In some very important ways, the task of writing a chapter on *queer methodologies* is doomed to failure; the two words of my title are reluctant bedfellows. Methodologies are logics that attempt the impossible task of arranging different ways of knowing into hierarchical orders, based on competing ethical, practical and epistemological values. While ‘queer’ is sometimes used as a supplementary term to ‘lesbian, gay, bisexual, and transgender,’ or as an overarching category that includes them all (and sometimes others), it is the understanding of ‘queer’ as an anti-essentialist counter-disciplinary project, committed to partiality and irony, that concerns me most here. *Queer* strives to trouble Enlightenment projects, including the fiction that the field of study known as ‘methodology’ is rational and coherent. As such ‘queer methodologies’ might be contradictions in terms; the first term insists on pluralism, heterogeneity, and understandings of difference that the second term writes off as error variance. Nor is it clear if a chapter on queer methodologies will review those that exist, or consider what queer methodologies of the future might be. When placed before ‘methodology’ is queer an adjective, a verb, or both?

This chapter can only be a partial account of these tensions, and it will be informed by my involvement in the fields of social psychology and the history of psychology in particular. My focus is going to be on that ‘workhorse of social psychological research, the experimental method’ (Aronson, Wilson & Brewer, 1998: 100). Coming from LGBTQ psychology, experiments might seem like an odd place to site a queer project. Kitzinger (1997; Kitzinger & Coyle, 2002) describe social constructionist and ‘traditional’ epistemologies (including experiments) in lesbian and gay psychology as engaged in politically related but epistemologically dissonant projects. Social constructionists critique the heteronormative, androcentric, imperialist edifice of psychology, while traditionalists use psychology’s fact-producing methods in the service of securing civil rights and other benefits for lesbians and gay men. Quantitative methods are often described as useful to lesbian and gay psychology as means of debunking homophobic myths and ‘bad science’ (e.g., Herek, 1998) while qualitative methods are described as means of enacting more collaborative, and less hierarchical relationships with research subjects (e.g., Coyle, 2000). Through careful and insightful readings of both homophobic and gay-affirmative quantitative research in psychology, Noam Warner (2004) reaches the conclusion that queer methodologies in psychology are *necessarily* qualitative methods. From these accounts, you could easily get the impression that political efficiency is the primary, if not the only, reason that savvy researchers would pick experiments out of the master’s toolkit to work with.

This chapter gambles on the idea that such an understanding of quantitative methods forgets much about the ways that these methods have been practiced in the past, and encodes a limited imagination of what they might do in the future. I am going to “celebrate the diversity” of the queer weeds that grow in the cracks of laboratory knowledge which positivist narratives rarely cherish and social constructionist quantophobia neglects to nurture. This chapter is not an argument against qualitative methods. Throughout, its logic will be transparently indebted to thinking from the qualitative social sciences. Rather, it is an attempt to historicize experiments by animating the queer voices in the ‘stubborn particulars’ of social
psychology (Cherry, 1995) and to use those conversations across time to push past the heteronormative and non-reflexive horizons that ‘methodologies’ so often create.

**EXPERIMENTAL AND QUEER SUBCULTURES**

Psychology laboratories and queer sub-cultures have mutually defining histories. The professions of psychology and psychiatry expanded rapidly in the years after World War II, peopled often by veterans who received generous financial aid for university educations through the GI Bill (Capshew, 1999; Herman, 1995). In the post war US, psychology laboratories multiplied with generous contract support from funding agencies (Darley, 1952), and appeared to some to afford a Positivist vision of an experimental science of social relations (see e.g., Allport, 1954). President Eisenhower described the Cold War as a psychological battle for the hearts and minds of Americans, making the psychology loyalty of individuals a matter of national security (see Lutz, 1997). However, gay men and lesbians were on the wrong side of this state sponsored war. Homosexuality was grounds for exclusion from military service for the first time in World War II. A range of ad hoc biological, psychiatric and psychological techniques were piloted as ways of detecting true homosexuals from heterosexual men who were feared to be ‘malingering’ as homosexual to escape active service (Berube, 1990). Servicemen dismissed from the army with ‘dishonorable discharges’ for homosexuality were barred from the GI bill that would have allowed them access to scientific training. Gay men and lesbians were further excluded from government work under a series of executive orders signed in the early 1950s which barred them from an ever widening range of government positions. Ultimately the FBI collaborated with local police forces to ensure that there were no ‘sexual psychopaths’ in government employment (see Terry, 1999). ‘Sexual psychopath’ laws were passed by over twenty states by the mid-1950s (Freedman, 1987; Miller, 2002). Enforcement of the military ban on homosexuality intensified after the war (Berube, 1990). Some psychologists even tried to develop experimental technologies by which gay men could be detected and excluded from intelligence services (Gentile & Kinsman, 2000). Popular fiction and social science writing conflated lesbianism with dangerous criminality, warning women of the consequences of nonconformity to increasingly restrictive heterosexual roles (Kunzel, 2002; Freedman, 1996).

