, Pain and Kesby, 2007) - came to light in the 1960s and 1970s from the synergies between emancipatory action research and conscientização, the Latin American critical pedagogy movement spearheaded by Paulo Freire in Brazil (1970; 2000). Marja-Liisa Swantz’s participatory research (Hall, 2005) in Tanzania; Rajesh Tandon’s community-based research in India and Orlando Fals-Borda’s participatory action research in Colombia (Kindon, Pain and Kesby, 2007; Hall, 2005) are also identified from the perspective of the Northern story as part of this radical participatory movement. These scholars tasked themselves with awakening a critical consciousness among communities so as to enable them to expose and confront “structures of oppression” within the state, political rule and economic and social structure (Hickey and Mohan, 2005, p. 241). This form of participatory research has been called an epistemology of practice grounded in the struggles of oppressed peoples (Kindon, Pain and Kesby, 2007) that has
been linked with the work and values of Mohandas Karamchand Gandhi (Narayan, 2000). These, of course, are the seminal scholars of the participatory research tradition and their version of their story will be told in the next section.

Subsequently, international development practitioners have explored participatory research as a methodology of empowerment (probably the most influential of these developments was Participatory Rural and Urban Appraisal, Chambers, 1997); though the participatory practice of development organisations like the World Bank has not been without its staunch critics (Cooke, 2004). Indeed some have argued that versions of participatory research evolved as a direct challenge to these development practices (Sohng, 1996). In the 1980s and 1990s, participatory researchers found common ground with critical social scientists (Whyte, 1991) with the erosion of classic anthropological and sociological norms such as objectivism, and with growing interest in critical theory, feminist and critical race theories (Denzin and Lincoln, 2005). Kemmis and McTaggart identify a number of approaches in the resulting “family” (2005, p. 560): Critical Action Research, for example, is sensitive to power dynamics, particularly in relation to social class, strongly associated with educational research and characterised as “the self-reflective collective study of practice” (Kemmis and McTaggart, 2005, p. 560).

Less radicalised versions include Classroom Action Research, which is focused on improving teachers’ practice and students’ learning. In an organisational context, there are Action Science and Action Learning, Soft Systems and Industrial Action Research (Flood, 2001; Huzzard and Björkman, 2012), all of which seem to emphasise the development and reflectivity of practitioners over social critique. A similar method, Community-Based Participatory Research (CBPR, Minkler, Wallerstein and Wilson, 2008), has origins in public health. CBPR is thought to have found its niche in that largely positivist field though its recognition that individuals suffer systematic health inequality (Marmot and Wilkinson, 2006; Scambler, 2012); are embedded in complex circumstances that shape both their behaviours and the resources to which they can access to maintain their health (Rayner, 2009; Kreiger, 1994; Simons-Morton, 2013); that some communities are known to be marginalised (Cook and Wills, 2012; O'Keefe
and Hogg, 1999) and that sensitivity is needed when researchers work in “diverse cultures” (Israel et al., 1998, p. 174; Kwate, 2008; Watson, 2008).

But when researchers in public health write about participatory methods, often they sound to me as though they are diffidently arguing for just a little space alongside proper scientific research; that they acknowledge their weakness but suggest in small voices they can fill gaps that positivism cannot (though there is awareness of the problem of power dynamics, Wallerstein and Duran, 2006). Worryingly, some health researchers appear to have taken this positivist perspective further, advocating that participatory research can be applied systematically, suggesting for example that criteria based on motivation theory be developed so as to select participants in such a way as to achieve “credible, valid, and applied outcomes” (White, Suchowierska and Campbell, 2004, p. 4). Which rather misses the point, as I will show in the following sections.

3.4. A story of the southern hemisphere

“Do not trust elitist versions of history and science, which respond to dominant interests, but be receptive to counter-narratives and try to recapture them” (Fals-Borda, 1995, p. 1).

Rajesh Tandon (2008) situates the development of what we would now call participatory research in a much broader socio-historical context than is recognised in the Northern story; beginning with what he calls “subaltern practices of community organising” (p. 285) expressed through art, music, poetry and drama. He draws attention to the philosophies of Gurus Nanak and Kabir; the latter whose writing is thought to have particular resonance with oppressed peoples, as a "protest against social discrimination and economic exploitation" (Lorenzen, 1991, p. 5). From this historical vantage point then, participation of oppressed peoples in generating their own knowledge has always existed, despite suppression and delegitimisation by dominant regimes. In this section, I offer a brief history of the context in which participatory research arose, followed
by an explanation of the method’s development with particular reference to the life and work of Paulo Freire.

In the 20th century, such ‘subaltern’ movements attracted the notice of management theorists, resulting in initiatives like the Hawthorne studies (Roethlisberger and Dickson, 1939) and work with British coal miners (Mayo, 1933). However, by far the most significant influence on the Southern story of participation, according to Tandon, is the phenomenon of post-colonial development. Throughout the 1950s and 1960s, a top down approach dominated development practice; with little regard for, or indeed awareness of, subaltern community practices. In the late 1960s and 1970s however, development organisations began to wake up to the potential of subaltern activities, prompted by mounting concern over the exploitativeness (both politically and in terms of natural resources) of the top down development model. Within this political context, Tandon identifies two strands of the Southern story: the first the critical pedagogy movement spearheaded by philosopher-activists Paulo Freire, Myles Horton and Julius Nyerere; whose ideas have been linked with those of Ghandi (Tandon, 2008; Narayan, 2000). The second source of ideas for contemporary participatory research focused attention on structural dimensions, highlighting incompatibilities between the legacy of colonial systems designed to control the masses and the relatively recent goal of facilitating participation among the poor. An example of this approach can be found in the United Nations Research Institute for Social Development’s (UNRISD) Popular Participation program. This strand will be picked up later in this section, but as Paulo Freire seems to have been a particularly significant influence in this movement, I will expand upon the story of his life and work in some detail before explaining how his influence fits with the other elements of the southern story.

Paulo Freire (1921-1997) was born in the Brazilian city of Recife to a middle class family that was soon to experience first hand the “plight of the wretched of the earth” (Shaull, 2005, p. 30) as a result of the economic crisis of 1929. This experience is thought to have had a profound influence on Freire in two significant ways: firstly, he became determined that no other child should have to know the agony of hunger that he had experienced; and secondly, he realised
that the dispossessed existed in a culture of hopelessness, of silence, in which a critical response was impossible. Of the relationship between poverty and education, he said:

“I wanted very much to study, but I couldn’t as our economic condition didn’t allow me to. I tried to read or pay attention in the classroom, but I didn’t understand anything because of my hunger. I wasn’t dumb. It wasn’t lack of interest. My social condition didn’t allow me to have an education...When I began to eat better, I began understanding better what I was reading.” Freire, quoted in Gadotti (1994, p. 5).

In his early 20s, Freire gained a place in the Faculty of Law of Recife and in 1946 started work for SESI (Social Service of Industry, an organisation for employers to assist workers) in their education department. While he criticised SESI for their idealism, which he felt put obstacles in the way of the working class achieving their own identity, it was his experiences there that Gadotti identifies as a significant turning point in the development of his theories: first in understanding the way that oppressed groups learned about their world in their own language, “the language of the people” (Freire, 2000, p. 96), and secondly, the value of learning through practice.

His own experiences with education are also thought to be foundational to his later theories; including the link between the aesthetics of language and the freedom of creativity, his words translated in Gadotti’s reflections as “creativity in teaching is linked to creativity in politics. Authoritarian teaching, or an authoritarian political regime, doesn't allow the freedom necessary for creativity. Creativity is necessary in order to learn” (1994, p. 8). In the late 1950s and early 1960s, Freire’s concern as Professor of Educational Philosophy and Director of the Social Outreach Program at the University of Recife was to inspire his students to learn the language of the people; enabling them to conduct research in “the service of the people” rather than as “instruments of the reproduction of inequality” (Brandão, 1984, translated and cited by Lownds, 2005, p. 45).

In 1962, Brazilian president João Goulart sanctioned Freire’s method officially, possibly as a way to swell the ranks of those eligible to vote (only those who could read were so). This method was refined as part of Freire’s role as co-ordinator of the Adult Education Project of the Movement of Popular Culture in
Recife, during which he facilitated “culture circles” (1973, p. 81), in which he and his participants attempted through group discussion to clarify situations and seek action. Some of his themes were nationalism, democracy and illiteracy, which were presented to the circle in dialogic form with visual aids; later Freire adopted the same basic method for teaching adults to read in Recife, during which he found the discussions “critical, stimulating, and highly motivating. The illiterate perceives critically that it is necessary to learn to read and write, and prepares himself to become the agent of this learning” (p. 86).

But in 1964, a coup toppled the progressive Goulart regime and Freire, along with many other left wing intellectuals, became an enemy of the state (Lownds, 2005). Military police confiscated Freire’s slide projectors housed in the Brasiliana library, which they burned along with the paintings that had been used as visual aids in Freire’s work (ibid.). Freire spent over a month in prison courtesy of the new regime, followed by sixteen years of exile, during which he spent some time teaching at Harvard University, only returning to Brazil in 1980 (Lownds, 2005). During his exile, he wrote his most well known book ‘Pedagogy of the Oppressed’ which set out the foundations of his philosophy: the notion that people are incomplete beings, but that each individual has the ability to become fully humanised through critical reflection and dialogue and thus transform the world. He distinguishes between the oppressors and the oppressed, but rather than condemning the oppressors he regards both as victims of a dehumanising and unjust social order. Other books included: ‘Education for critical consciousness’ (1973); ‘Education, the practice of freedom’ (1976); ‘Pedagogy in Process’ (1978); ‘The politics of education: culture, power and liberation’ (1985); ‘Pedagogy of the city’ (1993); and ‘Pedagogy of freedom’ (1998).

Diaz-Greenberg et al. (2000) identify four principles of Freire’s approach: dialogue and dialectic, conscientization and praxis. Critical consciousness (Freire, 1973; Minkler and Cox, 1980; Campbell and MacPhail, 2002), which has been translated from the Portuguese Conscientização to English also as conscientization (Freire, 1970; Nevin, Bradshaw, Cardelle-Elawar and Diaz-Greenburg, 2009), described as an individual becoming critically aware of themselves in the world, is perhaps the most well known. Critical pedagogy is
described by Freire as a process that facilitates people’s development of critical consciousness via an “active, dialogical, critical and criticism-stimulating method” (1973, p. 83, emphasis in original).

Freire defines his idea of critical consciousness partly in relation to the degree of sophistication with which people understand causality. “Naïve consciousness sees causality as a static, established fact and is thus deceived in its perception” whereas “critical consciousness always submits that causality to analysis; what is true today may not be so tomorrow” (1973, p. 82). Therefore “critical consciousness is integrated with reality, naïve consciousness superimposes itself on reality and fanatical consciousness, whose pathological naïvété leads to the irrational, adapts to reality” (p. 83).

According to Diaz-Greenwood (2000), people transcend naïve consciousness through dialogue and dialectic, which are about creating a ‘space’ for understanding one’s own voice, being willing to listen and thus developing an awareness of one’s own point of view and its opposite. She quotes a student, who describes the dialogic process as one that “make[s] the soul come out naked” (p. 875). Praxis is characterised as the process of through which individuals reach critical consciousness through action and reflection, a concept familiar throughout the action research literature (Davis, 2004; Kemmis and McTaggart, 2005) and beyond (Arendt, 1958; 1998); which is thought to transform an individual’s experience of the world (Diaz-Greenberg and Nevin, 2003). Freire’s critical pedagogy, along with the people centred philosophies of the other pioneers, were significant influences in the development of methods like Participatory Rural Appraisal (PRA) in the late 1980s (Tandon, 2008; Chambers, 1997).

By the mid 1980s, considerable effort was being expanded to include “beneficiaries” (Tandon, 2008, p. 289) in the design of programmes, particularly in relation to health, education and the management of natural resources. By the 1990s, many development organisations, including USAID (Atwood, 1993), had adopted participatory principles officially. The World Bank espoused formally a Participation Policy in 1994 (Aycrigg, 1998). Around the same time, geo-political
changes such as the demise of various totalitarian regimes, including the ending of apartheid, led to increasing interest in the concept of civil society (De Oliveira and Tandon, 1994); within which the participatory philosophy became subsumed. But patchy success and concerns over co-option led some scholars and development practitioners to wonder whether participation was in fact simply a new version of tyranny (Cooke and Kothari, 2001). I elaborate upon this particular issue in section 3.8.

Thus, the Southern story paints a picture of a participatory research that is a radical and political activity that regards the knowledge generated by the research process and the critical consciousness developed as a result as anything but neutral and objective. This is a position underpinned by specific ontological and epistemological assumptions that will be explained in the following sections, with reference to contemporary interpretations and applications of participatory research.

### 3.5. Cultivated on the spikes of injustice

At its core, participatory research is an approach that turns the traditional dynamic of remote researcher and submissive subject on its head by seeking to involve in the process those who are usually ‘studied’. This inclusion democratises (Fine, 2006; Fine and Torre, 2006) the articulation of research questions, selection and design of method, analysis, presentation, dissemination and follow-up action (Cahill, Sultana and Pain, 2007). In this section, I offer a brief review of the political foundations of contemporary participatory research.

Advocates of participatory research believe that people, especially those who belong to groups that historically have been oppressed - their own knowledge stigmatised (Kidd and Kral, 2005) - know a great deal more about their lives and experiences than researchers do and therefore should shape the questions and frame the interpretations of the research (Cahill, Sultana and Pain, 2007). There is no deferral to dominant research paradigms. Consequently, participatory research is said to be “cultivated on the spikes of social injustice” (Fine and
Torre, 2006, p. 255) in two ways: it democratises the research process, so that the researcher is no longer the aloof scientist who has monopoly on what is studied and how it is interpreted; and secondly, participatory projects are designed to give voice to those on the “bottom” (Matsuda, 1995, p. 324) and the “margins” (Afshar and Maynard, 2000, p. 806). Why is breaking the monopoly on truth by giving voice to the margins and the bottom significant? As Mari Matsuda explains in the context of critical legal studies:

“When you are on trial for conspiracy to overthrow the government for teaching the deconstruction of law, your lawyer will want black people on your jury. Why? Because black jurors are more likely to understand what your lawyer will argue: that people in power sometimes abuse law to achieve their own ends, and that the prosecution's claim to neutral application of legal principles is false” (1995, p. 323).

Though she doesn’t mention it explicitly, I think Matsuda is referring to the concept of privilege, which has been defined as “the unearned benefits and advantages that accrue to members of dominant groups as a result of the ongoing exploitation and oppression of members of dominated groups” (Monahan, 2014, p. 73). In Matsuda’s example, white jury members benefit from the privilege of not having experienced an abuse of the law by a person in power, and therefore they cannot conceive that such abuses could exist in a system that, to them, seems fair and utterly neutral.

The same is said to be true of research, particularly as privileged groups (western, white, male, able-bodied, heterosexual) historically have dominated research in which less privileged groups are the objects (Denzin and Lincoln, 2005). Because people who belong to privileged groups are largely ignorant (Monahan, 2014) of their status, often because they are not sexist, racist or indeed anything-else-ist in their personal belief system, they will simply not see that their ‘objectivity’ is in fact the subjectivity of the dominant position. Thus, the researcher’s claim to a dispassionate and objective stance is interpreted by critics of conventional approaches as oppressive in itself (Smith, 2005) because it will simply perpetuate the hierarchal status quo rather than challenge it.

Challenge and indeed change (Maguire, 2001), shattering what Hannah Arendt calls the “lying world of consistency” (quoted in Schiff, 2014, p. 58), is an
acknowledged aim of this type of participatory research. Smashed as well is the “the false consensus of neo-liberal institutional life” (Fine and Torre, 2006, p. 255), achieved by mounting a challenge to the apparent everyday inevitability of injustice. Fine and Torre wrote about institutional life in an article about a woman’s prison, but later in that same paper (and later in this thesis) they and I extend this critique to academic life as well. So unlike the public health researchers arguing that CBPR can help those operating within the dominant paradigm understand the objects of their research better, participatory research sets itself in open opposition to privileged, objectivist researchers. The consciousness raising aims of this type of participatory research has strong roots in the educational and political philosophy of Paolo Freire, as outlined in the previous section. The next section expands upon this theme with a consideration of the ontological and epistemological foundations of participatory research.

3.6. The moral dangers of distance

“Do not depend solely on your culture to interpret facts, but recover local values, traits, beliefs, and arts for action by and with the research organizations” (Fals-Borda, 1995, p. 1).

The title of this section is borrowed from a fascinating paper that charts the creation of a fictional textbook called “Methods for Researching Oppression and Resistance” (Fine, 2006, p. 83). The notion of distance in research as something that is undesirable, even dangerous, rather than something for which researchers should strive sets participatory research apart from more conventional approaches to knowledge creation; though things aren’t quite as simple as a stark modernist versus postmodernist dichotomy (Atkinson, 2006). In this section, I lay out the ontological and epistemological foundations of participatory research as presented in the literature, with particular reference Habermas’ critical theory. I conclude with a review of the most recent contributions, which argue for an interpretation of participatory research located in feminist post-structuralism.
One of the few areas in which the many and varied traditions of participatory research agree on is how it works in practice. All flavours of participatory and action research are based upon an iterative process, a spiral of action and reflection sometimes known as “spiral science” (Kindon, Pain and Kesby, 2007, p. 10). Participatory research is a social process (Kemmis and McTaggart, 2005; Kral, 2014), which draws upon the embodied knowing of participants, the self-reflective and self-critical abilities of the researcher and their ability to develop participants’ capabilities through their experience of the research process (Cornwall and Jewkes, 1995). Common in the literature also is the stipulation that participatory and action researchers are not distant, objective figures.

Reason, (1998; 1994) situates his epistemological critique of conventional approaches to social research in a historical perspective, identifying three phases of human intellectual development: the first, characterised by myth and romanticism, positions humans inseparable from their world. In the second, which Reason doesn’t identify as such in this paper but I assume refers to the Enlightenment, humans progressively separate themselves from their world, as “a Promethean biological and metaphysical rebel” privileging scientific detachment over embodied knowing, engaging in an “heroic impulse to forge an autonomous rational human self by separating itself from the primordial unity with nature” (Tarnas, 1991, p. 441).

This unashamedly androcentric perspective, with much metaphorical questing and heroism (one can almost hear the faint fanfare of trumpets) still dominates our thinking today, Reason argues. With a surprising lack of recognition for the “writing out” of women’s contributions to, well, everything (Lengermann and Gillian, 2006, p. 3; Russ, 1983), he situates this individualism as masculine endeavour that suppresses participation (which he identifies as feminine). While I disagree vehemently with this assignation of socially constructed categories to different ways of thinking, associated as this practice is with perpetuating oppression rather than challenging it (I expand upon the feminist critique of the participatory research literature in section 3.7), it doesn’t necessarily detract from the basic point that the third phase of knowledge, a “postmodern moment” (Reason, 1998, p. 10), is upon us. Though rather than following a postmodernist
epistemology, Reason has been a leading figure in the development of a participatory paradigm (see also Guba and Lincoln, 2005), within which “the world we experience as 'reality' is subjective-objective, a co-creation [there's that term again] that involves the primal givenness of the cosmos and human experience, imagination and intuition, thinking and construing, and intentional action in the world” (Reason, 1998, p. 11).

While we can be confident that participatory research is not founded on a realist or critical realist ontology or a positivist epistemology, at least as defined by Guba and Lincoln (2005), the assumptions that do underpin it are not simple to locate in one particular paradigm, as there seem to be several competing influences. One such influence is critical theory, in particular the work of Jürgen Habermas, whose writings on communicative action and the public sphere underpin the arguments in favour of a participatory approach in what has been termed the “northern tradition” (Wallerstein and Duran, 2006, p. 313). In Habermas’ theory of communicative action (1987), people are thought to reach unforced consensus about what action to take by way of intersubjective agreement as a basis for mutual understanding; opening up communicative space (1996), which in turn builds solidarity and legitimises the action (the issue of space is explored later in this chapter). These pillars form the foundational criteria for what constitutes knowledge (Habermas, 1972) in this northern tradition (Elliott, 2005; Kemmis and McTaggart, 2005).

Kincheloe and McLaren provide a very broad definition of a researcher in the critical theory paradigm that includes: the socially and historically constructed nature of reality and the inseparability of knowledge from knower; an unstable relationship between concept and object which is often mediated by the social relations of capitalism; the centrality of language to the formation of subjectivity; the privileging of some social groups and the oppression of others for varying reasons, often linked once again to capitalism; and that mainstream research practice has been implicated in the replication of oppressive social structures (see also Kinchloe and Steinberg, 1997). In their words, a critical researcher is someone who:
“...attempts to use her or his work as a form of social or cultural criticism and who accepts certain basic assumptions: that all thought is fundamentally mediated by power relations that are social [sic] and historically constructed; that facts can never be isolated from the domain of values or removed from some form of ideological inscription” (2005, p. 304).

In other words, a researcher who locates herself in this tradition is not neutral and distant from her research or from her participants. Thus participatory research is located largely in Habermas’ emancipatory category, rather than the technical or practical (Ozanne and Saatcioglu, 2008; Elliott, 2005). Kincheloe and McLaren explain in the introduction to their chapter that their “ideoyncratic ‘take’” (2005, p. 303) is but one interpretation that others may find problematic. They are correct. Not in the sense that others disagree with their characterisation (it is so broad that it would be difficult to find fault in this regard) but I infer that in its very inclusivity it may gloss over important distinctions.

One such distinction is between the critical and pragmatic “camps” (Hadfield, 2012, p. 573). The dilemma is that participatory research is not conducted exclusively in circumstances where there is obvious social injustice; examples of such a lack of obvious inequality might include an organisation or a classroom in the UK (Davis, 2004). I have encountered similar situations in my own research, for example in a collaborative project with women in a middle-class area to raise awareness of the range of breast cancer symptoms among their peers, there were no obvious social justice issues: the town was affluent, all the participants were educated, confident and articulate. They took minutes of their meetings and developed spreadsheets to track their activity. They were invited to represent themselves on a local ‘health and wellbeing’ board and are still working towards their aims as I write, four years later. Elliott argues, somewhat ironically, that the popularity of critical perspectives marginalises those more pragmatic studies by creating an “epistemological hierarchy” (2005, p. 365) with emancipatory perspectives above practical ones. He also contends, probably more importantly, that the Harbermasian underpinnings of Carr and Kemmis’ original work (1986) mean that there is a disconnection between theory and the practical business of making change happen. Elliott is not alone in critiquing the applications of
emancipatory participation (Cooke and Kothari, 2001), a topic that will be expanded upon later in this chapter.

Another epistemological distinction can be found in recent feminist post-structuralist perspectives on Freire’s original critical pedagogy "the term used to describe what emerges when critical theory encounters education" (Kinchloe and Steinberg, 1997, p. 24). Caitlin Cahill points out that a significant portion of what could be called the ‘emancipatory logic’ of participatory research has been explored thoroughly in the context of broad social change and consciousness raising, but less well understood is the role of participatory research in personal change (for an exception see Torre, 2005). Once again, this issue is concerned with the possibility of a disconnect between the theory of change and its actual occurrence.

At first glance, the underpinnings of participatory research as a method that privileges local authenticity and seeks emancipation of the oppressed may appear incompatible with post-structuralism, with its emphasis on everyone’s subjective being in the world (Cahill, 2007a). Certainly, with recourse to Guba and Lincoln’s (2005) framework, critical theory and post-structuralism are in different boxes, though it is interesting to note Fals-Borda’s (2001) observation that with the perspective of hindsight, he and his contemporaries working to develop participatory methodologies in the 1970s anticipated postmodernism. Cameron and Gibson (2005) identify three areas in which the “sea change” (Barrett, 1992, p. 205) of post-structuralist thought has implications for participatory research. These are identity and subjectivity; language and representation; and politics. The call for reciprocation comes from both sides of the divide though, with scholars like Patricia Ann Lather (1991) for example, asking what all this textual deconstruction is actually for, what it achieves, where the action is (Reason and Bradbury, 2001). And thus, ontologically at least, we are led back to Reason’s participatory ontology with an epistemology located in “critical subjectivity...experiential, propositional, and practical knowing; cocreated findings” (Guba and Lincoln, 2005, p. 195).
Clearly, this is a debate that is ongoing but in order to provide some sort of foundation for the work in this thesis, I will draw upon the collective of researchers who draw upon feminist post-structuralist approaches in their participatory endeavours. This collective includes some relatively new researchers like Caitlin Cahill (2007a; Cahill, Sultana and Pain, 2007) and Maria Elena Torre (2005), as well as more established scholars like Rachel Pain, Michelle Fine and Mike Kesby. I choose to draw upon their interpretations partly because they resonate strongly with my own experiences and perspective and partly because I can see strong links with the feminist post-structuralism of Laurel Richardson’s work on academic writing, which underpins my methodological approach (this will be explained in the next chapter). From this perspective, I will return to this crucial question of how practical change is to be achieved, if, as Elliott worries, “becoming critical is not enough to become empowered as a change agent” (2005, p. 362). This question of being a change agent, an activist, as well as a researcher is explored section 3.10, within which I adopt the perspective of the feminist post-structuralist participatory researchers outlined here. The next section continues the feminist theme by summarising explicitly feminist critiques of the participatory research literature.

3.7. All the reasonings of men

If participatory research is founded on the understanding that people who have experienced oppression are best placed to ask research questions and interpret the results (Torre and Fine, 2006), then feminist - and indeed critical race - perspectives should be integral to participatory research (Collins, 2000). And yet, gender as a source of oppression has received relatively little attention in the mainstream participatory and action research literatures (as opposed to social class, for example), with feminist perspectives largely “ignored, minimised or marginalised” (Maguire, 1987, p. 52); an omission Maguire finds “inadequate for its supposed liberatory project” (2001, p. 60). In this short section, I review the intersection between feminist and participatory research.
While women’s (and children’s) empowerment has received what Tandon (2008) characterises as paternalistic attention from developers, and there have been calls for greater attention to the social causes of women’s health problems (Israel et al., 1994; Israel et al., 1998; Shiell and Hawe, 1996), the exclusion of feminist or womanist perspectives has resulted in inadequate attention to the notion of seeking truly emancipatory knowledge (Harding, 1986) promised by participatory research. Of course, as Cahill (2007c) points out, the voices of women and other excluded groups do not represent “new perspectives, rather they represent voices that have been marginalised, silenced, or ignored” (p. 330). A sentiment echoed in Greenwood’s observation (in an email to Patricia Maguire, 2001), that his female students find action research easier to learn than men, a phenomenon he attributes to them having learned “how to manage without domineering, linking rather than coercing, respecting diversity and otherness rather than imposing sameness are lessons women often learn as a result of being coerced themselves” (p. 61). In other words, women are skilled at understanding oppression because it is woven through our lived experience.

There is of course a considerable literature on feminism and feminist research, which, while fascinating to me personally, has limited relevance to the overall trajectory of this thesis. However, what does seem relevant here is the discovery that, regretfully, Maguire’s chapter in the Handbook of Action Research was replaced in later editions by Reid et al.’s (2006) exposition of a ‘Feminist Participatory Action Research’ (FPAR), which they define as “a conceptual and methodological framework that enables a critical understanding of women’s multiple perspectives and works towards inclusion, participation and action, while confronting the underlying assumptions researchers bring into the research process” (2006, p. 316). They claim that feminist participatory researchers work towards a dual aim of changing the conditions of women’s lives and challenging conventional notions of power such that it can be used more responsibly.

But while this assertion that an explicitly feminist participatory research would draw upon feminist understandings of power and would seek social change specifically for the liberation of women seems sensible on the surface, what seems to be missing from Reid et al.’s analysis is an exploration of how this
advances our thinking. Is it being suggested that feminist research would benefit from the addition of some sort of concrete focus on ‘action’ or that the male-dominated traditions of action and participatory research would benefit from some feminism? This question seems pertinent because feminist research itself has always been characterised by “unwavering commitment to knowledge production as the scene of political struggle” (Wiegman, 2012, p. 71). In fact it could be argued that rather like Tandon’s subaltern community organisers (2008), feminist researchers have been around as long as participatory researchers, conducting socially engaged research in pursuit of liberation and change (Maguire, 2001; Harris, 2001; Cahill, 2007a). In this context, suggesting that, as a body of work, participatory research would benefit from a sprinkle of feminism could be rather insulting, as could the hint that feminism has been conflated with a rather more straightforward notion of issues that affect women primarily (Fields, 2013). Consequently, I do not associate the work I did in the project with this particular flavour of participatory action research; rather I prefer the messier, more complex version of Maguire and Cahill that acknowledges feminism and participatory research as two venerable traditions with much in common. Having located myself thus, in the next section I move on to another critique hinted at with the reference to critical race and post-colonial scholarship, engaging directly with the legacy of post-colonial development for participatory research.

3.8. Transformative or tyrannical?

A different flavour of criticism, a legacy of post-colonial development explained in the Southern story and one that has proved particularly influential for me, is that participatory research has become too mainstream. It has been co-opted by the establishment and, as their tool, reinforces oppression rather than resisting it: that participation has itself become just another tyranny (Cooke and Kothari, 2001). This critique appears to chime with post-structuralist conceptualisations of power as effect resulting from the interplay of various resources (Foucault, 2000). The resources associated with participatory methods include discourses and
practices around democracy, equality, raised consciousness and critical self-reflection; ultimately, the legitimacy of participatory approaches as a tool to achieve freedom from oppression. In this section I outline a second critique of participatory methods, founded in these theories of power.

In essence, this critique is located in the application of participatory methods, rather than their underlying theory. It recognises firstly that anyone can use participatory techniques and in so doing has the opportunity to co-opt the power of their heritage; and secondly, that even if well intentioned and critically sensitive academics adopt a participatory approach, who is to say that their work is not inadvertently oppressive in itself, perhaps due to a privileged perspective that they are unaware is influencing their relations with their co-researchers?

Participatory methods therefore, argue critics (Kapoor, 2005; Kothari, 2001), are not really so different from other, more conventional exercises of power and should be resisted. Bill Cooke (2004) uses the example of the World Bank to show how influential organisations can adopt participatory methods and rhetoric and thus benefit from the power of that discourse, without actually changing anything about their neoliberal philosophy:

> “Greater impact on people’s empowerment – in terms of, say their right to life through healthcare, water and education – is made by decisions taken by the Bank and the IMF on debt repayment than can be made by an infinity of face-to-face participatory events which have no power over debt” (p. 47).

Cooke’s point is that participatory methods are a smoke screen being used to legitimise the Bank’s promotion of its ideology and further, that practitioners of participatory methods who go along with such agendas are complicit in the continued oppression, even suffering, that results. Other criticisms of participation emerging from development studies include the creation of a hierarchy with participatory methods at the top (see also Elliott, 2005); the framing by outsiders of people as oppressed and thus creating them as objects requiring intervention who must perform appropriately in the participatory process (Cornwall and Brock, 2005); the reinforcement of researchers as experts, not in the subject matter but in the participatory process, thus reinforcing their control of that process (Mohan, 2006); and reinforcement of existing power structures via an overly simplistic
conception of local knowledge (Cooke and Kothari, 2001; Sanderson and Kindon, 2004; Mosse, 1994).

However, Kindon, Pain and Kesby (2007) argue for a more moderate perspective. Acknowledging that participation, like any other method, can be used in ways that are oppressive (religious proselytizing, for example, Zocher, 2010) and ways that are not, they suggest that the effects of participation can be different depending upon who is deploying the resources. Further, they caution against conflating power and domination. Drawing upon Allen’s (2008) identification of power’s modalities, they explain that domination might occur when participatory approaches impose a particular way of representing local knowledge (Alexander et al., 2007). Manipulation might occur when researchers engage peer researchers to overcome a group’s distrust of outsiders or draw elite groups into a critique of their own behaviour by way of topics that appear innocuous at first. I perceive strong parallels between the way development organisations adopted participatory methods and their associated language and the way that social marketers seem poised to adopt a similar philosophy. For this reason, these critiques are important, and I will return to them in the concluding chapters. The next section, however, deals with more practical considerations that are nonetheless important in the practice of participatory methods.

3.9. Bureaucratic barriers

The previous two sections have outlined some criticisms of participatory research based on the dominance of androcentric and neo-colonial perspectives; this section elaborates on a particular critique that emerges from within participatory research: that of institutional research ethics. Because my focus in this thesis is practice, I home in on the applied normative ethics that have been elucidated by participatory researchers, with reference to abstract theorisation as necessary. First, I place participatory ethics into a broad context of ethics in social research, following which I explain the particular challenges participatory researchers face in relation to issues of consent, anonymity and ending the research process.
It can be argued that the decision to work in a participatory way is often made explicitly with ethical considerations in mind, but this conviction does not exempt participatory researchers from wider consideration of ethical issues (Manzo and Brightbill, 2007). In 1979, The Belmont Report outlined three basic ethical principles: respect for persons, beneficence and justice. Which principles led to three applications: informed consent, assessment of whether the benefits of the research justify possible risks and that there be fair procedures and outcomes in selection of subjects. Christians identifies four ethical pillars of what he terms “value-free social science” (2005, p. 144), which tend to be enforced by an Institutional Review Board (IRB, see Lincoln, 2005; Bradley, 2007): once again that participants give fully informed consent and therefore that they are not deceived (though there can be some exceptions if the knowledge to be gained is deemed valuable to society, Soble, 1978); that participants’ identities are safeguarded by keeping personal data confidential and maintaining anonymity in publication; and finally that all data are accurate. But while participatory researchers strive for ethical practice and have no concerns with the principles of respect, beneficence and justice (Manzo and Brightbill, 2007), participatory research works differently from conventional forms that separate researcher from researched, knowledge from knower. If the participant is no longer a passive object from whom data is extracted, instead they are a co-researcher of equal standing, a person with whom the terms of the project are negotiated, then conventional guidelines for assessing whether research projects adhere to ethical guidelines may be less relevant.

In the case of academic research, it is the IRB that must determine whether a participatory research project meets ethical standards or not. However concern has been expressed over the politics of the IRB process, issues include methodological conservatism (Lincoln, 2005; Christians, 2005; Bradley, 2007), which may mean that projects are unable to go ahead or that participatory principles have to be compromised (Manzo and Brightbill, 2007). A second problem is that conventional approval processes may reinforce existing social hierarchy and power structures (Bradley, 2007; Fine, Weis, Weseen and Wong, 2000), anathema to a methodology that evolved from a conviction that such
structures must be challenged (Fals-Borda and Rahman, 1991; Freire, 2000). Finally, it is argued that foundational ethical guidelines encourage the formalisation of rule following as a substitute for ethically engaged scholarship (Cahill, Sultana and Pain, 2007; Ellis, 2007), which once again runs contrary to the underlying philosophy of participatory research as an ethical practice in and of itself. As well as issues related to institutional review and generic ethical guidelines, participatory researchers are troubled by the practical issues of how to gain genuinely informed consent to a process that is not pre-determined by the researcher.

One alternative to the fixed and singular notion of consent, which in practice often means participants sign a form or record a verbal agreement at the beginning of the research process (Bhattacharya, 2007; Marzano, 2007), is the idea of negotiated consent, a form of “ethics-in-practice” (Guillemim and Gillam, 2004, p. 261). An example of a project within which consent was negotiated can be found in the critical participatory social work study of young people in care (Renold et al., 2008). Sensitive to the politics of research with this group and keen to avoid perpetuating their stigmatisation (Cahill, 2007b; Garrett, 1999), Renold et al. elected to “foreground ethics-in-practise with young people throughout the research process” (p. 431). They chose methods like making films and music, keeping photo and textual diaries and scrap books, guided tours and informal conversations, which they hoped would demystify the research process for participants and serve to “disrupt the researcher gaze” (p. 432). Drawing upon the UK Economic and Social Research Council’s guidelines for participatory social science research, which frames consent in participatory research as…

“...an ongoing and open-ended process. Consent here is not simply resolved through the formal signing of a consent document at the start of research. Instead it is continually open to revision and questioning. Highly formalised or bureaucratic ways of securing consent should be avoided in favour of fostering relationships in which ongoing ethics regard for participants is to be sustained, even after the study itself has been completed...” (ESRC, 2012, p. 30)
…the researchers were careful to choose language for their consent forms (required by their IRB) that framed the young people’s participation as always negotiable.

Yet another perspective on consent is that it can be “layered” (Riecken and Strong-Wilson, 2006, p. 42), by which they mean that traditional conceptualisations of informed consent construct people as individuals who consent to their own participation, but that in the reality of a participatory research project that consent may need to be extended to others in the community who might be affected by the research. In Riecken and Strong-Wilson’s example, it was the social and political implications of participatory research into Aboriginal young people’s substance abuse, mental health and experiences of discrimination and racism that were found to have wider consequences.

An issue that is related to informed consent is deception. According to the American Psychological Association (2010, p. 11), deception is permitted in social research if it is “justified by the study’s significant prospective scientific, educational, or applied value and that effective nondeceptive alternative procedures are not feasible”. The APA stipulates also that participants aren’t deceived in such a way that pain or emotional distress would result and that any deception is explained as early as possible in the research process. In common with many economists (Barrera and Simpson, 2012) who think that honesty in research is crucial, participatory researchers are not fond of deception. Unlike the economists cited by Barrera and Simpson though, participatory researchers do not eschew deception because it leads to contamination and invalid data (Bonetti, 1998), they reject it on principle; even to the extent of making clear to participants elements of the research process that would normally be opaque, as with Renold et al.’s project (2008).

In addition to concerns over the politics and bureaucracy of the IRB process (Haggerty, 2004) and issues of informed consent, participatory researchers face the dilemma of negotiating the tensions between respecting participant anonymity and treating them as equal stakeholders with just as much right to be recognised for their contribution as the researcher themselves. Matt Bradley
believes his struggles with an IRB’s insistence on anonymity led to participants who were “silenced for their own protection” (2007, p. 339).

“the limitations on research and the insistence on confidentiality that were imposed by the IRB … have the ability to make research benign and meaningless and sustain the marginalization of people by relegating them to the status of anonymous objects of study. This is especially problematic for the participatory researcher who is interested in working with vulnerable and marginalized populations in a way that acknowledges their knowledge and experiences and seeks to amplify their voice and self-determination” (ibid., p. 346).

It has also been argued that anonymity takes away a participant’s right to be recognised as a contributor and to realise any benefits that might accrue from the research (Evans, 2004) as well as denying them the right to negotiate ownership of their own words and how they are used (Cahill, Sultana and Pain, 2007; Blake, 2007). Elwood (2007) for example, describes a project in which she worked with a community-based organisation to develop Geographic Information Systems (GIS) maps, which were useful both as part of continuing community development work and as archives of how work had developed over time. On publication of the research, Elwood was instructed to remove identifying information for ethical reasons. Her participants, however, argued that anonymity harmed them: it meant that others would overlook their contributions and their case for greater recognition locally would be diminished. In this instance, it could be argued that falling back on anonymity as a foundational standard of protection for participants was in itself harmful to them. But because of the traditional conceptualisation of participants as passive objects who simply “empty themselves into research” (Blake, 2007, p. 414) rather than participants in a reciprocal relationship (Domosh, 2003), ethics rules were ill equipped to manage the process. Elwood’s example illustrates the ways in which the traditional ‘knowledge production’ conceptualisation of research can disadvantage participants.

