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**UNIVERSITY OF SOUTHAMPTON**

FACULTY OF SOCIAL, HUMAN AND MATHEMATICAL SCIENCES

Sociology, Social Policy and Criminology

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**History, Drawing, and Power: Essays towards reflexive methodological pluralism in  
sociology**

by

**Rachel Elizabeth Ayrton**

Thesis for the degree of Doctor of Philosophy

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UNIVERSITY OF SOUTHAMPTON

## **ABSTRACT**

FACULTY OF SOCIAL AND HUMAN SCIENCES

SOCIOLOGY

Thesis for the degree of Doctor of Philosophy

### **HISTORY, DRAWING, AND POWER: ESSAYS TOWARDS REFLEXIVE METHODOLOGICAL PLURALISM IN SOCIOLOGY**

By Rachel Elizabeth Ayrton

There is growing consensus around pluralism as the orientation of the social sciences in general, and British sociology in particular, towards research methodology. However, the profession of methodological pluralism, is not always apparent in research practice. This thesis seeks to explore the dimensions of a truly pluralistic sociology. It takes as its starting point Pierre Bourdieu's central methodological arguments for pluralism and reflexivity in all aspects of research practice, which involves using "all the techniques that are relevant and practically usable, given the definition of the object and the practical conditions of data collection" (Bourdieu and Wacquant, 1992, p. 227). Each paper explores the implications of this mandate for a different facet of research methodology.

The first paper examines the social survey method in the UK and the US and its relationship to the social world that it sought to account for. Through a secondary review of historical literature, I observe how the social problems that demanded explanation, practical/technological constraints and advances, and the cultural climate shaped the development of the method, with the processes of institutionalisation, professionalisation and economisation shaping its trajectory in recent history. Methodological history, I argue, enables survey practitioners to reflectively appraise the idiosyncrasies of the method's development, current practices and future prospects.

The second paper problematises the compartmentalisation of sociology's diverse methodological repertoire within the data collection stage of research. I explore how a creative, multi-modal method – storyboarding – can serve as a useful tool to the researcher in the pre-empirical processes of conceptualisation and research design. This is exemplified through examining the challenge of operationalising trust. Using Bourdieu's notion of the construction of the sociological object, I argue that the use of creative, visual/multimodal methods serves to highlight where conceptual slippages, suggests new aspects of that concept and approaches to its investigation, and facilitates the researcher's reflexive exploration of their own relation to the object of research.

For Sociology to achieve genuine methodological pluralism, it is not sufficient to apply a wide range of methods: the implications of those methods in practice must also be continually questioned. The third paper argues that there lacks a sufficient theoretical account of the micro-dynamics of power in focus group discussions, while much of the standard guidance on their conduct serves to control interactions in a way that sanitises them of these dynamics. I exemplify an approach using Bourdieu's concept of fields to address these deficiencies, based on focus group discussions which used photo elicitation to examine national identity among South Sudanese diaspora in the UK.

These papers indicate that the attitude of 'relentless self-questioning' that Bourdieu describes as reflexivity is key to the achievement of pluralism. Pluralism sits in the tension between a permissive view as to what constitutes knowledge, and the constraints of methodological reflexivity and ethics: issues that are bound up in the relationship between the researcher and the researched.



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# DECLARATION OF AUTHORSHIP

I, Rachel Elizabeth Ayrton, declare that this thesis and the work presented in it are my own and has been generated by me as the result of my own original research.

History, drawing, and power: Essays towards reflexive methodological pluralism in Sociology

I confirm that:

1. This work was done wholly or mainly while in candidature for a research degree at this University;
2. Where any part of this thesis has previously been submitted for a degree or any other qualification at this University or any other institution, this has been clearly stated;
3. Where I have consulted the published work of others, this is always clearly attributed;
4. Where I have quoted from the work of others, the source is always given. With the exception of such quotations, this thesis is entirely my own work;
5. I have acknowledged all main sources of help;
6. Where the thesis is based on work done by myself jointly with others, I have made clear exactly what was done by others and what I have contributed myself;
7. Parts of this work have been published as:

Ayrton, R. (2017) Time for a revival? A historical review of the social survey in Great Britain and the United States. *NCRM Methodological Review Paper*.

Signed: .....

Date: .....





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## Definitions and Abbreviations

BSA	British Sociological Association
CBT	Cognitive Behavioural Therapy
DCLG	Department for Communities and Local Government
ESRC	Economic and Social Research Council
HaPS	Heads and Professors of Sociology
HEFCE	Higher Education Funding Council for England
IFRC	International Federation of Red Cross and Red Crescent Societies
LNRC	Liberian National Red Cross Society
LSE	London School of Economics and Political Science
MDT	Multi-Disciplinary Team
NCRM	National Centre for Research Methods
NDC	New Deal for Communities
NHS	National Health Service
NORC	National Opinion Research Centre
OED	Oxford English Dictionary
OFSTED	Office for Standards in Education, Children's Services and Skills
ONS	Office for National Statistics
OPCS	Office of Population, Censuses and Surveys
PGR	Post-Graduate Researcher
PSHE	Personal Social and Health Education
PTA	Parents' and Teachers' Association
RAE	Research Assessment Exercise
REF	Research Excellence Framework
SCPR	Social and Community Planning Research
SPLA	Sudan People's Liberation Army
SPLM	Sudan People's Liberation Movement
SRC	Survey Research Centre



# Chapter 1: Introduction

## 1.1 Introduction

This thesis undertakes the reflexive task of a sociology of sociological method: in particular, the aspiration to methodological pluralism. While this has become a mainstream position in British sociology in recent years, commentators have raised concerns that this profession is not borne out in the reality of sociological production (Payne *et al.*, 2004; Payne, 2007; Savage and Burrows, 2007; Williams *et al.*, 2008; Savage and Burrows, 2009; Williams *et al.*, 2016a). The divide between qualitative and quantitative approaches, which was most stark during the paradigm wars of the 1970s-1990s, continues to leave its mark on contemporary methodological practice. Against this backdrop, I conceptualise methodological pluralism drawing on Pierre Bourdieu's model for reflexive sociological practice, and in the papers that comprise this thesis explore some of the dimensions of a fully pluralistic sociology.

Following some initial comments on the theoretical and geographical scope of the thesis, in the next section of this introduction I outline Bourdieu's notions of reflexivity and methodological pluralism to position the arguments that follow. In particular, I draw out the productive tension between epistemology, methodological reflexivity and ethics, which I conceive as the dynamic forces which shape methodological pluralism. I then provide an overview of the historical and contemporary issues that surround methodological pluralism in British sociology. Within a reflexive sociological metier, this facilitates reflection on the sociohistorical conditions of possibility that this thesis emerges within, which paves the way for the reflexive self-analysis of the sociologist as a cultural producer which follows (Bourdieu and Wacquant, 1992, p. 36). Using Liz Stanley's notion of the intellectual autobiography and influenced by Bourdieu's own self-analysis in *Sketch for a Self-Analysis* (2008 [2004]), I account for the intellectual and social influences and circumstances that led me to generate this particular intellectual product. These three sections broadly follow Bourdieu's three stage methodology, which proceeds from the construction of the object (namely, methodological pluralism within a reflexive sociological practice), to analysis of the field (British sociology), and participant objectivation (myself, as a 'knowing subject') (Grenfell, 2012). Each of the three substantive papers which follow expresses aspects of reflexive methodological pluralism as outlined in this introduction. I resume these themes in the discussion which concludes the thesis.

Bourdieu's thought features heavily in this thesis: in addition to the positioning of methodological pluralism as he defines it within a reflexive model of sociological practice, the papers also draw on his notions of the construction of the sociological object and field theory. This does not betray a

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subtextual theoretical exclusivism trickling beneath my examination of methodological pluralism. I acknowledge that neither the advocacy of methodological pluralism nor the preoccupation with a reflexive mode of sociological practice are ideas exclusively proffered by Bourdieu. In the second section, in addition to elucidating these ideas as I read them in Bourdieu, I also consider two alternative theoretical orientations that further notions of pluralism and reflexivity and are usefully read alongside Bourdieu: feminism, and critical realism. Nevertheless, there are both sociological and biographical reasons for drawing on his thought here.

Sociologically, he was preoccupied with the “ruinous” opposition between subjectivism and objectivism which he perceived in mid-twentieth century intellectual thought (Bourdieu, 1990 [1980], p. 25; see also Williams, 2005; Williams, 2006; Letherby *et al.*, 2013)<sup>1</sup>. These designations implied differing accounts of human action based respectively on personal freedom and individual decision-making, or on establishment of determining social rules, with distinct methodological consequences. He objected to both methodologism and theoreticism, and sought to foster sociology as a *total science*, capable of accounting for human practice “across the mutilating scissions of disciplines, empirical domains, and techniques of observation and analysis” (Bourdieu and Wacquant, 1992, p. 26-27). He thus objected fiercely to the excessive specialisation and empirical fragmentation which provide fertile ground for the cultivation of methodological monisms (Wacquant, 1989). The ends of this endeavour were ultimately normative, as he sought to see “real effects” arising from sociology (Bourdieu, 2013 [1993], p. 7). He was dissatisfied with solely academic exercises, and saw scholarship and commitment, science and politics, the intellectual and the moral, as inseparable elements of the same programme of activity (Bourdieu, 2008 [2004]). These aspects of his thought evoke the qualitative/quantitative dualism which was prominent in British sociology in the late twentieth century, questions of disciplinary coherence that emerge from the relationship between theory and methodology in empirical investigation, and the concern for continued policy relevance. These are challenges that methodological pluralism in British sociology seeks to overcome, and I return to these themes in the third section of this chapter, below.

Biographically, Bourdieu’s thinking has served a unique function in the process of production of this thesis. Bourdieu has been my companion as I have worked through both the individual questions

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<sup>1</sup> More recently, Malcolm Williams and others have done important work on the false divide between objectivism and subjectivism, which originates in a ‘mythical’ version of science as ‘value-free’. It is wrong, he argues, to conflate objectivity with value-freedom; rather, objectivity itself is a social value, shaped by its context (but recognizable across contexts) and which is held in order to achieve a purpose. In sociology, that purpose operates at the levels of its relationship to civil society broadly, its relationship to specific aspects of civil society, and its expression through method (Williams, 2005; Letherby *et al.*, 2013).

and problems that the papers seek to address, and as I have come to understand my own academic trajectory towards conceptualising them as expressions of a reflexive mode of methodological pluralism. In particular, he has provided a means of dissecting questions of epistemology and ethics that have been key aspects of my exercise in learning to think sociologically. I return to this process in section four.

It is also worthy of comment that I restrict my focus here to British sociology. This does not imply a methodologically nationalistic stance: neither my concept of society nor of the sociological discipline are necessarily delimited by national borders (Chernilo, 2006). I recognise the growing need for global sociology to respond to issues of inequality that reach internationally (Burawoy, 2016). Indeed, the extent to which nationally defined sub-disciplinary communities form a coherent unit becomes a key issue in assessing the degree of methodological pluralism in sociology, discussed in section three. Nevertheless, the UK is the field of cultural production within which this thesis has been generated and that, accordingly, is within its scope. Fields are pervious and overlapping arenas of social relations, so to theorise British sociology as a space where field dynamics apply does not negate other potentially meaningful analytic units that make no reference to the nation state.

As a consequence of these theoretical and geographical choices, the resulting argument runs the risk of presenting a regrettably Eurocentric picture. As I describe in section four, the thesis has made a remarkable journey from its original purpose to its present form. I intended to engage in depth with the historical entanglement of sociology in empire, post-colonial and Southern theories, and emancipatory and indigenous methodologies (Truman *et al.*, 2000; Bhabra, 2007; Connell, 2007; Chilisa, 2012; Steinmetz, 2013). The traces of these aspirations remain in the papers: for example, in the discussion of the orientalisation of the urban poor in the nineteenth century in chapter two, and engagement with issues of representation and positionality in chapters three and four. Further, while I hope that my feminism is implicit in the thesis, I had planned for this to be a more central theoretical orientation in the original project. Bourdieu's critique of and active opposition to colonialism in Algeria, his occupation with symbolic domination (of which he considered gender inequality the paradigmatic form), and the "promising overlap between his habitus and that of feminists" provide some comfort in light of these limitations (McCall, 1992, p. 860; McNay, 1999).

## 1.2 Reflexive sociology and methodological pluralism

The notion of reflexivity has a wide range of applications both within and beyond sociology. To be reflexive, in a grammatical sense, involves actions that one does upon oneself. Philosophically, this denotes the turning inward of the faculties of the mind upon itself: intelligent, or thoughtful introspection and self-analysis. Within sociology, the term has had social theoretical applications. For example, Anthony Giddens proposes that the constant refraction between thought and action through the continual examination and reform of social practices characterises the shift to modernity (Giddens, 1990). The idea of *reflexive sociology*, however, indicates a particular conception and enactment of sociological practice. This concerns the relationship between the knower and the known, the researcher and researched, and how social knowledge is constituted in this relationship (May, 1999; Rappert, 1999). The way that sociological knowledge is produced has significance for the ability of sociology to enable us to know the social world – or not. From a Bourdieusian orientation, the ‘researched’ does not refer to social agents themselves: it is the object of research that is central. This object is always relational, and cannot be conflated with the population within which it is manifest. Reflexive sociology is concerned with critical examination of the relations between the researcher and the object of her research.

Bourdieu is not alone in his preoccupation with reflexivity and methodological pluralism: these themes have also formed significant areas of focus in other schools, notably feminism, and critical realism. I will briefly outline these parallel approaches to my key themes, their relationship to the thought of Bourdieu, and the reasoning for my theoretical orientation in this thesis, before I turn to unpacking in more detail Bourdieu’s conceptions of methodological reflexivity and pluralism.

### 1.2.1 Feminism, reflexivity and methodological pluralism

The feminist critique of social science had at its core the contention that in both theory and practice, social science omits and/or distorts the experience of women from its construction of social reality (Stanley and Wise, 1993). Within sociology, early feminists had some success in filling these gaps in knowledge through adding topics and challenging sub-fields; however, they did not initially extend their critique to the basic assumptions and conceptual frameworks of the disciplines (Stacey and Thorne, 1985). The second wave feminists of the late 1970s and early 1980s were more “self-consciously methodologically innovative,” as they moved beyond working within the existing frameworks of social science to questioning those frameworks themselves (Lather, 1988, p. 571; Stanley and Wise, 1993).

Coming from a place of marginalisation and oppression based on gender, feminists were critical of how the standpoints of the privileged infuse sociological knowledge and, conversely, how some



types of knowledge are heralded as 'scientific', while others are merely 'social', and exploited without acknowledgement (Stacey and Thorne, 1985; DeVault, 1995). They objected to the claim to 'objectivity' of a model of social science that mirrored the natural sciences, based as it was on the distancing of the researcher from the object of their research. This separation between subject and object, knower and known, was conceived as both morally objectionable and epistemologically misguided, based on the failure to recognise that research inevitably involves an intervention of some kind in the social world (Stacey, 1988; Oakley, 2000). The epistemological and ethical challenge for feminist research became, how to explain the lives of others without violation or distortion (Lather, 1988).

Liz Stanley and Sue Wise were among the early influential voices in the feminist attempt to epistemologically reorient the social sciences, notably through the publication of *Breaking Out*, in 1983.<sup>2</sup> They reject grand theory (and feminist attempts to emulate it), which attempts to explain phenomena based on general principles and independently of examination. Such abstract knowledge has no basis in practical, lived experience. Instead they argue for the "constant dialectical relationship" between practice and theory: feminist ontology consists in the claim that "all social knowledge is generated as a part and a product of human social experience" (Stanley and Wise, 1993, pp. 56, 192). This human experience is complex and indeterminate; the task of social science is the exploration of these multiple 'objective realities' (ibid., p. 171; also Lather, 1988). In light of this, Stanley and Wise object to the presentation of the social scientific research process as orderly and logically organised which is common to both 'positivism' and 'naturalism'<sup>3</sup>: "this 'hygienic research' in which no problems occur, no emotions are involved, is 'research as it is described' and not research as it is experienced'" (Stanley and Wise, 1993, p. 153).

The claim that research *itself* is a social experience like any other is key to the feminist understanding of reflexivity. As such, the social positions of those who are involved, and the historical social context in which it is produced, come to be of central importance (Stanley and Wise, 1993; DeVault, 1995). The production of data is not a socially neutral process; rather, it is produced by people in a relationship, and through an interaction between the researcher and the researched, whether 'the researched' is books, secondary data, other objects, or people (Lather, 1988; Stanley and Wise, 1993). The researcher is inevitably involved in the process of research, and her value commitments and purposes will inject themselves into the data she collects; the only choice is

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<sup>2</sup> I draw here on the 1993 revised edition, *Breaking Out Again*.

<sup>3</sup> I note Ann Oakley's (2000) observations about the vagueness with which terms such as 'positivist' are often applied in anti-positivist critiques. Although Stanley and Wise provide a detailed explanation of their use of the term, it remains problematic in the general context of a discussions of epistemology.

## Chapter 1

whether or not the influence of this presence is acknowledged (Lather, 1988; Stanley and Wise, 1993):

“The researcher’s own experiences are an integral part of the research and should therefore be described as such. The kind of person that we are, and how we experience the research, all have a crucial impact on what we see, what we do, and how we interpret and construct what is going on.” (Stanley and Wise, 1993, p. 60)

In practice, this reflexive practice involves self-critical and self-conscious efforts to identify and make visible the researcher in all aspects of the research experience (McCall, 1992). Drawing on earlier work by Dorothy Smith (1974), Stanley and Wise encourage the researcher to locate herself in her historical and spatial context. By thus situating herself, she is able to show how her direct experience of the world, and of the research process, shape her social scientific knowledge (Stanley and Wise, 1993, p. 163). The output of research, therefore, can be properly presented as *her* understanding, rather than passing it off as gleaned unproblematically from ‘the researched’, enabling the reader to examine and evaluate research within the context that it was produced (*ibid.*, p. 166). By *utilising* the presence of the researcher, rather than denying it, and acknowledging its locatedness and partiality, the rigor of social scientific research is *improved*: it can move towards greater objectivity, greater legitimacy, and “a stronger and more credible kind of truth” (Lather, 1988; Stanley and Wise, 1993; DeVault, 1995, p. 628).

The feminist account of reflexivity is detailed and well-established – it pre-dates Bourdieu’s focus on the topic. However, the relationship of feminism to methodological pluralism is more contentious. There is certainly room for a pluralistic approach within feminist epistemology: it recognises the spectrum of paradigms within which feminists operate (Lather, 1988; Stanley and Wise, 1993). Stanley and Wise, for example, resist the claim that there is one ‘feminist method’, arguing instead that “methods in themselves aren’t innately anything” (Stanley and Wise, 1993, p. 158). What they object to is the assumptions they perceive within ‘positivism’ about the nature of reality and the relationship of the researcher and the researched, rather than quantification or the use of statistical techniques as such. Nevertheless, as I will explore in more detail below, the paradigm wars that erupted from the 1970s onwards calcified ‘quantitative’ and ‘qualitative’ as mutually exclusive categories, and many feminists were partisan in this conflict. They objected to the apparent conspiracy between the dominance of men in social science and the hegemonic use of quantitative methods, particularly due to the systematic exclusion of women’s voices from these accounts. Both statistical and experimental methods were lambasted for their objectifying and alienating practices, which exerted control and manipulation over passive bodies (Oakley, 2000, pp.

33, 38). Although feminist epistemology allows space for methodological pluralism, historically the relationship between the two has been ambivalent.

What, then, of the potential for synergies between the thought of Bourdieu and feminism? Bourdieu's lack of attention to feminist theory, despite his interest in gender relations, is striking (Skeggs, 2004). Indeed, throughout both his theoretical and his methodological writings, he ignores much of feminism, and in the few references he does make he is less than generous towards the field (Lovell, 2000). Feminists have been rather more accommodating in their reception of Bourdieu's ideas. Theoretically, there are certainly a number of objections to Bourdieu's constructs from a feminist standpoint. He fails to consider gender systematically, and is often androcentric in his treatment, for example in his male-gendered conceptions of social structure in the public sphere of economic and cultural life (McCall, 1992). He tends to view women as 'capital-bearing objects' rather than as subjects with the potential for their own capital accumulation strategies (Lovell, 2000). He often conflates sex, sexuality and gender (McCall, 1992; Lovell, 2000), and his excessive focus on the middle classes leaves him unable to account for the nuanced practices of those who do not operate from a dominant position (Skeggs, 2004). Nevertheless, feminists have productively engaged with and adapted aspects of his theory. Leslie McCall (1992), for example, has adapted his notion of 'embodied cultural capital' to show how dispositions associated with gender position can take on the form of capitals in their most hidden and universal forms, while also showing the usefulness of 'habitus' for considering the gendered asymmetries between masculine and feminine dispositions. Others have shown how Bourdieu's thought has been useful to feminists seeking to reintroduce social class to the agenda (Lovell, 2000; Skeggs, 2004).

However, it is in Bourdieu's epistemological and methodological approach, and particularly in the notion of reflexivity, that his work really "parallels and enhances" feminist work (McCall, 1992, p. 837; Skeggs, 2004). The anxieties and concerns that motivate Bourdieu's reflexive sociology have significant overlap with those of feminist academic practice. Both conceive the struggle over what can count as rational knowledge as a social and political struggle. Both consider reflexivity, which sheds light on the social positions from which we speak, to enable an expanded definition of objectivity, since it is extended to the position of the researcher as well as to the results of research. And both are concerned to expose the insidious acts of domination that are embedded in the structure of social scientific research (McCall, 1992; Skeggs, 2004). It is disappointing, therefore, that Bourdieu took so little account of feminist epistemology, which could have supplemented and strengthened his argument for reflexive sociology. Although it is beyond the scope of my task here to undertake a full analysis of the parallels between the Bourdieusian and feminist accounts of reflexivity, in the discussion that follows I do draw out connections and synergies, and use the feminist approach to critique some of Bourdieu's claims, in particular those of originality.

### 1.2.2 Critical realism, reflexivity and methodological pluralism

The second approach I highlight is critical realism, in which I draw particularly on the work of Andrew Sayer (2000). From the 1980s, realist philosophy began to have an impact on social science (Sayer, 1992). The defining feature of realism is the contention that there is a world that exists independently of our knowledge of it. This necessitates a fallibilist philosophy of knowledge, as the objects in the world do not depend on the mind for their existence: there is no simple correspondence between the 'real' objects that we seek to study, and the knowledge of them that we claim. In other words, the (real) world should not be conflated with our (empirical) experience of it (Sayer, 2000; Roberts, 2014). Further, the distinctive properties of the social world make social phenomena much less durable compared with natural phenomena. The sensitivity that people show to contexts, and their ability to actively interpret situations, give rise to much greater fluidity of phenomena through time and space, which requires social science to persist in a preoccupation with conceptualisation (ibid.).

Beyond the observable patterns of events that a researcher may record, the world has ontological depth: "events arise from the workings of mechanisms which derive from the structures of objects, and they take place within geo-historical contexts (Sayer, 2000, p. 15). Critical realists contend that the world is organised into different, layered domains of reality: the empirical, the actual, and the real. As Dave Elder-Vass explains:

"the empirical domain includes those events that we actually observe or experience and the actual is the domain of material existence, comprising things and the events they undergo. The real also includes 'structures and mechanisms' that generate those events." (Elder-Vass, 2010, p. 44)

The epistemological consequences of this complexity, and the distance between what we can observe and the causal structures and mechanisms that may interest us, means that all knowledge is partial. Our conceptions of a concrete object are inevitably one-sided, partial, and grounded in a particular perspective and worldview. There are therefore different valid perspectives on reality (Sayer, 2000; Maxwell, 2012). This notion of the particularity of knowledge, and the positional character of its viewpoint, are epistemological preconditions for the notion of reflexivity, as Bourdieu or feminism conceive it. Sayer acknowledges that since social phenomena are intrinsically meaningful, this meaning can only be understood through interpretation. The social science, he argues, requires a "double hermeneutic", which implies:

“a ‘fusing of horizons’ of listener and speaker, researcher and researched, in which the latter’s actions and texts never speak simply for themselves, and yet are not reducible to the researcher’s interpretation of them either.” (Sayer, 2000, p. 17).

There is thus a two-way interpretation required which accounts for the context, perspective and worldview of both researcher and researched in order to arrive at an estimation of meaning. Although the kind of methodological reflexivity that I discuss here is implied in this, in practice the discussion of reflexivity in critical realism is largely social theoretical. This refers to the notion of conscious deliberation and reflection as the basis for human action. This is central to Margaret Archer’s account of human agency; however Bourdieu has been criticised for allowing that academics have reflective capabilities, while appearing to deny that social agents that lack the gaze of scholarly judgement think theoretically about the world (Elder-Vass, 2007). Against this, Archer argues that agents continually monitor themselves in relation to their circumstances, conducting internal conversations about ourselves with ourselves. The conclusions we reach through these reflexive deliberations influence our behaviour in the social world (Archer, 2003; Elder-Vass, 2007). Nevertheless, and although I consider critical realists and other critics to overemphasise the extent of determinism in Bourdieu’s notion of habitus (see, for example, discussion in McNay, 1999), there is limited discussion of the methodological role for reflexive deliberation in critical realism.

The strength of critical realism lies particularly in the way its conception of the social world translates into methodological pluralism. Sayer provides an explanation of this drawing on Rom Harré’s terminology of ‘extensive’ and ‘intensive’ research designs. In relation to the three-level model of reality outlined above, extensive designs are suited to concrete research studies of actual events or objects observable in the empirical domain, while intensive designs are able to engage with underlying mechanisms and structures, enabling a movement between the concrete and the abstract. The two approaches respond to different questions. Extensive research is concerned with common properties and general patterns across a population, offering descriptions of formal associations. Intensive research considers how causal processes work in a limited number of cases, showing in the context of those specific cases what makes things happen, or “what kind of universe of meaning exists” (Sayer, 1992; 2000, p. 20). This distinction avoids value judgements and provides a robust philosophical rationale for the use of a wide range of research methods, and indeed mixed methods designs, an observation which I will return to briefly in my discussion of British sociology, below.

It is clear, therefore, that feminism and critical realism both have ontological and epistemological foundations that are conducive to enlightening both reflexivity and methodological pluralism. While feminism makes a particularly valuable contribution to the former, critical realism

distinctively informs a methodologically pluralist stance. I do not suggest that Bourdieu has a monopoly on these interests, or indeed that his account would not benefit from a greater degree of interaction with those of other approaches. In what follows, I will draw parallels that begin to show the fruitfulness of dialogue between alternative perspectives. Bourdieu, like any social theorist, is limited by his own social positioning as a white, French male, born to a provincial, working class family but elevated through educational success to a position of significant cultural power. I adhere to a horizontal, cumulative model of methodological development where new developments are inevitably built on those of others, referred to evocatively as “conceptual composting” by Helga Wild (cited in 2012; Wiles *et al.*, 2013). Nevertheless, Bourdieu is distinctive in that he not only provides a detailed account of both reflexivity and methodological pluralism, but he explicitly connects the two in his account of sociological practice. This makes him uniquely suited to the task that I set myself here: to extend our conception of methodological pluralism by positioning it within a reflexive model of sociological practice.

### **1.2.3 Bourdieu’s account of reflexivity and methodological pluralism**

Reflexivity was Bourdieu’s “signature obsession” (Wacquant, in Bourdieu and Wacquant, 1992, p. 36). He reacted against what he terms the “mythology of the free intellectual” (Bourdieu, 2008 [2004], p. 23). He viewed the most serious errors in scientific thought as arising through the notion of the individual social agent as conscious, rational, and unconditioned. He thoroughly rejected the scholastic point of view, which seeks:

“to occupy a transcendent viewpoint with respect to the empirical viewpoints of ordinary agents or his competitors in the scientific world ... to attain a position of absolute knowledge and to speak with the voice of an authoritarian reason, as the sole possessor of the truth.” (Bourdieu, 2000 [1997], p. 119)

Bourdieu was critical of the pretence of omniscience in the total intellectual which he perceived in the mid-twentieth century academy. This was achieved in France quintessentially by Sartre, through the medium of philosophy, and through method in the empiricist mode of sociology practiced in the US, as embodied in Lazarsfeld (Bourdieu, 2008 [2004]). For Bourdieu, the sociologist is unequivocally situated within the object that she seeks to objectivise – the social world. She is therefore subject to “social conditions of possibility” that also apply to her work of sociological construction: her “act of objectification” is socially constrained (Bourdieu, 2000 [1997], p. 120). The limitation of social position will therefore tend to determine what is “thinkable” or “knowable”, unless this is controlled through active knowledge of the social universe within which social science is produced (Wacquant, 1989). In a similar vein, while some feminist researchers

responded empirically to the perceived deficiencies of social scientific research by supplementing the androcentric cannon with additional studies *about* women and aspects of social life that centralise gender, feminist standpoint theorists took the more radical position that a genuinely feminist perspective is moulded by the ‘feminist consciousness,’ which can only emerge through the direct experiences of oppression and domination that are the everyday reality for women. Therefore, the endemic sexism in social science can only be corrected through research *by* women, who are in a privileged position to generate knowledge of these phenomena (McCall, 1992; Stanley and Wise, 1993). Bourdieu does not go this far: for him, the limitations of the researcher’s social position will *tend* to determine the process and product of their investigation, but this is not insurmountable; reflexivity provides a means by which this limitation can be mitigated.

Reflexivity, then, is a crucial facet of sociological epistemology, and a prerequisite to rigorous social scientific practice (Bourdieu and Wacquant, 1992). Bourdieu’s mode of empirical enquiry requires simultaneously calling into question the intellectual world through objectifying the objectifier. The practices of the social analyst who proposes to offer theoretical accounts of others’ practices must also be exposed to the critical tools of sociology. This does not imply a shift to the extreme of relativism which denies the merit of any attempt to access reality; rather, through ‘radical historicisation,’ scientific reason can “control itself ever more closely” and thus “move progressively towards total independence of constraints and contingencies” (Bourdieu, 2000 [1997], pp. 121-122). Bourdieu’s outlook is therefore fundamentally optimistic: the social determinations that may bear upon sociology can be overcome through “continually turn[ing] back onto itself the scientific weapons that it produces” (Wacquant, 1989, p. 55). This is an exacting work that is carried out collectively (Bourdieu, 2000 [1997]).

This idea of the ‘reflexive return’ that involves “objectifying one’s own universe” is not exclusive to Bourdieu (Wacquant, 1989, p. 33; Bourdieu and Wacquant, 1992). Alvin Gouldner (1970), for example, also called for a ‘sociology of sociology’ which requires both the “sociologist’s knowledge of himself (*sic*) and his position in the social world” (Gouldner, 1970, p. 489; cited in Bourdieu and Wacquant, 1992, p. 38). However, Bourdieu claims that his demand for reflexivity extends further than most. He identifies three types of bias that may cloud the sociological gaze. Firstly, the individual researcher must understand her own “*social* origins and coordinates” – factors such as class, gender, and ethnicity (Bourdieu and Wacquant, 1992, p. 39). This is widely recognised amongst advocates of reflexivity (see Bourdieu and Wacquant, 1992, pp. 36-40, for discussion); for Bourdieu, failure of those who objectivise the social world to objectivise themselves only succeed in objectivising *their relation to* the object of research, rather than that object itself (Wacquant, 1989). Second, and more particularly, the researcher’s position in the field of cultural production – that is, the *academic* field, must be analysed, in order to appreciate the limited possible positions

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open to her at a given time. This is a relational assessment of proximity of others within the relevant field, which also takes account of the relatively dominant or dominated position of that field in relation to others in the field of power (Bourdieu and Wacquant, 1992). For example, Bourdieu perceived his own trajectory from training in philosophy to his self-definition as a sociologist in general and a rural sociologist in particular as a shift from the most noble of disciplines to the very lowest rank of the “social hierarchy of specialties” (Bourdieu, 2008 [2004], p. 60). Finally, the intellectualist trap is the most insidious form of bias. This is an effect of the scholarly gaze: the leisure to withdraw from the social world in order to study and talk about it. This necessitates retirement from the ‘game’ of human practice, and runs the risk of confusing the logic of practice with theoretical logic (Bourdieu and Wacquant, 1992). In other words, the intellectual may mistake the theoretical logic that is produced in isolation from human action for the way practice takes place in reality, treating the two as synonymous. The only way to avoid this pitfall is to scrutinise the work of constructing the object of research, as well as the object itself (Wacquant, 1989; Inghilleri, 2005).

Although Bourdieu and Wacquant claim the originality of the second and third aspects of his conception of reflexivity, this is disputable. Feminism is also concerned with the political task of transforming academia, particularly by retheorising the nature of knowledge itself and taking account of the reality of the researcher in the process, as well as the presence of emotion, although they make the more humble claim that these ideas are the *common property* of a number of different intellectual traditions (Stanley and Wise, 1993, pp. 190, 232). The position of the researcher within academia, as well as her culture, background and biography, are aspects of identity that enter with her into research and must be accounted for. Through taking seriously the power relationship between the researcher and researched in this way, feminists seek “an egalitarian research process characterized by authenticity, reciprocity and intersubjectivity” (Stacey, 1988, p. 22). In relation to the third aspect of reflexivity, the withdrawal of the researcher to ‘gaze’ upon the social world, a feminist viewpoint suggests that the difference between academic and everyday modes of experiencing is not as absolute as Bourdieu implies: this separateness is powerful through its *perception*, more than its reality. While the distancing of the researcher from the world they investigate might be a side-effect of academic institutional positioning to an extent, it is not desirable, and an aspect of reflexive research practice, as Judith Stacey’s comment cited above indicates, is to redress this imbalance to create a more egalitarian relationship. For Stanley and Wise, this is achieved by increasing the vulnerability of the researcher through “displaying her actions, reasonings, deductions and evidence to other people” (Stanley and Wise, 1993, p. 168). Although I agree with Ann Oakley’s (2000) observation that no methodology can absolutely resolve the power differential between the researcher and researched, since it is always the researcher



who retains the power to define, it is noteworthy that feminist scholars not only recognise the insidious impact of the academic's self-perceived separation from the social world but they identify reflexive attention as the means to mitigate this.

The notion of methodological pluralism in Bourdieu's thought sits within and is a consequence of a reflexive mode of sociology. At face value methodological pluralism is a simple idea: rejection of the exclusivist claim that there is one 'right' way to investigate the social world, and instead taking a non-restrictive view of what counts as rational enquiry, and the methods that can achieve it (Roth, 1987; Scott, 2014). However, in practice it suffers from a number of ambiguities. If sociology is best practiced in a pluralistic way, this may be evident at a number of levels – for example, in a particular study, in an individual researcher's (or research group's) body of work, in a substantive field, or at the level of an academic discipline. Although it is common for 'methods' to be considered relevant to the processes of data construction and analysis, presumably in a reflexive mode there are techniques that are applied by the researcher within other aspects of research practice which do not directly concern research data. It is clear that certain theoretical traditions tend to favour certain methods (Williams, 1998): does this undermine pluralism? Further, the use of multiple methods in a non-reflexive way which fails to account for the discrepancies between their implications is perfectly possible (ibid.). There are significant gaps between the idea of pluralism and its practical reality, which I will explore further in section three, and in the papers that comprise this thesis. First, however, I consider what methodological pluralism looks like within a Bourdieusian reflexive metier of sociology in order to orientate this discussion.

Bourdieu evaluated social science in the 1960s to be in the grip of theoretical and methodological orthodoxy (Bourdieu, 2008 [2004]), in the face of which he urged a forthright rejection of any such triumphalist claims:

“We must repel any unilateral, unidimensional and monomaniacal definition of sociological practice, and resist all attempts to impose one ... to question and constantly challenge methodological prescriptions and interdicts.” (Wacquant, 1989, p. 54)

A reflexive orientation involves a suspicious stance towards expressions of methodological exclusivism, as they betray uncritical submission to disciplinary convention (*doxa*) and failure to actively construct the object of research (Wacquant, 1989; Bourdieu and Wacquant, 1992). He describes methods as “instruments of objectivation” which enable the arrival at a “scientific relationship” to the object of research which transcends the familiarity, or strangeness, of that object to a socially situated researcher (Bourdieu, 2008 [2004], p. 61). Methods enable the “conversion of the gaze” that bridges the gap between “the primary vision and the scientific vision”

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(ibid.). Whether the object of research is the social world or reflexive examination of sociological practice, Bourdieu makes the following plea for methodological pluralism:

“We must try, in every case, to mobilize all the techniques that are relevant and practically usable, given the definition of the object and the practical conditions of data collection.” (Bourdieu and Wacquant, 1992, p. 227)

This statement is revealing of the “eclectic yet highly selective disposition” that Bourdieu professes (Bourdieu, 2008 [2004], p. 69). The object of research is at the centre of the choice of methods, which are assessed and selected according to their relevance and practical considerations in their application. The consequence is a ‘methodological pragmatism’ which denies any normative hierarchy of methods, instead searching for a clean ‘fit’ between method and question, and strong application of those methods selected (Lamont and Swidler, 2014). For Bourdieu, both theory and method are tied up in empirical research practice, and therefore much of his commentary on these themes are explicated within his substantive works. In *Weight of the World*, Bourdieu includes a chapter that reflects on the methodological principles that shaped the production of the volume (Bourdieu, 1999 [1993]). In this he is critical towards the “prescriptions of methodology” which he assesses “more scientific than scientific,” through their attempts at replication of the “external signs of rigor” of more established scientific disciplines (Bourdieu, 1999 [1993], p. 607). These, he argues:

“[fail to] do justice to what has always been done – and known – by researchers who have the most respect for their object and who are the most attentive to the almost infinitely subtle strategies that social agents deploy in the ordinary conduct of their lives.” (ibid.)

