Doing Gender in a Hospital Setting: Reflections of a Male Researcher

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Abstract

Very little attention has been afforded to how male researchers actively position their gender in their studies, particularly in ethnographic research located within settings populated largely by women. In this article, I reflect on my own gender work during an ethnography of prenatal clinics and how this was articulated with other aspects of my researcher self. By reporting on the successes and failures of this performance, I argue that my gender constituted an essential element in the everyday negotiations between myself and female participants. In so doing, I suggest that reflexive commentaries of how researchers perform gender should not be viewed as a form of egotistic self-indulgence. Rather, they should be read as valuable statements for rendering the researcher visible and, here, for revealing how issues of *doing* gender play out during fieldwork.

Keywords

In this article, I reflect on my experiences as a male researcher carrying out an ethnography in a setting occupied largely by women (prenatal clinics). Critical reflections and detailed confessions are a fairly common trait of the ethnographic craft; researchers have been rendered visible as a presence affecting all aspects of research, including constructing its narrative (Scott *et al.* 2012). Such accounts contain a wealth of information about – among other things – gaining access, collecting data, relationships, and exiting the field. Some of these arguments address gender reflexivity, a topic which has received renewed interest in sociology (but has a longer history in anthropology) – particularly from female academics researching primarily 'male' sites. For example, Poulton (2012) discusses her experiences of researching the hyper-masculine subculture of football hooliganism within the UK. She criticises the 'gender blindness' of male researchers doing similar studies who fail to consider the positionings, practices, and performances of the gendered self (2012, para. 1.1). In her study, Poulton describes

feeling obligated to prove she had 'balls' (para 7.2) to negotiate difficult situations and emotions. In describing her gender as a 'useful tool' (para 4.9), Poulton suggests that researchers should consider their gender more critically and work harder to disclose the complexities and messiness of qualitative work over offering purely sanitised accounts of methodological processes and practices.

Despite such incitements, very few male researchers have written about their own research experiences. Limited to 'bar-room confessionals' or amusing anecdotes, such accounts - according to Back (1993: 215) - point to a need for a 'more sensitive appreciation of the politics of research' and responding positively to feminist critiques of methodological practice. There may be reasons for this absence other than Poulton's charge of gender blindness. Do some men not see such reflections as a priority or as a determinant in attaining access and maintaining research relationships (McKeganey and Bloor 1991)? Do they find it too difficult to write from a position of reflexivity? Do some men perceive that they cannot access 'women's worlds' and, so, avoid this line of study? Is it just that male researchers studying scenes with a high proportion of women are relatively rare?

There is a problem here, of course, with categorising *all* male researchers as a homogenous collective, thereby discounting or muzzling considerations of other personal attributes such as class, age, ethnicity, and faith. That said, if understood as a unified group, there are few comprehensive reflections of the research process from male researchers – and even fewer from those in research settings populated largely by women[1]. Indeed, despite the widespread recognition of the crucial role of gender dynamics in social life, male sociologists have persistently snubbed how these dynamics affect their own work. In this article, I am not in the business of establishing why these accounts are absent. Instead, agreeing with Wade (1993: 200) that fieldwork is 'inevitably highly gendered', I unpack some personal reflections from my study, with my own gender identity under the microscope. In so doing, I am guided by Coffey's (1999) appeal to reflect on the relationship between the self and ethnographic fieldwork, together with the personal, emotional, and identity-work in such endeavours. For Coffey (1991: 1), many research methods texts 'remain relatively silent on the ways in which fieldwork affects us, and we affect the field'; we must, therefore, consider how the researcher self is a 'gendered, embodied, sexualised, and emotional being, in and of the research' (1999: 12).

I begin by contextualising my claims in the existing literature on gender in the field. After outlining the research background, I discuss how I negotiated embodied distinctions (gender in this case) during research – both successfully and unsuccessfully – and managed the inevitable emotions and affective labour that the study provoked. In so doing, I follow a long history of literature which identifies how gender is performed – how it is accomplished in mundane, taken-for-granted routines, rather than being something which simply *is* (Garfinkel 1967; West and Zimmermann 1987). Gender does not simply exist, but it is the doing of gender which brings it into being; it is 'a kind of a doing, an incessant activity performed, in part, without one's knowing and without one's willing' (Butler 2004: 1). This does not translate to the performance of gender being mechanical or automatic. Rather,

it is a 'practice of improvisation' which is malleable and negotiated with others but which, in turn, is performed 'within a scene of constraint' and is effortlessly transmitted across time, causing essentialist conceptualisations of what gender is or is not (2004: 1). The article is also located in the broad claims of masculinity studies, particularly with respect to the notion of plural masculinities, namely, the many complex and dynamic forms of masculinity that are (re)negotiated on a daily basis (Connell 1995).