Yet another issue that seems quite specific to participatory research, so much so that it seems to receive little scrutiny in mainstream literature, is the question of what happens when the research ends. Because participatory research is a
social, relational process (Kral, 2014), so much so that some even have argued it
has similarities with social work (Atkinson, 2005), when a project ends and the
researcher moves on, participants can feel abandoned (Atkinson, 2005;
Northway, 2000). The very concept of a relationship “dissolves” (Hastrup, 2005,
p. 121). Northway (2000) explains that ending participatory or emancipatory
research has received little attention in the literature (see also Booth, 1998;
Goodley, 1999). One reason for this lack of attention has been posited: that
research is a process (Maguire, 1993) that may be transformatory, rather than a
transformatory event and therefore it is inappropriate to conceptualise an ‘end’ for
a participatory project. This seems to me to be a semantic distinction however,
research projects initiated and organised by academics have clear start and end
points and the issue of what happens when an academic finishes their work on a
particular project appears to be under developed in the literature. I will return to
this issue in subsequent chapters. As Booth explains, “participatory research will
always be about more than just including people in the research as, by its very
nature, the process of involvement compels the researcher to become part of
their lives too” (1998, p. 133).

This idea of a genuine relationship between researcher and participant implied by
the notion that the end of the project is an emotional wrench suggests that
researchers become more involved in their participatory work, feel somehow
responsible for the ongoing wellbeing of their former participants in a way that is
closer to activism than more conventional conceptions of research. The next
section deals with this issue.

3.10. Beyond action

The previous sections have discussed the theories of knowledge that underpin
participatory research and where the boundary between researcher and activist
might lie. Before I started to look for literature on the varying and potentially
overlapping roles of research and activism in participatory projects, I knew that
the boundary between social researcher and social changer would be blurred.
Richardson, Ellis and other feminist qualitative researchers state explicitly that
they intend their work to stimulate action and change (Richardson, 2000; Ellis, 2000), a process that they locate in their writing. But what about hands-on, messy, placard waving, Occupying activism and outright advocacy on behalf of marginalised participants? Using the legitimacy conferred on academics through our status as scholars to tell another version of the story? In this section, I explain that the boundary between activist scholarship and participatory research is blurred, sometimes conceptualised as a continuum. I show how once again these ideas meet in what appear to be emerging as the central themes of this work: feminism and writing.

In the participatory research tradition in which I locate myself researchers seem to regard themselves as operating primarily on the research side of the “researcher-activist fence” (Fine, 2006, p. 85); meaning that participatory researchers work with, rather than as, activists (Brydon-Miller, 2001; Fine et al., 2002). Fals-Borda describes this as becoming an “organic intellectual” (2001, p. 30) who seeks reference groups composed of grassroots leaders rather than professors; while later he advises participatory researchers that it is “not enough to be just an activist” (p. 31), I think he is referring to his idea of vivencia, i.e. participatory morality as a way of life, not just a professional identity. In their writing, participatory researchers seem to construct their role as one of raising consciousness (c.f. Freire, 2000) to awaken, as well as the oppressed, those with material and cultural power (Apfelbaum, 2001). This appears distinct from the more direct action described by researchers like Paul Chatterton, for example, who has participated in boycotts of publishers because of their links to the arms trade and who has designed a Masters programme in Activism and Social Change, in which students and tutors are encouraged to embrace “visions for social change as part of what has become known as the “anti-capitalist”, “anti-globalisation” or “global justice” movement” (2008, p. 424). For Chatterton and his academic-activist colleagues, participatory research and “activist research” (Askins, 2009, p. 5) or “activist-scholarship” (D’Souza, 2014, p. 1) are not the same things (Chatterton, Fuller and Routledge, 2007). Activist scholarship appears to be a very broad collection of works ranging from the study of social
movements (e.g. Goodwin and Jasper, 2003) to a sociology for changing the
world (e.g. Frampton et al., 2006).

How is activist scholarship distinct from participatory research? One way that
Chatterton, Fuller and Routledge (2007) distinguish between the two is
participatory research’s perceived focus on research to inform service delivery or
policy, rather than research that can be used as a vehicle for radical social
transformation, of liberation from neoliberal hegemony; not the constitutive forces
of the discourses imposed by the latter (Davies, 2005) but the active pursuit of
change. How is change pursued more actively by activist researchers than by
action researchers? Chatterton et al. outline a methodology of commitment to
social transformation via challenging existing power structures, building solidarity
through admitting to and applying emotion like defiance, resistance, a sense of
injustice (Newman, 2006) and building spaces for critical dialogue (Routledge,
1996). In essence, they claim, activist rather than action researchers need to be
the change they seek. This chimes with Maguire’s (2001) summary of a feminist
approach to research. Taking this perspective to a conclusion, it could be argued
that more conventional researchers are being the system they want to preserve.
This is an issue that will be explored in concluding chapters.

It seems that the distinction between participatory and activist researchers is
partly a question of what the different traditions aim to achieve, and partly how
they achieve them. My interpretation is that the root of the difference has a great
deal to do with researcher positionality. A participatory researcher is encouraged
to be carefully reflexive about their positionality, to avoid “perpetuating neo-
colonial representations, having Western biases, and purporting to speak “for”
participants (Sultana, 2007, p. 375), whereas an activist researcher aligns
themselves directly with the marginalised group involved. So an activist
researcher goes beyond collaboratively producing knowledge with participants to
challenge oppressive power structures (Maguire, 2001), instead they work
alongside them to achieve social transformation. Implicit in this understanding for
me is that the activist researcher only chooses projects that suit their own beliefs
about what social changes are necessary, which Chatterton et al. acknowledge
may be problematic for those who hold researcher impartiality as an important
feature of research. However they identify a role for intervention and critique in the activist researcher’s repertoire as well, which may involve advising groups with an alternative worldview. Pain (2003) approaches the distinction somewhat differently, identifying three modes of research that she calls activist, participatory and policy; which, she argues, in practice often overlap. Indeed, she posits that activism “exists on a continuum and is embedded to some extent in all our activity as academics” (p. 652), citing as her example movements to challenge racism within the academy. Ruddick (2004) writes eloquently of the “undertow of biography that pulls us towards one issue or another” and the “sustained personal connections that keep us there” (p. 229). This perspective certainly chimes with my own experiences of the sorts of research that attracts me and the sort I would prefer to deflect; this seems to be a new sort of positionality, based on issues that a researcher cares about and can sustain a personal interest and energy for. A connection with feminist research once again, in the observation that second wave feminists sought to redefine power as energy, strength, motivation and successful mobilisation of resources for oneself and others (Maguire, 2001).

Kye Askins is one researcher who appears to have blurred the divide between participatory research as consciousness raising scholarship and activism. Or perhaps rather than blurring, she has built a bridge between the two approaches and crossed it. In her 2009 paper, she explains how despite efforts to be participatory and “non-extractive” (p. 5), she still felt that the confines of the research process she had followed left her in a position of power relative to her participants with which she felt uncomfortable. Consequently, once she had completed her PhD, she engaged in research in a different way, starting as a volunteer with a project to provide space and support for asylum seekers and their families, which morphed into a long-term ethnographic “activist research” (p. 5) project. Emotion appears to have been the key to Askins’ transformation as a researcher; emotion underpinned her ‘ideological commitment’ to social and personal change (Fuller and Kitchen, 2004). She relates her deconstruction of the boundary between research and activism to feminist and critical academics and
her desire to break disciplinary convention in her writing, inspired by the work of Hélène Cixous (1976; 1991).

Feminism, again. The significance of writing, again. Given these links it is interesting to me to note that the perceived researcher-activist separation may go back to the science-literature divide that Richardson (1997; 2000) identifies as unhelpful in her own work (for a full elaboration, see section 4.10). D'Souza (2014) writes of the Asian and Middle Eastern poet-saints, scholars she regards also as activists as we would understand the term today. But, D'Souza explains, the poet-saints too fell victim to colonisation and Enlightenment rationalism, creating a division between poetry and scholarship, literature and science, practice and theory. But fascinating as this exploration may be, because of the breadth and depth of activist scholarship I will need to set boundaries around what can be included in this thesis; therefore I have confined my discussion in this section to activism and activist scholarship as it meets participatory research. As described in the previous section, participatory research academics are concerned with the problem that the underlying theory of social transformation, of freedom from oppression, does not necessarily translate to changes in practice. Implicit in these debates, I think, are questions of social activism, particularly because they frame the disconnect between theory and practice as a weakness, a problem that needs to be resolved. In summary then, participatory researchers seek counter hegemonic knowledge (Kindon, Pain and Kesby, 2007), whereas activist researchers adopt an end goal of social transformation itself. The next section tackles the issue of space in participatory research.

3.11. Ghetto pride

Kesby (2005) argues that participation and empowerment typically are conceptualised temporally rather than spatially, partly as a consequence of the idea of working towards “enlightenment” (p. 2052) over time and partly because of the way that a project life cycle is conceptualised as something that progresses through time. Yet the idea of space is tacit within many descriptions of participatory working. For instance, in just one paper describing a project,
incarceration was described as a time when women may have access to a “safe space” (Fields et al., 2008, p. 79) and a workshop was referred to as “an alternative space” and “a co-operative space” (ibid.). Yet despite its evident metaphorical and linguistic significance, it is argued that recognition of physical space as it relates to participatory research is under-developed in the literature (Kesby, 2005; Cahill, 2007a). It is natural perhaps that social geographers like Kesby, Cahill, Kindon et al. argue for the central role of space in the research methodology they prefer. But they do seem to have a point about the lack of focus on space by researchers from other disciplines; in the entire Handbook of Action Research, for example, there are but four references to space, two of which are philosophical (Eikeland, 2001; Reason and Bradbury, 2001) and two metaphorical: representing the ‘space’ available for certain behaviours in a social hierarchy (Ludema, Cooperrider and Barrett, 2001) and a version of Derrida’s “becoming space… that supported open, unstructured exploration” (Treleaven, 2001, p. 263) in the action research project.

An overemphasis on time and metaphorical space risks overlooking physical space, the “politics of location” (Cahill, 2007c, p. 268), which has an important relationship with social disadvantage and exclusion, as I explained in the previous chapter. Indeed my own experience of participatory endeavour has been that often it exists more significantly in space than it does in time, by which I mean that if you took the process to a different place, even to the next town, then it would evolve into something very different; whereas time seems to be of less consequence. My experience allows me to nod along to social geographers like Kesby (2007), who point out that every location has a history and associations; a community centre might have a past that will affect how it is used in the participatory process, a religious site might encourage participation of older groups, a youth club that of younger. It seems obvious, and therefore surprising that it has received relatively little attention in the mainstream literature.

Cornwall (2004) proposes a distinction between what she terms ‘invited’ and ‘popular’ spaces. Invited spaces are, as their name would imply, spaces into which people are invited in order to participate; examples would include a Participatory Rural Appraisal event or citizen jury. These invited spaces bring
together actors who would not normally be in proximity and who possess varying social status. Conversely, popular spaces are those in which people join together in everyday sociality, to engage in collective action or support. There are examples of both invited and popular spaces in Chapter 5. But for Kesby, Kindon and Pain (2007), this taxonomy sets the two types of space too far apart and too much in opposition, thus replicating conventional understandings of power and how it is exercised. They argue that invited spaces can be radical and popular spaces conservative and that the modalities (Allen, 2008) of power and spaces become entangled (Kesby, 2007). This is a really interesting debate, but I am in danger of following it away from the main path of this thesis. So what do these emerging ideas of space mean for my work? I conclude for now that to the list of concepts that are under explored in the social marketing and wider social and behavioural change literature, which includes feminism and writing, I add the politics of space and location.

3.12. Concluding comments

In this chapter, I have defined participatory research as an orientation to enquiry that turns the conventional dynamic of studied and student on its head, deregulating and deobjectifying researcher, researched and process from beginning to end. Acknowledging the long and complex heritage of participatory techniques, nevertheless I distinguish between southern and northern traditions, based upon the latter’s emphasis on reflective practice and the former’s explicitly radical aim for conscientization and social transformation. Epistemologically, I note that participatory methods are conventionally associated with Habermas’ writings on communicative action and the public sphere but choose to align myself with the emerging feminist post-structuralist perspectives on participatory research, embracing a concern for issues of identity and subjectivity, language and representation as well as the politics and power dynamics that have always been a cornerstone of participatory approaches.

I identify several areas in which participatory research touches, overlaps and blurs into others areas of interest; in particular feminist research, writing and
activism, activist scholarship, and the under-developed role of physical space. While the sheer breadth, depth and quality of the literature in all these directions daunts me, I feel energised and enthused to move forward in the development of my ethnographic self. The next section explains this idea, as well as providing a more typical elucidation of the methodology that underpins this thesis.
Chapter 4: In which I say how I know

"'Would you tell me, please, which way I ought to go from here?'
'That depends a good deal on where you want to get to,' said the Cat."
(Alice’s Adventures in Wonderland, p. 39).

4.1. Introduction

It is important to clarify that the methodology of the project in Parkhood and Urbanwood and the methodology that underpins the contribution to knowledge presented in this thesis are not the same. The methodology that most closely resembles the project is participatory research, as outlined in Chapter 3, whereas the contribution to knowledge presented in this thesis was developed using autoethnographic methods. Autoethnography is an eclectic method (Chang, 2008), which has been defined as “research (graphy) that connects the personal (auto) to the cultural (ethnos), placing the self within a social context” (Reed-Danahay, 1997, p. 145). This chapter introduces and explains autoethnography, which rests upon a re-interpretation of data generated during the project and upon the reflective writing process itself; consequently, in some sections, the analytical processes themselves have been documented autoethnographically. I begin with an explanation of my ontological and epistemological assumptions followed by a critical review of the denunciations that have been offered of my chosen methodology. Next comes an explanation of my positionality with respect to the research and an elaboration upon what criteria exist for making an assessment of the quality of an autoethnographic work. I conclude with two narrative accounts on how the data was analysed and theory brought to bear. I close the chapter with a review of the ethical issues I considered in both the original participatory project and the subsequent autoethnographic study.
4.2. Research aim

As described in my introductory chapter, the aim of this research is to expose and contest unjust and alienating language, discourses and power structures, in particular the ways in which these discourses and power dynamics are typically unacknowledged in conventional modes of commissioning, implementing and evaluating social marketing initiatives in deprived neighbourhoods.

4.3. Ontology

In 1994, Guba and Lincoln identified the four over-arching paradigms that frame research: the received wisdom of Positivism and Post-Positivism and the postmodern paradigms of Critical Theory and Constructivism. Subsequently, Heron and Reason (1997) argued for an alternative argument for “a worldview based on participation and participative realities” (p. 275), which has been added to Guba and Lincoln’s analysis in subsequent editions (2005). This section locates this thesis in a relativistic ontology, justified with reference to the foremost critiques of this position.

Guba and Lincoln (2005) provide a helpful table to summarise the various implications of the main paradigms of inquiry. According to their categorisations, a constructionist - they term it constructivist but following Burr’s (2006) advice these terms are treated as synonymous in this thesis - paradigm is underpinned by a relativistic ontology, local and specific; rather than the ‘real’ reality, whether understood via naïve or critical realism, which underpins the positivist and post-positivist paradigms. The field of public health tends to embrace realist ontology; emphasising static and objective knowledge about a reality separate from discourse. In common with other ‘scientific’ (Loughlin, Lewith and Falkenberg, 2012) approaches, the researcher in public health strives for findings that are free from bias, where the context is controlled as far as possible and propositional knowledge (Guba and Lincoln, 2005) that separates research from practice (Israel et al., 1998) is prized. Conversely, the critical paradigm is founded upon
historical realist ontology (i.e. a conception of reality that is shaped by social, political, cultural and economic values). Disentangling the appropriateness of historical realist ontology from the relativistic assumptions of constructionism has been problematic for this thesis, because of the significance of the social, political, cultural and economic aspects of social deprivation experienced by the participants in the project and because of the critical heritage of participatory research methodologies (Ozanne and Saatcioglu, 2008). It could be argued that the research can draw upon both: it is recognised that a methodology can emerge from an amalgamation of different disciplines and perspectives, an “interbreeding” of paradigms (Guba and Lincoln, 2005, p. 192; Ozanne and Saatcioglu, 2008) and consequently, an examination of relevant paradigmatic differences, controversies and contradictions may be more helpful than attempting to categorise methodologies with rigid adherence to universally applicable rules. However, despite the political and social commentary emerging from the autoethnographic process, it is concluded that a relativistic ontology (Johnson and Duberley, 2000), rather than that of naïve, critical or historical realism, is the most suitable for this work. I contend that the reflective account, if applied to a context where issues of social inequality were less significant, would lose its apparent connection to historico-realist ontology and yet retain its ontological anti-realism.

Anti-realism has many critics, who argue that the perspective is simply idealistic (Collin, 1997), that it fails to account adequately for the ways that discursive practices must be grounded in an external reality (Nightingale and Cromby, 2002) and thus commits the “epistemic fallacy” (Bhaskar, 1975, p. 17) resulting in a partial and potentially misleading picture of the world (Williams, 2003). Williams criticises both naïve realism and anti-realism from the perspective of critical realism, which has been pronounced a third way (Sayer, 1992; Vandenberghe, 1999), i.e. an approach that retains an acknowledgement of an external reality that can be known without ignoring the mediating role of language and subjectivity. In this way, critical realists aim to solve the epistemic fallacy by seeking to explain phenomena and the relationships between them, rather than simply to describe (interpretivism) or demonstrate causation (positivism) see
These criticisms might be a problem if the contribution of this thesis rested upon the way that alcohol is used in deprived neighbourhoods or upon the idea of social deprivation itself as an object, a phenomenon that existed independently of the way that we discuss it and theorise about it, and sought to present new understanding. Rather it is argued that to adopt a realist perspective when seeking to reflect upon the participatory process would be to fail to recognise that “…the ground, on which [I] stand to frame its world, is [my] own creation.” (Reason, 1998, p.11). Nevertheless, I do not reject an objective external reality out of hand; rather I am “agnostic” (Nightingale and Cromby, 2002, p. 703) about the status of such a reality. What is questioned under the relativist ontology adopted here is the status of the interpretations of and truth claims made in the discourses (Done et al., 2011) about such a world. The next section explains the treatment of truth claims in this thesis in more depth.

4.4. Epistemology or anti-epistemology?

According to Anderson (2006), autoethnography has been advocated primarily by scholars with postmodern or post-structuralist sympathies; Richardson (1997) for example situates her work within a feminist post-structuralist tradition (Done et al., 2011). Such beliefs are founded in a rejection of epistemic absolutism, i.e. the conviction that there is one absolute authoritative epistemic standard against which the veracity of truth claims can be judged (Luper, 2004), where ‘truth’ is understood as an accurate representation of an independent reality (Smith and Hodkinson, 2005). This section clarifies the epistemological underpinnings of this work, with a nod once again to those who have criticised this position.

Hammersley (2008) writes that this postmodernist - a term he adopts to encompass the post-structuralist movement as well - conception of truth in social research focused in particular on what he calls the “crisis of representation” (p. 130) in anthropology: a rejection of the assumption that anthropologists can claim legitimately to produce factual representations of people, cultures and social interactions, representations “of what it is like to be someone else” (Van Maanen,
What ethnographers write, “cannot but reflect who they are, in socio-cultural terms” (Hammersley, 2008, p. 131); and it hasn’t escaped the attention of these scholars that most ethnographers have tended to be white, Christian, middle-class, heterosexual, able-bodied and male (Ellis, Adams and Bochner, 2011). Consequently, autoethnographers like Richardson, St. Pierre, Ellis, Bochner and Denzin reject the idea that they can represent others in thick description, rather they believe that language “…is how social organisation and power are defined and contested and the place where one’s sense of self is constructed…language is a site of exploration and struggle” (Richardson and Adams St. Pierre, 2005, p. 961). Power is particularly important on at least two counts: the first because of the dominance of the research establishment by socio-culturally powerful groups (male, middle-class) and linked to this, the ways in which the powerful discourses of rationality and scientism are thought to be oppressive, supporting replication of the status quo by privileged groups (Pascale, 2010). In essence, from this perspective knowledge is inescapably political; power is the fundamental concept in social science (Russell, 1938).

The consequence of these arguments about the impossibility of knowledge free from political influence is a relativist epistemology, which some have suggested is not an epistemology at all; rather it is an inescapable human condition (Schwandt, 1996) with which we must learn to live. This perspective has implications for the criteria against which this research can be judged, which are explored later in this chapter; and for the theory of knowledge that underpins the autoethnography presented in this thesis. The latter is addressed in the remainder of this section.

For Smith and Hodkinson (2005) these postmodern ideas represent an end to the epistemological endeavour, an evolution in metaphor from discovery of truth to construction of meaning. Social research becomes a practical and moral affair, the core of which is a healthy scepticism that any way of knowing has the right to claim a privileged position of authoritative knowledge (Richardson and St. Pierre, 2005). In sympathy with this view, this thesis is approached from social constructionist perspective, about which Burr (2006) identifies four characteristics: firstly, in opposition to the assumptions made by positivists and
empiricists that what exists is what can be revealed by objective, unbiased observation, proponents adopt a critical stance towards conventional ways of understanding the world, including themselves. Secondly, it is assumed that all ways of knowing are culturally and historically relative. Thirdly, what we regard as “truth” is not a product of objective observation of an independent reality, but of the social processes, practices and interactions (such as language) that fabricate what is regarded as knowledge of what is real. Finally, this perspective recognises that socially constructed knowledge and social action are entwined. In an example of the latter very relevant to the context of this thesis, Burr highlights the way in which alcoholics have come to be seen as victims of their addiction, with an appropriate response being medical treatment; rather than ‘drunks’ assumed to be responsible for their behaviour and therefore punishment being regarded as a suitable solution.

This position is not without its critics, those that have called for a neo-realist (or critical realist) perspective, perhaps most notably Popper (1979), see also (Bhaskar and Lawson, 1998; Manicas and Secord, 1983; Hammersley, 2008). Their objections appear to be founded upon five pillars: firstly, that it would be impossible to function as an epistemological sceptic in daily life, that nobody could live without a “commonsense theory of knowledge” (Popper, 1979, p. 3). Secondly, it is argued that it is possible to accept that language is not fixed and absolute and still use language precisely to communicate meaning in a way that is compatible with realism (Hammersley, 2008; 2007). Linked to this point about language, Hammersley argues further that it is still possible to claim knowledge about the world while stopping short of the claim to know its one essential nature. Next, Hammersley points out that while rationalism and the scientific method can be used in ways that are oppressive, they have also been used to such ends as to combat biological arguments for racism, for example. His point being that the method itself is not intrinsically oppressive. And finally, Hammersly argues, drawing upon a flamboyant piece of satirical writing, that postmodern qualitative research isn’t useful to policymakers, rather as if a dentist adopted such principles in their practice:
“I can’t work to order like a common tradesman! I decided her bridge should be enormous and billowing, with wild, explosive teeth flaring up in every direction like fire! Now she is upset because it won’t fit in her mouth! She is so bourgeois and stupid, I want to smash her! I tried forcing the false plate in but it sticks out like a star burst chandelier. Still, I find it beautiful. She claims she can’t chew! What do I care whether she can chew or not!” (Allen, 1972, p. 199).

Some anthropologists also express some concerns with the degree to which the ethnographer’s own interpretation may be privileged when adopting a postmodern epistemology. Ryang (2000) for example, wonders whether an autobiographical approach both alienates and privileges the ethnographer by failing to place personal experience in a social, cultural, and historical reality.

While committed post-structuralist autoethnographers like Richardson and Denzin reject these criticisms, others have argued that it is possible to merge the realist and postmodern perspectives in autoethnography (Ronai, 1996; Frank, 2000; Newmahr, 2008). Newmahr adopts a subjective perspective - which she distinguishes from self-reflexivity - to an ethnographic study of the sadomasochistic community. Deliberately, she interweaves between a treatment of herself and others as analytical subjects; while she maintains a subjectivist epistemology, she is able to layer a somewhat distanced analysis more typical of a realist ethnographer (Denzin, 2006) with narrative passages representing fieldwork experiences. This approach is the one I aimed to adopt in my own analysis of the participatory project, which is described in Chapter 5. The next section provides further elaboration on my autoethnographic method.

4.5. Why autoethnography?

“I wonder if I've been changed in the night? Let me think: was I the same when I got up this morning? I almost think I can remember feeling a little different. But if I'm not the same, the next question is, who in the world am I? Ah, THAT'S the great puzzle!” (Alice, Alice’s Adventures in Wonderland, p. 8).
The story of why autoethnography was deemed the most suitable methodology for this research begins at the end, with the notion that writing is itself a way of knowing (Richardson and St. Pierre, 2005). I began the process of writing this thesis assuming that the methodology that would underpin my PhD research would be the same methodology that was employed in the participatory project in Parkhood and Urbanwood: participatory research. But when I began to write, as I described in earlier chapters, it became obvious very quickly that participatory research was not the methodology I was using in this PhD. I wanted to study the participatory methods themselves; falling back upon traditional conceptions for a moment, they were the ‘object’ rather than the means of knowledge creation. This section explains my reasons for choosing autoethnography as method over reflective practice, as well as explaining why other possibilities would have been unsuitable in light of my aims and perspective.

In action research, the term ‘reflection’ appears to be synonymous with ‘thinking’ in the sense that it relates to acts of cognition that occur during learning (Csikszentmihalyi and Sawyers, 1995). Reflective practice on the other hand can be conceptualised as an “epistemology of practice” (Schön, 1983, p. 49) that consists of three types of reflection: reflection-in-action, reflection-on-action and reflection-about-action. The first encompasses the implicit thought processes that accompany and continually modify and reframe (Loughran, 1996) ongoing practice. The second type relates to a retrospective analysis of past practice so as to gain knowledge about those experiences; according to Russell and Munby (1992, p. 3), this is a “systematic and deliberate thinking back over one’s actions”. The third type of reflective practice has roots in critical theory (Leitch and Day, 2000) and is intended to stimulate reflection upon the social, economic and political aspects of one’s practice. In this way, reflective practice has also been defined as an “acquisition of a critical stance or attitude towards one’s own practice and that of one’s peers” (Johnston and Badley, 1996, p. 4).

It is this critical stance, reflection directed outwards to the social system of public health commissioning as well as directed inwards to individual practice, that seemed to link this thesis to ideas of emancipatory action research (Grundy, 1982). At first glance therefore, reflective practice seemed ideal as a way to
reflect critically upon the practice of participatory methods applied to changing behaviour in deprived neighbourhoods. Another advantage was that action researchers ‘freeze’ moments of their practice for later reflection, analysis and interpretation. (Davis, 2004). This seemed appropriate because I was commencing my PhD research after the project itself was completed; I was engaging in retrospective analysis of the process and the outputs, guided by project documentation, field notes and records. The type of reflective enquiry in which I intended to engage can be carried out in a variety of ways, including autobiography, dialogical conversations, fictional stories (Evans, 1994), reflective writing and journals (Holly, 1989).

The notion that writing, not just to inform but writing as a creative, generative process can be a legitimate way of knowing in and of itself (Davis, 2004) was without doubt the most significant epiphany of this thesis. Until I stumbled across the work of Laurel Richardson and Elizabeth Adams St. Pierre, I had no conception that the way ideas came to me and developed through the words appearing on the screen, seemingly of their own volition and sometimes surprising me with their insights, could be a valid way of creating knowledge. It was this insight that led to the final choice of autoethnography. The contribution I wanted to make with this thesis was located in a critical exploration of the use of participatory methods to change behaviour in deprived neighbourhoods: to bring together my reading from the literatures on participatory research, community development and social deprivation with the literatures on behaviour change in general and social marketing in particular, to explore fully some of my discomfort with the lack of reflection upon the theoretical, practical and ethical compatibility of participatory methods with the managerial approach to achieving change for social good that dominates in social marketing (Lee and Miller, 2012).

So far, this section has explained why autoethnography was chosen over reflective practice and participatory research; but this explanation seems incomplete without a consideration of whether other, more traditional research methodologies would have addressed the research aims adequately. Alternative methodologies can be divided into two broad categories: an approach situated in the positivist paradigm, probably quantitative; or qualitative methods like depth
interviews or focus groups, founded in interpretivism. Positivistic, empiricist methods were rejected on two counts: firstly that they position the researcher as an objective spectator (Smith and Hodkinson, 2005), rather than as the active participant in knowledge creation that I have been in writing this thesis. Secondly, I contend that scientific approaches are philosophically ill positioned for critical reflection about participatory methods (themselves described as political methodologies of empowerment, Rahman, 1995); associated as they are with imperialism, colonialism (Smith, 1999) and the location of power with the researcher in crafting their representations of the other (Denzin and Lincoln, 2005) as objective truth. In this autoethnography, I only ascribe truth to my representation of myself, see later sections for elaboration. More conventional interpretivistic forms of enquiry could have been suitable for addressing the research question, if the primary interest had been in what other project participants had thought about the role of participatory methods in public health. But while I am not so arrogant as to assume that my opinions are of higher worth than theirs (indeed their views are an integral part of the analysis) I can only see them through my eyes; in analysing and writing about their perspectives I would still be making assumptions about their truth, filtering their words through my writing as a way of making sense of the whole and integrating it within the thesis. It seems more honest, more authentic, to acknowledge my dominant position as the ‘sense-maker’ of all this information, as the arbiter of what is important, what is included or not and what it all means. Thus, autoethnography a methodology that seeks to understand the role of the self in the culture of interest seems the most appropriate choice for this work. The next section offers a fuller exploration of the autoethnographic method, beginning with a review of its roots in 1970s anthropology of familiar cultures and a response to the prominent critiques.

4.6. Academic narcissism and other critiques

The increasing popularity of studying familiar cultures, driven partly by the observation that western anthropologists could rely less on protection from “friendly colonial authorities” (Hayano, 1979, p. 99) and partly by a growing
number of “minority” [sic] anthropologists who wanted to study their own cultures, inspired David Hayano to coin the term autoethnography in 1979 (Boloz, 2008). Hayano in turn traces his idea to his recollection of an anecdote told by Sir Raymond Firth in 1966 (Hayano, 1979). More recent literature on autoethnography suggests that several versions of the methodology have emerged in the last forty years, synthesised by Doloriert and Sambrook (2011) into evocative, analytic and political autoethnography. Evocative autoethnography, also named Creative Analytical Practice (CAP) ethnography (Richardson and St. Pierre, 2005; Denzin, 2006), is described as a process where method and writing are closely entwined and the producer of knowledge cannot be separated from the knowledge itself or the mode of its production (Ellis, 2004; Ellis, Adams and Bochner, 2011). Holman Jones (2005, p. 763) writes of autoethnography as “a radical democratic politics”, sparking social change by creating space for dialogue and debate. It could be argued that these two versions, evocative and political, are so similar in aim and epistemology as to render distinction unnecessarily complex: Doloriert and Sambrook (2012) identify evocative ethnography as interpretivist, but scholars from within that tradition self-identify as post-structuralist, see (Richardson and Adams St. Pierre, 2005).

Analytic ethnography, on the other hand, is offered to those who feel that an alternative approach, one that harks back to the realist ethnographic tradition of the Chicago School, is called for as an alternative to the postmodern or poststructuralist “moment” (Denzin and Lincoln, 2005, p. 19) represented in part by evocative ethnography (Anderson, 2006).

Ellis and Bochner (2006) don’t agree with these classifications. For them, autoethnography is its own genre of ethnographic writing, one that is ontologically and epistemologically distinct from modernist or realist texts. They do not think that what Leon Anderson (2006) calls analytic autoethnography is authentically autoethnography at all, with Art Bochner suggesting provocatively that analytic autoethnographers rename their approach “aloof autoethnography” (Ellis and Bochner, 2006, p. 436) to capture more effectively its mood and tone. Ellis and Bochner’s point is that their own work, creative, literary and often playful in style as it may be, is analytical as well as evocative. The task of analysis and
theorising, they assert, is accomplished by stories, which they regard as meaningful phenomena that help people to make sense of themselves and of others (Adams, 2008; Bochner, 2001). Autoethnographers use stories to capture significant moments that have led them to rethink or re-evaluate a situation leading perhaps to a change of direction (Bochner and Ellis, 1992; Denzin, 1989) a new perspective or the ability to close a gap in existing stories (Goodall, 2001). While this type of autoethnography is heavily associated with autobiography (Pace, 2012), autoethnographers, like ethnographers, purport to set themselves the extra challenge of using the tools of research methodology and research literature to retrospectively analyse their stories, their epiphanies, so that others may learn from their personal experiences (Ellis, Adams and Bochner, 2011). Not everyone is happy to accept Ellis and Bochner’s claim to the analytical legitimacy of such personal narratives, however.

Atkinson’s critique of what he terms “contemporary fashions for subjective and evocative ethnographic work” (2006, p. 400) appears to be founded less in an objection to the postmodern foundations of this work and more in what he regards as the tradition’s exaggerated claims to novelty, both epistemologically and as method. Other critiques of autoethnography include an anonymous colleague of Andrew Sparkes, who characterised it as “academic wank” (2002, p. 212); and Delamont’s (2007) depiction of the method as “almost entirely pernicious…literally lazy and also intellectually lazy” (p. 2) and a “narcissistic substitution” for research (2009, p. 51). Delamont defines autoethnography with reference to Anderson (2006) as “texts which claim to be research but in which the only topic or focus is the author” (2009, p. 57) and thus separates it from critical autobiographical reflection upon fieldwork. Using this distinction, I could perhaps wheedle my way out of the need to respond to her critique, as this thesis is not a text that is only about me. But I feel compelled to offer a response nevertheless; I want to home in on her criticisms, perhaps partly for emotional reasons (narcissistic, moi?) but more importantly because I feel I can offer a worthwhile response to them. Not least because when I did what I could claim as ‘fieldwork’ (i.e. the project in Parkhood and Urbanwood) I wasn’t approaching it as such in methodological terms.
In 2004 and 2009, Delamont outlines a manifesto of six criticisms of autoethnography: that ethnographers should fight familiarity, however difficult it may be to do so, and that autoethnography embraces it; that autoethnography cannot be published ethically because of the dual problems of obtaining informed consent and preserving confidentiality (this I will tackle in section 4.12); that research is supposed to be analytical not experiential, and she is not convinced by Ellis, Bochner et al., that autoethnography is analytical enough (Atkinson, 2006; Atkinson and Delamont, 2006); that autoethnography focuses on the experiences of the powerful, not the powerless and unvoiced, as ethnographers ought; that ethnographers should be studying social worlds that are worth researching, i.e. those that are unknown and interesting, and the minutiae of the bodies and social lives of social scientists are neither unknown nor interesting; and finally that academics paid via public funds have a responsibility to do more than sit in comfortable offices introspecting about their lives, they should be out collecting data. In her 2004 conference paper, Delamont warns that she is deliberately controversial; I have been similarly robust in my consideration of her views.

What is interesting to me about Delamont’s perspective is that she doesn’t appear to acknowledge her own subjectivity. She seems to be claiming to know what is interesting, important, unknown (to which I will return in a moment) and worthy of public funding; she is claiming the authority to decide who is powerful and who isn’t without examining her own privilege and the assumptions that I argue must follow. She appears to stand primarily upon a relatively unacknowledged world of her “own creation” (Heron and Reason, 1997, p. 275) while criticising others for laying theirs bare. As well as an interest in and affinity with writing, what attracted me to Laurel Richardson’s version of autoethnography was its potential to deal with power, subjectivity and privilege in the research process. I concluded that Richardson’s articulation of autoethnography was the most honest and transparent way to tackle the power dynamics inherent in the process. I wanted to avoid committing the crime of privileging the experiences of the powerful, and I think this can be achieved by acknowledging that the only perspective to which I can realistically claim any real knowledge is my own.
No amount of data can elevate me to the authority to tell other privileged academics what it is really like to live in Parkhood or Urbanwood (though I can get quite passionate about it if they ask me, as my supervisory team can attest). I am not a survivor of domestic abuse; I have never felt compelled to hide any bruises because that's just what women do. While I have felt the loneliness, exhaustion, tedium and guilt of new motherhood, I have not felt it alone day after day in a one bedroom flat with mould growing on the walls and noisy, aggressive neighbours. I can tell these stories in what I hope is an evocative and interesting way, but I cannot and should not claim them as my stories to tell; all that is mine to offer is my interpretation, my account from my perspective. And to regard them as ‘unknown’ and ‘interesting’ is to offer an insult to the people who would be very happy for these experiences to be unknown to them, to have the privilege of enough distance to be able to summon an academic curiosity about them. If I may offer an introspective observation here, the notion that these lives are unknown and interesting and therefore worthy of study by scholars, who will produce texts to be read by other scholars, evokes in me a certain outrage on behalf of the people who must live them.

In this way, the method I have used in this thesis draws upon ideas from radical participatory researchers and activist scholars, of the former in particular Fals-Borda’s depiction of finding “little use for scholarly arrogance”, learning “instead to develop an empathetic attitude towards Others which [they] called vivencia, meaning life experience” (2001, p. 31). Vivencia is identified with the work of Spanish philosopher Jose Ortega y Gasset, and occurs when something is understood intuitively, while placing oneself in a wider context (Fals-Borda, 1984). I have experienced this personally as continued reinforcement of a realisation that what I have learned about people’s lives from reading about them invariably leads to prejudgement, to assumptions that are almost instantly dispelled by actually meeting and talking to members of that social group. My most recent opportunity to learn (once again) about my capacity to judge and the importance of vivencia came in a brief piece of fieldwork observing ex-soldiers on a team-building exercise; these men had struggled with their adaptation to civilian life, becoming homeless as a consequence. I had steeled myself for implicit and
probably explicit sexism, as well as the slight possibility of risk from men trained in violence (who are more likely than their civilian counterparts to commit a violent offence, MacManus, et al., 2013). Once again and as ever, these men were people with stories to tell, just like the rest of us. This is not to say that privilege is wiped away by autoethnography, but it does offer a way to name it as such and to set boundaries around what claims to knowledge one can make.

In conclusion then, an autoethnographic approach, for me anyway, serves as a reminder to respect, show compassion and not to privilege my interpretations as anything more significant than interpretations, epistemologically speaking. Autoethnography reminds me to be transparent in explaining how my interpretations were reached, to lay bare the reflective process in stories of intellectual wrestling with data and literature (more on judgement criteria later in this chapter). Earlier in this paragraph I made reference to the influence of activist scholars upon my application of autoethnography, a topic I will pick up in the next section, which continues this theme of positionality.

### 4.7. The ultimate participant

For the reader, viewer or listener, reflexive accounts provide invaluable insights into the personal perspective from which research was undertaken and interpreted, enabling others to make their own assessments about the conclusions (Saltzman, 2002). This section continues the direction I began in the previous section to articulate my own position with respect to this work.

According to qualitative researchers like Coffey (1999) and Anderson (2006), inward reflection has always been present to some degree in ethnographic research. Anderson argues that Robert Park’s interest in his students’ backgrounds encouraged them to draw upon personal experiences in selecting research topics, noting that they “walked the streets…worked for local agencies, and had autobiographical experience” of the communities they studied (Deegan, 2001, p. 20). But it is argued that these early Chicago School ethnographers, despite their connections to the settings they studied, did not attach particular
analytical importance to the role of the self in their research; they remained “professional strangers” (Denzin, 2006, p. 421). Reflexivity as relevant to this thesis is best understood as emerging from critical anthropology as a reaction against objectivist accounts (because, it was argued, knowledge of the social world cannot be generated independently of the researcher, Hammersley, 2008). Reflexivity can be characterised as an ongoing attentiveness to and assessment and reassessment of the researcher’s influences upon the research process and findings, a cyclical process that should lead to “further reflexive understanding, hermeneutic mediation, and philosophical critique” (Scholte, 1972, p. 431). In this way the ethnographer and their history becomes integral to the study. As long as they are in the field, and I would add as long as they are thinking and writing, everything they do and experience (and write) becomes data (Ryang, 2000).

Thus, the role of the researcher in autoethnography has been characterised by Robert Merton (1988, p. 18) as taking the role of “the ultimate participant in a dual participant-observer role”. In other words, the researcher is a member of the social world under study (Anderson, 2006). Evocative autoethnographers go further, aiming to produce texts that inspire the reader “to care, to feel, to empathize, and to do something, to act” (Ellis and Bochner, 2006, p. 422). There appear to be interesting and as yet tantalisingly unexplored links between these ideas and those of activist scholarship as outlined in Chapter 4, combining Ellis and Bochner’s exhortations to show vulnerability with Ruddick’s “undertow of biography that pulls us towards one issue or another” (2004, p. 229).

