

On Theory, Knowledge and Practice in Housing and Urban Research:

A Phenomenology of Conflict and Reconciliation

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Abstract

One year before my academic life began, Jim Kemeny published a book called 'Housing and Social Theory' (Kemeny 1992). This book has had a major impact within European housing and urban research over the last two and a half decades, not least by crystalizing its epistemic divisions into 'mainstream' and 'critical' (Webb 2012). In the face of Kemeny's critique, 'mainstream' housing and urban researchers have remained wedded to 'policy oriented' empiricist approaches about which they have been defensive. In fact, it could even be argued that policy oriented housing researchers have been emboldened during this period. On the other hand, some housing and urban researchers have spent the last 25 years exploring the relationship between housing and social theory with a view to developing a more 'critical' understanding of housing and housing policy. My own work falls into this latter category and can be broken down into three phases (represented in the three parts of this thesis) which all bear the hallmarks of Kemeny's influence, to greater or lesser degrees.

Kemeny's influence is most obvious in part I of the thesis. This contains a series of papers that represent my attempts to develop a sociology of housing and housing research. Although my initial contributions to the literature focussed on the social construction of housing problems and policy, my subsequent interventions recognise that it is not enough to focus 'critical' theoretical attention on policy issues alone; the context of research practice, itself, requires the same critical theoretical attention. This recognition set me on an intellectual track that resulted in published contributions to the sociology of knowledge literature, *within the entrepreneurial*

context of the contemporary university. These contributions examine how entrepreneurial contexts shape academic subjectivities and the sociological episteme.

If part I of the thesis finds sociology useful in illuminating housing policy and housing research practice then part II contains a book and two papers that call it into question. The origins of this 'hostile turn' towards sociology are in two pieces of research (into the lives of heroin users and visual impaired children) where sociology had hindered my attempts to develop an adequate knowledge of the phenomena under the microscope. The publications in this part of the thesis embrace phenomenology to make theoretical sense of the limits of the sociological episteme and to develop a more adequate understanding of the lives of heroin users and visual impaired children. They also set me on an intellectual path that led to my theoretical development of a more fundamental critique of housing and urban research and, eventually, a constructive and reconciliatory resolution to what I have argued are its epistemic limitations.

The book and two papers contained in part III of the thesis were produced in conditions of acute conflict. The book and 'fallacy paper' were written in response to the controversial housing market renewal programme but were contextualised within my wider intellectual concerns about the fundamental problems of housing and urban research. They represent a full-frontal intellectual 'attack' on the professional enterprise of housing and urban research and its social consequences. The 'impact' paper was written and published 5 years later, following a 'career break' during

which I had reflected on the fundamentally conflictual nature of housing and urban research and sought nonviolent alternatives to such conflict. It outlines a reconciliatory approach to housing and urban research that is true to the intellectual argument in the 'fallacy paper' whilst seeking to outline and advance the possibilities for collaboration between housing and urban researchers and their constituencies.

On Theory, Knowledge and Practice in Housing and Urban Research: A Phenomenology of Conflict and Reconciliation

A Critical Summary of the Submission

Introduction

My academic career, and contribution, has been mainly concerned with the issue of knowledge. My objective has been to develop a critical understanding of the social conditions in which knowledge production takes place, the social distribution of the capacity for knowledge production, and the contexts in which knowledge produced is mobilised and applied.

The PhD thesis is presented in three parts. Part I of the thesis contains a series of 4 publications that demonstrate my concern to develop a sociological knowledge of Housing Policy and Research. To make sense of what I was trying to achieve at this point, it is necessary to locate one's work within its social and biographical context. Thus part I of the critical summary begins by providing an introduction to the field of Housing Policy and Research as I found it. It pays particular attention to the debate about the problematic 'policy orientation' of Housing Research that was occurring when I entered the field in the 1990s. The papers in part I of the thesis are explicitly located within the context of this debate and are illustrative of my attempts to develop a sociological knowledge of Housing Policy and, latterly, Housing Research itself.

Part II of the critical summary contains a discussion of an autobiographical turn that led me to question the adequacy of such a sociology of Housing Policy and Research. This occurred during two research projects, in particular, in which I encountered problems with the sociological episteme. Sociology was hindering my attempts to understand urban experience. I subsequently embraced a phenomenological approach to housing and urban issues which I initially elaborated in publications that explored the urban experiences of visual impaired children and drug users and the experience of urban deprivation. These publications seek to understand the epistemic limits of social science and develop a phenomenological approach to overcoming them in an urban context.

If part II of the thesis has a phenomenological concern with the substantive aspects of housing and urban experience, part III works through the fundamentals of what a phenomenological commitment means for the enterprise of Housing Research *writ large*. It also elaborates an alternative way of 'doing' Housing Research *per se*. It begins with the phenomenology of conflict perspective in my book *Housing Market Renewal and Social Class* (HMRSC) and the 'fallacy paper' which are hostile to sociology and Housing Research on philosophical grounds of adequacy and relevance. However, as I suggested above, autobiographical context is never far away from our intellectual work. My negative experiences of the conflicts and hostilities that surrounded HMRSC and the fallacy paper were partly responsible for my decision to take a career break in 2011. Since returning to academe, I have been seeking to develop a reconciliatory approach to sociology and Housing Research within the context of my phenomenological commitments. This is evident in the final paper to appear in the thesis which was published in 2016.

Part I: For a Sociology of Housing Policy and Research

Housing Policy Research in Brief Historical Context

A key feature of the first part of my academic career was my attempt to grapple with the sociological problem of knowledge. My initial engagement with these issues occurred after the publication of a landmark text '*Housing and Social Theory*' by Jim Kemeny (1992) which made the argument that housing research had become disconnected from debates in the social sciences. For Kemeny, the key problem with Housing Research was its regression towards the 'policy orientation' that marked its historical beginnings but which it had superseded as it socially matured in the 1960s and 1970s.

Housing and Urban Research began in the early 20th century as an instrumental mode of empirical inquiry that serviced the needs of social reform, urban management and policy making (Savage and Warde 1993). This was in keeping with the classic 19th century urban poverty studies of Charles Booth and others which marked the beginnings of housing research and defined its remit. The key Housing Research task in this period was technocratic; to identify and solve housing problems in the public interest. This instrumental orientation lasted until the 1970s which saw a 'theoretical turn' in Housing Research. This was prompted by the emergence of the 'New Urban Sociology' (NUS), which had its roots in Marxism and the urbanisation of Marxist thought (for instance Harloe 1977 1981). The idea that cities had problems that could be objectively identified and solved by social scientists, or that they could be managed and reformed by technocrats, was anathema to NUS intellectuals. They embraced a

theoretically grounded conflict perspective which saw the city as a site of capital accumulation, social reproduction and class conflict and began to write about it as such – leading to the development of a raft of influential Marxist urban theory (Castells 1977; Harvey 1989; Lefebvre 1991).