I conclude by suggesting that reflexive commentaries on doing gender in research should be seen as a key component of the ethnographic craft since it reveals information about ethics, access, relationships, and 'rapport', a clichéd and under-theorised expression that, here at least, I define as a quality of relationship encouraging a participant to speak and behave as freely and frankly as possible. This level of reflexivity should not be confused with self-disclosure. Whilst self-disclosure concerns confusions and rueful accounts constructed through acts of contrition, reflexivity involves analysing the researcher's own thinking and how this becomes the product of complex social, political, cultural, and moral relations (Desmond 2007).

Gender and Qualitative Research

Gender is present in all human action and products, including research projects. As a fluid category negotiated from moment to moment, performed differently in various situations, gender should be taken seriously in research (particularly concerning power), that is, as a concept and accomplishment. Gender is performed, regulated, contested, and transformed – and with real consequences in research. Critical reflections around gender and fieldwork emerged in the 1980s and 1990s (Bell *et al.* 1993; Golde 1986; Warren 1988; Whitehead and Conaway 1986), coinciding with growing feminist thought which urged researchers to move away from purely practical accounts of methodology – commonly grounded within taken-for-granted sexist ideologies and practices (e.g. a male gaze) – and towards challenging researchers to recognise the political and subjective nature of research. This feminist critique of androcentric social science, showing how ideologies of gender can structure relations in research, gathered pace and challenged implicit power disparities identified between male researchers and female participants. It took the postmodern proclamation of a 'crisis of representation', in turn, to identify the need for a scrutiny of objectivity and ethnographic authority (Bell 1993). This work captured how there is no neutral ground from which scholars investigate men or women; researchers, thus, cannot be entirely detached from gender during the research process (Delamont and Atkinson 2008).

Since this period, the role of gender in research encounters has received greater attention, particularly in edited collections (e.g. Delamont and Atkinson 2008; Ward 2016) and with respect to how female researchers, specifically, reflect on issues of stereotypical gender beliefs and establishing relationships, gaining/preserving access, and ensuring safety (Bell 1993; Belur 2010; Bucerius 2013; Brown 2001; Enguix 2014; Jewkes 2011; Holmgren 2011; Horn 1997; Lumsden 2009; Mazzei and

O'Brien 2009; Oakley 2016; Poulton 2012; Sharp and Kremer 2006; Woodward 2008; Zubair *et al.* 2012). In a study of female midwives, for instance, Rayment (2011) reflects that being a woman helped her to integrate so thoroughly that it regularly became difficult to maintain an 'outsider' gaze with which to analyse the crucial role of gender in her study.

In a study of young black men in Philadelphia, Alice Goffman (2014) claims that whilst she occasionally experienced sexual advances from participants, others were highly protective of her. She adds that she regularly assumed the role as sidekick, chronicler, or adopted sister, yet did not always interpret her embodied difference as a young, White, and middle-class woman as an asset. At times, her identity became an 'encumbrance' and people were initially angry or threatened by her presence (race possibly played a stronger role than gender here), with Goffman handling this scene alteration by making a joke of her difference (2014: 231). She also acknowledged that gender came in and out of focus depending on what the situation but handled difficulties by becoming as small a presence as possible. Similarly, in their study of skinheads in Russia (a highly masculine culture), Pilkington et al. (2010) - a group of female researchers - had their femininity discussed excessively, particularly relating to issues of access (one participant claimed a male researcher would have 'got more') and personal information, including age and family/marital status. Yet in a similar study of Russian punk (another highly masculine subculture) by predominantly male researchers, their gendered attributes were not cited or asked to be accounted for (Gololobov *et al.* 2014). This can arguably be interpreted as one example of gender blindness (Poulton 2012), highlighting the need for male ethnographers to reflect more on how they 'do' gender in the field.

For now, I focus on the reflections of male researchers with respect to gender in the field. This topic has been occasionally explored with respect to *men studying men*, with the likes of Ugelvik (2014) reflecting on his ethnography of prison life, Thurnell-Read (2011, 2016) analysing his own age and gender in different studies around masculinity, and Mahoney (2007), Ryan (2006), and Walby (2010) discussing their research with men on same-sex relations. Two recent edited collections also explore similar topics: Pini and Pease (2013) on masculinity and men (with female contributors too) and Ward (2016) on gender identity and research relationships, with all four male contributors also narrating their experiences of research with male participants (Thurnell-Read; Parker; Morris; Stahl). Yet there are few accounts of male researchers discussing interactions with female participants, a few exceptions being Diamond's (1992) study in a nursing care home, McKeganey's observational project in a psychiatric therapeutic community (McKeganey and Bloor 1991), Ortiz's (2004, 2005) study of professional athletes' wives, Kolb's (2014) ethnography of workers in shelters for victims of domestic violence, Takeda's (2013) interviews with Japanese women, and Walsh's (2006) research on maternity services.