Epistemologically, this implies an extreme openness in how the social world can be known – a repertoire of methods as extensive as the strategies of social agents that they seek to objectivise. This is reminiscent of the recognition within feminism of the validity of experience as a basis for social knowledge (Stacey, 1988; Stanley and Wise, 1993). The assumption appears to be that in any programme of research, more than one technique is likely to be part of a thorough examination of any research object. This is evident in Bourdieu’s work on his home region of Béarn, which he reflects on as his “initiation” into sociology. His empirical activity took on a “hyperempiricist” fervour, including the use of photographs, maps, floor plans, statistics, genealogies, interviews, and in-depth observations (Bourdieu, 2008 [2004], pp. 61-62).

The extreme epistemological liberty that Bourdieu advocates does not, however, imply “laissez-faire” (Wacquant, 1989; Bourdieu and Wacquant, 1992): neither does anything go, nor does it cease

to matter what methods are employed. On the contrary, Bourdieu requires extreme vigilance in the application of method (Bourdieu and Wacquant, 1992). There are two sources of constraint that temper an eclectic approach to method: methodological reflexivity, and ethics.

Methodological reflexivity is an aspect of the global reflexive task that infuses sociological practice. Wacquant summarises it as “the relentless self-questioning of the method itself in the very movement whereby it is implemented” (Wacquant, 2008, p. 266). In *Weight of the World*, Bourdieu goes on to insist:

“it seems to me imperative to make explicit the intentions and the procedural principles that we put into practice in the research project whose findings we present here. The reader will thus be able to reproduce in the reading of the texts the work of both construction and understanding that produced them.” (Bourdieu, 1999 [1993], p. 607)

Sociologists are called on not only to monitor and continually question their own application of method; they must also record and make transparent the orientation and decisions that guide them, to enable the reader to simultaneously evaluate the process of production of knowledge as well as that knowledge itself. This focused attention should be equally applied to all aspects of research practice – Bourdieu places background tasks such as creating a coding schedule and conducting an interview on a par with constructing a theoretical model (Bourdieu, 2008 [2004]). Every action of research is to be exposed to the same reflexive scrutiny. This routine practice of “the objectification of the tools of objectification” reminds sociologists of the “double bind” that they face: they need the intellectual instruments of their scholarly tradition; however, unknowing acceptance of categories of perception leads to a “*half-science*” that reproduces its own doxa (Wacquant, 1989, p. 53). The practice of methodological reflexivity enables the sociologist to embrace the freedom of an expansive view of what constitutes data and how it can be generated, without allowing their methodological choices to unwittingly determine the knowledge they produce.

The second constraint on methodological pluralism is ethical. Although Bourdieu makes very little explicit reference to ethics in his theory of practice, his work shows a clear objection to human suffering and injustice. His engagement in direct political action notably increased from the 1990s onwards; however, it was evident in his earliest academic work in Algeria (Bourdieu, 2008 [2004]; Pellandini-Simányi, 2014). He has been accused of a “dismissal” of ethics, which he reduces to unconscious competitive strategies in the pursuit of legitimate power (Pellandini-Simányi, 2014, p. 652). Further, although his notion of habitus leaves significant scope for the embodiment of a practical sense of the good, the presence of ethical dispositions is neglected in Bourdieu’s account

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(Sayer, 2010; Pellandini-Simányi, 2014). The strongest statement of ethical conviction in Bourdieu's theoretical repertoire relates to symbolic violence, and it is this that he himself applies to the area of research ethics. Symbolic violence is the insidious and often brutal means by which oppression is reproduced. It refers to the misrecognition of culturally and historically specific and arbitrary systems of classification as natural, and thus the acceptance of them by both those on whom they confer power, and those who are subordinated by them (Schubert, 2012). Thus the social origins of suffering are internalised by members of society, which perpetuates structures of domination. Bourdieu and his colleagues examined contemporary manifestations of suffering in *Weight of the World*. His application of the notion of symbolic violence there to the ethical practice of research is worthy of an extended quotation:

“If its objective of pure knowledge distinguishes the research relationship from most of the exchanges in everyday life, it remains, whatever one does, a social relationship. As such, it can have an effect on the results obtained (the effects varying according to the different parameters that can influence the relationship.) Of course, by definition, scientific questioning excludes the intention of exerting any type of symbolic violence that could affect responses. Yet it remains the case in these matters that one cannot trust simply to one's own good faith, and this is true because all kinds of distortions are embedded in the very structure of the research relationship. It is these distortions that have to be understood and mastered as part of a practice which can be reflective and methodical without being the application of a method or the implementation of a theory.” (Bourdieu, 1999 [1993], p. 608)

Bourdieu is here primarily referring to the practice of interviewing in the context of this specific study. However, if we extrapolate the principles it becomes evident that, as in any other social relationship, the relationships in which research is constructed have the capacity to exert and reproduce symbolic violence. Although to do so may not be the intention of research, it is inevitable unless diligent reflexive attention to the implications of those relationships is practiced. Good intentions are not sufficient to eliminate the potential for oppressive structures to infiltrate research relationships. Where this happens, it is not only detrimental to the particular individuals involved: the knowledge that is produced as a result will also tend to perpetuate classificatory systems that impose sufferings on those that they subjugate. It is therefore imperative that in their practice the researcher seeks to minimise as far as possible the symbolic violence that is exerted through research relationships (ibid.). Similarly, within feminism scholars frequently engage critically with questions of how egalitarian or emancipatory methods really are, and the significant scope for exploitation to occur (Lather, 1988; Stacey, 1988). They are concerned, as Ann Oakley summarises, with developing ways of knowing that are both reliable and democratic, in that they

“bridge the gap between ourselves and others, and ... ensure that those who intervene in other people’s lives do so with the most benefit and the least harm” (Oakley, 2000, p. 3).

Methodological pluralism, then, sits in the space between how we can know and how we should know – between epistemology and ethics. Epistemologically, methodological pluralism rests on the supposition that we can know about the social world by every conceivable manner of investigation; however, methodological reflexivity is required to guard against the distortions that are introduced and perpetuated through the uncritical application of methods. Ethics expresses the limits of what is appropriate scientific practice – a relational endeavour, which must actively resist the reproduction of symbolic violence. Thus the contours of pluralism are sculpted by a productive tension between eclecticism and constraint, which is navigated through constant attention both to the object of research and the process of its objectification. This reflexive tension is exhibited in each of the papers presented here.

### **1.3 Methodological pluralism in British sociology**

Having defined methodological pluralism within the context of reflexive sociology, I now turn to considering the field of British sociology, within which this thesis is situated. The position of methodological pluralism within this space is ambiguous: on the one hand, and in contrast to previous eras, it now constitutes the dominant professed disposition among British sociologists; on the other, there are some noticeable gaps in the collective national output which calls the pluralistic commitment into question (Payne *et al.*, 2004). I examine the origins, current manifestation, and repercussions of this paradox here, before reflecting on the spaces this debate leaves and which the papers that comprise this thesis strive to flesh out.

It is significantly beyond the scope of this summary discussion to undertake a full field analysis of British sociology. This would be a significant empirical undertaking, which would require a detailed study of the social organisation of sociological practice (May, 2005), among other methods. Here I undertake a secondary review of literature that grapples with issues in methodological pluralism in British sociology. There are two main methodological approaches represented within this literature. Firstly, there are ‘insider accounts’ by those with substantial primary involvement in the field (Crothers, 2011). These situated analyses express themselves in more or less apparently objective terms and take a variety of viewpoints in relation to the current ‘state of the discipline’. Cumulatively, they provide useful insights broadly following an apprenticeship model of learning (Wiles *et al.*, 2009). Their more ‘technical’ corollary are analyses of journal publications. Taken as an empirical ‘genre’, this approach is highly varying in the quality of its execution. Jennifer Platt

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(2016) has assessed in detail both the generic problems with this tactic in representing a national sociology, and the numerous issues that confound comparability between such studies. For present purposes, it is particularly relevant to note that studies do not define what counts as a 'sociological' article, nor is it clear that inclusion in a British journal is the most adequate criteria for describing British sociology (as opposed to, for example, the national origin or present institutional location of authors). If, for example, sociologists in British institutions publishing 'quantitative' work are more likely to target international journals than their colleagues seeking an outlet for 'qualitative' research, there is a coverage problem which will bias the findings. Platt raises concerns about the gap between the conclusions reached through this approach and what is justified based on the data – particularly given the impoverished account of qualitative research as "the negative of quantitative" (Platt, 2016, p.33). Her analysis encourages caution in taking journal article analyses as an independent measure of the state of a national discipline. Nevertheless, alongside other forms of appraisal they can make a useful contribution to the kind of critical reflection on research practice that I undertake here (Brannen and Edwards, 2007).

Neither has British sociology always been broadly methodologically pluralistic, nor has this tendency emerged out of a vacuum. I discuss the institutionalisation of British sociology in parallel to that of the survey method further in the first paper presented here. For present purposes, it is sufficient to note a few key trends, particularly surrounding the 'paradigm wars' of the 1970s and 1980s. This is important because, as Rosemary Crompton (2008) observes, the legacy of this tumultuous debate continues to have repercussions on current discussions.

In the 1950s and 1960s, spurred on by the successful contribution of social research in wartime and its new legitimisation in government (Moss, 1991), sociological research expanded. These 'glory days' were fuelled by the willingness of a population unaccustomed to being researched, and sociology gained coherence through the common acceptance of Marx, Weber and Durkheim as the 'classical' canon of the new discipline (Savage, 2010a, 2010b). Nevertheless, longstanding binary divisions (fact/value; agency/structure; culture/economy; social/economic) that had bubbled under the surface since the nineteenth century boiled over in the 1970s (Crompton, 2008).

The 'paradigm wars' that ensued were based on radically different assumptions about the nature of the world, and how it can be studied (Hammersley, 1996). On the one side, research that was labelled as 'positivist' or 'scientific' was aligned with quantitative methods, while on the other, 'naturalist' or 'interpretivist' approaches championed the superiority of qualitative methods for the production of valid social knowledge (Oakley, 2000). Orthodox sociology remained rooted in classical concerns, with a positivist commitment and established in quantitative methods: indeed, in the 1980s sociology was still more "anchored" in positivist epistemologies than other disciplines

(such as anthropology, or literature), and persisted in favouring abstract, universal terms (Stacey and Thorne, 1985, p. 309; Crompton, 2008; Savage, 2010b). Quantitative methods and qualitative methods attracted “unequal prestige,” and there was a gendered dynamic to this distinction, reinforced by the dominant position of males in the mechanisms for evaluating scholarly work, determining what is printed, and consequently what is recognised as legitimate knowledge (Grant *et al.*, 1987, p. 857). Although women exhibited a more frequent use of qualitative methods compared to men, both men and women were more likely to use quantitative approaches in articles examining gender compared to non-gender articles, which Linda Grant and colleagues attributed to the avoidance of a “double non-conformity” of non-traditional topic and marginalised method (*ibid.*, p. 861). In response to the hegemony of quantitative methods and the ‘positivist’ epistemologies that accompanied them, a conflicting model of sociology emerged based on a radical critique of science, and propelled by post-modernist and post-structuralist currents (Oakley, 1999; Savage, 2010b). This interpretivist ‘revolution’ in the UK was anticipated by parallel sectarian methodological disputes between quantitative/macro and qualitative/micro factions in the United States (May, 2005; Lamont and Swidler, 2014). The influence of feminism began to be evident in sociology journals from the 1970s onwards, and post-modernism from the 1990s (Halsey, 2004).

Ann Oakley has noted how the arrival of feminism as a political movement that infiltrated academia had a particular role in ‘energising’ the paradigm wars in both the UK and the US (Oakley, 2000, p. 32). This emerged from a critique of how ways of knowing were defined, who was doing the defining, and the way that this was patterned to reflect the fundamental divisions in wider culture which marginalised certain knowledges, including women’s knowledges, and devalued the lives and experiences of other socially marginalised groups as worthy of study (Oakley, 2000). As a political and social movement, feminism identified the political importance of using qualitative research methods (Oakley, 1981, 1999). Some expressed caution about the co-optation of gender as a variable in early feminist quantitative research, as this assumes that it is a property of individuals, conceptualised as sex difference, rather than recognising it as a principle of social organisation (Stacey and Thorne, 1985). Others went further in claiming that certain methodologies are innately sexist due to ‘machismo’ elements such as creating controlled realities to be manipulated by (male) social scientists (Stanley and Wise, 1993, p. 34). This preference escalated into a strong philosophical opposition to quantitative methods and the kinds of knowledge they produced, which were perceived by some in this quarter as “the work of the patriarchal devil” (Oakley, 1999, p. 249; Cohen *et al.*, 2011). Oakley identifies three key tenets of the feminist critique of quantitative methods. Firstly, the notion of ‘objectivity,’ equated with ‘positivism,’ was condemned for the authority it was wielded to claim which risked objectivising passive ‘subjects’ and thus providing a justification for systems of domination. Secondly, the notion of ‘expertise,’ which is an innate part

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of quantitative research, set up a starkly unequal power relation between the distant, 'expert' researcher and the researched, which was at odds with the emancipatory and non-hierarchical aspirations of feminist epistemologies. Thirdly, the "corrupting effects of numbers" were condemned, as these create an artificial and abstract representation of reality that devalues people and their experiences (Oakley, 2000, p. 36).

These critiques were not solely 'owned' by feminists, and other voices advocating qualitative research for technical, philosophical or ideological reasons joined the fray. Nevertheless, a fundamental, though covert, aspect of the paradigm war was the gendering of methodology, which perhaps goes some distance to explaining the emotion that the oppositional framing of 'qualitative' and 'quantitative' engenders (ibid., pp. 23, 4-5). An epistemological version of the qualitative-quantitative distinction was assumed by the parties in this conflict, who perceived these methodological approaches to be "incommensurable, that is, they are based on such divergent and incompatible assumptions and presuppositions that they are incapable of resolution or of combination" (Bryman, 1998, p. 140). The terminology of "paradigms" itself sets up two normative, mutually exclusive universes of scientific assumptions which enable adherents to bind themselves to one side or the other in the committed belief that these worldviews are absolute and irreconcilable (Oakley, 2000). As a result of these critical movements, by the 1990s, journal publications in the UK indicate a substantial displacement of empiricism by interpretivism (Halsey, 2004; Crothers, 2011).

Beginning in the 1980s, and with increasing weight in the 1990s, critical voices emerged which questioned the chasm-like divide between qualitative and quantitative approaches (Bryman, 1984, 1988; Hammersley, 1996; Bryman, 1998). A 'technical' version of the qualitative-quantitative distinction increasingly gained currency, which denies the epistemological determinism of method the paradigm debate rested on. From this stance, the choice of method is seen as related to the research question, rather than a prior commitment (Bryman, 1998; Oakley, 1999; Lamont and Swidler, 2014). 'Qualitative' and 'quantitative' are thus recast as either a sliding scale (Hammersley, 1996) or 'modes' (Oakley, 1999) which can be flexibly adopted, or even combined (Bryman, 1998).

There are (at least) four grounds for the way that methodological pluralism has gained traction in British sociology. Firstly, it may imply that there is increased acceptance of the possibility that there might be more than one valid and useful way of knowing about the social world. Many now accept that such epistemological pluralism is a sign of health in a substantive research field (Isaeva *et al.*, 2015). It is noteworthy that this still allows for relatively bounded epistemological communities existing in tandem, which may retain silos for the application of method rather than integration.



Secondly, and perhaps most strikingly, the growth of epistemological convictions that embrace a variety of methods has created significant space for pluralism. Realism has become increasingly evident as the epistemological underpinning of much research, both qualitative and quantitative, as it conceives of method as a means “to provide some sort of approximation to what is ‘really’ going on” (Hammersley, 1996; Bryman, 1998; Oakley, 1999, p. 252). The central notion of ontic depth denotes “a conceptual map of the world’s nature that allows for multiple layers, complexity, interweaving and dynamic interaction of the parts of that world” (Olsen, 2010, p. xxi). Given this complexity, understanding why events observed empirically happened the way they did requires both “intensive” methods, which engage with the causal process of mechanisms and structures that operate behind concrete events, and “extensive” methods, which seek out common properties or general patterns in a whole population (Sayer, 1992, p. 242; Olsen, 2010). This provides a coherent basis for the use of qualitative, quantitative or mixed methods (McEvoy and Richards, 2006).

Thirdly, given the pluralistic orientation of Bourdieu’s reflexive mode of sociology outlined above, I would argue that the increasing canonicity of his thought in British sociology has fostered greater receptiveness to methodological pluralism (Outhwaite, 2009; Thatcher *et al.*, 2016; Adkins *et al.*, 2017). Finally, as Max Travers has noted, the cultural conditions of modernity have tended to place a premium on newness and innovation, and this “pursuit of the new” extends to research funding and publication practices (Travers, 2009, p. 162). As a consequence, there is a pressure to innovate methodologically, as well as in other respects, which necessitates moving beyond established methods (or at least appearing to).

To resume the example of feminism, there has been significant (although not absolute) acceptance of quantitative methods as a viable form of knowledge production, reflecting the epistemological openness to a range of methods that I observed in my initial discussion of feminism. They have engaged critically with the limitations of qualitative research, in particular through acknowledging that the intimacy and apparent mutuality of qualitative methods makes them amenable to greater deception and exploitation of participants than many ‘positivist’ methods (Stacey, 1988; Oakley, 2000). Oakley argues that women and other minority groups need ‘quantitative’ research in order to distinguish between personal experience and collective oppression, to show the *extent* to which men and women are structurally differentiated: in this sense, quantitative methods and questions of voice are not mutually exclusive (Oakley, 1999, 2000; Lauder *et al.*, 2004). In a review of ‘women-oriented journals’, Rachel Cohen and colleagues (2011) found that 51% of articles used quantitative methods, either alone or in combination with qualitative. Nevertheless, geographical and disciplinary factors are significant: the US is exceptional in the more widespread use of quantitative methods compared to other countries (including the UK), as are articles in the disciplines of economics, medicine and psychology, compared to interdisciplinary Gender or Feminist Studies

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journals (Hughes and Cohen, 2010; Cohen *et al.*, 2011; Hughes and Cohen, 2012). Although the 'orthodoxy' that feminists use qualitative approaches lingers, there is a growing appetite to "re-open quantitative methodologies to critical feminist epistemological and feminist empirics" (Hughes and Cohen, 2012, p. 2). This is enhanced by the opportunities presented by digitised and technological developments enabling the analysis of complexity, which some feminist researchers are innovatively embracing (Hughes and Cohen, 2010).

It is questionable whether the growth of methodological pluralism in British sociology represents a happy resolution to the paradigm wars of the 1970s and 1980s, or a more uneasy truce. Some claim that the distinction between qualitative and quantitative was political rather than real and, in light of this, the debate has "burned itself out": sociological methods have converged and the terms 'qualitative' and 'quantitative' no longer carry any meaning (Hanson, 2008, p. 106). Others suggest a more cautious 'coexistence' than this apparent resolution (Lamont and Swidler, 2014); indeed, a number of sociologists have observed continued explicit claims which assert certain methods over others (Crompton, 2008; Cohen *et al.*, 2011; Williams *et al.*, 2016a). Lamont and Swidler make the pluralist argument that:

"Different methods shine under different lights ... one should choose the most appropriate data collection technique based on the question being asked and the types of facts and theories one wants to operate with." (Lamont and Swidler, 2014, p. 166)

The implication is that, at a disciplinary level, this mode of practice should result in a more balanced range of methods being put into practice. Payne, Williams and Chamberlain (2004) follow Colin Bell (Bell and Newby, 1977; Bell and Roberts, 1984) in their assertion that it is the productive output of the discipline that should be the measure of pluralism, rather than the work of individual researchers (although a wide grasp of methods at an individual level is also needed in order to critique and evaluate sociological evidence - see Payne, 2007; Crompton, 2008). They describe the gap between the profession of pluralism and its actualisation in practice within British sociology as follows:

"An expanding discipline generally provided sufficient space for proponents of one methodology to work around those who espoused a rival position. Sociologists were largely able to subscribe to the view that, provided they themselves were not forced into research using methods that they personally found uncongenial, there was no absolute reason to prevent others from using alternative methods. Nobody asked whether sociological production as a whole could be characterized as methodologically pluralist." (Payne *et al.*, 2004, p. 155)

The extent of methodological pluralism, from this view, is more substantially indicated by examining the sum of sociological practice, rather than by the claims of its contributors. Taking this as a gauge, there are a number of worrying signs of morbidity confronting British sociology. Of these, the deficit of quantitative research (and skills) and the postulated over-commitment to 'traditional' methods have constituted the most prominent debates in recent years.

The deficit of quantitative research in British sociology is a widespread contemporary concern, and the greatest source of vulnerability to methodological pluralism in the UK at present (Williams *et al.*, 2008; Williams *et al.*, 2016a). As Geoff Payne (2007) has commented, this issue is important insofar as topics that lend themselves to quantitative exploration are worth studying, that society-wide patterns are valued aspects of sociology, and that sociology should have a role in public life. Those sociologists who would object to these normative assertions are surely in the minority, and unlikely to be among the pluralist mainstream. Having identified a knowledge vacuum in the evaluation of the totality of British sociological research, Payne and colleagues (2004) undertook a documentary analysis of published sources in mainstream British sociological journals, with the addition of papers presented at the most recent British Sociological Association (BSA) conference. They treated these published materials as artefacts of sociological production, indicative of the outputs of the discipline as a whole. Despite minimal criteria for qualification as containing a 'quantitative' component, they found no genuine plurality of methods in the national discipline, with only one in twenty published papers using quantitative analysis.

This approach has limitations, many of which the authors acknowledge (see discussion based on Platt, 2016, above). In particular, it has been argued that many British sociologists who engage in quantitative analysis of large-scale datasets are likely to publish internationally due to the perception of what is an appropriate outlet for their work, while multidisciplinary research groups are likely to target specialist journals that are not included in this study (May, 2005). There is evidence that supports both the notion of the globalisation of academic life, and the increasing number of specialist journals of international orientation in recent years (Outhwaite, 2009; Crothers, 2011). Nevertheless, what Payne and colleagues found is corroborated by numerous other sources. The (2010) benchmarking review undertaken by the BSA, Economic and Social Research Council (ESRC), and Heads and Professors of Sociology (HaPS) assessed that while the UK is pioneering in qualitative methods and in some mixed methods approaches, it lags behind international standards in quantitative approaches. The review found that only one in five End of Awards reports from ESRC sociology projects were quantitative. Similarly, in an assessment of the methods used by PhD students, Rose Wiles and colleagues (2009) found a general preference for qualitative methods in the social sciences, but this was most pronounced among sociology doctoral researchers, of whom 78% of respondents to a 2005 survey were using qualitative methods. This

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contrasts with the requirements of academic employers who were also surveyed: of those who responded, 65% required quantitative skills, and many identified a lack of statistical competency in the pool of potential applicants. In a more recent assessment of advanced training needs in the social sciences, concern over the deficit in quantitative skills continued to loom large (Durrant *et al.*, 2015).

There is general agreement that a quantitative programme is needed in British sociology at undergraduate level to address the supply of quantitatively skilled sociologists at doctoral and higher academic levels. What that programme should comprise is open to debate (Byrne, 2012). Much depends on the underlying reasons that explain why undergraduate training is not fostering greater uptake of quantitative methods. Malcolm Williams and colleagues (2016b) identify three likely issues.

Firstly, there may be a lack of ability or fear of number among students. This is no doubt part of the picture, but does not explain the problem sufficiently. A 2006 survey of second and third year undergraduate students in England and Wales found that while 42.9% of respondents reported a bad experience of maths at school, 50.1% did not, and while 41.9% perceived themselves not to be good at maths, 44.1% disagreed with this statement (Williams *et al.*, 2008). Further, between 2007 and 2013, the proportion of sociology students who had taken maths at A-level had risen from 14.8 to 20.3%, and there had also been a slight shift in perception of sociology, with a rise in the number of students perceiving it as closer to science than to humanities in 2013 compared to 2007 (Williams *et al.*, 2016a). Sociology undergraduate cohorts are clearly a heterogeneous group in terms of their number-confidence.

Secondly, the issue may be the overwhelming qualitative bias of current professorial sociology. A legacy of the paradigm wars is a “lost generation” of quantitative sociologists, which filters to subsequent generations as today’s teachers lack a strong foundation in quantitative skills (Crompton, 2008; Williams *et al.*, 2016a, p. 185). An examination of the perceived difficulty of various statistical techniques among undergraduates indicated a very basic level of understanding, with few students reporting having studied measures of association (Williams *et al.*, 2008). There may also be some continued explicit anti-quantitative attitudes transmitted through teaching (Byrne, 2012). The upshot of these factors is “a complex anti-quantitative mind set among some students that combines fear of number with a misunderstanding and lack of enthusiasm for quantitative work” (Williams *et al.*, 2008, 1013).

A final issue may be the separation of ‘knowing’ from ‘doing’ in sociology teaching. Williams and colleagues (2016b) have observed how quantitative modules exist in a ‘ghetto’ in sociology curricula, which prevents experimental learning from taking place. A small quasi-experiment

comparing the practice of embedding quantitative content in a substantive module compared to 'traditional' methods modules yielded complex results. While the group taking these options were less likely to be distrustful of statistics and more positive about the role of statistics in sociological research, their preference for essay writing also increased and they became less confident in their statistical abilities (Williams *et al.*, 2016b). The implication is that no one intervention will singularly resolve the issue, and there is a need for further comparative studies to understand more fully what is going on (*ibid.*).

It is clear that the quantitative skills deficit in British sociology remains an important, though complex, problem to resolve (Williams *et al.*, 2016a). It is strong evidence of the limits to disciplinary methodological pluralism in British sociology. What is exemplary, for present purposes, is the way in which the investigation of this problem is productively harnessing a diverse range of sociological methods to enhance understanding of the field itself, and to reflexively take remedial action.

Another body of work has questioned the outward expression of pluralism in British sociology on somewhat different terms, which I touch briefly on in the first paper of this thesis. Savage and Burrows (2007) have asserted that empirical sociology in Britain is approaching a crisis. This is based on the belief that the authority over the domain of the social that academic sociologists came to occupy in the post-war era was gained by virtue of the empirical research technologies over which they had jurisdiction: in particular, the sample survey and the in-depth interview. During this period social scientists in general and sociologists in particular could be forgiven for assuming an "intrinsic monopoly of social knowledge" (Savage and Burrows, 2009, p. 764) on account of their role in data-generation; however, our "methodological repertoires" (2007, p. 886) have been eclipsed by the routine way that data is now gathered and analysed, with a great degree of sophistication, and without any reference to academic sociology. The driver to this shift in the data landscape is the emergence of 'knowing capitalism' (Thrift, 2005), a new era in which capitalism has become increasingly reflexive, seeking "to know itself in ever more precise ways in order to better extract value" (Burrows and Savage, 2014, p. 2), and "make[ing] business out of, thinking the everyday" (Thrift 2005:1, quoted in Savage and Burrows, 2007, p. 886). Thus 'commercial sociology' (Burrows and Gane, 2006) has come to predominate and the position of academic social science is increasingly peripheral to knowledge of the social.

The phenomenon they identified is now commonly known as 'big data' (Burrows and Savage, 2014). Also more descriptively termed 'organic data' (Groves, 2011; Couper, 2013), this is digital information generated by systems automatically as a byproduct of a population's engagement with commercial bodies (transactional data), governmental institutions (administrative data) or

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each other (social media data). Of importance to the debate (and with the exception of social media data), this is (in theory at least) *complete data* on particular populations, thus negating the need for inference (Savage and Burrows, 2007, 2009), and it records *actual activities* rather than reported behaviour (Webber, 2009), eliminating some measurement error. It also requires no special effort (and little cost) to collect; however, it is privately owned, and access limitations can be significant (Savage and Burrows, 2007; Elliot *et al.*, 2013).

A key component of Savage and Burrows' argument is that 'traditional' research methods, namely the social survey and the in-depth interview, are increasingly peripheral to the production of social knowledge. If sociology fails to engage with the tide of big data and remains entrenched in these methods, its future is in jeopardy. To take the case of the survey, on account of plummeting response rates, the diminishing relevance of the nation state in delimiting populations and the commodification of data in commercial survey research, they boldly assert:

"It is unlikely ... that in the future the sample survey will be a particularly important research tool, and those sociologists who stake the expertise of their discipline to this method might want to reflect on whether this might leave them exposed to marginalization or even redundancy." (Savage and Burrows, 2007, p. 892)

In the interests of polemic, Savage and Burrows take a deliberately provocative tone, and this has been extremely successful in inspiring debate (and dissent). On the one hand, they argue against a singular focus on a limited set of methods, which they appear to perceive among some of their contemporaries (Savage and Burrows, 2007; Crompton, 2008). However, in exaggerating the near redundancy of the survey in light of the possibilities created through transactional data, they come close to fetishizing a different set of quantitative methods themselves. Indeed, in a subsequent paper they concede that that "surveys will remain important," stressing that "it is their unquestioning and assumed superiority that we would indeed wish to question" (2009, p. 767) – as would any reflexive methodological pluralist. There have indeed been major shifts in the data landscape but, as I argue in the first paper, it is unproductive to replace the old dichotomy between qualitative and quantitative paradigms/methods/sociologists with a new one between 'traditional' and 'new' data sources and analytic methods, and those who use them. In this regard, the approach taken by Elliot, Purdam and Mackey (2013) is of far greater promise. They propose that, in this "age of data," there is a shift in thinking from 'data sets' to '*data arrays*' which presents opportunities for researchers to combine different types of data in a way that bypasses traditional divides (Elliot *et al.*, 2013, pp. 9, 17). Nevertheless, in response to Savage and Burrows' call for attention to big data there has been a growth in debate alongside the establishment of a number of research

centres and groups, and addressing big data has become a major strategic priority for the ESRC's investments (Elliott, 2016; Williams *et al.*, 2016a).

Although these are two of the most prominent debates which question the practice of methodological pluralism in contemporary British sociology, they are not the only ones. For example, some sociologists have called for greater engagement with historical methods in sociological training and practice. This historical sensitivity is part of sociology's self-awareness, and without it the discipline risks uncritically reflecting the contemporary order, rather than interrogating it (Inglis, 2013; Kennedy, 2015). Between some types of qualitative enquiry, "intensified boundary work" has also been observed (Lamont and Swidler, 2014, p. 2). Taken in the round, these examples are sufficient to pose the question, why has a lack of pluralism come about, and should we be worried?

If British sociologists' profession of methodological pluralism is to be believed, there may be structural conditions that are constraining them in practice. Commentators observe substantive, theoretical and methodological "hyper-specialisation" in sociology, which creates strongly bounded fields of practice. This limits the potential intersectionality between fields that would otherwise promote knowledge production: there lacks a shared language or sufficient contexts for interaction (May, 2005; Payne, 2007). This tendency is fuelled by what John Holmwood (2010) describes as the 'enrolment' of British academics in the techniques of neo-liberal governmentality. The Research Assessment Exercise (RAE), now replaced by the Research Excellence Framework (REF), he argues, constructs excellence according to the silos of its subject sub-panels, which exacerbates disciplinary fragmentation. Further, this assessment regime places a premium on individual achievement, and limits the scope of sociological research by encouraging academic researchers to choose 'safe' options, such as repeatedly publishing in the same area (May, 2005; Gane, 2012). In this individualised institutional environment, research which is the outcome of individual creative labour and conducive to conferring intellectual standing is rewarded (May, 2005). At a more junior level, while some doctoral researchers benefit from integration in a research group, many PhDs are still solitary, minimally resourced exercises. These require approaches that are inexpensive and achievable through the investment of individual time and effort, which significantly limits the scope of what is methodologically possible (May, 2005). The instrumental focus on PhD output encouraged in this environment is reflected in the lack of priority placed on general skills advancement during doctoral studies (Wiles *et al.*, 2009; notwithstanding the considerable efforts invested by some ESRC Doctoral Training Centres and their successors, Doctoral Training Partnerships, to foster cross-cutting skills across disciplines). These institutional factors imply that many practical methodological decisions may be made for pragmatic, rather than ideological reasons (May, 2005).

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Evidence of a lack of pluralism is innately problematic from the Bourdieusian perspective taken here, as it is indicative of unreflexive sociological practice and failings in the construction of the object of research. There are a number of specific and interconnected concerns raised in the sociological literature about the consequences of methodological deficiencies in the discipline. Firstly, and as Bourdieu's perspective suggests, it inhibits our ability to know the social world. Some research topics particularly lend themselves to certain modes of exploration and, as a result of failure to significantly engage with the full range of possible approaches, "there is a real prospect of sociology locking itself out of wide areas of research problems" (Payne *et al.*, 2004, p. 162; Payne, 2007).

Secondly, and as a consequence, the relevance of sociology to public policy is compromised – a serious concern, in so far as this is perceived as a core purpose of sociological enquiry (Williams and May, 2002)<sup>4</sup>. Bryman (1998) interprets the qualitative-quantitative distinction as representing different rhetorics of persuasion. Weakness in either area is therefore liable to impact on sociologists' corporate ability to make their case in the public domain. There have been numerous calls across a range of sociological persuasions for sociology to engage more deeply with social problems or public issues, and to seek greater integration in policy-making (Oakley, 1999; Lauder *et al.*, 2004; Burawoy, 2005). However, there is a legacy of "mutual incomprehension" between academic social scientists and policy makers that needs to be overcome: this will require interaction, and education and effort on both sides (Johnson, 2004, p. 29; May, 2005). If social scientists are not providing a foundation of social knowledge to governments, other, less informed commentators will fill this void, and policy will suffer as a result (Johnson, 2004). Research data now proliferate, which poses a challenge to the dominance of university research (McKie and Ryan, 2016). As the discussion of 'knowing capitalism' above indicates, universities are no longer the privileged space for research. The commercial value of knowledge has led to its exploitation by private sector bodies (Holmwood, 2010): Skeggs and Yuill's (2016) research on Facebook's tracking of users to create marketable social knowledge provides a sobering example. There is also a growth of social research which operates as a 'technology' in the public and voluntary sectors. Social

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<sup>4</sup> I follow Williams and May (2002) in their observation that while all social scientific enquiry aims for 'illumination' through widening theoretical and empirical knowledge, the aspiration of transformation as a consequence is by no means a prerequisite. Rather, there is a continuum among social enquirers around the extent to which commitment – normative views as citizens – should impact on how research is done and what it achieves. I also note that although much policy-orientated research exhibits a preference for quantification, in some applied sub-fields (Carl May highlights workplace studies and sociology of health and illness) qualitative studies attract significant funding (Hughes and Cohen, 2012; May, 2005). I raise the issue of policy relevance here because it is prominent in the discussion within British Sociology around methodological pluralism, not because I consider this the only or main purpose of sociological enquiry as a whole. I also acknowledge, as will be evident in the next section, that ideological commitments have featured prominently in my own trajectory into sociology.



researchers trained in generic investigative skills, and lacking in a theoretical basis to understand the social world, are engaged in what Williams describes as “engineering solutions to problems” (Williams, 2000, p. 162). This, he argues, takes place with little, if any, connection to disciplinary social science.

Some observers have expressed deep concern over the implications for sociology if it fails to defend its place in the production of socially-relevant knowledge. John Holmwood is particularly pessimistic. Other social science disciplines have a more committed knowledge base, established through relative methodological uniformity: econometric modelling in economics, ethnography in anthropology, or archival research in history (Holmwood, 2010). This ‘openness’ in sociology, while perceived by some as a source of vibrancy (Savage, 2010b), can make it difficult for those outside academia to appreciate the uses of sociological knowledge (Johnson, 2004). This contributes to what Holmwood terms a crisis of disciplinary integration, as sociology lacks a theoretical, substantive and methodological ‘core’. As an exporter discipline, and given the rise of ‘mode 2 knowledge’ which is focused on application, it thus runs the risk of whole areas migrating outside of sociology (Holmwood, 2010). Indeed, in an environment of evidence-based policy Williams has observed that sub-disciplines which once represented a division of labour now risk being jettisoned altogether (Williams, 2000). This leaves sociology with a fundamental lack of evidence of its contribution within the neo-liberal governance structures of contemporary higher education (Holmwood, 2010). At the extreme, the kinds of consequences envisaged are bleak: inability to secure funding, redundancies from shrinking departments, and wholesale departmental closures.

To place these anxieties in perspective, it is worth noting that some sociologists have commented on sociology’s perpetual sense of crisis, and otherwise self-critical tendencies (Payne, 2007; Savage, 2010b; Crothers, 2011). It is important to remember that within a reflexive mode of sociology, the objective is to make research *more robust* through engagement with limitations, so the outcome should be to strengthen the discipline. Mike Savage (2010b) suggests a much healthier outlook for sociology in terms of the numbers of sociologists, their ability to secure funding and their prominent role in interdisciplinary groups – particularly in research-intensive universities. He also cites the 2010 benchmarking review which suggested that British sociology is second only to the USA in “intellectual density and vibrancy” (Savage, 2010b, p. 664). In responding to the deficiencies of methodological pluralism in British sociological practice, it is helpful to remember Gouldner’s approach, as one of the early ‘crisis claimants.’ He writes:

“The central implication of a crisis is *not*, of course, that the ‘patient will die.’ Rather the implication is that a system in crisis may, relatively soon, become something quite different than it has been. A system undergoing crisis will change in significant ways from

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its present condition. While some of these changes may be only temporary and may soon restore the system to its previous condition, this is not the distinctive implication of a system crisis. A crisis, rather, points to the possibility of change that may be more permanent, producing a basic metamorphosis in the total character.” (Gouldner, 1970, p. 341)

In so far as the hyperbolic terminology of ‘crisis’ is useful, therefore, it is in this sense of transformative opportunity, rather than imminent demise. The majority of British sociologists engaged with gaps in the reality of methodological pluralism take this view that these areas present challenges and opportunities (McKie and Ryan, 2016). This productive outlook is demonstrated in the response to the issues of quantitative deficit and big data that I have examined in detail above.