Although Kemeny (1982) had been critical of NUS, by the end of the 1980s and beginning of the 1990s he was expressing concern that Housing Research was abandoning the theoretical roots that it had laid down in the 1970s (Kemeny 1988 1992). He and others, including myself, have since blamed this situation on the changing political economy of housing research (see Clapham 1997; Allen 2005; Allen and Marne 2010). One of our key points has been this: Whereas the NUS intellectuals occupied relatively autonomous positions within the 1970s university from which they could produce ‘critical’ scholarship, reductions in the government funding of universities throughout the 1980s, alongside an expansion of student numbers, eroded the time and space for producing scholarship. This made later generations of Housing Researchers more reliant on ‘external’ funding in order to ‘buy’ themselves time to undertake research (Allen and Marne 2010). Moreover, it occurred at the very time that the Thatcher government was beginning to ‘contract out’ its research function to universities in order to shrink government departments (Clapham 1997; Hillyard et al 2004; Allen 2005; Allen and Marne 2010) whilst simultaneously placing more emphasis on ‘policy relevance’ in research supported by the Economic and Social Research Council (Hillyard and Sim 1997; Partington 1997; Allen and Marne 2010). Kemeny (1992) lamented these changes because they had led to an ‘epistemic drift’ of housing research into the language and concepts of the housing policy world and its disconnection from theoretical debates in the social sciences. This was having

huge consequences: Housing Researchers had begun to take-for-granted 'official definitions' of housing problems that circulated around the housing policy world they were now enmeshed in. Kemeny (1992) argued that countering this problem of epistemic drift into the language and concepts of the housing policy world would require Housing Researchers to return to their social science disciplines.

Initial Contributions to a Sociology of Housing Policy

The early part of my academic career was spent in a Housing Research centre at Cardiff University. From 1993, I was employed as a 'Contract Researcher' working on 'external' funded projects in the area of housing and social policy. The centre was enmeshed within Housing Policy networks and dominated by policy concerns. Although this suited my interests in the use of research as a tool of social improvement, I had broader interests in developing more 'critical' and 'deeper' theoretical understandings of society. I was only able to pursue this second ambition when David Clapham arrived as the new director of the research centre in 1994¹. Jim Kemeny subsequently became a Visiting Professor at the centre.

Although undertaking policy oriented research, the early part of my academic career was pursued under the mentoring of David Clapham and Jim Kemeny who were both advocates of a theoretical return to the social sciences. Moreover, they occupied institutional positions in Housing Research that were similar to those occupied by the

¹ My first attempt to write theoretically was a neo-Marxist account of community care in *Welsh Housing Quarterly (WHQ)* prior to Clapham's arrival. The then Director of my research centre, who sat on the editorial board of *WHQ*, told me that nobody would be interested in such an article and encouraged me, instead, to produce a policy analysis.

originators of NUS in the 1970s: In the same way that NUS intellectuals established their own institutions in order to re-shape the field of urban studies (e.g. Research Committee 21 within the International Sociological Association and an edited book series), Kemeny and Clapham wielded similar institutional influence in Housing Studies. By the mid-1990s, Clapham was Chair of the Housing Studies Association as well as Director of the Centre for Housing Management and Development at Cardiff University. He used these positions to inaugurate a number of events and conferences that addressed the interface of social theory with Housing Research. He encouraged and facilitated my interest in theoretical work and also promoted it by providing me with key opportunities to speak at these events. At the same time, Kemeny was appointed as editor of the international journal *Scandinavian Housing and Planning Research* and proceeded to re-orient and rename it, *Housing, Theory and Society (HTS)*.

Although Kemeny advocated for *HTS* to be a broad ranging journal that attracted work from across the social science disciplines² its early days were dominated by sociological contributions that reflected the work of its editor (Allen 2005b): Since Kemeny (1984, 1992) had long emphasised the need for critical accounts of the social construction of 'official' definitions of housing problems, the social constructionist perspective dominated these early sociological theoretical contributions to *HTS* and other journals in the Housing Studies field (Franklin and Clapham 1997; Allen 1997; Allen 1999a, b; Saugeres 1999; Hunter and Nixon 1999; Gurney 1999a,b; Clapham et al 2000; Jacobs and Manzi 2000; Jacobs et al 2003). This was not without

² Sociology, psychology, political science, philosophy, economics *etc.*

consequence for the field of Housing Research. Specifically, it had an unsettling epistemic effect on the dominant 'policy oriented' approach to Housing Research which had long claimed that it was involved in the task of using social scientific methodologies to investigate 'real' housing policy problems and produce 'housing facts' that would inform housing policy (Kemeny 1984). Such a claim could no longer be 'taken for granted' in a Housing Research community that now contained a growing number of researchers working on the social construction of 'official definitions' of housing 'problems' and policy.

My earliest contributions to this emerging body of literature were social constructionist studies of the housing policy aspects of community care (Allen 1997; Allen 1999a,b). Refusing to work with official definitions of community care policy, these papers provide a constructionist theoretical account that locates the housing role in community care policy within wider frameworks of ideological and welfare professional meaning (Allen 1997). However, these papers did not simply serve as a warning of the dangers of working with official definitions. They also opened up a discursive space in which new meanings could legitimately circulate and thereby influence thinking within the Housing Policy and Research community. My comparative constructionist study of housing and community care in the UK and Sweden was key to this attempt to create and mobilise new meanings. It enabled me to re-theorise the literature on 'welfare regimes' using new conceptual classifications derived from the divergent ideological and professional meanings that were attached to the idea of community care in the UK and Sweden (Allen 1991a,b). This provided a new framework of meaning through which the housing role in community care in the UK could be understood.

Initial Contributions to a Sociology of Housing Research

Although the emergence and growth of social constructionist epistemology was unsettling for policy oriented Housing Research, it did not affect its overall health. Conversely, policy oriented Housing Research continued to flourish, especially during the New Labour period (1997-2010) when Housing Researchers embraced the Blairite idea of 'evidence based policy' (Cabinet Office, 1999) and took to their 'fact producing' and 'policy influencing' task with relish (for instance Bramley *et al* 2004). Suffice it to say that, if these Housing Researchers were claiming that their activities were driven by an ontological commitment to the quest for 'housing facts', in the face of the social constructionist critique, then a consultation with the sociology of science literature would suggest otherwise. Specifically, we learn that scientific fields are also social fields in which power relations regulate the context of knowledge production by, for instance, offering or withholding 'credibility', recognition and distinction to scientific work (Latour and Woolgar 1979; Bourdieu 2001). This affects what is produced, how and with whom. Since Housing Research is a social field, like all scientific fields, it follows that Housing Researchers' knowledge production activities cannot be divorced from the 'politics of recognition' that governs activity in the field of Housing Research, which also happens to be embedded in a wider set of consecrating fields (e.g. government, business, university, sociology, geography, policy studies and so on).