Prior to this, the focus of male researchers working with female participants concern feeling 'mesmerised' by women (Skipper and McCaghy 1972) and, particularly in anthropological accounts[2],

sexual relationships with them (Abramson 1993; Turnball 1986; Wade 1993). Yet other scholars move away from sexualised accounts (as I do in this article too), including reporting on issues of access and problematic research relationships. Walsh (2006), for example, was told to identify in documents (e.g. consent forms) that he was a male midwife doing research since participants would expect him to be female. Similarly, Takeda (2013) describes how his position as a Japanese unmarried male generated unforeseen complications within the interview dialogue as well as general interactions with Japanese women. He outlines how gender sensitivities precluded discussions of intimacy in marriage, how his unmarried status hindered his ability to locate possible participants, and how this also raised concerns about his intentions – particularly from male partners of the women (2013: 294). In his research with adolescents in a south London adolescent community, Back (1993) suggested that when he spoke to some young women, he became open to accusations from the young men as making sexual advances towards the women. For Back, this put female participants in a vulnerable position. As such, he made a decision to meet up with these young women away from male peers with the intention of making them feel more comfortable and avoiding similar accusations in the future. The potential challenges of cross-gender studies are also explored by McKeganey (McKeganey and Bloor 1991) who describes how his gender dictated access to social spaces in a psychiatric therapeutic community. For instance, he was not allowed to observe female residents in dormitories – but he was allowed to observe men in such areas (both could be observed in [public] common areas, including the kitchen and lounge).

However, not all men have trouble researching settings in which a large number of women are present. In his ethnography of workers in shelters for victims of domestic violence, Kolb (2014: 142) says that 'being a man made [his] research easier'. Whilst women were initially suspicious of him and asked lots of questions, he passed a series of litmus tests and was handed a 'progressive medal badge', both as a researcher and a volunteer (2014: 146). Indeed, he describes how men (him included) regularly received more praise and esteem, compared to female colleagues, since they were viewed as working in a foreign territory and as exhibiting attributes – such as care and compassion – that were seen as unnatural for them, but as natural and expected for women.

This small but significant body of literature, taken together, stresses how the identity-work of the male researcher is a vital component of the research process. In what follows, I describe how I interpreted and translated my gender in various micro-practices throughout my own research in the gendered site of prenatal clinics.

An Ethnographic Background

My study took place in two prenatal clinics – an NHS hospital and a privately-funded institution – from November 2011 to November 2012. In order to secure access to the NHS hospital, I had to apply for NHS ethical approval (access to the privately-funded institution was secured by applying to (details removed)). This involved spending six months meeting healthcare professionals and related

others to seek support and solicit guidance on the study, which became vital to fleshing out the finer details of my ethical approval application. The formal support of two gatekeepers – one manager and one consultant – certainly lessened the prospect of the ethics application being rejected. Interestingly, my gender did not explicitly emerge as a concern prior to or during the application process. Fieldwork began almost immediately after receiving formal permission. For further details on the study and the methodology, see (details removed).

My study was primarily focused on Down's syndrome screening and the everyday practices and interactions of professionals involved in this process. I completed sixteen interviews with health professionals and over two-hundred hours of observations of patient-professional consultations, offices, meetings, seminars, and many other sites/encounters. The vast majority of professionals (mostly midwives and sonographers) in both clinics were women, White, British, and between 35 and 60 years old. It is my interactions with the female staff members that I focus on for this article. Before this, it is worth offering a biographical note for some context. I am a White male who, at the beginning of fieldwork, was twenty-three years old. I am of average height for a UK male with short hair, a slim build, and a reasonably neutral British accent. I self-identify as middle-class, as educated, and as only embodying a few traits of what is typically associated with 'hegemonic masculinity' (Connell 1995). As for my demeanour which is, of course, negotiable at different moments, I am a researcher who self-identifies as relatively confident, open, and cheerful. This should act as a starting point for readers to understand who/what participants encountered on me entering the field, and how – and in what ways – gender was negotiated thereafter. It is worth noting here that I believe my gender performance and emotional work did not alter depending on the specific method (i.e. observations or interviews). Rather, the claims below, in my view, apply to most moments within the research process.

Doing Gender in the Prenatal Clinic

I was worried prior to fieldwork that my gender role may present a major problem to gaining access and collecting data. In researching prenatal clinics, a highly gendered space in which pregnant women and female staff members dwell, I felt my gender could possibly be a 'liability' (Ortiz 2004: 266). Such worries were mostly unfounded; my ethnography was carried out without major concern, at least with respect to access, relationship-building, and collecting data. Although a small number of professionals had initial concerns, typically said jokingly, that I was evaluating their work (what one midwife called 'spying on us'), I found them to be unobtrusive and engaging on a professional and personal level; they were sincere, direct, and eager to participate in the study, granting relatively unbridled access to their working worlds.

I believe one reason for this, among others, was my gendered identity work. Rather than my gender being viewed as a possible obstruction, it could sometimes become an advantage. In my initial stages of fieldwork, for instance, my presence as a male in the prenatal clinics was viewed, by some,

as a welcome novelty. I was routinely asked if I was a medical student and my standard response – of being a sociology PhD student – led to assertions that my appearance, as a male researcher, was a rare but welcome presence. One has to be careful, however, of taking such claims at face value. It may well be that the professionals were uncomfortable with my presence but expressing this would appear rude or inappropriate. My understanding is that this was not the case and I was not viewed, in turn, as out of place. That said, it is possible that my own male gaze blinded me to their anxieties, irrespective of whether these anxieties related to my gender or not (more on this later).