This ‘sex panic’ (Rubin, 1984) created a climate of extreme suspicion a propos of research on homosexuality in the emerging human, cognitive, behavioural and psychological sciences, which curtailed the kinds of knowledge that experimental researchers in these various disciplines could construct. In light of Kinsey’s findings about the sexual practices of American women, the Reece Commission put pressure on the Rockefeller Foundation to withdraw their support for his Institute of Sex Research. Kinsey died, a broken and exhausted man only a few years later (Gathorne-Hardy, 1998; Jones, 1997). Evelyn Hooker, whose experimental work debunked much of the earlier wartime work on the ‘homosexual personality’ was scrutinized by her funders to make sure that she was not a lesbian (Hooker, 1993). Astronomer Frank Kameny lost his job in a government laboratory and later became an activist who spearheaded the move to have homosexuality removed from the DSM" (Marcus, 1992). This is not to say that lesbian and gay experimenters did not exist. Many gay professionals participated in underground networks such as those described by ‘Dr. Anonymous’ at the first meeting of the American Psychiatric Association in which openly gay and lesbian people spoke for themselves (Bayer, 1981). Roger Brown’s sexuality appears to have been an open secret (see Brown, 1989). Daryl Bem told his wife Sandra that he was more attracted to men than to women on their first date in the mid 1960s, but the couple went on to lecture publicly about how to have an egalitarian heterosexual marriage in the early 1970s (Bem, 1998).
In short, the closet was a card of entry into the experimental cultures of social psychology that developed in post WWII America. The gay and lesbian subcultures that developed at this time ultimately gave rise to ‘queer theory’ (Minton, 1997), but were deliberately expunged from the laboratories and university classrooms where psychology’s expert discourse and methodological norms were taking shape. In spite of this, curious similarities between the two can be noted; in both spaces people play dress up, explore new identities, and push past the received wisdom of what ‘the social’ might become (Hegarty, 2001). However, queer sub-cultures are not invested with authority as science is; some gender performances appear as ‘play acting’ while the artifice of social psychological experiments is understood to be exploring the reality of human sociality. Small wonder that these two sub-cultures often reach radically different epistemological conclusions about how gender might be known (e.g., Parlee, 1996).

Perhaps the most notorious social psychology experiment from this period is Stanley Milgram’s experiment on obedience. Ostensibly unrelated to queer theory at all, I would like to argue that this experiment was compromised in its design and interpretation by Cold War sex panic. Most of us who have been indoctrinated by the received wisdom in social psychology know the story; Milgram’s participants believed that they were following an experimenter’s orders by administering painful, and ultimately lethal, electric shocks to strangers as part of an experiment on learning. Participants delivered the electric shocks beyond the point where the ‘learner’ (actually a confederate) complained about his heart condition, banged on the wall, begged to be released from the experimental apparatus, and ultimately fell silent. Many psychologists found Milgram’s experiments unbelievable, replicated his procedures, and observed similar results in their own laboratories (see Milgram, 1974).

The received wisdom in the field is that Milgram’s experiment evidences how obedience to authority can lead ordinary individuals to engage in surprising evil actions, and thus provides a potent explanation of ordinary Germans’ participation in the Holocaust (e.g., Bauman, 1989; Brown, 1986; Hogg & Vaughn, 2005). This interpretation of the experiment has been critiqued within psychology in two principle ways. First Baumrind raised the question of the experiment’s use of deception. Wasn’t that harmful to both individual participants and the public image of science? A second concern was raised by the possibility that the participants knew the true purpose of the experiment, such that their behaviour in the laboratory was little more than assent to the experimenter’s demand, and the experiment itself was epistemologically void (see also Orne, 1962). In other words, while some critics thought the experiment too realistic, and others thought it was not realistic enough.