Although my earliest concerns were with the social construction of housing and social policy (Allen 1997, 1999a,b), I began to take an interest in the social construction of *the activity of* Housing Research in the late 1990s when I presented a paper on the

social production of housing and health research to a conference in Glasgow (Allen 1997) which was later published in *Housing, Theory and Society* (Allen 2000). My experience of presenting the paper, as well as the sociological analysis of Housing Research within it, brought what Latour and Woolgar (1979) call 'laboratory life' to the forefront of my attention. One of Latour and Woolgar's (1979) key points is that social scientists tend to invest their intellectual capital into laboratory life in a way that attracts credit and recognition. Whilst concurring with this general point about credit and recognition, my own work in this area suggests that there is a need to introduce some nuances to it.

For Latour and Woolgar (1979), pursuit of credit and recognition is generally the concern of individuals seeking to fashion and advance their social scientific career. Yet, my housing and health paper (Allen 2000) and some of my other work on knowledge production (Allen 2005) suggests that pursuit of credit and recognition can equally be the concern of social scientists merely seeking to create a space for themselves within academe. In this sense, it points towards a pursuit of legitimacy in order to survive as much as it might point to the pursuit of self-advancement. Moreover, and crucially, a Latour and Woolgarian reading of my housing and health paper (Allen 2000) would additionally suggest that this need for legitimacy can be collective (i.e. the business of an entire research community) and not simply the concern of individuals.

The core concern of my housing and health paper (Allen 2000) was to understand how the social and intellectual conditions in which housing and health research had

historically taken place had shaped the housing and health hypothesis (i.e. poor housing conditions cause illness) and thus Housing Researchers' understandings of what they were doing. A key argument in the paper is that a historical knowledge of the emergence of multi-disciplinary interests in 'public health' research in the 19th century is axiomatic to understanding the historical formation of the social scientific episteme and the claims that have subsequently been made in its name. Specifically, social science has been historically wedded to a particular 'over-socialised' conception of the human body which is captured in the housing and health hypothesis, i.e. poor housing conditions cause illness. This has led its practitioners to interpret 'recalcitrant data' (that raises questions about the veracity of the empirical relationship between poor housing conditions and illness) in ways that are favourable to its epistemic and hypothetical commitments. So in place of attempting to produce a philosophical answer to the epistemological problems raised by recalcitrant data, Housing Researchers have instead invested their efforts in discursive work to protect the integrity of the housing and illness hypothesis and therefore the legitimacy of social scientific 'interest' in the field of housing and health research. My paper tries to resolve these problems by producing a philosophical answer to the problem of 'recalcitrant data' and, in doing so (though perhaps naïvely when considered in the context of the sociology of science literature) concludes by arguing for a Housing Research that is more honest and humble about its own episteme. This is a theme that I later returned to in the fallacy paper (Allen 2009) and impact paper (Allen 2016).

If the housing and health paper (Allen 2000) was centrally concerned with the social production of knowledge within an inter-disciplinary context of 'paradigm wars' (Kuhn 1962), my other concerns with Housing Research emerged from my own personal

experience of working as a contract researcher within entrepreneurial institutional contexts within the university. I came to address these concerns on two levels; the institutional level and through the lens of academic labour and the academic career. I will address each of these in turn.

First, during my time in the university, the individual pursuit of social scientific credit and recognition has occurred in what Gibbons et al (1994) have referred to as a 'Mode II' context of knowledge production. This is a heteronomous context in which the sources of credit and recognition have expanded beyond the academy to include government departments and other agencies that are, in principle³, 'external' to the university and the social scientific field (see Bourdieu 1996). In contrast to Latour and Woolgar's (1979) classic sociological account of 'the construction of scientific facts' which emphasises the internal governance of a relatively autonomous scientific field, then, Gibbons et al (1994) draw our attention to the emergence of the business university (Allen and Imrie 2010) and academic capitalism (Slaughter and Leslie 1997) where the individual search for social scientific credit and recognition takes place in a context that rewards the ability to secure 'external funding'. As I describe in an ethnographic account of entrepreneurial-university.co.uk (Allen and Marne 2010) in my book, *The Knowledge Business* (Allen and Imrie 2010), this has led to a situation in which the economic value of Housing Research has become paramount. A key concern of my ethnographic account is to understand how this institutional economic prerogative has become an individual academic prerogative. This becomes apparent in my discussion of the institutional organisation of entrepreneurial-university.co.uk.

³ It should be noted that representatives of government and business are increasingly found 'inside' the university in the form of Visiting Professors and the like (Allen and Imrie 2010).

This describes how the entrepreneurial university has created 'disciplinary' and 'inspecting' physical environments and bureaucratic mechanisms that seek to mould the subjectivities of its functionaries in ways that are consonant with its business ethos. Interestingly, the ethnographic case study reveals a university that shows signs of successfully producing entrepreneurial academic dispositions in harmony with its own institutional requirements. So successful, in fact, that many of the entrepreneurial academics it has created show as much enthusiasm for the research business as the practice of research itself.

This institutional ethnography is informative but, taken on its own, insufficient, because it fails to give an autobiographical insight into the entrepreneurial academic. Thus the second level at which we must learn about the entrepreneurial context of knowledge production is that of the academic labourer. My paper on the social relations of contract research production (Allen 2005) was directed to this task. This paper draws our attention to the problem of reflexivity across the social sciences. I suggest that, too often, academic reflexivity is exhibited within the context of individual research projects e.g. through diary keeping. Yet our subjectivities as researchers are shaped over the course of a career rather than individual projects. I acknowledge that this omission is partly addressed in discussions of positionality which address themselves to autobiography. However, like Bourdieu (1977 2000), I am concerned that these discussions of positionality often address the societal (class, gender *etc.*) rather than institutional (university, business) contexts of academic autobiography and practice. Hence the paper provides an auto-ethnographic reflexive discussion of an academic career spent subject to the 'disciplinary gaze' of research funders. I discuss how the

politics of recognition (or, rather, 'satisfaction'⁴) *in an entrepreneurial context* has shaped my own attitudes towards, and practices of, knowledge production. Specifically, I show myself to be eagerly entrepreneurial but, in doing so, reveal another layer of complexity which shows that any characteristic enthusiasm for the business of research needs to be placed in its autobiographical context.