As fieldwork progressed, and patterns and personal relations solidified, my gender became a topic of discussion on several occasions. At times, this concerned why I – as a male researcher – was interested in prenatal medicine and, specifically, screening for Down's syndrome. Such requests were never interpreted by me as challenges or specific opportunities to justify my presence or agenda. Only once did I feel this when, at an academic conference, I was asked by a colleague 'why are you doing research in a woman's world?'[3]. This exchange, in what I felt was a challenge to justify my pursuits, led me to further consider the importance of my gender in the field (though this was clear to me prior to fieldwork) and, arguably, was the catalyst for writing this article. Although I was asked similar questions by professionals, they appeared to solely be expressing a genuine interest in my personal background and research more generally.

My performance of gender during fieldwork mostly involved evading a sense of hegemonic masculinity and managing an alternative, more 'muted' masculinity (Ortiz 2005). This contrasts with other research which shows how male researchers, when working with male participants, interpret embodying a sense of 'maleness' as a research imperative to help build relationships and to be seen as belonging in that setting (Parker 2016; Stahl 2016; Ugelvik), though some convey their discomfort at being privy to misogynist and related discriminatory discourse, thereby raising issues of masculine collusion. In my study, muting my masculinity, as an important frontstage process (Goffman 1959), involved avoiding qualities associated with being overly 'masculine' – including aggression, arrogance, and conversational dominance (Ortiz 2005) – which professionals clearly despised both inside the clinic (e.g. some male doctor colleagues) and outside the clinic. In both observations and interviews, I engaged in face-work and emotion management to appear sympathetic, collegial, and attentive to professionals. To some extent, elements of my performance were part of an 'authentic' self; my speech/accent is reasonably soft, I did not usually dress in an overly formal manner during fieldwork (I typically wore a jumper with chinos, albeit with an ID badge to verify my presence), and my conduct (e.g. mannerisms, demeanour, face work) in the field did not depart significantly from that outside of it. It seems, then, that my rather straightforward access and data collection processes are likely not totally attributable to my efforts of muting masculinity (Ortiz 2005).

Part of this masculinity work is not limited to self-presentation but also involves emotion work. I recognise my own affective labour, rather than being seen as a threat (losing objectivity), as

it frequently has, as a key component of the ethnographic craft, especially as ethnography confronts a researcher with a setting and its occupants for an extended period of time. On some occasions, together with feeling exhausted and 'reluctant' (Scott *et al.* 2012: 715) to revisit research sites, I was concurrently prone to feelings of doubt and self-consciousness, incompetence, imposterdom, being out of place, situational shyness (Scott *et al.* 2012), stress, boredom, confusion, and frustration. At times, I really enjoyed fieldwork. At others, I dreaded it. This worry did not relate to participants but a range of feelings and emotions, such as fatigue and anxiety around beginning my study, collecting data, upholding relationships inside and outside of the study, leaving the field, releasing findings, and credibility more generally – of me and my academic pursuits.

Moreover, I occasionally found fieldwork to be emotionally draining, specifically in situations in which expectant parents were told there was a 'problem' with their pregnancy (e.g. a diagnosis of foetal abnormality). In such moments, I became overwhelmed and troubled by the enormity of some decisions of both parents and professionals. Whilst my own experiences cannot be compared to the expectant parents receiving devastating news or professionals conveying this, I occasionally felt I had stumbled into intensely personal moments, exiting the scene on such occasions after interpreting my presence as inappropriate. The project presented several difficult, intense, and emotional moments which provoked various forms of affective labour. Fieldwork was, and is, a personal and emotional investment traced through feelings and bodies. Indeed, fieldwork encounters involve an intersection of sensory, embodied, and emotive aspects which help to generate knowledge (Thurnell-Read 2011). Since we grapple with the feelings and experiences of ourselves and of participants, there is a need to attend to the subjectivities of research, as valued and unavoidable elements commonly hidden or overlooked. As Hammersley and Atkinson (2007: 151) claim: 'feelings of personal conflict, anxiety, surprise, shock, or revulsion are of analytical significance' and various forms of embodiment in the field shape, enable, and restrict our sociological engagements and knowledge production. The place of emotions in research merit attention since they are embodied and involve judgements (Bondi 2005; Coffey 1999). But we must also be careful to acknowledge the dangers of such investments (such as exhaustion and managing one's own emotions) and to not veer into individualistic and narcissistic descriptions of our studies (Doucet and Mauthner 2012).