I have described how a methodologically pluralistic commitment emerged within British sociology as a truce to the paradigm wars of the 1970s and 1980s, which pitted qualitative and quantitative methods against each other on epistemological grounds. This involved recentring research methods on the particular question at hand, rather than associating them with absolute philosophical commitments. However, and despite widespread ascent to methodological pluralism in principle, the collective output of British sociologists demonstrates a gap between profession and reality. In particular, there is a bias away from quantitative methods in research practice and training, and there has been some question as to whether sociology succeeds in keeping up with the rapidly changing data environment. Nevertheless, these introspective tendencies are a productive aspect of a reflexive discipline, provided that they are positively embraced as means of increasing rigorous scientific practice, and that they are encountered as challenges to be overcome, and opportunities to be grasped. This is the disciplinary field into which the papers that comprise this thesis make their specific interventions – in more or less unexpected ways. I now turn to a reflexive account of how this thesis came about through recounting my own intellectual autobiography.

### **1.4 A journey towards pluralism: An unintended thesis by an inadvertent sociologist**

*“How can we claim to engage in the scientific investigation of presuppositions if we do not work to gain knowledge [science] of our own presuppositions?”* (Bourdieu, 1999 [1993], p. 608)

The 'reflexive return' that involves 'objectifying one's own universe' does not stop at observation of the field within which my work is situated. It also involves attention to my own social position within that field, and within other fields that have shaped my trajectory and dispositions. These create the parameters of the "space of possibilities that offered itself to me" (Bourdieu, 2008 [2004], p. 30). Through such reflection, it is possible to objectify and, potentially, transcend these limitations. I therefore turn now to an account of the biographically situated origins of my ideas about methodological pluralism and, parallel to that process, how I came to self-identify as a sociologist.

Although Bourdieu was committed to self-analysis as an aspect of rigorous reflexive sociological practice, he did not make his own account of himself until late in his career and, when he did so, he famously denied it as an 'autobiography' (Bourdieu, 2008 [2004]). He described his task instead as "taking account of my position and its evolution over time ... so as to try to control the effects they could have on my scientific position-takings" (Bourdieu, 2008 [2004], p. 111). His caution may be explained by his scepticism of what he saw as self-referential confessional accounts within anthropology (Bryman, 1998). Liz Stanley's notion of 'intellectual autobiography' has provided a means of reclaiming the term 'autobiography' as a legitimate and essential research practice.

Stanley seeks to deconstruct the intellectual divide between researchers and 'the researched' through reminding researchers that their understandings are "necessarily temporally, intellectually, politically and emotionally grounded and are thus as contextually specific as those of 'the researched'" (Stanley and Wise, 1990, p. 23). She advocates a different approach to this power relationship embedded in feminist praxis: "to make the researcher vulnerable, in the sense of presenting their reasonings, deductions and evidence to others" (Stanley, 1984, p. 204). She acknowledges, in a similar vein to Bourdieu, that as researchers we all construct understanding of our research objects. By presenting this intellectual experience openly, this work of construction becomes visible, and so the "research labour process" is uncovered (Stanley, 1984; Stanley and Wise, 1990, p. 43). By "concretely and analytically" locating the product of feminist academic labour within the process by which it was produced, the opportunity to create "unalienated knowledge" emerges (Stanley, 1990, p. 12). Each of my papers in a different way challenges the distanciation between the researcher and the object of their research, the power dynamics that this sets up and the potential for symbolic violence that ensues. I hope that this autobiographical account goes some distance towards making the particularities of my intellectual trajectory apparent, in order to ground the ideas that I claim within the conditions of their production.

For simplicity's sake I have chosen a roughly chronological version of my story, picking up on key experiences that, with hindsight, I perceive to be formative of my thinking in this thesis. In

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describing where I am coming from and how I got here in this way, there is a risk of presenting a deterministic picture where my thinking is an inevitable consequence of socialisation and formative events that have become ingrained in my perception. In a crude kind of defence, I simply observe that my trajectory is something that I have experienced as idiosyncratic, less predictable than I would have liked, and demanding of 'on the hoof' strategies to muddle through.

To get the categories out of the way, I am a white, British, middle-class woman in my thirties. More specifically, I am the middle-class daughter of middle-class parents and (mostly) working-class grandparents. My paternal grandmother grew up in rural Derbyshire and was the first in her family to go to university when she trained as a teacher at Homerton College, Cambridge. Two further grandparents grew up within the sound of the Bow bells.

While I was at secondary school my mother trained in evening classes as a person-centred counsellor. Her non-judgemental disposition, profound interest in others and empathy instilled in me a reverence for deeply personal narratives and experiences, and taught me to value the inner life of individuals. Whilst at school, I discovered an emergent concern with issues of global injustice and inequality. In a particular Personal, Social and Health Education (PSHE) session, a teacher showed us a video by Amnesty International which deeply troubled me; I also felt a shift in my orientation, and in the sorts of things I wanted to work towards. Through my developing Christian faith, I had an ontological belief in the connectedness of individual and communal experience. This was based on the dignity of human beings made in God's image and in specific relationship to Him, while related to each other in a 'family' community which is compared to a single body with many parts. I was also convicted that we should challenge oppression and actively take the side of those marginalised by society.<sup>5</sup> This solidified when prior to and during my undergraduate degree (2002, 2003, 2004) I spent the summers volunteering in a rural Romanian orphanage. The personal stories of the children and young people I befriended fascinated and appalled me. They learned their English through watching television because at school they were not allowed to take it (although they could learn German or Spanish, apparently), because, I was told, "they are orphans, and orphans are stupid." A particular girl, a few years younger than I was, went to great pains to explain that, despite her relatively dark complexion, she was "not a gypsy." Although not quite how I would have articulated it at the time, these accounts were revealing of institutional and everyday forms of oppression and racism which resulted in society-wide horizontal inequalities (Stewart, 2002). I went on to study theology and religious studies at undergraduate level, on the basis that I was interested in people and communities, and what makes them tick. In my third year dissertation I used Amartya Sen's (1999) notion of 'missing women' to shoehorn the topic of violence against

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<sup>5</sup> These ideas are found in Genesis 2, Psalm 139, Ephesians 4, 1 Corinthians 12, and Proverbs 31:8-9.

women into my theology degree, with a view to moving closer towards international development. With hindsight, although I had little understanding of what sociology was about, I was always interested in the connection between the private troubles of individuals and the public issues that concern societies (Mills, 1959). I was unknowingly practicing the sociological “feel” or “eye”, as Bourdieu (1999 [1993], p. 608) puts it, which provides a working instinct for the effects of social structure on what is happening in an immediate social situation.

For some time, I saw academic and creative trajectories as competing choices. At secondary school I became aware that I was academically competent, as the practice of termly subject-by-subject grading created an informal league amongst pupils. At the same time, I enjoyed drawing, theatre, story-writing and working with textiles. Despite possible career paths in creative writing, acting, theatrical costume or fashion design which I seriously contemplated for a time, I made conscious decisions for the ‘safe’ academic route, which I saw as a straight choice between the intellectual and the creative, science and arts, objective and subjective. These distinctions began to unravel, however, as I began to consider how to ‘measure’ or ‘know about’ social things. This started in my first ‘proper’ job, doing community development work in Thornhill, a Southampton estate which hosted a New Deal for Communities (NDC) regeneration programme. One of our challenges was how to demonstrate that our programme was making a difference. I was introduced to the world of outcomes and indicators, with our performance being assessed through the NDC Household Survey delivered by Ipsos Mori (Batty *et al.*, 2010). Within my thematic area, the key measures were the number of people engaged in volunteering and the perception that neighbours look out for each other – loose proxies for the achievements of our community development programme. To provide a fuller account of the difference our projects made, colleagues experimented with measures of ‘soft outcomes’ in order to demonstrate distance travelled by beneficiaries of interventions, while I worked on a ‘model’ for capturing community development that happened in projects across a range of thematic areas (health, education, employment, crime, housing and the built environment, and so on). I considered these approaches as equally valid evidence, but I nevertheless believed that without ratification by the quantitative indicators, our programme would never be rated as successful in DCLG<sup>6</sup> audits.

These experiences shaped the design of my proposed PhD project. There were push and pull factors back into academia. By 2010 I was working for a London local authority, supposedly doing community engagement work within flagship regeneration programmes across the borough; however, I was frustrated by the tokenistic institutional willingness to relinquish any control over

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<sup>6</sup> The Department for Communities and Local Government, which funded and monitored the 39 NDC programmes.

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decisions to residents. I was also commuting about 500 miles a week, perpetually exhausted, and recently married. I was looking for a change, and some recent research into social capital I facilitated at work captured my interest. I saw an opportunity to make the shift to international issues of inequality through pursuing specialist training in social research. So it was in February 2011 I quit my job, wrote a research proposal, and filled the intervening months managing the Census Coverage Survey in Southampton and doing community development consultancy work for some local churches.

I entered academia with a strong practitioner identity. I had little interest in doing a PhD for its own sake: it was a means to the end of doing (and learning how to do) a piece of research that I considered to address an important social issue: how the experience of violent conflict affects trust within and between communities. Through a regional focus on sub-Saharan Africa, I hoped to position myself to work in the international development field in the process. Without any particular background in the social sciences, I drew my proposed research design from a chapter on methods in a book about how to do a PhD (I forget which). It involved a staged mixed methods design, which began with mixed qualitative methods, then used those findings to design a micro-level survey in specific communities. I did not want to be pigeon holed as either a 'quants' or a 'qual' person. Further, I was convinced that accounting for something as intangible as trust required deep, contextualised accounts, while for research to have a public impact these would need to be quantified in some way. 'Qualitative', as far as I knew, meant interviews and focus groups, and 'quantitative' meant surveys, so these are what I planned to use (with some participative twists).

Initially, I considered my work to be firmly interdisciplinary. For the MSc component of my 1+3 funding, I chose sociology over politics and international relations as much to avoid assessment by examination as anything. However, I gradually found a sense of intellectual belonging in sociology (Burton, 2016). This was partly on theoretical grounds – as I read competing theories of trust, I became convinced that it was a relational phenomenon, something that took place between rather than within people. The more I thought about it, the more I suspected this thinking applied to other aspects of social life. I also found in Bourdieu's approach a distinctive 'methodology' that had nothing to do with qualitative or quantitative distinctions, but which was a mode of practice, a craft involving an eclectic range of materials. My sense of home also grew in the post-graduate community of Sociology, Social Policy and Criminology at Southampton University. Each year at our annual Post-Graduate Research (PGR) conference, we all emerged from our camouflage in mixed social science offices and presented our research. I found myself theoretically and substantively captivated on these days: I understood and valued the kind of questions that my colleagues were asking. During the question time, my contribution always seemed to gravitate towards questions

of research method: “How are you going to find out about that?” I began to be faintly disappointed when the response was, “I will conduct some interviews.” What kind of interviews? Why interviews? What are the implications of this apparently innocuous method? Are other data sources available? How might you incorporate them?

My own work continued to take shape. During my first year and (after some initial struggle) aided by Bourdieu’s notion of the construction of the sociological object, I developed a fairly clear working understanding of the key concepts behind my study, including trust, poverty, well-being, violence, conflict, and peace. Through my husband’s medical work in South Sudan we had a number of South Sudanese friends and contacts, both in the UK and in South Sudan. For my MSc dissertation I had undertaken some field-based research in Juba on mothers’ trust in medical care (Ayrton, 2012a, 2012b, 2017b), which had given me an opportunity to lay the groundwork for focusing my doctoral research there. I found a South Sudanese man in London willing to tutor me in Sudanese Arabic and travelled to London for fortnightly lessons in his home. He (and other South Sudanese friends) continually affirmed that my research was “important” for their country, which spurred me on. As I was about to start working up my institutional ethics application for three months’ fieldwork in South Sudan, in December 2013 fighting broke out between elements in the military in Juba. Although my Arabic tutor reassured me that this would be short-lived, the conflict quickly escalated into an all-consuming civil war. I was horrified by what this meant for the South Sudanese people, both physically and symbolically. Letting go of my plans to do research in South Sudan was also an emotionally charged struggle (Ayrton, 2014b).

Having been persuaded that I needed to look at other national contexts for my study, and in the absence of my prior personal connections, I needed the comfort of a systematic rationale for my selection. Using Uppsala Conflict Data Program datasets, I tracked conflict ‘severity’ (number and national distribution of recorded conflict deaths) and duration among countries that had ‘recently’ (in the preceding ten years) emerged from intra-state conflict. Once I eliminated those to which the Foreign and Commonwealth Office advised against travel (the litmus test for University ethics and insurance – at least for an individual doctoral researcher) and those with administrative languages other than English or French, I had a shortlist. Through a lucky break I was put in touch with a senior figure in the International Federation for Red Cross and Red Crescent Societies (IFRC) covering the Africa region, who agreed to a collaboration. Having never been to West Africa, I selected Liberia from my shortlist, and my IFRC colleague made the necessary connections with the Liberia National Red Cross Society (LNRCS). Through data from the Liberian Truth and Reconciliation Commission and the Uppsala Conflict Data Program Georeferenced Event Dataset I was able to map by county the number of battle deaths, civilian deaths, and human rights violations. A colleague in the National Centre for Research Methods (NCRM), Kaisa Puustinen, helped me to

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map these, which I referenced alongside historical literature to understand the dynamics of the Liberian civil wars. My research design was still fundamentally based on interviews and focus groups in the first instance, but incorporating creative approaches such as using marbles for mapping key relationships, and vignettes in focus groups which drew on a famous short story by a Liberian author (Moore, 1968).

I received an email notifying me that the University had approved my ethics application as I travelled home from a planning and orientation trip to Liberia in July 2014. During this time, with a great deal of very generous assistance from the IFRC/LNRCS staff and some friends I made through church-based contacts, I had put in place the practical arrangements for my data collection, which was due to start in September 2014. The outbreak of Ebola Virus Disease was a growing national concern while I was in Monrovia. The LNRCS hosted an Ebola Response Team, which was making trips up and down the country to Lofa County, the main area initially affected. People joked with nervous laughter about Ebola, and gently cautioned me to avoid unnecessary hand-shaking. In July and August, the outbreak escalated dramatically, taking the region in its grip. Not only would it be impossible to travel to Liberia for anything other than humanitarian work for many months to come; the questions I wished to ask were barely relevant any more. Devastated by the unfolding catastrophe, I set about fundraising and writing a blog to raise awareness and support the Red Cross' Ebola outbreak appeal (Ayrton, 2014a). After some negotiation, I also gained agreement from my funders to a six-month extension, to try to rescue my ill-fated PhD.

My final attempt at accessing the field in dogged pursuit of my imagined research was in Northern Uganda. Through one of my supervisors I developed some in-roads in Ugandan universities, initially in Kampala and subsequently in Gulu at the heart of the northern region. I made minor revisions to my research design and these were agreed by the University's ethics committee. Unlike South Sudan or Liberia, Uganda benefits from an institution-based ethics process which governs all research in the country. I identified an appropriate committee and submitted my application. This never arrived, so I submitted it again. Then there was silence for some time, before I received some clarifications on the regulations (which had not previously been forthcoming) which required further information. Through a number of non-dramatic instances of communication break-down, time elapsed. Without any remaining resources to travel to and fro in order to establish face-to-face relationships and trouble-shoot the issues, there was little I could do to speed things up. Eventually, a cut-off point arrived in May 2015 when I judged that the timescales to deliver my heavily empirical project would no longer add up. With the support of my supervisory team, I switched to what Ros Edwards had described as my "emergency break glass here plan": a desk-based three paper thesis drawing on my methodological thinking to date. I duly sat my Upgrade viva the following month and commenced with my new plan.



The origins of the first paper presented here are in some research assistance work I accepted in the uncertain months after the Ebola outbreak prevented progress in Liberia. This involved writing a literature review on the history of the social survey method. While somewhat outside my area, I was intrigued by the early poverty studies, the changing relationships between researchers and researched, and how the wider historical and social context played out in the development of the method in unexpected ways. The paper outgrew its original purpose, and became the starting point for my thesis; an earlier version is published as an NCRM working paper, and cited in a recent article by Rosalind Edwards and colleagues (Edwards *et al.*, 2016; Ayrton, 2017a).

In the second paper, I wanted to make use of my theoretical work on trust. Regretful of the lost participatory methodological emphasis in my study, I questioned whether a creative method could be applied collaboratively by a researcher with a visual artist to aid the conceptualisation and operationalisation of trust. I was interested in unsettling the break between methods that academics use for our own acts of objectification, and those that we consider to be useful devices for research participants. This is the basis for my argument in the second paper.

Having travelled some distance from my original aspirations, I was determined for my third paper to combine methodological discussion with substantive research. I was interested in how South Sudanese national identity had been transformed in the five years since independence. Having come to a highly permissive view of what could constitute 'data', I initially searched for textual resources online that could serve the basis for a discourse analysis. However, on a whim (inspired by my dabbling with visual methods in my second paper) I switched from a text to an image search, and spent the rest of the afternoon trawling through photographs haphazardly preserved online of visual displays of national identity created and displayed by South Sudanese people in the lead up to the referendum on independence in 2011. Rich themes jumped out at me through this informal archive, and I set about expanding it through contacting individual journalists, some of whom generously shared their personal archives with me. Having initially contemplated undertaking a visual analysis of this collection of photographs, I felt uneasy about my lack of qualification to interpret the messages about nation identity that they conveyed. I wasn't there in 2011, I am not a South Sudanese citizen, I have never experienced war, and my citizenship within a nation state has always been something I have taken for granted, rather than something hard-won and long imagined. Instead, I asked members of the South Sudanese diaspora in the UK to join me for focus group discussions, using these photographs as a starting point, taking the approach of analysing the 'audiencing' of the images (Lomax and Fink, 2010; Rose, 2016). These discussions formed the basis of my engagement with the issue of power relations in focus group discussions, in my third paper.

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Through this account I have outlined the social limits that have set the boundaries of my work of objectification. My conception of methodological pluralism, and the ways in which I have put it into practice, have been constructed in the process of pursuing a challenging and, ultimately, unsuccessful thesis. The present work has come out of pushing the parameters of my sociological imagination, and what I conceive to constitute sociological data, and method. Each paper speaks to an aspect of what a fully methodologically pluralistic sociology might look like, given the present conditions of the field. Although they are remarkably different to each other, themes relating to epistemology, methodological reflexivity, ethics, and the relationship between the researcher and the researched run through them. I will return to these connections in the discussion.

In a recent reflection, John Goodwin has suggested that, “Sociology ... appears to have fewer and fewer practitioners who span multiple areas, who are writing about whatever takes their interest and, for me, the discipline is poorer for it” (Goodwin, 2016, 977). Through the connecting narrative of a reflexive mode of methodological pluralism in British sociology, this thesis makes a contribution that is as marked by its breadth as by its specialism.

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## **Chapter 2: Time for a revival? A historical review of the social survey method in Great Britain and the United States, and its current prospects**

### **Abstract**

During the 1980s, the history of the social survey method in the UK and the US evoked significant scholarly attention; however, this has waned in recent years. Drawing on this historical literature, I review the origins and development of the social survey to its current ubiquity in serving the information needs of modern societies. Charles Booth's poverty study of London and World War II form two pivotal moments that set the direction for this history. There has been limited discussion of the trends that shaped the expansion of survey research in the post-war era. I identify the processes of institutionalisation, professionalisation and economisation as setting the course for social survey methods during this period. Historical reflection on the survey method in the late twentieth century made a case for greater integration of theory in empirical findings to enable the survey to continue to function effectively in the production of "argumentative knowledge" (Philip Abrams, in Bulmer 1985:x), which is central to its critical social function. Today, the shifting social environment and changes to the data landscape present new challenges to survey practitioners. I argue for the value of methodological history to enable the survey method, and its practitioners, to reflectively appraise the idiosyncrasies of its development, current practices and future prospects.

### **2.1 Introduction**

It is difficult given the variety and profusion of social surveys in social science, government, business and civil society to imagine a functioning modern society without the survey method as a foundation for knowledge and action. The growing information needs of modernisation have been met to so large a degree by social survey work that the "genealogy" of the research technology (Osborne and Rose, 1999, p. 368) is intertwined with the stories of the societies in which it emerged to such ubiquity. The extent of this conversation between method and object of research is sufficiently significant for the history of method to shed significant light on historical and cultural change (Savage, 2010).

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It is curious, therefore, that the history of the social survey has not yet been established as an enduring area of investigation. The commentators of the method through the twentieth century turned to historical reflection at certain junctures: in particular, with a peak in the 1980s and early 1990s. In this paper I undertake a review of this secondary historical literature. My main task is to provide an overview of the social survey method's particular development, bringing this history up to date. In particular, while existing histories of the social survey describe quantitative growth in survey research following World War II, there is little further identification of trends in this period which may give the (mistaken) impression that meaningful developments in the survey method had reached a conclusion. I therefore propose the processes of institutionalisation, professionalisation and economisation as overarching categories in the survey's recent history. On the basis of this historical review, I evaluate the underlying rationale that inspired historical commentary on the social survey method and discuss implications for the present challenges that the survey and its practitioners face, demonstrating the value of a historical approach within contemporary social science to speak to current debates.

In order to meaningfully discuss history, it is necessary to break it down into units which enable interpretation. I have chosen to arrange the history around two innovative watershed moments in the history of the social survey: that is, events which triggered a step change in the recognition, acknowledgement and uptake of the method as modelled at those junctures through diffusion across a scientific community (Bengry-Howell *et al.*, 2011; Wiles *et al.*, 2013). The first is Charles Booth's publication of *Life and Labour of the People of London*, which was tremendously influential on both sides of the Atlantic (Converse, 1987; Platt, 1991), inspiring a boom in empirical social research. The second is World War II, which provided the context for the survey method to 'prove its worth' (Marsh, 1982) within government, laying the foundation for its institutionalisation and professionalisation in the years that followed.

The broad historical scope of this study inevitably limits the detail with which I am able to treat particular movements or studies mentioned: my material is the historical literature itself rather than the primary sources to which it refers (for a slightly fuller account, see Ayrton, 2017). I have limited the geographical scope of my study to the UK and US in part due to the historical interaction between these English-speaking contexts in relation to survey research and, in particular, due to the shared significance of Booth and World War II as defining moments in this history.

There are other, subtler respects in which this historical review is partial and selective. I argue that selectivity is a necessary facet of historical work, and is driven by (at least) three factors: the proportions, the purpose, and the positioning of the piece. In terms of proportions, any academic work requires difficult decisions about what is essential and relevant, and what must be omitted

due to the necessities of space. As a review article which emerged inductively through a review of secondary literature, the material that this paper includes is inevitably brief in comparison with other, weightier works. Further, many of the movements and individuals to which I make fleeting reference are worthy of extended study in their own right. In some cases, such detailed works have been my sources and are signposted in the text.

Given the limitations of proportions, in shaping historical work the material that is included and excluded and the degree of detail with which it is treated it are driven by the purposes to which it is pursued. For example, Jennifer Platt's (1996) *History of Sociological Research Methods in America* is concerned with sociological research methods at large, which includes (but is not confined to) the social survey, although her geographical and temporal focus is narrower than I attempt here. She is concerned with the non-determinative relationships between general theory, methodological thought and methodological practice, and outlining the connections and disjunctures between these facets of sociological research shapes the focus of the book. Jo Deegan (1990) writes a quite different kind of history in her intellectual biography of Jane Addams. She situates Addams as a sociologist and explores the origins of the gendered split between 'scientific' sociology and social work which took place in early twentieth century America. She does this by connecting these disciplines to the (male) institution of the University of Chicago and the (female) institutions of settlement houses in the US. Different again is the detailed institutional history that Louis Moss (1991) provides in his distinctive insider account of the Government Social Survey in the UK. These examples represent histories of method within a discipline, within the work of an influential individual, and within a specific research institution. My purpose here overlaps with but is distinctive from these concerns. As outlined above, I pursue a historical overview of one particular research method – the social survey – with the threefold purpose of updating this history, of evaluating what concerns give rise to interest in methodological history, and reflecting on how attention to history shapes our response to current methodological dilemmas. As a result, I have emphasised what I perceive to be transitional moments in the history of the survey and the implications of external social influences and public debate on the development of methods. However, I have omitted other research methods that are significant in the history of sociology (Mass Observation, for example) which in many respects interact with the social survey but which I have not judged to be the most prominent influences on shaping its contemporary manifestation.

Thirdly, my social positioning inevitably impacts on the selections that I have made. My socio-cultural background, biography, position within the sociological field and intellectual interests which have brought me to this task affect my judgement of relevance and, equally, what captures my interest. Although the survey method both transcends and permeates a range of disciplines within and around the social sciences, I acknowledge the biases that I bring to this task: as a

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sociologist in a British university, I lean towards these disciplinary and geographical contexts. Substantively, my long-term interest in issues of poverty, inequality and social exclusion have also slanted my attention to where these themes have inspired and interacted with the development of social survey methods.

Jennifer Platt (1996) has observed how accounts of disciplinary origins favour explanations grounded in social theory over attention to research methods. Given that, as I have argued, no single historical narrative can be comprehensive, it is important to draw on a multiplicity of overlapping accounts which are explicit and reflective as to their proportions, purpose and positioning. I make no claims to providing a definitive account; this brief discussion is one of many possible histories, and is best read alongside those of other writers pursuing their own purposes in order to approach something more general. However, I do exemplify the kind of reflective transparency of proportions, purpose and positioning that enables historical accounts to work together in this way to achieve more than the sum of their parts. This history will be useful insofar as my purpose resonates with the reader's own objectives; to the extent that they can make allowances for the proportions of my study; and if it is read in light of the particularities of my positioning.

### **2.2 Origins: social investigation before Booth**

The need to collect quantitative information for the purpose of effective governance is not new. In the United States the constitutionally-mandated census originated from the need to apportion territory into constituencies of equally-sized electorates (Crothers and Platt, 2010; Rossi *et al.*, 1983). In Great Britain, a controversy emerged in the eighteenth century over the size of the population, in particular a fear of decline. A turning point in this debate came with the publication of Malthus' *Essays on Population* in 1798, which conversely projected population over-growth, and influenced Parliament's legislation for the first British census in 1801, although an assessment of military capacity in response to the Napoleonic Wars also played a part (Crothers and Platt, 2010; Kent, 1981; Tonkiss, 2004).

Beyond the establishment of regular censuses, exercises in 'political arithmetic' began to take place around the turn of the nineteenth century. 'Population' emerged as a meaningful concept, conceived as not just a mass of people, but a set of demographic variables that can be measured, compared and investigated in terms of distribution and change over time (Tonkiss, 2004). Osborne and Rose (1999) observe a shift from an 'elite society' to a 'mass society' where it became possible to speak in terms of 'the people as a whole'. Although they consider this process to have happened

by the 1920s, its roots reach much earlier: during the nineteenth century it became not only meaningful but necessary for the purpose of governance to speak in terms of “disembodied social aggregates” (Savage, 2010, p. x).

The twofold motivation of early empirical studies was a growing awareness of the negative consequences of industrialisation and urbanisation, as well as the Enlightenment desire to study society ‘scientifically’. Investigators attempted to identify ‘laws’ that could be quantified and predicted in a manner directly analogous to the natural sciences and, through greater understanding, aspired to enable tighter control over their rapidly changing social world (Bulmer *et al.*, 1991; Crothers and Platt, 2010; Reid, 1987; Tonkiss, 2004). According to Catherine Marsh (1982), the nineteenth century brought with it the need for a new, more deliberate approach to obtain information about what was happening in society, on account of the corruption of social relationships by capitalism. The factory system carefully ‘zoned’ people with similar life-styles in the new towns and cities, strengthening within-group contact among the classes but insulating the middle- and upper-classes from undesirable contact with impoverished migrant workers. Face-to-face relations between classes broke down, “effectively hiding one class from another” (Marsh, 1982, p. 11).

Despite the widely-recognised Victorian ideology of laissez-faire individualism, through the nineteenth century the idea of action to address the ills of society, whether by individuals or the state, arose. Whether or not this originated from social conscience (Crothers and Platt, 2010) is debatable, at least at a general level, although it is common for historians to refer uncritically to ‘Victorian philanthropy’. Michael Gordon (1973) helpfully adapts the popular language of ‘ameliorative social policy’ of the Victorian era to ‘palliative social policy’, in demonstration of the impetus to maintain the status quo of class-structured society that underlay both charitable and governmental interventions. That intervention in the maladies of industrialisation was necessary gradually became beyond dispute through the nineteenth century (Abrams, 1951; Gordon, 1973; Marsh, 1982). The social survey therefore emerged in close relationship to public policy and social reform and in the context of a “community of practice” including both reformers and state officials (Crothers and Platt, 2010). As Mark Abrams asserted, “surveys ... are carried out as an indispensable first step in measuring the dimensions of a social problem, ascertaining its causes, and then deciding on remedial action ... Most surveys have been concerned with curing obviously pathological social conditions.” (quoted in Bulmer *et al.*, 1991, p. 3)

Raymond Kent (1981) describes the ‘social accountants’ as the first to attempt a quantitative examination of social phenomena through both primary data collection and examination of official statistical information. Driven by a firm belief in rational governance, they considered the ‘facts’ to

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speak for themselves, and to provide a 'scientific' basis for social reform. This strand of investigation was from the 1830s embodied in the statistical societies (Willcox, 1934). 'Facts' about social conditions were perceived as the "raw material" of a new "science of society" (Reid, 1987, p. 3; Tonkiss, 2004). A sharp division was envisaged between the collection of 'facts' and the policy decisions that would be made on their basis (Bulmer *et al.*, 1991; Kent, 1981; Marsh, 1982). Despite pretensions to "exclude all opinions" (London Statistical Society, quoted in Marsh, 1982, p. 14), it must however be noted that the sponsors of these surveys restricted the release of inconvenient facts, for example relating to wages and the relationship of the factory system to poor living conditions (Marsh, 1982).

The 'social accountants' were the first to move beyond description to explanation and, in the attempt to test specifically formulated hypotheses, mark the "birth" of empirical sociology (Kent, 1981, p. 6; Reid, 1987). The sophistication of the statistical societies' studies, such as the use of precodes, percentages, and carefully-trained interviewers following a schedule, has been noted by Marsh (1982), who suggests that their contribution is often neglected through an excessive focus on the 'philanthropic investigators' who will be the focus of the next section. Despite the presence of the societies themselves as organisations that transcended individual achievements (Willcox, 1934), Marsh attributes this lack of methodological consolidation and retention to the absence of research organisations that could code, disseminate and cumulate their procedures and experience. As long as quantitative research continued to be conducted by wealthy individuals, it would be unable to institutionalise and thus provide a platform for continuous consolidation.

By the mid-nineteenth century, there was a shift from the collection of rather dry facts to direct observation by middle class observers: the 'social explorers' (Kent, 1981). The plight of the poor attracted significant attention in both fiction and non-fiction from the 1840s and 1850s. This tended towards detailed and often emotive qualitative accounts, where the urban poor are seen as:

"strange 'tribes' or 'wandering hordes' who inhabit a separate 'territory' or 'dark continent' that remains to be 'penetrated' like the darkest forests of Africa and yet can be 'discovered' by the middle class and the wealthy in the very heart of Britain's industrial cities." (Kent, 1981, p. 37-8)

'The poor' were portrayed as an exotic other, which could be subjugated to the gaze of the affluent observer. There has been little consideration of how the fascination of the middle- and upper-classes in the nineteenth century with their new urban poor parallels the information-gathering practices of colonial administrators for the purpose of subduing and exploiting their territories,

despite the strikingly orientalisising language (Said, 1978; see Baucom, 1999, and Varma, 2011, for exceptions).

Henry Mayhew's *London Labour and London Poor* (1861) is perhaps the most influential example of this kind of political radical investigative journalism (Marsh, 1982). His interest was in the conditions of employment and, particularly, wage levels of the poor in London, on the implicit assumption that low wages were the cause of poverty (Kent, 1981). His approach was to talk to people as a "disinterested observer", and this aspect of "detached observation" would over time become an integral part of the survey method (Bulmer *et al.*, 1991, p. 14). Another noteworthy aspect of his work is the use of direct questioning. Mayhew's assertion that the majority of London's needlewomen and journeymen tailors subsisted on twopence-halfpenny per day was rejected in *The Economist* as "entirely false and irreconcilable with known, recorded and public facts", being derived from the accounts of the poor themselves, whose "utter untrustworthiness" was well known (Thompson, 1973, p. 43, in Marsh, 1982, p. 18). As Marsh has observed, "the idea of interviewing a *respondent*, who was at the same time the subject of the inquiry and the informant, was very slow to develop" (Marsh, 1982, p. 19, *italics original*).

From around the 1880s the settlement house movement began in Britain, and was quickly emulated in the United States: indeed there is good evidence of interchange between the two, including a visit to Toynbee Hall by Jane Addams in 1889 which was formative in shaping the vocation she would fashion at Hull House (Husock, 1992; Platt, 1991; Deegan 1990). Settlement houses involved affluent, reform-minded volunteers, particularly academics and students, taking up residence in 'poor' urban neighbourhoods to "assist the poor and to bind the classes in a common purpose" (Husock, 1992, p. 56). This activity was particularly geared towards engaging future leaders, "bringing them face to face with poverty, and giving them the opportunity to develop practical solutions that they could take with them into national life" (Toynbee Hall, n.d., para. 2). In Britain, settlement houses were particularly effective in strengthening the success of the survey as a policy tool, as the British civil service encouraged elite young men to serve in settlement houses, where they undertook survey work, as part of their transition from undergraduate education to political offices (Bulmer *et al.*, 1991).

For our purposes, it is important to observe at this juncture that these early precursors to the social survey were carried out by private individuals, members of particular professions (notably medical), voluntary associations and journalists. There was no conception of a 'professional' social researcher, no institutional context for such studies to be situated and consolidated, and very little interplay with academia.

### 2.3 Birth: Booth and subsequent foundational studies

Charles Booth's first investigation in the late 1880s is widely considered to be 'the start' of the social survey, in that his study amassed sufficient characteristics in common with the survey of today to be considered its earliest example (Fink, 2005). It laid the foundation for the significant field of poverty studies in the UK, and led to significant policy changes (Abrams, 1951; Moser and Kalton, 1971; Platt, 2014). Booth did not simply collect 'facts' but strived to present "systematic and defensible numerical statements" which he formed into categories (Bulmer *et al.*, 1991, p.42; Tonkiss, 2004), and in so doing brought together empirical investigation and social theorising in a way that anticipated contemporary empirical sociology (Kent, 1981). It is unsurprising, then, that Booth has achieved "a certain mythic proportion that is pleasing in venerable forebears" (Converse, 2009, p. 12).

Booth was a successful businessman who owned a shipping line based in Liverpool. He was a member of the London Statistical Society, and took to exploring the East End of London in order to consider poverty, which he described as the "problem of problems" (Kent, 1981, p. 53). Although commentators often describe Booth as the first in a line of philanthropists it is important not to oversimplify his influences. Booth was highly sceptical of the accounts of Mayhew and others, which he considered to vastly exaggerate the reality of urban hardship. The trigger to his own investigation was a report published in 1885 by the Social Democrat Federation claiming that 25% of the workers in London's working class areas were unable to live on the wages they received (Gordon, 1973). Booth set about to personally disprove this assertion.

The astonishing scope of Booth's study was enabled by the use of what Beatrice Webb termed 'wholesale interviewing' of middle class professional informants - predominantly School Board Visitors, which meant the exclusion of individuals and families without children. This could also be construed as an indication of distrust in objects of enquiry to "speak for themselves" (Marsh, 1982, p. 18); however, it is often overlooked that Booth effectively undertook an ethnography to assist his analysis through spending extended periods as a lodger in the houses of working people to ensure he could fully appreciate the interview data (Abrams, 1951). In a sense he was therefore closer to the objects of his enquiry than some modern survey investigators who may rarely come into contact with respondents through the now tightly structured intermediary role of the interviewer.

Lucinda Platt (2014) has suggested that Booth's researchers were one of his greatest legacies. The effective team of interviewers Booth employed included Beatrice Potter, who would later marry Sydney Webb and establish with him the London School of Economics and Political Science (LSE). She also wrote one of the earliest texts in research methods, *Methods of Social Study* (1932).



Octavia Hill went on to become influential in housing, and Hubert Llewellyn Smith was responsible for a follow-up to Booth's study in the 1930s.

Booth's surprising finding was that the proportion of 'poor' living in London was in fact 30.7%, higher than the 25% figure that he initially took issue with. However, and importantly, only 0.9% fell into his lowest class of 'occasional labourers, loafers, and semi-criminals'. This enabled him to make the claim, which was counter-cultural in the mainstream of the middle- and upper-classes of his time, that a key cause of poverty was conditions of employment rather than individual responsibility. At the same time, he was able to demonstrate that the urban poor did not represent as great a threat to social stability as many imagined – the very poor were “a disgrace, but not a danger” (Kent, 1981, p. 55; Tonkiss 2004). In his conclusions we see only a slight “tempering” of his economic individualism (Gordon, 1973 p. 288):

“Thorough interference on the part of the state with the lives of a small fraction of the population would tend to make it possible, ultimately, to dispense with any Socialist interference in the lives of all the rest.” (Booth, 1892, p. 167, in Marsh, 1982, p. 18)

In this we do not hear not the words of a reformist, nor an indication that in the 1880s the “doctrine of laissez-faire had passed into history” (Abrams, 1951, p. 32). However, Booth does represent a small but enormously significant shift to the *mainstream* of Victorian individualism and concession to the necessity of state intervention, and there soon followed a body of legislation which “altered deeply the character of the British polity” (Abrams, 1951, p. 115).