Both of these critiques of the canonical reading of Milgram’s experiment take on new meanings when we keep in mind the concerns of queer subjects that his own experimental subculture disavowed. First consider the Milgram experiment as an analogue of the Holocaust. Queer people who are particularly vulnerable to state-sponsored violence (including that of the Nazis) have a particular stake in the production of this critical knowledge. However, in the narratives of the Holocaust to which Milgram’s work refers, the persecution of gay men and lesbians is never explicitly mentioned. Indeed, in the early 1960s when Milgram’s work was published, any such claims would have been controversial. The German law anti-sodomy law, Paragraph 175, had, since 1872, banned ‘unnatural fornication.’ In 1935 the Nazis re-wrote this law to categorize sodomy as a felony subject to imprisonment, affording the arrest of homosexuals who were sent to the camps. After the war, Paragraph 175 was deemed not to be one of the Nazi laws that contravened democratic
principles, and which the Allied Control Council were empowered to overturn. Remembering the lesbian and gay victims of the Holocaust would have required Milgram to query the sex laws of Western democracies, both at home and abroad. He failed to theorize the relevance of his experiment to those laws.

Next consider the claim that the participant in the experiment simply agrees to the experimenter’s ‘demands’ and knows that the situation is so extreme that it cannot possibly be real. The ‘cover story’ for the Milgram experiment trades on the existence of, and public familiarity with, behaviourist psychology; the participant arrives at the laboratory armed with the knowledge that psychologists deliver electric shocks to people to make them learn new things. Not only was this believable in the early 1960s, but some of Milgram’s participants may have been aware that such procedures were used to try to make gay and lesbian people ‘learn’ heterosexual responses. Reparative therapy uses electric shocks, delivered in response to arousal to homoerotic stimuli to make gay/lesbian people ‘learn’ to be straight.¹ Along with electroshock, castration, clitorectomy, lobotomy, and arduous psychoanalysis it was part of the psychiatric arsenal used to extinguish homosexuality from American selves during the time of Milgram’s experiments (Katz, 1976).

Similar procedures were used in UK psychiatry from the 1950s until the 1980s. Some psychologists who practiced this work believed that they were helping unfortunate homosexuals to become better people. Others knew from the start that reparative therapy would be a failed project. Still others remind us of the shared history that queers have with Milgram when they recall that they were just obeying authority:

Well I didn’t have much choice. That was a clinical placement. I was [the consultant’s] first student. Basically the first year I was there, more or less all I ever did was shove electricity down homosexual patients (cited in King, Smith & Bartlett, 2004)

Thus, while Milgram’s work was well-positioned to comment on the criminalization and pathologization of lesbians and gay men, and may even have depended on that pathologization for the coherence of its cover story, widespread state-sponsored wars of gay men and lesbians fell beyond the domain of evil deeds to which Milgram generalized his work.

Finally, consider Baumrind’s critique that the Milgram experiment is unethical for its use of deception methodologies, a criticism that Milgram countered by citing a follow-up questionnaire of participants showing that virtually all were unharmed by the experience, and supported the continuation of the experiment. Social psychology laboratories were not the only closed spaces in which people played with pain, obedience and power in the post war era. During this period, autonomous SM cultures also developed in North America (Sisson, 2005). In Milgram’s writings the existence of SM provides a background hypothesis which is refuted in favour of his claim that evil in the service of authority is a capacity of ordinary people;

In the minds of some critics, there is an image of man (sic) that simply does not admit of the type of behaviour observed in the experiment. Ordinary people, they assert, do