However, we have still heard too little on the embodied experiences of fieldwork from male researchers. For the most part, I hid or, at least, attempted to rectify such concerns and emotions once in the field. I worried that exposing some feelings, such as frustration and weariness, may have led to me being viewed by participants as incompetent, rude, uninterested, or – at worst – ungrateful for their kind invitation to observe their worlds. Here, I deployed 'emotional labour' (Hochschild 1983), referring to how we monitor our impression management in public. Hochschild (1983: 188) refers to this emotion management as 'surface acting', as being *part of the show*. Like Hochschild's (1983: 7) flight attendants, I sometimes concealed my 'true' feelings via smiles and explicit signals of

enthusiasm, with these sometimes being *on* me, not *of* me. Such emotions were mostly fleeting and resolved in a short space of time, yet their emergence was important for my own identity-work.

Yet with respect to my own gender work, I tried not conceal emotions *too much*. Disguising my 'true' emotions in their entirety, and over a protracted period, would likely have been interpreted as strange and suspicious. As part of my alternative (i.e. muted) masculinity, thus, I openly conveyed some of my more 'adverse' emotions. One example involved an observation of a feticide (the act of causing foetal death before termination). This consultation disturbed many people for several days after the procedure, including myself. Conveying my own genuine upset and dejection pertaining to such moments to the professionals was, I believe, intimately bound up with my own gendered identity work and muted masculinity. It is possible that in expressing 'real' emotions, rather than concealing them in an attempt to enact competence as a more detached researcher, played a role in talking with professionals and, in turn, collecting data in their workplace. Equally, I was able to empathise with, and understand, professionals' personal dismays and aggravations. I witnessed the difficulties of their labour first-hand and whilst I was not able to provide a concrete solution, being a person who was a supportive, concerned, and attentive listener (Ortiz 2005) seemed to help establish trust, rapport, and meaningful relationships.

Confronting the Male Self

My gender work was, I believe, mostly invisible. But there were occasions in which my gender was thrust into the limelight in more explicit, but largely unproblematic, ways. Here, my identity was both self-performed and ascribed but, on the whole, my male identity was figured *from the outside*. Although I was often involved in the process of crafting a male identity, I was not always in control of it (Back 1993). More specifically, there was mostly two ways that I was talked about by several professionals in the setting. Firstly, I was described as a family member. My gender and age led to me being ascribed, and later assuming, the role of what one professional called an 'adoptive son'. In one clinic, midwives referred to colleagues as a large family. Long working hours, tough conditions, close physical and emotional proximity, and cohesive experiences meant many professionals grew close to one another. In this site, I was also identified as a 'part of the family'.

In her cross-gender research of male boxers, Woodward (2008) assumed a 'maternal' role, with her nonthreatening status reducing tensions around sexualities and arguably eliciting responses on boxers' fears rather than them reiterating a sense of machismo perhaps more likely with a male researcher. Similarly, in Simpson's (2004: 357) study of men based in 'female dominated occupations' (librarians, nurses, primary school teachers, and airline stewards), the older female colleagues of young men took on a protective 'mother' role, with such men 'looked after' compared to other women in this setting. This correlates with other work on men based in occupations traditionally undertaken by women. For example, Wingfield (2009: 5) argues many men working in such professions experience

a 'glass escalator' effect, in which subtle interactions, norms, and expectations push men upward and outward into higher-status, higher-paying, and more 'masculine' positions over female colleagues (but this does not happen for certain men in culturally feminised fields, such as Black male nurses).

Whilst the likes of Wingfield (2009) and Simpson (2004) describe how men in occupations populated mostly by women (even if warmly welcomed) produces a hierarchy between both parties (namely because men are often assumed as more skilled, as holding more authority, and as associated with a more careerist attitude to work), my experience was comparatively unproblematic. In addition, although Lupton (2000) and Simpson (2004) describe how males face a range of challenges to their sense of 'masculinity' when entering such occupations, I did not feel this pressure to adopt strategies to re-establish, or reconcile, a preconceived notion of masculine identity[4].

Instead, in my study, I was interpreted as a 'family member' which helped uphold meaningful research relationships. This manifested itself in general interactions but also, more regularly, during conversations with staff members about my current marital status and future employment plans. At the beginning of fieldwork, for instance, I discussed my PhD with a midwife who jokingly warned me I should 'keep away from girls - they're a distraction!' Such interactions were located in configuring me as a 'family member' susceptible to (parental) advice and forms of protection. This was a role, then, both ascribed (by others) and performed (by the self in conjunction with others).

Second, I was sometimes configured in more jovial terms. For instance, I was occasionally described by some professionals as a 'toy-boy' (UK slang for a young man being kept as a lover by an older person, usually a woman) based solely on my status as a young male. This label resulted in teasing comments or crude jokes being made at my expense, with conversations appearing to be good-humoured and designed to embarrass me. Comedy played a key role in the prenatal clinic, such as the use of dark humour to, as one professional explained, 'cope with things' in a complicated and emotionally-taxing environment. In interacting with me, professionals' humour could occasionally be sexualised and innuendo-laden. To me, its function was nothing more than playful talk and an attempt to build relationships and trust (Parker 2016; Stahl 2016).