Consequently, reflecting upon one’s role in the research, one’s position with respect to the process as I attempt to show throughout the remaining Chapters, is thought to help bolster the work by enabling readers to judge for themselves how I as an ethnographer may have influenced aspects of the work, such as the types of information collected and the way it has been interpreted (Saltzman, 2002). Caution is advised though in adopting generic categories such as race, social class or gender to delineate positionality; while they can be useful, their utility must be elucidated in the narrative, not assumed to be self-explanatory, “ready to wear” characteristics (Robertson, 2002, p. 788). In common with a number of the other postmodern autoethnographers, I am female and feminist, I
am sensitive to issues of power and politics and I am deeply sceptical of claims to objective truth made about the social world; perhaps because the social world feels ‘made up’ to me, a series of rules of thumb that careful observation of skilled people will allow unskilled people to mimic in order to negotiate that world almost as successfully. I like to feel as though I am being as honest as my scholarship allows, acknowledging that there is always so much more to know. The next section expands upon this idea of honesty and transparency in the context of criteria by which to judge autoethnographic research.

4.8. Acting with conscience

In addition to the ontological and epistemological considerations outlined in previous sections, it is helpful to clarify whether claims to knowledge are judged by foundational criteria (i.e. criteria that are final and absolute determinants of a claim to truth) or nonfoundational criteria (i.e. complex, negotiated and specific to certain circumstances). This section addresses this issue by exploring the issue of criteriaology (Smith and Hodkinson, 2005) in research. Given the diverse influences that underpin the scholarship offered here, this section could easily expand into be a PhD in its own right, consequently this section is limited largely to a discussion of criteria developed to judge autoethnographic work. First I review briefly the role of criteria in research before explaining how autoethnographers have translated these ideas, concluding with an articulation of how I judge my own work.

For positivist or post-positivist scholars, truth can be claimed as a result of methodological rigour, typically quantitative, to remove bias and other forms of human contamination from scientific results (Guba and Lincoln, 2005). Early work to advance foundational criteria for qualitative research in social science included criteria to parallel those widely applied to quantitative research, such as credibility and transferability in place of internal and external validity, dependability for reliability and confirmability rather than objectivity (Lincoln and Guba, 1985). However, such criteria have been challenged on the basis that interpretive enquiry is incompatible with foundationalism, that foundational criteria would have
“a restrictive, limiting, regressive, thwarting, halting quality to them, and they can never be completely separated from the structures of power in which they are situated” (Bochner, 2000, p. 269; Smith, 1984; Smith and Heshusius, 1986). The distinction between positivist and non-positivist approaches is not straightforward; in critical theory, foundational truth claims are located in specific oppressive infrastructures (Guba and Lincoln, 2005). Therefore, the question of judgement criteria means that once again the intersection between critical and constructionist paradigms in this thesis needs to be explicit, as despite the appropriateness of critical theory for issues of inequality and social deprivation, criteria are not situated in historical structures, rather, consistent with a social constructionist ontology and postmodern epistemology, any claims to truth are based upon nonfoundational criteria: an unwillingness to accept that there are or can be universal standards for truth (Lincoln, 1995; 1998).

One of the criticisms of foundational criteria for non-positivistic research outlined above was that such criteria cannot be separated from the power and political structures within which they are set. The same is likely to be true for nonfoundational criteria, perhaps even more so since they are negotiated via a complex social and political process (Smith and Hodkinson, 2005; Bochner, 2000). Smith and Hodkinson define the political aspects of judgements about research quality as the process of allocating scarce resources; by resources they mean something that is desired but not available to all, examples include funding, recognition and prestige (ibid.). Politics is a theme that connects almost every chapter of this thesis, from the emancipatory ambitions of critical pedagogists (Freire, 2000; Fals-Borda and Rahman, 1991) to the structural inequalities underpinning poverty and deprivation (Hickey and Mohan, 2005; Mohan and Stokke, 2000). In the context of criteria, it is apparent at both macro and micropolitical levels, with influences upon the setting of research criteria for public health at a national level (e.g. guidelines for evaluating complex interventions published by the Medical Research Council, Craig et al., 2008) and institutional, group and individual influences upon what is regarded as ‘proper’ academic research (Hammersley, 2008; Delamont, 2007).
Another aspect to the contestability and politics of research criteria, rather an important one given the purpose of this document, is how institutions and examiners judge the quality of a doctoral thesis. Doloriert and Sambrook (2011) identify the issues that can arise when there is a mismatch between the autoethnographer’s conception of quality and that of their institution or examiners. Their example highlights the question of whether it is appropriate to present a thesis that ‘shows’ a PhD candidate’s development during the course of their study or whether the entire thesis should represent the polished pinnacle of their achievements: an airbrushed photograph rather than a documentary. Because of all these questions and because the form of creative autoethnography that I have chosen to frame this work is epistemologically and stylistically distinct, it seems sensible to explain how the leading scholars in that tradition have articulated the criteria by which they judge and seek to be judged in turn. I have been inspired in particular by the writings of two leading figures in this movement, professors Laurel Richardson and Carolyn Ellis; both of whom have given considerable thought to how this form of scholarship should be evaluated.

Carolyn Ellis adopts a very evocative style and demands a high standard from other autoethnographers whose contributions she reviews. She has experimented with form and format, publishing a methodological novel in 2004. Not unexpectedly, she has written a short story to explain how she judges a piece of autoethnographic writing. First, she wants to be engaged by the story, to be interested in what the author has to say both emotionally and cognitively. She wants to “feel and think… back and forth, until thinking and feeling merge” (Ellis, 2000, p. 273). After she has read the whole piece through and once she has distanced herself from this first reading, she reflects on her experience with the paper. Was it engaging enough that she read the whole thing through in one sitting? If not, if she experienced what she calls “distanced cognitive reading” when the piece “interrupts itself” (p. 274), then this might indicate that the writing hasn’t lived up to its evocative billing. But if she decides that a piece might be worthy of publication, then comes a stream of questions about what it might have taught her about social life, what might be new or innovative about the work; the authenticity of the story, its logic and flow, whether it rings true; the quality of the
writing in a literary sense, the goals of the author and whether they seem to have achieved them; whether the story seems likely to stimulate some form of social action. Finally, she considers whether the author has behaved ethically in their research and their writing. Ethical considerations go beyond the conventions of informed consent; Ellis questions whether characters have had the chance to contribute to the story and if not, is this omission justifiable? She asks if the characters (including the author) are sufficiently developed and complex and whether the contribution the story makes outweighs any inherent ethical dilemmas.

Though she has also written in experimental ways (Richardson, 2012; Richardson, 1997; Richardson, 2013), Laurel Richardson’s writing on evaluative criteria is less evocative in style than Ellis’s short story. Richardson’s criteria are based on her belief that ethnographic writing is an essentially human activity and that ethnography is created through research practice. Science, she writes, “offers some practices, literature, creative arts, introspection, and memory-work offer others” (Richardson, 2000, p. 254) and the ideal is to bring both creative and scientific lenses to bear on autoethnographic writing. Richardson’s criteria are near identical to those of Ellis, though more tightly corralled into five named categories. The first is substantive contribution. As Ellis, Richardson asks if the piece contributes to our understanding of social life but she goes further, asking that the contribution be “deeply grounded” (p. 254) in an understanding of a human world and that this understanding has informed the construction of the text. Richardson’s second criterion is aesthetic merit, which means, to her, that the text is “artistically shaped, satisfying, complex, and not boring” (ibid.), that creative analytical practices have opened up the text to interpretive responses from readers. Her third is reflexivity, which deals with the ethical issues around representation that Ellis identified but once again goes further, asking whether the writer has demonstrated enough self-awareness, exposed enough of themselves and their thought processes to enable the reader to arrive at their own judgement. Richardson’s fourth criterion is impact, which seems similar to Ellis’s desire to be engaged emotionally and cognitively. Richardson, like Ellis, thinks of the work’s impact on herself as a researcher, asking herself is if the
piece moves her to write, to experiment with different research practices, to ask new questions, to act. Finally and once again in concert with Ellis, Richardson desires that any creative autoethnographical text will seem to speak truth, that it embodies “credible account of a cultural, social, individual, or communal sense” of something that seems real (ibid.).

Richardson also writes that ethnographers “should not be constrained by the habits of somebody else’s mind” (2000, p. 254). Which is helpful to me, because the criteria I use to judge my own work, while similar to Ellis, Richardson et al., are my own as well. As highlighted in the previous section, I judge my work against the criteria of honesty: to what extent have I been honest with myself and transparent in the way that I have written my explanations, preserving the process by applying what Doleriert calls “logic in use” (2011, p. 587). I have great sympathy with Doleriert’s desire to preserve in her own thesis her development as a scholar, evidenced by what she calls the “rawness” (ibid.) of her early chapters. While I have made analogous attempts to conserve the authenticity of my own progressive narrative, my reliance on writing as an epistemic as well as a communicative activity has meant that earlier chapters needed to be rewritten to help prospective readers navigate the work without tossing it aside, frustrated by its lack of cohesion. This paragraph for instance, is one of the last to be made “differently contoured and nuanced” (Richardson and Adams St. Pierre, 2005, p. 964); the only section that remains unwritten at this point in the chronological story of my thesis is a very final piece of entirely experimental writing.

Other criteria I apply to myself include the question of whether I have been fair to the other scholars whose work I cite; whether I have given their ideas enough time and critical reflection so as to appreciate their complexity and context. Whether I have fought effectively my tendency towards the judgemental (it is actually easier to put this tendency aside when interpreting the lives of participants, much harder when critiquing the work of scholars who sometimes appear at first glance both privileged and complacent). To what degree have I been fair and ethical in my representation of others? Are they sufficiently anonymous in the work or do they need to be offered the opportunity to contribute, to set the record straight on their terms? And finally, more recently I
find myself questioning whether I have taken the easy path, the comfortable, low risk route. Consequently, due to my antagonistic relationship with foundational criteria and my attempts to present a document that enables the reader to judge its quality for themselves rather than asserting that quality is processually inherent, I would locate myself in the evocative interpretivism quadrant of Doloriert and Sambrook’s (2011) framework.

I have found it rather helpful to write about and therefore to reflect upon the criteria with which I would judge my own work; as I do so I notice how they are beginning to move away from the autoethnographic towards the activist, that as the story captured within this thesis unfolds I am hankering to take risks, to be more confident in laying claim to my beliefs about research and scholarship, to think about how I might use my work to make some sort of positive contribution to something. As I mention earlier, this tendency is given somewhat free rein in the final chapter with a piece of experimental writing, a “text of illegitimacy” (Richardson, 1997, p. 137). For the next section however, I explain how I performed the autoethnographic analysis that underpins the next three chapters.

4.9. Building blocks

Autoethnographers explain the nuts and bolts of their method as discerning patterns of experience evidenced by data, and then describing these patterns using facets of storytelling (Ellis, Adams and Bochner, 2011). This section explains what data was available to me and how it came into existence.

As explained in previous sections, the project that inspired the autoethnography was underpinned by an action research methodology. According to Kemmis and McTaggart, while there is “no clear agreement about the character of Action Research: what it is, what and who it is for, and how it should be done” (1988, p. 21), the heart of Action Research is the pursuit of change via the dual media of action and research (Davis, 2004). As has been explained in previous chapters, the method is not linear (ibid.), rather the metaphor of a spiral (Kemmis, 2001) is employed to conceptualise an evolving process of planning, action and reflection.
Because the processes of the project generated the research data, it is probably helpful at this stage to explain briefly how the project evolved.

The initial project plan outlined in May 2011 specified beginning with a process of asset mapping (Kretzmann and McKnight, 1993; Goldman and Schmalz, 2005). Asset mapping creates an “inventory of community strengths” (Baker et al., 2007, p. 438) and is thought to foster social capital upon which change can be built by emphasising and working with these strengths, rather than concentrating upon deficiencies and their solution. It was decided that the optimum way to begin the asset mapping for the project was to ask stakeholders to share their local knowledge. ‘Stakeholders’ for the purposes of this thesis can be defined as a person, group or organisation with an interest in the project or the neighbourhood. It was agreed that a workshop, to which a range of local stakeholders would be invited, was the likely to be the most effective way to introduce the project, seek input from stakeholders and enable them to contribute their knowledge of Parkhood and Urbanwood to the asset mapping process.

Building upon the local knowledge and relationships begun during the asset mapping work, the intended next step was to undertake a planning process to collaborate with participants to design suitable methods to answer the research questions about drinking. Following this, a period of fieldwork was planned, the exact nature of which would depend upon the outputs of the participatory process. The output from these first two stages of the project was intended to be a written report for the funder, and an appropriate format to disseminate the findings and conclusions to the participants and neighbourhood (this could be a video, road-show or posters for example, which we would be designed collaboratively with the participants). The nature of participatory methods is such that the further into the future one attempts to plan, the more vague those plans must be. Thus, phase three would be to continue the collaborative working via the co-design and implementation of an intervention aimed at reducing drinking levels in the two neighbourhoods, based upon the learning and the relationships and networks we developed during earlier stages of the project.
The research data that recorded the ethnosc, the moments of practice that were ‘frozen’ for later analysis and interpretation, emerged from the action research and include three types of record: Firstly, documentation created during the course of the project, including the original proposal to the funder and subsequent proposals for additional funding during the course of the project; the information submitted to the University’s ethics committee to enable the project to gain approval; 20 individual planning documents recording ideas and intended activities; a total of 12 internal and external progress reports and one final project report; a spreadsheet of stakeholders and assets and 2 conference papers written during and about the project. I wrote the majority of these documents, but other members of the research team were responsible for some. Secondly, 378 emails or email strings sent and received by me during the course of the project, between October 2011 and October 2012, totalling over 78,000 words provide a record of discussions, disagreements and decisions during the course of the project. Finally, over 48,000 words of field notes, “gnomic, shorthand reconstructions” (Van Maanen, 2011, p. 123) captured during the data collection phase of the project, written by Anna and based on conversations with 23 males and 41 females in Parkhood, and 19 males and 39 females in Urbanwood. The research questions for this phase were: Why do people drink, why do they feel they can’t stop and how does this affect them and those around them?

This section has reiterated that action research is a cyclical process of planning, action and reflection and explained the initial plan that was made for the project in Parkhood and Urbanwood. It has described the data that underpins the contribution of this thesis as ‘frozen moments’ of the practice that created the intervention and explained that this data is primarily documentation produced during the course of the project as part of the cycle of planning, action and reflection, supported by the record of written communication provided by the emails exchanged between project team members. The next section will explain how this data was analysed and how the analytical and writing processes themselves generated additional data.
4.10. Sense making writing

“Do not impose your own ponderous scientific style for communicating results, but diffuse and share what you have learned together with the people, in a manner that is wholly understandable and even literary and pleasant, for science should not be necessarily a mystery nor a monopoly of experts and intellectuals” (Fals-Borda, 1995, p. 1).

At core, the methodology I have adopted in this study is about making sense of phenomena by writing about them in an analytical way. But unlike realist ethnographers (Anderson, 2006; Atkinson, 2006), who can apply the rigour of analytical procedures like grounded theory (Strauss and Corbin, 1998), the autoethnographic literature is relatively light on practical suggestions for analytical writing practices, confining itself to vague references to discerning patterns of experience from data (Ellis, Adams and Bochner, 2011). Consequently, rather than describing the ways in which I have brought to bear established procedures, this section describes in some detail how I made sense of the data that I started with; how I transformed my collection of notes, documents, emails and memories into the three chapters following this one. In a very through the looking glass way, this section could be characterised as a micro-autoethnographic account of the three stages by which I constructed this autoethnography (Wall, 2006). I begin with a brief explanation of how I subverted the conventional role of academic writing, followed by a description of the three different phases of writing that resulted in Chapters 5, 6, 7 and 8.

The traditional approach to social science writing is linear and static, stemming from the 17th century division between science and literature and designed to cohere “with mechanistic scientism and quantitative research” (Richardson, 1997, p. 87). Consequently, scientific (academic) writing is supposed to be transparent, a clear pane of glass that allows the reader to see straight through to the truth it reveals (Richardson, 1997; Richardson, 1990). The three chapters following this one do not contain writing that is intended to function in this way; instead, the
writing is my construction of truth and my truth is constructed through writing. These chapters represent the generation and codification of understanding, as such, the writing of which they are composed is not innocent or neutral and in some aspects has come close to Foucault’s ‘Parrhesia’ (1983), or truth telling for the purpose of critique (Robinson, 2014). The process was simultaneously analytical, generative of data and transformative, both of my understanding of myself as a researcher and how I could articulate a contribution to knowledge from this work.

**Writing Chapter 5**

Despite the lack of a blueprint, autoethnographers claim to draw upon techniques that autobiographers use to produce texts that are both evocative and aesthetic (Ellis, 2004; Ellis, 2013; Richardson, 2012). Mills writes that “story implies structure, and structure meaning” (2006, p. 1) and I found this very helpful as the first stage of my analytical writing. What I aimed to create in Chapter 5 was a complex and detailed full narrative of the project, upon which I could base subsequent analyses, corresponding to what Hatton and Smith (1995) have called descriptive writing. The emails, which I had placed in chronological order, formed the backbone of this generative writing process; these enabled me to construct the narrative and also to situate what we were doing in the project in time and space, which is recorded in italic text under each sub heading of Chapter 5. Context and detail were aided by reviewing the other project documents in line with the chronological narrative. But how can this process of writing an account be justified as an analytical one? If someone had been watching me write, it would have appeared as though I was just writing. And at times, I really just did write, in an almost trance-like state, similar to the ideas of free (Boice, 1990) or therapeutic (Richardson, 2013) writing. This, allied to the absence of protocol, brought its own questions about the validity of the method and the work: was this too comfortable to have epistemological value? A journal entry from March 2014 records how I felt worried “about whether the easiness of autoethnography and creativity means that it’s less ‘good’ than stuff I find hard”. I will return to this issue later in this section.
As I wrote the account, which has many characteristics of a thick description, several things happened: first, connections and ideas began to occur to me. For example, while I had concluded that something had changed in the PCT during the course of the project that transformed their attitude from a relatively managerial focus on milestones and specific, measureable outcomes to a more relaxed and collaborative desire to view the project as a foundation for long-term community development, I hadn’t connected this perception to a confluence of events in March that had both national and local significance (see section 5.13). Secondly, it seemed significant that my memories of some events were very clear, things like the atmosphere of a room, how people moved around the space, who spoke to whom while getting coffee. These memories evoked by the writing process enabled me to generate additional ‘data’ in the form of detail and subsequent reflection. And further it seems likely that the events I remembered were those that had significance for my ethnographic self, and thus those events seem to have shaped my analysis towards ethical and epistemological issues, as well as those of power and politics. The extract below provides an example of the extra information that these recollections created:

We, the audience, gathered in a large room in the town hall, a Georgian building with an impressive façade, I thought this showed the community researchers were being taken seriously, that the quality of the venue reflected the respect that they were being afforded. I thought that this would make them feel as though their views were significant; it didn’t occur to me at the time that it this might make them feel nervous or out of place, though it may well have done (Extract from Chapter 5).

The event described in this extract was a significant one in that it brought home to me how differently I was treated as an academic compared to ordinary people in Parkhood and Urbanwood. In this way, almost from the beginning of writing Chapter 5, I was simultaneously organising, analysing and generating new data. I knew that this process was as important as the words I was writing and wanted to capture it, to be able to explain the analytical writing as well as to write the story. At first, I elected to note what I regarded at that time as emerging themes in a draft version of Chapter 6. But very quickly it became apparent that this method of recording data about the writing process would expand into writing Chapters 5
and 6 simultaneously, which I felt would be unhelpful; primarily because I wanted to preserve the authenticity of the process I had elected to follow. And, because of the fluidity of the methodology, the lack of established protocol and the fact that I was my own data set (in a sense) it seemed important to impose some form of external structure, otherwise I felt the thesis would spiral into utter confusion. I also needed to rein in my tendency to dash off in pursuit of new and interesting ideas as the expense of important but less exciting tasks. So on 10th October 2013 I wrote my first entry in my research journal, a document saved on my computer that I have maintained throughout the reflective writing process and to which I refer in subsequent chapters.

**Writing Chapter 6**

Once I had completed Chapter 5, I intended to engage in a different sort of reflective writing, characterised by Hatton and Smith as dialogic reflection. This is a “‘stepping back’ from the events/actions leading to a different level of mulling over, discourse with self and exploring the experience, events, and actions using qualities of judgements and possible alternatives” (1995, p. 48). But rather than plunge headlong into Chapter 6, instead I decided to use the qualitative analysis software programme NVivo to engage in an intense process of open coding the narrative I had written for Chapter 5. I felt that this would be a useful way to take a mental step back, to halt the momentum of the very free writing that I had fallen into. I wanted to interrupt myself, to pause for reflection, make time to allow the ideas to settle and give new interpretations and structures a chance to emerge. In fact what I found myself doing was including the entire original dataset, emails and project documents and all, as part of a theoretically (or perhaps thematically) directed recoding process. I followed the processes outlined by Braun and Clarke (2006), a method that is often framed as realist (Aronson, 1994; Roulston, 2001), but characterised by Braun and Clarke themselves as equally congruent with the constructionist paradigm. At the start, there were four concepts that seemed important, all recorded in my journal: illegitimacy (how to make sense of self and behave ethically during such a process); social injustice (the feeling that current practices and discourses, through a focus on power and politics that is
inadequate, serve both to hide and reinforce inequality, to blame the victim); the boundary between activism and research (the idea that a researcher can be an advocate for a disadvantaged group and what that means); and the Medusa’s Gaze (absent referent and other linguistic devices to allow the powerful to avoid responsibility for the injustices they perpetrate).

From the coding process emerged four themes, similar to those I had started with, but more focused and detailed: the absent referent (which became negative space as I started to write about it), legitimacy, performance and resistance. My next step was to try and represent the themes graphically, with diagrams that began as maps with pencil lines crisscrossing the page in ever increasing complexity but ended as simplified depictions of my ideas and observations, as illustrated below:

![Figure 1: First sketch of the themes](image)

With these four themes and their representations as a starting point, I engaged once more in reflective writing but this time the themes cut across the chronology of the story. Originally, Chapter 6 was written as four relatively long narratives that served to generate and depict each theme. But this seemed to embody my
concerns about the ease with which the therapeutic writing flowed and whether
the output was justifiable analytically. Shouldn't the process be more painful
somehow, if it were to have any claim to being ‘research’? Yvette, my second
supervisor, agreed about the lack of analytical transparency:

“I found myself going along with your description, getting absorbed in the
autoethnographic account, but then couldn’t help a little knee-jerk response every
time you mentioned your data or your themes ... I had to remind myself that ‘the
data’ in your thesis are not just the vignettes that you describe in the previous
chapter but also all of your (old and new) reflections on these. Essentially your data
have emerged as the writing has evolved - new concepts and frameworks have been
introduced (e.g. absent referent etc.) which, in turn, have led to new reflections and
new ways of framing your data” (email from Yvette, 23 March 2014)

Following Yvette’s feedback, I decided to restructure Chapter 6 from the four
themes into a series of smaller reflective vignettes (Doloriert and Sambrook,
2011), each telling the story of how the knowledge therein was produced through
reflective writing (for a similar approach, see Ellis, 2013; Richardson, 2013). This
process was even closer to the therapeutic writing described by Ellis, Adams and
Bochner (2011) as writing that helps writers make sense of themselves and their
experiences and to question dominant perspectives (Bochner, 2001).

**Writing Chapter 7**

The next type of writing, which constitutes Chapter 7, is critical reflective writing
(Hatton and Smith, 1995). This writing seeks explicitly to reference the multiple
discourses, historical and socio-political perspectives that influenced my practice
and may be influenced by it in turn. This final phase of the analysis brings the
academic literature into the writing (or perhaps the writing into the academic
literature), both to integrate the critical reflection with the literature on social
marketing, participatory research and other relevant topics and to include new
directions as deemed relevant to the contribution being attempted. This chapter
offered a challenge to my desire to preserve the authenticity of the
autoethnographic narrative - the ‘logic in use’ - because it presented a more
sophisticated perspective on the social marketing literature than I had originally
written in Chapter 2. After some reflection, I elected not to rewrite Chapter 2
completely, as if I had always had such a refined critique on social marketing’s limits. But I did use the critique I developed in Chapter 7 to simplify and hone Chapter 2; in particular I synthesised what were disparate reviews of literature on the Total Market Approach, markets and systems theories, collaborative and community-led approaches and the S-D Logic into the three emergent ‘revolutionary’ approaches.

**Writing Chapter 8**

I decided to write Chapter 8 in March 2014, shortly after finishing Laurel Richardson’s book Fields of Play: Constructing an Academic Life (1997), in which she applies “the sociological imagination to the act of writing” and asks how “what we write affect[s] who we become” (p. 1). I felt then that I had read a reasonable amount of literature about writing as a way of knowing and had applied it myself, cautiously and mostly within conventional structures; now I wanted to unleash it, so see where and how far I could take the idea.

Inspired by Richardson’s conception of writing as “a site of moral responsibility” (p. 34) that will allow academics to use their skills and privileges to tell the stories of the silenced and “advance the case of the non-privileged” (p. 34) and feeling that the God of Convention had been placated by the humble offering of Chapter 7, ambitiously I decided to write a story based on what I conceived as Academic Science Fiction. I wanted to invent a parallel universe where things were different, where the most powerful discourse wasn’t Enlightenment rationalism but subjective, lived experience founded in compassion and respect. It felt idealistic, but also interestingly radical.

Quite quickly though, I abandoned this idea in favour of one less Tolkienesque in scale and aspiration, electing instead to write a short story that I called the Viva of the Oppressed. The title and basic idea for the format was inspired by Augusto Boal’s Theatre of the Oppressed (2000): I planned to write an alternative viva voce, in which I would defend this thesis to a panel of people whom I would invent. These characters of my own creation would ask me the questions I would probably find hardest to answer: what had I actually achieved with this work? How might it benefit anyone other than myself? I made some progress along this
path, fleshing out some characters and thinking of the sorts of things that they might say. But this idea wasn’t really working for me; the Theatre of the Oppressed is supposed to dramatise an oppressive situation, enabling the ‘spect-actors’ first to recognise and then, on the second run through of the drama to intervene and change the story, “taking possession of the stage in the fiction of the theatre… By transforming fiction, [they are] transformed into [themselves]” (2000, p. xxi).

Finally, I settled on an approach to which I had been introduced at a creative writing course on autobiography, led by author Sarah LeFanu. Sarah explained to us the difficulties she had faced in structuring her biography of Samora Machel (2012) and how, inspired by the work of her friend and fellow author Michèle Roberts (1993), she had decided to structure the work like a glossary or, in her words, a ‘lexical biography’. This format employs an alphabetic structure, with a single word representing something of significance to the author. I found it rather enlightening to write Chapter 8 using this technique for assigning structure; as well as satisfying my evident need to rebel against convention, it served to highlight and to juxtapose the many ideas emerging from the work of this PhD, constructing both significance and relationships between ideas that I might have missed, had I written a more conventional final chapter.

In conclusion then, the writing in this thesis is not regarded as an innocent medium that conveys ideas formed separately to the writing process. Instead, writing has been used as an active and generative part of the research method (Done et al., 2011). The original data (the emails, documents etc.) was transformed into knowledge in a three-staged process: first, by imposing a chronological narrative structure, then cross cut into thematic vignettes written to capture the story of the analysis as well as its findings and finally reintegrated with existing literature. The next section explains the relationship between writing and theory.
4.11. The role of theory

As part of a methodological elucidation, it is conventional to clarify how theory is going to be used in the process. In action research for example, scholars write that findings should help build, inform and test social theory (Reason and Bradbury, 2001), leading ultimately to wider social change (Somekh, 2006; Ozanne and Saatcioglu, 2008). In this brief section, I explore the ways that theory has been written about in the autoethnography literature before explaining how I used theory in this thesis.

As with the absence of defined analytical protocols described in the previous section, the role of theory in autoethnographic research is not made explicit; rather it is presented as a personal and subjective process of recognising and making meaning from personal epiphanies, “self-claimed phenomena in which one person may consider an experience transformative while another may not” (Ellis, Adams and Bochner, 2011, p. 2).

On the surface, it might seem helpful in attempting to clarify the role of theory in autoethnography to compare it to ethnography. Ellis et al. locate the difference between these two forms of study in the mechanism by which the researcher makes sense of a culture (Maso, 2001): autoethnographers make sense via their personal experiences; ethnographers do so primarily via other means. While some forms of ethnography are very explicit that their aim is to develop theory systematically (Strauss and Corbin, 1998), most ethnographic research is more concerned with description and explanation of particular phenomena (Hammersley and Atkinson, 2007). Hammersley and Atkinson go on to explain that rarely are ethnographic studies derived from existing theory (giving Festinger’s study of cognitive dissonance as one of the few examples of this approach); more often they are motivated by lack of or dissatisfaction with existing knowledge on a subject; by what they call “surprising facts” (p. 22) that the researcher is interested in exploring; or are opportunistic, such as studying a natural disaster. All of which are as likely to be politically or practically motivated as theoretically so.
The first way I used theory in this thesis was in conceiving this study, in a way that is probably closest to dissatisfaction with existing knowledge. I noted the variety of literatures in which participatory methods (or co-creation, co-production and other terms) appeared as theories of change. I noted also the way in which references to participation, collaboration and co-creation were starting to appear in the social marketing literature, often presented as the responsible or ethical choice, simply because they were participatory and collaborative. But of course, as Chapter 4 explains, other disciplines had considerably more experience of these methods and offered a much more nuanced and sophisticated perspective. Consequently, my intention at the start was to “sort out” these issues theoretically and to explore whether this sorting could help make sense of some of the difficulties I had identified in practice; two examples being firstly, the conflict between the managerial outlook of commissioners and the free flowing spiral of participatory approaches and secondly, the problem of whipping up enthusiasm among participants, only to abandon them later, once the project was over.

Quite quickly though, my attention moved from a desire to sort out the literatures on participation to the problem of methodology, as explained earlier. First, I assumed that my methodology was participatory action research, but of course it was not. This led to a period of exploration to determine what methodology could underpin authentically what I was aiming for within the constraints that existed. And thus, my work - and my ethnographic self, another theoretical influence - began to be informed by theory in a second way, with postmodernist theories of qualitative research, in particular the post-structuralism of Laurel Richardson, Norman Denzin and Carolyn Ellis. This affected significantly how I approached the study, how I wrote chapters 5, 6, 7 and 8 (and indeed how I am writing now, at this moment). This perspective sensitised me to process and narrative, evocation and emotion and encouraged me to experiment with voice and style in deliberate and experimental ways in order to create knowledge.

The third way in which I used theory in this study was during the analysis and sense making stages. Hammersley and Atkinson’s depiction of the way ethnographers should use theory in the analysis of their data is reflective of my approach; they say that theorising should involve an “iterative process in which
ideas are used to make sense of data, and data used to change our ideas” (p. 159). This iterative process came to the fore when writing Chapter 6, during which time I studied a range of theoretical perspectives from the theories of space in fine art to the language of eco-feminism. The backbone of my analytical work was formed by postmodernist theories of power and subjectivity, which striated every reflection, vignette and conclusion. If I were to have the luxury of repeating the study right from the beginning of the work in Parkhood and Urbanwood, I would probably choose to investigate particular aspects of my analysis in more depth, for example, post-structuralist ideas of performativity (Butler, 1999) as they might pertain to a performance of empowering behaviour by project leaders, or the Foucaultian notion of ethics and truth-speaking (Foucault, 1983) in relation to participatory projects in deprived areas. The next section takes a slightly different perspective on ethics, explaining how I dealt with the twin issues of ethics within the project itself and subsequently, the ethics of writing the autoethnography.

4.12. Ethical implications

As with all research projects conducted by staff or students, the university required me to seek ethical approval for the participatory project (for which I applied using the process for staff rather than for students). I later applied for ethical approval as a student to conduct the autoethnography; the faculty ethics committee approved both applications. This section draws upon earlier writing about ethics in this thesis to explain the ethical issues and how they were dealt with in the project itself and subsequent issues related to writing this autoethnographic reflection.

As outlined in section 3.9, ethical considerations in social research include the need for researchers to ensure that their participants have given informed consent, that any risks posed by the research have been considered carefully and it can be shown that any such risks are outweighed by the benefits the new knowledge will bring, that the protocols and practices outlined in the research design are fair, that anonymity and confidentiality will be maintained and that the
research will be accurate. Additionally, ethnographers are advised to be sensitive to power dynamics (Taylor, 2002) and to the possibility that they might change a person or community by dint of the reflections prompted by the questions they ask (Agar, 1982); though to effect change through research is rather the point, for participatory researchers. As well as different conceptions of the ethicality of seeking change, participatory researchers, as section 3.9 explains, face some distinct ethical challenges in their work, which relate primarily to consent and to anonymity. It has been argued that insisting upon anonymity as an ethical absolute can have the effect of “silencing participants for their own protection” (Bradley, 2007, p. 339); failing to credit them for their contribution and to give them a meaningful say in how their words are used (Blake, 2007; Cahill, Sultana and Pain, 2007; Elwood, 2007). The other issue relates to consent and whether it is ethical to expect people to consent to participate in a project when nobody knows how the work will unfold. In fact, there were some issues with consent in the project, but not of the nature explored in the literature to date, as will be explained in a moment.

On 11th May 2011, I submitted an ethics approval form for the project. This form outlined three possible ethical issues: First, that residents might be content with the way things are in their lives and become distressed or angry at perceived criticism or judgement from outsiders. I proposed ameliorating this issue firstly by framing carefully the way that the project was described to people so that rather than studying ‘problem behaviours’ we would be looking for ‘assets’ in the neighbourhood; and secondly, by working closely with trusted local stakeholders who could help us to avoid causing offense.

The second issue was the possibility that we may come into contact with people that have been harmed by alcohol in some way; perhaps alcoholism or alcohol related violence or abuse. In preparation for this, Anna was equipped with knowledge of a local organisation that supported individuals, families and communities with concerns about mental health, alcohol or drugs, to which we could refer anyone in need of support. During the course of the project, she did indeed encounter people who told her some very distressing stories and she did pass on the details of the support organisation several times. At this point, I made
her aware of our staff counselling service should she need to talk to someone in confidence about what she had heard (Adams and Moore, 2007). The third issue related to vulnerable adults. While the project was not about vulnerable adults, I was aware of the possibility that we may come into contact with them in the course of our work. I specified that should we encounter a vulnerable adult, we would work closely with their carer or the organisation that introduced them to us to ensure that they were not harmed by their participation and that any researcher that comes into contact with a vulnerable adult will be CRB checked. We only encountered one vulnerable adult in the project, during some data collection in Urbanwood. Because this man was unknown to us and not accompanied by another person, I elected not to proceed with an informal interview for ethical reasons.

The approval paperwork also required a description of the procedures for gaining informed consent and assuring confidentiality. I specified that people would be provided with an information sheet (see appendix 1) and further informed through face-to-face conversations with the researchers and co-design facilitators. If any doubts were expressed, I stated that researchers would not attempt to do any more than reassure and that if individuals were not keen to take part in either the co-creation or the research, then no attempt would be made to persuade them to do so. Further, I specified that any recorded data would be identified using first name only, and that the analysis would make use of fake names or identifiers such as Male A, Female B. I specified that audio recordings would be transcribed using an internal transcription service, that identifier codes rather than names would be used and that all audio files would be erased from the recording device following transcription. In fact, no participants agreed to audio recording and a great many were even unwilling to give their names or written consent to the research team. Consequently, we developed two levels of consent: written and verbal. With the verbal consent, the participants consented only to the information they provided being used as background and thus no direct quotations of their speech were included in any published reports. Much of the data from these anonymous participants is captured as observational field notes and recollections of the main points of a conversation, not specific words or
information. For this reason, there was no particular issue with ownership of words or ideas coming into conflict with principles of anonymity.

Finally, it is typical that research undertaken by the National Health Service (NHS) or on people who can be considered to have been selected on the basis that they are NHS patients or staff receives full NHS ethical approval. However, exceptions can be made if the research is deemed to be outside the scope of these guidelines, as was the case with this project.

My second application to the Faculty Ethics Committee was made on 10th June 2013 and covered the autoethnographic study described and created in this thesis. This was a very straightforward application covering the use of personal communications and collaboratively written documents as a basis for the analysis and the inclusion of direct quotations and extracts. I identified no risks to those individuals involved: Anna, Louise, Sarah and Phoebe. I explained that I would send each colleague a summary of the quotations I had used in context, and engage in discussion and negotiation with them until they were content that they were represented fairly. I suggested that the use of a pseudonym would help keep their identities confidential from all but those who knew the project well. So while Delamont’s (2007; 2009) concerns that my colleagues would be identifiable to someone who knows the project well are pertinent, I have ameliorated them by giving my colleagues the opportunity to review, change and remove information about themselves and their role. Indeed, some revision and removal of personal information has occurred, at their request. I have also used a pseudonym for the two neighbourhoods, for the same reason. All colleagues to whom I have referred by pseudonym in this thesis have given their full consent to be represented here, in this version.

This section has summarised the procedures that were put in place to meet the guidelines set down by my institution’s ethics committee, which included minimising possible distress, safeguarding vulnerable people, making sure informed consent was secured and data recorded so as to ensure the terms of that consent were honoured and confidential data kept safe. Throughout the next
three chapters though, there is a focus on ethical issues that are unrelated to these concerns, which will be highlighted in the text as they occur.

4.13. Concluding comments

I began this chapter with the reminder that the overall aim of my research is to shine a light upon the under-explored issues of language, discourses and power relations that may present themselves when a participatory social marketing intervention is commissioned in a deprived neighbourhood. In this context, I have explained and sought to justify the methodology I have used to study this question: autoethnography. I began by setting out my relativistic ontological position, which is easy to explain in contrast to ontological realism with its emphasis on an external reality independent of language but more difficult to disentangle from historical realism. The latter is accomplished by dissociating methodological concerns from the context of the research, bound up as they are with the issues of class-based analysis of primary concern to historical realists.

Next, I align this work and myself with the feminist post-structuralist perspective articulated by Laurel Richardson et al., founded in a rejection of epistemic absolutism and a belief in the power of language to define and contest social organisation and power, as well as to function as a place for struggle and exploration, in which the self is constructed. I have explained that in this work, knowledge and self are constructed through writing as a creative and generative way of studying the culture of the project through the lens of the self: these three elements forming the auto, the ethnos and the graphy of the study. However I note several criticisms of autoethnography, particularly the evocative autoethnography that I have sought to embrace here: most significantly that the method is narcissistic, uninteresting, unethical to publish and a waste of time and money. I respond to these criticisms by pointing out the unacknowledged subjectivity of these opinions and the unconscious privilege displayed by academics setting themselves up as the arbiters of what is unfamiliar, interesting and thereby worthy of study.
Following a brief elaboration of my position with respect to the study, I expand upon the issues of honesty and transparency within a summation of the judgement criteria that leading autoethnographers have proposed, which include: substantive contribution, aesthetic merit, reflexivity, impact and credibility. To these, I add my own suggestions of fairness and willingness to take risks. The concluding sections of this chapter represent a bridge between the more conventional literature reviews in Chapters 3 and 4 and the stately progress through ontology, epistemology, methodology and method that I have presented thus far. I become more autobiographical and reflective of how my engagement with autoethnography intersected with my reading about participatory research to create the next four chapters.
Chapter 5: In which I tell a story

“No, no! The adventures first, explanations take such a dreadful time” (The Gryphon, Alice’s Adventures in Wonderland, p. 85).