¹ As one mental health nurse recalled it “We had to become electrifying geniuses! The situation was you had the screen, the person sat at the table with the things [equipment] on and with a lever that they had to pull to avoid the shocks. The pictures started off with pretty men, working their way through ugly men into ugly women and into pretty women. That was the whole process literally.” (cited in Kind, Smith, and Bartlett, 2004, p. 3).
not administer painful shocks to a protesting individual simply because they are
ordered to do so. Only Nazis and sadists perform this way. In the preceding
chapters, I have tried to explain why the behaviour observed in the laboratory comes
about: how the individual makes an initial set of commitments to the authority, how
the meaning of the action is transformed by the context in which it occurs, and how
binding factors prevent the person from disobeying (Milgram, 1974:169, emphasis
added)

Milgram here contributes to a long history of conflating the psychological motivations
of SM practices with Nazi violence (see also Moore, 2005). For Milgram, both are
exceptional, and different in kind from the evil actions of the ordinary people who find
their way into Milgram’s experiment. Yet, very similar social psychology experiments
were later referenced in debates among feminists about SM. Linden, in the
introduction to Against sadomasochism: A radical feminist analysis, described the
experience of sadists and masochists as akin to that of the prisoners and guards in
Zimbardo’s famous prison experiment;

We can speculate that the experience of submission in a sadomasochistic
relationship is far more intense than it was in the prison simulation. Just as the
“guards” made an easy accommodation to wielding power, we can expect that sadists
who regularly practice dominant roles would become habituated to sadism, perhaps
failing to comprehend its extremity (Linden, 1982: 8-9).

Yet, contrary to Linden’s and Milgram’s suggestion, the practice of sadists in SM
cultures might have provided Zimbardo and Milgram with methodological resources
to engage differently with the ethically dubious practices of deception experiments.3
Here, I’m referring to the practice of using agreed-upon signals, such as safety
words, that provide the M to limit the scope of SM play and so to enter into that play
more fully:

As we progress toward the heavier action, it might be well to point out again that a
true leather scene is not merely an exchange where one guy binds the other and
whips the shit out of him. There are a few M’s who want this; most don’t. For this
reason it is always important that the M be given an “out.” In a situation, for example,
where the M is gagged and blindfolded – rendered almost incapable of expression – it
is still essential he be able to let the Topman know when he’s had enough. It may
only be case of needing to take a break, or it may require a complete shift in the type
of action. Regardless, if the S is worth his salt he will have made some provision for
this. It is much less disruptive of the role-situation for the S to say: “When you can’t
take any more, do such-and-such” than for the M to set the signal (Townsend, 2000:
131)

Indeed, in his account of this experiment, Philip Zimbardo (1999) admits that he did
become habituated to the guards’ cruelty and was caught in the double role of chief
prison guard and experiment. Had Milgram and Zimbardo found common kinship
with such practices as the use of safety words in SM practices their experiments
might have created still bolder knowledge about the ethics of consent in situations
structured by obedience, power, and role-playing. Rather, these experiments were
structured and maintained by the fact that there was nothing that the participants
could have said to the experimenter’s that would have stopped the experiments in
their tracks.

As it is, social psychologists have not solved the dilemma of how to do experiments
without deception (Sieber, Iannuzzo & Rodriguez, 1995), it remains an ordinary
practice in their laboratories, and one that has costs for researchers as well as
participants (see Oliansky, 1991). In spite of these obvious problems in experimental
culture, there is a failure to look at the kinds of practical knowledge developed in
queer sites, such as SM subcultures, for ways of pushing beyond these dilemmas about consent, freedom, power, and coercion.

**HOW DO YOU IDENTIFY?**

My discussion of the Milgram experiment presumes that there are multiple ways of writing about, and of remembering, what happens in experimental situations, and that queer methodologies might be practiced through close simultaneous attention to the implicit sexual politics involved in both concrete methodological practices and the general claims about human psychology that experiments warrant. The reporting of experiments is highly codified, most obviously but the Publication Manual of the American Psychological Association (1994). Eager to distance itself from psychology’s troubling homophobic past, the manual urges that ‘people who are not heterosexual’ should be described in published works as follows:

The terms *lesbians* and *gay men* are preferable to *homosexual* when referring to specific groups. *Lesbian* and *gay* refer primarily to identities and to the culture and communities that have developed among people who share those identities. Furthermore, *homosexuality* has been associated in the past with negative stereotypes. . . . The clearest way to refer inclusively to people whose orientation is not heterosexual is to write *lesbians, gay men, and bisexual women or men* – although somewhat long, the phrase is accurate (American Psychological Association, 1994: 67).