As a precursor to the modern social survey, Booth's contribution has been side-lined by some for being “untheoretical” and showing continuity with his predecessors rather than innovation (Marsh, 1982, p. 17; Hoinville, 1985; Platt, 2014). For those that take this approach, his primary contribution is as an antecedent to the work of his immediate followers: the idea of replication itself can be seen as an important principle of social enquiry (Platt, 2014).

Seebohm Rowntree was the son of a famous Quaker chocolate manufacturer and philanthropist, Joseph Rowntree, and was thus “brought up in an environment in which good business was combined with concern for employee welfare and education” (Platt, 2014, p. 34). His studies concerned his own city of York: the first was conducted in 1899 (published 1901), which he then replicated twice (published in 1942 and 1951 respectively). Although inspired by Booth, his 1899 study added the “touch of an academic approach” (Fink, 2005).

Methodologically, Rowntree continued Booth's geographically defined 'census' approach, but used 'retail' interviewing in the first application of direct questioning at this scale (Bulmer *et al.*, 1991). He employed “professional interviewers” who he valued for both their persistence and for their

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interpersonal skills, as they exercised “no small amount of discernment and tact” (Rowntree, 1902, p. 14, in Platt, 2014, p. 35). His study is generally accepted to be more systematic, for example through the employment of nutritionists to define a subsistence diet (Alcock, 2006). Theoretically, his study formed the benchmark for what would become known as an ‘absolute’ approach to poverty. He showed that the majority of the ‘poor’ in York earned a regular wage but one that was insufficient to meet their basic physical needs (Tonkiss, 2004). He also identified strong life-cycle effects, where families were lifted out of and pushed back into poverty as their circumstances changed over time (Marsh, 1982).

Arthur Bowley is best known for pioneering representative sampling, supporting its endorsement by the International Statistical Institute in 1903, and implementing the approach in his own 1915 study of working class economic conditions in four towns (Hoinville, 1985; Dale and Kotz, 2011). As an academic researcher, he brought the social survey closer to the academy through applying statistical techniques to the measurement of poverty (Bulmer, 1985; Fink, 2005). He also developed methods for adjusting for non-response, paid greater attention to precise question wording, and standardised the definition of the unit under investigation (Fink, 2005; Marsh, 1982; Tonkiss, 2004). His influence extended to the introduction of sampling into British government research, and, through his assistant, Margaret Hogg, to the application of statistics within the Russell Sage Foundation and at Federal Government level in the United States (Hoinville, 1985; Platt, 1991).

Sampling did not catch on quickly; Rowntree, for example, only reluctantly agreed to “test” sampling in his third poverty study of 1950 (Platt, 2014). Catherine Marsh has attributed this to the Enlightenment philosophy of the early social researchers, who:

“saw their job as filling in the cracks, the unknown parts of the world ... It would have been as unthinkable for these investigators to do a one in ten sample of households as it would have been for a cartographer to map only one square in ten on his grid.”  
(Marsh, 1982, p. 26)

Even within the Royal Statistical Society, the drawing of a biased sample was in 1924 accepted as a ‘fact of life’ and, throughout the 1930s, a substantial gap remained between the theory and widespread application of probability sampling methods (Hoinville, 1985).

The British tradition had a significant influence in the United States, as Booth “spoke to an aroused audience of activists” in “reform circles of philanthropy, settlement house work, and Progressive politics” (Converse, 2009, p.22). Jane Addams, for example, acknowledged that *Hull House Maps and Papers* was modelled after Booth’s study of London’s East End (Deegan, 1990, p.56). The prolific output of Hull House residents remained at arm’s length from academic circles, although

there was clear interplay between the University of Chicago faculty and the researchers at Hull House. While the former viewed settlements as a means to ‘colonize’ the city for social scientific experimentation, the women residents of Hull House resisted analysing populations as ‘specimens’. They viewed their vocation as that of citizens, not students, and took a “holistic view of women’s professional and personal lives” (Deegan, 1990, pp. 35, 41). Nevertheless, the academic credentials and statistical expertise of Hull House personnel - in particular, Florence Kelley, Edith Abbott, and Sophonisba Breckinridge – showed a stark contrast with the antistatistical tradition that lingered in Chicago’s sociology department until the late 1920s. Statistical work was central to the scholarship at Hull House through the mapping of social and demographic characteristics of the population in a defined geographic area. The empirical rigor of this extensive body of work is illustrated by the publication of over fifty articles written by Hull House residents in the *American Journal of Sociology* (AJS) in its first forty volumes. Nevertheless, the overriding goal of this pioneering quantitative research was democracy and education in the community, rather than the advancement of academic careers (Deegan 1990:47).

Two American ‘foundational studies’ deserve explicit mention due to their continuity with the British tradition and influence over American studies that followed: *The Philadelphia Negro* by William Du Bois (1899), and *The Pittsburgh Survey*, led and edited by Paul Kellogg (1910-1914). Du Bois stands apart from the mainstream of the American social survey movement in his focus on a particular sub-group and due to his stronger ties to academic sociology (Bulmer *et al.*, 1991; Gordon, 1973; Tonkiss, 2004). Rather than poverty per se, Du Bois viewed the ‘Negro problem’ as the core issue of race and class in American society, and he asserted that the black population of Philadelphia (and, by implication, elsewhere) needed to be considered in terms of different and specific social characteristics and forms of behaviour (Tonkiss, 2004). The Pittsburgh Survey can be seen as the most direct ancestor of the American social survey movement through the important influence of charity workers, the non-academic status of investigators (Kellogg’s background was journalistic) and its support through Russell Sage Foundation funding (Gordon, 1973). Although later this received little acknowledgement from the male-dominated sociological mainstream, Hull House residents worked on the Pittsburgh Survey and brought to bear their significant quantitative research expertise, honed by the earlier production of *Hull House Maps and Papers* (Deegan 1990).

The processes of industrialisation and urban growth came later in the United States than in Britain (Gordon, 1973). These were characterised by a dramatic escalation of immigration in the United States in the second half of the nineteenth century, which cultivated an interest in mapping the ethnic and racial geography of American cities which is lacking in early British studies (Gordon, 1973; Tonkiss, 2004). It is not altogether surprising that the British and American survey movements quickly diverged, as each was oriented towards differing social problems (Platt, 1991).

Also of significance is the differential policy impact of early surveys in Britain and the United States due to the greater distance between academic and policy circles and the strength of belief in individual virtue in the United States (Abrams, 1951; Bulmer *et al.*, 1991).

## 2.4 Adolescence: between Booth and the Blitz

During the interwar years, despite the opportunities opened up by the foundational studies, survey research in Britain seemed to have made a “false start” (Reid, 1987, p. 6), with little evidence of methodological advancement in the twenty years preceding World War II (Marsh, 1982). Empirical sociology “floundered,” as the followers of Booth “reverted to social accounting” in a way that perpetuated the divide between empirical research and social theorising (Kent, 1981, pp. 6, 74). Although a Chair in Sociology was established at the LSE, its incumbents, Leonard Hobhouse (1907-1929) and Morris Ginsbery (1930-1954) fostered a “philosophical and anti-empiricist tradition” that was reviewed by the Dean of Sociology at Chicago, Albion Small, as lacking any clear interest in sociology at all (Bulmer, 1985, p. 15). By 1943, Kent reports that one American observer described sociology in Britain as “all but dead” (1981, p. 6). There were two main strands to British survey work in the interwar years: the replicators of Booth, and the community self-surveys championed by Patrick Geddes.

Although the interwar years included more surveys than ever before, these were “rigorously factual” and lacking in reference to general ideas (Bulmer, 1985, p. 9). The idea that surveys should be ‘factual’ was not new, but in these years, fuelled by a “mood ... of institutional sobriety ... the range of the factual domain became severely curtailed” (Marsh, 1982, pp. 29-30). So limited were these enquiries that the Registrar-General of the Royal Statistical Society suggested that their main purpose was to fill gaps in knowledge between censuses, and that perhaps a quinquennial census would be more effective (Marsh, 1982). Among the replications of studies that were carried out, Bowley repeated his five towns study in 1923-4 and Sir Hubert Llewellyn Smith, by now working at the LSE, replicated Booth’s study of London with Bowley in 1930-35. Similar works were undertaken in Bristol, Birmingham, Southampton, and other urban areas.

The amateur status of the social survey was continued through the advocacy of community self-surveys by biologist-turned-sociologist, Patrick Geddes. He was responsible for founding the Sociological Society of London in 1903, which largely promoted his idea of ‘civics’. The community self-survey was mainly carried out by local volunteers and publicised in town halls. With the purpose of studying family and community in their “ecological setting” (Bulmer, 1985, p. 10), they resulted in a kind of “patchy anthropology” (Marsh, 1982, p. 31). The idea of applied social science

that Geddes fostered was “naïve and messianic” compared, for example, with that being developed in the field of psychology at the time (Bulmer, 1985, p. 11), and through its weak theoretical and methodological base it has been attributed with “a large burden of responsibility for convincing a generation of British sociologists that surveys were not for them” (Marsh, 1982, pp. 31-32).

It was in this period that the United States’ “intellectual leadership” (Bulmer *et al.*, 1991, p. 39) in survey research was established. This was aided by the spectre of the depression, which became a significant focus of research, while relief policies provided an unprecedented level of personnel for implementation (Platt, 1996). From around 1920, American university sociologists started carrying out empirical research and writing about methods (*ibid.*), which began to provide a platform for consolidation of methodological advances. The three strands of survey work I will describe here are the growth of opinion research, the American social survey movement, and the first steps of the “survey entrepreneurs” (Converse, 2009, p. 132).

Mark Abrams (1951) has described market research and public opinion surveys as ‘borrowers’ of the survey method. This distancing is not uncommon of an internalist history of social research, where survey researchers prefer to see their “ancestors in science” as being “more weighty” than those in politics and business (Converse, 2009, p. 4). However, notwithstanding the impact of Booth and other community studies, and of attitudinal surveying in psychology and sociology, these are the “most immediate ancestors of survey research” in the United States (Converse, 2009, p. 87).

Consumer market research began after World War I using carefully and consistently administered standardised questions to examine preferences (Rossi *et al.*, 1983, p. 3). During the same period, the link between democracy and opinion flourished in the United States, as the opinion of the collectivity was reconceived from an irrational force to the aggregate of individual opinions (Osborne and Rose, 1999). The real impetus came from George Gallup’s American Institute of Public Opinion, which he initially set up in his lunch hours in 1935 (*ibid.*). Political polls in the 1930s made use of quota sampling, in a partially successful attempt to represent the American population (Rossi *et al.*, 1983). Gallup’s “great coup” (Osborne and Rose, 1999, p. 377) came in 1936 when he correctly predicted the outcome of the Roosevelt-Landon presidential election based on a sample of 1500 individuals, while *Literary Digest* magazine predicted incorrectly based on a postal poll of millions of telephone subscribers. Gallup demonstrated convincingly that a small, carefully drawn sample could outperform an enormous sample drawn from an incomplete sampling frame with poor response (Rossi *et al.*, 1983). In 1937 *Public Opinion Quarterly* began publication in the United States, which Osborne and Rose date as the moment at which public opinion research became academically “respectable” (1997, p. 93). At the same time, the victory of the random sample laid the foundations for surveys to undertake manipulation and measurement rather than simply

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surveillance, and to become a crucial tool in the government of whole populations (Osborne and Rose, 1997; Osborne *et al.* 2008).

Perhaps the most prolific source of surveys in the first half of the twentieth century was the social survey movement in the United States. In 1912 the Russell Sage Foundation set up its Department of Surveys and Exhibits in order to elicit community participation in surveys with the goal of “changing the community’s consciousness ... to become sensitized to the problems of their community and aroused to undertake a program for their solution” (Converse, 2009, p. 25). The American community study quickly outgrew its British counterpart (Converse, 2009, p. 23), with 2,775 projects completed by 1927 in a craze of religious proportions (Gordon, 1973). Although these surveys brought together reformers with scientists and citizens, their fundamental aim was for “community betterment,” and as a result they were closer to social reform and political action than they were to academia (Crothers and Platt, 2010, p. 52). That is not to say that academic sociology was unaware of the movement: some viewed the survey as a means of transforming sociology from a ‘soft’ to a ‘hard’ discipline (Gordon, 1973). However, around 1930 the movement petered out through a distancing between sociology, which aspired to become a “detached and objective science”, and social work, which attached itself to psychoanalytic theory (Gordon, 1973, p. 295).

It was at a similar time that the first ‘survey entrepreneurs’ emerged - in particular, Paul Lazarsfeld, Samuel Stouffer, Hadley Cantril and Rensis Likert - making important links between academic culture and the applied research of business and government, which would later be consolidated. In the 1930s, Lazarsfeld and Stouffer began using sampling theory and statistical analysis within their sociological research and later in the decade Cantril established the Office of Public Opinion Research at Newark University, while Lazarsfeld set up the Office of Radio Research that would later move to Columbia University and become the Bureau of Applied Social Research (Rossi *et al.*, 1983). Within government, the Bureau of the Census began developing sampling methods with a view to providing timely measures of unemployment, while the first National Health Survey was conducted in 1935 using clustered sampling (*ibid.*).

By 1935, there was an enormous discrepancy in the number of surveys in the United States compared to Britain. This is partly due to increased government expenditure on surveys in the 1930s and 1940s, while a similar increase did not occur in Britain until the 1960s (Hoinville, 1985). According to A. F. Wells, who listed the studies in the two countries, this was due to the greater centralisation of government in Britain compared to the United States, which therefore did not require as many surveys, as well as the absence of an equivalent to the Russell Sage Foundation as a funding body in Britain (Bulmer *et al.*, 1991). Bulmer and colleagues add to this the greater

receptivity to such social knowledge in the United States, being aimed at local audiences and appealing to a “populist tradition” (Bulmer *et al.*, 1991, p. 39). It is clear that between Booth and the outbreak of World War II, the balance of social survey work both in terms of volume and quality swung heavily westwards.

## 2.5 Coming of age: World War II

The experience of total war brought advances in public opinion research into a policy setting (Osborne and Rose, 1999). In both Britain and the United States, World War II gave survey research its “real impetus” (Morton-Williams, 1993, p. 1); however this worked out slightly differently in each context.

In Britain, surveys of morale were important as the civilian population was under direct attack (Marsh, 1982). At the onset of war, a new Home Intelligence Division was set up within the Ministry of Information; however it came under public attack in the media as interviewers were accused of being ‘snoopers’ (Marsh, 1982; Moss, 1991; Whitehead, 1985). After a change in leadership and some restructuring in response to this crisis, the Wartime Social Survey emerged in 1941 under the leadership of Louis Moss and with a varied staff including market researchers, a former researcher on Rowntree’s second York study, and two people from Mass Observation (Moss, 1991).

By the end of 1944, Moss reports that over one hundred enquires had been completed, with samples ranging from five hundred to five thousand people. In terms of substantive focus, 28% of these related to the Board of Trade interests in consumer needs and shortages; 25% were focused on food and nutrition; a further 18% related to publicity and information activities (Moss, 1991). The intensity of governmental survey work was quite unprecedented in Britain. Further, there was a significant shift in public receptiveness to governmental social investigation: an article in *The Times* newspaper on 25<sup>th</sup> July 1945 asserted, “No single duty imposed on the new Government is more urgent or important than the collection and dissemination of the facts on which public policy must be based” (Moss, 1991, p. 13). Within government it was eventually agreed on that something akin to the Wartime Social Survey had a permanent role in efficient governance (Moss, 1991). The survey since Booth had been influential over governmental policy. From World War II onwards, the survey was brought *within* government, as a civil service occupation and a recognised input to the policy process.

In the United States, there was also a need for information on the ‘home front’ as involvement in the war escalated and required a considerable degree of buy-in from the American public

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(Converse, 2009). In response to this perceived need, the Federal Government called survey 'entrepreneurs' into governmental service: Cantril, for example, was invited to investigate public sentiment towards pro-Allied measures (Converse, 2009; Rossi *et al.*, 1983). Although data collection took varied forms, from old fashioned 'spying' to monitoring media sources, the survey was used on an extraordinary scale within the context of federal government for explicit policy purposes, which substantially increased its credibility (Rossi *et al.*, 1983). As Paul Lazarsfeld has observed, the demand for opinion and attitudinal research during the war offered social researchers "an unprecedented opportunity to contribute their skills and knowledge" (quoted in Converse, 2009, p. 186).

The most important product of wartime survey work in the United States was *Studies in Social Psychology in World War II* (1949-1950) which comprised reanalysis of the wartime survey work by Stouffer, Hovland, Guttman and Lazarsfeld. Stouffer stands out for his work heading the Army's Information and Education Branch, and is credited with the first two volumes, entitled *The American Soldier*. Beyond its substantive interest, this work is significant as it demonstrates how applied survey work can provide findings of "basic" scientific interest (Converse, 2009, p. 217).

In comparing the wartime survey work in the United States and Britain, it is noteworthy that a degree of distinction between more 'factual' investigation in Britain and attitudinal research in the United States continued. There was also more substantial involvement of academics in the United States, due to their larger and more flourishing sociological community (Marsh, 1982). This impacted on the post-war period in that while *The American Soldier* provided a significant "spur" to American sociology, in Britain wartime data was not only not offered to academics, but was covered by the Official Secrets Act (Marsh, 1982 p. 34). Nevertheless, in both contexts there emerged a new "government-sponsored research culture" (Stanley, 2008, p. 536) with the social survey as a leading tool of policy, whether conducted 'in house' or through renewed funding for academic social scientific work. Aside from the explicit outputs that were generated by World War II, Hitler's persecution of populations in Europe led to a flow of European academics to the United States. America's political influence in post-war Europe in turn assisted with the diffusion of the survey method back across the Atlantic "at the height of its novelty and vogue" (Platt, 1996, p. 2).

### 2.6 Maturity: After the war

Following the Second World War, there was a 'routinisation' of pre-war and wartime research activities as well as further growth in information needs in an increasingly complex society (Osborne



and Rose, 1999; Rossi *et al.*, 1983). Survey work in the past seventy years has been shaped by three trends: institutionalisation, professionalisation and economisation.

### 2.6.1 Institutionalisation

In the aftermath of World War II, the GI bill was established in the US to help veterans re-establish themselves in civilian society. This led to an expansion in universities as the availability of free tuition and stipends enabled the recruitment of a large cohort of graduate students and the creation of jobs for them to fill (Platt, 1996). American social scientists were “demobilised” back from Government service into academia, bringing with them the experience of applied survey work (Abrams, 1951, p. 122; Converse, 2009; Rossi *et al.*, 1983). This enabled them to establish ‘primary organisations’ bringing together social research practice and academic social science: The Bureau of Applied Social Research (‘the Bureau’), the National Opinion Research Centre (NORC) and the Survey Research Centre (SRC), “foreign bodies” which were “grafted” on to the Universities of Columbia, Chicago and Michigan, respectively (Converse, 2009, p. 239; Bulmer, 1985).

These organisations provided the institutional context for career academics with social research expertise to achieve a “triangular set of relations between academic social scientists, survey practitioners and policy makers” (McKennell *et al.*, 1987, p. 260), enabling the “*diffusion* of survey methods to the traditional disciplines” (Converse, 2009, p. 385, italics original). The NORC and SRC continue to dominate academic survey research in the United States today (Wright and Marsden, 2010). Survey research was taught as a ‘trade’ (Converse, 2009) and, combining the essential ingredients of an academic approach, practical experience and research opportunity, a firm platform for methodological research was established (Crothers and Platt, 2010; Hoinville, 1985). Federal government support for social survey work increased, particularly in the 1960s, both through funding to “basic” social science and instigating surveys to provide information relating to social programmes (Converse, 2009; Crothers and Platt, 2010; Rossi *et al.*, 1983).

Following the Second World War, survey research in Britain was revitalised compared to the preceding years, as a tool for social and economic development (Abrams, 1951). However, the legacy of the War, in Britain, was the Government Social Survey, which existed almost entirely separately to academic social science (Bulmer, 1985; McKennell *et al.*, 1987). Therefore, while governments in both Britain and the United States continued to fund large-scale, descriptive surveys, the engagement of social scientists facilitated greater incorporation of social scientific perspectives in the United States compared to Britain, where ‘in-house’ survey research remained rooted within the fact-finding tradition (Marsh, 1982; McKennell *et al.*, 1987).

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The Government Social Survey – later to become the Office of Population, Censuses and Surveys (OPCS) in 1970 and the Office for National Statistics (ONS) in 1996 - can be seen as the mode by which survey research was institutionalised in British government and policy-making. It evidences a “partnership between the state and the social researcher” which is still a hallmark of survey research in Britain (Marsh, 1982, p. 16). Key figures such as Mark Abrams, Henry Durant and Louis Moss were students at LSE in the 1930s, but had no other formal academic affiliations. There were no equivalents to the American survey entrepreneurs, who combined “sociological imagination with considerable statistical or mathematical competence” (Bulmer, 1985, p. 28). Nevertheless, the Government Social Survey did provide a platform in Britain for substantial methodological research, particularly through working papers, albeit with limited diffusion as a result of their restricted circulation (Hoinville, 1985).

Although as early as 1950 extra-academic social enquiry was reasonably healthy, academic sociology was slow to form connections with empirical social research (Bulmer, 1985). Bulmer attributes this substantially to the failure of ancient universities to accept sociology into the “academic Pantheon” (1985, p. 25). Philip Abrams, who achieved controversial approval for the first Social and Political Sciences (SPS) Tripos at Cambridge in 1969, emphasises the importance of an academic sociology separate to governmental or any other authority base, requiring “an institutional space of its own from which its special scientific task can be pursued ... the pursuit of argumentative knowledge, the refusal of authoritative knowledge” (quoted in Bulmer, 1985, p. x).

The acceptance of the social survey within sociology is significant as even in the 1980s there was continued reticence among sociologists to accept the social survey as a legitimate methodology. Whether this was caused by scorn for the amateur tradition from which survey research originated (Bulmer, 1985; Reid, 1987) or a rejection of the excessive positivism of survey research (Marsh, 1982; Reid 1987), it is clear that late into the twentieth century much of the best survey research in Britain was undertaken outside universities, either in government or through Social and Community Planning Research (SCPR – later to become NatCen), which was the only survey agency to resemble those of the United States (Hoinville, 1985).

### **2.6.2 Professionalisation**

During the fifty years following World War II survey research went through a shift from amateur to professional status (Morton-Williams, 1991), although it is arguable that this transition happened earlier and more quickly in the United States, perhaps in the 1930s (Burawoy, 2005; Platt, 2002; Platt, 2014). This process can be conceived from the viewpoint of three key actors: the investigator,

the interviewer and the respondent. Since the professional context of the investigator is within the institutions discussed above, I focus here on the interviewer and respondent.

The in-home interview with a sample of the general population has become the paradigmatic form for the social survey, such that it is difficult to imagine this was anything other than inevitable (Morton-Williams, 1991; Bulmer *et al.*, 1991). Until at least the 1970s it is reasonable to suggest that survey interviewers were a non-professional group, although research on the role of interviewers by NORC in the late 1940s and early 1950s, and in Britain by Belson in the 1960s (Morton-Williams, 1991). Nevertheless, the majority of survey interviewers were lacking in professional status (Rossi *et al.*, 1983) such that Morton-Williams can describe them as “the Cinderella of the survey industry ... undertrained, undersupervised, underpaid and undervalued” (1991, p. 3). This is despite the earlier identification by Bowley in his contribution to Llewellyn’s *New Survey of London Life and Labour* (1930-1935) that differential non-response between geographical areas can be associated with interviewer skill (Platt, 2014).

The professionalisation of interviewers advanced significantly in the 1970s under Jean Atkinson’s leadership of the Government Social Survey fieldwork department. She insisted on rigorous training and wrote the classic guide, *Handbook for Interviewers* (1971) (Marsh, 1982). In the Survey Methods Centre at SCPR in the 1980s, Jean Morton-Williams undertook systematic investigation of the interchanges that take place to negotiate access to respondents’ homes (Morton-Williams, 1991). The main thrust of ‘professionalisation’ has been rigorous training and the introduction of computer technology with a view to improving consistency.

There has been some controversy about the role of the interviewer in survey research: some emphasise standardising the interview to give a uniform stimulus to respondents, while others encourage a more active role for the interviewer in interpreting the research intentions (Platt, 2002). Evidence suggests that ‘conversational interviewing’ improves response accuracy for certain kinds of questions; however, there is a trade-off with survey costs, as this approach increases interview duration (Conrad and Schober, 1999; Schober *et al.*, 1999; Couper, 2011). There has been a decline in the role of “interviewer-as-observer” (Converse, 2009, p. 409), and there is now a greater segregation between the investigator and the interviewer, through the intermediary field management role (Morton-Williams, 1991), in stark contrast to the team approach of Booth. The interviewer role has certainly shifted into the purview of specialists; however, interviewers have been transformed into technicians. Training and technology serve to control the interviewer’s input into formalised practices, such that the individual characteristics and interpersonal skills of the interviewer are contained or excluded, in case they introduce error into the data. This has heightened the barrier between data collection and data analysis that prevents the interviewer

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from having substantive input and distances the investigator from the object of their research. Although in some senses the role of the interviewer is 'professional', in others it is very much diminished.

It is also meaningful to talk of the 'professionalisation' of the survey respondent, at least in metaphorical terms. Not only are the public trusted to speak for themselves, but the autonomy of respondents is acknowledged, particularly due to their capacity to refuse. With the 'routinisation' of social research (Savage, 2010), or 'researchification' (Crothers and Platt, 2010), of late modern society, response rates have fallen severely. However, 'researchified' research respondents are, without formal training, well-versed in the protocols of survey participation. The survey interview is a well-understood social form that does not need a great deal of explanation (Savage, 2010). It is a routine and formulaic interaction that involves a transfer of information about the respondent to the interview schedule with as little interference from either respondent or interviewer as possible.

### 2.6.3 Economisation

From the 1970s survey costs began to soar. A diminishing pool of people willing to take on skilled yet low-paid part-time work and the professionalisation of interviewers meant a rise in wages. At the same time, fewer people were found to be available at home during daylight hours, so the number of attempts required rose (Rossi *et al.*, 1983). Combined with increasing participant reluctance in a society saturated with social research (Crothers and Platt, 2010; Osborne and Rose, 2008; Savage, 2010; Savage and Burrows, 2007), response rates plummeted (Tourangeau, 2007).

Many methodological innovations in the last forty years have been driven by the need to reduce, or at least stabilise, relentlessly rising costs, while maintaining response rates (Wright and Marsden, 2010; Converse, 2009). Interest was renewed in the potential of postal survey methods, although this was quickly eclipsed by the move to the web (which I return to in the next section). Technological advancements made telephone interviewing a legitimate option, particularly since the advent of Random Digit Dialling (RDD), although mobile technologies are eroding a landline-based sampling frame. Computer-assisted interviewing improves both data quality and the time and costs associated with data cleaning (Rossi *et al.*, 1983; Converse, 2009).

As an industry, survey research was already estimated in 1983 to be worth in the region of \$2.5 - \$5 billion (Rossi *et al.*, 1983). In the following twenty-five years, this industry grew in size, value and complexity: the number of academic/not-for-profit survey organisations in the United States almost doubled (Converse, 2009), in addition to the existence of substantial private research firms and in excess of 7000 market research firms (Wright and Marsden, 2010).

The economic dynamics of survey research are not of purely practical concern: there is an “enduring tension” between social scientists, wishing to seek “Truth”, and the people who pay for the work (Converse, 2009, p. 411). Survey research continues to be reliant on insecure subsidies from foundations or governments to make ‘basic’ science possible, as applied work is more marketable. While in the lead up to the Second World War charitable foundations were a major source of funding in the United States, in the post-war era corporate financing became increasingly mainstream (Burawoy, 2009). In the UK the future state of funding for social science seems to be perpetually precarious (Gane, 2012), while there has been a shift in the nature of the university which is becoming more corporate rather than cultural (Readings, 1996, in Gane, 2012). This leads to a conflict between what Mills calls the “economics of truth” (that is, the costs of research) and the “politics of truth” – that is, the use of research to clarify significant issues (Mills, 1959, p. 75, in Gane, 2012, p. 154). The drive to economise permeates not only methodological directions of travel, but also the substantive range of options accessible to survey research.

#### **2.6.4 The end of history?**

I have observed that there was a flurry of explicit historical reflection on the social survey method in the 1980s and early 1990s, and before turning to the contemporary prospects for survey research it is useful to consider what sparked this historical turn.

It is arguable that by the early 1980s the history of the survey method had reached a conclusion: an ‘end’ precipitated by its institutionalisation as a full part of “the academic machine” (Burrows and Savage, 2014, p. 2), its strong connections to social policy, or in a methodological sense that defining innovations had by then taken place and it entered a new phase of ‘routine maintenance’ (Wiles *et al.*, 2010). This directional narrative bears a closer relationship to the situation in the United States than in Britain. Alternatively, it could be suggested that the history arose in the early 1980s in reaction to a crisis point in continued or renewed resistance to the social survey method within academic sociology (Bulmer, 1985; Marsh, 1982; Reid, 1987), particularly in Britain. This was fuelled externally by politically-inspired attacks to sociology (Crompton, 2008) and internally by the ‘paradigm wars’: indeed, Burawoy (2005) has observed the powerful narratives of decline that circulated in this period, albeit the reality was short-lived. However, I suggest that it is more productive to frame the history in terms of transitions rather than endings or crises.

In both the United States and Britain there is evidence of a push towards more theoretically-informed survey research. In Britain, this is discussed in terms of a coming together of social theory and empirical social science in order to go beyond ‘fact-finding’ in pursuit of ‘argumentative knowledge’ (Bulmer, 1985; Kent, 1981; Marsh, 1982), while in the United States there is a concern

around the continued necessity of ‘basic’ social science that underpins the more fundable applied work (Converse, 2009; Rossi *et al.*, 1983). The survey historians of the 1980s suggest that a transition was required. Greater ‘accuracy’ could no longer be the only concern – even if it was achieved by professional practitioners in the context of facilitative institutional infrastructure and at a competitive cost. Social survey research also needed to aspire to engage at a deeper *theoretical* level if it was to continue to function as a primary means of explanation in complex societies. Indeed, survey research continues to attract ‘entrepreneurs’ who are willing to push the boundaries of what surveys are equipped to measure

### 2.7 Survey research in the 21st century

Reflecting on survey research in the early 21st century and into the present, there are both opportunities and challenges. Survey research continues to attract ‘entrepreneurs’, who are pushing the boundaries of what phenomena can be measured: surveys are employed to gauge levels of happiness, or trust, for example (Layard, 2011; Gillespie, 2012; Helliwell *et al.*, 2017). Nevertheless, challenges continue. These arise from a combination of the changing social environment which survey practitioners investigate and operate in, and shifts in the wider data landscape. The survey method will need to continue to adapt in response to its fluid context (Couper, 2013; Elliot *et al.*, 2013; Groves, 2011). For this to happen, survey investigators will need to overcome pathologies of the processes of institutionalisation, professionalization and economisation that characterised the method’s late 20th century trajectory, which tend to reward repetition of the same and hamper innovation (Gane, 2012).

If, as recent history suggests, the value of survey research (and social science more broadly) arises through mobilising both concepts and methods in response to empirical problematisations (Gane, 2012; Osborne and Rose, 1997), survey research must guard against both non-theoretical ‘fact-finding’ and disengagement with the pressing social problems of its day. These continue to be significant challenges for an institutionalised discipline which is highly internally specialised (Groves and Lyberg 2010; Crompton 2008; Payne 2007). The use of regular surveys to monitor social trends is routine and widespread: international comparative initiatives such as monitoring the Sustainable Development Goals require standardisation; however, this can be at the expense of innovation, and in the interests of comparability can instil a reluctance to question the suitability of measures. Professionalisation, as we have seen, creates a significant distance between the investigator and the phenomena they seek to understand, and in this sense can involve a “retreat” from public engagement (Jacoby, 1987, in Burawoy, 2005, p. 15). Nicholas Gane has observed the enduring

relevance of C. Wright Mills' critique of 'abstracted empiricism', where an "a priori methodological commitment" leads to data and methods determining what is investigated (Gane, 2012, p. 154). In a recent consultation a survey respondent observed, "there is a growth of under-theorised empiricism in social science ... uncritical use of data with limitations in coverage or definitions and the steering of research to things that happened to be measured" (Elliot et al., 2013, p. 17). This is exacerbated where the 'politics of funding' serve to silence critical voices (McKie and Ryan, 2012) – as this history has shown, by no means a new experience.

If institutional constraints encourage an inward-looking mode of survey research, there are competing explanatory voices prepared to account for the social world. Historically, as we have seen, academic credentials have only ever been one possible source of authority to speak in a social voice (Osborne and Rose, 1997). 'Commercial sociology' (Burrows and Gane, 2006) is increasingly dominant, as 'knowing capitalism' (Thrift, 2005) seeks "to know itself in ever more precise ways in order to better extract value" (Burrows and Savage, 2014, p. 2). Although, as Liz Stanley (2008) eloquently argues, capitalism has "always known" and academic social science has always been "other" to its knowledge producing mechanisms, nevertheless there has been a marked change in the scale of commercial sociology (Purdam et al., 2004) and our complicity in the process. Not only do 'we know that they know,' we also know that commercial stakeholders use the information that we ourselves provide in order to manipulate our behaviour. This gives rise to the need for a shift in consciousness from data subjects to data citizens, aware of and able to exercise agency over our own data (Elliot, n.d.).

Survey research that follows Philip Abram's call to generate "argumentative knowledge" equipped to refute "authoritative knowledge" provides a much-needed balancing of the power of large-scale data controllers in this space (Elliot et al., 2013, p. 21; Burawoy, 2005). Producing the research, however, is no longer sufficient. In 2016, the Oxford English Dictionary selected 'post-truth' as the word of the year, which denotes where "facts are less influential in shaping public opinion than appeals to emotion and personal belief" (OED, n.d.). Today's outward-looking survey researchers need to involve themselves directly in public debate, capitalising on the opportunities presented by niche and micro-media in our multi-mediated social world (McRobbie and Thornton, 1995).

Meeting this need rides on the availability of properly motivated and skilled personnel. In the UK in particular, there is a well-recognised deficit in quantitative and statistical skills in the social sciences, which threatens the UK's historic position of influence in survey research (British Academy, 2012). Among undergraduate sociologists, for example, there is widespread anxiety about the use of statistics, which is correlated with poorer performance; students lack enthusiasm for quantification, perhaps influenced by poor exposure to techniques that enable complex

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explanation (Williams et al., 2008). This filters to the discipline more widely, where professed methodological pluralism does not hold in reality (Payne et al., 2004). The Q-Step Quantitative Methods Programme<sup>7</sup> has been initiated to facilitate a step change in the quantitative capacity of UK social science, and this will need to be supported by further strategic interventions to provide attractive post-16 options in mathematics and to raise numerical attainment at secondary level (Hodgen et al., 2013; Nuffield Foundation, n.d.).

If economisation drove advances in survey methodology in the late twentieth century, it has also jeopardised the hegemony of the sample survey as we know it. There has been a rise in opt-in online surveys, including open invitation self-selected surveys and volunteer panels of web users (Couper, 2000). With lower costs and fast turn-around times, these web-based modes have in most cases replaced traditional face-to-face or telephone approaches. Although from an academic perspective bias resulting from major undercoverage, high nonresponse and self-selection makes population estimates derived from such non-probability approaches worryingly unreliable, online panel research in the US was estimated in 2009 to be a \$2bn industry (Baker et al., 2010). Approaches to achieve the cost benefits of online modes whilst maintaining survey quality have also emerged, for example through offering participants web-based options for repeated waves of longitudinal surveys, and developing probability-based web panels (Nicolaas et al., 2014; RCUK, n.d.).

Concurrently, social scientific interest in and use of big data has seen rapid expansion (Burrows and Savage, 2014), and the accessing, handling and analysis of digital data has been identified as an important emerging training need (Durrant et al, 2015). 'Big' or 'organic' data includes digital information generated by systems as a byproduct of a population's engagement with commercial bodies (transactional data), governmental institutions (administrative data) or each other (social media data) (Couper, 2013; Groves, 2011). Some commentators suggested that the advent of big data would eclipse the sample survey (Savage and Burrows, 2007). It is unproductive, however, to project a false dichotomy between 'traditional' and 'new' data sources and analytic methods, and those who use them – not least since we have observed the use of administrative data for research purposes in the nineteenth century, and Booth combined survey, administrative and qualitative observational data in his studies. Of far greater promise is the approach taken by Mark Elliot and colleagues (2013), who have developed a typology of data based on the way in which it is generated. This is not only more informative than generalisations such as 'traditional' and 'big'; it

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<sup>7</sup> Jointly funded the Nuffield Foundation, the Economic and Social Research Council (ESRC), and the Higher Education Funding Council for England (HEFCE), this programme has established 15 specialist centres for quantitative methods.



also complements their proposal that in this “age of data” there is a shift in thinking from ‘data sets’ to ‘data arrays’ which presents opportunities for researchers to combine different types of data in a way that bypasses traditional divides (Elliot et al., 2013, pp. 9,17).