5.1. Introduction

The next two chapters chronicle the findings of the autoethnographic study and the generation and analysis of the research data. As I explained in the previous few sections, my aim when I started this chapter was to create a rich and detailed account of the project; I aspired to bring together all the data from the emails we exchanged, the field notes we wrote and the numerous planning documents we created with my recollection of these events, some of which seem significant because they can be called to mind in some detail without reference to any data, but also those that were dredged up during the writing process. The final element I wanted to bring into the narrative was my reflections, those retrospective understandings that come with distance, with musing actively on past events and their significance. In the straightforward storytelling of this chapter, my recollections and reflections are interwoven in a single narrative, an evocative thick description (Goodall, 2001) drawing upon various autobiographical techniques like showing and telling and varied authorial voice. It is presented (and was written) broadly in chronological order, with one or two overlaps and exceptions; they are identified as such in the narrative.

Before the story begins, I need to explain a few things. Probably the most important is that I do not intend to describe to the reader at the outset how this chapter is structured or how it ends. You will not know where you are or where you will end up as you read. You will not understand the entirety of the story until you have reached the end. This is unconventional in academic writing where signposting is integral to presentation, but this unconventionality is embraced deliberately in this chapter because it is consistent with how participatory methods work. I do not want to create a false certainty of logical progression
towards a known outcome. In an active, interactive example of the autobiographical technique of ‘showing’ (Adams, 2006), you will need to trust the process, trust that the narrative will make sense in the end, as the project team had to trust the participatory process of the research. This break with convention may be somewhat uncomfortable; this is my intention. I want you to experience some of the discomfort we felt, the discomfort of not knowing what the outcome will be. But I do not intend to cast you entirely adrift; I will conclude this introductory section with an explanation of the main characters in the story and the scenes in which it is enacted.

The story is set in two neighbourhoods, Parkhood and Urbanwood (not their real names), which are suburbs (technically wards, i.e. geographical subdivisions within a local authority) of a medium sized regional city. Both fall within the within the most deprived 5% of areas nationally (South West Observatory, 2011) and both contain large estates of social housing built between the 1950s and the 1970s (Lambert, 2014; McKee, 2013). According to the Office for National Statistics (2011) Parkhood has just over 10,000 residents, with levels of unemployment and people with a long-term health problem or disability both slightly above average for city, region and country. At 89% white ethnicity, it has less racial diversity than is average nationally, but is slightly more diverse than the regional average. Driving to Parkhood, one proceeds along a main road lined with large detached houses. Turning off this road, the scenery gradually gives way to the typical mid-century ‘council’ style of houses (Ravetz, 2013). Things begin to look run down, but always in the distance is greenery. Parkhood has several churches, a youth club, two Children’s Centres and a primary school, the eponymous park, a run of shops, a library (sadly in the process of closure during the project) and a committed amateur rugby club.

Urbanwood, closer to the city centre, is greyer and more compact; it is the smallest ward in the city with only 3,000 residents. There is a central green area with single shop and closed down youth café below a small block of flats on one side and closely packed small houses on the other. Urbanwood is split into two areas, separated by the Urbanwood Road: the more affluent Treepark area and the very deprived Avenues area. There is a well-used community centre and just
over the Urbanwood Road boundary in Treepark is a sports club. The percentage of people in Urbanwood with a health problem or disability that limits their daily activities (including problems related to old age) is double that of the city average and almost double the regional and national averages. Unemployment rates are also higher but the percentage of people who are carers is no different to city, regional and national averages (Office for National Statistics, 2011). The inhabitants are mostly white ethnicity, as in Parkhood.

5.2. Balancing act

_Parkhood, June – July 2011_

Right from the start of the project, despite what we were proposing seeming to be basic common sense and knowing now that the PCT was open to more collaborative approaches like this, the challenges associated with what we aimed to do became apparent. If we wanted funding for the project, it had to come from within the budgeting system, and commissioning structures organised around topics (like alcohol, smoking or obesity) were difficult to cut across. Or perhaps our timing was off: the appearance later on of initiatives like a significant Asset Based Community Development project (see section 5.13 and Robinson, 1995) indicates that generic funding linked to a particular neighbourhood rather than a topic was theoretically possible. Nevertheless, working within rather than outside the existing structure, we initiated contact with a budget holder who was keen to fund a participatory project on the topic of alcohol.

Developing the formal project proposal highlighted some inconsistencies between participatory principles and traditional social marketing management. As explained in Chapter 2, social marketing principles dictate that audiences are segmented into smaller identifiable groups and measurable goals for behaviour change are set. And true to the strictures of social marketing, we were tasked with reducing alcohol consumption among ‘increasing’ and ‘high-risk’ drinkers (as defined by their unit consumption), male and female, age 35-55. In reality, Louise and I agreed that a pragmatic approach to targeting made the most sense. Aside
from a cursory mention in project documents, presentations and the final report, the question of a target audience was not brought up again in a practical sense. What I took very seriously though was the stipulation that the project be participatory, defined in the original specification from Louise as

“...a way of working whereby service providers and users work together to create a decision or a service which works for them all. The approach is...built on the principle that those who are affected by a service are best placed to help design it” (Project Specification, 15th March 2011).

As explained in previous chapters, projects that employ a participatory methodology do not begin with the development of a detailed project plan; and here the process developed exactly as the literature said it would, as a cycle of planning, action and reflection (Kemmis and McTaggart, 2005) interspersed with problems, surprises and serendipitous moments. The first step of the first cycle was to organise a stakeholder workshop. The purpose of this workshop was to start to get to know the ‘gatekeepers’ to the two neighbourhoods: as outsiders, we needed people on our side, people who could warn us of potential transgressions, share their local knowledge and introduce us to others as part of the socially negotiated process of gaining access. Planning the workshop brought to the fore a number of issues relating to how the project should be presented and the extent to which we could (and should) attempt to control the degree of input that stakeholders, and later participants, would be allowed.

A particular conundrum was the degree to which stakeholders and participants would be able to deviate from the alcohol agenda laid down by the system. Whilst the funders were fully supportive of the participatory ideals upon which the project was founded (and now that I know them better, I believe that they were as committed to these ideals as they could reasonably be); they were concerned that if stakeholders, and later participants, were to be given completely free rein to decide upon priorities (as they should in a Participatory Action Research project) they could elect to focus upon issues like healthy eating or even topics unrelated to health like litter and dog mess.
The whole team was aware that senior figures, despite their realistic perspective on the challenges involved, might be concerned about what could appear to be a failure to demonstrate a measurable performance against risky drinking objectives. I understood and in principle agreed with this concern: publically funded organisations have a responsibility to use their funding wisely and certainly shouldn’t waste it on initiatives that are not successful. But I was concerned that an overly managerial focus on short-term ‘outcomes’ would prevent us from giving our vision the chance it needed to demonstrate what it might achieve and how.

Attempting to balance constraint with principle, we all agreed that some compromise would be necessary. Consequently, we all decided that some terms of the project would have to be set. These included the selection of Parkhood and Urbanwood as the target neighbourhoods, the focus on risky drinking, the final deadline for completion and the available budget (because of the terms of the funding) and the primary focus on older drinkers. The latter seems related to the problem of a specific ‘target audience’ identified in the previous section, but really it had more to do with an assumption that this group was suffering most in health terms (based on hospital admissions data) and also tended to be passed over in favour of other, hotter topics like under-age or street drinking. But regardless of the reasons, right from the start of the project, it can be seen that our approach began to deviate from a strict adherence to the principles of participatory research as well as from those of social marketing.

After some discussion about the best and most sensitive way to negotiate these tensions between the participatory ideals and the managerial system of set milestones and objectives, we decided to be honest with stakeholders (and later, participants) that these non-negotiable terms were a condition of the funding being made available and this was out of the project team’s control. Thus, right from the very start of the project, resisting this impulse to take back control became a battle to be fought with each other and within us.
5.3. The keepers of the gate

*Parkhood, August 2011*

As explained in the previous section, the first step of the project was to talk to local ‘stakeholders’ about the project. Stakeholder is a term with multiple meanings; stakeholder theory is used frequently in Organisation Studies for example (Freeman, 1984). In our project, we used the term stakeholder to mean a person who had a stake in the project or someone who could help us. Someone whose toes we might tread upon if we didn’t develop and maintain a good working relationship with them. In practice, this meant people working in the public sector with some kind of responsibility or interest in the neighbourhoods, for example the police community support officers and beat officers, people working for the housing associations, local council employees and people who managed community organisations like the youth club.

During our workshop, we asked stakeholders, numbering 10 professionals from the police, council, community organisations and two local charities who delivered support to people with alcohol and drug dependency, to identify and prioritise what they thought the goals of the project should be. Overwhelmingly, they felt that improving self-esteem and feelings of competence, allied to increasing involvement and engagement in the community, needed to be prioritised over outcomes related specifically to drinking. Consensus among the stakeholders at the workshop was that heavy drinkers in Urbanwood and Parkhood used alcohol to cope with their difficult lives and that before any change to drinking behaviour could be achieved, overall wellbeing needed to improve. It is interesting to reflect that the stakeholders’ views on what the project should be aiming for were broadly consistent with our original vision of a holistic, neighbourhood specific approach.

At the time of this first workshop, I was not aware of the degree of cynicism and disappointment in the neighbourhoods about the number of initiatives that had come and gone and while stakeholders were polite and professional, several appeared reserved. Perhaps this climate of resignation was the reason? I felt at
the time that if we were able to show that we could deliver on our promise of making a genuine attempt to understand fully the issues and give local people a voice then they would support us; but while they were willing to give us the benefit of the doubt, they would wait and see. One stakeholder in particular: Sarah, a local councillor, seemed sceptical about whether we realised the complexity of the problem we intended to address and the degree of empathy we would need to show. The following extract is taken from an early Stakeholder Engagement Plan, these notes relate to Sarah:

*Passionate about her community. Thinks we don't 'get' what it's like there. Willing to play along for now. Seems supportive. We'll need to prove we mean what we say. Could be embarrassing for her if she supports us publicly and we fail (text from internal document prepared in August 2011).*

Later, I became aware that Sarah had had personal experience of deprivation and had very little patience for people who thought that the issues were simple or who appeared to blame people for the circumstances in which they live without a full understanding of the context. Upon reflection, it seems obvious to me that from Sarah’s perspective, all four of the workshop organisers appeared to hail from privileged backgrounds with a minimum of personal experience of struggling to get by on a low income. Clearly, Sarah could see we were outsiders (Sixsmith, Boneham and Goldring, 2003).

At the stakeholder workshop, we were told about a community fun day that was being organised by an important stakeholder, the manager of the Parkhood youth club. She hadn't been able to attend the workshop itself because of other commitments, but had allowed us to hold the workshop in the youth club building free of charge and appeared at that time to be both supportive to the project and likely to be an important ally.

This section has described the first steps in gaining access to the two neighbourhoods. It has explained that the scepticism stakeholders appeared to feel about our project was probably founded in a combination of doubt about whether we realised the complexity of the task we had set ourselves and the fact that we were obviously outsiders from relatively privileged backgrounds. Looking
back, it seems clear that many stakeholders felt very protective of the people living in Parkhood and Urbanwood, tired of the implicit blame directed at them for their circumstances, wary of outsiders come to judge. The next section looks at our first tentative steps towards meeting the ‘real’ residents of Parkhood.

5.4. Assumptions and misconceptions

*Parkhood, August 2011*

The first piece of engagement with potential participants was at a community fun day at Parkhood Rugby Club on 27th August 2011. We viewed this event as an opportunity to explore response to the project, in particular whether people experienced a negative reaction to being asked about their drinking. Primarily though, these exploratory interactions were an opportunity to learn about Parkhood and start making contacts there.

The meeting to plan our approach to the fun day took place in a dilapidated and draughty building in Parkhood. Louise and a colleague, a representative from a local charity and I sat in chairs around a small table in the centre of a hall suitable for a much larger gathering. Phoebe hoped to join us from her office in London via Skype but there was no Internet connection in the hall, so she became a disembodied and slightly delayed voice from the speaker of my mobile phone.

We dealt swiftly with practical matters such as who would attend the fun day to represent the project and broadly what they would aim to achieve by doing so. Then, we had a lengthy discussion about whether we should be direct about the project’s focus on risky drinking or whether we should talk to people more generally about their community and their health.

The risk of opening conversations with people using the more general community and health approach was that people might not see drinking as an important topic; rather they might have other concerns. A project following participatory research principles more closely would accept and embrace this; encouraging people to set their own agenda would be an important goal. But we couldn’t allow this within the limits that existed and the pressure to show that the project had
achieved outcomes related to drinking weighed heavily on Louise’s mind. Because it was far too early in the process to know who might get involved, how they might work with us and what might happen, we all felt quite uncomfortable with the almost total openness of the project in these early stages.

During this discussion, it became apparent how easy it would be to manipulate people in the direction that suited us. We could begin conversations with people using the community and health angle, knowing in advance that we would attempt, probably with some success, to steer the conversation to drinking. Phoebe in particular was very uncomfortable with this possibility.

We settled on an approach that would explore both avenues. We would experiment with a conversational opener specifically about alcohol and one about the neighbourhood. The goal of this small and very unscientific experiment was to get a sense of what happened when people were approached in these different ways: were they offended by being asked about drinking? Did they refuse to discuss it with strangers seeking to impose themselves? With a more general opener, did the conversation include issues related to alcohol or did it go in a totally different direction?

While we didn’t follow a script, we referred to the following conversational openers:

*Opener 1: Hi, we’re working on behalf of the NHS as part of a project looking at the impact of alcohol.*

*Opener 2: Hello, we’re working on behalf of the NHS as part of a project to involve local people in improving the health and wellbeing of the community.*

*(Text from internal document prepared on 26th August 2011)*

Speaking generally about the community didn’t lead naturally to alcohol. People wanted to talk about issues like opportunities for young people and healthy eating on a budget, and once they’d had an idea they weren’t keen to divert.

Conversely, people didn’t seem to mind being asked about drinking, provided that specific details about consumption were not requested (open questions and vague categories like ‘medium, heavy’ were preferred). Several males appeared
proud of their heavy drinking, and wanted to joke about it, pointing out the “alcoholics” in the group. One male, noticeably overweight and looking uncomfortable in the heat, did wish to reduce his drinking but felt that this was something he needed to manage in private. Apart from this man, there didn’t seem to be much stigma attached to drinking; though the majority of the people to whom we spoke thought of themselves as ‘social drinkers’ yet several people had stories about others’ problem drinking. Drinking at home was seen as a particular issue, and once again several people knew of others who they felt drank too much.

There was a strong tendency to assume that we would be interested in youth and street drinking. We could not know whether this was because youth and street drinking were particularly evident locally or because of the media and policy focus upon these two issues had sensitised people to these concerns. However it was apparent that young people were a source of concern among the fun day attendees we spoke to: manifesting as concern for their wellbeing and their future prospects as well as regarding their behaviour as anti-social and threatening. The youth club seemed extremely well regarded.

Why did we assume people would be embarrassed about their drinking? Upon reflection, this conjecture on the part of the project team could have reflected a subconscious judgement that people who drank far too much alcohol ought to be embarrassed about it, to seek to hide it from others. Potentially, we were projecting onto the residents of Parkhood our assumption that we ourselves might find questions about our drinking habits intrusive or inappropriate.

This section described a small piece of exploratory fieldwork at a community fun day, with particular emphasis on our concerns about how to explain our project to people. Two main issues were identified: the first, once again, relating generally to some of the limits that precluded a fully participatory approach and specifically to the temptation to use a veneer of participation while knowing the destination all along. The second issue identified in this section is related to the ‘outsider’ status of the research team, exemplified in this case by our incorrect assumption that asking directly about alcohol would cause offense.
5.5. Lynchpin

*University, September - October 2011*

If the project were a work of fiction, Anna would probably have been the main character. Before coming to work as Research Associate on this project, Anna was a community fundraiser for a hospice charity. She had an MSc (with distinction) in Psychology and had undertaken professional qualifications in and fundraising and palliative care; the latter she had studied while working as a fundraiser for a palliative care charity so as to gain a better understanding of the complexity of caring for terminally ill people. She was about to start another Masters degree in Health Psychology. She wanted to help people.

To appoint a Research Associate for the project, I had to follow the university’s Human Resources procedures, which meant that I had a stack of CVs to score against the criteria set out in the role specification. Anna’s CV stood out, partly because she was eminently qualified for the post but mainly because of a story she told about how she had set up a support group following a chronic illness she had suffered.

I found her honesty engaging and her obvious talent for making things happen inspiring. We had shortlisted four candidates for interview; Anna was the last person we saw. Before she walked into the room we thought we’d already found our ideal candidate but Anna greeted the panel with a smile and during her interview made us laugh several times. She answered our questions thoughtfully and had clearly researched both the university and the topic. Afterwards, we agreed that she projected a warmth and wit that would be a great asset to the project. She was the riskier choice: she didn’t know anyone in Parkhood and Urbanwood, unlike two of the other candidates. She hadn’t worked on the topic of risky drinking before, unlike one of the others. But there was… something… about Anna. The interview panel, composed of Louise, Phoebe, an Associate Dean and I, felt that we didn’t want the opportunity of working with her to slip away. With the perspective of hindsight, Anna did contribute significantly to the success of the project (where success is defined on my terms as remaining true
to our ideals of honesty, respect and authenticity in our dealings with people and as creating – actually, legitimising – knowledge about risky drinking in deprived areas).

This section, which Anna has read and agreed may appear in its current form, has given brief biographical details of a key figure and explained how and why she was recruited to the post. Anna’s confident personality, her ability to connect with people and gain their trust and her commitment to this work were a significant factor in the project and her influence will be a reoccurring theme in subsequent sections.

5.6. Turf wars

**Parkhood and Urbanwood, October - December 2011**

In the early weeks of the project before Anna joined the team, I was made aware of another initiative happening in Parkhood and Urbanwood to recruit and train ‘community researchers’; local people who would receive six weeks of training in research methods and then conduct their own research on a topic they thought would be important. Their project had been agreed for a while, but was struggling to get off the ground.

Anna and I were given the contact details of the organisers, and resolved to try and collaborate closely. We hoped to become great friends, but it seemed that it was not to be. We heard on the grapevine that those involved with the community researcher project seemed worried that local people would find our project more exciting (because of the fun sounding ‘co-design’ compared to the arduous sounding ‘6 weeks of research training’). So, in an example of the issues that can result from several independent initiatives working in the same place at the same time, they proved cautious about collaboration.

A few weeks later, Anna emailed me to let me know of another helpful person she had met, who had posted some information about our project on a local website in the hope that interested parties might get in touch. Later that day,
Anna mentioned in an email that promoting our project in this way would be perceived…

“…like a declaration of war as [she] was originally interested in the other researcher project… But I think we have to go for it…”

I replied:

“…let’s just do the best job we can possibly do on our project, avoid passing any judgement on them publically and remain open to future collaborations.” (Direct quotations from email conversation on 6th December 2011).

This exchange seems relatively magnanimous given that it was a discussion between people whose offers of alliance had been brushed aside, but we were both sincere in our intention to treat our (in their eyes) rivals with consideration. Perhaps this stemmed from our genuine belief that the neighbourhoods would be better served by collaboration rather than competition between service providers. It may also have been because of the critical heritage of participatory research; the way it heightened our awareness of power structures and political factors in such a way that it enabled us to break some of the rules of the political game by refusing to play.

Another explanation for their wariness might be that the community researcher project leaders were seeking to minimise a perceived risk to their ability to achieve their objectives. If their project was being measured against how many volunteers they trained or how many completed their peer research project (which seems likely), perhaps we were seen as somewhat of a threat to their ability to achieve their goals? But why didn’t we worry that their presence would threaten our ability to achieve our goals? In our project, whilst we were ostensibly aiming to reduce risky drinking, I don’t believe that any of the project team actually thought that this was a realistic or achievable goal in the time available and with the funding we had. I certainly didn’t and was open about it, tactfully, using the metaphor of an oak tree to explain how short-term measures like people attending events or using tools should lead to medium-term changes like
increased self-esteem. A reduction in risky drinking was the oak tree; not something that can grow in 18 months.

Figure 4: Slide from working group meeting, 28th November 2011

So perhaps, rather than perceiving ‘failure’ as not reducing risky drinking, collectively we perceived it as not staying true to our original vision of working holistically at neighbourhood level, acknowledging the full extent of the significant and complex problem of health inequality and seeking to do something that would make an actual difference to the real problem (as we saw it), even if that difference was very small. So collaboration did not pose a risk to us in the same way it may have to the community researcher project leaders.

A few weeks later, in January 2012, relations with the community researcher project showed signs of thawing. Representatives spoke to Anna about the possibility of involving their volunteers in analysing the results of a baseline survey we had conducted, as a way of building up the links between the two initiatives. This didn’t happen, mainly due to the ethical implications of sharing our data. So, once again an opportunity to collaborate drifted away.
This section has recounted briefly our failed attempts to collaborate with a project to recruit and train community (peer) researchers that was operating in Parkhood and Urbanwood contemporaneously. I suggested several reasons why we might have been keen to collaborate where they were unwilling: first, that we had a superior understanding of the benefits of collaboration (there is no evidence to support this rather arrogant assumption); second, that the critical heritage of participatory research sensitised us to the power relations and political factors and thus made it easier for us to identify and dispense with them. The most likely explanation however is that the community researcher project leaders perceived our work as a threat to their ability to achieve the outcomes to which they’d agreed. Whereas a successful collaboration, regardless of the number of participants each project could claim as their own, would be counted as a success for us.

5.7. If you provide fish and chips, they will come

*Parkhood, December 2011*

On 12th December 2011, we held an Ideas Workshop at the youth club in Parkhood, our goal to start involving people in designing the ways in which we could begin to research the issue of risky drinking together. The manager was very enthusiastic about the workshop, offering to invite people herself and encourage them to attend (often this can be a significant challenge in a new project). She believed that women were the easiest route into the neighbourhood; if we could involve them, then others would follow.

The workshop was designed to address one question: how can people with no training in research methods be empowered to make a substantive contribution to designing a research project? We knew that if we simply asked open questions about research methods, the answers we got would be constrained by people’s existing knowledge, probably a variation on the theme of focus group or survey. After some discussion, we concluded that in the absence of offering to educate people in research methods, the best way to facilitate an informed contribution
was to encourage our participants to experience a range of different methods, both as researcher and researched. So, we developed a very simple typology of methods: talking, answering, choosing and creating and finally, we developed four ‘method stations’.

For this initial workshop, the method stations were set up using a mixture of the furniture available to us in the youth club and our own materials: a circle of chairs comprised the talking station, a table with pens, stickers, paper, glue and a range of printed materials the creating station. The answering station was three handwritten questions (why do people drink, why do they find it hard to stop and how does this affect those around them?) on large pieces of paper stuck to a wall. Finally, the choosing section was a simple game consisting of a questions die and some word cards for people to choose the word that best fitted their answer to the question. Blank word cards were provided for flexibility.

Figure 5: The ‘creating’ station in action, 12th December 2011

Unfortunately, the weather was blustery, wet and cold. The door, propped open welcomingly, kept breaking free of its restraints and banging loudly against the outside of the building, making us jump as we waited anxiously for the first
participant to arrive. But none of the women the manager had invited came, despite them all assuring her that they would be there to enjoy the ‘mocktails’ (alcohol free cocktails) and pampering treats that we had laid on to tempt them. Some had decided to stay at home because of the weather and others had gone to another event, the organisers of which in a flash of brilliance had offered free fish and chips.

Anna was clearly mortified; her first event, and nobody had turned up. Phoebe and I were disappointed, but sanguine. We had several similar projects under our belts, we knew that not everything goes to plan and that this setback was no reflection on Anna’s competence, nor did it mean our endeavour was doomed to failure. We reassured Anna and shared similar stories from other projects. Then, in a miniature version of the classic cycle of reflection, planning and action, we regrouped and decided to run the workshop with the manager, three youth workers, three males who agreed to help, two of the therapists who lived locally and a stray 15 year old girl who had heard about the project from the manager and wanted to be involved because of her struggles with an alcoholic mother. Our ethical approval didn’t allow us to work with anyone under 18, so we did not consider the girl a participant and recorded no data about her, but it seemed cruel to send her away. All the workshop attendees were able to experience all the stations in the 90 minutes available, though two young men left early because of other commitments.

Was this a failure? In a traditional project, the fact that we didn’t recruit our intended ‘sample’ of women aged roughly between 30 and 50 would represent a serious setback and ‘collecting data’ from anyone who happened to wander into the building would not have been an acceptable substitute.

However, casting aside these conventional criteria for judgement, I concluded that the workshop was a roaring success. The methods stations worked, our participants understood what we wanted and made comments about how suitable the methods may or may not be. We gained understanding from observing and interacting with them as they engaged with the activities. Our
original vision for the project was genuine collaboration with a neighbourhood, and this is what I felt we had begun to do.

However this workshop was the first time in the project where adherence to the participatory ethos had wrested control of events from me and upon reflection, it’s quite possible that if different people had come to the Ideas Workshop, the project could have developed quite differently from that point. Nevertheless, we learnt some worthwhile things about the methods themselves, which are discussed in the next section.

5.8. **Scissors and glue, questions and conversations**

*Parkhood, December 2011*

The previous section explained the planning process for the Ideas Workshop and our last minute change of direction from hosting a group of fourteen local women to running activities with a team of youth workers, some beauty therapists and a couple of passers-by. This section deals with the substance of the workshop: what methods could we use to investigate risky drinking in Parkhood and Urbanwood?

There was debate at the talking station about whether an independent researcher or a peer researcher would be most effective. Some male participants felt they would be able to be more honest with an independent researcher with no ties to the neighbourhood because there would be less temptation to show off (though Anna’s field notes consistently suggest otherwise!). The other advantage of a university researcher was that participants thought people would more easily accept being asked questions, as there was a ‘legitimate’ (my term) reason for doing so whereas a peer might be perceived as ‘snooping’ (a participant’s term). Others felt that a peer researcher would actually be more equipped than an academic to challenge this ‘pub talk’ style of exaggeration that participants felt would be ubiquitous. While most of the discussion was centred on the best way of getting to the ‘truth’ (i.e. seeing beyond exaggeration), there was a brief
exploration of the relative power and status that might exist between researcher and researched:

*Needs to be someone in the community on equal terms. Not the vicar or the councillor. Someone like you (reported speech summarised in facilitator notes).*

We were saddened (and somewhat daunted) to hear that there had been three alcohol related deaths in the area recently.

“If that won’t stop ‘em then what will?” (Anonymous quote from participant recorded in facilitator notes).

The choosing station worked quite well as a way to gain people’s interest and break the ice, but didn’t elicit a great deal of useful information. Participants also warned us that many people would be put off by the idea of having to read or write in public. This was yet another example of our status as outsiders: it simply hadn’t occurred to any of us that adults might struggle with basic literacy, though on reflection I should have considered it. That small insight represented a significant personal epiphany for me at the time: I believe it was at that point I was awakened to just how substantial the barriers faced by people in Parkhood and Urbanwood could be.

The answers station was probably the simplest of the stations and was well received. However this method also faced the barrier of literacy as well as being rather a public way to elicit what could be extremely personal and even distressing information. Participants mulled over the idea of simply leaving the questions around the neighbourhood along with the means to answer them so people could do so in relative privacy. Maybe even amalgamate this with the creativity idea and invite graffiti in the name of research? But that idea was rejected swiftly for a very practical reason: our materials would quickly be vandalised or stolen.

The creating station worked very well, with participants seeming to enjoy making their pictures once they had overcome their initial stage fright. I found that the most understanding came from asking participants about their visuals as they
worked. The youth club manager in particular seemed to think that this would be a good method to elicit rich data about drinking.

Figure 6: Collage photographed with permission on creating station, 12th December 2011

She thought that taking the station around existing groups would be the best way to engage people. In fact, drawing upon visual methods (Banks, 2001) had several exciting possibilities: the manager and I had an animated discussion at the workshop about working with local artists to create works and hold a community exhibition, which in itself would also be a useful way to increase our understanding of the issue of risky drinking as we spoke to people about the works, what they mean and whether their experiences are similar or different. I also noted in a debrief written for Louise that one of the stakeholders that had attended the Stakeholder Workshop (described in section 5.3) was an expert in visual therapy. An exhibition of creative works had another advantage as well: it would function as dissemination of the research within the neighbourhoods. This seemed like a brilliant idea at the time and upon reflection it still does. But it was just too brave; too risky. Not close enough to the sort of intervention that might reduce risky drinking. It had captured my imagination though, and I sought to
return to the idea several times as the project unfolded. I will explore this and several other good ideas that floundered in the theme of ‘negative space’ in the next chapter.

One unexpected piece of feedback from the workshop harked back to our earlier conundrum about the best way to introduce the project’s focus. We had worried about people’s reaction to the topic of alcohol but hadn’t thought to be concerned about people’s reaction to the selection of Parkhood and Urbanwood as the locations. However, it is recorded in the field notes that the male participants objected to the rationale that Parkhood and Urbanwood were high in alcohol-related hospital emissions. This made them feel as though we were criticising their neighbourhood, saying that it was:

...crap and that community was part of it (reported speech summarised in facilitator notes).

This was an issue that hadn’t occurred to the team at all, that locals were undoubtedly aware of their reputation as ‘problem’ neighbourhoods and resented outsiders highlighting this. This feedback from participants occurs in subsequent sections as well.

This section has summarised conclusions from the Ideas Workshop, a workshop designed to empower attendees to participate in designing the research methods for the project by encouraging them to experience being a researcher and participant in different types of data collection. The two methods preferred by participants and researchers were the talking and creating stations, from which we had learnt that people might well boast about their drinking, but had no definite conclusion about the best way to mitigate this. We learnt that literacy could be a significant barrier, but that people enjoyed the opportunity to be creative. And we were alerted to the issue that people may feel sensitive about their neighbourhoods having a negative label applied by outsiders, so we would need to explore this issue carefully as the research developed. But despite being pleased with our progress, we agreed that more input from a wider range of participants was needed before it was appropriate to launch the data collection phase of the project. Given the challenges of persuading people to turn up to a
workshop style event, we decided to take the methods stations out into community settings. We called this phase the Methods Roadshow, which is reported in the next section.

5.9. The show on the road

**Urbanwood and Parkhood, November 2011 – January 2012**

Throughout November and December 2011, while we had been planning the Ideas Workshop, Anna had spent time in Parkhood and Urbanwood visiting first the stakeholders we had met at the workshop back in August and then making contact with others suggested by these original contacts. In December, Anna was invited to watch the first Urbanwood pantomime, ‘Snow White and the Seven Antisocial Dwarves’, which was co-written by the young cast. Sarah took Anna for a walk around the streets, to the Sensory Garden, described in Anna’s notes as ‘a fenced-off collection of benches’, and other areas where young and adult drinkers gather. Sarah described to Anna how some adult drinkers choose to drink on the street because they are unhappy at home. Anna’s notes from these early explorations give a flavour of the sorts of information that would typify our later findings:

...one man said he would be much happier staying at home and cut back his street drinking if his living room was made habitable and redecorated (reported speech summarised in field notes, December 2011).

Consequently, in January Anna’s name was becoming known among local stakeholders and there were a number of friendly venues to which she could take the Methods Roadshow in Parkhood. Throughout that month, she visited a local church group, a toddler group, an art group, a voluntary organisation providing financial and employment advice and a tenants’ surgery run by one of the housing associations. This section examines the Methods Roadshow and explains how it evolved into the data collection phase of the project.

The field notes from early Roadshow sessions reported similar findings to the Ideas Workshop: the talking and creating stations were preferred by participants,
the creating activities in particular offered an opportunity for the researcher to refer to the emerging visuals during the concurrent conversation. Anna also observed that giving participants something to focus on might have made it easier, less awkward, for participants to speak about difficult topics. Once again, anything that required participants to read or write proved unpopular and ineffective:

_I got the feeling that writing anything was a chore for some of them, as the only time they had to write was filling out forms for benefits / housing etc. – one person said they didn’t ‘do’ writing (reported speech summarised in field notes, 11th January 2012)._  

One participant at an ‘employment surgery’ visited by Anna on 11th January suggested we should offer a drop in ‘surgery’ about risky drinking; the following day, at a ‘tenants’ surgery’ (evidently a popular model for delivering services) the community engagement officer for the local housing association made the same recommendation. However, Anna records in her notes her concern that not only did she lack the necessary skills and experience to counsel people concerned about drinking, adopting such as model would place us in direct competition with an established alcohol treatment centre. This was the first of several other indications that while help was available; something was going awry in the delivery that meant perhaps that local people weren’t aware of its existence, didn’t think that the service was suitable for their needs; or perhaps they didn’t trust the providers.

It was also becoming apparent that while our intention to tackle risky drinking (the sort of drinking that is bad for one’s physical health but wouldn’t be classed as ‘dependent drinking’) and risky drinking only, separating this behaviour from other sorts of drinking was drawing a distinction that really only existed in health policy documents. So while the notion of segmentation was working in terms of targeting particular neighbourhoods, it was inappropriate both in terms of the age of the ‘target group’ and the sort of behaviour that we were aiming to change. Real life, when we attempted to study it closely, was simply too complicated to fit neatly into a predetermined model. The participatory mode of managing the project exacerbated this disconnection, because participants didn’t want their
experiences constrained and compartmentalised into a predetermined model. They just wanted things to improve and it really didn't matter to them which organisation was responsible for what particular facet of the problem. I described earlier in this chapter that I resisted passively the constraint of the age limits, primarily by ignoring them because they didn't fit with the participatory ethos of the project. However, ethically we couldn't treat variances in drinking behaviour in the same way; ignoring limits upon participation without acknowledging the seriousness of some problems would have been a serious breach of trust on our part. Consequently, Anna sought advice from the alcohol treatment centre and obtained some leaflets, so as to enable people to make contact if they appeared in need of practical help for issues related to drinking.

After 11th and 12th January, while we were still ‘officially’ in the Roadshow phase of the project, the field notes themselves make no further reference to methods. This was because Anna was finding exploring the methods themselves a distraction; rather, simply talking to people was most comfortable for everyone and was most effective at eliciting information. It seemed to be the informality of the situation that worked for participants, they were put off by any suggestion of formal interviews, recording, reading or writing. Participants felt concerned that they would lose control of their stories (a word used frequently by Anna in her notes of reported speech); as though a story was something tangible that could be kept safe only if it were kept secret. Trusting Anna with a story was a significant thing to do and something that would only happen if it were informal and personal between Anna and the storyteller. I suspect that whatever quality Anna possessed that made us so certain she was right for the project also encouraged people to trust her with their stories.

Anna felt strongly that continuing with a formal Roadshow format was getting in the way of making these personal connections, connections that were too valuable to cast aside in favour of strict formality. But informality created an ethical dilemma: if people were unwilling to read, to sign anything or to be recorded, how could we demonstrate that informed consent was provided?
As explained briefly in Chapter 4, we settled upon varying levels of consent: A few participants were willing to sign a consent form and talk to us in the understanding that their stories would be kept confidential. We would know who they were and what they had said, but we wouldn’t allow others to identify them in any materials we produced. A second level of consent we named ‘background’: at this level, participants gave their consent to Anna verbally, but were kept totally anonymous. Only the gender and approximate age of these participants are recorded in the field notes, along with the date and location where the conversation took place. Most field notes therefore are in the form of reported speech, with few direct quotations.

This section has explained how the Methods Roadshow, designed to gather a wider range of input about the most appropriate data collection method for the project, evolved swiftly into a method founded in informal conversations. The very informality of which, allied to Anna’s warm and empathetic personality, served to gain participants trust and encourage them to share their stories. The next section, while remaining true to the chronology of my autoethnography, breaks with the narrative thread of the project to introduce a vignette of our involvement in the concluding stages of the community researcher project.

5.10. No, you’re wrong about your life!

City Centre, February 2012

In section 5.6, I wrote about mine and Anna’s failed attempts to collaborate with a community researcher project that seemed to have a similar ethos to our own, concentrating particularly on exploring the reasons why we were unsuccessful. Our final experience of the community researcher initiative was to attend the presentation of their findings on 15th February 2012; this section presents an account based upon my memories of the event.

We, the audience, gathered in a textbook ‘invited space’ (see section 3.11): a large room in the town hall, a Georgian building with an impressive façade. I thought this showed the community researchers were being taken seriously, that
the quality of the venue reflected the respect that they were being afforded. I thought that this would make them feel as though their views were significant; it didn’t occur to me at the time that it this might make them feel nervous or out of place, though it may well have done. Or, they might just have been apprehensive at the idea of speaking in public… We sat at small tables, 3 or 4 people at each. Many people in the audience seemed to know each other and many now knew Anna through our project. We sat at a table with an older man in a prominent public service role. Four community researchers stood at the front, three females and one male, all young, in their late teens or early twenties. They had prepared some large pieces of paper with handwritten bullet points to hold up while they spoke. They told us what they had investigated, why and how. They told us what they had found. The specifics aren’t really relevant here, but the reactions they garnered were quite shocking at the time and more so on reflection. Some vocal members of the audience seemed to view the findings as a personal affront to their work, in particular they disagreed with the overall conclusion that people in Urbanwood (where the presenters and their respondents lived) felt that they didn’t have access to many facilities or activities. This observation is of some significance and will be brought up again in later sections.

One audience member stood and told the community researchers angrily that what they had said wasn’t true, as she personally distributed a leaflet detailing many activities happening in Urbanwood. Others questioned their interpretation of the findings: the man at our table stood to inform the researchers that Urbanwood didn’t need a gym as they (and many of their respondents) had suggested but in fact needed a boxing club to provide young men with discipline and an outlet for aggression.

Why did the audience appear to feel so threatened by what the researchers told them, so keen to dismiss it and impose their own interpretation instead? Why didn’t the audience accept the findings in the way that they would probably have been more likely to do if I, with my university researcher stamp of authenticity, had presented them? Was it to do with confidence? Vocabulary? Social status? Once again, this observation about ‘legitimacy’, about who is entitled to present their truth, is a significant one that will receive more attention in later sections.
The community researcher project was participatory in the sense that researchers had autonomy to choose to investigate an issue that they felt was important. Consequently, should they not have been encouraged to present their findings in a way that gave them the power, not the audience? Instead, they were taken out of their world and placed into ours. Was this an attempt to show respect that actually showed a lack of empathy, or was it a subconscious reminder to the community researchers that they were just young people from a deprived neighbourhood who didn’t get very far in education? I think it might have been better to take the audience out of their comfort zone: to have taken them for a walk in Urbanwood, shown them the boarded up community venue, the locked building where the Internet café sponsored by a local service provider had once existed, while curious passers-by stared at them. Asked them to sit in the community centre, surrounded by a high security fence. This is the approach I would take now, should a similar opportunity arise in the future.

After this, the community researcher project disappeared from our radar, as far as I know it fizzled out completely, except that two of the community researchers became closely involved with our work in Parkhood. The next section describes their and others’ involvement as participants, including a detailed summary of the main themes of the research phase itself.

5.11. The solution is... solve poverty

Parkhood, February – March 2012

In February, as the data collection phase was about to move into full swing, it was evident that Louise was still uncomfortable with how open the project still was. The pressure for details about what was going to happen and when was increasing steadily. It was at about this point when the project started to feel like a battle to be fought by me alone, a battle to keep everyone else calm. Or perhaps an illusion of calm to maintain for both sides: Anna needed space to explore the issues and reassurance that she was doing the right thing even though there were no outcomes on the horizon. For Louise, I needed to appear in
control with definite plans in place. While Louise knew that in a participatory project all dates and milestones were provisional, I felt the need to maintain a performance of control with provisional dates and status reports. One such event was the commencement of the co-design phase in March 2012, even though this created an artificial deadline we had to rush to meet. Fortunately, by early March, Anna had reached the point in Parkhood where she felt she was reaching saturation. She was also aware of the pressure to make progress in the project and was keen to start collecting data in Urbanwood, which she knew would be more challenging.