This shift from medicalized terms to community-derived terms attempts to effect a break with the stereotypes of the past. Yet, as Walsh-Bowers (1999) has noted, the genre of writing proscribed by the Publication Manual also proscribes particular ways of seeing the world. For example, by subdividing the ‘method’ section, into a quite cursory ‘participants’ section, and more lengthy ‘materials’ and ‘procedure’ sections, the Publication Manual treats people as inert resources, much like the materials that appear in natural science laboratories. Yet, as Walsh-Bowers (1999) notes there is nothing value-neutral about a natural science model of understanding that positions research participants as objects of knowledge, separate and distant from the psychologists who study them. By miming the forms of writing that have long been practiced in the natural sciences, the Publication Manual not only proscribes writing within a genre, but also denies that that genre is a genre. Since its inception in the 17th century, the scientific journal article has been promoted as a means of reporting ‘matters of fact’ that avoids “meddling with Divinity, metaphysics, Morals, Politics, grammar, Rhetorick, or Logicks” (Hooke, 1663, cited in Bernal, 1965: 455). As such it is one of the literary mediums through which scientists circulate an image of their activities operating in what Traweek (1988) calls a ‘culture of no culture.’

Are there possibilities of queer knowledge that are suppressed by the codification of ‘lesbians, gay men and bisexual women or men’ as the accurate way or referring to ‘people who are not heterosexual’? This question recalls Judith Butler’s (1993) claim about identification that ‘to write or speak as a lesbian appears a paradoxical appearance of this “I,” one which feels neither true nor false. For it is a production, usually in response to a request’ (p.307). Butler elaborates:

I’m not at ease with “lesbian theories, gay theories,” . . . identity categories tend to be instruments of regulatory regimes, whether as the normalizing categories of oppressive structures or as the rallying points for a liberatory contestation of that very oppression. This is not to say that I will not appear at political occasions under the

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2 Here, it is worth remembering that the term ‘subject’ long used for participants in psychology experiments is derived from French pathologists term for a corpse, see Danziger (1990).
sign of lesbian, but that I would like to have it permanently unclear what precisely that sign signifies (Butler, 1993: 308).

It is this opaqueness of identity categories that the Publication Manual works to render transparent through prescriptions of a single accurate way to describe ‘people who are not heterosexual.’ Yet some of the best research on sexual identity is done in antithesis to this, as in Diamond’s (2006) or Rust’s (2000) research on sexual minority women who do not identify with any label at all. In other contexts the explosion of possible terms for sexual minority identities points out the limits of the publication manual’s proscriptions. SIGMA Research’s 2004 gay men’s sex survey is one of several annual surveys conducted ‘to collect a limited amount of information from a substantial number of men’ (Weatherburn, Reid, Hickson, Hammond & Stevens, 2004: 2). More than 32,216 self-report questionnaires were distributed to 113 HIV health promotion agencies in England, Wales and Scotland. The survey was also posted on-line in English and Spanish. Of the 19,210 surveys returned, 3,208 were excluded. Most exclusions occurred because the respondents were not resident in the UK, but 383 men were excluded because there was neither ‘evidence of sex with men in the previous year or … gay, bisexual, or queer identity’ (Weatherburn et al., 2004: 4).

In making this determination about identity, the researchers relied on the men’s own words. These were occasioned by an open-ended questionnaire item; “What term do you usually use to describe yourself sexually?” While most respondents identified as gay (n = 13,030) or bisexual (n = 1961) in response to this question, 893 specified that they usually did not use a term. However, these terms failed to exhaust the diversity of identities that men used to describe themselves:

Of the 92 men (0.6%) who ticked other and remained in the sample, 18 identified as ‘queer’ and one as ‘queer as fuck.’ Eight identified as homosexual alongside one who considered himself a ‘homosexualist.’ Five men identified as ‘bi-curious.’ Another five were ‘curious’ including one that was ‘curious, not hetero and not gay’ and one that was ‘still curious.’ Five men identified as ‘open minded’, one simply ‘open’ and one ‘open to offers’. Two identified as ‘confused’ including one that was ‘confused and experimenting’. Four men identified as ‘straight’ and three as ‘heterosexual’ or ‘hetero’. Two men identified as transgendered and three as transsexual (two male-to-female and one unspecified). One man identified as a ‘transvestite’. One man each said: ‘anything – whatever you want I got it; ‘batty boy’; ‘batty’; ‘bent’; ‘bloke who likes having sex with men’; ‘discreet’ (sic); ‘dilemma’; ‘experimental’; ‘free-spirited’; ‘goth’; ‘happy’; ‘I am me – no pigeon holes’; ‘I like both, but very picky’; ‘lad who only fancies lads’; ‘non-heterosexual’; ‘not sure’; ‘man who has sex with men’; ‘proficient’; ‘raving old poof’; ‘sexually active’; ‘trysexual – I’ll try anything sexual’; ‘unique’; ‘wish to try other sex’; ‘women alluring, men re-assuring’ (Weatherburn et al., 2004: 6)

To my knowledge, Weatherburn et al. (2004) have not published their work in an APA journal or subjected it to that organization’s norms. I think that this is a very good thing, for in telling us more about these men’s descriptions of themselves than the APA would advise, some horizons of quantitative research remain visible in this report. When I read this list of terms some of them made me laugh because they seemed to subvert the question that the researchers asked. Others made me feel uncomfortable as they are terms I once used to describe myself but have long since given up. Collectively, they made me wonder when and where sincerity, irony, cooperation and dissent might be the intended effects of nominating the sexual self with a particular label. I was thrown back on the conclusion that it would be impossible to now know which motives lay behind the writing of particular terms.
In other words, by telling their readers more than the APA might proscribe, Weatherburn et al. (2004) also provided us with the text we need to recognize our ignorance about what ‘people who are not heterosexual’ are thinking when they give of their time, effort, and privacy to research. Nominations of one’s identity, even those that appear on factual social science questionnaires, are never simply accounts of pre-existing identities; rather those identities are called into being through the act of naming itself. Such an identification is, in Butler’s terms, ‘a production, usually in response to a request.’ ‘Experimental’ and ‘free-spirited’ may be obviously produced accounts of identity, but their reprinting here shows how the more quotidian ‘gay’ and ‘bisexual’ are productions also; sex with men, or with men and women is no guarantee that a man will identify as ‘gay’ or ‘bisexual.’

Finally, this list shows the impossibility of both the liberal humanist fantasy of celebrating the diversity of queer identities and the empiricist fantasy to sample those identities representatively. With categories like ‘free-spirited,’ ‘batty boy,’ and ‘raving old poof’ at play, the notion that we will one day add enough categories to ‘lesbian, gay, bisexual, transgender, queer, questioning….’ to saturate the possibility or representing identity categories seems to be about as far off as the positivist fantasy of representing each category with a recognizable representative sample. Given that this study examines sex between men, which can be put to work as both ‘normalizing categories of oppressive structures’ and ‘rallying points for a liberatory contestation of that very oppression,’ the visible signs of these failures which limits our confidence in psychological knowledge may be a very good thing.

EXPERIMENTS WITH NORMATIVITY
Given these discursive limits of quantitative research, why would someone conduct social psychology experiments at all? My own work in this field exemplifies how the experimental study of situationism has changed since Milgram’s time. Social psychologists are less regularly concerned with such big and dramatic demonstrations of the power of immediate situations as those constructed by Milgram or Zimbardo. In recent decades, as a result of ethical concerns raised by such experiments, and under the influence of cognitive psychology, we have become far more engaged with questions about how people make sense of their social worlds through attributions, stereotypes, and cognitive heuristics (see e.g., Fiske & Taylor, 1991).

As part of this shift, experimentalists have often examined how and why people rigidly cling to beliefs in the face of evidence to the contrary. For example, in an early experiment Chapman and Chapman (1967) examined clinicians who erroneously judged that responses stereotypically associated with homosexuality were more likely to be produced in response to Rorschach cards by gay men than by others. Snyder and Uranowitz (1978) studied the ways that people selectively remembered events from a woman’s life that are consistent with lesbian stereotypes after they learn that she has come out as lesbian. Deaux and Lewis (1984) showed that people were described as transgressing norms for gender were more likely to be judged to be gay or lesbian than heterosexual.