That said, it is important to note that the politics of recognition is complex and not simply the result of naked self-interest and careerism (or even docility) even though it might be misunderstood as such. For instance, Atkinson and Jacobs (2009) provide a summary of my auto-ethnographic account of a career subject to the 'disciplinary gaze' of research funders (Allen 2005) which concludes that:

“Writing so candidly about his experiences in the research process in this way should be welcomed. In particular, Allen’s account makes more explicit how research activity is also a political activity in that it is strategically used by academics as a way of securing promotion and collegial respect” (Atkinson and Jacobs 2009: 243)

There is no doubt that Housing Research is 'strategically used' to secure respect and recognition. This is why academic researchers make their intellectual investments where credit is available, and also why we can see a shift in the focus of intellectual investment activity over time – as currencies change. So whereas Latour and Woolgar's (1979) academics made investments in the relatively autonomous field of scientific production rather than outside of it, Gibbons et al (1994) and a host of other

⁴ My paper talks about producing research that 'satisfied' funders. This then enabled me to secure future streams of funding. Thus one element of a meta 'politics of recognition' – in an entrepreneurial context - is a 'politics of satisfaction'.

writers have noted how contemporary social scientists now make investments in activities 'external' to the field of social scientific production where newly salient forms of credit and currency are to be found, e.g. enterprise activities such as 'knowledge transfer partnerships' (Allen and Marne 2010). Nevertheless, it is important to note that investments in pursuit of recognition may be made for a variety of reasons beyond naked self-interest, and this is one of the things that the auto-ethnographic account of my research career (Allen 2005) was seeking to highlight in the context of power relations and positionality. A key point in this paper is that the neoliberal university is a site of extreme job insecurity and that this is especially the case for 'early career' and working class academics trying to maintain a foothold in the academic labour market (Allen 2005). It follows that intellectual and other investments in organisational life are often made for reasons of basic survival rather than, simply, credit or recognition in the service of self-advancement. As I suggested earlier, then, they can be made to secure legitimacy in the field so that one's craft may be practised - which may be for political rather than personal reasons⁵. Suffice it to say that Gorz (1999) and Frayne (2015) would point out, and I would concur (Allen 2005; Allen and Marne 2010), that the manufactured insecurity of academic labour only makes it easier for employing organisations such as universities to colonise the lifeworld of academic labourers and, in doing so, govern their subjectivities to the point of even shaping their intellectual outlook and values⁶.

⁵ My own reasons were highly political and relate to my class background and upbringing as well as passionate interest in inequality.

⁶ This point might have greater application to the early stages of an academic career than its later stages. For instance, my 'contract career' paper (Allen 2005) addressed my 'early career' phase in which entrepreneurial compliance was important to survival. As I explain below, my later career has been very different.

Part II: Epistemic Limits and Phenomenological New Beginnings – An Autobiographical Turning Point

My initial concerns were to draw attention to, and generate understandings of, the new contexts in which knowledge is being produced within Housing and Urban Studies. The inescapable conclusion of the knowledge studies that I have included in part I of this thesis (Allen 2000 2005; Allen and Marne 2010) is that an epistemological scepticism of academic text is a requirement since texts can only be properly understood when located within the context of their production (Latour and Woolgar 1979; Bourdieu 2000). This consigns all work, including my own, into the category of contingency.

As I approached the mid-2000s, I began to develop new understandings of the nature of this epistemic contingency. As a ‘critical’ social scientist, I had initially held the view that sociological knowledge was problematic *but necessary*. Sociological theory is, after all, our route to critique. This began to change when I was researching the housing and urban experiences of visual impaired children and the urban phenomenon of heroin use in the early and mid-2000s. These two pieces of research threw up unique fieldwork challenges (which I explain more fully below) that led me to question the fundamentals of critical social science and taking a sociological theoretical point of view; Sociological theory was hindering my attempts to develop an adequate knowledge and understanding of the everyday urban experiences of the visual impaired children and heroin users. I initially tried to make intellectual sense of these theoretical problems in a series of papers (Allen 2004a; Allen 2004b; Allen 2005c) and a book (Allen 2007) where I identified some limits of the sociological episteme and

developed a phenomenological perspective of the specific experiences of visual impaired children and drug users. These papers appear in part II of the PhD thesis. They mark a phenomenological new beginning which set me on an intellectual track that culminated in my 'fallacy paper' (Allen 2009) and *Housing Market Renewal and Social Class* book (Allen 2008) which form key publications in part III of this PhD.

Towards a Phenomenology of Urban Experience: Disability and Drug Use

Bourdieu (2000) argues that an objectifying and reflexive attitude to the social world is an epistemological requirement of social science and axiomatic to its ambition to construct a form of theoretical reasoning that takes us beyond 'common sense' reasoning (see also Berger and Luckmann 1966; Francis and Hester, 2004). However, strengths can also be weaknesses; theoretical reasoning can equally hinder our understanding of the social world. This hindrance was the intellectual problematic that led to my papers on visual impaired children and drug users which were concerned with the lack of access that social science theory and methodology gave me to their experiences, and which led one respondent to say "I don't know what you're trying to establish" (Allen and Milner 2004). Specifically, the theoretically reasoned research questions that I was asking in the field during both of these research studies were failing to elicit 'satisfactory answers' or, indeed, any answers at all. On hearing my questions, then, drug users were unable to provide contextualised accounts of their drug use that contained 'reasoned reasons', i.e. answers to 'why' questions (Allen 2005c, 2007). They 'just did it'. The children in my visual impairment study had similar difficulties in providing me with a 'reasoned' account of the social nature of their

problems with housing and the built environment. It was just 'there' – a feature of an everyday life that simply 'happened' (Allen 2004a).

My task now was to understand why social science theory and methodology had been so inadequate in my attempts to understand experience. Some of the classic critiques of sociological theory and method have been provided by Garfinkel (1967) in 'Studies in Ethnomethodology' and Schutz (1972) in 'The Phenomenology of the Social World'. Although ethnomethodology provides a useful critique of social science theory and method, its emphasis on the *methods* that people use to create and maintain local order (e.g. uncovered through 'breaching experiments') was less useful to my concern with how the social world is experienced and felt. I began to engage with phenomenology because it emphasised the limits of social science that I was concerned with *and* the requirement to understand the 'primary experience' of the world, i.e. as it is encountered by participants prior to the arrival of social scientists 'on the scene' (Merleau-Ponty 1962).

My phenomenological turn initially took two forms. First, following Merleau-Ponty's (1962) discussion of the body-in-space, I began to grapple with the idea that knowledge of housing and the urban environment was corporeal rather than merely intellectual and reasoned, i.e. the result of a body that inhabits and knows space through its intentional everyday activities without needing to intellectually comprehend it. That is to say, it is a body that appropriates space 'in the hands' and 'in the legs' through time-space routines which sediment into a corporeal schema that exists beyond everyday consciousness (Allen 2004a). On undertaking a phenomenological

re-examination of the 'data' that I had collected from visual impaired children, then, I began to understand that it pointed to the existence of corporeal schemas that left visual impaired children unable to account for their experiences of housing and the built environment with the 'reasoned reasons' that I was originally asking for (Allen 2004a; see Seamon 1979 for a similar problem of accountability in 'able bodied' people). Their knowledge of housing and the urban environment was simply sediment in their phenomenal body which, accordingly, was frequently unable to account to me for its actions and inactions. This led me to raise a series of questions about the sociological problematisation of experiences of disability and impairment (Allen 2004a,b).