Because we had only a short time to prepare for the workshop, our analysis of the Parkhood data in March 2012 was shallower and less reflective than I wanted. I have since performed a much more extensive analysis of the entire collection of field notes and was pleased to find that while some subtleties of interpretation were glossed over in our haste to prepare for the workshop, the broad themes we developed broadly held true. However in the interests of preserving this narrative’s authenticity and also in recording the interpretations we shared with participants at the time of the workshop, this section reports our interpretations as they were in March 2012, with some very basic commentary indicating that richer interpretations can be made.

Because there were significant challenges associated with engaging participants beyond ad-hoc – and largely anonymous – informal conversation, Anna and I undertook the analysis of the Parkhood data ourselves. Field notes relating to conversations with twenty three males and forty one females were included at this stage. Of these only fourteen had given written consent to be quoted. First, we performed a thematic analysis (Braun and Clarke, 2006) on the field notes, starting by reviewing the data independently to generate initial codes. Next, we compared their interpretations of the data and codes to create initial themes. Each theme was reviewed, discussed, edited and given a single word to describe it. Then, the data and original codes were reviewed against the named themes to check consistency with the overall story of the data. The themes were: Family, Trapped, Worry, Apathy (which workshop participants later changed to Powerless), Alone, Ashamed and Confused. Themes integrated a range of
factors: from significant structural barriers like housing regulations to individual obstacles like mental health, all of which were woven through participants’ stories. The following paragraphs are based upon the notes I prepared to talk through the themes at the workshop so as to provide an accurate report of our interpretations at that time, integrated with illustrative extracts from field notes (which were not used in the workshop itself due to the risk that the details would identify individuals to their friends and neighbours).

One of the largest and most complex themes related to family. Family can be a trigger: for example experiences in childhood or losing a job can turn moderate drinking into risky. It can be part of the consequences of risky drinking: a family has to pick up pieces with little formal support, drinking can lead to domestic violence (which leads to more drinking) and more family problems.

“... And you want to keep it to yourself, it’s shaming especially when you get bruises – but that’s why I left in the end... And I think it’s the wives and girlfriends who need the help just as much, otherwise they can start having a drink just to help them cope with it all. It’s the women who are keeping it all together.” (Female, 40s)

But family can also be helpful: it’s harder to hide problems from people who live in close proximity, family can make it easier to cope without alcohol and not wanting children to copy unhealthy behaviour can encourage change.

Participants felt trapped by their responsibilities, which often included minding young children and to keeping things going when times are tough. They felt trapped because they could not move away, could not escape anti-social behaviour or bullying, could not offend neighbours because they’re stuck with them.

...[she] knew they had spread stories about her around the area – that she was selling drugs and everything else (she used hand gestures to indicate that they thought she was a prostitute)... One of her neighbours also frightened her – he had shot at her with an air rifle, put a samurai sword through her front door... (Reported speech and anonymous quote from female participant, field notes).

Participants also reported feeling trapped by peer pressure: on men to be “manly” and drink and women to do what their friends are doing. They felt trapped by
circumstances, struggling to get by on low income and cope with debt, unemployment and benefit problems. Mums felt they had nowhere to go; men only have activities that revolve around drinking.

People also felt powerless; if they were not working (unemployed or caring for children) they felt they have nothing to do.

“You get bored and frustrated... I was drinking 2 bottles of wine a night by the end. I’d pay my bills and my rent first, because otherwise you’d have a shitty house or be out on the streets. But that meant I couldn’t afford the drink, so I started stealing it. I’d go in with the pushchair and use that, I’d hide the bottles in that” (Female, mid-20s).

People explained that they felt that there was nothing to look forward to; they felt low, de-motivated and depressed. They got up late and started drinking at lunchtime, there was no reason to bother trying to change; they think there are bigger problems than health. A few described serious mental health issues.

She is hearing voices, and this is frightening her. She also has suicidal thoughts...the father of the youngest [redacted] is getting out of jail in May and she is terrified because she says he won’t leave her alone... She is suffering from anxiety and panic attacks about this...This all affects her drinking and drug use (marijuana). I gave this woman information about the mental health and drug/alcohol support service. (Reported speech from female participant in her 20s, field notes).

Others simply can’t get out of their rut; they have a routine where they use alcohol to relax or as a treat at the end of a busy day. This is OK until something else happens (job loss, unexpected pregnancy) that tips them into drinking more at home alone.

Participants also described feelings of physical isolation: their friends are at work; the traditional pub with games and caring landlord has gone.

“I’ve worked on the door and I’ve also been out getting drunk so I’ve seen both sides. The real problem now is the big companies owning all the local pubs. The managers just take your money – they don’t care about you” (Male, early-40s).

Participants worried about the practical consequences of seeking help; losing their children, benefits or home. They also worried about the social
consequences of seeking help: that their friends would be hostile or they would face humiliation when others heard about their problem. Emotional and social isolation were also common themes, as were feelings of shame, which once again seemed to be experienced differently by male and female participants: Men felt ashamed about admitting weakness, letting down their mates and family, losing face and not being able to hold their drink. Women felt ashamed about others judging them as bad mothers.

“You can’t be a good mum and get drunk every week, but you can still be a good dad, what does that say?” They thought this attitude not only made it more likely for mums to drink at home but also less likely to seek help if they thought they needed it. (Reported conversation with two female participants in their 30s, field notes).

A relatively insignificant theme in the overall context of a complex and distressing set of stories was the insight that several participants felt confused about government guidelines on alcohol units and about where they were supposed to go for help. A theme that appeared during the March 2012 analysis to be a facet of confusion was differing interpretations of what constituted ‘normality’ in relation to drinking and behaviour related to alcohol. However upon re-analysing the field notes, it became apparent that I had grievously underestimated both the size and the complexity of this issue.

For each theme, a graphic illustration and brief description was created, which were used to reflect our interpretations back to people in the neighbourhood in a co-design workshop, which is described in the next section.

5.12. Getting to the “Hub” of the issue

Parkhood, March 2012

Cook (2011), a practicing social designer in the UK, recommends Bradwell and Marr’s operational definition of co-design (2008). They characterise the technique as a collaborative, developmental process that involves a great deal of transparency. Co-design locates power in the process rather than with the funder, client or designer, which is thought to generate a sense of collective
ownership between all parties involved. On 21st March 2012, we ran a co-design workshop following these principles, held in the local Baptist Church hall. Five residents attended for the whole workshop, two from local churches along with stakeholders from the local provider of alcohol treatment, children’s centre and the housing association.

After we had welcomed everyone and thanked them for coming, we pointed to several visuals on display around the room, including a handwritten agenda and co-design rules, deliberately simple and informal, and the images that an illustrator had created for us to represent the themes.

![Figure 7: Visuals ('Apathy' to 'Powerless'), 21st March 2012](image)

A map of Parkhood also adorned the room, upon which we had stuck bright orange star shaped post-it notes to represent various Assets we had found. The asset map was there partly to demonstrate that we had made the effort to find out about Parkhood (to help mitigate our outsider status) and also to encourage people to think of using and improving existing assets, not just about creating new ones.
Anna and I talked the workshop attendees through our findings using the themes and four case study ‘stories’ that Anna had written based upon an amalgamation of the most typical experiences (see appendix 2). We had had some discussion about these stories while planning the workshop, attempting to strike the right balance between anonymising the stories enough so as to keep our promises of confidentiality while avoiding appearing to have ‘made them up’ (Anna’s term); the latter she felt would reduce our credibility with participants, who had quite firm ideas about what research was supposed to be. I have to assume we succeeded, as there were no issues with the case study stories at that workshop, or indeed anywhere we used them subsequently.

Following our feedback to the group and the concurrent reflection upon our interpretations, we asked the participants to form four groups, each was provided with copies of the case study stories. This time we had paid attention to the possibility of barriers due to literacy: to avoid causing embarrassment to anyone, we organised each group so that it contained a mixture of stakeholders and local people. Facilitators (Anna, Phoebe and I) offered to read the case study out loud to each group as we distributed them, ostensibly so that each member had an equal chance to listen and make notes. This worked well, ameliorating any awkwardness that might have been created if we had expected everyone to read the story individually.

We asked each group to work with one of the case studies to consider what might have led to that person starting to struggle with drink and what might contribute to them continuing to drink in a harmful way.

Next, we asked the group to generate at least 10 ideas that could have stopped the person in the story on their path towards struggling with drink and at least 10 ideas that could support that person to recover now. Ideas could be written or drawn on “idea sheets” we had provided. We emphasised that the ideas didn’t have to be brand new, they could be ways to make existing services or support more effective.
More than 40 ideas emerged; some quite specific to a particular age and life-stage (e.g. extreme sports could help divert groups of young men from organising all their social activity around alcohol) but with general principles that apply to all (e.g. provide activities that don't revolve around drinking).

To end the workshop, two groups were asked to visualise the idea that they felt was the most likely to make a difference. Independently, both groups created a ‘Community Hub’, located in the centre of Parkhood (to remove the territorial issues of people from one end of the neighbourhood feeling unwelcome at the other end).
The Hub would host a range of services for all ages as well as being a venue for ‘positive’ (i.e. not stigmatising) reasons to visit such as a café, evening social club and venue for short courses. Having somewhere ‘positive’ to go with activities for all ages as well as somewhere to socialise would reduce isolation (locals felt there were no social spaces for adults, not even a café) and help mitigate the stigma associated with accessing help.

Participants thought that this would reduce drinking and the harm associated with it in several ways: by making services easier and less embarrassing to access and also by giving people reasons to leave their homes (where they drink because they are isolated, lonely and bored) by offering things to do that do not revolve around drinking.
5.13. Beware the ides of March

_Everywhere, March 2012_

As well as starting to make some progress understanding the neighbourhoods and making the first tentative steps towards co-design as described in the previous two sections, a confluence of events in March constituted a defining moment in the project’s narrative. It was around this time that things started to gain momentum: Anna was about to switch the bulk of her attention from Parkhood to Urbanwood for data collection, whereas Parkhood was moving into the co-design and implementation phase. Concurrently, Sarah heard that she had been successful in her application for a significant grant to be used for community development in Urbanwood, and she was keen to involve us. March also saw the initiation of a multi-agency collaboration, which intended to take an Asset Based approach in partnership with a local third sector organisation to tackle deprivation in the city, primarily in Parkhood and Urbanwood. Sarah was part of this nascent initiative and keen for our endeavours to compliment rather than compete. She was a leading figure in all these initiatives, and I was glad of the relationship of mutual respect that was growing between her and Anna.

Throughout all these developments, it appeared as though we were moving away from a definite status as outsiders, not really becoming part of the family, but perhaps closer to _consigliere_, trusted friend and confidant to influential stakeholders in Parkhood and Urbanwood. This change in our status was largely due to Anna’s evident commitment to and regular presence in the neighbourhoods, her friendship with Sarah and her warm and empathetic personality. I was a more formal and distant figure, the one who spoke at meetings with other formal and distant figures, controlled the process and the purse strings. This arrangement wasn’t engineered; rather it evolved due to our different personalities and roles. Instinctively, it seems, we played to our strengths.

March, then, represents several shifts: in our status in Parkhood and Urbanwood and more widely towards significant investment in more holistic, neighbourhood
based approaches, sympathetic in fact to our original vision for this project but significantly better funded. Nationally, things were transforming as well: on 27 March 2012, the Health and Social Care Bill gained Royal Assent, becoming the Health and Social Care Act (Department of Health, 2012). This dissolved the PCT, the entity that had commissioned the project. Public health professionals would work instead for a national body, Public Health England, or for local authorities (county councils).

The consequence of the Health and Social Care Bill passing in the middle of our project meant in practice that suddenly we had more freedom: not only had the intense pressure to deliver very specific outcomes related to health been relaxed, we were now serving as somewhat of a trailblazer for a new approach, working as we were with other local authority functions like community policing, youth services, housing and employment organisations. The ides of March were not the time of betrayal by our friends; they were the time we were liberated to work more closely with them. The ways in which this changing context affected the project will be highlighted throughout subsequent sections.

5.14. A missed opportunity for Parkhood?

Parkhood, March – July 2012

This section resumes the chronological narrative of the chapter, picking up the story from the day of the Parkhood co-design workshop on 21st March 2012. After the workshop, we went to see Louise and a colleague, who were delighted with the themes, the visuals and the Parkhood Hub idea. This may have been the first real evidence they saw that Anna and I had been doing any work at all.

However, I was keen to convey that our experience and research to date in Parkhood suggested that if the Hub was simply created, without work to engage residents in the process of designing it, it risked becoming an expensive provision of facilities and services that the community simply didn’t use. In order to fulfil the vision the Hub would have to be developed collaboratively, with Parkhood residents; it would have to be their hub, not ours. After our meeting
with Louise I rang Phoebe, who explained about the concept of prototyping, a technique designers use to develop ideas. Prototyping, according to the Design Council (2014), involves building models of an idea, which can be used to test form and function as well as communicate design ideas to different groups. Phoebe sent me a document she had prepared for another client detailing a ‘pop-up children’s centre’ that explained how such a concept could be ‘quick-and-dirty’ (so people aren’t afraid to criticise it) prototyped, evaluated and prototyped again in a cycle of development where each prototype is more sophisticated and closer to a final product. Prototyping was the way Phoebe would recommend developing the Hub idea, making use of models, sketches and later, temporary structures and equipment.

But the funding and time allocated to our project would not stretch to a full collaborative development; if we could not secure additional funds, a compromise would be necessary. First, I attempted to interest the a newly constituted working group to tackle deprivation, preparing a short paper that was presented to the group prior to their March meeting. While they carried a motion to adopt what they termed a “total place approach” (email from a contact at the PCT, 23rd March 2012) they elected not to pursue the Hub idea directly, instead endorsing formally the Asset Based Community Development (ABCD) approach.

The closing of this door left us in somewhat of a conundrum with our project. We did not want to develop anything “in silo” (email, 23rd March 2012); in other words, something that would either replicate or compete with the ABCD work happening in the neighbourhood. But we had to do…something.

So, we deconstructed the Hub idea. What was at its core, the underlying principles behind it? We went back to the outputs from the co-design workshop, and concluded that at the essence of the Hub idea was positive, non-stigmatising, informed social support. We visualised the Hub as an idea that could develop over time, starting as a loosely connected network of people offering friendship and advice on what services were available (we had already found some volunteers who were keen to get involved; they had been our original co-designers). In time, this network would develop its own identity, a ‘brand’ of
sorts, with a logo and literature, which would become recognised in the
neighbourhood. The ultimate vision for the Hub would be to bring together the
network and identity into a physical location, the concept visualised at the
workshop.

So, a decision was made to develop the Hub as a network of befrienders and
mentors, along with providing clear information and busting some of the myths
that scare people away from accessing help. Anna spoke to the co-designers
about this idea; they were very keen. We started referring to these potential
volunteers in communications between the project team as “Champions” (Anna’s
term, 22nd March 2012).

And so began our investigations into the practicalities of setting up. We were
particularly concerned with making sure our scheme and the volunteers were
looked after properly in the long-term. Otherwise we knew that not only would the
scheme fizzle out quickly, but having their expectations raised and then dashed
could damage volunteers themselves: we would have done exactly as described
in section 5.3, setting up a short-term initiative destined to close down, leaving
participants let-down and disappointed, yet again. We were determined to avoid
such an outcome.

...any volunteers we recruit will need to be really made a fuss of, trained (if need be)
and supported for as long as possible... most of the people I’ve spoken to have really
low self-esteem and a desire to not do anything that will draw attention to them, and
I don’t want to leave them high and dry (Email from Anna to me, 18th April 2012)

Our investigations centred on three areas: firstly, how to set up such a scheme
sustainably. Primarily we were concerned with who would look after the
volunteers in the long-term. Secondly, we needed to decide how to train our
volunteers, to equip them with the skills and knowledge to be a Champion. And
finally, we needed to provide them with a list of local services and facilities, to
which they could ‘signpost’ anyone in need of support or advice.

Local stakeholders were keen to help. We found that there were several
individuals and organisations with relevant and very valuable knowledge and
skills, including an expert on setting up schemes according to the principles of
the Mentoring and Befriending Foundation. The same organisation offered free training in techniques called ‘Identification and Brief Advice’ and ‘Motivational Interviewing’ (for a review, see Vasilaki, Hosier and Cox, 2006) and the Royal Society of Public Health’s Level 2 award in understanding health improvement.

Finally, we began to compile a list of what services were available locally and how they could be accessed, with the intention of working with volunteers to co-create suitable materials to help them communicate with those in need of help. Volunteers had suggested leaflets, but were also keen to explore more innovative ways of breaking down barriers to services and dispelling myths, such as a video created by the volunteers showing people where they would go, who they would meet and what to expect from the service. This, they felt, could be shared on smart phones and Facebook.

Were we behaving ethically, in attempting to set up such a scheme so quickly and on a shoestring budget? Our intentions were good, but upon reflection it is clear that we knew at the time that our scheme stood no chance of sustaining itself as an independent initiative. It needed a home, and we had two candidates in the running: one was the Community Health Trainers, who had been with us right from the start of the project and participated fully in the co-design process. They could provide the necessary training, knew about all the services and facilities locally and, even better, actually had plans to set up their own “health champion” scheme. Another possibility was the local Timebank, who had social networks and the infrastructure to manage volunteers already in place.

Timebanking is a means of exchange founded upon the principles of co-production (Boyle and Harris, 2009) and community capacity building (see for example Craig, 2007) where time is the principle currency. People ‘deposit’ their time and are able to ‘withdraw’ equivalent support as needed. Each participant decides what he or she can offer and everyone’s time is equal (Timebanking UK, 2012). Anna, Louise and I had an inspiring meeting with the Timebank staff, following which Louise intended to formally commission the Timebank to run the volunteering scheme. On reflection, I think Louise was delighted to have found a viable conclusion to the work that didn’t rely on the complicated manoeuvring of the various organisations involved in the various ABCD initiatives.
But the seeds of change sown in March with the establishment of the deprivation group, the ABCD project and the changes to commissioning structures dashed our hopes of a simple conclusion. I thought at the time that developing our own scheme, while simpler because it would be under our control and easier because we would be able to make all the important decisions ourselves rather than reach consensus with other stakeholders, would be the wrong decision:

_It seems unthinkable that we should disregard the opportunities that these collaborations present for us, and more importantly, for the communities. I think that, at best we are likely to duplicate these initiatives to some degree, and at worst create an atmosphere of competition among providers and confusion in the communities._ (Email from me to Louise, 16th May 2012).

Anna and I were so sure that full collaboration with the Health Trainers and the Timebank was the right course of action that we both offered to work as volunteers beyond the scope of our official funding. But, in an irony that was almost cruel, we needn’t have worried. While we were researching the practicalities of our network and discussing the possibility of local collaboration, the ABCD project had gained momentum and had chosen Parkhood as one of their ‘Learning Sites’. This meant that we couldn’t commission formally our scheme, as the whole thing was about to be subsumed in the wider ABCD initiative.

We were cordially invited to meet the representatives of the ABCD project to hand over the reins to them. But despite assurances that the ABCD team saw our work as a “foundation for the future” (email to me on 9th June 2012), my instinct at the time was that our ABCD contact wanted to find her own path. This rather unsatisfying handover marked the end of our involvement in Parkhood. ABCD were the new sheriffs in town. In subsequent informal conversations with Louise, despite her professionalism and tact, I got the impression that she shared my conclusions and also my disappointment at the way our work in Parkhood had ended.
5.15. A tough neighbourhood

Urbanwood, January – June 2012

In section 5.9 I explained how Anna had begun to get to know Urbanwood, primarily with Sarah’s help: Sarah had walked Anna around the streets chatting to residents and continued to invite her to various events and meetings. This section provides a complete narrative the Urbanwood part of the project, and because I go back to January 2012, there are some overlaps with previous sections, which will be explained throughout. In fact, the section starts with one such link…

Previously, I have explained that there was a feeling of disappointment, of disillusionment, related to the number of projects like ours that had come and gone (see section 5.3 on the stakeholder workshop). From the start of our work in Urbanwood therefore, we were sensitive to this and resolute that we would not echo past mistakes. Early on, Anna and I began to think of possible sources of funding for something permanent in Urbanwood, the challenge being of course that due to the participatory nature of the project we had no specifics. We met with our university’s development team to pick their brains about long-term funding for community-based initiatives. We also researched the issue of street drinking, having been forewarned by Sarah that Urbanwood residents would be quite likely to raise this as an issue. We discovered that one of our original stakeholders was responsible for a street drinking initiative in the centre of the city.

We filed away what we had found out about community funding and street drinking for future reference. But in fact, we needn’t have worried about either issue: funding, because of the ‘My Neighbourhood’ award in March (see section 5.13) and while some residents did talk about street drinking, other issues, which I will describe later in this section, were thought to be more significant. Mainly I think, trying to anticipate and prepare for such outcomes provided Anna and I with the reassurance that we were approaching the work in an ethical manner, we were not going to be just another project that stirred up enthusiasm and then
let people down. Perhaps this gave us the confidence to keep going in the face of a very tough set of issues.

Venturing out from under Sarah’s wing in January 2012, at the same time as much of Anna’s energy was being directed towards the methods roadshow in Parkhood (see section 5.9), she began what would be two enduring and productive relationships: one with the local housing officers and the other with the Police Community Support Officers (PCSOs). She knew from her early efforts to find out about Urbanwood that there would be fewer opportunities to tag along with existing activities at local venues, because as the community researchers had found, these were limited. Anna concluded that she would need to rely primarily on word-of-mouth. Briefly, we considered the possibility of incentivising people in Urbanwood to talk to us; this, the PCSO told Anna, was really the only way to motivate people to participate. We had a modest budget allocated to the data collection phase of the study which would have covered incentives in Urbanwood, but we held back, partly because we were concerned that Parkhood residents might feel short changed because so far (we had been talking to people in Parkhood for around 3 weeks at this point) we had not incentivised their participation. Primarily though, I felt that incentives would change the dynamics of the work, create a transactional relationship based on bribing people to engage. Better if people can believe that participation is something worthwhile for its own sake. Indeed in observing activities unrelated to our project at a community event later in the year, we did conclude that incentives created a sort of “going through the motions” style of participation (email to me from Anna, August 2012). As it turned out, the use of incentives was unnecessary; Anna’s social skills were sufficient to engage people in productive conversation.

Right from the start, we knew that Urbanwood was likely to be a tougher proposition. Unlike Parkhood, with its range of activities, there were few venues or services Anna could attend. She visited an art group, a toddler group and some debt advice sessions, but most of the data in Urbanwood was collected through a combination of brief conversations in the local shop “half of the stock is alcohol, the other half cheap frozen food and ready meals” (personal conversation with Anna, 5th December 2011), on the street and on people’s
doorsteps and longer interviews in participants’ homes organised through the housing officers and anti-social behaviour team. Anna was also invited to an event aimed at breaking down barriers between young and elderly people.

Many of the stories from Urbanwood, especially those from the in-depth interviews, were harrowing. The trauma participants describe juxtaposes uncomfortably in the field notes with the matter-of-fact way that they described their lives. Anna’s notes suggest she thinks them enormously courageous despite living with personal tragedy. The extracts I present in this section are longer than the more conventional two or three line quote because I think it is important to see how much each person has to deal with, how so many issues related to alcohol are interwoven in one short conversation (each conversation generated at most a page and a half of typed notes):

*Her bungalow flooded last year and she went through a really rough time. She even thought about suicide. Her drinking went right up because she wanted to blot out the total mess she was living in... She spoke about how hard it was to lose the photos of her children as she had to give them up to care when she had a bad spell of drinking and drugs a few years ago, and that was all she had to remember them by. She told me she was beaten by her first son’s father for 15 years... He drank heavily and she would dread him coming home from the pub – she tried to keep her son out of the way but when he was about 5 or 6 his father started hitting him as well. He used to wake him up and bring him downstairs to beat him so he wouldn’t wake up the other children.* (Female, 50s. Extract from field notes May 2012).

Anna told me that the themes that appeared most prominent in Urbanwood were feelings of being trapped and isolated; some of this was emotional but there seemed to be more people who were physically trapped due to caring responsibilities, physical disabilities and mental health problems. She recalled one man who had been given a flat when he moved back to Urbanwood, but he chose to go and live rough on the streets in the town centre with a bunch of street drinkers because he didn’t want to be on his own. Another had

*... acted as a carer for his wife for the last 14 years... He spends most of every day washing, dressing, feeding and entertaining her. He spoke of feeling exhausted and hopeless... He told me that they still love each other... But he did tell me that he feels like he sometimes fails her as a husband because he should be able to look after her yet he sometimes loses his temper and doesn’t give her the care he thinks she should*
have... On a bad day he sometimes thinks it would be better for both of them if he left the gas on and neither of them had to wake up in the morning. (Male, 40s. Extract from field notes May 2012).

Physical barriers increased isolation:

Over the years she has tried lots of external support services... She spoke about the self-consciousness of other people knowing she had a problem, and the effort to get out of the house when she had depression. (Female A, early 40s. Extract from field notes May 2012).

Anna didn’t think that people felt as worried or as ashamed about drinking in Urbanwood as they seemed to in Parkhood; instead she perceived a “stronger and heavier drinking culture” in which heavy drinking, violence and crime are simply “part of their lot” (Email from Anna to me, 30th May 2012); a deep seated resignation that permeates people’s lives from childhood.

She has had a drug and alcohol problem since she was 9 years old. She has been in and out of prison for shoplifting and spoke about how she would feel good in there because she ate better ...they used to call the discharge grant a ‘relapse grant’ because they would go straight out and buy booze or drugs with it. (Female A again).

Combined with loneliness, anxiety and fear these factors have a powerful effect on mental health and people’s drinking. Antisocial behaviour featured often as an explanation for people feeling trapped; they “can't move away” from violent, disruptive or even criminal neighbours. “One man who lived in a pre-fab told me about a local gang who just kicked a hole through his living room wall to steal from his house” (Email from Anna to me, 30th May 2012). Another man…

...boasted about some of the pranks they’d played in different blocks and streets over the years and the fights they’d had with each other, local neighbours or passers-by. ...He lives in one of the blocks of flats, and regularly gets together with friends to drink. He has received a number of ASB [Anti-Social Behaviour] orders in the past due to issues caused by some of these sessions because his neighbours complain about the noise and sometimes they [the participant and his friends] cause trouble, get into fights or break something. (Male, 30s. Extract from field notes May 2012).
Thus illustrating the perspective of the people on the other side of the antisocial neighbour trap, though it seems to me that this man is just as trapped by patterns of behaviour and sociality as he makes his neighbours feel.

5.16. What if you had a cheque for £1 million and a superpower?

*Urbanwood, May – June 2012*

Because we knew that people would be very unlikely to attend the more formal workshop format, Anna attended two community events in popular spaces (see section 3.11): one by herself and one with a Codesigns Associate to try and engage people in ‘pop-up’ co-design activities.

The first of these was at Urbanwood Pride in May 2012. Anna chose to concentrate on four themes: Alone (being isolated); Confused (about health messages); Worry and Family. She had printed some pretend cheques for £1,000, £100,000 and £1 million, which she used to engage people in conversation about how to tackle each theme. Because it was so difficult to persuade people in Urbanwood to involve themselves in anything unfamiliar, we were unsure about how successful any co-design would be and Anna was quite nervous that the event would be a failure. So, rather than the primary objective of the co-design being viable ideas, we decided to treat this event as a methodological experiment: if the pop-up format was successful, then we would throw more resources (including Codesigns’ time and budget for more sophisticated stimulus materials) at the forthcoming Jubilee Fun Day on 5th June 2012. If not, then we would have learned something and could rethink. This sort of thinking typifies participatory approaches in the sense that the researcher does not know whether an idea will be successful. There are two ways to deal with this uncertainty: to accept it as a necessary part of the participatory process, as an opportunity to learn, even if one is learning from failure. Or to be afraid of it, to regard failure as simply failure. By this point in my own experience with participatory approaches, I had learned to see co-design as the former and so could approach the possibility of failure with equanimity. But I had also learned to
minimise risk, hence treading softly and carefully before committing too many resources to a particular course of action that, if not successful, would mean those resources had been expended to little gain. Like much else in the project, there was a balance to be struck. In fact, the pop-up co-design format was reasonably successful despite people being unwilling to spend more than a few moments in brief conversation.

Suggestions in response to the Confused theme ranged from reopening valued local venues, building new facilities (a health club and GP surgery, harking back once again to the community researchers’ findings and reinforcing our impression that people in Urbanwood felt as though they lacked basic facilities) to using Facebook and designing an ‘Urbanwood app’ to communicate with people about their health and what help is available.

In response to the Worried theme (which most people interpreted as mental health problems like depression or anxiety), participants suggested a free counselling service that would visit people at home; this particular idea will be revisited in later sections. Education in general and about mental health issues in particular was a related suggestion, particularly for children because participants felt prejudice was difficult to challenge among adults. With £100,000, one participant would provide support and respite for carers, days out for families, and with £1 million; another would remove what they saw as the cause of much of people’s worry: “give them a job and a way to get out of debt”.

The Alone theme generated the most ideas. These ranged from simple suggestions for local, free clubs people could join to more family fun days. Participants spoke about the barriers between people: between older and younger people; the latter are perceived to have little to do other than hang around on the streets, which intimidates older people and discourages them from going out. If people do go outside, there are very few things for them to do in Urbanwood “there’s nothing here, GP, dentist, drop-in centre”.

Somewhere to go to just talk to people – all there is in [Urbanwood] is waiting in the shop queue, then you go back to your flat alone. Need somewhere to go where you don’t have to join in activities or events but can just be with other people, like a
coffee shop or drop-in centre. (Reported conversation with Pride Day co-designer, field notes).

Allied to the lack of facilities locally, many people are unable to access things outside the neighbourhood because they have mobility issues or they can’t afford the bus fare. A participant suggested a minibus for Urbanwood, free or very low-cost local transport that could take people shopping.

Emboldened by the relative success at the Pride Day, Anna, Phoebe and I made plans for the pop-up event at the Jubilee Fun Day. This time, we decided to explore a different approach to see whether very different ideas were generated. Rather than using the individual themes and the cheques, a Codesigns Associate created simple comic strip visualisations of the four case study stories: Lauren, Jane, Alex and Michael (see appendix 2). We planned to engage participants’ imagination by asking them what superpower they would use to help these characters. Anna and a Codesigns Associate hired superhero costumes to help participants get into the spirit of the activity (and make them laugh!).

The presence at the event of a Codesigns Associate who was a talented illustrator opened up the possibility that ideas could be visualised, even mocked up, as participants were talking opened up even more creative possibilities. We made plans to borrow and table and Phoebe arranged to courier the necessary equipment to Urbanwood.

This approach generated some very creative suggestions: telekinesis could bring Lauren’s support network closer and zap the unhealthy social influences around her to the moon. A “Love Laser” could make Michael feel loved and able to deal with all he’d lost through zapping him with a love beam.

But once again, people were difficult to engage for more than a few casual moments. Anna and the Codesigns Associate found that they had to go free range, leaving their table and visualising equipment behind. The weather was wet, most of the people attending the event were congregated in the bar area, relaxing on comfortable chairs. So, most of the creative ideas (except for the superhero costumes) were abandoned in favour of, once again, simply engaging people in conversation.
The ideas generated were very similar to those at the Pride day (and similar to many of the ideas from the Parkhood workshop as well): breaking down barriers between people and befriending schemes; combatting isolation, reassuring people; promoting opportunities to make friends, giving people something to get out of bed for; local transport, a “community bus” or services that would come into Urbanwood.

Phoebe’s conclusion was that we had partially fulfilled our aims: we had generated some good ideas, but there hadn’t been time to work them up with participants. Consequently, because any further development would have to be led by the research team, she didn’t feel that we had “empowered” participants to “own the ideas”. The problem was, time was running out. Anna’s contract had only a few weeks to run and it didn’t look as though anyone was going to rush in, thank us for our work and ask for a straightforward handover, as they had in Parkhood.

The insight that led us to our pilot was that people had said that they couldn’t access fresh fruit and vegetables in Urbanwood even if they wanted to, because the shop only sold alcohol and cheap frozen food. Yet, there was a man with a small vegetable delivery business who was keen to gain new customers in Urbanwood. What was the problem? Why were this need and this solution not connecting? This micro-problem began a conversation between Phoebe, Anna and myself that linked the issue of isolation and consequent low emotional wellbeing of many people in Urbanwood with the strong feeling that the neighbourhood lacked access to important facilities. Thus, the mobile hub concept was born. The mobile hub was based on the idea of the 1950s mobile library cum shop but with a doctor on board ready to offer consultations to anyone who needed help. With a café. The next section describes the development of the pilot.
5.17. It’s the Sex Bus!

*Urbanwood, June - August 2012*

Throughout the remainder of June, Anna, who was by now very involved in the Urbanwood community, took the mobile hub idea to various events. Armed with a printed out cartoon of a bus and a biro, she captured a range of ideas from local stakeholders.

They suggested that the mobile hub be given a permanent name; and while Urbmobile was felt to be a bit ‘cheesy’ most people liked it. They also liked the idea of images and branding that could be used for future engagement and ‘My Neighbourhood’ events. To achieve this, the branding would have to be something identifiable as belonging to Urbanwood and related to improving the community in general. They suggested that each day of the pilot be themed so that people would know what to expect, and information about what was happening could be updated easily using a menu type blackboard, which would also fit nicely with the suggested daily street café; something positive, non-stigmatising, somewhere to just go and chat.

The final design of the Urbmobile was inspired by two ideas from stakeholders: the first was to use a map of Urbanwood landmarks, a way of making it feel like it belonged to the neighbourhood and was not shared with anywhere else. The other was to continue to co-design the vehicle as part of the pilot. We were slightly concerned (based on stakeholder advice) about the possibility of destructive graffiti, so a blackboard format with some space for volunteers and stakeholders to decorate together was felt to be the most effective compromise.

The Council’s Youth Services vehicle (known locally as the Sex Bus because of its usual role in youth outreach, presumably because youth workers offered advice on sexual health) became the Urbmobile for four days. We procured picnic tables, as well as a café style blackboard and a supply of chalk markers. Our ill-fated gazebo, originally signed for by the enterprising ‘Darren in Flat 4’ and only replaced by the supplier when the University’s finance team got involved, was accidentally reversed over by the Urbmobile on the second day of the pilot,
requiring regular repair with strong adhesive tape. But these challenges, while a source of frustration at the time, served to unite the team of local volunteers. Appropriate permissions were gained from the council to provide a street café, and supplies of beverages and snacks were arranged. Our friends from the housing association and in the youth work team offered to drive and insurance was organised via the Youth Services team. Each of the four days of the pilot was themed around a particular topic: community engagement (primarily aimed at engaging people in the ‘My Neighbourhood’ project), youth and family, housing and money and health.

The first few days of the pilot were deemed a success; summarised as “lots of people and amazing weather” (email from Anna on Friday 10th August). On the first day, which coincided with yet another Urbanwood Community Fun Day, Anna estimated that around 200 people engaged with the Urbmobile in some form; as had become the norm, most of these interactions were brief and anonymous conversations. Anna thought that so many people attended because they had been offered incentives (free tickets for the fairground rides, for example), but

...the quality and in-depth nature of the info suffered in my opinion because the majority were just going through the motions to get their freebee. (Email from Anna to me, 12th August 2012).

Much like our co-design stimulus at the Pride day, people were asked what they might do to make things better in Urbanwood if they had £1 million. Except this time, the £1 million was really there.

The range of suggestions was familiar: a communal space like a cafe, where people could meet for informal social support and also have other services on offer (ranging from fresh healthy food to more statutory advice, chemists and even hairdressing upstairs). Very similar in fact to the Parkhood Hub concept. All the suggestions were collected in one big box, which we gave to the ‘My Neighbourhood’ team.
The second day things were closer to normal in Urbanwood. Without the incentive of the fun day, Anna and the Codesigns Associate found it an uphill struggle to get people to engage and they spend a great deal of time knocking on doors. She also noted a very different reaction from residents in the relatively affluent Treepark estate compared to the more deprived Avenues residents. The people in Treepark estate were reluctant to talk to the Urbmobile volunteers; they lived in a nice area and didn’t need any improvements made, they said.

_They also didn’t want to investigate our mobile cafe, and looked on it with suspicion ([someone] told me that travellers have previously been a problem in the area). (Email from Anna to me, 12th August 2012)._ 

Yet Sarah had told Anna that Treepark estate residents often say they want more activities to happen in their part of the neighbourhood. Perhaps it was the ‘rough and ready’ nature of our pilot, with our cheap, slightly bent gazebo and plastic chairs that put them off? Perhaps they sensed that it was activity designed for people in a ‘deprived neighbourhood’ and didn’t want to associate themselves with such a label?

Anna was keen to communicate that if we found that engagement was poor over the final days of the pilot, we shouldn’t write off the concept of mobile services completely. She reported conversations with the youth workers and with Phoebe, all of whom said that it often takes a few days before a mobile engagement unit is a familiar enough presence for people to be comfortable. Anna wanted to be sure that I understood that poor engagement during the pilot was quite likely to be because the disengaged and cautious residents are suspicious of anything new and especially anything that is related to authority, rather than flaws in the concept of mobile services itself.

On 10th August, Anna made me aware of some sad news: a close relative of hers had passed away. I had already planned to attend at least one day of the pilot, so I offered to lead the day in Anna’s stead so she could attend the funeral, despite knowing that the social aspects of engaging strangers in conversation is not a strength of mine (to put it mildly).
The prospect of taking responsibility for the day was even more terrifying because Anna herself wouldn't be there with her strong relationships with stakeholders and deep local knowledge. I would be alone; people would be looking to me for direction and decisions about what we should do.

Deep breath.

I arrived in Urbanwood early on Tuesday morning. I can't remember what the weather was like, other than it wasn't raining; I was too focused on the nerve-wracking task that lay ahead. First I concentrated on sorting out some logistics: I needed to get the key for the community centre and then collect the volunteer who had offered to make healthy soup (this was the health day of the pilot) and deliver her and her equipment to the community centre kitchens, the only place with appropriate hygiene certification. Then I helped set up the café: put chairs and tables out and repair the gazebo once again. It was clear that the volunteers knew what they were doing; they didn't need my direction, so I decided to act mainly as an observer. I chatted to a couple of stakeholders about their work, there was a lady there who was hoping to interest people in a community garden that she was setting up. She was an experienced community organiser and had set up several similar projects elsewhere. She told me that she was finding it quite difficult to engage with people in Urbanwood. Looking back now, it seems as though there was an invisible wall between stakeholders and Urbanwood residents: stakeholders desperate to engage people in something, anything that would make their lives better. Residents desperate for some sort of recognition that they deserved facilities and services. So why weren't they connecting? I can only speculate, but I think that the problem is less that the barriers to engagement are too high or that the stakeholders are doing the wrong thing. In my experience it was closer to a parallel universe occupying the same space. Residents and stakeholders used the same language, saw the same sights, but the meanings they took from these were very different. One example Anna gave me was this issue of access to a GP in Urbanwood: when residents told us that they didn't have access, we were told that the PCT would look at this; and it was found that the distance between Urbanwood residents and a GP was within the guidelines. But what residents meant when they said they didn't have access to a
GP was that to get to their designated practice, they needed to take public transport (especially if disabled, and as explained previously, many more people than the city average had life limiting disability). And the cost of public transport meant that they would have to miss a meal that day. That is what Urbanwood residents mean when they say they have no GP:

“I regularly come across residents who chose to eat before paying for transport to attend doctors surgeries a couple of miles away or to visit the City.” Sarah, 23 October 2012

Another example of this parallel universe is found in a conversation I had with a well-respected lady who was involved heavily in youth work in Urbanwood. As we sat on the Urbmobile benches together, watching the local children, some as young as 5 years old, wander past us towards the shop where they would buy sweets and energy drinks, she spoke of how she hadn’t allowed her own daughter to roam free in this way. Her neighbours, she told me, had disapproved, felt that she wasn’t equipping her daughter with the skills to be streetwise, to look out for herself. Later I reflected upon the disapproval I would have been subjected to from my own peers in my neighbourhood if I had allowed my daughter (then 5) to wander our neighbourhood unsupervised. I have no doubt that my neighbours would have felt this warranted a call to social services, yet in Urbanwood, within a group of people born in the same region and ostensibly of the same culture as I, not encouraging a young child to develop the skills to be streetwise was viewed with equal disapproval.