In evaluating data sources, it is instructive to recall that the survey method emerged in direct response to the challenge of understanding the dimensions of poverty and directing social policy to its amelioration. We need to take very seriously the risks of once again – administratively and digitally – hiding the “have nots” of our societies (Couper, 2013, p. 147). It is those populations for whom data is not available organically or voluntarily that are the greatest priority for many academic social scientists, even if they are of marginal relevance for some commercial interests.

## 2.8 Conclusion

For empirical researchers who aspire to self-reflexive work, it is necessary to account for the biography of one’s stand point as well as current social positioning. Just as historical approaches are capable of enlightening what is ‘thinkable’ or ‘knowable’ in theoretical terms at different points in time, methodological history can enlighten how different kinds of evidence come to be considered valid or invalid in a way that enables critical reflection on contemporary practice. This is the kind of historical work that has something to offer to the present: that allows us to see where present experience is not as new or different as it appears (Osborne and Rose 1997; Stanley 2008), and fashions the past into a kind of “laboratory” that enables the exploration of “various kinds of creativity and inventiveness” (Osborne *et al.*, 2008, p. 519).

Despite being limited by its broad scope and reliance on secondary literature, this study begins to illustrate the productivity of methodological history for enlightening current practice and pointing to future directions for methodological exploration. It protects us from assuming that methods which have been attempted in the past can be dismissed as ‘outdated’ without consideration of the idiosyncrasies of the social-cultural context and purpose to which they were applied. It also enables us to make a more mature evaluation of what constitutes ‘progress’ methodologically. I have pointed to the need for a revival in methodological history as a lasting aspect of social scientific research, and there are a number of immediate avenues that this paper identifies. In particular, the question remains as to whether the need has been met for a social science that continually harnesses social theory alongside any and every empirical method that can shed light on the pressing social issues we face. This is key to the explanatory value of social research, and unifies the surge of survey history on which this review draws.

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## **Chapter 3: Sketching trust stories: A practical examination of how a multimodal method can inform the conceptualisation and operationalisation of a process approach to trust**

### **Abstract**

Creative, visual and multi-modal research methods are commonly used in data collection, presentation, and dissemination of research findings; however, they are rarely applied to the research practices of conceptualisation and research design. Responding to Pierre Bourdieu's call for the construction of the object to be rigorously undertaken in every moment of research (Bourdieu & Wacquant 1992), I explore how such methods can be employed by the researcher to understand, communicate, build on and operationalise an abstract concept - trust - as a precursor to empirical investigation.

Trust is a phenomenon which is commonly invoked both in everyday life and in academic discourse; however, its familiarity obscures the lack of conceptual clarity surrounding the term, which has deep implications for how it can be approached methodologically. In light of dominant constructions of trust as an attitude or a behaviour, I argue that trust is better conceived of as a process (Mollering 2001, 2006, 2013; Dietz 2011). I exemplify and advance this approach using a creative research method - storyboarding. Based on three imagined trust dilemmas developed with the involvement of a visual artist, I demonstrate how a visual creative process can safeguard the interconnection between theory and method through encouraging consistent attention to the construction of the research object. It also speculatively reveals new facets of phenomena under investigation, and supports reflexive attention to the researcher's relation to the object of research, thus enhancing reflexivity and rigor in empirical research.

### **3.1 Introduction**

Many of the most compelling and significant aspects of social life that social researchers seek to account for lack obvious or agreed-upon definitions, and belie straightforward measurement. The

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tried-and-tested tools of academic thought provide means of articulating conceptual arguments and methodological propositions; however, many of the most intriguing of social experiences remain mysterious. Trust is one such phenomenon. It is immaterial, relational, taking place between rather than within social agents, and is therefore not accessible to straightforward observation. It has a subtle but decisive influence on action, but cannot be conflated with the action that it produces. It is much exercised, both in everyday life and as an explanatory concept in social science, but little explained. These are some of the challenges that trust researchers have to grapple with, which will be familiar to others outside this field who attempt to account for abstract and illusive concepts.

In response to this challenge, I explore the use of an expressive multi-modal method – storyboarding – by the researcher, as a heuristic device in the interrelated processes of conceptualisation and research design. I apply this method as a measure to reflexively uncover and examine assumptions and conceptual pre-constructions that may otherwise creep into trust research. Although it is uncommon for visual methods to be taken up in pursuit of objects that have no visible or material substance, this is not without precedent (Grady, 2006; Pauwels, 2015). Visual approaches have the capacity to reveal “subtle but significant” dimensions of social life that may have been difficult to represent or gone unnoticed using more routine approaches (Eisner, 2008, p. 11). I argue that the incorporation of creative methods such as storyboarding has the potential to increase the rigour of conceptualisation and operationalisation in research through evoking an attitude of “relentless self-questioning” (Wacquant, 2008 p. 266) that Bourdieu describes as methodological reflexivity.

Trust provides a good test-case for this experiment: those in the vanguard of trust research acknowledge openly that, despite decades of investigation, there lacks a generally agreed-upon definition of trust (Gambetta, 1988; Brown and Calnan, 2012; Li, 2012; Lyon *et al.*, 2012; Möllering, 2014; Li, 2015). Nevertheless, empirical interest in trust continues to grow, as the conditions of late-modern society make trust increasingly vital, yet more difficult to achieve (Lewis and Weigert, 2012). Outside the field of trust research, social scientists frequently posit trust as relevant, either in passing or as an explanatory factor, without sufficient attention to the ambiguity of the concept itself (Gambetta, 1988; Furlong, 1996; within the field of peace and conflict studies, for example, see Barakat, 2010; Justino, 2012; Rohner *et al.*, 2012). Within trust research, two competing accounts of trust dominate: trust conceived as an attitude, and trust as a form of cooperative behaviour (Li, 2012). As a result, issues of construct validity in the operationalisation of trust loom large, which hampers the quality, comparability and cumulative contribution of studies (Gillespie, 2012).



In Pierre Bourdieu's methodological discussion, he warned against inconsistent attention to the construction of the sociological object in the process of research, which is responsible for lapses to pre-constructions – dominant conceptions which already circulate – resulting in breaches between that which research purports to illuminate and that which it succeeds in showing (Bourdieu and Wacquant, 1992; Silva and Edwards, 2004). After an initial overview of the field of trust research, I begin by applying this notion to critically examine existing dominant conceptualisations of trust – as attitude, and as behaviour - and their associated methodologies. Based on this critique, I argue for a process approach to trust, as outlined by Guido Möllering (2001, 2006), who theorises trust as a process of interpretation, suspension, and favourable expectations. Nevertheless, proponents of process approaches to trust admit the difficulties in designing a research methodology that is consistent with this perspective (Möllering, 2013a). This makes it all too easy to slip into research practices that assume one of the more dominant conceptual constructions. To facilitate the operationalisation of trust, I then turn to the application of storyboarding. I first outline my methodology, including engagement in both philosophical and practical concerns, such as the status of the resultant visualisations (are they data?) and the role of artistic skill. Next, I present and discuss three diverse storyboarded scenarios, assessing the contribution their production has made to my operationalisation of trust, before considering how this process may enhance the reflexivity with which researchers approach their research objects.

Although my focus is primarily methodological – on the application of a multi-modal method as a heuristic device to enhance the conceptual rigour of research practice – I examine theoretical approaches to trust in some detail. This will be of interest to trust researchers, both for the Bourdieusian arguments in support of a process approach and for the avenues for future research that the process of storyboarding yields. For other readers who investigate similarly perplexing concepts, or those interested in the methodology alone, the in-depth treatment of the case remains important in order to reveal how the practical application of storyboarding has exposed and remedied potential conceptual slippages. For Bourdieu, theory and methodology form two inseparable actions of research that are held in continual conversation with each other, and this is the approach that I take here.

### **3.2 Trust research: A brief overview**

Trust is a fundamentally social phenomenon. Although psychologists have observed some interesting correlations between the tendency to trust and other facets of personality (John *et al.*, 2008; Kausel and Slaughter, 2011; Oskarsson *et al.*, 2012), trust itself is by definition relational: it is

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not the property of individuals, but of collective units (Lewis and Weigert, 1985; Frederiksen, 2014; Li, 2014; Möllering, 2014). Scholarship has tended to focus on one side of the relationship – the trustor – which has encouraged a neglect of the relational dynamics of trust (Mitchell, 2001). It comes into play where there is interdependency between social agents, and although this may be seen as a condition of the notion of society, this interdependency is heightened in certain circumstances, where trust is most relevant (Lewis and Weigert, 1985; Li, 2012). Trust occurs in situations of vulnerability, and has been defined by some as “willingness to be vulnerable” (Mayer *et al.*, 1995; Rousseau *et al.*, 1998), as it involves a positive acceptance of vulnerability to the control of another (Mitchell, 2001; Möllering, 2006). The context of trust is also one of either risk or uncertainty, a distinction that I will return to shortly.

There has been some debate as to whether trust operates differently dependent on contextual conditions, or whether it has a universal dynamic, which affects the scope of this exercise (Bachmann, 2011; Dietz, 2011). It is sufficient here to accept the possibility of a general theoretical construct, albeit this is likely to have dramatically different presentations dependent on context. Of particular relevance, it is possible to differentiate between different levels of trust dependent on the kinds of social agent involved – individuals, groups, or institutions (Bourdieu and Wacquant, 1992). Generalised, or ‘basic’ trust (Giddens, 1990) thus constitutes ‘trust in most people’ or ‘trust in strangers’ (Stolle, 2002), purportedly based on either the moral conviction of shared underlying norms or values (Uslaner, 2002), or institutionally-created societal conditions where others are viewed as equal citizens (Stolle, 2002; Rothstein and Stolle, 2008). Group-based trust is thought to reflect an enhanced perception of similarity resulting from shared membership of a salient social category (Tajfel *et al.*, 1971; Tajfel and Turner, 1979; Brewer, 1981), whilst the object of personalised trust is a known other. An adequate model of trust needs to encompass a broad range of relational types, “from basic interdependence to absolute bonds and from loose acquaintance to total identification” (Möllering, 2014, p. 17). Although my focus here is on personalised trust, other levels are implicated, and these should be seen on a continuum of lesser to greater *familiarity* between agents, rather than as discrete categories – indeed, although the division between social trust and institutional trust is rather sharply drawn in the literature, Peter Li has suggested that the distinction between relationship-based (in-group) and relationship-free (out-group) forms may be more significant (Frederiksen, 2014; Möllering, 2014; Li, 2015).

The characterisation of trust as attitude or behaviour stems from the siting of early trust research in psychology and political science, where it was principally conceptualised as “a psychological event within the individual,” while methodologically, in keeping with its disciplinary origins, trust was reduced to its “cognitive content” through the dominant use of psychometric scaling or laboratory behavioural experimentation methods (Lewis and Weigert, 1985, p. 967). More

recently, Li (2015) evaluated that in contrast to the pre-paradigmatic conceptual diversity of the field of trust research, methodologically there is surprising conformity, with a strong bias towards positivist epistemologies and quantitative methodology. In a recent study of the epistemologies of leading trust researchers, Isaeva and colleagues (2015) also demonstrate a strong explicit or implicit dominance of positivism and, above all, that not all prominent trust researchers actively engage with epistemological assumptions in their own or others' work. Some seemed unfamiliar with the term, while others considered it to be either unimportant or irreversibly inculcated through academic training, and therefore not an area for choice.

### 3.3 Constructing trust: attitude, behaviour, or process?

Both the privileging of certain methods and failure to reflexively engage in questions of knowledge production demonstrate a failing in "the relentless self-questioning of the method itself in the very movement whereby it is implemented" (Wacquant, 2008, p. 266), and are excellent conditions for preconstructed sociological objects to thrive (Bourdieu and Wacquant, 1992). For Bourdieu, the construction of the object is a most crucial but frequently overlooked aspect of research: a "protracted and exacting task that is accomplished little by little" through the continuous engagement of theory and practice in every act of research, however inconsequential (Bourdieu and Wacquant, 1992, p. 227-8; Wacquant, 2008). It involves maintaining the tension between the constructed, "purified objects" of research and the concrete to which they refer (Wacquant, 2008, p. 265), and avoidance of substantialist thinking, which collapses the object of research, rightly positioned within the network of relations that ascribe its distinctive properties, with an actual population that embodies the phenomenon of interest (Bourdieu and Wacquant, 1992). In trust research, failure to adequately construct the object of research is betrayed by assuming that attitudinal or behavioural measures directly access and fully encompass trust, neglect of its relational character, and the equation of trust with cognitive processes of a trustor.

Both attitudinal and behavioural approaches to trust have in common a rational form of strategic thought and/or action, a reflection of the encroachment of rational choice thinking on the social sciences beyond economics to which trust research is not immune (Bourdieu and Wacquant, 1992; Williamson, 1993; Mitchell, 2001; Frederiksen, 2012). An early and influential example of this is found in Oliver Williamson's (1993) argument for calculativeness as a *more adequate* account of (economic) behaviour than trust. Based on transaction cost economics' principles of bounded rationality, which states that economic agents are "*intendedly* rational, but only *limitedly* so" (Simon, 1957, p. xxiv; cited in Williamson, 1993, p. 458), and opportunism, that is "self-interest

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seeking with guile” (Williamson, 1993, p. 458), Williamson asserts that “diffuse terms” such as trust are better avoided (Williamson, 1993, p. 469). Instead he argues that scenarios in which trust is conceptually mobilised can be better understood as boundedly rational, calculative risk-taking against the backdrop of calculable safeguards (Möllering, 2014). For Williamson, “if [trust] obtains at all,” it is only in the most intimate of personal relationships, which take a “nearly noncalculative” form through the deliberate (calculative) suppression of calculativeness (Williamson, 1993, p. 479; Möllering, 2014). Although trust scholars have argued against Williamson’s dismissal of trust, influential constructions of trust, whether conceived of as attitude or behaviour, have been infused with self-interested rationalism. I will briefly discuss each before turning to the most promising alternative to these preconstructions: trust as a process.

Cognitive conceptions of trust as a belief or attitude are probably the leading preconstruction at present. In particular, the ‘perceived trustworthiness paradigm,’ which operates by “probabilistic trustworthiness estimates” made through rational information processing in response to risk, has become particularly dominant (Möllering, 2013b; 2014, p. 11). Although there are various iterations of trust as an attitude (Coleman, 1990; Mayer *et al.*, 1995; Misztal, 1996), Russell Hardin has become prominent, not least due to his influence in the Russell Sage Foundation series on trust. For Hardin, trust applies exclusively in relations of mutual familiarity and acquaintance, where “A trusts B to do, or with respect to, X” (Ullmann-Margalit, 2004; Hardin, 2006, p. 19), due to the necessity of sufficient knowledge. The assessment of trustworthiness, which constitutes trust, takes the form of ‘encapsulated interest’ – where the trustor perceives that “the trusted encapsulates the interest of the trustor and therefore has incentive to be trustworthy in fulfilling the trustor’s trust” (Hardin, 2002, p. 24; 2004). The perception of trustworthiness is thus based on the calculation that the other party, acting in their own self-interest, will act in a way that aligns with one’s own interests. For Hardin, distrust sits in an asymmetrical relation to trust: it is impossible to trust a group because the interests of the group are not the sum of the interests of the individuals it comprises; however, a group may be distrusted if it is clear that their (individual and collective) interests do *not* encapsulate the trustor’s interests, either through incompetence or negative motivations (Hardin, 2004, 2013). In other words, less knowledge is needed to distrust than to trust.

Other, less narrow cognitive approaches leave space for affective or moral bases of trust as an addition or alternative to purely instrumental versions (Li, 2015). For example, Mayer, Davis and Schoorman’s (Mayer *et al.*, 1995) classic model incorporates perceived ability, benevolence and integrity; for Almond and Verba (1965; cited in Hardin, 2013, p. 33), belief in the benignity of other citizens is key; while for Paul Stoneman (2008) anticipated action is not only in the interests of the trustor, but is taken *because* it is in their interests. Nevertheless, where trust constitutes a

conscious, rational appraisal of the motivation and qualities of another, methodologically this “psychological state” can be accessed by direct questioning (Rousseau *et al.*, 1998).

At a macro level, generalised trust questions are included in many cross-national attitudinal surveys, which form a popular data source for studies outside the trust field per se, as well as having defenders within (Uslaner, 2011, 2012). The ‘standard’ two-part question as posed in the World Values Survey and Afrobarometer\* asks, “Generally speaking, would you say that most people can be trusted, or that you can’t be too careful (\* or, ‘need to be very careful’) in dealing with people.” This measure is itself not without problems. The accuracy of this question in distinguishing between levels of trust has been called into question, suggesting a gap between the researcher’s operational definition of generalised trust and respondents’ interpretation. Sturgis and Smith (2010) found a significant level of variation within the same national context and survey in relation to who respondents think of when they answer, and this ‘radius of trust’ problem is amplified in cross-national studies that use this measure (Delhey *et al.*, 2011). Some reject the supposition that trust can be captured dichotomously, and some surveys (such as the European Social Survey) have moved to a broader scale, although these too are criticised for ‘clumping’ of responses around the mean (Uslaner, 2009, 2012). Neither formulation is particularly agile in *explaining* the descriptive patterns they find (Möllering, 2013a, p. 285). More detailed, multi-item psychometric instruments are used to measure individual-level trust, particularly in organisational contexts; however, few trust scales are well-validated and in many cases there is a gap between the conceptualisation and measurement of trust (Gillespie, 2012). More fundamentally, although standardised, closed-ended direct questioning boasts the attractions of convenience and comparability, its underlying conceptual assumptions that trust can be measured as an attitude are weak.

Conceptually, constructions of trust as an attitude tend to conflate trust with its antecedents, and the more calculative versions ignore the moral dimension of trust, which is better termed as a *commitment* than a calculation (Cohen, 2014; Möllering, 2014). They assume a directional, uneven kind of relationship (Ullmann-Margalit, 2004) by excessively focusing on the trustor, failing to acknowledge the other part that is necessary to trust – being trusted, which involves being held accountable to our own feelings, other’s responses, and in some cases the law, in a way that “demands that we behave at a level that reflects that gift” (Mitchell, 2001, p. 110). Similarly, in response to Hardin’s comments on trust in government, Möllering asserts, “citizens are not passive perceivers of government action but active partners in the trust relationship with government,” a relationship which requires work on both sides (2013b, p. 57). When the perspective of the trusted person enters the frame, self-interest becomes insufficient; rather, Mitchell draws on moral psychology to assert that our common humanity and sense of shared vulnerability leads us to empathise with the vulnerability of someone who trusts us and experience “a sense of

responsibility ... commitment and obligation, that leads us to fulfil their expectations” (2001, p. 122).

Crucially, there is a logical disjuncture between the psychological state that is reached through an assessment of perceived trustworthiness, and any behavioural decision which is necessary for vulnerability to be *consequential* (Li, 2015). Assessment of the character of others and expectation of them do not necessarily coincide (Tanis and Postmes, 2005); ‘good reasons’ do not inevitably produce trust, and anticipatory belief is insufficient to constitute trust (Sztompka, 1999; Möllering, 2001). For some, the solution to this is to construct trust as the behavioural decision to accept vulnerability: commitment through action (Sztompka, 1999; Li, 2007, 2012).

In order to capture ‘trusting behaviour’ empirically, trust games have been employed to create an artificial scenario where different behaviours can be evoked and observed (see, for example, Deutsch, 1958; Schweitzer *et al.*, 2006). Game theory is suited to the analysis of how perfectly rational individuals behave in circumstances of interactive choice, and therefore measures calculative forms of action where there is a choice between opportunism and cooperation (Furlong, 1996). It overcomes the ‘one-sidedness’ of attitudinal accounts as cooperation on both sides of the transaction is captured (*ibid.*). Although these games take various forms, some of the most intriguing work has experimentally manipulated knowledge of the identity of others, for example through pseudonyms which make an ethnic identification or visual clues that give away a relevant group affiliation, and have enabled some valuable observations regarding conditions of cooperative behaviour between identified groups (Tanis and Postmes, 2005; Habyarimana *et al.*, 2009). As a measure, however, trust games assume that the competitive, individualised, self-interested objectives of a game and consciously strategic mode of action is analogous to the mode of action in everyday life. As Bourdieu has observed, social action takes a variety of forms, and is not always “the deliberate pursuit of a conscious intention” (Bourdieu and Wacquant, 1992, p. 121).

More generally, should behaviour be captured in a way that could account for more diverse modes of action, such as participant observation, the behaviour alone would still be insufficient to account for trust. Behavioural approaches struggle to account for the ‘agnostic’ position of neither trust nor distrust which may arise passively through lack of entering into the process of trust or distrust, or actively where information is lacking (Ullmann-Margalit, 2004; Stoneman, 2008). For example, where there is insufficient information to warrant trust, cooperative behaviour may be undertaken as a means to generate the requisite information for future trusting (Hardin, 1993; Lewis and Weigert, 2012). This connects to a deeper problem, in that observations of behaviour alone assume that certain kinds of behaviour denote trust and others denote distrust. This is a serious logical leap, as cooperative behaviour may be based on many other factors than trust (Rousseau *et al.*,

1998; Möllering, 2001): for example, in the presence of sufficient institutional safeguards to mitigate vulnerability (Cook *et al.*, 2005), where there is a trusted third party who can act as a repository of trust (Kelman, 2005; Ayrton, 2012), or where choices are constrained and competing values compel action (Ayrton, 2012).

At the heart of rationalist accounts of trust, whether attitudinal or behavioural, is the assumption that trust applies in situations of risk, which is taken for granted in the majority of approaches (for example, Deutsch, 1958; Luhmann, 1988; Coleman, 1990), although a minority have asserted that radical *uncertainty* better accounts for the context of trust (Möllering, 2006; James, 2014; Möllering, 2014). The distinction between risk and uncertainty is categorical, but rarely properly maintained, as classically articulated by Frank Knight (1921). For Knight, risk constitutes unknowns that are *measurable*, while uncertainty arises from partial knowledge, which makes it impossible to classify outcomes, let alone assign them probabilities (Langlois and Cosgel, 1993). Risk belongs to a mechanistic model of human behaviour where sufficiently complete knowledge to form a calculation is present, while uncertainty is more at home in organicist thinking which allows for messiness, change and process, calling for *judgement*, rather than calculation (Langlois and Cosgel, 1993; Karpik, 2014; Möllering, 2014). Not only is trust possible in the absence of sufficient knowledge for calculative thought and action to be possible, this is where it becomes most interesting (Möllering, 2013b, p. 54). As Bourdieu has argued:

“To reduce the universe of forms of conduct to mechanical reaction or purposive action is to make it impossible to shed light on all those practices that are *reasonable* without being the produce of a reasoned purpose and, even less, of conscious computation.” (Bourdieu and Wacquant, 1992, p. 119-120, italics original)

This does not suggest that conscious and carefully assessed - even self-interested or opportunistic - action never exists. Trust may still be relevant in these spaces, although it will be based on *deliberative expectation*, rather than on calculation since under conditions of uncertainty rather than risk the latter cannot function (Barbalet, 2009). Nevertheless, it is not the only (nor the most usual) form of action, which may also be driven, for example, by moral, emotive or habitual modes – and, in human association, multiple modes usually play together (Simmel and Hughes, 1949; Möllering, 2006; Elder-Vass, 2007). It is clear that simply combining assessments of attitude and behaviour is insufficient, as I have shown that there is no automatic logic connecting the two (Möllering, 2001). A shift in focus to the process of *trusting*, rather than the justification of trust is necessary, that can account for its dynamics rather than taking for granted the crucial questions of how it comes about, how it fails, and how it is built (Frederiksen, 2012; Möllering, 2013a). Rather than being a past accomplishment that holds until otherwise stated, or a present singular act, the

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notion of trust as a process, or *trusting*, brings trust into the present continuous, as something that goes on being made and renewed.

Although there is a range of process approaches available (Möllering, 2013a), I adopt the Simmelian approach expounded by Guido Möllering and Morten Frederiksen. Frederiksen positions Simmel's work on trust within his formal sociology. Forms, as "the synthesizing principles which select elements from the raw stuff of experience and shape them into determinate unities" (Levine, 1971, p. xv), provide regularities to relational dynamics (Frederiksen, 2012). A social form may appear very differently dependent on specific circumstances, but has sufficient stability to classify diverse iterations, albeit it is dynamic, emerging, developing and waning over time (Levine, 1971; Frederiksen, 2012). For Simmel, trust is situated between knowledge and ignorance; however, the exercise of judgement, or interpretation, based on limited information is insufficient to warrant the state of favourable expectations regarding the outcome of dependency that trust enables to be reached (Frederiksen, 2012; Karpik, 2014; Möllering, 2014). There is a "further element of socio-psychological quasi-religious faith" (Simmel, 1990 [1907], p. 179), which Simmel describes elsewhere as the "affective, even mystical, 'faith' of man in man" (Simmel, 1950 [1908], p. 318; cited in Möllering, 2006, p. 109). Möllering adds to the Simmelian term 'leap of faith,' which appropriately "connotes agency without suggesting perfect control or certainty" (Möllering, 2006, p. 110), his own term, suspension, which indicates the leap over the "yawn[ing] gorge" of ignorance and the unknown, a move which "brackets out uncertainty and ignorance, thus making interpretative knowledge momentarily 'certain'" (Möllering, 2001, p. 414). This process of bracketing indicates putting aside uncertainty and vulnerability, without eliminating them, a process that takes place embedded within relationships and networks (Möllering, 2006). The practice of trusting forms a kind of feedback loop as trusting enables the production of trust, through learning (Dietz, 2011; Möllering, 2013a). For Möllering, therefore, trust comprises a sequential process of interpretation, suspension, favourable expectations, and feedback/evaluation. It may operate on a number of bases (he cites reason, routine, and reflexivity); however, all of these are enabled by suspension, which allows positive expectations of others to be reached (Möllering, 2006).

While an appealing solution to the limitations of purely attitudinal or behavioural constructions of trust, the process approach is not so straightforward to operationalise (Möllering, 2013a). Methods need to provide rich detail about actual trust experiences, while capturing dynamics of trust over time, whether through longitudinal study or the use of questioning to access the development of trust (Möllering, 2006). They need to hermeneutically connect actual human interactions with intentions, whilst enabling sensitivity to how relations, situations and objects of trust are combined in a particular case (Breeman, 2012; Frederiksen, 2012). They need to be sensitive to the immediate



and wider context in which trust is operating – the uncertainty and vulnerability experienced historically and embedded in relational and institutional networks (Möllering, 2006; Li, 2012; Möllering, 2014; Siebert *et al.*, 2015). They must also enable difficult and relevant topics to be investigated in ways that foster the generation of problem-specific theory (Möllering, 2014; Ferrin, 2015). These qualities, Möllering (2006) suggests, are difficult to achieve through highly standardised techniques (although the potential of working towards quantitative approaches in mixed methods studies should not be excluded), and therefore interpretive, in-depth qualitative approaches, in particular interviews, are likely to form the baseline in a particular substantive application.

The sensitivity and flexibility of in-depth qualitative methods make them suited to investigating trust processes; however, in the context of dominant preconstructions I have described, they also pose greater threats, by virtue of their non-standardised instruments, of conceptual lapses. The specific formulation of direct questions becomes all the more critical where a degree of improvisation is incorporated into a research design, as it may be easy for the balance to implicitly shift towards a preconstructed account of trust. For example, assumptions may be made around the meaning of behavioural decisions reported, or through favouring questions such as “why did you trust ...” and “what were your reasons ...” which ask participants to generate post-hoc rationalisations that may only provide a very limited account of their engagement in the process of trusting.

In light of the dominance of preconstructions in the field of trust research, therefore, the challenge remains as to how to ask good questions that are consistent with a process perspective on trust without lapsing into rationalist assumptions, whether attitudinal or behavioural. To assist in the rigorous construction of the object in research design, I have experimented with the use of an expressive multi-modal method, storyboarding, as a heuristic and reflexive tool. I will briefly outline my methodology in this creative approach (Kara, 2015), before presenting and discussing the storyboards themselves.

### **3.4 Methodology**

Proponents of visual or arts-based methods assert that visualisations should not only inform the audience, but also the thinking of the producer(s): generating unexpected results, revealing associations not otherwise evident, and enabling the portrayal of complexity (Grady, 2006; Tufte, 2006; McNiff, 2008). Artistic products may legitimately enable knowing in their own right (Graham, 2005); however, the process of production is also fruitful, as stepping beyond one’s habitual ways

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of working can be a source of inspiration and a fresh perspective (Eisner, 2008; Jacobsen *et al.*, 2014):

“A shift in methodology can bring tremendous insight and relief ... the use of our hands, bodies, and other senses as well as the activation of dormant dimensions of the mind, may offer ways of solving and re-visioning problems that are simply not possible through descriptive and linear language.” (McNiff, 2008, p. 33)

Visualisations are routinely used in the construction of social scientific theoretical frameworks. They produce scientific reality through inscribing and thereby externalising the researcher’s internal imagery of a phenomenon, creating an intersubjective object that mediates between persons and things (Lynch, 2006; Wagner, 2006). Ordinarily, these visualisations encompass a very restricted range of ‘cultural goods’ (Bourdieu, 1984 [1979]), such as flowcharts, checklists, figures and tables (Grady, 2006; Wagner, 2006). In some cases, these are accompanied by paralleled textual description that renders them “a picture of nothing with no distinctive role in the text” (Lynch, 1991, p. 6). Michael Lynch has observed in the common features of social theoretical visualisations, including bounded labels, quasi-causal vectors, and spatial symmetries, a “rhetorical mathematics” which operates through employing “modes of representation that act as emblems of scientific authority” (Lynch, 1991, p. 18). Social thought has as yet made very limited use of the range of visual forms available.

In data collection, however, and in particular within the field of participative methodologies, a diverse array of visual/creative practices are legitimately used, including mapping, sorting and ranking exercises (Rietbergen-McCracken and Narayan, 1998), photovoice (Wang and Burris, 1997; Evans-Agnew and Rosemberg, 2016), community-based participatory video (Chávez *et al.*, 2004; Mitchell and de Lange, 2011), participatory theatre (Kaptani and Yuval-Davis, 2008; Abah *et al.*, 2009), and drawing or other ‘handmade’ or folk art techniques (Anderson and Gold, 1998; Feen-Calligan *et al.*, 2009; Theron *et al.*, 2011). In addition to the visual data that is created, the measured pace of participative activities demarcates “reflective time to construct knowledge” (Gauntlett, 2007, p. 185; Mitchell *et al.*, 2011b), the opportunity to enter into a ‘playful’ mode, but with a serious and higher purpose of “information not distraction” (Grady, 2006, p. 250; Gauntlett, 2007). Although deliberation is an essential part of standard academic writing-based practices, Gauntlett suggests there is something qualitatively different about the reflective process involved in physically making an artefact – the time that is taken, the physical act of making, and the product that you can look at, think about, and adapt (Gauntlett and Holzwarth, 2006). The systematic application of creative/visual methods has the potential to enable greater conceptual clarity and consistency of application as these concepts are generalised.

In order to explore trust in a creative mode, I made use of storyboarding. This technique provides a bridge between the static visual art of drawing, the dynamic art of film, and the textual art of narrative fiction, harnessing the potential of these forms for the imaginative telling of sociologically significant stories (Graham, 2005; Drake, 2014; Klorer, 2014). It is multi-modal, in that it interdependently incorporates both text and image, giving freedom where one mode sufficiently communicates meaning for the other to take on a greater level of abstraction or expression (McCloud, 1993; Jewitt, 2008; Mitchell *et al.*, 2011a). Within the social sciences, its main application is as a stage in the process of community-based participatory video, where it provides an outline with each drawing representing a camera shot, although it has been used as a stand-alone participatory method (Mitchell *et al.*, 2011a; Labacher *et al.*, 2012).

Storyboarding is similar to Edward Tufte's notion of a 'confection', which uses dramatic tension, an author's voice and rhetorical exposition to "make reading and seeing and thinking identical" (Tufte, 1997, p. 151; cited in Grady, 2006, p. 253). It is also related to cartoons, comics or graphic novels, which I will generalise under Scott McCloud's term 'sequential art', defined as "juxtaposed pictorial and other images in deliberate sequence, intended to convey information and/or to produce an aesthetic response in the viewer," sometimes incorporating humour as a central aspect (McCloud, 1993, p. 9; Bartlett, 2012). This connection yields further social scientific parallels, in the production of highly personalised life-narratives, or 'autographics' (Whitlock, 2006; Cameron and Theron, 2011; Hughes *et al.*, 2011), in contrast to the far more 'objective' use of 'concept cartoons', commonly used in scientific teaching to stimulate thought and learning about conceptual ideas found in the material world (Keogh and Naylor, 1999). I favour the term 'storyboard' in this study due to the status of the storyboard as an intermediate art form in the context of film-making, which echoes my use of the technique as a mediator between conceptual and empirical stages of the research process. Nevertheless, the technique shares with sequential art the capacity to foster engagement with abstract concepts as well as generating personal, expressive responses, which are both useful qualities for present purposes.

Although within participatory approaches participants are actively encouraged to put aside questions of skill (Gauntlett, 2007; Theron *et al.*, 2011), within arts-based research this issue is hotly contested. In the extreme, researchers are warned against "amateur" (Eisner, 2008, p. 9) or "inferior" attempts that appear to "mock" the artistic domain (Piirto, 2002, p. 433), while others have taken the more pragmatic view that it depends on the purpose of the visual/artistic medium within its particular application (Sinner *et al.*, 2006; Pauwels, 2015). While I favour the latter approach, in this case I worked with a skilled illustrator (as described below), Jo Le Prevost, in order to produce the storyboards shown here. Since my primary objective is to learn through the process of production, it is arguable that my own source images were sufficient; however, secondarily, I

intend these images to be useful for intersubjective discussion, and Jo possesses the skill that I lack to communicate the expressive meaning my scenarios seek to convey. Further, through the dialogic process of co-production this meaning was clarified and refined, although this may be a benefit of collaboration more generally rather than a specifically artistic advantage.

I began the process using a scenario which I presumed to be relatively innocuous – of a mother, Julie, undertaking a charity parachute jump to raise funds for the Parents' and Teachers' Association (PTA) at her daughter's school (*Figure 3.1*). The focus is on Julie's trust in Instructor Joe from *MEGA PARACHUTES* to ensure her safety. I produced basic pencil and pen drawings of the scenario, reflectively annotating them on a larger piece of paper. I then chose two additional scenarios, deliberately very different to the first and each other, but where

### The Dilemma

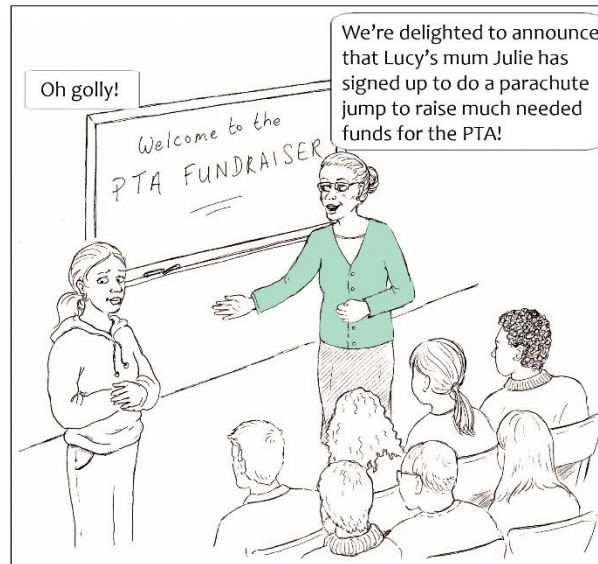


Figure 3.1 The trust dilemma

the degree of uncertainty, vulnerability and interdependence made trust particularly relevant (Li, 2012). The second scenario considers advocacy in the context of mental health, and arose through discussion with a friend who has significant personal experience in similar contexts. The third explores sexual decision-making in a context of high HIV prevalence, and was inspired by a sexual health campaign undertaken in Kinshassa, Democratic Republic of Congo, in the 1980s to promote condom use (Piot, 2012; the brand name and tag line on the billboard in *Figure 3.5* are original). I planned and drew the scenarios in full, annotating with notes and diagrams as I went. Alongside the artistic process, I kept separate notes reflecting on the process, which I also refer to below.

I next shared the annotated storyboards with Jo Le Prevost, and we discussed the theoretical ideas they contain. Jo replicated my storyboards faithfully in relation to the visual and verbal content as well as the expressive intent, and I only asked for a few minor amendments to her early drafts, particularly the addition of colour. Although I worked relatively directly with Jo on this occasion, in our discussion she voiced her own reflections on the choice of an appropriate style between realism and a more abstract, informal style, in light of the range from more frivolous to extremely serious subject-matter in the scenarios, and the product is marked with the subtlety of this artistic interpretation which is entirely her own. The process was therefore iterative and dynamic, making use of the expressive capabilities of both verbal and visual components (Cameron and Theron, 2011; Pithouse, 2011).

Given the more usual application of expressive visual methods in the context of participative, autoethnographic or arts-based data collection/generation within the social sciences, it is important to clarify the status of the storyboards in this case. While arts-based researchers recommend bracketing out existing theories and ideas as they enter the process of artistic experimentation (McNiff, 2011), in this case I explicitly set out to explore ideas and theories found within the intellectual context through creative means, attempting to hold these conceptual ideas and the imaginative story I was depicting in mind together, and asking them to speak to each other. The visual is not the “primary” mode (McNiff, 2011 p. 385); rather it is an equal partner to more conventional verbal discussion.