Thurnell-Read (2011) similarly discusses how participating in the cultures and conducts of participants (drinking alcohol and bantering as part of pre-marital stag tour groups, in his case) was an important facilitator for group membership, both for participants and himself. In my study, I acknowledged and engaging with such (sexualised) joking. Doing so also occasionally involved de-muting and reconstructing my masculine identity to align with this more sexualised framing, albeit in a jovial and frivolous way. This differs from Beusch (2007) who, in a study of male users of a Nazi fetish website, carried out interviews which became sexualised by participants, but in a more 'serious' and explicit way[5]. Indeed, I interpreted the professionals' ascriptions of my sexual positionality as an exercise in playfulness rather than anything more authentic or sordid. In his study of a nursing home, the discomfiture felt by Diamond (1992) when helping female residents out of the shower was

mitigated by residents telling jokes and making sexual remarks and innuendo, interpreted by Diamond as an effort to reduce embarrassment through humour. Similarly, in my study, by being positioned as a young heterosexual male and a target of gentle ridicule, I sustained productive research relationships and unwittingly seemed to obey Goffman's (1989: 128) advice to be 'willing to be a horse's ass'.

Another moment in which my male status was brought to the fore, and again in a relatively trouble-free manner, was when professionals talked about male partners or prospective partners. In one exchange, a professional had received a sexually-explicit message from an unidentified male via a dating 'app' on her cellular phone. After reading this message aloud to colleagues, which was greeted with a great deal of laughter, the professional asked me 'why do men send messages like that?' Here, and in similar situations, I was seen as embodying the role of 'male expert' (Ortiz 2005: 265). Ascribed an authoritative position to provide insight on the hypothetical highly-masculine Other (this is what I believe the professional meant when referring to 'men' who send sexually-explicit messages), I was seen as someone who could embody *this* form of masculine identity (albeit perhaps temporarily) and who had the essential grammar to clarify and shed light on behaviours. As such, my own gender work involved, in such moments, explaining traits of the masculine Other to account for certain conduct, but I did try to distance myself from this position through my own gendered performance. Although this framing could be tricky since it put me on the spot, such instances were mostly beneficial in that they fuelled conversations and helped uphold research relationships with women in the clinic.

Performing 'Male': Age, Gatekeepers, and Other Factors

At this point, and as identified to varying degrees throughout the article, I recognise that being male was not the only quality to impact upon securing access and developing constructive relationships. It is misleading to talk about gender in a vacuum, namely, as something not intersecting with other traits including age, ethnicity, sexuality, faith, and personal and professional relationships. Gender is not a thing in itself; it is articulated with other aspects of a researcher's self. Indeed, this study was helped owing to other attributes and developments. First, the research was championed by two respected and leading figures of authority in the prenatal clinics. As architects of trust and linchpins of cohorts, they influenced how others perceived the study and enhanced the credibility of both the study and of me, especially since I was an 'outside' researcher. In my study, my gatekeepers rarely had to vouch for my presence yet my affiliated status was enough to limit questions regarding my presence and give me the authority to be there. I felt, at times, that their support was fuelled not so much by their perception of the study but, rather, by their perception of me. For example, my presence would be justified with reference to my personal attributes (e.g. perception of friendliness and enthusiasm, 'fit', etc.) as opposed to the project itself (perception of validity, credibility, etc.). In the clinics, my success as a researcher was dependent on – among other things – my ability to present a personal (i.e. a more 'attached' role), and not always 'professional' (i.e. a more 'detached' role), self.

Second, using 'props' (Goffman 1959: 32) – such as an ID card and smart clothes (but not *too* smart) – legitimised my presence. Third, I made active efforts to keep out of the way, fill downtime with conversation and engage in chitchat, ask professionals about their lives outside the hospital, and be helpful (e.g. fetching drinks, switching off the lights prior to ultrasound scans, disposing of waste). Such efforts helped maintain relationships and gain trust. Being able to offer something, however small and mundane, is valuable currency when seeking observational rights.

Fourth, my status as a relatively young student was valuable. Gurney (1985) suggests that in her study, her gender, youthful appearance, and student status created an impression of naivety and of being non-threatening, thus easing her participants' initial anxieties concerning the presence of an observer. Likewise, in his study of ale drinkers and beer connoisseurs, Thurnell-Read (2016) claims that the large age gap between him (as a young male researcher) and his older participants meant his relative youth appeared to lend a leitmotif of paternalism to the relationships he established. In my research, being viewed as a young student arguably afforded me a leniency and juvenile status which legitimised my presence – or it could be that I was vulnerable to problems of mistaken identity as a medical student, which may have led to carrying out the research fairly uninhibitedly. Interestingly, several professionals advised referring to me to patients as a 'student', rather than a 'researcher', as it was felt that the latter was, in the words of one professional, 'too formal' and 'sounding too much like you're scrutinising the actions of [professionals and expectant parents]'. This may have also facilitated access by reinforcing this rookie status.