Later, Sarah told us that the ‘My Neighbourhood’ team had received 140 completed questionnaires, and found overwhelmingly that people identified a need for better health facilities and activities for young people. Further work is planned to develop these ideas over the next 10 years. A small survey of 68 residents aged between 35 and 55 conducted before and after the project showed that recollection of the Urbmobile pilot was high and the idea was very popular (86% thought it should go ahead), though some people felt that affordable, local public transport would be more empowering.
5.18. Concluding comments

In this section I have constructed a complex narrative of an 18-month project informed by participatory methods. This chapter hasn’t described every detail of the project. It couldn’t possibly. The narrative includes only those stories and recollections that seemed most important to me at the time of writing and those I remember as having significance at the time (which aren’t always the same things). When I showed this section to Anna as part of the process of getting her consent to feature in the story, she told me that reading it had brought the project back to life for her, in particular it had reminded her of what she called the “emotional work” (personal conversation, 15th April 2014) that was involved.

For me, the project involved a different sort of emotional work to that described by Anna. I was responsible for everything, driving it forward, making sure we didn’t over spend the budget, making sure Anna was safe while doing fieldwork. And of course trying to hold to participatory and ethical principles within a system of public health commissioning and knowledge creation that had very different ideas about research. Something that strikes me in particular having written this story and now reading it back to myself is how hard we tried to find people who wanted to work with us, to be like Cahill’s (2007b) Fed Up Honeys and take the reins of the project, creating knowledge of themselves, for themselves. But nobody really wanted to engage and I am frustrated by my lack of insight as to why that might be. I could explore the idea that it was a class issue, education, lack of motivation, ambition, belief in their capacity to effect change, but I feel that to go down this path is to excuse my failings by elevating myself, educationally, intellectually, above people in Parkhood and Urbanwood. And I don’t think that’s right. It’s not right ethically and it isn’t correct, either.

No, my guess is that they had bigger things to worry about, stressful lives filled with tasks and worries and a project about drinking offered nothing in particular to make them want to add to their daily burden. And we were not offering the sort of liberation Freire describes in the people he worked with in Brazil, one of whom told him "I now realize I am a person, an educated person" (2000, p. 33).
next Chapter, in which I make some sense of it all, homes in on some of these reflective moments and is presented as a more conventional account organised by theme. As I described in Chapter 4, the themes were identified in two ways: I kept a diary during the writing of this first chapter, noting ideas and their possible significance and I used the narrative from this chapter as the basis for thematic analysis.
Chapter 6: In which I make some sense of it all

“I could tell you my adventures beginning from this morning,” said Alice a little timidly: “but it's no use going back to yesterday, because I was a different person then” (Alice, Alice’s Adventures in Wonderland, p. 71).

6.1. Introduction

Denzin and Lincoln hope “that social sciences and humanities become sites for critical conversations about democracy, race, gender, class, nation states, globalization, freedom and community” (2005, p. 3). This second results chapter, in which I make some sense of it all, is presented as a somewhat more conventional account organised by theme and attempts to engage with some of these issues. In particular I explore issues of class and gender, freedom and community, as well as exploring the ways in which language, discourses and power relations manifested themselves. As explained in Chapter 4, I developed the themes described in this chapter by combining different analytical techniques: First, I kept a diary during the organisation of the data and throughout the generative process of writing the previous chapter, in which I noted down nascent ideas as they started to emerge. Then, once I had finished writing the story of the project that forms the previous chapter, I analysed it, employing the principles of thematic analysis (Braun and Clarke, 2006). I undertook an intense and extensive period of open coding, in which I coded the original dataset, the previous chapter and a small selection of relevant theoretical literature in order to generate the building blocks for the analysis. Then, as Braun and Clark suggest, I used mind maps (see also Wheeldon, 2010), which I found invaluable in exploring and making sense of the complex array of codes and ideas I had created. I generated a total of four mind maps on large sheets of plain paper, referencing the codes I had developed and the ideas noted in my research diary; each map became less complex as the process of representing visually the core ideas helped clarify and solidify the themes.
As I write this introductory paragraph, I have yet to undergo the transformational process (to me) of converting the various disparate strands of thought, scribble, mind map, sub-title and the occasional paragraph captured in a fit of creative inspiration into a chapter of coherent prose that explains my conclusions and justifies my reasoning. I call this process transformational in the sense that Richardson seems to mean when she calls writing itself a way of knowing. So, like the previous chapter, the very process of attempting to make myself understood will contribute to the richness of the themes, enhance their rigour and depth; will clarify to myself what I mean and what I can claim to know. I anticipate continuing to work with the data as I write this chapter and I will continue to keep the research diary. In the conclusion to this chapter and in the final conclusions and discussion chapters, I will reflect not only upon the ‘findings’ (if it is appropriate to call them this) of this autoethnographic study, but also upon the process of what seems to have turned out to be a multi-layered study of a method within a method within a method.

I have continued to write the themes in a narrative style, this is to preserve the authenticity of the process by which the knowledge was created and, by meeting the criteria of honesty and transparency, help readers to judge the interpretations offered here.

6.2. To infinity and beyond

“In common usage, positive space is the part of a painting that carries the creative action, the aesthetic or artistic manifestation of the work. As the primary part of a painting, the positive space concentrates in itself the dynamic strength of the painting. It is the field charged with the painter's message.... The negative space is subordinate to the positive space. It is inferior in importance, since its content is minimal to non-existent. Nevertheless, its presence in the painting is both desirable and functional” (Kočíb, 1986, p. 141).
When I think of the hundreds of reports and articles I’ve read and the handful that I’ve written that document a project or a process, I cannot recall a great deal of attention being paid to what didn’t happen. Yet reading through the raw data in preparation for writing the previous chapter, one of the first reflective thoughts that I recorded in my notes was the number of promising ideas that came up during the course of the project that went nowhere. I was struck by the apparent arbitrariness of which ideas ‘stuck’ with a participatory project like this one. Why did some ideas get implemented and others not? Was it because they were better ideas or were there other reasons?

Following the analytical act of writing the narrative and subsequently coding and mapping the themes, this emerging idea of things that didn’t happen, of things that were abandoned, was still significant. Particularly so given the strength of feeling in Urbanwood that the people there had been abandoned: let down and forgotten by the authorities and the way that we were required to abandon our vision for the Parkhood Hub to make way for the Asset Based Community Development project (which is linked to themes around politics, which will be discussed in later sections). In essence, then, this theme is about whether there is significance in the things that did not happen, and what that significance might be.

One way to conceptualise this did / didn’t happen dichotomy is the idea of negative space. In art, students are taught to study the negative space as well as the positive

“...if you are drawing a model with a hand on her hip, the triangle of space inside her bent her arm is as much a part of the drawing as the arm itself... Betty Edwards (1997) talks about how when we start drawing, we tend to get stuck on visual representations of things rather than seeing what is there. In other words, we might draw a child’s version of a flower (a round circle surrounded by petals) rather than the actual flower that is in front of us. By looking at negative spaces, we take our focus from the idea of a flower to the reality of an object in space. And that is when we start being able to draw what is there” (Dembling, 2012).

This notion leads me to wonder whether the things that didn’t happen should be considered as important a part of the overall picture of a participatory project as the things that did. To focus only on the positive space is to offer only a partial,
unsophisticated understanding: a child’s simplistic drawing of a flower. Or perhaps, worse, ignoring the negative space presents a story that is not neutral but instead conspires to hide, even silence (Ward and Winstanley, 2003), the difficulties and the failures and (probably most importantly) the reasons for them.

There is an obvious conundrum associated with this concept though: how does one identify and delineate this negative space? In theory, it could be infinite, which would make the concept rather useless. So I reviewed the project narrative to try and give the idea of negative space some form and boundaries. The first two examples of negative space appear early in the project; that stormy day in December 2011 when the invited workshop participants didn’t attend and during the workshop when we abandoned the idea of creative ‘research graffiti’ because of fears that it would simply be vandalised. These two examples seem different because one was partially out of our control; we didn’t know that the women invited to the workshop wouldn’t come, though we could have regained control by choosing to reschedule or engage with those women in a different way at a later date. Conversely, the other represented a conscious decision not to pursue a particular option. But in both these examples, it seems to me that what became negative space were the options that were more risky, less familiar. We ran the workshop with the youth workers because they were there and were enthusiastic. It was a safer (and less costly) option than cancelling it and trying a different approach to work with less available, less engaged people. And the research graffiti idea was abandoned precisely because it was considered too risky.

The research graffiti was merely the first of several other ideas linked to art and creativity, of giving participants the opportunity to express themselves, that became part of the project’s negative space. In the narrative, I describe an animated conversation with the youth club manager about expanding the ‘creating’ method station into an activity whereby local people collaborate with local artists to create an exhibition of work that expresses the role of alcohol in local life. This seemed like a good idea on several counts: the positive reaction people had to the creating station, once they’d overcome their stage fright, the way it gave Anna the opportunity to talk about what the participant was doing as a way of exploring the issue, the way making something gave participants
something to focus on, removing some of the awkwardness of talking about such
difficult topics with a stranger. It could have functioned as a consciousness
raising (Freire, 2000), even transformational activity both within the
neighbourhood and outside.

But this fragile shoot of an idea got trampled as the project ground forwards;
primarily, I think, because it wasn’t linked clearly enough to the project’s
‘outcomes’ but also, again, it seemed risky, difficult to control and predict. I tried
to resurrect the idea a couple of times, including making several separate
applications for funding, but without success. My continuing failure to make
participant creativity and expression central to the project appears central to the
negative space. Other examples of were the collaborations and relationships that
appeared promising at first, but that fell by the wayside. An early case was the
community researcher project, another was a local charitable organisation, which
was closely involved in early meetings and which we assumed would be a
significant partner in the work but who, for reasons I do not know, kept their
distance. Yet another example from much later in the project was the emerging
relationship between our work and the Timebanking organisation; this was almost
certainly stymied by political factors: the new and powerful ABCD project wanted
to call the shots. Compare these people and relationships in the negative space
with our relationship with Sarah and the way in which she supported (and
consequently helped shape the direction of) our project. It seems reasonable to
assume that if we had met someone different, if the community researcher
project had embraced collaboration, for example, then we could have gone in a
very different direction.

6.3. Making room for absence

But while it is both easy and interesting to sit back and speculate on what might
have been had things been different, to make the observation that projects can
take many different paths without an examination of the reasons why some
things fall into the negative space and what the consequences of failing to
acknowledge the negative space might be is to make rather a humdrum point.
Negative space, if it is not to be defined as the infinity of possibilities that are created and closed off whenever any action is taken, needs elaboration. Figure 10 attempts to define the project’s Negative Space as emerging from the data:

![Diagram of Negative Space](image)

**Figure 10: The project's Negative Space emerging from the data**

I have shown in the discussion above that the negative space of this project was composed of rejected or forgotten ideas and people and relationships that could have been significant but that drifted away. To this list, I would add the voices that went unheard (or perhaps were even silenced, like those of the community researchers when they presented their findings at the town hall) and the alternative interpretations that could have been made of our research data had we a) had more time to dedicate to analysis and b) had realised our original ambition to involve participants in a process of co-analysis. There are a number of ways that the data could have been interpreted differently: first, the original Family theme for example, contained within it data about acceptable and unacceptable parenting and about how participants who had been affected negatively by alcohol often seemed to begin their stories in childhood. Another observation that cut across several of the original themes was that people often seemed to have constructed ‘normality’ differently to the way that public health
professionals would have liked them to. An example being how ‘normal’ or acceptable violence might be in different situations, or what might constitute ‘problem’ drinking. But the specifics of how the analysis could have been different, while interesting, are less relevant in the context of negative space than the observation that there these alternative interpretations existed; and therefore that a full understanding of the whole picture of the project should acknowledge the implication of their absence as well as the implication of the presence of the themes we took forward into the co-design phase.

The emails between the project team provide some enlightenment about why negative space might occur as well: highlighting that negotiations about what can and can’t be done, about who will and won’t be involved link both to the resistance and legitimacy themes (the subject of later sections in this chapter). Ideas and interpretations are judged, often openly but probably subconsciously as well, against the criteria set for the project and against the degree of risk and impracticality they might pose. And in the writing up, the process is made logical, sequential, erasing the negative space in sympathy with “the positivist view of research process, which attempts to eradicate all uncertainty and mess and ambivalence… Thus we abandon our intuition, vision, creativity, and the hopes of our participants, in the face of epistemic antipathy towards uncertainty.” (Y Morey, personal communication, 28th February 2014).

6.4. Shadow boxing

Probably the most significant aspect of the construct of negative space in a participatory project is the consequences of what doesn’t happen. There are three implications of negative space in my autoethnographic reflections: the first is rather obvious: if an idea becomes part of the negative rather than the positive space of a project, then it won’t be counted as part of a project’s effects or be evaluated; it will also be absent from the project’s reported ‘outcomes’ and also from any contributions to knowledge in the form of publication unless explicitly recognised, as is happening here in this thesis. However, the two implications of negative space that are the most interesting and consequential relate to
epistemic and to ethical issues. In other words, what might be different about the knowledge available to us in the negative space compared to the positive? And have the right things, the ethical things, gone into each?

As outlined in Chapter 4, this autoethnography is founded in a rejection of epistemic absolutism; consequently I do not adhere to the belief that there is an authoritative epistemic standard against which an accurate representation of an independent reality (Smith and Hodkinson, 2005) is deemed ‘Truth’ (Luper, 2004). So epistemologically, the negative space here may represent an alternative truth, a more nuanced and complex truth or a less powerful truth, rather than untruth.

Keeping in mind the recognition that certain groups are thought to dominate conventional social science research (i.e. middle-class, heterosexual, white, able-bodied and male, Ellis, Adams and Bochner, 2011), we sought to prioritise the voices of people normally marginalised by virtue of their status as ‘working-class’ or residents of a deprived area. These voices and those of our stakeholders (the people who have first-hand experience working in deprived areas) form the positive space of the project, as much as is possible given that they are filtered through my interpretation. An example of this from the data was our change of direction early in the project when we realised that participants were not, as we had assumed they would be, embarrassed to talk about drinking. In fact, what seemed to happen was that our status as researchers served to legitimise these voices that would not normally be listened to. I regard this observation about the legitimisation of participant voices by researchers as a separate theme, which will be discussed in a later section.

However, while an explicit recognition of the power dynamics of social class was part of the project’s positive space, issues of gender, race, disability and sexual orientation were firmly in the negative space. There is nothing in the data about race and sexual orientation that gives any shape even to the negative space, and very little related to disability other than a recognition of different conceptions of ‘nearness’ of facilities like a GP practice, which may be dependent upon one’s physical abilities.
6.5. What about teh menz?

“What about teh menz?” is feminist shorthand for the phenomenon sometimes observed in online discussions about women’s issues where someone will interrupt the discussion to argue that whatever issue is being discussed affects men too. This is usually interpreted as derailing the discussion (i.e. “the act of throwing a thread in a discussion forum off topic, oftentimes [sic] so much so that the original discussion is unable to continue”, Urban Dictionary, 2009). Thus having the effect, whether intentional or not, of silencing the women involved in the original discussion. But in this project I think there is a good argument to be made that despite our lack of deliberate recognition of gender issues in the research, the positive space of the project represents a feminised interpretation and the negative space a masculinised one. So, what about the men?

I reach this conclusion partly because of the imbalance in participants; quantitatively, there are more female stories in the data than there are male. It is also relevant to consider why, in Parkhood, the co-designed idea that was selected to take forward was one that resonated with engaged female participants; and conversely why the ideas that weren’t pursued (primarily activities to divert men from organising their social activity around drinking) were those that would have had more impact upon male drinkers and which would have needed the committed involvement of male participants, of which there were very few.

While I would identify as feminist and have sympathy with the aims of a feminist epistemology, neither the participatory project nor the autoethnography was undertaken from an explicitly feminist epistemic standpoint (Harding, 1991). Partly because I recognised from the start of the work that women were in control of the project: women conceived it, organised its funding, agreed its objectives and terms of reference, managed it, collected and interpreted all the data, forged the relationships and wrote all the documents. In this project we held the positions of power; we were the dominant group. Men and male perspectives, conventionally those that are dominant, largely fell into the negative space. A
good example of our feminised interpretation from the data is the way we constructed stories from men as “boasting” about drunken exploits, exploits that frequently involved some sort of violence. Our tone is one of disapproval; our choice of language betrays our lack of sympathy for the viewpoint of someone who could be proud of getting into a drunken fight; of finding violence amusing, a source of pride. This stands in contrast to our sympathetic portrayal of the lonely, powerless young mother who turns to drink in desperation. She inhabits the positive space in this project; the drunken violent male dwells in the negative.

The observation that, upon reflection, we seemed to have judged male behaviour and perspectives more harshly has an obvious ethical dimension as well as epistemological consequences; particularly given that our subsequent actions (advocating the Parkhood Hub vision and its semi-adaptation into the Urbmobile) are more strongly associated with female stories of loneliness and isolation rather than male stories of a social life organised around drinking and finding entertainment in violence. It may be obvious to the reader by now that I still hold this perspective. I do disapprove of the notion that drunken violence is amusing (not to say this is an exclusively male phenomenon, though it was so among our data). I do sympathise with a lonely young mother. But arguments about epistemic standpoint aside, is it ethical to allow personal judgements like these, no doubt informed by more general feminist reading on the phenomenon of male violence, to influence which ideas form the positive space and which the negative in a participatory project? The question of ethics was addressed in the methodology chapter, but was primarily concerned with positionality and representation in the research process. This question of the ethics of positive and negative space needs wider input from the ethics literature, which will be dealt with in the concluding chapter.

6.6. The silence of the insider

“I do not know if you realise the importance of this piece of work. Your conclusions are not new as we have all been saying it for ever but by producing the report as clear and decisive as this then commissioners
The idea of ‘legitimacy’ as a name for this theme originated from the work of Laurel Richardson (2005); she writes of “illegitimacy”, conceptualising it as her struggle with the academy “being in it and against it at the same time” (p. 966) and wondering if others faced a similar struggle to make sense of themselves and act ethically. As a subjectivist researcher working primarily in an objectivist domain (i.e. public health) I identified strongly with this dilemma. Thus legitimacy / illegitimacy, like the idea of abandonment, was present in my earliest reflections. In fact, I first wrote about the idea in June 2013 as part of an early draft of the methodology chapter, where I conceptualised it as being inside the system of public health research and against it at the same time. Indeed this notion of being against the system has evolved into to the resistance theme, which is articulated later.

Like the construct of positive and negative space, the theme of legitimacy also invites dichotomy. In this project, at first there appeared to be two types of legitimacy: legitimacy of voice (i.e. whose interpretations were listened to, as illustrated by the stakeholder quotation that opened this section). And secondly, legitimacy of outcome: which outcomes were regarded as legitimate for the type of work that had been commissioned; an example being the arts-based work, the potential of which I struggled to realise despite stakeholder support. I came to suspect that this might be because it was not regarded as a legitimate outcome for a social marketing project. However, later and more critical reflections have revealed a more nuanced interpretation, which will be outlined in this section.

Unlike the concept of negative space though, the construct of legitimacy carries with it an enormous weight of academic literature and of particular relevance seems to be the conception of legitimacy from political science (as opposed to the meaning of the term in law, i.e. a child born of parents lawfully married to each other, Oxford Dictionaries, 2014). Political legitimacy, as I think it is relevant to this study, relates to the authority a decision-maker has, i.e. others’ “belief in the rightness of the decision or the process of decision making” (Dahl, 2013, p.
46). In political science, the decision maker to which scholars refer is a government; but in my initial reflections, legitimacy seemed to be a useful way to conceptualise the rightness of someone’s claim to truth or the processes of arriving at that truth. In other words, who has the right to speak and from where or what is this ‘right' to speak derived?

A stark example of this phenomenon from the project can be found in the vignette from the previous chapter about the community researchers’ presentation; concerning why the community researchers themselves weren’t really believed when they told people what they had found out about their neighbourhood, whereas when I drew similar conclusions later in the project, my claims were accepted. Another, perhaps more subtle example, is the outcomes of the very first workshop with stakeholders, when they told us that the reason people drink too much is that they have little motivation to change, because they are disengaged and often suffering from mental health issues. This room full of experienced stakeholders knew what the problems were, they had understood them for a long time and yet they felt that those with the power to make changes hadn’t listened. Why not?

6.7. It’s not just voice

These observations led me to reflect deeply upon the different degrees of legitimacy afforded to different voices in different contexts. Another example of legitimacy working in a slightly different way can be found in my notes about our status as outsiders, particularly whether our lack of lived experience of deprivation would lead us to draw simplistic and blame-filled conclusions (in other words, illegitimate conclusions) about people’s lives. Two instances early in the project were Sarah’s concerns about whether we realised the scale of the problem and our mistaken assumption that participants would be embarrassed, even ashamed, to talk about their drinking. In those examples, we were the ones who lacked legitimacy. Following this reflection, I have concluded that there is more to legitimacy in a participatory project like this one than simply the right to a voice.
Before describing the sorts of phenomena that the data implies we scrutinised for legitimacy during the course of the project, it is important to make clear that nowhere in the data does it suggest we thought consciously about legitimacy *per se*. The closest we got to thinking about the construct as I have described it here were the discussions we had about different courses of action (such as the ones about how to introduce the project to participants), but we didn’t ask each other and I didn’t think until I started the reflective writing process, “is this a legitimate thing to do?” No, the judgements we seemed to make about legitimacy were in some cases totally subconscious reactions and in others very practical in nature; what we should do, what it might lead to, what the problems associated with a particular course of action might be. What is apparent in the data and in my own recollections though, is that the way we deliberated was often related to notions that speak to legitimacy in the context of participation: were we excluding anyone, imposing our own perspective, making assumptions, guilty of manipulating the outcomes? In essence, frequently we asked ourselves whether our actions were consummate with a legitimately participatory process.

But I argue here that the subconscious verdicts and the practical discussions about what course of action we should take can be conceptualised as judgements about what was and what was not legitimate in a political sense; whether there was, as Dahl writes, a belief in the rightness of the decision or the
decision making process (2013). I found four different sorts of things in the data that seem to have been judged as legitimate, or not. These are conclusions and decisions (which I consider analogous to Dahl’s notion of a decision) and actions and goals (decision making process).

The first type is conclusions, which encompasses the observations that first led to the identification of this theme in the data: the question of who gets to claim knowledge of something; of who gets to be believed. The second type, a decision, is often followed by an action but can also be a decision to allow something to happen or to react in a certain way should something happen; for example, our decision to allow the young girl to participate in the ideas workshop because to do otherwise seemed cruel. Another example of a decision was our need to consider the ethical implications of people’s unwillingness to engage in formality and paperwork (see the ‘Show on the Road’ vignette in the previous chapter), which led us to abandon confidentiality in favour of anonymity for most participants so they would not be excluded.

The third type, actions, relate strongly to the participatory process, in the sense that at each stage of plan, act, reflect and plan, an action is taken, something is done (and inevitably, other things are not done). In the context of the previous theme of negative space, this construct of legitimacy seems to help explain why some actions might be taken whereas others are not. Of course, legitimacy is not the only reason for actions to fall into the positive rather than the negative space, as identified earlier, other factors like risk and politics play their part, as well as mundane practical considerations like time and budget. The fourth category is a goal, the ultimate outcome against which something would be judged. In the larger sense, the whole project’s goals seemed to be arbitrated with reference to ideas of legitimacy: to aim only for reduction in drinking seemed illegitimate, both because it was unrealistic but also too simplistic. Whereas goals related to engagement and overall wellbeing seemed legitimate in the context of the project, but less so in the overall context, an issue I explore later in the section.

I don’t mean to imply here that whether something is deemed legitimate or not is a systematic or predictable process. Rather, legitimacy seems to be a unifying
construct that can help explain why, in the inherently social process of designing and managing a participatory project, some things happen and some things don’t, some experiments work and some don’t, some ideas are well received and others fall flat. In other words, legitimacy helps understand what goes into the positive and what into the negative space of a project.

In my data, there appear to be three intersecting sub-themes that can help explain the process by which a conclusion, decision, action or goal is deemed legitimate, or not. These are: who is making the claim to legitimacy and linked closely to this, the basis upon which the claim is made. The third sub-theme deals with the context in which they make the claim, by which I mean the question of insider-outsider status; this seems linked in turn to the context and physical location in which the claim occurs. An example of this latter point being the community researchers once again; would their findings have been judged more legitimate if they had presented them on their turf, where they were the insiders and the audience the outsiders, rather than the other way around?

![Diagram: How claims to legitimacy are judged]

**Figure 12: How claims to legitimacy are judged**

Once again, my choice to use words like “claim to legitimacy” to describe what I see happening suggests that this is an openly negotiated, conscious process; in most cases these claims and the judgements made about them do not seem to be made in this way, the evaluation of legitimacy seems to be tacit, even subconscious. Yet I argue that such judgements do occur and that they affect how a project proceeds, and consequently the knowledge it develops.
6.8. Pangs of conscience

Firstly then, when weighing up a claim to legitimacy, there seems to be considerable significance associated with who makes the claim. In our project, there appeared to be three broad categories of people that might make a claim to legitimacy: an insider (for example, a community researcher or a person living in Parkhood or Urbanwood, participant or not); an outsider (for example, a stakeholder, commissioner or researcher) and an academic. While the insider-outsider boundaries aren’t fixed (Sarah, for example could be placed in both insider and outsider categories), the category of academic appears to be rather a special case because it seems evident that academics’ claims to legitimacy are treated somewhat differently to the claims of other outsiders. Anna, on the other hand, was also somewhat of a special case. She seemed to travel between the categories with ease: she didn’t live in Parkhood and Urbanwood, but became accepted as a sort of honorary insider quite swiftly. I suspect because of her personal qualities and social skills, though Sarah’s advocacy probably helped. Anna was also, of course, an academic. The less socially skilled and physically present academic (me) was always an outsider in our project.

Figure 1: Who makes claims to legitimacy

There isn’t anything explicit in the project documents that would explain this difference between academics and outsiders more generally, but reflecting back upon the process, it seems likely that the claim of an academic is taken more seriously because we benefit from the legitimacy conferred by an association with a research process and further, such legitimacy appears to be granted
because of an assumption that ‘academic research’ is synonymous with ‘scientific’ (as in positivistic assumptions of objectivity, rigour of method and freedom from bias).

Of course, such claims to objectivity are not made in participatory research, yet my experience of the project and subsequent reflections tell me that I benefited from the power inherent in that objectivist discourse in successfully making claims to legitimacy. In other words, I conclude that it is this association with the scientific tradition, even though it is misplaced, that made the academic’s claims to knowledge more likely to be accepted than those of local stakeholders or people like the community researchers. But it was an illusory power; I didn’t deserve it.

The academic’s associations with the objectivist discourse of ‘research’ also seems to underpin the differing levels of legitimacy conferred by the basis upon which someone claims it, which can be divided into two categories: data or lived experience. The outsider (including the academic) seems most likely to base their claim upon data (by dint of their status as outsider, typically they have little or no lived experience anyway), whereas the insider tends to base theirs primarily upon lived experience. One interesting point about the properties of these two categories is that despite lived experience appearing to be a less powerful basis upon which to claim legitimacy, when it is combined with data as it was in the case of Sarah, it does not seem to detract from the claim; rather a combination of data and lived experience, an insider and an outsider perspective, seems to lend weight in a cumulative way.

6.9. Who’s game is it anyway?

The final sub-theme is the context in which the claim to legitimacy is made. In the data, there were two aspects of this: the first was the system, the structure, in which the claim was being made (an example being the commissioning system, another being the discipline of public health) and the other aspect was the physical surroundings in which the claim is located. These elements speak to the
question of whether the claimant is on their turf, both metaphorically and physically; are they playing by their own or by someone else’s’ rules?

Before elaborating on this sub-theme in more detail, it is relevant (and also somewhat uncomfortable for me to note) that in this project, the outsiders and outsider-academics tended to be of a more privileged social class than the insiders. There are a significant number of theoretical traditions that could help explore the relationship between context and legitimacy that appears to be present in this project, but my preference would be to draw briefly (in this section, though I will return to the topic in the conclusions) upon the work of Bourdieu, because his theory of social structure seems to encompass all the elements I have found in my analysis on this topic.

Bourdieu conceptualises what I have noted in my analysis as context as a ‘field’, which can be defined as the relationships between “the social positions which guarantee their occupants a quantum of social force, or of capital, such that they are able to enter into the struggles over the monopoly of power, of which struggles over the definition of the legitimate form of power are a crucial dimension” (Bourdieu and Wacquant, 1992, p. 229-230). A field therefore, according to Dick (2008), is analogous to a game; an association that is in itself interesting given my choice of language and metaphor to describe it in my narrative: ‘on their turf’ ‘playing by the rules’ ‘home or away’. Most significantly in the context of this thesis, is that the theory of fields helps enrich understanding of why some claims to legitimacy in some contexts were more successful than in others. According to Bourdieu, the explanation is founded in the relative capital of the different actors and their ability to deploy these both as part of their claim to legitimacy and to influence the process by which something is deemed legitimate (c.f. Dahl, 2013). It is possible that Bourdieu’s work could form a unifying theoretical framework for my contribution; I will explore this possibility in the next and final chapter.
6.10. My ethnographic self, Part I

Who says or does something, and when and how they say or do it, matters in a participatory project. It is part of the tacit and complicated process of whether someone is believed, whether their recommendations are followed, whether they are permitted to act and how their contribution is judged. This has both ethical and epistemic implications.

But before I can try to tackle these issues, it is important to reiterate Richardson’s (2000, p. 253) belief that “the ethnographic life is not separable from the self”. Increasingly, as I explore the themes that are emerging from my reflective data about the project, things that I have learnt outside of the project and the thesis writing are influencing my thinking, and reflecting on whether a researcher in public health or social marketing (however pure their motives and however good their intentions) should seek to legitimise a less powerful participant’s voice is one such issue.

The online feminist community I frequent is full of intelligent and articulate women learning and teaching each other about feminism. Recently, there has been an outcry about a leaked document from well-known human rights organisation Amnesty International, which appears to suggest that being able to purchase sex is a ‘human right’. During the robust discussion that followed, a poster known as Beachcomber (who has agreed to be quoted) wrote that “men speaking for women, is not men representing women, it is men oppressing women” (1st February 2014). This led me to wonder whether this same dynamic is present when researchers go into deprived neighbourhoods and then represent people’s lives to policymakers. Is this an empowering thing to do, because the views of local people are heard, legitimised? Or is it disempowering, because yet again, it is only a small number of privileged interpretations that are afforded legitimacy? Is what we did in Parkhood and Urbanwood advocacy or oppression? As Richardson points out, research and writing are complex political activities; ideological agendas are likely to be present in our writing and “speaking for ‘others’ is wholly suspect” (2000, p. 254; Spivak, 1988).
6.11. Viva la resistance

“...what we can study, how we can write about that which we study - is tied to how a discipline disciplines itself and its members, its methods for claiming authority over both the subject matter and its members” (Richardson, 2000, p. 253).

As soon as I began to write the previous chapter, it became obvious to me that something I named in my notes as ‘resistance’ was woven through both my actions on the project and the way I see the project now, in hindsight. I even resisted disciplinary convention in the way I have written this thesis, despite the concerns of my supervisor (who was, thankfully, willing to afford me the benefit of the doubt).

I resisted the issue led commissioning structures and resisted the overall knowledge system that underpins the positivistic progress towards predetermined ‘outcomes’. I resisted the conventional approach to social marketing outlined in Chapter 2: I did not aim to change individual behaviour among a particular target audience using all four Ps. What I did could almost be classed as outright rebellion against social marketing; in many ways I rejected it, stamped upon almost everything related to its neoliberal, managerialist philosophy.

When Richardson writes of resistance, she is referring to the constraints that she perceives to be imposed upon scholars by the structures and conventions of their home discipline. I started developing this section with the idea that I would write about the different ways in which the notion of resistance manifested itself in the data, probably organising this around sub-themes of resistance to a managerialist notion of control, resistance to the dogma of ‘what social marketing is’, resistance to positivistic notions of ‘what research is’. In fact what happened with this theme is I engaged in a whole new micro-study, a reinterpretation of the autoethnography to look for theoretical constructions of resistance, in the project and in myself, where resistance is understood primarily in the anthropological sense rather than in the Marxist interpretation of labour relations. In a context of differential power relationships, Seymour defines resistance as “intentional, and
hence conscious, acts of defiance or opposition by a subordinate individual or group of individuals against a superior individual or set of individuals” (2006, p. 305). To meet her definition, acts of resistance should be counter-hegemonic and should not be subconscious (which she would prefer to call “subversion”, p. 312), but they don’t necessarily need to be successful in effecting change. For me to conceptualise my behaviour as resistance in this way, I need to see myself as subordinate to more powerful others, in this case these are the academic establishment, my supervisor and particularly the examiners who possess the power to confer my doctorate upon me. Or to withhold it, of course.

6.12. My ethnographic self, Part II

I think I have always resisted. “Because I said so” has never been a compelling reason for me to accept anything. I have a clear memory of myself aged 16 or 17, sitting in my chemistry classroom on the hard, high wooden stool, arms resting on the venerable wooden bench, scarred by 20 years of student experimentation and petty vandalism. I was watching our teacher demonstrate a titration experiment, observing the colour change that indicates an acid turning to a base. I can remember thinking to myself, how do we know that this colour change means that all these ions are detaching and reattaching in different forms? All we can see is the colour changing. We just have to believe her when she says it’s true. But how does she know that it’s true? She can’t see either; like us, all she can do is believe the books, believe what her tutors at university told her.

Even earlier than this, it was chemistry once again that sparked my personal consciousness of what turns out to be postmodernist epistemology. For our GCSE science exams, aged 16, we were taught to regard the atom like a ball (the nucleus) surrounded by other balls (the electrons) whizzing round on little wires (the shell), rather like the planets round the sun. Moving to A ‘Level, we were told to forget all that, that was a lie, developed so that 16 year olds could understand it well enough to grasp the basic principles of chemical bonding. I suspect that the version we were taught at A ‘Level is a construction too. And the version that undergraduates are taught to describe in their exams. Knowledge
constructed for a purpose. A useful one, probably, but not a neutral, objective truth. And this was chemistry. Hard Science. On reading a draft of this chapter, Tim, my Director of Studies, asked:

“As a critical realist (I think that is what I am) I take the view that there is some kind of reality out there, but that reality is complex and the research we do to understand that reality often only offers a partial explanation and is subject to updating and new interpretation as new knowledge becomes available. Often we have to use metaphors as a way to aid understanding in terms that human beings can relate to. Hence I would interpret your chemistry example...as a metaphor to aid understanding rather than as a lie.” (T Hughes, personal communication, 26th March 2014).

I found this an interesting question to reflect upon. Where are the boundaries between truth and lie, when we draw so often on model and metaphor in to make ourselves understood and, particularly relevant in this thesis, to be evocative in our writing? Tim’s question enabled me to be more specific about what exactly makes me uncomfortable about the way I remember being taught chemistry. It isn’t that I expected the teachers to be able to tell us the whole and perfect ‘truth’ all the time. They wouldn’t have been able to, of course. What bothers me is the omission; the intellectual dishonesty (whether they are being knowingly dishonest to their students or unknowingly dishonest to themselves) in not explaining that the planets round the sun metaphor was a metaphor to help us understand a complex thing. These pretences, though kindly meant, contribute to the lack of epistemological nuance that underpins the general assumption that research equals objective validity.

In fact, when I look back on the process of writing this thesis, like the examples above from my school days, I will probably identify it as a transformational one not just in the conventional sense of gaining academic status, but in discovering the writings of critical postmodernist scholars, in particular Laurel Richardson. Reading the work of these accomplished and successful literary ethnographers has been akin to finding a group of like-minded friends, with whom I don’t have to conceal the way my mind grapples with a problem; I don’t have to resist anything. If I had begun this project knowing that this creative qualitative research is practiced, was valid in the sense that it exists as a tradition, I would not have
needed to struggle against disciplinary convention. If I had not found it, I would have missed the peace that comes with simply acknowledging subjectivities, of laying bare the frail humanity that underpins the multitude of little decisions and actions that shape a research project, participatory or otherwise.

So now, safe on my autoethnographic island, I look back on the stormy waters of the project and the early stages of writing this thesis and see clearly a process of resistance. There is no diagram to accompany this theme; just three simple subheadings to organise instances of what I perceive as resistance in the data: to the hegemonic managerialist notion of control, to the dogma of ‘what social marketing is’ and to positivistic notions of ‘what research is’.

6.13. **Neoliberal managerialism managed**

The whole project was rooted in resistance. Right from the start, we wanted to resist the conventional issue led approach of designing and managing health interventions on a topic-by-topic basis, “in silo” (to borrow a phrase from a project email, 23rd March 2012). This thought process, along with our failure to frame the project in the way we wanted, is documented in the previous chapter and there is no need to repeat these reflections here, merely to acknowledge an attempt at overt resistance against commissioning structures that appeared unsuccessful, in that we did not persuade the PCT to fund a cross-issue project.

There were more covert forms of resistance though, and these were more fruitful. Another early example recorded in the project documentation is my resistance against the assumption that we would achieve a measurable reduction in drinking. For a start, this was impossible because we were unable to determine where people lived from hospital admissions statistics; the best the statistician at the PCT could offer was the admissions data for the whole of the city, which was no help to us. But also, I knew that one short project would be very unlikely to make enough of a difference to people’s lives such that they reduced their drinking. As explained in the previous chapter, I drew upon the metaphor of an oak tree, positioning our work as planting the acorn and caring for it as it
emerged. The oak tree wouldn’t grow for 10, 20 years. This tactic of resistance via gentle diversion was successful in this instance. I think at least partly because it chimed with what the stakeholders knew to be true about the neighbourhoods and the people who lived there: that drinking was related to the more serious issues of disengagement and deprivation and therefore a simplistic solution stood no chance of making a difference. In fact I think it likely that arguing the converse, that the project would reduce drinking at population level, would have been more difficult.

Another way I exhibited resistance was through creating an illusion of control, of milestones planned and met, of logical progression towards a successful outcome. Throughout much of the project, it felt like I was fighting for the principles we had all agreed to follow and it took quite a toll on my personal resilience. At times, the temptation to stop resisting and actually take control in a non-participatory way, just to get to the end of the project without censure, was strong. But as explained in the previous chapter, I maintained an illusion of control rather than its reality.

Other examples of resistance to the managerialist hegemony were also about adherence to participatory principles, but rather than resisting these covertly by diversion or illusion, in these examples I resisted overtly, by arguing my corner. The first instance of this happening in the data was with the Parkhood Hub and my insistence that the PCT should not simply take the idea away and develop it in a non-participatory way. Linked to this was my determination that our ‘community champion’ scheme would be set up carefully, involving the right stakeholders, rather than rushed through as an isolated initiative with limited support. A small example of what I interpreted as resistance upon similar lines from Louise was her strong desire to commission the Timebanking organisation to run our ongoing scheme, which I assumed was at least partly related to a desire to have some independence from other initiatives happening concurrently, a desire to wrestle back some control.