The second form that my phenomenology took was concerned with the use of language and, in particular, social scientific language. In my *Housing, Theory and Society* paper on the phenomenology of urban deprivation (Allen 2005c) and my book *Crime, Drugs and Social Theory: A Phenomenological Approach* (Allen 2007), I was particularly concerned with the social scientific approach to accounting for urban behaviours such as drug use which I suggested was based on asking 'why' questions and eliciting 'reasoned reasons'. Moreover, I was concerned with the way in which social scientific language, which is produced at a social and epistemic distance from urban phenomena as they occur, provides limited access to, and understanding of, urban phenomena. A key purpose of these two publications, therefore, concerned the assigning of articulatory authority to the experiencing subject. First, I suggested that this should entail a relinquishing of the social scientific demand for 'satisfactory answers' from respondents, which is an explicit, if not implicit, requirement in the social scientific quest for 'reasoned reasons' (Bourdieu 2000). Thus I began to understand

drug users answers to 'why' questions about their drug, such as 'I just do it', as the utterances of an unselfconscious habitus that was offering access to the primacy of its lived experience. I no longer considered them to be prompts for 'probing' questions (such as 'when you say "I just do it" what do you mean by that?'), which are the stock-in-trade response of a 'normal' social science which forgets that:

“The practical privilege in which all scientific activity arises never more subtly governs that activity (insofar as science presupposes not only an epistemological break but also a social separation) than when, unrecognised as privilege, it leads to an implicit theory of practice which is the corollary of neglect of the social conditions in which science is possible” (Bourdieu 1977: 1)

Second, it also entails allowing everyday speech forms that emanate from the density of lived experience to 'speak for themselves' in social scientific text rather than through the 'sugar coating' of a social scientific language that, as Gadamer (1975) suggests, hermeneutically 'flattens' and disfigures experience. This prompted me to pay attention to the way in which my own use of social scientific concepts and language, explicitly in text and implicitly fieldwork situations, constituted 'linguistic violence' because they hoovered up the 'primary meanings' that respondents gave to me and then re-presented them in a social scientific language that violated those meanings. An example of this occurring was when I probed respondents to account for their drug use ('when you say 'just happens' what do you mean by that?') rather than allow myself to encounter and accept their descriptions of drug use 'in the thick of it' where it 'just happened', i.e. in a language that gave access to the primacy of experience. Respecting the social scientific 'authority' of the interviewer, respondents tended to acquiesce to these probing invitations to provide new meanings to their practices ('I

suppose I was really doing this or that') even though they constituted a violation of the primary meanings that they had already given to me of things 'happening'. The point now was to avoid such 'misunderstandings'.

Part III: From a Phenomenology of Conflict to Reconciliation

A key characteristic of my work on visual impaired children and drug users was the epistemic conflict that lay at the heart of the relationship between social scientists and their respondents. A key aim here was to question the veracity of social scientific understandings of the social world and, in doing so, open up a discursive space in which alternative understandings could articulate themselves. This continued in my book *Housing Market Renewal and Social Class* (Allen 2008). The social scientific context for the book is three-fold. First, it was set in the context of a research community that had become deeply divided over the policy of Housing Market Renewal. Second, it makes a phenomenological argument that 'class matters' and how this is the case. Third, it articulates phenomenological perspective on working class experiences of urban regeneration.

Housing Market Renewal and Social Class: A Study OF and IN Conflict

Taking the issue of research context first. My *Housing Market Renewal and Social Class* book (Allen 2008) was published during the New Labour era when 'evidence based policy' was in vogue. During this period, there was an abundance of government funding for Housing Research and Housing Researchers were generally embracing it. This was especially the case with the government's 'Housing Market Renewal Programme' (HMRP) which was marketed as an evidence based programme

par excellence (Cole and Nevin 2004; Leather *et al* 2007). Moreover, HMRP took the idea of 'evidence based' policy to new levels (Webb 2010 2012). Housing Researchers were now not only providing 'independent evidence' to inform government policy. HMRP had also been devised by Housing Researchers, and was now even being evaluated by the very same Housing Researchers (see Cole and Nevin 2004; Leather *et al* 2007). This was not merely a temporal transgression of Weber's sacred line between science and politics (Weber 1946). It was tantamount to setting up camp on the other side of the fence.

During the period in which HMRP was being 'rolled out', I had undertaken research into the existential relationship between people and housing in HMRP areas of Liverpool and Salford. I was analysing the data from this research when Lee Crookes, a PhD student from the University of Sheffield, approached me – then a University Professor - about giving evidence to a public inquiry that was taking place into the issue of Compulsory Purchase Orders (CPOs) on working class people living in HMRP areas of Liverpool that I had studied. Having been analysing my data on working class experiences of housing in these areas I was sceptical of HMRP, but had not made any commitment to become involved in the public inquiries. However, I was horrified to hear what Lee told me about the working class experience of the public inquiry⁷ and so indicated that I would give evidence at the public inquiry in support of a community that was facing the prospect of losing home. As I threw myself into the now urgent task of completing my analysis of the effects that HMRP was having on working class

⁷ Lee Crookes' claims were verified by my own subsequent involvement with, and observations of, public inquiries in Liverpool and Oldham.

people I realised that my evidence would need to be in outright opposition to the CPO for reasons that I gave in the book that followed.

'News' quickly circulated throughout the Housing Research field about the 'confrontation' that had taken place at public inquiry between myself and the Housing Researchers that were advocating for HMRP and CPO⁸. My *Housing Market Renewal and Social Class* book, which was then forthcoming, subsequently became a topic of much discussion, disagreement and even controversy⁹ within the Housing Research and Policy community. This shaped its conditions of reception. Moreover, there was already an intellectual¹⁰ and institutionalised¹¹ divide between 'critical' and 'mainstream' Housing Research (Hamnett 2010, Webb 2012) which had led to intense disagreements and controversy within the research community over the heteronomous involvement of Housing Researchers in HMRP¹². This meant that most responses to my hostile book fell on one or other side of the mainstream / critical dichotomy¹³.

⁸ Newspapers even wrote stories about my confrontations with these Housing Policy Researchers (Qureshi 2009; Corbyn 2009).

⁹ My publisher, Routledge, was threatened with legal action by one of the architects of HMRP and a HMRP partnership in advance of publication. From the letters received, the intention behind the legal threat seemed to be to acquire an advance copy of the book in order to influence its contents before publication. (A similar intervention was later made in relation to a PhD thesis). This was refused and, in the event of publication, the action did not materialize. Throughout this period, however, I received threatening emails warning me to "behave".

¹⁰ For instance, between 'theory' and 'policy' oriented research.

¹¹ For instance, in journals, conferences and academic networks.

¹² For instance, Housing Researchers occupied government positions and government consultants were appointed to visiting professorships (Allen and Imrie 2010).

¹³ An exception to this is Flint (2010) who produced a critical appreciation of my arguments about HMRP.

Housing Researchers that had been closely involved in devising and implementing HMRP produced dismissive accounts of my position that did not entirely conform to the conventions of normal academic discourse (Sprigings 2010; Cole 2008). My 'name' was damaged by these interventions and I became marginalised within Housing Policy research networks where even long standing friends began to dis-identify with me. On the other hand, my reputation and credit-rating simultaneously grew in the 'critical' Housing Research community where I became something of a cause celebre. I received numerous invites to speak and write in 'critical' spaces that may never have materialised without the interest that my book had attracted. Moreover, on publication *Housing Market Renewal and Social Class* was well-received by critical urbanists and has since been named in a Top 10 of Urban Studies Books in the journal *Urban Geography*. Although I was marginalised in the Housing Policy Research community, then, I was suddenly in email contact with Loic Wacquant and Neil Smith.