In other words, social psychology experiments have become ways of calling attention to the irrationality of modern life, and the impossibility of creating a social world in which people don’t automatically reach consensus about what evidence means. Allport’s (1954) positivist dream has been lost. My own experiments concern the ways that people make sense of evidence about group differences by thinking normatively. The first experiments I carried out in this domain were concerned with the ways that heteronormativity affects the way that people explain differences between people categorized by sexual orientation. Felicia Pratto and I asked
undergraduates to write explanations of scientific findings that we presented to them (see Hegarty & Pratto, 2001, 2004). We told half of them that interview studies had shown that gay men and lesbians reported more gender non-conforming childhoods than heterosexual women and men. The other half were told that the heterosexuals recalled more gender non-conforming childhoods. The participants’ written explanations were coded for references to members of each sexual orientation group. Participants overwhelmingly focused their explanations on behaviours, traits, and other attributes of lesbians and gay men, but mentioned far fewer particularities of heterosexuals that might have contributed to the group differences. Similar findings have been observed when people explain other kinds of group differences; gender differences are explained by describing how women are different from men rather than how men are different from women (Hegarty & Buechel, 2006; Miller, Taylor, & Buck, 1991). In the US, race differences are explained by taking Whites’ behaviour as the norm and explaining how Blacks’ behaviour differ from it (Pratto, Hegarty, & Korchmaros, in press).

I will leave it to the reader to adjudicate whether this work is queer or not, but I have explained elsewhere how the experiments’ design was informed by queer theory and a desire to theorize human cognition in a way that could easily account for the ways that people think about group identities with non-essentialized categories (Barker & Hegarty, 2005). The experiments were also conducted a propos of the resurgence of interest in gender-inversion models of homosexual development in psychology (e.g., Bailey & Zucker, 1995; Bem, 1995). We aimed to demonstrate that people make sense of such findings in culturally particular but systematic ways. Explanations of group differences involve drawing on existing knowledge about those groups; they require the citation of stereotypes, and as such are occasioned performances rather than literal truths. However, scientists explanations are readily confused with facts, and when such explanations are heteronormative they become a site where heteronormativity is sedimented.

We were particularly concerned with heteronormativity in this research, and, following Butler (1993) understood research on gender-inversion models in psychology to normalize heterosexuality in at least three ways. First, heterosexuals were taken as the norm for comparison, such that their particular attributes remained unspoken while those of lesbians and gay men incited psychological explanation. Second, the genders that are understood to be ‘inverted’ in this model are assumed to be heterosexuals’ genders. The possibility that gay and lesbian genders might be the ontological basis for describing heterosexual genders beyond the epistemological horizon of gender inversion theories, even though heterosexual genders might be most obviously performed through the disavowal of homosexuality. Third, in our experiments, the evidence supporting gender-inversion models was granted greater weight by the participants. These findings were essentialized and deemed to be reports of real childhood differences. In contrast, when we told participants that heterosexuals reported gender non-conforming experiences, they assumed that this mean that gay men and lesbians had lied to the interviewers or remembered their own childhoods wrongly.

I think that these experiments not only say something about the stereotypes of the student participants, but also trouble the methods of professional social scientists as well. Typically social cognition research assumes scientists to be more rational than laypeople, and describing the cognitions of the latter group as error-prone, naïve, biased or intuitive versions of the former (e.g., Kahneman, Slovic & Tverksy, 1982). However, this is a norm my colleagues and I are keen to publicly break. So far we have three methods for so doing. First, we study scientists’ explanations of group differences in their published texts as well as conducting experiments on
undergraduates. Thus far, we have found that psychologists and biologists accounts of sexual orientation differences are routinely heteronormative (e.g., Hegarty, 2003; Hegarty & Pratto, 2001) and their accounts of gender differences are routinely androcentric (Hegarty & Buechel, in press).