But as the project unfolded, we were able to edge closer to our original vision. Can this development be characterised as resistance? Not entirely, because it
came about largely as a consequence of other changes to the overall hegemony in the PCT at the time, which were described in the Beware the Ides of March vignette in the previous chapter. But I think that it is fair to claim that our early, more covert resistance (for example, not being rigid about only including as participants people who met the age requirements set out in the commissioning specification) meant that because we had resisted the constraints of the old approach, when the hegemony of the PCT changed to one that was more supportive of our original vision we had crafted a project that suited the new direction. And the fact that we were now going with the flow of PCT strategy rather than against it meant the overt resistances had more chance of success.

6.14. Social marketing snubbed

As I showed in Chapter 2, models of behaviour change that assume people are individuals free to make decisions, which in turn are based on rational or semi-rational weighing up of positives and negatives, underpin much theory in social marketing. This is why exchange is one of the fundamental principles many social marketers rely on to structure their thinking about their work: make people an offer they can’t rationally refuse, and behaviour change is sure to follow.

Most of this participatory project was predicated upon a rejection of this notion, which I would characterise as overly simplistic. Or perhaps not a rejection per se, perhaps a belief that social marketers can’t (or won’t) acknowledge that the ‘offer’ they would have to make to the people I met in Urbanwood and Parkhood would be very expensive and impractical within the current hegemony. We would have to offer them a different life, one where they were not born into a family that lived in a deprived area, one where their parents valued education and encouraged them to attend a school where other children were keen to learn and behaved well, one where they had a fulfilling job and earned enough money to live in pleasant surroundings without worrying about making ends meet. I don’t think social marketing is able to claim legitimately to offer such an exchange.
And really, what I have just described is not necessarily how to empower people in deprived areas to live lives that they are happy with; it’s a recipe for how to be more like people with class privilege (c.f. Lindridge, 2012); in other words generally the sort of person that becomes a professional social marketer or academic.

I worry also that to assume that an exchange will stimulate a change in behaviour among people who are drinking to cope with what are in in many cases incredibly difficult circumstances, is at best naive and at worst harmfully dishonest. What if they don’t change? Can we blame them for that, given the structural factors that constrain their choices? Are we, in normalising this approach, contributing to the stereotype of people who live in deprived areas as “feckless” (Walters, 2012) “scroungers” (Sloan, 2010; Jones, 2014) too lazy and ignorant to make sensible decisions about their own lives? I fear that social marketing is not helping dispel this impression.

Autoethnography, as described by scholars like Ellis and Richardson, demands critical self-reflection, which can at times be an awkward and painful endeavour, as Davies (2005, p. 2) points out, “critique is risky work, not just because it might alienate those who are deeply attached to, or personally implicated in, the discourses to be placed under scrutiny”. With this in mind and while it is difficult to acknowledge this in a formal document, I can’t help but feel, I think, embarrassed to be associated with a discipline that makes such apparently unsophisticated claims about behaviour change. It seems small and petty and too concerned with its disciplinary status, with what’s in and what’s out of consensus based definitions (see also Spotswood, 2013). And yet big questions, philosophical questions, remain unanswered; I have significant reservations about our claims to legitimacy, for example. Social marketing is at the same time too bold and not bold enough.

As my work on this thesis has developed and my thinking refined, I have engaged in small acts of resistance, even defiance, like presenting a conference paper in 2012 that was knowingly and critically self-reflective about how we (mis)use the construct of empowerment in standard social marketing discourse
(Collins, Spotswood and Manning, 2012). Drawing upon theory from international development, my presentation used the project in Urbanwood and Parkhood to illustrate the mistakes we are making and their likely consequences. It went against the prevailing upbeat ‘social marketing can change the world’ mood of the conference, and some delegates who spoke to me afterwards were quite put out that I hadn’t offered them a story of ‘success’. Indeed, one even tried to reconstruct the story for me, suggesting that I should claim to have achieved an objective of ‘understanding’ rather than admit to failing to achieve an objective of ‘empowerment’. She appeared to think that I would find this interpretation reassuring. Other small acts of resistance include not answering “I am a social marketer” when people ask me what I do, removing the words ‘social marketing centre’ from my email signature and changing the hyperlink to my personal staff profile rather than one associated with social marketing.

One consequence of this particular personal resistance is that having rejected social marketing as a disciplinary identity; I am cast adrift. I do not know where I belong or where my skills and outlook on research would be welcomed and valued.

6.15. Positivism eschewed

Continuing the concluding point in the last sub-section, as well as setting myself free in disciplinary terms I have done something similar epistemologically. Or perhaps what has happened is that I have finally developed sufficient erudition to claim an alternative perspective with confidence.

But that observation relates only to my internal struggles to find a place that is comfortable intellectually, it has no bearing on how I navigate the wider research culture in which I find myself. As Boje (2001) points out, most business school academics (to which I would add public health professionals in particular and almost everyone who has not studied some form of social theory or theory of knowledge in general) are trained to follow a functionalist ontological and positivist epistemological stance. This means that in the minds of many people
with whom a non-positivist researcher will come into contact, research is synonymous with the discovery of “value-neutral, empirically verifiable facts” (Boje, 2001, p. 357).

It is this assumption that all research leads to empirically verifiable ‘fact’, free from any taint of value judgement or political motivation that seems to give the academic much of their perceived right to claim legitimacy, as argued in an earlier section. But I resisted this, both in the project itself and in the rather tortuous processes of, firstly, working out that if I wasn’t developing in this PhD a contribution to knowledge founded in what we had learnt about alcohol in deprived neighbourhoods (which could have been an option), what exactly was I doing? And secondly, in writing up this thesis in such a way that it would achieve the dual standards of a) avoiding unnecessary confusion to the reader by failing to follow accepted convention with things like the order of chapters and the writing style (or worse, suggesting to them that the underpinning scholarship was lacking); and b) remaining true to the actual process that I had followed to create the contribution to knowledge I present here; to tell a story that is authentic, rather than one that is sanitised with every trace of mess and uncertainty removed. I was keen to ‘own’ the methodology that I had followed, to stand by my work with pride rather than to ‘fake’ (as I saw it) a more conventional PhD by obfuscating the process through retrospective writing up.

There are two issues to deal with in this section then, the first being instances in which I resisted a positivism-inspired conception of ‘what research is’ in the project; and the second how I navigated my instinct to resist the conventional way of structuring and writing about a PhD.

In essence, I can argue that the whole project was a resistance to ‘what research is’ in public health, because I saw the entire project as wholly and completely research and have consistently written and spoken about it on those terms. Whereas in a more conventional understanding, the project would have been split into formative research (the ethnographic phase) then the development of some sort of intervention (not thought of as ‘research’), surrounded by more research to evaluate the intervention: a sort of research-intervention-research sandwich.
Other examples of resisting conventional understandings of research and its role included this issue of evaluation, which was rather more of a practical problem than an epistemological one in most cases. But there was the underlying feeling that the only way to really show that the project had achieved anything was to prove that drinking had reduced, and that the reduction was due to the project and not confounding factors. This would have been largely impossible anyway due to all the other things happening locally, like the ABCD project and the ‘My Neighbourhood’ grant. But in fact, this was one area of the project where I felt resistance would be futile, so I designed a short and simple pre/post survey asking questions about drinking behaviour, community engagement and wellbeing. In my mind, this survey was unlikely to show any statistically significant differences anyway, because the changes in drinking behaviour would need a long time to manifest themselves. In fact, there were small differences in Urbanwood, which I thought at the time were more likely to be related to a growing feeling in that neighbourhood that things might actually get better this time, largely because of the symbolic value of the £1 million grant. In a way, I suppose, developing this survey was similar to the illusion of control I described in the section on resisting managerialism, only this time it was an illusion of scientific evaluation that was being created.

Another example of resisting the traditional conception of ‘what research is’ in public health can be found in my need to emphasise that just because the research had ‘found’ something, didn’t mean that just implementing it as a ‘solution’ was going to be successful. I think this assumption comes from the conventional approach of doing formative research to understand the issues, and then designing some kind of remedy that is implemented separately from the people who will use it. I resisted strongly the idea that this would work in the neighbourhoods, largely because of emotional and political factors such as local people resenting the power to impose things upon them that remote others possess. Conventional health services research, it seems to me, prioritises fact over politics.

Another small example is the use of incentives. Orthodox approaches to formative research do typically rely on an incentive of some kind to encourage
participation. But we resisted the temptation to offer these, partly because we felt that this would create a transactional feel, but epistemologically, the use of incentives suggests to me that the information gained is separable from the person providing it, so the extent to which participants feel that they are valued as individuals with something to offer, rather than people who have been paid for what they know and then sent away, will affect the quality and depth of their engagement with the issues. In essence, I think all these relatively small instances of resistance to ‘what research is’ can be grouped as a resistance to the traditional power dynamic inherent in research, where the researcher holds the power, not the participants. We tried to operate differently.

The second form of resistance identified in this sub-theme was that which I showed towards conventional approaches to doing PhD research and writing a PhD thesis. Others have written of their struggles with unconventionality in this context as well. Laurel Richardson, as mentioned earlier, writes generally about illegitimacy in a disciplinary sense; and more specific to PhD research, there is Julie Davis, who invented for her thesis an alternative structure to try and more accurately represent the process of knowledge creation she followed. Rather than the conventional separate, sequential chapters starting with a review of extant literature, findings and “interpretations detached from accounts of the research process” (Davis, 2004, p. 13), she aimed to show the ways in which processes and outcomes were intertwined by intermingling accounts of action, reflection and literature in her thesis in the form of reflective “cycle” (ibid.) chapters.

Her examiners, unfortunately, were unimpressed with her approach. She describes the first examiner as a researcher knowledgeable in action research; this academic felt that there was foundation for her desire to present her work unconventionally, but he felt that the intermingling of literature and action interrupted the flow of the story in a detrimental way. Her other examiner was less sympathetic overall, advocating a more conventional approach simply because, in Davis’s opinion, he had “missed the point” (p. 17) of what she was trying to achieve.
According to my research diary, in October 2013 I realised that my ambition for the thesis was:

...to “sort out” all the different literatures that dealt with the topic of co-creation: SDL, participatory research, co-design, ABCD, that sort of thing. They all seemed to be saying similar things without reference to one another.

Consequently, I found it acceptable to start the thesis with literature review chapters that framed the thesis in line with my own starting point for the actual research, a process of “sorting out”. The methodology chapter that came next helped me to realise that I was not in fact ‘doing’ participatory research; rather I wanted to study it. But I couldn’t study it as Davis had done, knowing from the beginning that I was concurrently doing action research and studying it. It had already happened, I didn’t have the right sort of data and didn’t want to imply that I had. So I started to read about reflection, which led quickly to autoethnography, which provided an authentic framework from which to look back on past events and write about them to generate new knowledge about them. The chapter that tells the story of the project and this one flowed ever more smoothly from the autoethnographic framework. And thus there is really no need to present the thesis in an unconventional structure. As a result of my exploration of resistance against ‘what research is’ in the context of a PhD thesis, I conclude that rather than choosing actively to resist conventionality, I found no particular need to do so.

In overall conclusion for this section, I have found broadly four strategies of my own resistance in the project and subsequent work on the thesis. These I had named confrontational, diversionary, illusionary and unconventionality.

Confrontational strategies include directly addressing whatever it is I want to resist, whether by presenting arguments or making statements of belief or intent.

Diversionary strategies include avoiding direct confrontation by gently directing the focus in other, more desirable directions or using metaphor to suggest compliance when really, resistance is occurring, such as the oak tree.
Illusionary (or performative?) strategies include creating the impression that something is happening when in reality it is not. It is important to note that this does not encompass outright untruth, more creating an impression of, for example, logical progress towards agreed milestones while knowing that those milestones are not as fixed as the illusion implies.

There were hints at resistance among Parkhood and Urbanwood residents in the field notes as well; though as the project wasn’t framed in these terms, I suspect that we will not have recorded or even really noticed much of the resistance in which they engaged. Briefly, examples included people finding their caring responsibilities overwhelming and behaving (in their eyes) badly because of their frustration. Avoiding social services even though they needed support, avoiding asking for help generally so that they stayed off the authorities’ radar, reluctant to engage, being unwilling to trust. Not turning up to things, even though they had agreed to. This area of resistance to health intervention and even health related research is one that would benefit from some attention.

6.16. Empowerment of the powerless?

Sadie: So are you arguing that this empowerment of the powerless is simply an illusion? A performance, maybe? But doesn't actually do anything in terms of a real transfer of power...

Katie: I like the idea of empowerment as a 'performance'. I am going to explore that I think. Thanks!

Sadie: You're welcome. It comes from my experiences within the youth and community work sector. Empowerment is bandied around a lot but you get the impression they are only talking about it and performing it in order to get funding. The people who work at the grassroots see the irony of it, but they have to play ball: "Empower" the kids and we'll give you more money. Nothing ever changes. (Extract from Facebook chat, 28th November 2013).
My friend Sadie has a job doing pioneering participatory work in film production. Her suggestion about conceptualising what happened in the project as a performance of empowerment turned out to be a very helpful one, not least because there were other examples that could be constructed as performance or indeed performativity in the project.

The notion of performance as a theme stems from this conversation with Sadie, but the idea that something was amiss in how we were using the term empowerment in fact came from the conference I describe in the previous section on resistance. That conference paper (Collins, Spotswood and Manning, 2012) was the first piece of truly reflective writing I did about the project in Urbanwood and Parkhood. Ostensibly, I wrote in that paper, participatory methods like those to which we attempted to remain true in the project offer a promising way to empower “surplus” (Hickey and Mohan, 2005, p. 239) or “at-risk” populations (Pechmann et al., 2011, p. 23), like people who live in deprived neighbourhoods. However I noted that this premise is not uncontested; that criticism that participation has failed to achieve meaningful change had been mounting (Cook and Kothari, 2001). The problems I identified from a review of the international development literature included inattention to issues of power and politics (Hickey and Mohan, 2005) coupled with an unsophisticated understanding of the mechanism and constitution of power (Mosse, 1994; Kothari, 2001); an overemphasis on local concerns to the detriment of structural inequality (Mohan and Stokke, 2000) and a conceptualisation of the relative functions of structure and agency that is inadequate (Cleaver, 1999). Finally, I argued that mainstream participatory approaches are too voluntaristic in regarding any form of participation as superior to non-participatory practices (Chambers, 1997) without considering the risk that those with disempowering agendas may adopt (or co-opt) initiatives that serve their purposes (Rahman, 1995). I contended that that these considerations should be of grave concern to social marketers dipping their toes into the turbulent waters of participatory methodologies.

What I had not realised at the time that what I secretly worried was a pretence of empowerment could actually be better described as a very convincing
performance. So convincing, I think, that many ‘empowerers’ have even convinced themselves.

But arising out of my reflective writing about the project were other things that could also be constructed as performative; as well as empowerment, there was a performance of control and perhaps even a performance of legitimacy (exemplified by the need to design a conventional form of evaluation in order to satisfy the need to ‘prove’ that the project had achieved its ‘outcomes’).

Performativity is a term that has come to be associated closely with feminist scholar Judith Butler, who characterises it essentially as “fabrications manufactured and sustained through corporeal signs and other discursive means” (Butler, 1999, p. 173). She goes on to suggest that if the gendered body (the subject of her analysis) is performative, “it has no other ontological status apart from the various acts which constitute its reality” (ibid.).

However, performativity could simply be another way of looking at the phenomenon in the last section that I characterised as resistance through illusion; for example in the project, these illusions were related to the hegemony of neoliberal managerialist control. As Davies explains:

“In speaking ourselves into existence as academics, within neoliberal discourse, we are vulnerable to it and to its indifference to us and to our thought. It can become the discourse through which we, not quite out of choice and not quite out of necessity, make judgements, form desires, make the world into a particular kind of (neoliberal) place.” (2005, p. 1).

But really, this theoretical perspective suggests that more could be happening than simply an alternative nomenclature to the types of resistance I identified in myself during the course of the project and latterly, writing this thesis. It can be argued that Butler’s theories of performativity and subversion could in fact present an alternative lens to the theme of resistance I elucidated in the previous section. This would be the case if I were to conceptualise the system against which I identified my own resistance (composed of public health commissioning, social marketing dogma and positivistic notions of research) as neoliberal hegemony, as Davies evidently does. So rather than managing managerialism by playing along, as theorised earlier in this chapter, am I actually performing it, and
thus making it tangible? This question of the individual’s role in performing, or indeed resisting, neoliberalism or any other ideology is explored in the final two chapters.

6.17. Concluding comments

When I wrote in the introduction to this chapter that my thesis seems to have become “a multi-layered study of a method within a method within a method” I did not think that I would end here, with a thesis that, at its core, is about things that are hidden, tacit, taken for granted; about underlying structures that govern and influence how we judge people and information and how these translate into the actions we take, that is concerned as well about my own academic identity, my place in the world. I have arrived at this point via the notion of negative space, which seems to me to stand alone as valid in and of itself, particularly in relation to action research projects where the process is not mapped out at the start. In contrast, the other themes seem to be concerned less with the role of participatory methods in public health and more with finding an identity for myself, finding my place among competing structures and ideas.

My themes of legitimacy, resistance and performativity are concerned with the sort of projects I want to work on and how they can be done ethically, so that as academic researchers we work against exploiting people, stealing their stories or treating marginalised groups as ‘other’. This involves some difficulty when they are funded by public health because of the positivistic and managerial hegemony; the thesis tells the story of this difficulty, this conflict, between that system and me. The conclusions and contributions to knowledge about social marketing practice that I articulate in the next chapter represent sites of conflict from which seems to be emerging strong beliefs about what we can claim to know, which I am reasonably comfortable with; and my disciplinary identity, which I am not; some broader implications and nascent contributions are explored in the final chapter, along with the usual reflections and assessment of the study’s limitations.
Chapter 7: In which I go back to the beginning

“What do you mean by “If you really are a Queen”? What right have you to call yourself so? You can’t be a Queen, you know, till you’ve passed the proper examination. And the sooner we begin it, the better” (The Queen, Through the Looking-glass and what Alice Found There, p. 70/1).

The sort of telescope I had in mind for my metaphor is called a Galilean telescope, which is of the refracting type. Refracting telescopes are optical telescopes that consist of two lenses connected by a tube. The objective lens is convex, the eyepiece is concave and the tube needs to hold the two lenses the right distance apart so that the eye can focus on the magnified image the system produces. But because the point of focus lies inside the instrument and the eye cannot be placed there, it has to be placed as close to the ocular lens as possible, thus restricting the field of view (The Penguin Dictionary of Physics, 2009). But when you turn a refracting telescope around and look through the objective lens into the ocular lens, things that once seemed very familiar (like how far away your own feet are when you look down at them) now appear strange and distant. Writing this thesis, for me, has been like turning the telescope around. My thinking has changed from an external but narrow focus on critiquing social marketing as a social change technique to asking some very searching questions about how I want to define myself as an academic and what sort of research I feel I ought to conduct. Consequently these concluding chapters feel like the start of a new approach for me. What can I claim to have contributed to knowledge? What do I have to show for these last 18 months of writing? What will happen when I turn the telescope back and look outwards through the ocular lens again? What will be my focus?

I aim to explore these questions through two different forms of writing (Richardson, 1997) in which I attempt consciously to show as well as tell. There are two characters (Richardson and Lockridge, 1991) in this piece of writing; I am both of them. Through the objective lens, in this chapter, I write as I have been...
taught to write, following disciplinary convention, telling how the findings of my autoethnography challenge and develop existing theory in the discipline in which I began: social marketing. Through the ocular lens, in Chapter 8, I show in more experimental forms of writing how each aspect has challenged and changed me, as a researcher, and explore what I want to do with this new consciousness.

In previous sections, I have said that my aim in this thesis is to critique and reconstruct the ways in which language, discourses and power relations are not typically acknowledged in conventional positivistic, ‘evidence based’ modes of commissioning, implementing and evaluating social marketing initiatives, particularly when asymmetric power relations are obviously present, as with interventions in deprived neighbourhoods. In this Chapter I draw upon my four themes of negative space, legitimacy, resistance and performativity to argue the following conclusions: that social marketers may use language to reinforce an assumed consensus of a paternalistic interpretation of social good, which draws its authority from the perceived legitimacy of the speaker; that politics and ideology are manifested in the discourses of social marketing and, related to this, that social marketing in turn may be deployed ideologically and finally, I discuss the consequences of erasing the social marketer themselves, the wielder of the tool, from the process through the language we use. I close by synthesising these points into the literature on social marketing ethics, generally and, with more confidence, specifically in in relation to the idea that increasing participation can be a solution to the ethical dilemma of how to articulate a definition of the social good.

7.1. The authority of princes

“When I use a word,” Humpty Dumpty said in rather a scornful tone, “it means just what I choose it to mean - neither more nor less” (Through the Looking-glass and what Alice Found There, p. 46).

In their 2011 book, French, Merritt and Reynolds claim that “the words matter less than the application of the principles” (p. 13), which they consider to be the
use of a finite set of concepts in a systematic process defined by learning and evaluation, aiming towards the social good via focus on specific behavioural objectives (see also section 2.1). In other literature, social marketing has been described in similar terms, as a toolbox, technique or technology (Hastings and Domegan, 2014; Andreasen, 1994; Hastings, MacFadyen and Anderson, 2000) or a process (Lee and Kotler, 2011; French and Blair-Stevens, 2010; Kotler, Roberto and Lee, 2002).

One consequence of this particular construction of social marketing as, essentially, a neutral instrument (Dann, 2007) is that the language and related assumptions are allowed to drift along largely unexamined (Wymer, 2011; Brennan, Voros and Brady, 2011; Brenkert, 2002). And yet as someone who thinks that knowledge is socially constructed, I cannot but assume that underlying assumptions there are. One of the biggest, in my opinion, is the belief that people in academic or public service roles, those people with significant claims to legitimacy who believe that they are working towards social good, must automatically be working towards the social good. But why does their opinion count the most? What gives them the right to decide what the legitimate version of social good is? The people in Parkhood and Urbanwood didn’t get a voice in the debate about what the social good for their neighbourhood might look like, or at least they didn’t get a voice on their own terms.

There are strong parallels here with the international development practices discussed in Chapter 3: in that example, Western ideals of what it meant to be a ‘developed’ nation were imposed on other cultures felt to be ‘developing’. In the case of social marketing, it could be argued that people from relatively privileged backgrounds are guilty of a similar degree of false consciousness, of assuming that everybody should aspire to share their own ideas of what is best; ideas which are, it could be argued, “culturally and geographically contingent phenomena that should not be uncritically subscribed to or exported throughout the globe” (Tadajewski and Brownlie, 2008, p. 2). But, like in the preface to this thesis when I described a very enlightening conversation about differing ideas of what it meant to be a good parent in different contexts, I argue that individual subjectivities mean that ‘social good’ is likely to have a different meaning for
different people and further, that some versions are likely to be judged more legitimate than others. This is an issue that has received surprisingly little attention among social marketers, with only one conference paper engaging explicitly with it (Lindridge, 2012).

To acknowledge subjectivities is not to say that social marketers have no concern with ethics, an issue that is expanded upon later in this chapter, but that its evident functionalist underpinnings have led to an unchallenged assumption that there is “a unity of interest among members of social systems” (Boje, 2001, p. 357), i.e. this assumption of a universal ‘social good’ towards which we all work. With some notable exceptions (e.g. Gerard Hastings’ stinging critique of corporate dominance, 2013; Lefebvre’s ongoing interest in the TMA) and acknowledgement of contextual issues like health inequality, social marketing doesn’t appear to hold as a foundational premise that there is something wrong with the status quo per se, at least not in the same way that orientations underpinned by critical theory do. Indeed, it has been argued that social marketing is a neat ideological fit with activities of organisations like the World Bank and the IMF, whose “literature extols the multiple virtues of the private sector’s ability to reach target communities more efficiently than state services to change the presumably harmful health behaviors of the poor” (Pfeiffer, 2004, p. 78). But as described in section 3.8, these corporate inspired, Western development policies have not been without their problems, not least among them that they haven’t always achieved the changes they promised.

The strength with which we cling to the idea of a unified perspective is still apparent. Examples include the TMA, which is predicated upon privileged groups knowing what is fair and just; libertarian paternalism (Thaler and Sunstein, 2008), which embraces this idea almost gleefully; and in the debates about whether commercial organisations have a role in promoting social change (see Wood, 2012, for a review; also Beall et al., 2012; Gordon, 2011). This largely unexamined construct of a universal ‘social good’ stands in contrast to explicitly critical approaches, such as Freire’s conscientização, Denzin and Lincoln’s call for “critical conversations about democracy, race, gender, class, nation states, globalization, freedom and community” (2005, p. 3), activist scholars like...
Chatterton’s open allegiance to “global justice” movements (Chatterton, 2008, p. 424) and, closer to home, critical marketers (see Tadajewski, 2010, for an extensive review). The contrast I perceive comes not from the assumption that these perspectives have espoused worthier versions of the social good, merely that the underpinnings of all three have been articulated more fully and, more importantly I would argue, more critically; acknowledging subjectivity.

A counter argument made by some of my colleagues is that more explicitly principled approaches like those identified above may make their practitioners “feel good” (Spotswood et al., 2011, p. 167) but such philosophies struggle to achieve their goals. And as outlined in Chapter 3, participatory and critical scholars do struggle with the problem of disconnection between emancipatory knowledge and actual social change. Social marketing’s “practical realism” (ibid.), on the other hand, leads proponents to consider any ethical technique with evidence of effectiveness (Gordon, 2011). When they and other social marketers write of ‘effectiveness’, Spotswood et al. (see also Helmig and Thaler, 2010; Stead et al., 2007; Truong, 2014) appear to mean whether the chosen technique has achieved the specific objectives it set out to achieve, not whether it has succeed in bringing about large scale social change. I expand upon this utilitarian understanding of ethical decision making later. But I can’t help but notice that this pragmatic argument shares similarities with Machiavelli’s notion that

“...in the actions of all men, and most of all of Princes, where there is no tribunal to which we can appeal, we look to results. Wherefore if a Prince succeeds in establishing and maintaining his authority, the means will always be judged honourable and be approved by every one” (Machiavelli, The Harvard Classics 1909, p. 34).

Authority albeit constrained by the need to reach “a balance between our power to change people and the legitimate demands of ethical transparency” (Spotswood et al., p. 167).

They probably don’t mean to imply that provided the objectives are set by the person with the most authority, achieving them will always be judged ‘honourable’. So what might these concerns over the legitimacy and authority of social marketers and their funders mean in the context of this study? It would
seem outlandish to suggest that an intervention that persuaded people in a deprived neighbourhood to drink less alcohol could be anything other than for the social good, both for the people concerned and for a society that would otherwise foot the bill for the costs of their health and social care. Could it? And yet, drinking was so bound up with structural inequality that individual behaviour change, unaccompanied by significant social and political transformation, was an utterly unrealistic goal (Collins, 2014), which would explain why I spent so much energy on resisting this narrowly behavioural focus throughout the project. Given this assertion, I find myself wondering whether spending relatively small amounts of money here and there with a positivistic focus on measurable behaviour change objectives, helpful as they might be to those who wish to prove social marketing’s effectiveness quantitatively (Truong, 2014) and consistent as they are with ‘evidence based’ (Killoran et al., 2012) approaches, could be considered wasteful and demeaning to a ‘target audience’ subject to such significant structural constraint. Wasting public money and demeaning the target audience do not sound socially desirable to me. In fact I do not think that the money was wasted or the target audience demeaned in the case described here, but this outcome was only avoided because of the considerable efforts put into stakeholder relationships and the genuine desire to adhere to participatory principles (which is not without its own issues, as outlined in Chapter 3). And the good fortune that other, better funded and longer-term development initiatives took the reins when we ran out of time, of course. Nothing inherent in the social marketing process was responsible.

In conclusion then, social marketers have argued that they are well placed to address the deficiencies of commercial marketing (Kotler and Zaltman, 1971; Hastings, 2007; 2013), but have perhaps neglected to turn this critical lens upon themselves (Tadajewski and Brownlie, 2008), in particular upon social marketing’s legitimacy and authority to claim to be working towards a functionalist-derived uncritical consensus of the social good. It seems that words may matter, after all.
7.2. Speaking into existence

Taking up the challenge offered in the previous section might be one way to articulate a contribution to knowledge from this thesis. Attempting to remedy Tadajewski and Brownlie’s concerns by turning a critical lens on social marketing; asking what if, instead of being defined as a neutral process or a tool, social marketing were to be thought of as a methodology or orientation towards social change, similar to the way that participatory research is conceptualised? If social marketing were to be retheorised as something with epistemological and political significance in and of itself, then a number of underlying assumptions would be subject to scrutiny and constructive critique.

As explained previously at length, much of the social marketing literature is concerned with evidence for effectiveness; application of behavioural models developed with positivistic underpinnings. Consequently, according to Guba and Lincoln’s (2005) typology, a social marketing orientation, whether drawing upon participatory methods or not, seems to correlate to an understanding of a single reality that is real and perfectly (or imperfectly) apprehensible, findings that are true (or probably true), a strong preference for quantitative methods in a search for cause effect links, facts and verifiable laws. Knowledge is accumulated like “building blocks adding to the edifice” (p. 196) and judged using conventional criteria like validity (Brennan, Voros and Brady, 2011), reliability and objectivity. The social marketer is a “disinterested scientist” (Guba and Lincoln, 2005, p. 196) whose work informs policymakers; a tool wielded by Princes (those with the authority to set objectives and the funds to pay for an intervention). It is assumed that personal values, politics and ideology are thus removed from the knowledge presented to the world and the results of the intervention.

But it has been argued, probably because of social marketing’s positioning as a neutral, scientific, pragmatic process, that social marketers “may be used by an unjust system to protect itself from challenge” (Brenkert, 2002, p. 21). For example, in the context of the UK Coalition Government’s public health policy, which promotes personal responsibility and privileges individual and corporate
freedom (Department of Health, 2010), commercial organisations are to be encouraged to use their resources and creative capabilities to tackle lifestyle diseases. However, as the British Medical Association (2012) points out, there is an obvious conflict of interest in allowing organisations that profit from unhealthy behaviours to assume any responsibility for public health policy. Undoubtedly, there will be a temptation for organisations that rely on the purchase of alcoholic beverages for their livelihood to protect that livelihood, thus there is a risk that a system that facilitates risky drinking will be perpetuated rather than challenged (see also Rahman, 1995 and Chapter 3 for analogous issues in the International Development literature).

Thus I would argue that while an individual social marketing intervention may be designed according to positivistic principles of objectivity and neutrality within itself, politics and ideology are present in the wider context nonetheless; especially now that some social marketers are arguing that they should cast off their humble tool-like status in favour of a seat at “the top policy and strategy development table when social interventions are being conceived” (French, J, personal communication 08 May 2014). These interventions are activities that aim to influence “the behaviour of citizens in relation to important activities… which are politically defined” (Raftopoulou and Hogg, 2010, p. 1209). This is particularly concerning given recent examples where social marketing has been deployed in ways that are overtly political; for example, anti-immigration (Elkin, 2007) and promotion of American values to Muslim people (Klein, 2002).

Consistent with the argument presented above, politics are present in all four of my themes, which were developed from a project that was, ostensibly, social marketing: politics and power dynamics were present in negative space, delineated as it was by which voices were heard and which were shut out, in which ideas were acted upon and which rejected. They were present in legitimacy, again of voice and of outcome: in the observation that whether an individual’s views were heard depended upon whom they were and the perceived authority of their position. In the way that I could identify my own personal resistance to the positivistic hegemony of conventional approaches to social marketing and public health more generally and in the way that I could identify
and critique what all too often appears to be a performance of empowerment and a performance that the process was under control. Taken together, these four themes represent what might be called ‘internal’ and ‘external’ politics, politics within the project and in its wider context.

Social marketers are also concerned with the politics of their own survival, with pleas to stop arguing over definition and instead concentrating on ‘proving’ (there’s that positivistic underpinning again) social marketing’s value to policymakers (Wood, 2012) and carving out a space between other disciplines (Andreasen, 2002). Calls to “build on the success of the field to date” and “aggressively market and promote the proven value of social marketing” (Beall et al., 2012, p. 111) appear to be mounting. An upcoming conference will aim to “showcase examples of good practice and debate how social marketing can be further developed in Europe” (World Social Marketing Conference, 2014). Social marketers worry about ‘brand competition’ from other behaviour change techniques, such as health promotion, enter-educate programs (Piotrow and Coleman, 1992) and more recently, behavioural economics (Dibb and Carrigan, 2013), which are perceived by some social marketers as threats. There are some significant exceptions, social marketers who argue in favour of a multidisciplinary approach (Collins, 2014; Spotswood, Unpublished); but there is also jostling for recognition and status for social marketing, which sits uneasily, in my mind, with the rhetoric of ‘the social good’.

If I am to define social marketing as an inherently political activity, one significant question relates to its legitimacy as such. In previous chapters, I chose to define the idea of legitimacy by drawing upon political science as a “belief in the rightness of the decision or the process of decision making” (Dahl, 2013, p. 46). I identified four occasions upon which something might undergo a conscious or, more likely, subconscious weighing up of its legitimacy: The first type was a conclusion; the question of who gets to claim knowledge of something; of who gets to be believed. The second type was a decision, to take a particular course of action, to allow something to happen or to react in a certain way should something happen. The third type was an action and the fourth a goal, an objective against which something would be judged. The latter is probably the
most significant form of legitimacy to examine here, partly because of social marketing’s pragmatic and utilitarian underpinnings outlined in the previous section (that it is judged ‘good’ if it achieves its objectives) and partly because it is the objectives that are most likely to be politically significant. A directed review of the social marketing literature reveals no explicit recognition of the issue of legitimacy, though there is some general exploration of ethical issues, to which I will return in a moment. There are however a few references to the notion of legitimacy in the critical marketing literature.

Marion (2006) links legitimacy with ideology, pointing out that two broad interpretations of ideology exist: the first a Marxist interpretation, which has ideology as “set of illusory beliefs and a ‘false consciousness’ by which the ruling class maintains its dominance over the working class” (p. 246). Under this interpretation, a marketing ideology has manipulated our culture into a consumerist one, largely to our detriment (see Ewen, 1988). And social marketing doesn’t look squeaky clean from this perspective either, largely because of the lack of acknowledgment of the political issues and subjectivities. However, social marketing’s underpinnings and vocabulary provide a significant clue as to its underlying ideology. “Social marketers are in the business of behaviour change” (Wood, 2012, p. 96, emphasis added). “Consumer behaviour is the bottom line” (Andreasen, 1995, p. 14, emphasis added). Such language indicates in the most basic sense that social marketers have taken, largely uncritically, the modernist, neoliberal ideology of market capitalism and translated it directly to the idea of social change. This interpretation appears to resonate strongly with the concerns I have identified throughout this thesis regarding the lack of awareness of the power dynamics inherent in the social marketing process (to name a few examples, issues of unacknowledged subjectivities linked to privilege, the legitimacy afforded to outsiders assumed to be objective and the protection afforded by an assumed consensus of the social good). I could conclude that social marketers subscribe to illusory beliefs about the goodness they are doing and false consciousness about their neutrality. More bravely, I could claim that social marketers prop up the power of an elite political class by helping to
legitimise, as cost effectively as possible, the blame attributed to less fortunate people for the social and economic structures that disadvantage them.

Marion’s second interpretation of ideology is based upon the work of Louis Dumont, who conceptualises it as collective representation composed of an array of social phenomena, including social norms, values, beliefs, symbols and customs: a shared intellectual heritage and common understanding of the rules of the game. For marketing, the significance of this interpretation is that collective representations and shared beliefs about marketing forms one of three layers; the other two being marketing practice and marketing as a branch of knowledge. For social marketing, most thought has gone into the practice layer, very little into the epistemological or ideological layers. Which brings the discussion back to the same problem: the consequences of a lack of critical reflection upon the epistemological and ideological dimensions of social marketing. Drawing once again on the work of Bronwen Davies, it seems that social marketers are speaking and writing ourselves (themselves) into existence within neoliberal discourse (2005), without being aware that this is what is happening. False consciousness again: thinking that we are doing good when in fact we are unthinkingly helping to perpetuate an unfair system.

This observation about social marketing’s political and ideological dimensions leaves me (and others who share my concerns) broadly with two options: either reject the discipline or seek to develop it somehow. I will address this question in the concluding chapters.

7.3. Speaking louder than words

My observations about negative space in the previous chapter call to mind a brief conversation I had with an experienced and well-respected professor, after I had given an informal presentation of the project described in this thesis. Our discussion centred upon the degree of input researchers (or public health professionals) should seek to have in a participatory process. His view seemed to be that all the ideas should come from the participants. Mine was that the
process should be collaborative, with both ‘sides’ making contributions where they could, but with the researcher working to eschew their status as privileged knowledge producers. Previous sections have examined the context in which social marketing strategies are commissioned and judged; this section looks inwards, to the role of the social marketers themselves.

Reflecting once again on the idea of negative space, it seems obvious to me that even if a researcher (or social marketer) intends to work in a fully participatory way, they still maintain considerable influence. Even if they try not to, they are still present; for one thing they have initiated the process, it would not have started without the catalyst of researcher or funder. The researcher is the one who knows what is supposed to happen, who understands the ‘spiral science’ of action research, who is aware of the arguments for participatory approaches. At a more prosaic level, the researcher tends to be the person who organises the events, books the venue. In the specific example of this project, it was the research team’s energy that drove the project forward and it was perhaps our interests and goals that decided where that energy was expended, what battles were worth fighting. In this project, the researcher’s voice seemed to carry a great deal of legitimacy and the researcher was the one who fronted the ‘performance’ of participation and empowerment. The researcher was the person who resisted.

On another level, a researcher working on a social change project might become more than involved; they might become active, even activist, in their contribution. It seems reasonable to conclude that during the course of the project Anna and I become advocates for the neighbourhoods; we saw and heard for ourselves how people felt trapped, abandoned. We were on their side; we wanted change. If we accept this argument that the researcher is always present and therefore always exerting some influence, then the discussions over what that influence might be and how it manifests itself becomes very important.

As explained in the previous sections, social marketing is written about as a tool to be wielded, a game to play (Bloom and Novelli, 1981; Goldberg, 1995; Andreasen, 2012). All the emphasis is placed upon the puppets, the scenery and
the audience, with very little on the puppeteer and this reinforces once more the argument that at core, social marketing has taken much of its inspiration from positivistic paradigms. The absence of an embodied researcher (or social marketer) in that literature implies that it doesn’t matter who the researcher is, what they want, what they believe, because they are neutral, objective observers following a process. That because objectively observed behaviour change is the goal it doesn’t matter what goes on in the researcher’s mind, provided they follow the protocol. I think that this lack of acknowledgement of the role of the researcher (or social marketer) means that important questions about social marketing’s legitimacy and ideology are not being posed. Such scrutiny of the role of a researcher is known in the methodological literature as positionality (see Chapter 4), an idea that acknowledges the researcher’s position as a member of the social world under study (Anderson, 2006). But social marketers do more than simply study the world, they are active change agents and so also relevant here is the notion of the activist scholar.

From the post-structuralist perspective I have adopted latterly, the researcher is never neutral, never detached from the research. And within this perspective, I would argue that the social marketer, like the activist scholars I discuss in Chapter 3, is never detached from their intervention. This question relates to epistemology, but it occurs to me that the role of the researcher, social marketer or activist will depend on what underlying theory of change is being relied upon. In social marketing, that theory of change is predominantly psychosocial theories of behavioural change, as I have explained in earlier chapters. These theories remove individual subjectivity from consideration and assume that provided the process is followed correctly, the researcher or social marketer will always be neutral and thus the outcome will be unaffected. Conversely, activism can be understood as a much broader spectrum of change, from rethinking social relations through to street demonstrations, including choosing to work on projects that align with the activist’s goals and beliefs about the sort of society they want to create. “Activism thus defined carries its own contradictions, as the committed activist makes choices about how much time, energy and emotion to commit to
social change; and as she attempts to manoeuvre the complex political currents through which social power is channelled” (Kobayashi, 2003, p. 346).