Moving onto the content of the book and, in particular, the issue of class. The book was written during a time when working class studies were losing currency in sociology (Skeggs 2004; Charlesworth 2000) and Housing Research (Allen 2010b). Sociology was becoming dominated by concerns with self-identity, reflexivity and consumption whereas Housing Research was preoccupied with new patterns of housing consumption. To some extent, this reflected the intellectual influence that Anthony Giddens had exerted over sociology from the mid-1980s but also the effects of the New Labour consumer boom in the housing market. This context is key to understanding the argument in *Housing Market Renewal and Social Class*.

A core argument in the book emerged from my ongoing auto-ethnographic (Allen 2005) and ethnographic (Allen 2008b; Allen and Marne 2010) studies of Housing Policy Research which were facilitated by my then institutional proximity to it. This enabled me to emphasise how the social and scientific positionality and habitus of Housing Researchers was governing their 'interest' in the market for houses in highly specific ways: As social scientists that had a social and epistemic proximity to the 'housing market' but were socially and spatially separate from HMRP neighbourhoods, Housing Researchers took a panoramic 'interest' in the market for houses within and between urban neighbourhoods (which they only knew from the epistemic distance of the panoramic 'scientific gaze') as a space of positions and position taking, e.g. they were interested in 'market behaviour', 'market change', 'neighbourhood change', 'housing aspirations' and so on (Allen 2010b).

In this relational schema, urban neighbourhoods were subjected to quantitative and qualitative comparison and then judged with reference to what the panoramic and normalising scientific gaze revealed as 'normal' in economically 'successful' neighbourhoods. Economically 'failing' neighbourhoods were thereby condemned for 'lacking' in features that made the housing market in a 'normal' neighbourhood 'functional'. Specifically, they were considered to be 'lacking' in housing types that would attract middle class and 'aspirational' households. Hence they required 'housing market renewal'. Suffice it to say that this way of thinking about the market for houses was so inscribed into the epistemic mentalities of HMRP researchers, which was revealed in their language and discourse, that I described it as a specific form of market 'doxa' (Allen 2008, Allen 2008b, Allen 2010b). My key problem with it was this: Given that this doxa was cultivated 'outside' of working class experience, it

was precluding a class perspective of the market for houses that was germane to everyday life within working class neighbourhoods.

If Housing Research was failing to offer a working class perspective of neighbourhoods then, following Skeggs (1997 2004), I argued that sociology was faring no better. In her book *Class, Self, Culture*, Skeggs (2004) made a phenomenological argument that class intellectually mattered less to sociology because it personally mattered less to sociologists. In her argument, which contained shades of standpoint epistemology (see Harding 1991) and existential phenomenology (Schutz 1972), Skeggs not only emphasised the inseparability of social science from experience but also the idea that the former is parasitic on the later (Gadamer 1975). Following Bourdieu (1977 2000) she argued that most sociologists embodied a middle class habitus; a scheme of perception that not only emerged from and normalised their own relation to economy, culture and consumption but also reified them. In this scenario, the world as it appeared to sociologists *in experience* was tacitly being mistaken for the world as it was *for others*. This world was all about self-identity and consumption as, indeed, a consultation with the sociological literature would verify.

I proceeded to argue that this sociological preoccupation with 'lifestyle politics' (Giddens 1990) was having consequences for the way in which working class people were appearing in sociological and urban literatures. Whereas the classic sociological accounts of working class life and culture had emphasised *what it was* (Wilmot and Young 1962; Willis 1977; Williams 1957) working class people now began to appear

in the sociological literature as 'failed consumers' and objects of disgust (e.g. Tyler 2008), i.e. in terms of *what they were lacking* and thus *what they were not*. This also applied to the urban gentrification literature which had become preoccupied with the urban lifestyles of a middle class that it celebrated (Slater 2006). Working class people now began to appear in this literature as beneficiaries of the middle class gentrification of neighbourhoods that had previously been 'lacking' (see Freeman 2011) or, in the more 'critical' literature, as victims of displacement (Slater 2006). Either way, working class people were not appearing as human agents in the housing market in their own right, that is, as people that were actively involved in creating their own forms of housing consumption outside of the dominant market emphasis on the space of positions and position taking. Put simply, working class experience and culture was ceasing to exist in its own right (Charlesworth 2000).

I returned to my phenomenology to overcome these problems and thereby seek to recapture working class experience. My key point was that the urban experience of working class people could not simply be understood *in relation to* a social scientifically normalised view of housing markets or urban lifestyle – however benevolent the intent of such an enterprise may be. Beyond the relational politics of social positioning, to 'be' working class was *primarily* an existential matter and needed to be addressed as such, i.e. as a specific form of being-in-the-world that was able to speak-for-itself as well as relationally. Now although a defining characteristic of urban working class existence is proximity to economic necessity, this does not mean that working class people define or mark themselves out in terms of what they 'lack' in resources and thus do not have. Conversely, the understanding that I reached as a result of my own phenomenological research was that a life lived at proximity to necessity shaped

working class subjectivities in specific ways; resulting in a form of being that *actively* grasped and *shaped* the world around it in ways that were specific to the urgent necessities that governed its existence and conditioned its being.

This drove to the heart of a key argument about class in my book, which was this: It is important to 'get inside' the 'being' of working class people in order to understand their *primary* experience of housing, home, neighbourhood and so on, i.e. *before* and *outside* of theirs or any attempts to relate their housing practices to those of middle class people or, indeed, social scientific schemas. In attempting to be faithful to working class 'being' as it presented itself to me, the book therefore developed the idea of a 'practical economy' of working class housing consumption in which people primordially relate to the social world through the necessities that govern their own practical existence rather than in relation to other idealised lives and lifestyles. Housing, home and neighbourhood were 'dwelling space', that is, 'spaces of being' rather than spaces of lack, deficiency, absence or even accumulation.

This brings me to the third issue, which is regeneration. Policy makers usually represent urban regeneration as a 'win-win' scenario that is in the 'public interest' (Slater 2006; Allen and Marne 2010b). This was certainly the case with the urban regeneration programme under consideration in the book. The Housing Market Renewal Programme (HMRP) emerged out of a series of academic studies of 'low demand' for housing in urban areas throughout the Midlands and North of England (see especially Nevin et al 1999). The authors of these studies argued that housing markets were 'failing' in some urban areas because the demand for housing was low

and falling. This was deemed to be problematic for a range of reasons; two of which will be mentioned here. First, low demand for housing was associated with disinvestment in the housing stock and therefore neighbourhood decline. Second, low demand was associated with low prices and therefore growing inequalities between those living in these areas and people living in areas where prices were rising. This second problematic was deemed to be especially salient in a context where housing wealth was growing as a proportion of overall household wealth (Forest and Murie 1995). HMRP addressed itself directly to these problems and, in doing so, aimed to 'fix' housing markets that were 'failing'. Key instruments that were used in this respect were demolitions of 'outdated' and 'unwanted' housing (which required the issuing of CPOs on people that refused to leave their homes) and the erection by volume builders of 'high value' homes that were said to be more suitable for 'modern and contemporary living'. Perhaps strangely, in the context of a housing affordability crisis, the ultimate objective was to increase house prices.