Further, in this case, I do not consider the storyboards to form data. In order for them to qualify as such they would need to be produced within an autoethnographic framework, which explicitly harnesses the self as a means of accessing culture through the use of introspection as a data source (Wall, 2006). This would require a focus on a particular aspect of culture of which I had personal experience (auto), enabling understanding of cultural experience more broadly (ethno) through systematic analysis (graphy) (Ellis *et al.*, 2011). Although autoethnographies usually take the form of personal narratives, drawing has been productively used (Derry, 2005; Pithouse, 2011). In this case, however, while I am drawing on my experience of trusting and I have a relation to the storyboarded scenarios implicated by my choice of them, they do not reflect historical personal experience. As products of my imagination, they are limited to what is conceivable to me (see discussion). There is no referent in the real world; rather they are “sociologically-informed speculative scenarios” (Jacobsen *et al.*, 2014 p. 12) whose referent is *conceptual*: the process of trusting. In my view, they hold the same status as pre-empirical logical argumentation, including where verbal scenarios are used. They do not reveal anything about the substantive topics that they refer to, which requires empirical investigation. The addition of this creative phase to the process of research design is rigorous to the extent that it refines the operationalisation of trust and enhances reflexivity in the research process, including ethical consideration of the positionality of the researcher, which I return to in the discussion, below.

### 3.5 Trusting: in pictures

Returning to my original scenario, *Figure 3.2* shows the parachute jump according to the Simmelian process construction of trust outlined above. The context is one of high vulnerability, with Julie’s life in jeopardy as well as the potential for serious repercussions for the school and others associated with it; however, the level of uncertainty is low: it is highly unlikely that Instructor Joe

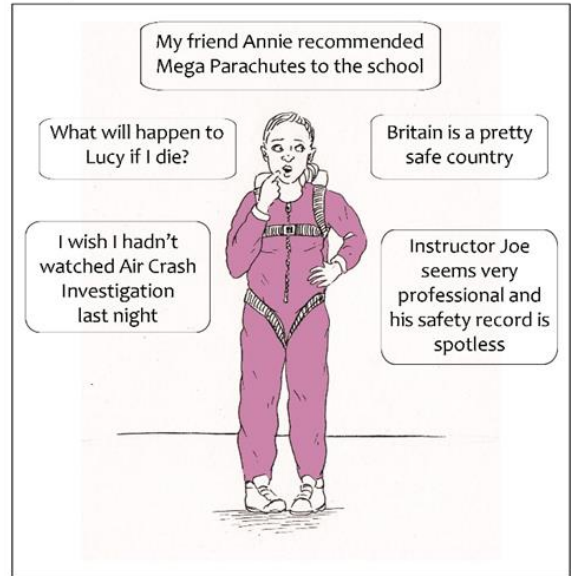
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will act negligently or maliciously. In her process of interpretation, Julie exercises judgment with reference to her personal impression of Instructor Joe and knowledge of his credentials. She also considers the national-structural context and (implicitly) safeguards in place, as well as relying on her trust in a third party intermediary. However, her daughter's dependency on her is at the forefront of her mind and this concern and responsibility heightens the anxiety that naturally results from her vulnerability. She is also impacted emotionally by her cultural environment: in this case, her consumption of television programmes that affect her judgement of aviation safety. Although uncertainty remains, in the moment of suspension Julie brackets it out as she reassures Lucy in earnest that the outcome will be favourable. She is able to act with the expectation that all will be well, and as she experiences the exhilaration of the jump, she forms a memory that would (consciously or otherwise) inform future dilemmas as to whether to engage in extreme aerial sports.

**Context**



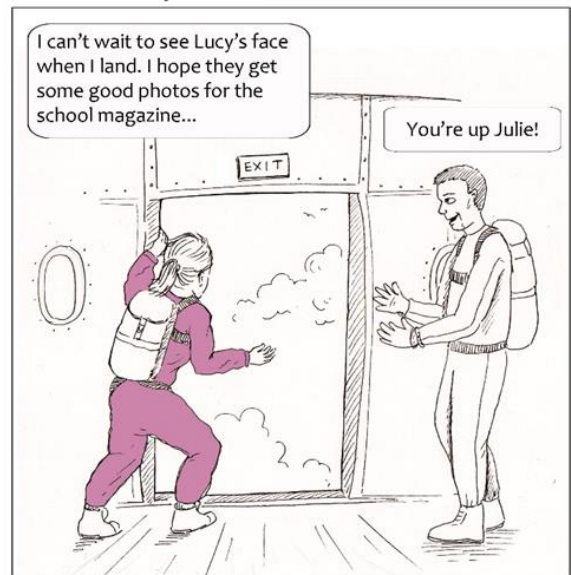
**Interpretation**



**Suspension**



**Favourable expectation**



**Evaluation/Feedback**



Figure 3.2 The PTA parachute jump

Although the expressed purpose of this simple scenario was to focus on the personalised trust between Julie and Instructor Joe with respect to her safety in the parachute jump, I was struck by the complex network of relationships involved within the institutional environments of the school and the company MEGA PARACHUTES. Throughout these relationships trust is functioning, often reciprocally, with respect to the dilemma of Julie's safety, and under the influence of external structural forces, as indicatively illustrated (adapted from storyboard annotation) in *Figure 3.3*.

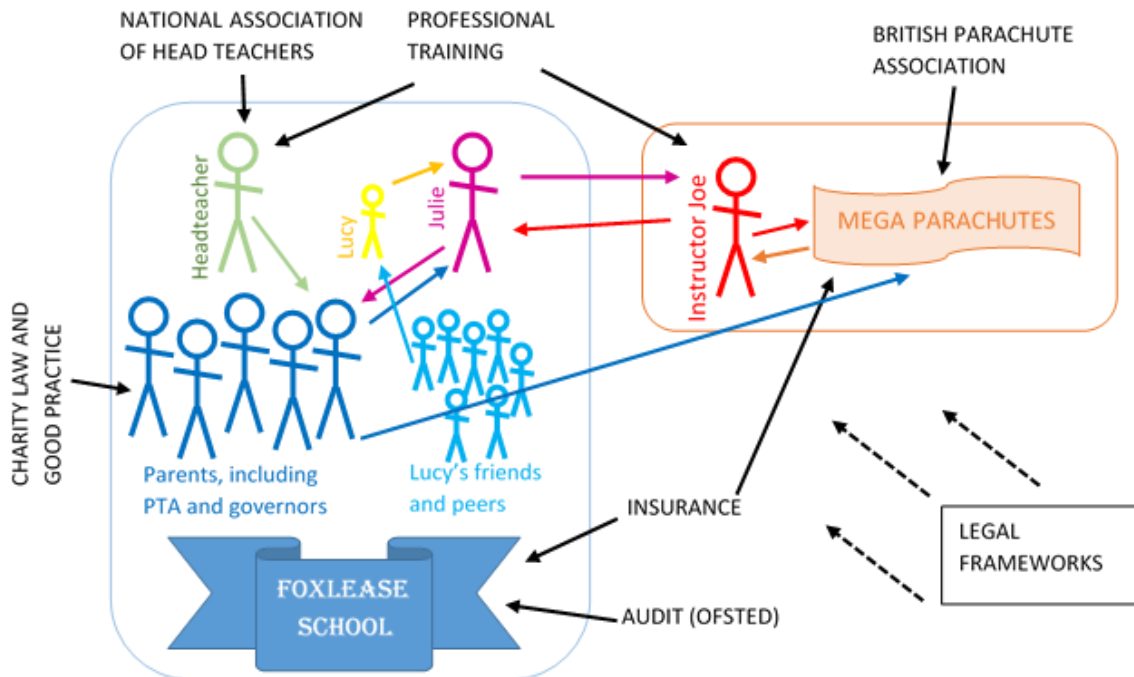


Figure 3.3 Trust between social agents implicated in the parachute scenario

I began working with a tri-part relation between two specified individuals and the relevant matter to which trust related (Hardin, 2006). However, the process of storyboarding quickly made it apparent that this relationship is inseparable from the broader relational, institutional and cultural context within which it is situated. It is feasible that the “moral psychology of being trusted” (Mitchell, 2001) by the PTA and by Lucy to do the jump has substantial influence over Julie's action, alongside her trusting of Instructor Joe. Rationalist accounts of trust tend to treat it as a substantial property of social agents, and this is encouraged by research designs which take individuals, groups or institutions as the units of analysis. In generalised survey research, for example, although some surveys ask a series of questions of specified others<sup>8</sup> to circumvent the ‘radius of trust’ issue (Sturgis and Smith, 2010; Delhey *et al.*, 2011), such attempts do not identify the *object* of trust (trusted in regard to what), nor the wider circumstances (Frederiksen, 2012). A relational approach may

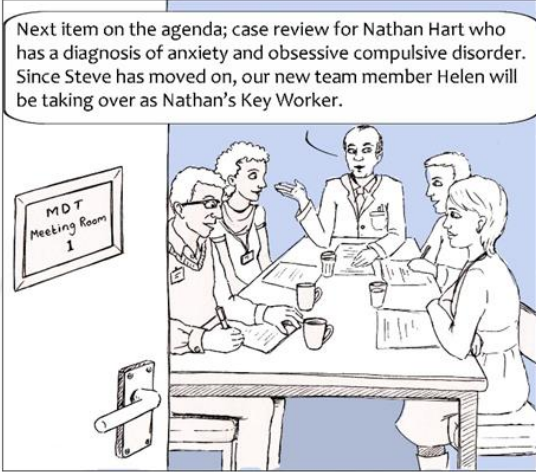
<sup>8</sup> Afrobarometer Afrobarometer (2012) Liberia Round 5 Questionnaire: The Quality of Democracy and Governance in Liberia. *Afrobarometer.*, for example, distinguishes between “fellow citizens,” “your relatives,” “your neighbours,” and “other people you know.”



benefit from taking as its focus the *trust dilemma*, which is likely to encompass multiple interdependencies, vulnerabilities and uncertainties. Although in *Figure 3.4* my protagonist is generally Julie, in the 'context' frame the perspective shifts to the head teacher. This could be expanded on by generating multiple storyboards that exploit the filmic-literary device of point of view. My interpretation of the storyboard tentatively indicates that through taking trusting dilemmas rather than trustors (or dyadic relationships between trustors and trustees) as the object of trust research, substantialist conflation of the object of research (trust) with the population that embodies it can be avoided (Bourdieu and Wacquant, 1992; Grenfell, 2014), and the dynamics of trust may be more adequately captured in their relational context.

The second dilemma (*Figure 3.4*) focuses on Nathan, a UK mental health service user, and his trust in a new key worker, Helen, to advocate on his behalf in relation to the various agencies he depends on. He is vulnerable to the loss of Cognitive Behavioural Therapy as well as the review of welfare benefits he receives on account of his condition, and there is uncertainty as to what the Multi-Disciplinary Team (MDT) his case sits under will decide. A lot is riding on this first meeting as these decisions are imminent. Nathan's process of interpretation is complicated by personal impressions of Helen being combined with her institutional affiliations and her professional group identification, as well as seemingly extraneous aspects of his circumstances and wellbeing which enter into his judgement. Nathan's leap of faith is to suspend his reticence and confide in this near-stranger, believing that she will be assertive on his behalf, even as a newcomer in an established professional team. Helen's early success in securing an extension to Nathan's therapy is likely to be a consideration in the building of trust going forward.

**Context**



**Psychologist:** Nathan has received 12 weeks of CBT and I would like to review this therapy as progress hasn't been substantial.  
**Social worker:** Nathan's benefits are due to be reassessed next month too.

**Interpretation**

I wonder where her accent is from, I hope she knows the UK system.

She never returned my wife's call yesterday.

She seems friendly, but I've never met her before.

Judith asked me to try to make this relationship work.

I heard on the news they're cutting mental health services - all they care about is their budgets.

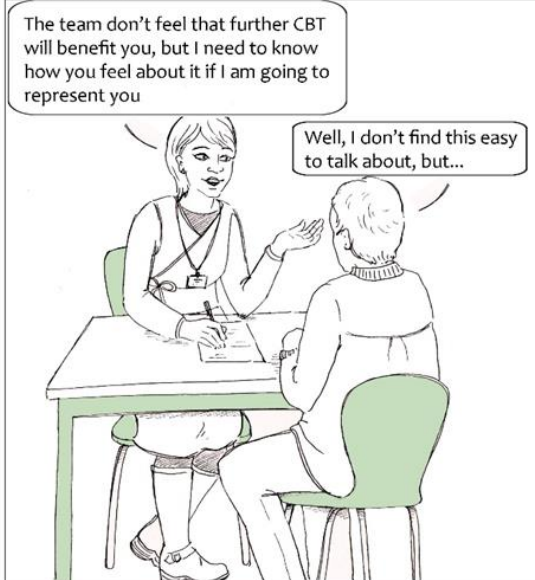


My last key worker messed up my benefits application.

I feel very anxious that they might take my benefits away, I'm hardly sleeping at all.

**Can I trust this professional to advocate for my interests?**

**Suspension**



**Favourable expectation**



**Feedback**

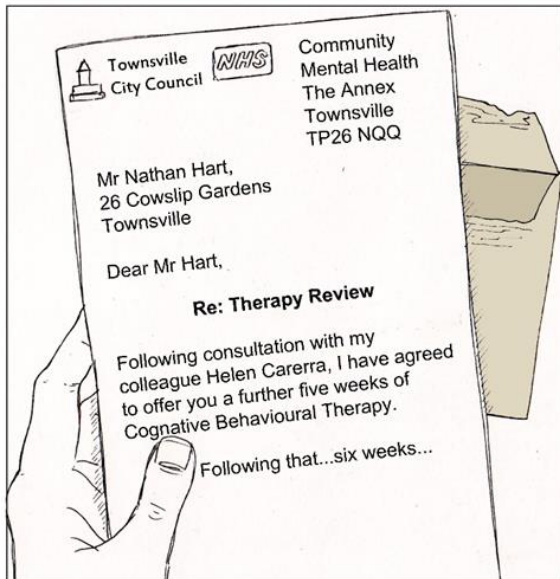


Figure 3.4 Advocacy in the context of mental health

The bases of Nathan's interpretation particularly interested me in this scenario, as I imagined what might go through his mind as he first met Helen (see next section for a discussion of the limitations of this exercise), both in terms of content and structure. In relation to content, the storyboarding format only enabled me to present a limited indication of the broad bases of interpretation that I imagined, which are listed in full in *Table 3.1*. In general, the complexity of this scenario made it more challenging to keep the balance between visual and textual elements. Although both play a distinctive role, this storyboard is more text-heavy than the others. In examining substantive topics with this complexity there may be more dependency between the multi-modal storyboard and the surrounding textual explanation, or longer and more complex storyboards, as overloading with text begins to eclipse the interplay between textual and visual that gives the storyboard its particular explanatory potential.

The central relationship between Nathan and Helen stimulated me to reflect on the distinction between institutional, group- or identity-based and personalised levels of trust, which trust research often deals with discretely. This is conveyed in the 'interpretation' frame, where the artistic device of perspective enables Helen's name badge to occupy the foreground, acting as a filter to the personalised relationship between Helen, who is depicted, and Nathan, whose view the frame takes. The institutional logos raise the issue of Helen's affiliations: she does not enter Nathan's life solely as a generalised stranger; she is a representative of Townsville City Council and the NHS. Nathan is likely to have sustained historical interaction with these agencies, enabling him to hold views as to their competence, be familiar with their self-presentation (branding and PR) and their positioning relative to other institutions, groups and individuals. In other words, it is questionable whether, where there is sustained interaction, institutional trust can really be considered 'relationship-free' (Li, 2015). Helen's name badge also identifies her as a member of the Community Mental Health Team. Specifically, she is a key worker - an out-group category with which Nathan has past experience - and this group-based identity also enters into his judgement. Although as a personalised relationship grows this may have greater influence over his interpretation, it is likely that these institutional and (professional) group identities will continue to mediate the dynamics of trust between Nathan and Helen, and it would be an assumption to expect this to happen in a linear or evolutionary way (Lewicki and Bunker, 1995; Frederiksen, 2012).

Table 3.1 Possible bases of interpretation

<b>Base of trust</b>	<b>Explanation</b>
Personal knowledge:	
Historical interaction	Past experiences of interdependency feed back into present interpretations.
Competence evaluations	Judgement may be based on past experience, subjective impressions, direct questioning/testing, recommendation, objective measures or assumptions from institutional affiliations.
Appearance	Influence of visible/auditory characteristics, including body shape, posture, facial expression, voice, style of dress.
Socio-cultural positioning and identity	For example, group-based identifications associated with race, ethnicity, social class or age.
Institutional affiliations	Known or suspected affiliations with institutions of which agents may have personal knowledge and/or distinct vulnerabilities.
Regulatory context	Recourse to rights or provisions through legal frameworks, political processes, institutional bodies or media that are protective of individual vulnerabilities.
Emotions	Including those related to the people and/or situation at hand, as well as contemporaneous but external experiences.
Interpersonal influences:	
Trusted third party intermediary	Availability of a third party repository of trust.
Third party opinion and experience	Awareness and influence of the views and experiences of others.
Cultural influences	Broadly conceived, including norms and values, in addition to cultural products.
Spiritual or ideological frameworks	Formational beliefs or ideas that consciously or routinely influence thought and conduct.
Physical factors	For example, alertness/fatigue; hunger; influence of drugs; physical pain or illness; disability, e.g. visual or hearing impairment.

My third scenario departs from the UK context, being located in an unspecified place in sub-Saharan Africa where HIV prevalence is high. Unlike the previous scenarios, the focal relationship is an intimate one – between a couple, Grace and Luc. Grace understands her vulnerability to contracting HIV if she engages in an unprotected sexual relationship, and she is uncertain as to Luc's views on condom use (Figure 3.5). She involves a trusted friend, Mayifa, in her interpretation process where her emotions are in tension with popular wisdom and recent experience of a known

### Context



Figure 3.5 Grace's dilemma

third party (Figure 3.6). The dilemma reaches a moment of decision at a wedding, where Luc and Grace have the opportunity to be alone together (Figure 3.7). What response on Grace's side would indicate that she trusts Luc?

Practitioners of community-based participatory video suggest that the storyboarding task involves both the telling and the resolution of a story – it must have a beginning, a middle and an end (Mitchell *et al.*, 2011a). In conceiving of a resolution to this scenario I began to find my authorial voice extremely problematic (which I will return to in the next section). Initially I assumed that trusting Luc would involve proceeding with sex without instigating a conversation around condom use. However, I realised that this was exactly the kind of external labelling of behaviour that I was seeking to avoid. In fact, each of the three

### Interpretation

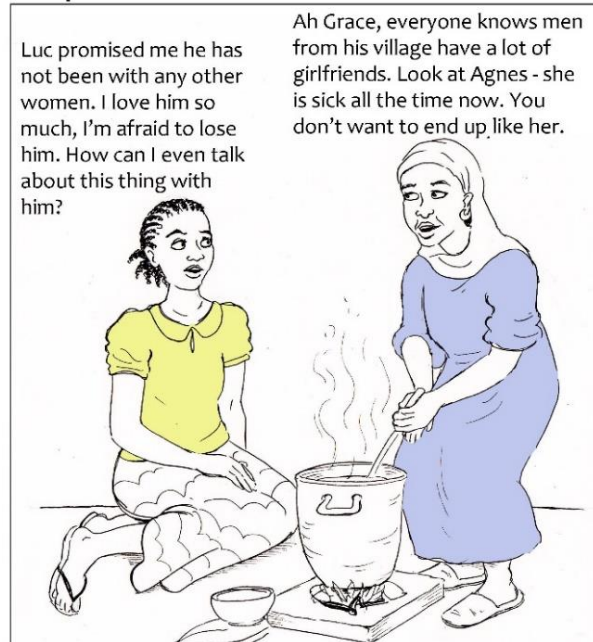


Figure 3.6 Grace's interpretation

responses in the 'suspension' frame could have a variety of interpretations. If she agrees to go with him it may indicate that she has suspended her uncertainties regarding the outcome. It may also be that she is undecided (agnostic) or even distrusts Luc, but that physical and/or emotional

dimensions override, or alternatively that she lacks a language or is otherwise disempowered to engage in a discussion, due to cultural taboos or more generally the agency of women in her society. If she declines Luc's proposition, this may indicate either distrust or agnosticism – that is, that she is still in the process of interpretation – or it may simply be a truthful statement that she wants to stay at the party. Alternatively, if she instigates a conversation about condom use, this could indicate distrust – that she does not believe he is not at risk of infection. However, if in Grace's judgement her greatest vulnerability is a break up, this may indicate trust: the leap of faith to raise her concerns and feelings believing he will be receptive.

**Suspension**



Figure 3.7 Grace's leap of faith

In depicting the outcome of the scenario I was concerned about how contrived the story had the potential to be, a caution that Ruth Bartlett (2012) expresses based on her experience of using cartoons to disseminate research around dementia activism. In particular, I was uncomfortable with the risk of stigmatising people in situations like those of my characters through my representation of them. I was torn between depicting a 'tale of woe' that I have absorbed through representations of gendered inequalities in developing countries by campaigning and fundraising efforts, and a 'tale of empowerment' which may be more reflective of the freedoms I am privileged with, the absence of freedoms (according to Sen, 1999) being the very definition of poverty, and an inescapable aspect of the field conditions I am attempting to portray. I decided to open up a range of potential responses for Grace, by shifting from the storyboard approach that seeks a singular resolution and combining it with a 'logic modelling' approach which is commonly used in programme design and evaluation (W. K. Kellogg Foundation, 2004), as outlined (and, in some cases, depicted – *Figures 3.8 and 3.9*) in *Table 3.2*.

This breaking of artistic forms enabled creative progress (McNiff, 2011). Considering a range of favourable (or unfavourable) expectations that Grace may come to when she makes her leap of faith suggested that she would have contemplated these possible futures as part of the process of interpretation. Interpretation not only draws on thoughts, feelings and sensations, but is also an imaginative process where possible futures are envisaged. This underlines the interconnections between stages in the trust process and reaffirms that this process framework is a conceptual construction, a tool for thinking (to adopt Bourdieu's term), that can be powerful analytically

without these stages being neatly distinguishable in empirical reality. At a more detailed level of operationalisation, in order to avoid questions that will implicitly illicit a rationalist response within the context of an in-depth interview, based on the tentative reworking of ‘favourable expectations’ as one among possible ‘imagined futures’, I might ask, “what did you imagine might happen depending on how you acted?” This formulation assumes a retrospective approach; however, it would be possible to plan research around times of transition and investigate the trust implications of such conditions in the present tense.

Table 3.2 Grace's 'imagined futures'

Immediate outcome	Medium-term outcome	Long-term outcome
Luc dumps Grace	Grace is happy with another man.	No one will marry Grace because she is HIV+ and has another man’s child.
Decision is postponed	Grace is single and Luc has spread vicious rumours about her	
Sex without protection		Grace and Luc are married to different people. They are all now HIV+. None of them know this.
Sex with protection	Grace and Luc are still together; both of them are HIV+ but they do not know.	
Luc suggests they both get tested so they can be sure they are safe.	Grace and Luc will soon be married. They used protection until they could be tested for HIV.	(Figure 3.9)

(Figure 3.8)

Across the three scenarios I sought to identify dilemmas of interdependence where vulnerability and/or uncertainty varied (Li, 2012). Retrospectively, comparing the scenarios highlights some difficulties in making these kinds of evaluations. Does the threat to Julie’s and Grace’s lives make their vulnerability greater than Nathan’s? This overlooks the potential for self-harm in Nathan’s case. Is Julie more vulnerable than Grace on account of having a dependent child? These considerations would disrupt the hierarchy of vulnerability that I would intuitively have

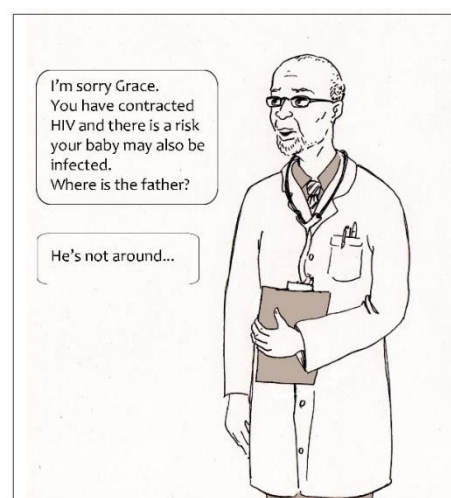


Figure 3.8 Medium-term outcome

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assigned. What follows, in agreement with my previous findings, is that the paradigm of risk where an external 'expert' can form a calculation is inappropriate; rather, taking the perspective of research subjects, what is relevant is their response to the threats they face, which takes the form of fear, or anxiety (Ayrton, 2012). The contextual conditions of the three scenarios make the fear experienced by the central characters in a real sense incomparable. This suggests that assessments of vulnerability, in parallel to the distinction in poverty studies (Alcock, 2006), may need to be contextualised and relative, rather than absolute.



Figure 3.9 Long-term outcome

### 3.6 Discussion

A cognitive theory of art suggests that, as works of imagination, art enables us to better understand the human condition, not by revealing how the world is but by enriching our experience of it: "The question to be asked of such a work is not, 'Is this how it really was?', but rather, 'Does this make us alive to new aspects of such an occasion?'" (Graham, 2005, p. 69-70). My discussion has shown how the creative practice of storyboarding has revealed new aspects of situations in which trust is necessary through ideal type characters, speculative scenarios and the use of literary-artistic-filmic devices (Drake, 2014; Jacobsen *et al.*, 2014).

Images, or art, are also deeply revealing of their producers and the artistic process is a rich source of self-knowledge (Piiro, 2002; Cole and Knowles, 2008; Weber, 2008). Bourdieu's call to greater reflexivity in sociology involves rapt attention to subject-object relations – not only objectifying the object of research, but also the process of *objectification* of the object, in recognition that I enter research with a pre-existing relation to my research object (Inghilleri, 2005; Deer, 2014). Expressive visual methods such as storyboarding provide an equally valuable means of "drawing [myself] into research" (Mitchell *et al.*, 2011b p. 34) that will enable the kind of reflexive attention to how I am implicated in it that I would want to achieve.

For Bourdieu, social agents are continuously directed by the habitus: a structured system of dispositions which are formed through absorbing the history of the social field in general and their own particular trajectory within it, "embodied history, internalized as second nature and so



forgotten as history” (Bourdieu, 1990 [1980] p. 56). Although I may imagine a universe of equally possible possibilities for my own thinking, or for characters in scenarios I devise (*ibid.*), in the former case this is an illusion and in the latter it demonstrates the disjuncture between imagined scenarios and historically situated human action: “the virtuoso finds in his (*sic*) discourse the triggers for his discourse, which goes along like a train laying its own rails” (Bourdieu, 1990 [1980], p. xx). In social science, Bourdieu suggests that through examining the object of research, as I have done, it is possible “to find the social conditions of possibility of the ‘subject’ and of his (*sic*) work of constructing the object ... and so to bring to light the social limits of his act of objectification” (Bourdieu, 2000 [1997], p. 120). My construction of trusting situations here, according to Bourdieu, is revealing of my own habitus in relation to the problems I address and, therefore, through reflexively analysing and objectifying my own social position, makes it possible for me to transcend my internally structured limitations in the empirical stages of research that follow (Bourdieu and Wacquant, 1992; Webb *et al.*, 2002). I will exemplify this by drawing on my experience of producing the storyboards depicted here.

As I began work on scenario two, I was conscious that although I have no personal experience of mental health systems, I have some knowledge of them through close friends who have required these services and a relative who is a Consultant Psychiatrist. I considered this relationship of the scenario to my own biography as basically informative; however, as I took on the role of the author (Grady, 2006), able to tell the story of my choosing, I was emotionally exercised by Nathan’s situation, and tempted to use the storyboard as a platform to air my political views relating to mental health care provision. My self-imposed remit to present a scenario where trusting took place was a frustration in this regard. At the same time, I feared misrepresenting the diligence of competent, caring professionals who work at the front line of mental health. There are clues to this struggle in the storyboard: in particular, Nathan’s invisibility - he is spoken about ‘behind closed doors’ in the first and last frame, and his face is never shown.

In scenario two and, to a greater extent, scenario three, I felt increasingly troubled by my ability to dictate the outcome of the central characters. Although the storyboards are fictional, they depict dilemmas faced by many people who I did not want to presume to speak for. I was aware, however, that creating representations of characters *like* them was not a morally neutral act (Tufte, 2006). In this sense, my training in ethical practice prepares me far better to undertake research with participants than it does to take up the authorial voice. The social and cultural difference between myself, as an educated, married, British woman susceptible to few serious health risks, and Grace, made me cripplingly aware that I could know virtually nothing about how she might manage her vulnerabilities. Bourdieu uses the term ‘hysteresis’ to describe the experience of one’s habitus being out of synch with the social world, or field, in which one finds oneself. As a result of the

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mismatch between my own habitus and the field I was depicting, I found that I lost confidence in my own “practical mastery” or “feel for the game” (Bourdieu, 1989; Bourdieu and Wacquant, 1992; Maton, 2012), and became sensitised to the distance between the social worlds of my characters and any in which I would have tacit knowledge of how to act. For this reason I avoided imposing a fixed conclusion in the third scenario.

Obvious as it is that I conceive of myself as an ‘outsider’ to the experiences explored in scenarios two and three, the barrier this presented to the creative process caused me to reflect critically on the ease with which I manipulated the characters in scenario one. I felt no sense of uncertainty or ethical compromise in making their decisions for them and speaking on their behalf. Although there are key differences between the character Julie and myself, as a white, middle class woman with a number of voluntary sector affiliations, I implicitly considered myself an insider to the situation I was depicting. Unconsciously feeling as a “fish in water” in Julie’s social world, and therefore taking aspects of it for granted (Bourdieu and Wacquant, 1992, p. 127), there is a risk of being blinded to power relations and dismissing as ‘the way things are’ aspects that may be significant (ibid.).

Although in this case these are purely speculative scenarios, were I pursuing any of these substantive areas in practice, being conscientised of my political and emotional responses, my ‘insider’ assumptions, and my ‘outsider’ ignorance, would be invaluable to ensure that these dispositional tendencies did not inadvertently enter into empirical research. While it is feasible to an extent that I could explore any trusting dilemma in this hypothetical way, irrespective of my own experience, what is revealed by doing so would be limited to what my habitus enables me to conceive of. Substantive knowledge and the testing of conceptual suppositions such as those suggested above is only possible through empirical practice. However, the deliberative creative process of storyboarding imagined scenarios has uncovered assumptions and generated possibilities with regards to how to operationalise a working theoretical approach to trust, as well as enabling greater reflexivity towards how my habitus is implicated in the object of my research.

### **3.7 Conclusion**

Popular wisdom states that if you do what you have always done, you are likely to get the same results you have always got. The accepted repertoire of methods for thinking conceptually and designing research – namely, logical, verbal argumentation and quasi-mathematical visualisations - are heavily conventional and standardised. As such, they are not necessarily conducive to yielding new insights or accounting for complex, immaterial forms of social practice, such as trust. In this paper I have used Bourdieu’s notion of the construction of the sociological object to argue for the

inadequacy of attitudinal and behavioural approaches to trust on account of their excessively rationalist and partial account of human action. It is not surprising if academic theorising is inclined towards rationalism: Bourdieu warns that the luxury of the academic gaze convinces researchers of the fallacy that people in everyday life theorise the world and their actions within it as they do (Elder-Vass, 2007). On this basis, I have shown the productivity of disrupting habitual academic practices through the introduction of a creative, multimodal method as a reflexive device for the researcher in the pre-empirical stages of research.

This practice firstly encourages continual attention to the construction of the object of research by strengthening the interaction between theory and practice. In this example, it has achieved this by uncovering potential lapses to pre-constructed notions of trust and by suggesting new conceptual dimensions of trust to explore empirically, both of which have practical consequences for operationalising the concept. Secondly, it has enabled reflexive attention to how the researcher is implicated in the object of her research. It provides space to observe emotional responses to the phenomenon under investigation and to reflect on their origins. It is revealing of how habitus limits the scope of what is 'thinkable' or 'knowable', which may predispose the researcher towards certain kinds of findings. These are highly valuable processes to engage with prior to entering the field.

What is it about the creative/visual/multimodal that distinctively opens up these possibilities? There are (at least) three mechanisms at play. The first concerns the creative process, which iteratively opens up and narrows possibilities. Working through a trust scenario, for example, involves imagining many possible responses of the protagonist to their dilemma; however, at some point I had to choose one response, and account for the repercussions of that choice in detail. Movement between the general and the specific is at the heart of learning and the structured use of a creative process harnesses these skills. Secondly, the particular qualities of the visual pose unique questions: in particular, around setting, point of view, what is included or excluded from the frame, perspective, style, or the use/significance of colour. These decisions bring to light different facets of the object of research. Finally, aside from the visual/multimodal product itself, the process of its production opens up deliberative time to enter into a reflective mode of thinking which is conducive to the generation of new insights.

This approach is perhaps particularly relevant where the object of research is abstract, or where dominant conceptualisations circulate which may hamper active engagement with its construction. This is highly relevant to research on trust and the application of storyboarding here has important repercussions for its operationalisation. It suggests that designing research with the trust dilemma as the unit of analysis, rather than the individual agents involved in it, is more suited to examining

the relational dynamics of trust. It sensitises trust researchers to the interaction between different levels of trust: for example, institutional and identity-based factors may be involved in an apparently personalised trust relationship. Studies may have one level in the foreground, but it is important to design into research sufficient space for the complexity of these interactions to be manifest. It also posits a connection between stages of 'interpretation' and 'favourable expectation' in the trust process, as it is articulated by Möllering, through the notion of 'imagined futures', which shifts the way that questions about trust are posed. These are not findings as such, since the material they are drawn from is not data; however, they provide promising directions to be explored empirically.

It is quite possible that others may have different, complementary or even conflicting insights based on examining these storyboards. This demonstrates the intersubjective strength of a multi-modal approach. Michael Lynch has argued that the intermediary status of visualisation is its most distinctive quality: images "stand between persons, and between persons and things; they are both material and symbolic ... they incorporate verbal references into their frames and supply scenic contexts for interpreting them" (Lynch, 2006, p. 37). He observes these features through the case of early ethnographies of scientific laboratories (in particular, Latour and Woolgar, 1979; Knorr-Cetina, 1981; Lynch, 1985). In these studies, literary inscriptions and visual renderings of research objects were iteratively constructed by groups of scientists as a collective work of representation with the purpose of dissemination in a wider scientific community (Lynch, 2006, p. 29). Given the interface of visual and textual in storyboards, there is every reason to expect a similarly productive intersubjective role for these and other visualisations in social science. There are many research contexts where this tripart relation between 'persons' and the 'things' that they study require an intermediary representation to enable dialogue. Storyboards may thus provide a useful platform for discussion in contexts of teaching, for research groups working to co-construct the theoretical basis for empirical work or for shared processes of analysis, or for the sharing of these multi-modal products for discussion and engagement with the wider academic community, in presentation and publication. Multi-modal outputs lend themselves to the corporate and dialogical dimensions of research practice: I follow Rose Wiles and colleagues (2013) in supposing the most fruitful sites of innovation in social scientific research to be found in the diffusion of developments across a community for engagement and adaptation. Multi-modal approaches have a valuable role to play in this horizontal developmental process as they make thought processes, as well as their outcomes, transparent, create a space where abstract discussion can become concrete, and thus provide a platform for dialogue.

Storyboarding has proved itself a useful and, arguably, undervalued tool. It brings together words and pictures to produce a sequential narrative which can represent the intermingling of thought

and action. It is therefore highly versatile to a wide range of applications. It certainly lends itself to examining complex, processual and/or longitudinal phenomena, of which trust is just one example. Although I have shown the value of storyboarding as a heuristic device in the pre-empirical processes of conceptualisation and research design, it has significant and underexplored potential as a data collection method. In qualitative research designs, storyboarding can form either an individual or a group elicitation activity within the context of interviews or focus groups, either with or without the involvement of a visual artist to work alongside participants. It can be used by the researcher as an expressive mode in autoethnography, or by the artist-researcher adopting an arts-based approach. In quantitative or experimental research designs a pre-designed storyboard could serve as a tool to elicit structured verbal or practical responses from participants, in a similar but extended way to the common use of vignettes (Hughes, 1998; Hughes and Huby, 2004). Conversely, other creative and participative methods that are routinely used for data collection may also be fruitful tools for the researcher to hone her reflexive muscles, and further experimentation could expound the specific affordances that different methods offer. More broadly, this paper calls into question the dividing line that is so often drawn between researchers and research participants, and the methods that are appropriate to each. In most respects we are not so different, and techniques that participative researchers use for data collection are available to disrupt, inspire, and reflexively shed critical light on our own thinking.

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## **Chapter 4: The micro-dynamics of power and performance in focus groups: An example from discussions on national identity with the South Sudanese diaspora in the UK**

### **Abstract**

In recent years there has been an increasing recognition that both the content of focus group discussions and the interaction that takes place form indivisible facets of focus group data. Interaction, however, is not a neutral activity but one that is infused with the dynamics of power in wider society and in the immediate context of the group discussion. Approaches to the analysis of focus group data to date have lacked a sufficient account of the micro-dynamics of power that play out within focus group discussions.

In this paper, I use Bourdieu's notion of fields of power to analyse focus group discussions on national identity with South Sudanese diaspora in the UK. I argue that the micro-dynamics of power in focus group discussions have relevance to the relations of power in the population group from which participants are purposively sampled and, consequently, their observation enriches research findings. Further, I observe that the guidance literature on the conduct of focus group discussions encourages power-reduction strategies, and requires updating to allow space for the power-infused character of social interaction to manifest itself in focus group discussions.