Finally, my role as an 'outsider' (I use this term very loosely) helped the research process. As a confidant, I had few alliances and was often a sounding board where professionals aired grievances and discontents possibly withheld from both colleagues and intimate others. This could have altered the field, in that providing a forum to vent may have prevented frustrations from emerging elsewhere. Nonetheless, being an outlet for listening to their gripes and grumbles was a valuable strategy for developing relationships with professionals and, subsequently, maintaining access for collecting data.

In this study, my gender identity was performed in conjunction with many other elements, particularly age and social relationships (e.g. gatekeepers). It is clear that this identity-work – ever-changing and regulating the extent to which we become active participants in the social worlds we observe – held value for data collection and developing social relationships. However, being a male researcher also presented various challenges. I reflect on such difficulties below.

'Bless Him': The Challenges as a Male Researcher in Prenatal Clinics

The difficulties of doing ethnographic research in various settings have been well-documented. Such endeavours involve flexibility, patience, and investment of time and energy; the researcher must be prepared to 'cut [themselves] to the bone' (Goffman 1989: 127). Alongside such practical difficulties are limitations relating to being a male researcher in a site largely populated by women. In my study,

being male posed two recognisable problems. First, certain medical procedures presented an obstacle. Doing a trans-vaginal ultrasound scan, to examine a woman's reproductive organs (uterus, ovaries, cervix), was one instance where observations were problematic. My appearance as a (non-clinical) male meant my presence was perceived as inappropriate. There were occasions, for instance, in which I was present in an ultrasound scan but when it was announced that the transducer (usually applied via the stomach) would be applied trans-vaginally owing to poor image quality, I was asked to leave the room. There was an expectation, and one that I completely accepted and respected, that I would not be present for such 'invasive' procedures. Because the male gaze is, arguably, commonly seen as a potentially perverted one, my male presence was disrupting a private interaction between a female patient and professional. Interestingly, when I observed consultations that did not require a medical incursion of the female bodily interior, this was sometimes prefaced by professionals saying it was 'non-invasive', as if to verify my presence as proper. This may explain why collecting data was largely straightforward. Most of the study involved observing consultations defined by professionals as non-invasive and 'routine', meaning I was unlikely to be interpreted as being out of place in such situations.

My male identity was also potentially problematic in that participants *may* have changed their conduct owing to my gender, thereby possibly threatening the data collection process. In two separate studies with young men at University, Sallee and Harris (2011) suggest that their responses changed depending on whether they were interviewed by a male or female researcher, thereby showing how cross-gender interactions can alter the research process. The following fieldnotes detail the aftermath of an ultrasound in which a pregnant woman was accompanied by two female friends:

> After the scan, [sonographer] and I leave the room. [Nurse] asks how it went. [Sonographer] laughs, saying the three women's behaviour '*changed*' when I initially entered the room. I asked how:
> [Sonographer]: He didn't even notice, bless him. They all changed the way they were behaving and how they were. They made themselves more proper and ladylike by changing their posture and stuff. It was funny!

In his ethnography of male firefighters, Desmond (2007) felt that some of his participants may have sometimes altered their behaviour; his dislike for homophobic and sexist comments meant a number of crew members did not always relax in front of him and perhaps changed gestures accordingly. We know that researchers change the *natural* order of things merely by their presence. In my study, being male may have impacted on such an order. It could be that if people were wary of my presence (as a male researcher or otherwise) and desisted from behaving *naturally*, I might wonder how much was staged for my benefit. Earlier in the article, I said that my male gaze may have blinded me to certain issues. Whether concerning access or relations, or my perception that gender did not always hinder research, my own subjectivities and understandings of situations may have resulted in aspects of such

interactions being invisible and inaccessible to me. In the case of changing conduct, this is likely to occur in any ethnographic endeavour and it did not appear, for me, that participants were 'inauthentic' in any capacity. Yet this 'humbling' (Goffman 2014: 229) interaction caused much self-doubt and I will never know just how frequently my male presence disrupted the performances of those involved in the scene.

Discussion

Here, I have shown that as a fieldworker, my gender identity 'formed an essential component in the negotiations that took place between myself and the people I was working with' (Back 1993: 229). By positioning, practicing, and performing a gendered self, I engaged in self-modification, mostly with respect to doing a more 'muted' masculinity (Ortiz 2005) as a tactic in strategic interaction (Goffman 1959). I also described how my gender was articulated with other aspects of my researcher self. There are many elements at play – age, status, sexual orientation, faith – when conveying a gender identity; male researchers, as an assorted (i.e. not a homogenous) group, will bring different characteristics to their activities. That said, whilst otherness can be present or absent at different moments (Desmond 2007), my gender meant that my otherness was always on display since I occupied a setting that was populated mostly by women. Still, my differentiated body and performance did not necessarily hinder the study, but, in fact, played a role in ensuring access, developing relationships, and collecting data. However, it is best to be modest about this as one rarely knows how participants truly see a researcher or how they would have acted otherwise. In addition, I resist overstating the importance of difference as I shared certain attributes with participants such as ethnicity, nationality and background, religion, interests and outlooks. Mutual commonality helped shape and uphold meaningful relations too.