Second, we try to model what opposition to normativity would look like in the way that we describe our own results. In our experiments people write sentences that compared gay people to straight people more than the reverse, and preferred ‘more than’ constructions of difference to ‘less than’ constructions. In contrast, we pepper our results sections with sentences such as this one;

\[
\text{Across the experiment as a whole, fewer references to straight men (M = 0.49) than to gay men (M = 0.78) were produced, } F (1, 122) = 6.20, p < .05 \text{ (Hegarty & Pratto, 2001, p. 727).}
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Some readers have told us that when they read our experimental findings, their tacit knowledge about norms for explanations of group differences becomes engaged by our writing practice. Like Lorde (1984), we understand such ‘mythical norms’ as whiteness, Christianity, heterosexuality, etc. to inhabit the edge of consciousness; we can bring them into view but we habitually do not, and the consequence of our habitual non-conscious thinking about norms is a set of scientific representations of group identity that conflate certain groups with the norm.

Third, we openly describe ourselves as scientists situated within culture, whose thinking is shaped by the tacit knowledge we describe (see e.g., Hegarty & Pratto, 2004, p. 452). Thus, we work against the image of laypeople as biased version of scientific rationality, concluding instead that “psychologists are very ordinary, very much influenced by culture, and very much like other people” (Hegarty & Buechel, in press).

CONCLUSION
In this chapter I have argued that queering methodologies in psychology involves memory work, epistemological bravado, and literary subversion that cares enough about the ritualized practices of ‘methodology’ to blaspheme against them. The equation of critical work with qualitative work on psychology has opened up many new ways of producing knowledge about queerness, many of which are described in other chapters in this book. However, queer theory could be quantophilic as well as quantophobic. There is much to play for in laboratories, even if the pleasures of playing in that space have been largely claimed by straight men (Lubek & Stam, 1995). Access to experimental spaces where ‘the social’ is materialized through practices of artifice, performance, and social roles has presented - and continues to present - difficulties for queers, who were deliberately excluded at its foundational moments. All the more reason why this is a space to which we might lay claim.
Endnotes

1. The “GI Bill” or 1944 Serviceman’s readjustment act provided college and vocational education for returning World War II veterans in addition to unemployment benefits and housing loans.

2. The “DSM” of ‘Diagnostic and Statistical Manual is the codification of psychiatric disorders institutionalized by the American Psychiatric Association. Both the first DSM (published in 1952) and DSM II (published in 1968) listed homosexuality as a mental illness. Subsequent to the Association’s vote to remove homosexuality in 1973, DSM III (published in 1980) listed only ‘ego-dystonic homosexuality’ to refer to gay and lesbian persons who themselves perceived their homosexuality to be an impediment to psychological adjustment. This category was removed from the Revision to DSM III (published in 1987).

3. In Zimbardo’s experiment, male college student volunteers were randomly assigned to play the roles of ‘prisoner’ and ‘guard’ in a mock prison in the basement of Stanford’s Psychology Department. Although the experiment was scheduled to run for two weeks, it was stopped after six days for ethical reasons. Accounts of this experiment attributed the guards’ brutal behaviour and the prisoners’ acquiescence to their roles in terms of the ‘power of the situation’ over the personality of the individual. While Zimbardo (1999) also reproduces this narrative, it also shows clearly that the prisoners often rebelled individually and collectively and that the guards’ brutality was supported by Zimbardo’s own dual role as ‘experimenter’ (outside the experiment) and chief prison guard (within it). My point here is that SM, at least as described by Townsend (2000) – who was exiting a career in psychology around the same time that Zimbardo’s experiment was garnering him fame within the discipline – had lessons for the ethical means of managing consent in situations where role-play and power are concerned; lessons which, many within the discipline of social psychology were resistant to learning.

4. In 1994 SIGMA research evolved from Project SIGMA which had conducted longitudinal research on the health of gay and bisexual men in the UK since 1987. SIGMA Research is “a social research group specialising in the behavioural and policy aspects of HIV and sexual health” (http://www.sigmaresearch.org.uk/index.html).

References

Allport, GW 1954 ‘The historical background of modern social psychology’ in Handbook of social psychology, Worchester, MA: Clark University Press.


Coyle, A, 2000, ‘Qualitative research and lesbian and gay psychology in Britain’ *Newsletter of the British Psychological Society Lesbian and Gay Section*, Issue 4, pp. 2-5.


Herman, E, 1995, The romance of American psychology: Political culture in the age of experts, University of California Press, Berkeley, CA.