### **4.1 Introduction**

For focus group practitioners, interaction between participants is a defining characteristic of the method. Since the mid-1990s, interaction has been centralised in methodological discussion: many consider it to be analytically indivisible from the 'content' of focus group data. The opportunity to directly observe participants' interaction is now recognised as key to its relevance to a variety of social research pursuits. Crucial as this development is, it does not go far enough. Interaction is not neutral; rather, it is shaped by and revealing of the power relations that exist between group members. Insofar as the characteristics which define the selection of participants are meaningful

categories, the micro-dynamics of power that play out in a focus group discussion are revealing of those that operate in the wider population. The ability to observe these power relations is a significant affordance of the method, and a major factor that would indicate its selection. However, focus group literature to date lacks a sufficient account of power. There is little theoretical grounding for or guidance on the analysis of power in focus group data. Further, literature on the conduct of focus groups encourages strategies that serve to minimise power, and thus hide it from view.

In this paper I exemplify an approach to analysing focus group data, with implications for the conduct of discussions, drawing on Bourdieu's notion of fields of power. Participants, I argue, are implicated in positional relations of power which shape their interactions with each other and, in turn, how knowledge is discursively produced. I draw on data from a study that used focus group discussions to explore national identity with the South Sudanese diaspora in the UK, five years after secession from Sudan. In the interactions that took place, four field mechanisms were evident, which I identify and describe. Each of these has implications for an aspect of the guidance on conducting focus group discussions: the selection of participants, the style of facilitation, the positionality of the researcher/facilitator, and follow-up to the discussions.

### **4.2 Interaction and power in focus group discussions**

The term 'focus groups' incorporates discussions that take a wide range of different styles and format, which fall under the general definition of "facilitated group activity" (Kamberelis and Dimitriadis, 2013, p. 6). Focus groups provide an arena for "performances in which the participants jointly produce accounts about topics in a socially organised situation" (Smithson, 2000, p. 105; 2008). They are semi-informal spaces where participants, knowingly observed by the facilitator and (usually) inscribed by a recording device, rehearse social relations. These performative interactions are mediated versions of the everyday interactions that may take place without the intervention of the researcher. As a group-based method, interaction between participants is essential to the generation of data. However, there are markedly differing understandings of the epistemological function of this interaction.

The strong historical association of focus groups with market research has encouraged a substantial school of focus group research for which interaction is a means of eliciting more in-depth content. From this perspective, focus group discussions are viewed as means of 'obtaining' participants' pre-existing, personal ideas and opinions, and thus form a "window" on participants' lives (Wilkinson, 1998; 2004, p. 194; CPRC, n.d.). Group interaction, proponents suggest, serves an *instrumental* role

in data production by bringing about a “synergy” that elicits “rich details” (Carey and Asbury, 2012, p. 28). This stimulates “a more complete picture of attitudes” through provoking agreement and disagreement (Greenbaum, 1998, p. 143), and drawing out more sensitive data from participants (Morrison-Beedy *et al.*, 2001). While interaction is key to the collection of good quality data, it has done its work, for better or worse, from the moment the voice recorder stops.

This content-driven approach to focus group data has been criticised by researchers who accord interaction a more wide-ranging role. Following Jenny Kitzinger’s (1994) landmark paper, a competing school has emerged which gives a more nuanced explanation of the collective dimension of sense-making that participants engage in. By this account, focus group discussions themselves constitute a social context, and interactive processes are therefore open to direct observation (Wilkinson, 1998; Madriz, 2000; Wilkinson, 2004; Duggleby, 2005; Halkier, 2010). Interaction is thus treated as an *intrinsic aspect* of data, and the content of the data indivisible from the social process which produces it. As a consequence, approaches to the analysis and reporting of focus group data need to accommodate both facets (Farnsworth and Boon, 2010).

The beginnings of an approach that allows space for power are evident in Kitzinger’s early article. She writes:

“Interactions can make groups seem unruly ... but such ‘undisciplined’ outbursts are not irrelevant or simply obstructive to the collection of data about what people ‘know’ ... Tapping into such variety of communication is important because people’s knowledge and attitudes are not entirely encapsulated in reasoned responses to direct questions. Everyday forms of communication ... may tell us as much, if not more, about what people ‘know’.” (Kitzinger, 1994, p. 109)

Although power has not been centralised in the focus group literature, an integrative approach to content and interaction implies allowing sufficient space for it to be expressed. However, the guidance on the conduct of focus groups remains to a significant degree rooted in the content-driven perspective. Although there is little explicit treatment of power, it crops up incidentally around three fields of claim: the relationship between participants, the relationship between participants and the researcher/facilitator, and the extent to which focus groups are ‘empowering’.

According to the (content-driven) mainstream guidance literature, power differentials between participants are a “plague” to face-to-face focus groups, which result in what is constructed as ‘problem behaviour’ among participants (Krueger, 1998, p. 57ff; Krueger and Casey, 2015). Power is positioned as a procedural problem that sabotages interaction and therefore limits data quality - for example, through preventing the establishment of group rapport, creating reticence to share or

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self-censoring, encouraging conformity and silencing (Stewart and Shamdasani, 1990; Greenbaum, 1998; Bloor *et al.*, 2001; Carey and Asbury, 2012). Two strategies serve to minimise the dynamics of power in focus group discussions. Firstly, the skilled facilitator may mitigate power through exercising covert and explicit control strategies, for example through room layout, their body language, seating positions, selective use of eye contact, interruption, direct challenge of dominant individuals and expulsion (Greenbaum, 1998; Krueger, 1998; Carey and Asbury, 2012; Krueger and Casey, 2015). Secondly, the selection of 'homogenous' participants, who are likely to share more because they perceive each other as similar (Greenbaum, 1998; Carey and Asbury, 2012). Taken to the extreme, these measures would produce a heavily sanitised discussion which leaves little space for the observation of interaction, much less any insight into power dynamics within the group.

The relation of the participants to the researcher/facilitator makes an important contribution to the creation of a "permissive environment" which encourages free-flowing conversation among participants who feel at ease and unlikely to be judged for their opinions (Krueger and Casey, 2015, pp. 2, 4; CPRC, n.d.). In comparison to individual interviews, focus group discussions are often described as *relatively* egalitarian, due to participants' numerical advantage and their collective power through shared knowledge of the subject matter (Wilkinson, 1998, 1999; Smithson, 2000). Nevertheless, participants' perceptions of the researcher/facilitator's authority position and personal attributes endow them with a disproportionate influence over the discussion (Stewart and Shamdasani, 1990; Krueger, 1998). However skilled the facilitation, some sources of power asymmetry are innate and self-evident. In some studies, particularly with traditionally marginalised groups, researchers have found that authentic sharing is encouraged where the facilitator shares a significant aspect of culture or experience with the group members (Fallon and Brown, 2002; Rodriguez *et al.*, 2011; Caretta and Vacchelli, 2015). Others have found that techniques to de-centre the researcher have served to mitigate issues of positionality and power differential (Jakobsen, 2012). In either case, the position of the researcher/facilitator as an implicitly powerful out-group member is presented as a problem to be overcome.

It is noticeable that these first two fields of claim situate power as a negative force within the focus group discussion and, by extension, in social life. With strong social Darwinist undertones, this approach to power assumes that it is used to marginalise, coerce, dominate or exploit others, and in the focus group discussion this has only negative repercussions for the quality of data. However, this is a very narrow conception of power, which can equally be harnessed to redress inequalities, to promote the interests of a third party, or to suppress self-interest. It is at least conceivable that within focus group discussions power could be exercised by participants in ways that support the interests of the researcher-facilitator or that encourage free expression and equitable representation among participants.



The third field of claim in focus group literature acknowledges the possibility of a beneficent expression of power. Power is collective as well as individual; it does not only operate in the 'top-down' mode of domination, but can represent a collective pursuit of emancipation (Gaventa, 1980; MacKenzie, 1999). This raises the question as to whether focus group discussions foster shared power among participants – that is, whether they have an empowering capacity. Kamberelis and Dimitriadis (2013) identify three functions to focus groups: inquiry, pedagogy, and politics. All three operate simultaneously, but one function is usually dominant, depending on the design of the study and the researcher's objectives. Where inquiry is primary, participants may experience benefits internal to the discussions, such as enjoyment or interest (Morgan, 1998; Kidd and Parshall, 2000), or slightly more lasting effects, such as feeling listened to and a sense of catharsis (Morgan, 1998; Barbour, 2007; Carey and Asbury, 2012). Beyond the discussion itself, the knowledge produced is primarily owned by and of benefit to the researcher. Some commentators have found that focus groups open up permissive spaces where participants can talk about issues that are not usually discussed, which can yield new insights for them as well as the researcher (Kitzinger and Barbour, 1999; Barbour, 2007). This indicates a shift towards pedagogy: the recognition of issues, and the process of understanding their dimensions, is an important step in the direction of empowerment. Where this knowledge transfers from the private context of the focus group to the public sphere, the political function of focus groups comes into view. Knowledge is not only owned by participants; it also enables corporate action that has the potential to affect power relations in the wider society. Focus groups have the *potential* to play a role in empowerment, but do not necessarily do so.

Without an explicit theoretical foundation for the micro-dynamics of power in focus group discussions, recommended practice tends towards the reduction of power between participants and between participants and the researcher. This serves to manipulate participants' performances away from 'everyday forms of communication,' and censors an important aspect of social relations out of the data. For those researchers who aspire to discussions which enlighten and empower participants in ways that transcend the immediate discussion, there lacks a framework for assessing the extent to which this is achieved within the discussion. Bourdieu's construct of 'fields of power' provides a theoretical means of filling these gaps. I will briefly outline the study that forms the basis of this discussion, before demonstrating the potential of this approach.

### 4.3 The study

#### 4.3.1 Background

South Sudan seceded from Sudan on 9<sup>th</sup> July 2011, granting international recognition and legitimacy for the collectivity that had long been imagined by Southerners. The two Sudanese civil wars (1955-1972; 1983-2005) arose from “an acute crisis of national identity,” as the country’s multiple racial, ethnic, cultural and religious identities were implicated in “the shaping and sharing of power, wealth, services, development opportunities, and the overall enjoyment of the rights of citizenship” (Deng, 2006, pp. 155, 157). Although the stated goal of the Sudan People’s Liberation Movement/Army (SPLM/A) under John Garang De Mabior was revolutionary - the reform of the whole of Sudan as a single, united democratic state - the majority of the population continued to hope for separation (LeRiche and Arnold, 2012).

The referendum of January 2011 was internationally recognised as free and fair, had a turnout of 97.58% of registered voters, and led to overwhelming support for separation (98.83%), both within South Sudan and in diaspora communities that were enabled to vote at internationally dispersed polling stations (SSRC, 2011; The Carter Center, 2011).

Realising secession required significant unity of purpose among the Southern Sudanese, and at independence South Sudan inherited a “fledgling national identity” based on the “common struggle for recognition, dignity, and equal rights” (Awolich, 2015, pp. 1, 6). It was also clear that a process of nation-building was urgently needed to create a sense of shared identity and national unity in an inclusive state for the diverse population, and that South-South reconciliation would be part of this process (Jok, 2012; LeRiche and Arnold, 2012; Zambakari, 2013). Fears that with insufficient institutionalisation of a national identity, and in the absence of a common enemy, the young state could descend into civil war were realised on 15<sup>th</sup> December 2013 when fighting broke out between troops in Juba<sup>9</sup> (Jok, 2012; Awolich, 2015; Zambakari, 2015). The civil war that quickly escalated has been tragically characterised by the deliberate targeting of civilians and has forced the exodus of over a million refugees, while a further 1.6m have been internally displaced (HRW, 2014; UNMISS, 2015; UNHCR, 2016).



Figure 4.1 The symbols representing unity and separation on ballot cards.

<sup>9</sup> The capital city of South Sudan.

The new crisis of national identity that South Sudanese citizens now face is particularly problematic for groups geographically dislocated from the homeland that confers their political rights and who, as such, are subject to further discrimination and marginalisation (Zambakari, 2015). As long-distance nationalists, the South Sudanese diaspora view themselves as part of a transborder citizenry which shapes their ideological beliefs and related political practices. Their relationship to the current state is not purely ancestral, but is an active national identification that coexists with their long-term (or even permanent) residence in the UK (Schiller and Fouron, 2001). This study explored how a specific group of South Sudanese citizens – the diaspora in the UK – jointly articulate their national identity after five years of independence, and against the troubling historical-social-political backdrop I have briefly described.

### **4.3.2 Methodology**

The work of identity negotiation and sense-making in light of present conditions was something that I believed to be going on amongst the South Sudanese diaspora, but as a white British woman I did not have natural access to these conversations. Focus group discussions provided a means of inviting members of the diaspora to model their thinking-in-process for an external audience. Directly, this audience comprised a research assistant, who was also white British and who took notes to aid transcription, and myself; indirectly, it included the possible academic and non-academic audiences of my research, of whom participants were aware.

I facilitated two focus groups, one with men (4 members) and one with women (8 members), all of whom are adult South Sudanese citizens (including those with dual citizenship) and are usually resident in the UK. They were recruited through the networks of a cultural community association; therefore, while I set the sampling parameters, the selection and invitation to participate was initiated by gatekeepers. Nick Emmel and colleagues (2007) have observed how, where a gatekeeper is involved, the relationship between researcher and participant flows from the relationship between the gatekeeper and the participant. Power differentials between these parties may be exhibited through signs of control or possessiveness (Goode, 2000). Through inviting gatekeepers to be included as participants in the discussions, I was able to observe their interaction with other participants. This was characterised by deep respect for them as community leaders, although I saw no sign that this inhibited the free expression of others in the group. I was the sole facilitator to the discussions – the research assistants did not participate verbally – although at times, as I will examine below, participants spontaneously supported my facilitation efforts by asking questions of each other, praising and encouraging constructive contributions and chastising behaviour they perceived to be inappropriate to the setting. Discussions took place in either a community building familiar to participants, or one of the participants' homes, in June 2016

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– shortly before South Sudan would celebrate five years of independence. They were audio recorded and later transcribed, with some minor abridgement to protect the identity of participants. Both substantive themes and interactional traits and patterns were included in the analysis (Kidd and Parshall, 2000).

The South Sudanese diaspora is a relatively small and highly connected network in the UK. As a consequence, and particularly in light of the mode of recruitment, participants were embedded in existing relationships to each other. Where focus groups involve pre-existing groups, this can provide greater access to everyday conversations, tapping into group life that exists independently of the research (Kitzinger, 1994; Bloor *et al.*, 2001; Byrne and Doyle, 2004; Carey and Asbury, 2012). Nevertheless, the focus group context is innately artificial by virtue of the researcher-facilitator's initiation, presence, tasking, and prerogative to intervene, and is rightly conceived as “somewhere between a meeting and a conversation” (Agar and MacDonald, 1995, p. 80; Morgan, 1998; Caretta and Vacchelli, 2015). The use of English, under the advice of gatekeepers, as the language of discussions further distinguished them from everyday interactions within the community. This semi-informality was important to retain. Any research with conflict-affected communities has the potential to be highly sensitive – particularly on the controversial topic of identity, and in the context of ongoing relationships between participants (Chaitin, 2003; Zwi *et al.*, 2006; Chandra *et al.*, 2009; Cramer *et al.*, 2011; Mazurana *et al.*, 2013). It was therefore ethically important to impose some element of structure and parameters to the conversation (Anderson, 1999; Hofmeyer and Scott, 2007). There is a tension between this ethical obligation to contain the discussion and leaving space for power relations to be observed; I return to this in my discussion of facilitation, below.

The impetus behind the study came from a set of recent-historic photographic images that I had gathered (see *figure 4.2* for examples). These depicted visual-verbal displays of national identity, such as banners, posters, t-shirts and placards. These were statically displayed or animated at rallies in South Sudan during the three months prior to the referendum as part of the pro-secession campaign. The displays were produced and exhibited by South Sudanese groups and individuals; however, the photographs of them which I accessed were taken by journalists and bloggers, some South Sudanese but the majority international, for the purpose of accompanying textual reporting in online media. I collected images from online sources and supplemented them through direct contact with photographers in order to access their personal archives.

The digital preservation of contemporary images forms a haphazard archive that translocates images from the mode of their production and the purpose and temporal circumstances which occasioned them. In repurposing these images through an audiencing with South Sudanese diaspora in the UK, I was interested in how their meanings were renegotiated, and how “multiple readings emerge and collide” in the process of collective viewing (Lomax and Fink, 2010, para. 2.10; Rose, 2016). Previous research using photo elicitation with individuals or groups has found that photographic images can form an “additional presence” which enables participants to introduce sensitive or contentious topics, and to combine reminiscence with reflection on present experience (Byrne and Doyle, 2004; Croghan *et al.*, 2008, p. 355; Banks and Zeitlyn, 2015). The photographs were the ‘centrepiece’ of the table around which the discussion took place, and were passed around, referred to, and appropriated as a common language by participants, both spontaneously and through activities I instigated, throughout.



Figure 4.2 Displays of national identity in South Sudan

#### 4.4 Power and performance in focus group discussions: a Bourdieusian approach

In both focus group discussions, individual and communal power was contested as participants attempted to position themselves in response to both my questions and contributions by others, asserting, relinquishing and conferring influence, and problematizing the basis for their corporate

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empowerment or disempowerment. As I sought to account for these micro-dynamics, there were several theoretical approaches to power available to me.

A classic approach originating in early studies of group dynamics asserts six bases of social power, which is defined as “potential influence” (French and Raven, 1959; Raven, 1993). ‘Coercive power’ operates through the threat of punishment, while ‘reward power’ influences through the proverbial counterpart to ‘the stick’ – ‘the carrot’. ‘Legitimate power’ arises from the obligations others associate with someone’s recognised position, while ‘informational power’ is achieved by clear logic, argument or information. Relatedly, ‘expert power’ relies on the recognition of the influencer’s authoritative knowledge or judgement in relation to an issue, whether or not the grounds for agreement are explained. Finally, ‘referent power’ operates through a sense of identification with the influencer, who is a model by which others evaluate their behaviour and beliefs (Raven, 1993, pp. 232-233). The limitation of this approach and others that build on it, from the perspective of this study, is the excessive focus on the influencing agent, and the very directional exertion of that influence. Power is apparently vested in the individual influencer, and exercised in order to assert their interests. However, in the discussions I observed and facilitated, power was much more fluid, dynamic and negotiated than this account suggests. It seemed to circulate *between* participants, rather than being ‘owned’ by them individually, and was harnessed to achieve more subtle ends than solely to influence the behaviour and attitudes of others. I have found Bourdieu’s theory of fields of power provides a useful vocabulary for understanding the social practices that were in play in these discussions.

For Bourdieu, inequality is a fundamental aspect of how social space is organised, as:

“an ensemble of invisible relations, those very relations which constitute a space of positions external to each other and defined by their proximity to, neighbourhood with, or distance from each other, and also by their relative position, above or below or yet in between, in the middle.” (Bourdieu, 1989, p. 16)

He uses the notion of ‘field’ as his overarching metaphor to theorise how power structures the objective positions of agents (individuals, groups and institutions). Fields are multi-level, “nested” sites of struggle (Wacquant, 2013, p. 276) which operate semi-independently at four levels: the field of power, the general field under examination, the specific field, and the agents within that field constituting a field in themselves (Thomson, 2012). Insofar as an application of the focus group method assumes that participants form some kind of group that surpasses an aggregation of unconnected individuals (Hydén and Bülow, 2003), the criteria by which participants are selected projects the existence of fields at these levels. In this study, since the focus was national identity and belonging, participants are located in national-community fields at the macro level of the

general field – what is distinctive in this particular case is that they are members of two fields at this level: as South Sudanese citizens, and as citizens or residents of the UK. At the meso level of the specific field, the South Sudanese diaspora in the UK forms a semi-autonomous field with its own internal dynamics and admission criteria.

For Bourdieu, a field constitutes any social space where the conditions of a field are present. At the micro level, then, by virtue of being drawn from a shared field with a specific focus, a particular resource at stake (knowledge) and with relatively stable boundaries (set by who is invited and the limited time available), participants in a focus group discussion form a field in themselves. Field conditions may be stronger where, as in this case, participants have prior relationships, but are likely to be present to some degree even where this is not the case, since they share a central common experience that is theorised as a field in the context of the study. Although distinct from one another, Bourdieu suggested that fields are homologous, which he defined as bearing a “resemblance within a difference” (Bourdieu and Wacquant, 1992, p. 106): they share similarly patterned and predictable practices and similar kinds of agents dominate in each social field (Thomson, 2012). Attention, therefore, to the micropolitical, routinized exercise of power in group discussions has the potential to reveal global regularities of power within the population from which participants are purposively sampled. If these micro-dynamics of power are observed and allowed to play out, analysis may reveal how these dynamics are implicated in the topic under discussion (Blase, 1991; Morley, 1999). This is only possible if the groups are conducted in a way that allows sufficient space for power.

In this discussion, I have identified knowledge as the resource at stake in focus groups, and it is obvious (though rarely stated) that this resource is generated and accumulated through the use of language. The literature on linguistic power ranges from the very specific, micro-level analysis of the impact of language formations that are ‘powerful’ or ‘powerless’ (Holtgraves and Lasky, 1999), to macroscopic engagement with the political struggle for the right to ascribe meaning in public life (Lakoff, 2000). Although beyond the scope of my purpose here, I note the potential for a fruitful engagement of linguists in understanding the micro-dynamics of power in focus group discussion. I am also influenced in my analysis by some of their insights. Robin Lakoff (2000) has described the intensely political character of language – the question of who has the power to make language, and through it make meaning and define culture for a wider group, is highly contentious in contemporary society. Language both *creates* and *enhances* power relations (2000, p. 28). She observes that the process of making sense with language is a collaborative and indeterminate business. Meaning is not singularly and self-evidently produced by a sole speaker, but is co-constructed through dialogue. Focus group discussions provide a means of accessing this collaborative endeavour. However, focus groups also tend to feature a conspicuous outsider – the

researcher/facilitator – whose identity and position exclude them from a full understanding of the context shared by other participants. The paradoxical consequence of this uninvolvedness, for Lakoff, is that “the greater the objectivity, the greater the unreliability” (2000, p. 11). In my analysis, I have attempted to be open to the possibilities as well as humble in relation to the limitations of my anomalous presence in the discussion. Finally, I note that languages themselves are not politically neutral, but that certain languages and accents are normative and legitimate: linguistic and racial hierarchies are deeply intertwined, and in multi-lingual contexts, language can be deeply colonising (Motha, 2006). This was a particular concern in this study, as participants contributed in their third language. Some participants shared the same ethnic group with an associated first language, and corporately they would usually speak Sudanese Arabic. Although I was advised by gatekeepers to conduct the discussions in English, I was concerned by the symbolic and practical implications this imposition would have. However, as emerges in the analysis, I found interesting patterns of compliance and resistance to the status quo that I imposed in terms of language use, as well as occasions where participants co-opted either multi-linguistic skill or self-perceived lack of ability to further their purpose in the construction of knowledge or to offer mutual support and solidarity among members of the group.

In the following analysis, I use aspects of Bourdieu’s theory of fields to illustrate how power operated in these focus group discussions, and what implications this has for the guidance literature. Participants’ names have been changed to pseudonyms of their choosing.

### **4.4.1 Defending, contesting and relinquishing the rules of the game**

Bourdieu uses the analogy of a game to describe the operation of relations within a field. The specific logic of practice within a particular field is governed by rules or, better, ‘regularities’ which are embraced by players who, by the very act of playing, “accord ... that what happens in it matters, that its stakes are important ... and worth pursuing” (Bourdieu and Wacquant, 1992, p. 116). Agents may resist through attempts to change the “tacit rules” of the game (Bourdieu and Wacquant, 1992 p. 99). However, more frequently they will use strategies to transform the field and their position within it within the ‘rules of the game,’ although doing so inadvertently serves to reinforce existing power relations (Bourdieu, 1989; 1998 [1996] p. 40-41):

“social categories disadvantaged by the symbolic order ... cannot but recognize the legitimacy of the dominant classification in the very fact that their only chance of neutralizing those of its effects most contrary to their own interests lies in submitting to them in order to make use of them.” (Bourdieu, 1994, p. 160)



In the context of a focus group, for a discussion to be successful the researcher/facilitator hopes that participants will be “taken in by the game” of knowledge co-production and will “collude” in its construction (Bourdieu and Wacquant, 1992, p. 98). Some of the ‘rules’ that apply are inferred from the semi-informality of the setting – in this case, conventional practices familiar to meetings in the UK, such as speaking in turn, submitting to the chair’s direction and staying for the duration. Others are explicitly stated in the verbal invitation to participate, the written participant information sheet and the verbal briefing/introduction to the discussion. By these means, the researcher/facilitator makes clear their expectation that participants will engage in discussion with others about the imposed topic and that everyone will have equal chance to speak. An unintended consequence of consent processes is that participants ‘sign up’ to play according to the stated rules of the game. At different times, participants in the diaspora focus groups defended, contested and relinquished the ‘rules of the game’ in ways that had implications both for their power relations with each other, and their relationship with me as the researcher-facilitator and primary instigator of those ‘rules’.

Defending the ‘rules of the game’ included participants explicitly clarifying what was expected of them, challenging each other when another member deviated from responding to a question that had been asked or spoke in a language other than English, advocating on someone else’s behalf when they were interrupted so that they could finish what they were saying, or affirming my role as “chair,” encouraging others to respond to my interventions.

The follow excerpt took place about mid-way through the men’s discussion, and exemplifies both contestation and defence of the ‘rules of the game.’ Other participants had previously challenged Bel Pen about the length of his contributions, which they considered to be inequitable given the parameters of time. We were mid-way through an activity where each member of the group chose a photograph that particularly struck them. Bel Pen had chosen a picture but instead of describing it he had offered a reinterpretation of a personal account another participant had given which had sparked some debate. This exchange follows my intervention to return to the activity and ensure other members also had the opportunity to choose a picture:

Bel Pen: I haven’t finished. You want me to finish, or that is okay?

Rachel: Oh, yeah, if you want to say something [particularly about that image ...

Bel Pen: [Because I was never allowed to finish ... My contribution

Rachel: I’m sorry – it’s because we have a limited time, you know?

Bel Pen: (laughs)

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Paul: When you hear the record here (gestures to voice recorder) you are the one, [your voice is ...

Bel Pen: [It's not about the time you are talking, it is about how much can you give as a good contribution to this research ... for example. And if you cannot definitely when you talk more it means you have more information. And if you talk less it means you are satisfied with the few information that you give. So here ....

John: That's an interesting way of looking at it Bel Pen.

(laughter)

John: But carry on.

Rachel: We can maybe debate that one later – tell us about that picture.

Bel Pen: Really, these little girls here [Amos: Yeah; Rachel: Yeah] had the feeling like me that this means peace, it means there will be no bombing, no fighting, nothing at all [...] And she was right, and she was right. Whatever happened after that, whatever war you guys had done after that, that was not in, in, in her mind, completely. She was free, she was free like me, on that day.

Amos: Good point.

John: Mmhmmm.

At this and various points in the discussion, it seemed that Bel Pen was working to a different set of regulations which caused ongoing conflict within the group; it may be that he was drawing on community ground rules for performance in formal speech situations within South Sudan that collided with the focus group environment (Bauman and Sherzer, 1989) – although his peers did not acknowledge this. He valued speaking at length without interruption and often made comments on a range of issues on his way to reaching his main point. This circuitous style meant that other participants, who expected a more direct response, did not know he had not yet made his point. For Bel Pen, the capacity to make what he perceived to be a more informative contribution to the topic was of greater importance than allowing even contributions between participants, and so he contests their attempts to defend the 'rules of the game' as they saw them. However, he proceeds to complete the task of describing his chosen photograph, and this adherence to the 'rule' of answering questions is endorsed and encouraged by Amos and John, as well as myself.

Although silence is a normal feature of social interaction (Smithson, 2000), prolonged or deliberate silence can be a means of relinquishing the 'rules of the game' through asserting the power to not speak. In the women's discussion, one participant made only one contribution, which was in response to my direct request for her to describe the photograph she had chosen:

Achol: When the people talk a lot a lot a lot about Sudan (Liz: South Sudan) and the I ... about South Sudan, I'll keep quiet, because when war returned in my country between South Sudan and North, they make very bad from myself. They kill a lot my people, including my children, including, but I know people fighting and then somebody dies, when people fighting. Then when er, we got er, referendum, I'm very happy, I said, my family they're going to make like that (gesturing to photograph). They're very happy, the faces okay, we are dead, but our country comes back. I am very happy. When war returned again in each other (others murmur) in Sudan, it make my body all, what the, the word in ...

Oliba: Loose

Nyanpath: Like spiders

Achol: My body all worn like you tied them like that, hey! Why when people talk about Sudan I keep quiet (Liz: South Sudan), because it makes my body, my head, my bottom, my heart (gestures), because I am very happy when er, we got er, referendum I though we going, because the people died in Sudan not me. A lot people lose their people. A lot people. Any qabayla (Nyanpath: Any tribe) innit. But me, what, what, what.

Nyanpath: When you say "what" like that, her close relatives, were killed one day.

With the support of her peers, Achol eloquently describes the physical manifestation of her emotional suffering as a result of the post-2013 internal conflict in South Sudan in which her family members have been killed. Although she did go on to comment on the picture she had chosen, she maintained her silence for the remainder of the discussion. Silence can be a culture-bound and context-bound coping strategy which it is important to recognise, understand and respect (Tankink and Richters, 2007; Brun, 2013) – even if this means contributions between participants will be uneven.

If we treat the performance of power relations as a useful aspect of data, this poses interesting questions for the approach to facilitation. When participants contest or relinquish the stated or implied 'rules of the game', should the facilitator enforce them, make exceptions to them, adapt them, or abandon them? In order to allow space for power, the researcher/facilitator will generally prefer to be as unobtrusive as possible, which suggests a minimum of control. However, where

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participants have different understandings of the 'rules of the game' this can generate conflict. These 'rules' therefore have implications not only for the relationship between participants and the researcher/facilitator, but also for the relationships between participants, who may also be invested in compliance by everyone. The skill of facilitation, then, becomes to leave as much space for a variety of forms of interaction as possible, whilst maintaining ethical parameters. The difficult decision to intervene becomes necessary as a result of ethical concerns relating to the impact of the discussion on participants (Kitzinger, 1994; Wilkinson, 1998).

In searching for this balance, the examples described above yield some insights. Firstly, rather than dismissing some participants' behaviour as 'problematic,' 'dominant' or 'unruly,' it is more informative to attend to the different sources of 'rules' that participants follow and to acknowledge these as worthy of interest in their own right. Secondly, facilitators can be responsive to the group's self-moderation. This allows participants to determine what practices were acceptable and unacceptable. Participants are often better placed to determine which 'rules' are important and which can be relaxed, and the researcher/facilitator can observe this process of negotiation. Finally, as focus group practitioners we need to have a clear awareness of the parameters that we take for granted and those that we impose, implicitly and explicitly. By questioning these, it becomes possible to apply them flexibly, and to open as many 'rules' as possible to negotiation with participants.

### 4.4.2 Negotiating a game of shifting stakes

Fields are arenas of struggle for valued resources, or 'capitals,' the relative value of which differs between fields, and it is this process of valuing that differentiates between fields and between agents within fields (Grenfell and Hardy, 2007). Those forms of capital that are recognised as at stake in a particular field hold "ace card" status (Bourdieu, 1989, p. 17). These are transformed into symbolic capital, the monopoly of which confers symbolic power: the "legitimate mode of perception" that yields the capacity to name, to categorise, and to impose recognition, thus consolidating existing social arrangements and the patterns of dominance therein (Bourdieu, 1985, p. 730; 1989; Bourdieu and Thompson, 1991; Swartz, 1997; Bourdieu and Wacquant, 2003).

The notion of capitals appears to centralise the social agent as the nexus of power in a way that could be accused of substantialism and runs contrary to Foucauldian notions of power as dispersed and pervasive (Foucault, 1998). However, although individuals may have access to economic, cultural, social and other resources, it is the relational *valuing* of these resources that translates them into capitals. Capitals themselves are arbitrary; it is the social process by which they are

misrecognised as such which confers their efficacy (Bourdieu and Wacquant, 1992). Power can therefore be conceived as diffused in the regularities of valuing that govern social space, and its effect can be discerned in agents' trajectories rather than it being vested in them as such. Agents may, however, develop the *appearance* of 'having' power as they internalise a limited system of categories imposed by the history of the field in general and their own trajectory of accumulated experience within it (Bourdieu and Wacquant, 1992) – the “durable and transposable” system of dispositions which Bourdieu terms 'habitus' (Bourdieu, 1990 [1980], p. 53).

Within focus group discussions, the resource at stake is knowledge relating to the topic at hand. The right to speak, and the authority with which one's contributions should be weighted, are a space of contestation. Participants draw on other resources which confer, or diminish, either their own or others' authority to speak. This suggests (perhaps misleadingly, as I will show) an arena of competition between participants to achieve recognition of their knowledge claims both in each other's estimation and in the judgement of the researcher-facilitator, as the audience to the discussion. Due to the homologous relation between multi-level, nested fields that I have described, the resources that are mobilised within a focus group discussion are related to those that carry weight in the specific field and the general field. Conversely, participants occasionally make explicit statements about power hierarchies, and the resources that underpin them, in the general/specific fields. These shed light on the categories that influence participants' performances of power in focus group interaction. Attention to both overt statements and the bases on which participants valorise or downplay contributions within the discussion shed light on relationally valued capitals. These, in turn, influence social position both within the focus group and in the specific/general fields of substantive interest.

In the men's group, an explicit discussion arose around the position of the specific field – the South Sudanese diaspora – in the general field of South Sudanese society:

John: Do you think there was, in the run up to independence and after independence, there was a tension between the South Sudanese community who stayed in South Sudan during the war, those in the immediate diaspora, and those in the wider diaspora – Australia, Canada, US, UK – three constituencies, a tension there.

*(some general discussion around how people come to occupy political/administrative positions in South Sudan)*

Amos: What I want to say regarding this diaspora role [...] There is actually a pecking order in South Sudan. A complete class system has already developed. And ethnicity is a factor, but it's a small factor [...] The main factor really, is, is, the, the er hierarchy goes like this: at

the very very top are people who are in SPLA,<sup>10</sup> are soldiers, trained, and in combat, until 2005, until the peace. Who persisted, did not break away, did not, um, did not become disabled, they were there serving all. This, these are the people who are right at the top [...] So these are the people who are going to get the very first opportunities [...] The second one is anyone at all who's ever been in the SPLA. So for example where we see the clash now between Riek Machar and Kiir,<sup>11</sup> there is a point where they will become allies because they were ex-comrades [...] The third layer are those people who were inside South Sudan, what you call the liberated areas, during the war. Not like us who went to the diaspora. 'Cause they say, "You ran away, neither did you join the war front, nor did you stay under the bombs, when we were being shelled," or starved, or whatever [...] they consider themselves to be the next, these were the front, the internal front of the war [...] And then after that the East African diasporas will come, and then the last really are the Western er diaspora. These are the last people at the bottom. And then there's an ethnic dimension in there as well.

Following John's prompting, Amos outlines the basis for their shared marginalisation within the general field of South Sudanese society; aspects of his analysis were reaffirmed in both groups. He identifies the efficacy of certain resources in gaining recognition, influence and position in South Sudanese society. These relate to direct participation in the freedom struggle, physical presence within or proximity to the homeland, and participation in suffering.

Although his analysis placed focus group participants on a par in relation to wider South Sudanese society, within the discussion criteria emerging from this structure were constant reference points by which participants evaluated the authority of their contribution to the discussion. None of the participants had been soldiers within the SPLA; however, those who had spent a significant amount of time living in South Sudan referred to this as a basis for their superior knowledge. The claim "I was there" was difficult to argue against. Those who had spent relatively little of their adult life in South Sudan worked hard to demonstrate how physical absence did not prevent them from being deeply connected to the land and the people – for example, through regular visits, frequent contact with wide networks in their home community, use of Sudanese Arabic and through up-to-date knowledge about what is happening in the country through personal contacts and monitoring blogs,

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<sup>10</sup> The Sudan People's Liberation Army is the military wing of the political movement, the Sudan People's Liberation Movement, which led the fight for self-determination in the second Sudanese civil war 1983-2005.

<sup>11</sup> Two central figures in the SPLM/A: Riek Machar, the First Vice President of South Sudan and Salva Kiir Mayardit, the President of South Sudan. The post-2013 war in South Sudan was between troops loyal to these two parties.

national television and other news media. At different times, participants appealed to their connections with family and wider community networks in South Sudan:

John: I wasn't around in Kokora days, all I know is everything I hear, okay?

Similarly, Paul commented:

Paul: There are so many history that South Sudanese people don't know – it is unwritten. I tell what I also hear from others.

Knowledge gained through being embedded in South Sudanese networks, within South Sudan, was used to valorise contributions. By identification with their home communities, participants could share in the legitimised knowledge-base of those networks.

Similarly, the discussions were littered with references to suffering. These included everyday indignities resulting from living in the context of war such as being unable to access the outdoor toilet at night, as well as more extreme experiences such as being forced to flee the country, the death of close relatives and persecution of home communities that participants identified with. Participants acknowledged and responded respectfully to each other's claims to suffering. The belief that 'all have suffered,' including those in the Western diaspora, was a key aspect of the right to have a say in conversations such as these discussions that concern national issues. This inclusive assertion formed a refrain within the groups that ascribed value to their corporate claim to knowledge (possibly for my benefit), as well as implicitly including each other within this category of the knowledgeable and promoting each other's right to speak.

Participants in discussions referred to a number of other categories which conferred (or diminished) either their own or others' authority to speak. These included deference to age, valuing of education, and historical-political literacy. This exchange follows from a discussion of the likelihood of external intervention providing a successful solution to the conflict, in particular by ensuring there is justice for human rights violations:

Liz: This er, joint monitoring and evaluation [<sup>1</sup>commission – they are not doing it because (*Public figure A*) [<sup>2</sup>is threatening them.