In some ways, the stated aims of HMRP were laudable. Who could object to a programme that promised to tackle growing wealth inequalities? However, the phenomenological class perspective in my book threw a different light on it. I argued that HMRP was not a technocratic 'fix' for 'failing' housing markets that was in the 'public interest', as its adherents claimed. HMRP was political. Specifically, it was the product of a market doxa that constituted and naturalised the market for houses as a 'space of positions' and 'position taking' in which neighbourhoods were comparatively understood according to key features such as price levels, housing infrastructure and reputation. Within this schema, HMRP areas were seen as 'lacking' (in attractive house types) and 'failing' (in price competitiveness). Put sociologically,

then, the problem that HMRP was seeking to address was the 'lack', 'failure' and resultant disconnection of some neighbourhoods from the space of positions because it was inhibiting position taking.

Far from accepting the policy view that HMRP was in the 'public interest' and in the interest of working class people living in 'failing' neighbourhoods, the phenomenology of working class housing practices presented in my book raised a wide range of serious questions about HMRP. I will mention two at this point. First, my phenomenology of working class experiences of 'failing' neighbourhoods suggested that the objective indicators of decline produced by social scientists (e.g. low prices, higher than normal vacancy rates) were irrelevant in the context of the practical economy that governed working class relationships to housing, home and neighbourhood. Most people went about their everyday lives in these neighbourhoods, and described doing so, in largely unproblematic ways. As such, they were often bemused by objective descriptions of their 'failing' neighbourhoods and would point out that they had been erroneously produced at a social scientific distance from the 'reality' of their neighbourhood lives. In other words, they located 'failure' in the descriptions of their neighbourhoods rather than the neighbourhoods themselves.

Suffice it to say that working class residents were not merely concerned to contest social scientific descriptions of their neighbourhoods. Perhaps more importantly, they also had something to say about what made their houses, homes and neighbourhood valuable *to them* and how they had created this value in their own practices. This was contained in their constant references to the 'lived value' of their homes and

neighbourhoods, which spoke of a practical economy of housing consumption that valorised dwelling as a space of being and a space for the realisation of being. These were not people that were overly concerned with their house prices or objective indicators of decline, then. Moreover, they had no interest in the rhetorical HMRP idea of building 'high value' or 'modern and contemporary' homes which, in fact, bemused them. They were people that were busily engaged in the urgent task of attending to the necessities of everyday life whilst also being knitted into the fabric of each other's lives in the 'lived spaces' and spaces of being of their homes and neighbourhoods. For all of these reasons, the proposed housing demolition programme made no sense.

Knowledge of this did not deter the advocates of HMRP who saw regeneration in narrow technocratic terms. This became particularly apparent in the debate that took place about the affordability of the new 'modern and contemporary' houses that would take the place of the demolished homes. Under questioning at one of the Liverpool Public Inquiries, a volume builder stated that the *average* selling price of the new houses would be £125,000. He argued that this was "affordable" because a couple earning £22,000 each (which was the average income *in the city* at the time) could, at a multiple of 3.5, purchase a house for £154,000. Yet 57% of households in my case study areas had a household income of less than £10,000 per annum and a further 16% of less than £15,000 per annum. In other words, the price of houses in the new dwelling scape was going to be far beyond what people already living there could afford. Yet, this technical issue of the affordability of the new homes was not the point. Working class people generally did not want to be stretched to the outer limits of affordability, even if the new houses were technically affordable to them which, as the above calculations indicate, they were not. From the phenomenological point of view

outlined in my book, the more fundamental issue concerned the violation of working class ways of being toward housing by HMRP. This violation was happening because HMRP was seeking to eliminate 'low value' housing stock from urban areas in order to reconnect 'failing' neighbourhoods to the space of positions. In doing so, HMRP was forcing working class people to participate in the space of positions and, as such, to view their homes, houses and neighbourhoods as positioned within the space of positions; not least because of the level of financial investment that was now required to buy a house. As such, it was eradicating the possibility for working class households to make their choice to prioritise dwelling over consuming and, with it, the practical economy of housing consumption. That is, it was wiping a whole way of 'being-in-the-city' off the urban map.

Ways Forward: From a Phenomenology of Conflict to Reconciliation

Having been through the experience of publishing *Housing Market Renewal and Social Class*, which included attendance at Public Inquiries where social science played such a key role in silencing residents voices, I now felt compelled to produce a fundamental philosophical critique of Housing Studies. The 'fallacy paper' (Allen 2009) was published in 2009 and was written amidst the debates that were occurring about HMRP and my book. The paper constitutes a philosophical attack on the legitimacy of Housing Research and uses HMRP as key case study material. As the paper explains, I deemed this necessary because Housing Researchers had used their status and work as social scientists to claim – in the forum of public inquiries and elsewhere - that they had produced a superior knowledge of neighbourhoods to that held by people that lived in them. In fact, this is what ultimately justified the planning inspectors'

decision to issue CPOs in Liverpool (Grainger 2006). Yet, as the phenomenological narrative in *Housing Market Renewal and Social Class* suggested, such claims to superior insight are contingent and certainly held little weight inside the neighbourhoods themselves.

Suffice it to say that the point of the paper was not to go over old ground. It was to lay down a philosophical challenge to Housing Research in the hope that researchers would be forced to confront the basis of their own *a priori* claims to knowledge and knowing. Hence, I extended the argument well beyond HMRP in order to emphasise its general applicability. As such, the paper provides a comprehensive philosophical critique of the social scientific idea of “Housing Studies” drawing on the phenomenological insights of a range of scholars, most notably Hans Georg Gadamer, Martin Heidegger, Michael Lynch, Maurice Merleau-Ponty, Alfred Schutz and Peter Winch. Their insights enabled me to raise fundamental issues about the social scientific nature of Housing Research, which I call into question on the phenomenological grounds that a Comte-like ‘scientific’ master view is beyond the capabilities of Housing Researchers *who* can only, after all, relate ‘being-ly’ to housing (Heidegger 1988). The implication of this is that the development of a scientific master view (of, say, housing markets or neighbourhoods) cannot be task of Housing Research. The research task, rather, can only really be to understand how our being (as social scientists or residents of a neighbourhood) makes housing intelligible to us.