7.4. A contribution in conscience

In much of this chapter so far, I have been critical of current social marketing thought and practice, albeit with the recognition that social marketers believe that they are doing good and seek to behave ethically in their work. In this section, I aim to show how the findings of my autoethnographic study contribute to the body of literature that seeks to help social marketers make more ethical decisions; generally and, with more confidence, specifically in relation to the idea that increasing participation can be a solution to the ethical dilemma of how to articulate a definition of the social good (Andreasen, 1995; Brenkert, 2002).

According to Smith (2001), in his introduction to one of the few books about ethics in social marketing, ethics is about making decisions. To elaborate, these considerations are the domain of normative applied ethics, which can be understood as the attempt to arrive at standards that will regulate right and wrong conduct and apply them to a particular situation or question (see Fieser, 2014). I argue that there is a broader perspective to be taken on how social marketers currently conceptualise and prioritise their normative ethics; one that links with the other findings described in this thesis. I synthesise the problems into two distinct yet linked categories: Firstly, the functionalist assumption outlined earlier in this chapter that everyone is working towards a known consensus of the social good, and that this mutually agreed upon and simply defined articulation of the social good is sufficient as a benchmark against which to decide whether the intervention is ethical. And secondly, Brenkert’s (2002) concern that social marketing bypasses the usual democratic processes of grassroots action and political discourse, and thus the people social marketers believe they are helping are in fact being stripped of their voice and rights to self-determination and participation.
Few critiques have engaged explicitly with the issue of who defines the social good for social marketers. Exceptions include Webster, who suggested, “the marketer who wishes to serve the public welfare must now bring his [sic] own personal values to bear on his [sic] professional decision making” (1975, p. 77); or Fox and Kotler (1980), who rely on legislative mandate to ensure social marketing programmes are working towards an uncontroversial, legitimate social good. I have argued that the social good is defined according to an assumed and normative consensus, a social contract (Friend, 2014), that citizens should obey the law, take care of their health and the environment and generally behave in ways that are socially responsible, consummate with current cultural mores. A consensus Brenkert (2002) has described as individual and social welfare. And I don’t think that many would argue against these abstract ideas of the social good per se; the problem arises, as has been discussed in previous sections, when this abstract idea of the social good is subject to insufficient critical analysis. One issue, for example, is the observation that this version of the social good is viewed from the perspective of those whose voices are conventionally regarded as legitimate, those with social power. As a consequence, Brenkert asserts, there is an asymmetric power relationship between social marketers and those they target; an assertion that this study supports and reinforces.

There are different perspectives on the problem of how social marketers can evaluate whether they are comfortable that they are working towards an ethical interpretation of the social good in an ethical way; but most appear to ignore the asymmetry noted above. One way is to rely on the judgements of commissioners, as advised by (Fox and Kotler, 1980) or upon one's own values, as recommended by (Webster, 1975). Another is to develop codes of ethical practice, which are intended to increase “awareness of ‘ethical moments’ and the chances that…decisions will be based on a social conscience that leads us to ‘higher ground’” (Kotler, Roberto and Lee, 2002, p. 395). In 2009, a detailed report was prepared for the National Social Marketing Centre (NSMC), an organisation established in 2006 by the UK government (NSMC, 2010). This report reviewed the typical ethical dilemmas inherent in social marketing practice and proposed a draft code of ethics for social marketers. This code, like others...
before it (Rothschild, 2001; Fishbein and Cappella, 2006; Laczniak and Murphy, 1991) advises avoidance of harm, education and professional development of practitioners, equal and fair treatment for all involved, an assessment of suitable goals based on the greatest good for the greatest number, recognition of individual autonomy and choice and formalised ethics procedures; principles that appear to be based primarily in a utilitarian ethical framework. Like the frameworks developed to guide the decisions made by social researchers (see Chapter 4), these frameworks seem to concentrate primarily on avoiding causing harm, also described as unintended consequences, as a result of social marketing interventions and on making sure the benefits of those interventions are sufficient to outweigh any risk, rather than any wider social effects.

Cho and Salmon (2007) identify several such unintended consequences of health communications campaigns. These include the potential for creating confusion or distress, desensitising people to health messages or creating a desire to rebel against them (as with the scorn with which ‘5-a-day’ messages were regarded in Parkhood and Urbanwood). For example, a recent study found that fear appeals in communications to counter domestic violence can have the unfortunate effect of encouraging men to think of domestic violence as less of a problem after the campaign than they did beforehand (Keller, Wilkinson and Otjen, 2010). In addition to these consequences for individual wellbeing or attitudes, Cho and Salmon identify broader societal consequences: culpability, or appearing to blame individual for structural problems (similar to the idea of victim blaming discussed at length in this thesis); and social reproduction, forming and enabling, in which existing knowledge and power structures are reinforced, to the detriment of marginalised groups. I suggest that the latter type of unintended consequence has received too little attention from social marketers, when compared to the utilitarian concerns of whether a particular intervention has achieved something deemed positive by the social marketer and those initiating the intervention (typically senior policymakers or individuals in the public sector).

But while focusing exclusively on the symptoms of a social problem without considering structural factors could be argued to be unethical in itself, as I have discussed at length, doing the opposite presents ethical challenges as well. All
the themes described in the previous chapter are relevant: questions of legitimacy, how decisions are made, what voices are heard and which are silenced, who resists and against what, who is empowered and is this empowerment really empowering or just offering the trappings while the real power remains elsewhere. In so doing, social marketers lose claim to their mantle of objective, apolitical neutrality; their right to claim simply to be technicians (Brenkert, 2002, p. 21). And still, even when social marketers focus on the causes of social problems as well as their symptoms, follow ethical codes of behaviour and seek to sensitise themselves to and respond appropriately in what Kotler et al. term ethical moments (2002), this issue of asymmetry of power and privilege is likely to remain. Of course, this is a problem widely recognised in postmodern qualitative research and in participatory research, as outlined in some detail in Chapters 3 and 4. It could be argued that the sort of participation I sought to enable in Parkhood and Urbanwood represents progress for social marketing. To present a solution to these ethical questions represents another turn around the spiral, it is too significant an issue to be explored in depth here. Consequently, I will conclude this section and Chapter by noting areas of the literature that I think offer the potential for future scholarship in this area: these include McCormack’s notion of ethics as a “processual enactment of non-representational processes” (2003, p. 489) rather than a function of predetermined code and Renold et al.’s (2008) ideas of reflexive praxis, which holds ethics as locally negotiated, dialogic and political.

In this chapter, first I have explored the dominant construction of social marketing as an apolitical instrument that helps policymakers in their quest to achieve socially beneficial outcomes, under which construction there is an assumed consensus that there is a single social good that benefits all. I have highlighted the parallels between this perspective and that of Westernised ideas of international development, which has been subject to significant critique, primarily due to concerns over exploitation. I locate my own critique of social marketing in similar terms: that, unlike participatory research, the literature is limited in its acknowledgement of political and ideological factors, including social marketing’s legitimacy as an activity that I argue has political significance.
Further, I contend that that social marketers may be speaking and writing themselves into existence as a neoliberal discourse, assuming pragmatically that the ‘social good’ is the standard against which our activities are judged when really the only authority may be that of a Machiavellian Prince: an argument that circles around to equate good with the power to define objectives. And as well as speaking social marketing into existence in this way, social marketers may be speaking themselves out of the process, allowing once again important questions about power, politics and ideology to remain comfortably unasked.

There is a danger though that in attempting to respond to these issues, social marketing draws upon the radical discourses of participation without challenging the underlying assumptions about neutral positionality and paternalistic assumptions of a consensus-based, unproblematic idea of a single social good. I argue that the solution to these issues is not to co-opt the language of participation, rather a reconceptualisation of the role of ethical frameworks in social marketing, drawing upon ideas from participatory and feminist research, could lead to decisions that are more critically informed.

7.5. Concluding comments

“…writing is precisely the very possibility of change, the space that can serve as a springboard for subversive thought, the precursory movement of a transformation of social and cultural structures.” (Cixous, 1976, p. 879).

We sat on the spine jarringly unyielding leather sofas outside the meeting room, both of us noting the student sitting in our line of sight. He sported a vivid lime green knitted hat perched on the top of his head, incongruous with his formal business suit. As he became absorbed in the serious discussion that engaged him, he appeared to forget the hat’s jaunty, irreverent presence; his intense pose and expression better suited to his suit. It is a memory that makes me smile as I write. There wasn’t much to say while we waited; Yvette thought my viva voce
had gone well. She suspected that the examiners’ only real concern was that I had been too constrained in how I had articulated my contribution, that I had placed too much emphasis on content over praxis. She was right, as it turned out. We were called back into the room after twenty-five phone fiddling, mindfully slow-breathing minutes to learn that I had been awarded a doctorate, subject to minor amends. The amends the examiners wanted included this postscript documenting the contribution that they had pushed for, teased out, during my defence.

A number of times during the viva I had tried to argue that there were two separate things going on. Often brought to life with decisive hand gestures of division; or invisible circles drawn in the air simultaneously with the fingers of both hands; or space encased in two half closed fists: self and findings, method and writing, negative and positive space. But instead of two separate entities, the examiners saw in my responses to their questions an integrated whole comprised of three: a triangle, with inequality, identity and evocative writing at each corner. An integration that contributes to our understanding of social science and how social policy is enacted. Identity and the struggle to negotiate it are overt in the text from the beginning. My identity as an academic, that is. But tacit are other identities as well: oppressive and oppressed. Who listens, who speaks, who isn’t heard and who is, what’s in and what’s out? Increasingly for me these are questions of identity, and the struggles of the other characters in my story partially mimic my own: to work out who you are, who you want to be; to resist unwanted identities imposed by others. I wrestled with questions of identity through evocative, performative (Denzin, 2014) writing.

Feminist philosopher Jean Harvey uses the term civilised oppression to refer to the “vast and deep” (1999, p. 3) injustices suffered by some groups, which she regards as a consequence of the unconscious assumptions of well intentioned people, supported by stereotype, bureaucracy and market mechanisms. I have shown in this work how behaviour change interventions, however participatory they purport to be, are founded on an edifice built at least partially around external imposition of identity and therefore may well entrench, rather than challenge, inequality. This entrenchment could be happening through
reinforcement of stereotype; construction of excluded or problematic identities (from the interventionist’s perspective); or because interventions might operate in ways that could be interpreted by its targets as coercive control, punishment or removal of choice. We might imagine risky drinking as a problem to be solved; but for some, alcohol is a solace that they cannot imagine living without (Cameron and Jones, 1985). So like the jaunty green hat, the promise of empowerment is incongruous with the actuality.

That’s two sides of the triangle reconciled, how does evocative or performative writing fit? To explain this I want to return to the metaphor described on page 114: that scientific writing should be a clear pane of glass that does not obscure the categorised, organised truth beyond. Except, in common with post-structuralist theories of knowledge, I do not think that writing does (or can) work like this. Instead, I argue that writing is more like a Hall of Mirrors than a flawlessly transparent window to the world beyond. When you stand in a Hall of Mirrors, you see a reflection of the world, which changes dramatically depending on how you are positioned. Similarly, when I write, I want readers to see a different reflection of the identities I am writing about, I want them to feel first and then to think what it might be like to stand by that person in the Hall and see how things look from there. This is a positionality that draws from feminist post-structuralist ethnographers like Richardson and Ellis, as well as Fals-Borda’s conception of the “organic intellectual” (2001, p. 30); crafting written representations that may be liberatory rather than oppressive: advocacy through the amplification of the dangerous (Freire, 1970), challenging words of the oppressed. I take these ideas forward in the next and final chapter, an illegitimate text that attempts to combine theories of ethical writing with the contribution I present in this thesis.
Chapter 8: In which I consider, what next?

“I know who I WAS when I got up this morning, but I think I must have been changed several times since then.” (Alice, Alice’s Adventures in Wonderland, p 26).

This chapter represents what Laurel Richardson might call a “text of illegitimacy” (1997, p. 137). I wanted to write it as the culmination of the PhD process, to push the boundaries of my own understanding of the way of knowing that writing can be. Originally, I wanted to write science fiction. To conceive of a parallel universe in which it wasn’t a source of power in discourse to be objective, a distanced and aloof observer and documenter of interesting facets of social life; rather it was unethical to be so. I located this difference in my feminist ideas: wondering what if women hadn’t been written out of social theory? What if instead they had dominated intellectual endeavour? How would things be, what would we believe about knowledge and truth, in that world? When might the turning points have occurred (they would have to be relatively recent because otherwise the structures of oppression that prompted feminist theorising wouldn’t have been there to stimulate it)? What if…

But quite quickly I realised that constructing an entire fictional universe to tackle major epistemological debates like these would take a great deal more time than I had available to me, and as with anything that attempts to break with convention, it has to break it while at the same time being very, very good. So while I haven’t abandoned entirely the idea of academic science fiction, the idea was consigned to the future. Instead, inspired by an exercise I undertook with author Sarah LeFanu and my nascent creative writing group, I have adopted a lexicographical format for my illegitimate text.
Action. I am left with the vaguely uncomfortable feeling that nobody really knows exactly how all this action, this social change, this transformation that is all over the literature is actually enacted. In order of appearance, orthodox social marketers imagine a rational assessment of advantages and disadvantages followed by willingness (or not) to participate in an exchange. Communitarians envisage strong, supportive communities and systems perspective advocates imagine fairer markets. Participatory researchers write of knowledge for social transformation, but acknowledge the problem with the underlying mechanics; feminist post-structuralist participatory researchers hope that subjectivity and personal transformation might hold the answer. Autoethnographers write of stimulating emotion in their readers that will provoke them to curiosity, reflection and change. Activists write of being the change they want to see, and I think that this is the closest that any of the groups come to identifying how their philosophy of change actually works. Overall, theories of change and actual real life change appear to be disconnected.

Activism. One consequence of these 300-odd pages of writing has been that I am inclined now to redefine myself as an activist scholar. This, I think, brings together the disparate elements of this thesis and hints at an underdeveloped yet potentially more significant contribution: linking the epistemological work of Richardson, Ellis at al., with the ideas of Freire and Fals-Borda and Ruddick’s “undertow of biography that pulls us towards one issue or another” (2004, p. 229). I find myself asking the question, can we (should we) use the legitimacy conferred on academics through our status as scholars to tell “advocacy tales” (Van Maanen, 2010, p. 19)? Is there a niche among all this scholarship for explicitly participatory and evocative writing as activism?
B

**Backwards.** Throughout the writing, I have wrestled with the problem of how to preserve the authenticity of the research process. I think that one significant way that this sort of research can be judged (the validation procedures, to borrow the language used in my institution's guidelines for doctoral students) is to lay bare the process by which the final, honed contribution was constructed. If I had followed this conviction fully, rather than compromising somewhat for the convention of a PhD thesis, this document might well be different. Chapter 4, with its story of my journey from the spirals of action research to post-structuralist autoethnography, via reflective practice and emancipatory action research, would have been the first I would have presented. Followed by Chapters 5 and 6. In this order, my thesis would have been a story of searching for a methodology that could make sense of a practice already completed, a reinterpretation of that practice and a theorising about it. Only then would I have wanted to write about participatory research and activism. My writing about social marketing would have been very different then, I think. I would have wanted to take a broader perspective on UK public policy about deprivation and health, situating the increasing interest in communities and their capacity in an overtly political context. And so the dizzying spiral continues.
C

Contribution. Mindful of the requirement to conduct enquiry that leads to creation and interpretation of new knowledge and to communicate and justify an independent judgement of issues in the field (Institutional Doctoral Guidelines, 2014), articulating and situating a contribution to knowledge has been on my mind throughout the writing of this thesis. As I said in the methodology chapter, my thesis holds that methods of intervention design that use the language of participation and empowerment are associated theoretically, linguistically and ethically with participatory research methodologies and that this heritage brings both opportunity and responsibility to the practitioner, particularly when working with disempowered groups. So what can I offer the responsible practitioner who wants to use participatory methods and call them social marketing (or vice versa)? I would suggest that if you know what the outcome is likely to be before you begin, you should not call what you are doing participatory. And if you do not already know the outcome, you should be ready to trust the process and prepare yourself and your collaborators for a reasonable amount of discomfort along the way. I would say be honest with participants and do not make promises you are not certain you can keep. Continually reflect upon yourself, your assumptions and whether what you are doing is ethical, because it will probably not turn out the way you think it will. Embrace failures. Realise that, like any sort of genuine social process, participation takes time and requires you to give as well as to receive. Recognise that you are not doing people a favour by turning up in their neighbourhood and expecting them to fall in with your plans. Appreciate that if you are successful in stimulating participation, you cannot expect people simply to forget about the work and move on when you do. It is your project; it is their life.
D

Development. One of the things I liked about the notions of reflective practice discussed earlier was the idea that researchers could seek explicitly to develop in their practices, as well as their knowledge. I have certainly developed in my knowledge, but now feel motivated to ask myself what I would do differently in the future; how I have developed in my practice. My first thought is a relatively personal one: I would not agree to take on a significant project like the one described in this thesis and call it participatory. I have come to the conviction that participatory research is founded upon the notion of consciousness raising and thus transformative upon the individuals that are involved, not upon an entire neighbourhood. That’s not to say that individuals cannot or should not play a leading role in co-designing public health interventions, but these interventions shouldn’t be constructed as participatory because they are unlikely to be underpinned by the discovery of self-knowledge or raised consciousness of oppression. So if they are not participatory, what are they? Calling them co-designed is probably helpful, as this allows for other theories of social change to be brought to bear. Another way in which I have developed in my practice as a researcher is via a changed perspective upon my professional life: everything appears as ethnography now. I keep a daily journal, which is part field note and part autobiographic reflection. I envisage that any future research projects will attract this dual focus.
Empowerment is a word much misused, I think. The World Bank, much criticised by participatory researchers like Bill Cooke, define empowerment as “the process of increasing the capacity of individuals or groups to make choices and to transform those choices into desired actions and outcomes” (2011, p. 1). It seems to me that this word, as my friend Sadie described in Chapter 6, has become a buzzword, something that people claim to be doing because they want to imagine people empowered as a result of their work, rather than because people actually are. I experienced this in a meeting recently with the representative of a corporate social responsibility scheme, who spoke of ‘empowering’ a marginalised group through an activity. I challenged this, asking what power this group were actually being given, what capacity? What decisions could they now make, with this new power that they had? Everyone around the table reflected for a moment and then the representative agreed that they often used the term without thinking through what and how the actual empowerment would be.

Geography. Who knew? So much interesting work is happening under the rubric of a discipline closely associated (in my mind anyway) with eccentric teachers in corduroy trousers and checked shirts waxing enthusiastically about the evolution of farming and the Duke of Edinburgh award scheme.
Journal. I found keeping a personal research journal valuable both as a record for how ideas develop and a source of new ideas. This observation prompts me to wonder whether academic journals serve the same purpose in a collaborative way or whether they have come to represent the ultimate driver of what has been conceived as academic exploitation, i.e. taking the contributions and stories of participants and benefiting from them personally. I was going to write about this in my fictional viva, through the medium of Ada. Ada was a character I developed; a teenager with a quiet calm expression, she would have spoken in very precise way, suggesting that English may be a second language. She would have wanted to explore the link between consciousness raising and actually making some sort of real difference. She would have wanted to know how I justify what I have contributed via this study. I envisaged her saying: “you know that what is important is what difference you have made to people’s lives, not what you might be able to publish in one of your journals. That is for you and your academic friends, if you think it will be empowering for your community to share your findings according to their cultural norms. Of course, that is a very worthwhile aim for you. But it is not relevant for us.” In this way I wanted to highlight the way that culturally appropriate dissemination is spoken about as a necessary yet ultimately less valuable form of dissemination than that which is affected by publishing in high-ranking journals.
Limitations. If I had known when I began the participatory project that I was also going to be studying the process as well as implementing it, I may well have been able to go further than what amounts to a number of critically informed questions about social marketing. Drawing more deeply on feminist post-structuralist thinking, I could have framed more specific questions about language and inequality and health policy in the UK. With this hindsight, the research presented here would have been closer to ethnographic, perhaps one of Richardson’s (1997) collective stories; rather than the autoethnographic retrospective that I present here. Another alternative could have been fully textual; using discourse analysis to understand how power relations were negotiated throughout the project would have been very interesting, for example.

Commenting on this draft, Tim pointed out that another limitation of my approach is likely to be that it is not very fundable. He is, as ever, right: this type of research does not add much to an evidence base for public health intervention, it is incompatible with the pragmatic rhetoric of “what works” (Perkins et al., 2010, p. 101). A public health funder would no doubt be astonished to receive an application for an autoethnographic critique of their policy. Tim asked if I think it is better to secure funding than not, accepting all the limitations and biases I have discussed. The answer to this question could be very long. In brief however, my reflections centre on two issues: firstly, the ethical criteria I would now apply to the sorts of projects for which I will seek funding (and to decide whether to accept an offer of funding if approached) are more refined. And secondly, it seems feasible to design projects that are both ethically informed and likely to be funded, and simultaneously to study and critique those projects as I have done in this thesis. The challenge with this approach would centre on the ethicality of such critiques and whether eventually I would alienate myself from funders.
Memory. As Coffey writes, this project has been about “experiencing and remembering; ordering and giving frameworks to [my] memories” (1999, p. 127). One of the limits of this piece of work, in contrast to a more conventional ethnography of either Parkhood and Urbanwood themselves or of the project, is that it relies heavily on my recollections to interpret the data. On the other hand, the filter of memory, or perhaps hindsight, has been a helpful lens through which to regard the project.

Marketing. Probably isn’t the solution to society’s problems. Sorry.
Performance. The theoretical construct of a performance and its implications are an area for further research. In the context of commercial marketing, Tadajewski and Brownlie (2008) conceptualise performative marketing as that which is in the “sole interest of developing knowledge to enable marketing managers to maximise the sales of goods or services with minimal expenditure” (p. 9, emphasis in original). This does not seem related to Butler’s ideas about performance in relation to gender and heteronormativity; speaking of the gendered body, she states that “acts and gestures, articulated and enacted desires create the illusion of an interior and organizing gender core, an illusion discursively maintained for the purposes of regulation of sexuality within the obligatory frame…” (Butler, 1999, p. 173). To me, the latter is closer to what seems to happen in participatory projects when we create a discourse of empowerment; we are using words and gestures (for example, co-design events) which enact the idea that participants are being given power to influence the process, but there is a lack of critical engagement with the extent to which they really can do so. Rather than performing gender to regulate sexuality, we are performing grassroots engagement to legitimise and regulate the power of policymakers to define the issues and their solutions. This notion requires further study.

Process. It’s probably the only thing you can trust.

Positionality. I seem to have developed a relatively novel sort of positionality for myself in this thesis, based on issues that a researcher cares about and can sustain a personal interest in and energy for. A connection with feminist research once again, in the observation that second wave feminists sought to redefine power as energy, strength, motivation and successful mobilisation of resources for oneself and others (Maguire, 2001).
Responsibility. I can conclude that a useful addition to ethical codes for any participatory intervention is one based on the idea of responsibility. As explored in Chapter 3, researchers and participants can build up quite close links, which can be hard to let go once the project has come to an end. In addition to this observation however, I would note that in a deprived neighbourhood, the promise of empowerment and change that can be made by the rhetoric of participatory interventions can reinforce a climate of resignation founded in hopes raised and dashed; caused by the sheer number of well meaning initiatives that are started and then have to end for one reason or another (often cuts to funding or other political changes). While I was aware of this problem right from the start of the project, as I report in several chapters, I don’t think I really understood its extent, nor the challenges in surmounting it. If participatory inclined researchers are good at their jobs, they will awaken something in the people they work with, that is the point. And unless they plan to stay and personally oversee work in that neighbourhood for a long time, they cannot personally guarantee that the hopes they have helped nurture will not be left to wither away. So, what to do? In my experience of the participatory work I have done, there are two possibilities, one better than the other. The less ideal option is to make sure that a permanently employed individual has a leading role in the work; this person provides sustained interest and support after the research project ends. Better, though, would be to work with participants to secure their own funding and management, so that the neighbourhood really controls any legacy. Both have their issues, but both are better than simply waving goodbye at the end of a participatory project, taking empowerment with you.
As we construct ourselves as academics and researchers, we are carving our minds into the instrument that does our work, through reading, reflecting, writing and experiencing the worlds we aim to study. We read and we think and we experience and we write so as to add something to the body of knowledge that constitutes our disciplines and our lives. We do this so that we can continue to build up our faculties, so that we can engage with a bigger and more complicated picture; and thus continue to make more and better contributions to knowledge. When musing in this direction, I often wonder why it is such an effort to cast aside the positivist inspired thinking patterns: is it because this is how we are trained to think, like scientists, designing a fair test, doing every measurement three times and taking an average? Or is it because this way of thinking about the world, as something real and tangible and out there that everyone can see in the same way (your grass is green too, right?) comes naturally to us as humans and other epistemological perspectives take effort to attain?
Zeal. I perceive a danger that, in revelling in the opportunity to critique social marketing (and by extension social marketers) that I become divorced from the messy reality in which they practice and forget that they are intelligent, civic minded people doing work that in many cases is of significant social value. As part of my attempt to write my fictional viva, I noted down the following dialogue between my examiners and I. An examiner begins: “So your colleagues, these Social Marketers you keep referring to. Who are they? Are they bad people?” I reply, “No, they’re not bad. They believe they are doing good, important work.” The examiners pepper the walls with my own internal questions: “Why don’t they see things the way you do? Won’t they be angry with you for criticising their work? Won’t they think you are wasting your energy on picking apart their ideas instead of doing something yourself?” Certainly, I can hear some voices, offended at a perceived criticism of their motives, arguing that what they are doing is beneficial for society; it’s better than using their skills to market Coca Cola, they reason. I can see their point. Of these affronted colleagues I would ask only that they acknowledge that no technique is neutral or independent of the political context in which it is deployed; and that they take some time to read the critiques of international development, in particular participatory development, to inform their own practice as social marketers.
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Appendices

Appendix 1: Consent Form

Getting involved in some research about drinking

What is this study about?
One of the main goals of the project is to find out about the impact of alcohol on the lives of local people: what they drink, where and how much. We don’t just want to find out about drinking though; we’d like to work with people to find out more about what it’s like to live here and how to help people become healthier in general.

Who is organising the study?
The study is being organised by the Bristol Social Marketing Centre at The University of West of England, and is being paid for by [PCT name redacted].

What will happen during the project?
You can become as involved in the project as you like: a little or a lot! The first thing that we’re going to do is work with local people to understand more about what it’s like to live here, how drinking fits into their lives and about their everyday health. Then, we’re hoping to be able to work with you to use what we find out to help local people reduce the amount they’re drinking and become healthier.

People generally enjoy taking part in research like this because it gives them a chance to get involved with something new and interesting, meet people and make a difference locally. But we’re not asking for a big commitment, and you can stop being involved at any time, you don’t have to give us a reason.

If you do decide you don’t want to continue being involved, we might use any information about yourself you’ve already told us, unless you ask us not to.

The discussions and meetings that we have might be recorded so that the research team can remember what is being said, but what you say will always be reported anonymously. It is totally confidential research. Nobody will hear the tape other than the research team and the person who types it up. We might also take photos and video, but we will always tell you beforehand, and you don’t have to appear in any photos or videos if you don’t want to.

Are there any disadvantages in taking part in the project?
We do not expect there to be any downsides or risks if you take part. But, if you are worried about anything please contact Katie Collins at the University of the West of England, Tel [redacted] or email [redacted].

Confidentiality
All information we collect during the study will be kept strictly private. Information about you or anyone else you talk to won’t be kept with your or their name so no one except the researchers will know who it is about.
All the personal information will be kept in secure premises at the University of West of England. A password-protected computer accessed only by the research team will be used to store this information.

**Ethics**

All research carried out by the University of West of England is looked at by a group of people, called a Research Ethics Committee. They protect your safety, rights, wellbeing and dignity. This study has been reviewed and given permission to go ahead.

**What will happen to the results of the study?**

We will share what we find out with anyone in your community who is interested, and we might also tell other organisations about our project. But we will not give out any of your personal information to anyone.

If you would like to know anything else about the project or about what we find out, please contact Katie Collins at the University of West of England.

Signature______________________________________

Name __________________________________________

Contact number _________________________________

I agree that you can contact me again ☐
Appendix 2: Case study stories and illustrations

Case Study 1: Young mum

Isolated and unhappy since she gave birth as she has can’t go out with her group of friends anymore, so drinks to combat feelings of boredom and depression

Lauren* is 21 years old and lives in a two-bed flat. She has a little girl who is 10 months old. She split up with her ex 6 weeks after her baby was born, because he couldn’t cope with the stress, sleepless nights and changes that a baby brought to their home. He still sees his daughter every week and tries to help out financially occasionally, but he has moved out of Parkhood to find work.

Lauren had her first drink when she was 12, along with her mates. They always used to go out together as a group; they would have a few bottles of wine at each other’s house before they headed into town, to get drunk on less money. They didn’t cause much trouble, they had a laugh and were just doing what everyone their age did. Ever since she had the baby, Lauren has stopped going out regularly with these friends. She can’t even go round to have a drink with them before they go out, like she used to do when she was pregnant, because she can’t leave her daughter. In fact she can’t go out at all in the evening, and stays in alone with nothing to do but watch TV.

Lauren has one brother in Parkhood, but he works full time and has a wife and a kid so he can’t help much. Her mum lives nearby and has limited mobility so would love to come over more often to help with the baby but she finds it hard to get out and walk to the bus stop and can’t afford the taxi fare. The only person Lauren has really talked to since her daughter was born is the Health Visitor, who keeps encouraging her to try to get out during the day to get to local toddler groups and meet other mums. But she doesn’t know anyone who goes to the toddler group, and is worried that it will be a bunch of older mums who will criticise her for being young and single. It is also at the other end of Parkhood and she feels more nervous going up there. Lauren feels let down by her friends,

* Not their real names
who seem to have dumped her since she had her baby. She used to feel like she had a lot of fun and friends, and that she had a lot to look forward to, but now feels like she will be trapped as a lonely mum for years, with no chance to get a proper job or get out to see her friends.

She knows some other mums where she lives and she sometimes gets invited to their flats, where they get all the kids playing together then spend the afternoon over a few bottles of wine. She knows this isn’t a good thing to do but she likes being with them most of the time because she isn’t lonely then, and she can’t be there and refuse a drink because they’d think she was criticising their drinking.

They are really critical and bitchy about other mums in the area, and she doesn’t want them to find out what a bad mum she thinks she is, so any time she thinks she has done something stupid or wrong it turns into a big deal for her. She wants to get on with them because she knows they could really cause trouble for her if they wanted to.

Lauren does worry about the amount she is drinking and that she is a bad mum; she is frightened to ask for help or even tell the Health Visitor how much she is really drinking because she is worried they might take her daughter away. She sometimes resents her daughter and this makes her feel worse, especially in the evening when there is nothing else to do but sit alone and watch TV. So she started having a drink when she puts her daughter to bed and now she finds she is drinking for at least 4 hours a night. She picks up cheap deals with her weekly shopping, so she feels better that she is not going to the off-licence every morning because someone would notice.
Case Study 2: Carer

_A wife who is trying to live with and support someone who is drinking too much._

_They have difficulty in getting support for the person who needs help and also feel a lack of support themselves_

Jane* is 46 years old. She is married to Bill*, and they have a son aged 17 and a daughter aged 13. Bill had an accident at work three years ago, and he has very little mobility in his left leg now. He had to give up work and although he was given a good settlement, nearly all that money has gone now and he is finding it hard to get another job. He can’t move very far, can’t sit for long and is often in pain.

As a result, Jane has had to increase the hours she works at a café in town, but money is really tight. Bill feels like he can’t look after his family and that he is failing them, and is getting increasingly depressed. He used to have a few good friends that he went to play skittles at the pub with a couple of times a week, but now his leg prevents him from going so they have replaced him on their team. He doesn’t really see anyone outside of the immediate family; instead he watches a lot of TV and gambles on the Internet.
Bill helps a bit with going to the shops around the corner, but can't manage any of the housework or gardening, so Jane and the children divide these between them. Jane is exhausted a lot of the time as she has to work nearly full time, and then comes home to manage the house as well.

She feels that Bill resents her for managing to juggle a job, the family and the home as well, but she doesn’t do it to make him feel bad - she does it because if she didn’t then it wouldn’t get done. She would like a bit of gratitude and recognition of the hard work she puts in; she struggles to cope with Bill’s depression, as he often gets angry and frustrated, and takes it out on her and the kids.

He usually just shouts, but has recently started throwing things and has smacked her across the face twice. She is also worried about Bill’s drinking as this has steadily increased since his accident, partly to manage the pain and help him sleep but also partly because he is bored and depressed so he has a drink whenever he wants to forget the way his life has changed. She is now sure that he starts drinking at lunchtime as she is measuring the amount of empties.

She tries to restrict it by limiting the amount of money he has around the house, but the old man across the road is also a heavy drinker and has started coming over regularly to keep Bill company in the afternoons so he is also contributing to the problem. Their son is also drinking more of the alcohol lying round the house, and when she challenged him he said ‘if it’s alright for dad then it’s alright for me’.

Jane feels certain that if something could be done about Bill’s pain and mobility then he wouldn’t feel the need to drink so much, but at the moment he needs help with his depression. She doesn’t know of any services that could help her or offer advice, and she doesn’t know anyone else who has gone through anything similar.

Certainly none of her friends or neighbours seem to have problems like this, and she doesn’t want them to find out because it would be humiliating for her and her husband. She used to feel that they fitted in well and they were liked by the other people in the street, and she hates the idea of them talking about her family
behind her back. She doesn’t even want to tell the doctor because everyone she knows goes to that surgery and she’s sure it will get out.

Although she has her family around her, she feels really alone and that she is trying to keep her family together on her own, and that it will reflect badly on her as a wife and a mother if she can’t keep it up. She is also worried about the GP passing it on to social services who might interfere with her kids; she doesn’t trust health professionals.

Case Study 3: Younger man

He experiences social pressure to keep drinking and pressure to keep things fun and not to seek help. He feels he has the right to drink; he earns his money and can decide for himself how to spend it. If mistakes have been made while drunk, these are still framed as fun (e.g. injuries or crimes committed)

Alex* is 34 years old. He has a big group of friends that he has grown up with in Urbanwood, and they all go out together. They started drinking when they were about 12 or 13, the same time they started smoking. They go out Friday and Saturday night into town, and sometimes mid-week as well, although this is sometimes more local like the rugby club. Before he goes out Alex will drink a
few shots or beers, depending on whether he plans on having a heavy night, because it will make the evening cheaper if he is already tanked up before he leaves. So he regularly stocks up at the supermarket when he does his shopping as the cheap deals are much better than the local shop. He works full time for a local construction company, and has a 2 bed house and his own car. He doesn’t really get involved in the community as he usually goes outside Urbanwood for entertainment and just looks on it as a cheap area to live. Drinking is part of Alex’s identity – he thinks about himself as part of that group of friends, and drinking is what they do together: they keep in contact during the week via texts and Facebook and plan what they are going to do that weekend.

The alcohol just makes it possible for them to act the way they want to have a laugh and a good time, even if he had a bad day or week at work he can forget it and have a good weekend. He has no real stress or worries in his life, drinking is just for pleasure and he feels that it is his right to drink as much as he likes because he has earned it through hard graft during the week. He resents anyone telling him what he should do with his own money, especially when no-one else is involved. He is aware that his lifestyle is not very healthy, but he doesn’t want to think about that now, no more than he wants to think about getting married or having kids – he can think about that later.

He likes the feeling of being able to do what he likes when he’s had a few drinks. A lot of the men in the pubs and clubs he goes to are in their 30s and 40s, and they all act a lot younger after a few drinks. He has been mistaken by girls for being in his mid-twenties a number of times when he’s been clowning around, which he quite likes. If someone gets hurt or something gets damaged then it’s all just part of the fun, you make a joke out of it the next day because you never know how the night is going to turn out. His sister did ask him to cut back after his mate broke his own foot one night, and she might have a point because Alex couldn’t work if he was injured like that. But he wouldn’t listen to her or anyone else really, because if he did then the rest of his mates would take the mickey – it would be ok if they all decided to cut back but if he just wanted to drink less on his own then he’d have to find a different group of mates. And he couldn’t
imagine dropping them, they’d definitely hassle him about it and say he thought he was better than them.

Alex doesn’t think that he has a problem with drinking, or that it is risky – apart from if someone got seriously hurt which would be unlikely because they don’t usually drink and drive. He thinks people who do have a problem with alcohol are old and often homeless street-drinkers, who buy cheap booze every morning from the off-license and start drinking before lunch.

Case Study 4: Older man

He has been drinking too much for a while and now lost things that are important, such as job, wife and children. He has reached the ‘bottom’ and it is now more acceptable to seek help.

Michael* is 56 years old. He used to live with his wife and two children in a 3-bed house, where he ran his own business as a cab driver. He grew up in Urbanwood, with his brother and parents. His dad was a heavy drinker and used to regularly beat him and his brother from a young age. He was constantly nervous because he didn’t know when his dad would strike out at him. He fell out with his dad when he was 17, and didn’t speak to him again. His dad died 15 years ago from liver cancer. Michael started drinking at home when he was
young – he used to steal some of his dad’s booze. When he left home aged 17 he was already drinking every day, and he used it to help him relax and sleep at night.

He didn’t used to see drinking as a problem. He would go out with his friends for a few pints two or three times a week, when they would play darts or skittles. He’d have a few glasses of whisky when he got home too. But then the pubs changed and stopped offering the games, so they just spent all their time drinking instead. The work dried up as well, and he had more time on his hands so he started drinking in front of the TV in the afternoon. His wife moaned at him to get out the house, so he used to go to the pub instead, for some peace and quiet. The pub was the only place he could think of going - he didn’t know of anywhere where men his age could go and do something other than drink. This extra time drinking meant that he couldn’t take up the odd bit of work that did come through, because he’d had too much to drive. So his business gradually folded, and his wife kept blaming him because they couldn’t afford the things that they used to be able to buy. He felt like a failure because he had built his business up, and so he went to the pub more often to get away from the house and everything that reminded him. He found that he was drinking there on his own more and more, especially after a couple of his close friends died.

Michael didn’t think he had a problem with alcohol, but he knew that he was depressed. But he didn’t feel able to go to the GP or speak to his wife, because it would be admitting to another weakness or failure on top of losing his business and putting his family in debt. He knew that his friends would also tell him to keep it together for his family, and that he was no use to them if the doctor sent him away for treatment. He thought it was a private family matter and he didn’t want any outside person interfering. It was up to her to tell him when she wasn’t prepared to put up with it anymore, and then he could do something about it. But she didn’t, she just decided to take the kids and leave.

This was four years ago - his wife then remarried and his children who are now 15 and 12 years old have taken on their new step-dad’s surname. Michael had to move out, and now lives in a room in his friend’s house. He doesn’t sleep well
and he doesn’t get up until lunchtime because he doesn’t need to go to work. He finds it difficult to get up because there’s nothing pushing him to and he often feels depressed about the day ahead so then it’s easier to skip breakfast and go straight into lunch and a beer. His friend is also a fairly heavy drinker, and they share the cost of rent and food between them and then spend the rest on alcohol. On a good day Michael feel like he wants to do something to cut down his drinking, because he knows how much he has lost as a result of it. But on a bad day he feels really low and worthless, because he has lost his business and family and his children don’t even want to share his name anymore. On these days he feels like he wants to drink to forget.

When he does think about wanting to get help and cut down drinking, Michael doesn’t know what he would do with his time. The men he knows are all drinkers, though some of them now encourage him to get help because they can see how much he has lost through drinking. He wouldn’t think to get involved with any community activities because he thinks these are for women and children, not divorced middle-aged men. So the only places he can think to go to get him out of the house would all be places where he could drink, and all the people he now sees regularly have a drink in their hands.