There is an argument to be made that I may have missed certain interactions and situations in my fieldwork on account of the male gaze and my own gendered assumptions. However, this is the same for most researchers/ethnographers. As a heterogeneous group, researchers will inevitably possess attributes, perform identity markers, and carry a set of expectations that are likely, to some degree, to blind them to certain moments. This is not a limitation, per se, but stresses the importance of being reflexive about the art of fieldwork. My interpretation of situations in the clinic is likely to be seen through a gendered lens, but what I put forward here, in my reflexive thinking, is that whilst the hypothetical female researcher (who also performs many other parts of her sense of self) may gather different data and experience a different level of access to me, my study suggests that carrying out research in a largely female-populated setting was not necessarily problematic.

In his research on female sex work, Hubbard (1999) asks whether men *should* do research in sites occupied mostly by women (he concludes they can, but with some caution). My intention here is not to offer, or even pretend to provide, instruction on how males *can* and/or *should* do research in locations populated mostly by women. Rather, my aim is more modest: to make the male researcher

visible. Thus, I have offered a story, not a guide, which engenders insight on the complex nature of ethnographic undertakings and, so, appeals for more reflexive and gender-focused dialogue. Indeed, I hope to have convinced other male researchers, by engaging in self-reflection and interrogating their own speaking position (possibly one which is privileged in certain fields), to get some purchase on the gendered nature of fieldwork that will allow our work 'to be read and evaluated in an honest way' (Back 1993: 230).

My intention is that this article is not read as a biography, as meaningless navel-gazing, or as a platform to share curious – but relatively futile and useless – anecdotes. Indeed, I do not want my account of the 'dramaturgical complexity of role performance in the field' (Scott *et al.* 2012: 730) to be seen as unnecessarily self-indulgent. Rather, by making explicit our role in shaping the research process and communicating our sense of self-awareness, and writing ourselves into the world, we can distinguish how emotion and identity-work are managed and negotiated in fieldwork; 'the complex relationships between field settings, significant social actors, the practical accomplishment of fieldwork, and the self are present and salient for all of us who engage in qualitative research' (Coffey 1999: 14). With regard to this article, I appeal to (particularly male) researchers to discuss and debate how their gender takes shape in interpersonal or group dynamics in the field, and to share tales about 'performing in (and messing up) their own shows' (Scott *et al.* 2012: 718). This level of honesty and reflexivity would be a welcome addition to the literature.

Acknowledgements

Notes

1. However, there is a history of male researchers reflecting on the disruption of gender difference in educational ethnography (e.g. King 1984; Mac an Ghaill 1994; Meyenn 1980). For example, Erickson (1986) analyses a joke he encountered in his academic department in the 1980s: 'Real men don't do ethnography'. He uses this to consider why some men in educational research at that time may have avoided ethnography – namely owing to an absurd perception that quantitative research was a 'male domain' and quantitative research was a 'female domain', a view which feminist thought intended to disturb (Delamont and Atkinson 2008). Thanks to [colleague] for these insights.

2. I am cautious here about drawing too many comparisons between my own experiences and that of male anthropologists as the accounts of the latter mostly concern settings regulated by strict rules about gender segregation. In Abramson's (1993: 70) study, his male status prevented access to certain

situations and locations (e.g. domestic spaces occupied by women) and this inhibited his knowledge; participants played a role pertaining to 'where he is placed, what is presented to him, and what he is prevented from easily seeing'. In this article, I focus on my research in a UK setting where there are mixed institutions with few *formal* barriers to physical presence or social interaction. But whilst such institutions will often not have any consistent rules on a male/female presence, such spaces are still highly gendered and the gender identity work of researchers should be considered. I thank (colleague) for this observation.

3. This is similar to Diamond's (1992) interaction with an administrator when he applied to work as a nursing care aid. The administrator recognised the conflation of gender, race, and class dynamics (many workers were women, non-White, and poor – Diamond was male, White, and an academic) by suspiciously asking 'now why would a White guy want to work for these kind of wages?' (1992: 187).

4. In Lupton's (2000) research with men doing 'women's work', he describes how men felt excluded from some dialogues where there were only women present and were worried about stigmatisation, feminisation, and limited opportunities for a high-income. They describe a restriction of masculinity (e.g. not feeling able to fully relax in the exclusive presence of women) and fear of becoming 'invisible as a man, being adopted as a woman and becoming feminine through working with women' (2000: 40). According to Lupton, they rectified this by redefining their job to fit closely with an acceptable version of masculinity and by adapting masculinity and accepting/promoting that compromise.

5. For further accounts of how sex and sexuality impinge upon the research process, I refer readers to an edited collection by Kullick and Wilson (1995) and the bibliography of Beusch (2007).

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