The unavoidable conclusion of this philosophical argument was the need to dispense with the elitist idea of intellectualism and to work instead with the Gadamerian and

much more democratic idea of intelligibility which “implies the end of ‘Housing Studies’, at least insofar as it has a philosophical justification for constituting itself as such” (Allen 2009: 72). This allowed me to propose a Gadamerian conception of ‘housing studies’ in which Housing Researchers were “well within their rights to continue to do what they do and to make their own arguments about housing, [but] they cannot constitute what they *do* and what they *know* as superior to ‘what everybody knows’” (Allen 2009: 74)¹⁴. They now needed to “afford equal recognition to the knowledge claims made by others” (Allen 2009: 74). As such, the Fallacy paper opened up intellectual space so that ‘ordinary’ and ‘everyday’ understandings of housing could legitimately impose themselves. Had this occurred during the process of implementing HMRP, it might have saved a lot of pain and suffering.

My “getting out there and impacting paper” (Allen 2016) was published after I had taken a ‘career break’. The paper emerged from a great deal of reflection on the conflict that characterised social scientific debates about HMRP and, as such, represents an attempt to develop, out of my phenomenology, a reconciliatory and collaborative approach to housing research and urban change. The paper begins with a point of self-critique: Although the debate about HMRP was rancorous on all sides (Webb 2010) and resulted in my receiving threatening emails (Allen, 2010), the paper openly acknowledges my own role in these conflicts. My specific contention is that my ‘fallacy paper’ pitted social science knowledge against everyday forms of knowing and, in doing so, reproduced the epistemic conflict between them rather than sought a

¹⁴ At this point it is relevant to note that my use of capital letters to refer to Housing Research is a reference to its institutional form and the epistemic superiority on which it is based. My reference to housing studies is indicative of the more democratic research system that I would like to see that contained no such hierarchy.

resolution. Moreover, as Webb (2012) suggests, my 'fallacy paper' was "forceful" in emphasising the conflictual basis of these different forms of knowing. He is correct: The fallacy paper was written in a context in which residents' everyday knowledge of housing and neighbourhoods was being patronised by Housing Researchers, so it sought to 'turn the tables' by 'attacking' the foundations of the social scientific episteme.

Having made this admission, I set out to achieve some sort of reconciliation in the main body of the paper. I seek to achieve this reconciliation within the context of the REF 'impact' schema, which seeks to assure the relevance of sociological knowledge to society. My starting point is to acknowledge the fractious division of Housing Research into 'mainstream' and 'critical' approaches (Webb 2012) which, as I indicated above, can shape the conditions of knowledge reception in unhelpful ways. I suggest that the answer to the problem of knowledge relevance is neither to be 'critical' or 'mainstream' - not least since the adoption of such stances can create and perpetuate unnecessary conflict. Looking at things phenomenologically, I suggest that there are deeper commonalities between critical and mainstream Housing Research which make them both problematic. A key commonality between them is the idea that there is an 'inside' and 'outside' of social science. Both critical *and* mainstream Housing Researchers position themselves on the 'inside' seeking to impact the social world 'outside' using the theoretical and methodological instruments of their social science. In this schema, the publication of social scientific text is idealised as a "moment of rupture" that provides the catalyst for housing and urban change 'out there'. As such, they both maintain a dualism between social science and everyday knowledge, regardless of how 'critical' or anthropologically 'out there' they are.

Now any attempt to overcome the dualism between social science and everyday forms of knowing requires an intellectual attitude of humility which, in turn, implies acceptance of the phenomenological critique of social science. This requires us to move *beyond social sciences* such as Housing Research in order to embrace the phenomenological idea of a 'housing studies'. In fact, it is a prerequisite. However, we already know much about this (Allen 2009). The problem now is that it is one thing to scale back social scientific knowledge claims but quite another to know how to engage with the knowledge claims of others. Thus we require guidance on how to construct a more reconciliatory and collaborative social science. To begin this process, the paper turns to anarchist social thought. In doing so, it seeks to reconfigure the objective of housing research away from the task of seeking 'theoretical clarity' and 'winning' arguments and towards the 'double hermeneutical' (Giddens 1984) idea of an ongoing dialogue between academic housing researchers that are pre-figuratively embedded in the world, rather than 'outside' of it, and those that are living struggles. In this schema, text becomes a multi-authored aspect of a dialogical 'journey' which is undertaken on 'common ground' rather than another site on an intellectual battleground. It should serve to inspire whilst also being humble about its shortcomings.

The paper gives examples of how such a pre-figurative academic housing research can be developed in practice. It shows that many social scientists are already familiar with ideas of the co-production of knowledge and text, which represents welcome progress towards a more reconciliatory and collaborative practice. However, it also

notes that knowledge co-production seems to be the limit of the mainstream social scientific ambition as it stands. Moreover, *it is limited*: Although the academic tendency is to locate the agency for housing and urban change in text, anarchists would point out that change is not all about text – no matter how democratically it is authored. It is for this reason that my paper also introduces the idea of ‘solidarity action research’ which presents the much more fundamental challenge about ‘living on common ground’ and is less familiar to Housing Researchers. In a nutshell, it asks Housing Researchers to think about the context of their own lives and how they fit into the research they undertake. This might sound personally intrusive but it is certainly consistent with feminist thinking on positionality (Harding 1991) and also Gandhi’s call to ‘be the change you want to see in the world’ (Vinthagen 2015). It is also consistent with Castree’s (2000) Bourdieusian plea for social scientists to liberate themselves from their own privileges by re-embedding themselves in everyday contexts. As the respondents in my *Housing Market Renewal and Social Class* book frequently said ‘how can they say that about my area unless they live here?’¹⁵

Although the ‘getting out and impacting’ paper (Allen 2016) has been an important rejoinder to my fallacy paper (Allen 2009) it represents one part of a wider programme of work I am now pursuing which is building a collaborative, reconciliatory and nonviolent approach to housing research and housing practice. This programme of work is emerging out of my phenomenological critique of social science *and* my current interest in bringing hitherto different and even hostile forms of knowledge together. These forms of knowledge are disciplinary (e.g. a new interest in theology) as well as

¹⁵ This could be taken to imply that Housing Researchers are not ‘well within their rights to continue doing what they are doing’.

experiential (e.g. my longstanding interest in primary experience). One of these papers in this current programme of work examines the ontological and epistemological contours of the 'versus' relationship between social science and theology (Allen 2017a). This was a complex task but it enabled me to find a space in which social sciences might collaborate with theologians and, in doing so, produce a more compelling voice for social change. Another paper is producing a comprehensive theoretical examination of the violence of housing policy (Allen 2017b). Building on my phenomenological work, this paper points to the limits of social science which has the epistemic tools to identify violence (albeit, I claim, randomly) but lacks those to map a route out of it. To achieve this, I engage with other disciplinary traditions (especially theology) and literatures (such as nonviolence). When a nonviolence perspective is posited in this way, housing policy debate begins to look very different to the way it is currently argued between critical and mainstream perspectives. A collaborative and reconciliatory approach becomes a perforce necessity.

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Appendix

List of Published Works Submitted

Part I: For a Sociology of Housing Policy and Research

Allen C. 1997. The Policy and Implementation of the Housing Role in Community Care - A Constructionist Theoretical Perspective, *Housing Studies*, 12(1), pp85-110 <https://doi.org/10.1080/02673039708720884>

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