Shola: [<sup>1</sup>Ah they are not doing anything

Cynthia: [<sup>2</sup>They are not doing anything

Shola: (*Public figure A*) is threatening them.

Liz: They are threatening them.

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Nyanpath: Eh!

Liz: They are doing nothing.

Shola: (*Public figure A*), (*Public figure B*), er ... and that, that other one.

Nyanpath: (*Public figure C*)

Lamba: Mama mama you are really clever, you know all of them! Me I don't know anybody!

Liz, Shola, Cynthia and Nyanpath share knowledge of current political initiatives and the barriers to their success, including the interventions of key political figures. When Lamba interjects, she singles out Nyanpath, an elder in the group, and after respectfully addressing her as such she praises her for her political knowledge and, through juxtaposition, devalues her own ability to contribute. It is clear that traditional aspects of culture, such as deference to age, respect for education and political acuity remain important. These, idealised in diaspora, have also been shaped through exposure to British culture. Nevertheless, the rapid emergence of South Sudan as a modern state since 2005 has created a state of flux. This uncertainty as to the 'stakes of the game' has opened opportunities for agents to creatively mobilise new categories, as demonstrated by Oliba. During an early monologue about how she was happy to be separated from the North due to the inequalities South Sudanese experienced, she makes this argument:

Oliba: But now, we are alone, we're still suffering, I don't know until when. Me myself I don't, because I am non-educated, I am non-politician, I'm talking as a primitive woman. But, without primitive, who can vote you to come as a President? Those people without an education is the one, they use it. And then brought them up.

Although Oliba classifies herself in relation to her lack of education and position, she turns this negative assessment on its head through identifying with the power of those lacking in education, through democratic processes, to elect the President, and conversely his dependence on their votes to be "brought up" to his position. The sense of agency and dignity that she expresses as a voting citizen directly challenges her self-appraisal as a "primitive woman" that would traditionally have marginalised her right to speak.

Comparative attention to the expressed bases of power in the general/specific field and the categories to which participants give credence in assessing the relevance of contributions is a useful analytic device. It enables insight into processes of valuing capitals that structure power relations in the field of the focus group, as well as in the specific and general fields which form the substantive interest of the study. This is only possible through engaging participants who are *different* in key



respects. However, as I have discussed, general guidance recommends that participants are 'homogenous,' in order to reduce any 'interference' by power differences that are expressed through 'group dynamics.' True homogeneity between participants, of course, never really exists (Kitzinger, 1994). In this study, for example, I held separate groups with men and women to accommodate gendered patterns of social interaction. I could have made further sub-divisions - for example by age, level of education, duration of residence outside South Sudan or to separate out the narratives of different ethnic communities. However, this would have reduced the potential for power to be observable. By allowing the heterogeneity that exists in the specific field to be reflected in the composition of groups, the resources that effectively conferred power within that field became evident in the process of interaction. This allowed participants the opportunity to negotiate over the relative value of these resources and discursively reconcile differences. From this orientation, a better principle of focus group sampling is *commonality*, rather than homogeneity. This shifts the priority away from eliminating power to creating a sub-field in the focus group discussion that captures some of the diversity of the wider field of interest from which participants are drawn. Indeed, through allowing heterogeneity within commonality, participants themselves can theorise about why diversity, expressed through dissent, exists, and how this affects them.

#### **4.4.3 Mobilising the boundaries of the field**

Fields are conceived as relatively stable, where the specific logic of the field and those capitals legitimised in it provide clear boundaries by which agents are recognised or excluded from participation. Nevertheless, Bourdieu suggests that there are relationships of exchange between fields, and the boundaries of the field themselves are sites of contestation (Bourdieu and Wacquant, 1992). In a focus group discussion, I have suggested that the sampling criteria postulate shared characteristics between participants which are commensurate with their positioning within a specific field, of which the focus group is a sub-field. Who is present and the time that is allotted make the boundaries of the space fairly stable; however, depending on their own positionality in relation to the research focus, the researcher-facilitator may be positioned within, at the margins of, or outside this field.

Within this discussion, my positionality was an anomaly as I was not part of the field from which participants were drawn – the South Sudanese diaspora in the UK. Therefore, although I clearly had a role in the sub-field of the focus group discussion, my position in it could not follow the same logic as participants' positions in relation to each other. Participants frequently made great efforts,

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without prompting, to include me through explaining cultural references, filling in historical background, and translating when others made use of Arabic (as Nyanpath did in the discussion with Achol quoted above). These useful interventions paradoxically both included me in their community of understanding, whilst highlighting my distinctness from it. They also emphasised our commonality as citizens/residents of the UK, with a shared understanding of the political freedoms and processes we enjoy. In an early monologue, for example, Bel Pen used our shared understanding of the rights participants and myself enjoy as UK citizens/residents to create humour as he relayed his discovery of these privileges when he arrived in the UK, implicitly drawing a contrast with the unfamiliarity of such freedoms for a South Sudanese citizen who has not experienced transnational migration:

Bel Pen: And then when I came here I found that there were like ... oh equal rights for blacks, and women, and different religions, they are all displayed here. And I asked myself, "Can I be free in this country? Can I be equal to anybody? Oh!" When I got the British passport, "Is my passport, this passport, the same that is the same passport the Queen has? Is this true?! I cannot believe it! The Queen has the same passport as I have! Oh! I am a human being! Oh! Oh! I cannot believe it!" You can never find this in, in, in, in, in the Sudan.

Participants were also aware that I had been to Juba, and generously assumed a certain amount of knowledge on my part – for example, while relaying how a demonstration in Wau<sup>12</sup> had been violently quelled Cynthia inserted, "I'm sure you heard about the incident that happened recently." Through these measures, participants actively diluted the boundaries of the field to include me within the sub-field of the discussion that they were constructing.

Nevertheless, participants were able to mobilise the boundaries of the field through using my outsider presence to strengthen a rhetorical purpose. This forms part of a response by Cynthia to my question about what connects members of the group personally to each other:

Cynthia: Aaahhhh, I have to start! (general laughter) Oh I have to start! I think it means identity. It means home. We don't go home often [...] So, this is to to me the South Sudanese community here is like, er, er, a glimpse of home. You know? And um they, I, I run to them for support, just to talk the language. And, and, and I don't have to make up myself. Like if I'm, I'm going to speak to a white lady for you for example, oh God, I have to push all these things in a cupboard in my head, so I do not have to express myself. But with them, I just feel at home. I meet one of them on the road, I meet Shola on the road, I, she helps me with my shopping bag, we have five minutes chatting, and then I feel better,

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<sup>12</sup> City in the north west of South Sudan.

I go home (Nyanpath: <click>) it de-stresses me, and as I said I just feel home, you know I, I, I feel I relate to these people, these are my people [...] But yeah, so South Sudanese here are my support, are my piece of home in the UK, and the people that I, I feel myself with, I don't have to make up another personality, I'm just Cynthia .... You understand? So, yeah, it's everything. It's what keeps me going. You haven't been in a foreign land, it's tough. Different culture. Different faith. Even all this freedom, sometimes too much freedom is, is (laughing) even scary (Liz: Is a problem), you understand?

Cynthia, with supportive interjections from Nyanpath and Liz, builds a compelling picture of shared identity within the diasporic community, which is enriched through the contrast between their experiences and my position as an indigenous British woman. She simultaneously identifies and nurtures the bonds that connect participants to each other, while highlighting my distance from the experiences that connect them. Through naming the racial-cultural and migratory boundaries of the field, Cynthia is able to persuasively elucidate the basis of their solidarity by drawing attention to my privilege outside of the conditions of self-censorship and marginalisation that they tolerate. Thus a flexible and shifting orientation to the boundaries of the field is a useful device which participants can enlist to perform ideological work.

Although with some communities and in relation to some topics it may be preferable for the focus group facilitator to be a *relative* in-group member, it is clear that the same person occupies multiple real and perceived positionalities. The fact of the facilitator's distinctive role in the group and the implications this has in terms of their credentials necessarily distinguish them from other participants. Where there is a particularly steep power gradient between participants and the facilitator, such as across majority/minority world difference, this can override the researcher/facilitator's attempts to reposition herself (Jakobsen, 2012). In this case, although I shared the experience of being a UK resident/citizen with participants, in relation to the specific field commonality that drove my sampling strategy, I was a clear outsider. As such, participants had the opportunity to cast me in a variety of roles: as a chair, a safe third party (Kelman, 2005), a witness, an ally, a conduit to a wider audience, and a student in need of instruction. Further, they mobilised the porous and dynamic boundaries of the field to include and exclude me at different points in the discussion. From a field perspective, 'insider' and 'outsider' are negotiable categories. Participants can use their indisputable position within the field to foreground aspects of the researcher/facilitator's identity that enable them to accomplish rhetorical tasks. This has implications both for their relations with each other, as it affirms the basis of their commonality, and for their relationship to the researcher/facilitator, which is more ambiguous and open to creative manipulation. The apparent status of the researcher/facilitator is not as important as the way that boundaries are creatively extended and contracted by participants, and to what end.

#### 4.4.4 Searching for collective empowerment

So far, I have focused on the potential of a Bourdieusian approach to reveal how power *differences between* participants, and *between* participants and the researcher/facilitator, manifest themselves in focus group discussions. However, focus group discussions are not only useful for observing individualised performances of power. Power, or the lack of it, is also experienced collectively (Stewart, 2002). This may occasionally emerge in explicit discussions, as Amos' narrative about the position of the Western diaspora in relation to the rest of South Sudanese society indicates. However, can the processes of interaction in focus group discussions tell us any more about collective power? If so, this would be further evidence of the utility of focus groups to provide insight into power relations. Moreover, are there any indications that participation in focus group discussions can itself bring about a change in horizontal power relations, through empowering participants?

Although Bourdieu has been criticised for lacking “a politics of collective mobilisation” (Swartz, 1997, p. 136), his notions of doxa and the field of opinion provide a useful way in to examining processes of conscientization (Freire, 1970) which can form the basis for empowerment. Doxa, for Bourdieu, represents a state where the hierarchy of power in the field is experienced by agents within it as inevitable, commonsensical and beyond question:

“the established cosmological and political order is perceived not as arbitrary, i.e., as one possible order among others, but as a self-evident and natural order which goes without saying and therefore goes unquestioned, the agents' aspirations have the same limits as the objective conditions of which they are the product.” (Bourdieu, 1994, p. 161)

The conditions of doxa enable the free exercise of symbolic violence by dominant groups – that is, oppressed groups are complicit in the violence that they are subjected to through the construction of their mind “according to cognitive structures that are issued out of the very structures of the world” (Bourdieu and Wacquant, 2003, p. 272). Under such circumstances, the emergence of the field of opinion reveals and negatively constitutes doxa through confronting it with competing discourses: “the critique which brings the undiscussed into discussion, the unformulated into formulation” (Bourdieu, 1994, p. 164). When the realm of doxa is infiltrated by opinion, discourse between orthodoxy and heterodoxy emerges. For Bourdieu, ‘orthodoxy’ refers to the “straightened opinion” that constitutes defensive “conscious systematization and express rationalization” of the status quo. It is the language of doxa, while heterodoxy – which

acknowledges competing possibilities – is the language of the field of opinion (Bourdieu, 1994, p. 165).

Within focus group discussions, attention to the orthodoxies and heterodoxies that participants discursively construct is indicative of the extent to which conditions of doxa serve to suppress members of the specific field within the general field. It was significant that while in the discussions some narratives were clearly aimed at me, during these aspects of the discussion participants seemed, in my perception, much more focused on each other. Amongst the South Sudanese diaspora, orthodoxies emerged that indicated their sense of their own powerlessness in the wider South Sudanese citizenry. In addition to the class structure in Amos' narrative, participants spoke of the unravelling of unity within the diaspora that had taken place since December 2013, and shared the view that they lacked a voice in the political sphere. Lamba, for example, picked up on the imagery of two clasped hands on the referendum ballot papers (see *figure 1*) to signify unity, and lamented, "We before were hands together, but now ...". Similarly, Oliba explained:

Oliba: Even here, we are not the same like before. If I have my occasion sometimes Liz cannot come, Shola cannot come, Cynthia cannot come, Achol, cannot come, Nyanpath can come. But before, we are not like that, I feel sorry. Here in this country, now the division is too much. I cannot say no. Me myself I am opening. I cannot say we are, the same like before, no! Because many things happen here, it's not like always. But me myself I feel sorry for myself.

Focus groups can provide a context for the 'taken for granted' to be expressed and collectively affirmed. However, it can also invite the establishment of a field of opinion – a space in which discourses that compete with the established order can emerge. Towards the end of the discussions I asked the groups to imagine that they had been invited to send representatives to a peace rally in Juba, and to decide what they would put on their banner to express what makes them South Sudanese. As they worked through this process, the following debate emerged among the women:

Aba: That one there we don't have problem. We don't have problem because those people there they don't listen to us, what we are saying.

Cynthia: No but now we are pretending that they have paid for us.<sup>13</sup>

Shola: Let's pretend that they listen to us (two others: Aye), and they invited us.

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<sup>13</sup> i.e. The government has paid travel and hotel expenses to enable the group to be represented at the imagined rally.

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Aba's view that the diaspora lacked voice in political affairs represented the mainstream of the discussions. However, Cynthia and Shola galvanised the others to persist with the exercise. This group work enabled imaginative engagement with a world where the orthodox view that they were not listened to was not true. They jointly went on to rehearse the messages they would promote in this space. In situations where a strong dominant narrative exists, the act of speaking can be a highly politicised action. The acknowledgement that there is a range of possible ways in which the social world could be arranged is a *precursor* to affecting change. Participants in this study indicated that the process of engaging in discussion can be as productive as what is said. The following excerpt took place at the end of the men's discussion:

Bel Pen: It is not for anything other than for a national issue, I think we should remember that day that we were four South Sudanese and we had some discussion ...

Amos: We spoke frankly.

Bel Pen: We spoke frankly.

*(brief exchange about whether any communities were missing from the discussion)*

Bel Pen: This war in South Sudan that killed many thousands and displaced many thousands. If it was handled like our discussion at this table, who do we kill now? We kill no innocent person, isn't it?

*(digression around some issues earlier in the discussion)*

Bel Pen: So what I'm trying to say is, the SPLM party, i.e. the Chairman President Salva Kiir, and the Vice Chairman Dr Riek Machar, if they could just have actually sat down like us, which has not been an easy day for that discussion, to find out who we are and whether we love ourselves or not, you know? I think we came down to some agreement that you know we are one people and one country, you know? The discussion was so good, you know? So I think, if we carry this to somewhere else, let us just behave the same.

As they reflected on the authenticity with which they had approached the discussion, Bel Pen and Amos were able to envisage the productive transfer of this style of interaction to other spheres. Through engaging in a process of discursively attempting to understand their present positioning within the general field of South Sudanese society, the diaspora groups opened up a field of opinion in which a change in their shared sense of powerlessness was plausible. This represents the seed of a process that, if prolonged and incremental, could in the long term have tangible repercussions (Kelman, 2001).

Focus group discussions draw attention to the commonality between participants, and invite consideration of its implications. They encourage expression of shared beliefs that are usually taken for granted about that grouping – but they also create space for reflection and questioning of those orthodoxies. Through attention to the expression of orthodoxy and heterodoxy in focus groups, it is possible to observe collective aspects of power within the specific field from which participants are drawn. Focus groups invite the setting up of a field of opinion, which creates space for focus groups to perform a pedagogical function. What of the empowering potential of focus groups – their ‘political face’ (Kamberelis and Dimitriadis, 2013)? Due to the homologous relationship between multiple, nested fields, what happens in the focus group discussions *may* have repercussions outside of that discussion: in the first instance, in the specific field from which participants are drawn, and potentially further. This is by no means guaranteed. It depends on the will and readiness of the population and the identification and removal of barriers. This may be undertaken spontaneously by the community themselves, for which the focus group can take little credit; in the context of research, is only likely to take place through prolonged engagement in a deliberate programme rooted in an emancipatory research paradigm (Humphries *et al.*, 2000). To assess the extent to which a group is empowered would involve examination of processes outside of and subsequently to the research. This is beyond the scope of the approach taken here, although it is an important area for further research in its own right.

## 4.5 Conclusion

Power is an inevitable facet of social life and the fields of power from which focus group participants are purposively drawn. It is therefore not only inevitable that power will be manifest in focus group discussions – it is desirable. I have argued that power is undertheorised in focus group methodology, and undervalued as a rationale for selecting the method. There lacks a conceptual framework to enable the analysis of power in focus group data, and the guidelines on the conduct of focus groups treat power as a procedural problem to be minimised and mitigated. This serves to conceal an important aspect of social relations, and distorts the interaction that the researcher/facilitator seeks to observe.

Nevertheless, approaches that take seriously the importance of interaction in focus group discussions lay crucial groundwork for making the most of focus groups’ capacity for revealing power. Pierre Bourdieu’s notion of fields of power provides a flexible ‘thinking tool’ to open up this opportunity. The homologous relationship between multiple, nested fields suggests that the micro-dynamics of power that are performed in focus group discussions are revealing of those that pertain

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in the wider population from which participants are drawn. To the extent that the field theorised through sampling characteristics is meaningful, field mechanisms are likely to be present in focus group discussions. Therefore, mechanisms such as the 'rules of the game', the valuing of capitals, the boundaries of the field and the expressions of doxa or the field of opinion provide useful analytical tools to understand the dynamics of power within the focus group. During the course of focus group discussions, participants lay claim to power, surrender it, confer it on others, negotiate its bases, harness its dynamics to strengthen a rhetorical purpose, and reinforce or question its corporate experience. The way that these processes play out is not only important for understanding the kind of data that focus groups produce: it provides valuable insights into the relational significance of the substantive topic that is the *focus* of discussions.

Earlier in this paper I identified the power relationship *between participants* and the relationship *between participants and the researcher/facilitator* as two separate considerations in focus group literature that considers the implications of power. Although I have attempted to distinguish these relationships analytically, it is clear that in practice they are deeply interrelated. In relation to the 'rules of the game', participants were as likely to enforce the perceived 'rules' among each other as they were to resist my attempts to maintain them, and I relaxed some 'rules' where I perceived they were contrary to participants' expectations. Although the resources, or capitals, that I noticed participants mobilising to strengthen their own or others' claims were drawn from within the cultural and experiential frameworks of their field (as opposed to second-guessing what different resources I might find compelling), I was an audience to this process and participants performed with an eye to my recognition, as well as each other's. The creative manipulation of the boundaries of the field had implications both for participants' relationships with each other and for my position in relation to them. Only when grappling with expressions of orthodoxy and heterodoxy did the balance of participants' attention noticeably shift to each other. Therefore, while it is useful to distinguish between the types of relationship at work in focus group discussions, it is important not to overstate these distinctions or to neglect the interaction between them. Although the researcher/facilitator may not be included in the specific field from which participants are sampled, they are part of the focus group discussion, albeit with a specific and distinctive role. Even this is not concrete – at times the researcher may share a personal experience, or a participant may make facilitative interventions. I have suggested that the focus group discussion operates as a field in itself, within which everyone present is included. An openness to the variety of ways in which power may be negotiated will enable a more nuanced analysis to emerge.

An approach which centralises power requires some rethinking of the standard guidance that usually applies to the conduct of focus group discussions. Received wisdom on approaches to facilitation, sampling, the position of the researcher/facilitator and activity outside of the



discussions themselves is geared towards producing content rather than co-privileging interaction. The observation of power requires sufficient space for its performance by participants in a social situation that is semi-informal – both structured and familiar. This can be achieved through flexible facilitation that enables negotiation of the ‘rules’ that researchers impose; through recruitment based on commonality; through attention to the multiple, flexible positionalities of facilitators and how these are mobilised; and through consideration of the wider political implications of the discussion. Such measures shift the conduct of focus groups away from control towards the managed cultivation of field conditions. There are very few areas of social scientific study where the social implications of power are not of interest. The reorientation of focus group methods towards allowing space for the micro-dynamics of power to be observed is the next key area for literature concerning both the conduct and analysis of focus groups to explore.

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## Chapter 5: Discussion

### 5.1 Future directions for reflexive methodological pluralism

My arguments in this thesis have extended from a central endorsement of Pierre Bourdieu's call for methodological pluralism in sociology: the use of any and every method which is relevant and practically usable considering the object of research and practical conditions (Bourdieu and Wacquant, 1992). This is an active concern for British sociology, as it appears that there are key deficiencies in the methods and data forms that are the basis of our collective output. I have also described the circumstances in my own academic trajectory which have provoked me to engage reflexively with questions of method. These have led me to challenge my own assumptions as to what methods or data 'count', and the practical implications of methods when they are applied in practice. The papers in this thesis are the outcome of this critical exploration of methodological pluralism.

At face value, methodological pluralism concerns a sociological corpus that draws on a wide and balanced range of methods. This appears to be the main interpretation within British sociology at present. These are important health indicators of a discipline; programmes to understand deficiencies in quantitative skills/output or to explore the possibilities new forms of data present are and will continue to be core concerns. Nevertheless, through positioning methodological pluralism within the practice of reflexive sociology, a far broader interpretation is possible which, I suggest, opens up many fruitful lines of methodological enquiry.

I have argued that reflexive methodological pluralism is shaped by the productive tension between a highly permissive and eclectic epistemology, mediated by methodological reflexivity, and the constraints of ethical commitment, in particular as this relates to the relationship between the researcher and the researched. These forces are fault lines that run through each of the papers that comprise this thesis, which I will reflect on in this concluding chapter. As I do so, I take the idea of a physical landscape as an overarching metaphor for the terrain of methods. A landscape is made up of a variety of materials which are shaped into landforms by geological processes which construct and erode, sometimes gradually, sometimes suddenly. Although it may take a particular shape at a snapshot in time, it is a dynamic sculpture which responds to climatic and environmental conditions, as methods respond to the social and historical circumstances in which they are applied. A landmass may be anything from a small, rugged island emerging from the ocean to an expansive sub-continent. Similarly, although I have taken sociology as my focus here, methodological pluralism can be evident at a variety of levels: within the practice of an individual researcher; within

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a research group, programme or substantive field; in a discipline, nationally or internationally; or in social sciences as a whole.

The constructive processes that expand the methodological terrain of sociology are the consequences of epistemological liberty, which allows for a repertoire of methods as wide-ranging as the strategies of social agents themselves (Bourdieu, 1999 [1993]), and for 'arrays' of data to be available for selection (Elliot *et al.*, 2013). In the history of the social survey narrated in the first paper, it became clear that this expansion is non-linear: it can involve seismic shifts, such as I identified in the publication of Booth's study and in the information needs precipitated by World War II. These two events generated a sudden acceleration in the uptake of survey methods both in the UK and the US. It is arguable that the rapid growth of big data in the late twentieth and early twenty-first centuries represents a similar shift. More usually, however, expansion takes place through gradual processes of deposition which cumulatively form sedimentary strata of methodological learning. This requires a sufficient institutional apparatus for deposits to be retained and assimilated. The second paper of this thesis has shown the benefits of interrogating the apparently natural barriers that shape our thinking about method. In this case, I have queried the compartmentalisation of certain methods within specific actions of research, while excluding them from others, although disciplinary barriers are also relevant here. A more expansive view of method is possible through collapsing these barriers, and enabling methods to be 'carried by the tides' from one discipline to another (what Rose Wiles and colleagues term an 'adoptive innovation'; see Wiles *et al.*, 2011), or from one aspect of research practice (such as data collection) to another (such as conceptual or research design work). Specifically, in the second paper I have shown how, through disrupting the distinction between heuristic methods (for researchers) and data collection methods (for participants), the availability of creative, visual and multi-modal methods vastly expands the types of thinking tools available to sociologists. The third paper considers how recognising new facets of what familiar methods reveal can expand the scope of their application. An approach to conducting and analysing focus group discussions that makes space for the micro-dynamics of power to be observed has the potential to indicate the application of this method to new uses – a kind of metamorphosis that diversifies further the methodological terrain.

If any method *can* be used, that does not mean to say that it *should* be. There are further processes that shape, refine, and, potentially, remove certain methods from a particular landscape. Methodological reflexivity mediates the application of any method and requires unremitting scrutiny of those methods even as they are put into practice. This process can be both generative and erosive. In the development of the social survey method, active attention to the implications of research decisions enabled practices such as direct questioning and sampling to emerge,



alongside a growing appreciation of the survey's useful role in governance. More recently, however, concerns have been raised that pathologies of institutionalisation, professionalisation and economisation have eroded the attitude of relentless self-questioning of method. Methodological history helps to overcome this risk, through the reflexive activity of turning method on itself – using one method to examine another – which ensures continued critical awareness. The second paper considers the particular affordances of creative, visual, or multi-modal methods as means to refine the construction of the sociological object. The iterative opening up and narrowing of possibilities involved in creative processes works alongside the unique questions raised by the distinctive qualities of the visual and the generation of deliberative time required for artistic production. While in the case of operationalising trust, I found these properties to be deeply constructive, it is at least conceivable that under different conditions (for example, in the case of better defined or more obviously measurable phenomena), their impact may be negligible. The third paper makes a detailed examination of what focus groups are good for – what they can tell us about the relationships between participants and the extent to which micro-dynamics within the group are of substantive interest in themselves. This has implications for how focus group discussions can be used; in particular, it cautions that simply 'extracting content' without any consideration of how it is co-produced may be a misguided objective. In sum, methodological reflexivity takes the eclectic range of methods that could be applied to *some* purpose, and selects and shapes those that are suited to a *particular* purpose.

The contours of methodological pluralism are defined by further attrition from ethical commitments. Sociological research has the potential to, either actively or passively, condone, reproduce or create symbolic violence. When we use the shorthand 'do no harm,' this is usually an aspect of what we mean. This could involve the direct exploitation or misrepresentation of individuals or groups, or the reinforcement of structures of domination in wider society. This is evident in the shifting representation of the poor through poverty studies from the nineteenth century social explorers onwards. Further, recent history has shown that, in societies saturated with research, there is a need to be aware of the accumulating pressure on potential participants, and to take greater care in which questions are asked, and how, to avoid undue imposition (Crothers and Platt, 2010; Savage, 2010; Couper, 2013). The issue of what constitutes data is also an ethical concern. This arises in the second paper, where I argue that the lack of any real-world referent disqualifies the storyboards as data. This becomes important when the thought processes, concerns and decisions of people with particular social experiences – such as mental health service users, or women in countries affected by the HIV/AIDS pandemic – are presented in a certain light. If such speculative scenarios were misread as data, they could serve to replicate and reinforce stereotypes or crowd out empirically-grounded counter-narratives. The third paper identifies an

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ethical tension between allowing space for the micro-dynamics of power to be observed, without condoning attempts to marginalise or oppress; although I reiterate that power is conferred on others as well and claimed, and can serve emancipatory functions. These considerations have implications for both sampling and facilitation choices. Finding the ethical parameters of a focus group discussion is a contextual and negotiated activity, which I argue can be undertaken interactively with participants. The point of symbolic violence is that it is not recognised as such: focus groups can provide spaces for dominant orthodoxies to be questioned.

Symbolic violence is, of course, relational. Reflexive sociology is about the relationship between the researcher and the researched – by which I mean both the object of research, and the population that it is relevant to. This is a central aspect of the ethical commitment that weathers the methodological terrain. The first paper draws attention to the increasing distance between the researcher and the population that they research through the professionalisation of the survey method. There is no clear equivalent to Charles Booth's ethnographic observations, or Peter Townsend's fieldworkers' paradata, to contextualise or enable interpretation of numerical data (Abrams, 1951; Edwards *et al.*, 2016). The second questions the hierarchical separation between researchers and research participants that is implied by the distinctive methods of knowing that are ascribed to each. An aspect of disrupting this imbalance is for researchers to make themselves more vulnerable – for example, by taking on a more playful mode of theorising (Stanley, 1984; Grady, 2006). The third paper also engages with bases of commonality and difference between participants in focus group discussions, and between participants and the researcher. Positionality has implications for power dynamics, and the relational processes involved need rigorous attention. Nevertheless, differences are not only inevitable, but also open up opportunities to be harnessed to include and exclude others, with significant rhetorical effect.

I have suggested that research methods are socially situated and respond to the social, cultural and historical conditions in which they surface. They are dynamic, being shaped, refined, eroded, and expanded by realisation of eclectic epistemology, methodological reflexivity, and ethical rigor – particularly with regards to the relation between researcher and researched. These themes, drawn from Pierre Bourdieu's description of reflexive sociology, have formed the basis for an expanded view of what constitutes methodological pluralism in sociology. Bourdieu has provided useful thinking tools for achieving this goal, in particular through his positioning of methodological pluralism as an aspect and consequence of the researcher's wider reflexive task. Nevertheless, although he is distinctive in bringing these ideas together, the content of his methodological writing is not as distinctive as he claims. Indeed, as I have shown in the introduction to this thesis, his ideas benefit from dialogue with other schools, such as feminism and critical realism, which also have valuable contributions to make to the themes of reflexivity and methodological pluralism. His

failure to engage with these significant bodies of work that were his contemporaries and, in some cases, his predecessors, is a significant weakness in his work. Fortunately, this is something that those of us who draw on his ideas can attempt to remedy by maintaining wider theoretical horizons. A particular weakness of Bourdieu's notion of reflexive sociology is that it assumes a fairly individualistic notion of sociological production. Although Bourdieu himself formed a research group and frequently published with others, his ideas seem to suggest *individual* introspection and analysis, rather than providing any acknowledgement of the collective character of academic research, or guidance on the application of his methodology within an epistemic community. Addressing the reflexive practice of methodological pluralism intersubjectively is a key need that arises, given the contemporary conditions of production in British sociology.

In the introduction, I described how, in the 1990s, a 'technical' version of the qualitative/quantitative distinction emerged which attempted to dispense with the oppositional portrayal of these approaches that characterised the paradigm wars, downgrading the distinction from a philosophical/ideological commitment to one of modes that could be adapted or combined depending on the requirements of a particular research task. This went some distance towards enabling a rapprochement between qualitative and quantitative methods, aided by increased acceptance of epistemological pluralism, the growth of interest in realism, the increasing popularity of Bourdieu's social thought, and the drive for innovation and 'newness' – as well, perhaps, as the openness to quantification that found a place in feminism. I have described this as a 'truce,' rather than a resolution: the division between qualitative and quantitative persists, both within and outside of feminism (Hughes and Cohen, 2010). Although it may be demonstrated that the opposition between qualitative and quantitative doesn't hold at a technical, or practical level, as Ann Oakley has observed, the fundamental dispute is ideological, between differently gendered ways of knowing (Oakley uses 'gendered' here partly as a wider metaphor for the powerful and the powerless) (Oakley, 2000). While the open conflict may have subsided, there is certainly ongoing isolationism between spaces where qualitative and quantitative methods are practiced, and the practitioners that adhere to them. The self-identification "I'm a quants person," or "I'm a qual person," is still a common shorthand academic researchers use to describe their position in the academic field, even among professed methodological pluralists. Perhaps for practical administrative reasons, or perhaps due to the skills sets of the teaching generations of researchers, qualitative and quantitative methods are usually taught to undergraduates and postgraduates in separate modules, by different staff, who are often attached to different disciplinary departments. These everyday occurrences in academic life subtly reinforce the cultural separation of methods along qualitative and quantitative lines.

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For those who actively resist positioning according to this dichotomy, there are further challenges that make space for methodological pluralism or for the broader practice of reflexivity problematic. The cultural and practical conditions of knowledge production in contemporary academia are heavily constraining for researchers in this environment. The commodification of higher education is serving to increase the sway of economic over cultural capital in universities, which has numerous repercussions for staff: fewer support staff means a greater administrative burden, higher student numbers increase teaching workloads, and less secure working conditions foster generalised anxiety. The individualised performance culture nurtured by the Research and Teaching Excellence Frameworks makes the mantra “publish or perish” take on a more ominous tone. These features of contemporary academic life are both stressful and time-consuming, allowing little opportunity for reflective thought and diversification of one’s methodological repertoire. Against this backdrop, Bourdieu’s description of the “leisure” of the academic gaze suffers from a certain irony. I noted in the introduction that Bourdieu objected to excessive specialisation and empirical fragmentation, as well as the “mutilating scissures” of disciplines (Wacquant, 1989; Bourdieu and Wacquant, 1992, pp. 26-27). However, both the structure of academic career trajectories and technological advances in research methodologies make extreme specialisation almost inevitable. The practicalities and necessity of publication encourage the ‘safe’ route of researching and writing in closely related areas. Within my own institution, I observe among my peers in the multi-disciplinary Economic and Social Research Council Doctoral Training Partnership that the majority of those using quantitative methods to any great extent are positioned in Social Statistics or Demography departments, although I would argue that there is a significant sociological component to their research. These features of academic life favour repetition of the same and encourage the cultivation of methodological monisms, while leaving little room for critical reflection, dialogue, or experimentation.

Against this apparently rather bleak social, historical and institutional backdrop, what does this thesis offer to researchers seeking to cultivate a reflexive methodological pluralism within their own work, within their research groups, and within their disciplines? At the micro level of the individual researcher, I have found it a useful tool to undertake an initial reflective process of constructing an intellectual autobiography. This aids the researcher in coming to an understanding of her methodological commitments, their technical, philosophical, ideological or pragmatic origins, and how this influences research decisions. This is not necessarily a one-off exercise: the demands imposed and inclinations aroused by different research situations may make the researcher alive to new aspects of her dispositions that influence what is thinkable or knowable in that new context. Relatedly, the researcher can examine how her position in the disciplinary and general academic fields shape limit the world of possibilities available to her. These are not necessarily solitary

exercises: horizontal dialogue among peers and vertical exemplars from researchers in different power positions within the field can enable dialogue and inform the practice of position-analysis. At the meso- and macro- levels of research groups and disciplines, this is likely to require continued reflexive analysis of those fields by practitioners positioned differently within them: a task which much of the literature I have drawn on in my earlier discussion of British sociology has contributed to.

There are a number of other approaches that the papers in this thesis point to, which may be useful to researchers with both qualitative and quantitative leanings. By taking a historical view and making research methods an object of inquiry, it is possible to account for the particular social and cultural conditions that influenced the development of methods along certain trajectories. This enables the critical consideration of the relevance of those methods, in the range of forms they were practiced historically, to contemporary research concerns. Researchers can cultivate an awareness of the barriers and assumptions that we operate within about how 'the research process' is structured and the tools and techniques that are 'appropriate' for different activities. By taking an approach that breaks with 'normal practice' we can disrupt our thinking and enable creative progress. Extending this further, there are numerous aspects of academic practice that are outside of 'the research process' as it is normally conceived. Where, for example, research governance structures, the organisation of degree course material, publication procedures or funding practices may inadvertently favour certain kinds of methods over others, or serve to reinforce methodological siloes, reflective dialogue by those in positions of influence may create space for experimentation with how academic life works, with wide-reaching consequences. In the fourth chapter I showed how the re-evaluation of common research methods from a fresh perspective can assist researchers and research groups to 'make the familiar strange' (Mills, 1959) methodologically. This may both expand the potential scope of application for a method, while also enriching understanding of the implications of a method, so that methodological or ethical parameters can also be reviewed in a given application. Finally, reflection by individual researchers, research groups and indeed disciplines on the relationship between the researcher and the researched, and the implications of our interventions in wider society more broadly, are an essential aspect of reflexive methodological practice. This may come more naturally in the application of autoethnography, for example, compared to secondary analysis of survey data. There is significant scope for dialogue among practitioners of many methods, but perhaps particularly in the case of quantitative research, about how the relationship between the researcher, the (conceptual) object of their research and the (embodied) population they observe it in works, what implications this has in terms of power and in the production of knowledge, and how can this distance be mitigated.

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These are corporate endeavours: reflexivity is best practiced intersubjectively, and this is the only way that reflexivity can be accountable, or assessed. At a disciplinary level, this involves creating space in national conversations to engage with reflexive methodological pluralism, in sociological journals, sector magazines, or online platforms. A culture of continual questioning and open dialogue about research *as it is practiced*, not just as it is written about, can be fostered in research groups (Stanley and Wise, 1993). Individual researchers can expose their thinking and practice to the scrutiny of students, peers or mentors in order to avoid replicating what they know. The more interdisciplinary these spaces for dialogue are, the more challenging, and interesting, the consequences are likely to be for sociological practice.

There remains significant scope for a programme of research which builds upon the papers presented here by providing new explorations of eclectic epistemology, methodological reflexivity, and ethical constraint, and how they interact with each other to produce distinctive methodological outcomes. Nevertheless, this thesis has begun to show how these forces can interact to challenge assumptions about how we know the social world. I have argued for methodological pluralism in sociology from the conviction that different methods are able to bring us alive to different facets of social life. As a result, if the methodological repertoire of sociology is limited by sectarian dismissal of any method or group of methods, by historical blindness or failure to understand the implications of methods in present social conditions, or by limiting the scope of application of methods through assumptions about their uses, this impoverishes the ability of sociology to fulfil its critical task: to provide an account of the social world, which may inform attempts to change it for the better. The kind of reflexive practice I have outlined here provides a set of tools that enable sociology, and sociologists, to become aware of their biases, and to harness these in order to know the world better. To repurpose Rogers Brubaker's observation, "to alter the principles of sociological vision of the social world is to alter that world itself" (unpublished paper, cited in McCall, 1992, p. 859). By expanding the definition of methodological pluralism through positioning it as an aspect of reflexive sociology, methodological pluralism can go beyond those aspects that are traditionally conceived of as pertaining to method, and begin to extend to every action of research.

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