Measuring harm: Governing the effects of child sexual abuse

by

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Abstract

My primary research question is 'How have techniques of psychological measurement and statistical analyses been made to function as objective methods for determining the effects of child sexual abuse?' This question has been developed through an understanding that research on the effects of child sexual abuse, both on children and on adults who were abused as children, is often performed as a scientific practice (the field has 'crossed the threshold of scientificity'). This frequently involves measuring differences in psychological attributes between abused and non-abused individuals. The representation of the harm of child sexual abuse in a scientific style has been important for the recognition of the suffering caused by such abuse, yet there are implications in a disciplinary sense; subjecting individuals to the power/knowledge nexus of scientific reasoning *also* involves relations of power, which is a central problem in child sexual abuse itself.

The 'field of child sexual abuse' is not one stable homogenous field. Developmental (scientific) psychology sits alongside a range of feminist analyses (I draw particularly on self-identified post-structuralist style of feminist styles of reasoning), contesting this space and its objects. I adapt the notion of 'diffractive reading' to utilise these two traditions that are influential in and formative of the field of child sexual abuse.

The historical origins of psychological measurement are located in the natural sciences, particularly physics. The specific scientific model in question can be named as a classical model, aspiring to *objectivity* and ascribing to an ontological commitment to *causality*. Current research texts on child sexual abuse consistently perform measurements, as well as statistical

analyses based on those measurements, and frequently and explicitly confirm commitment to these aspects of classical scientific ontology. I argue that objectivity, as a feature of scientific measurement, produces its object of knowledge while also disciplining the conduct of researchers and counsellors. Objectivity itself is analysed as a constructed and contested practice, and I outline some competing versions of objectivity in contrast to the classic scientific model on which much psychological measurement is based.

I develop an analytical approach to this question drawing on the field of science studies on the one hand, and the Foucauldian informed disciplinary literature on the other. By understanding psychological measurement as a performative practice, rather than a neutral 'window on the world out there', it becomes less important to establish whether such measurements are strictly scientific. Instead, phenomenon of object construction and subjectification become the focus of analysis.

I draw upon published research texts, and two small focus groups I held with counsellors working in the field of child sexual abuse. I report on how these investigations led me to be troubled by the production of the object of child sexual abuse in the research texts, and the implications for the subjectification of counsellors when engaging with practices of objectivity called for by measurement.

I do not offer any prescriptive conclusions on how research or therapeutic practice ought to be done better, or propose a 'way out' of power relations for researchers and practitioners. Instead, this investigation has invited me to account as fully as possible for my entanglement in these taken for granted research and therapeutic practices.

Chapter 1- Introduction

Without statistics, scholarly conversations about social problems become virtually impossible; effectively there would be no problem, or at least not one that can be talked about in public (Reekie, 1998, p. 45)

Numbers do not merely inscribe a pre-existing reality. They constitute it (Rose, 1991, p. 676)

In this thesis, I investigate ways in which practices of measurement and statistics have shaped the current field of professional intervention and research in the field of Child Sexual Abuse.

The use of techniques of measurement (e.g. psychological testing) is mundane and commonplace in therapy and research with adults and children who have been subjected to child sexual abuse. My intention in this thesis is to disturb this sense of ordinariness associated with these practices, to highlight their contingency and make them appear 'strange'. The thesis is guided by the following questions:

1. **How have techniques of psychological measurement and statistical analyses been made to function as objective methods for determining the effects of child sexual abuse?**

1a. What relations of power/knowledge do practices of measurement require and produce? What are the 'conditions of possibility' for psychological measurement?

1b. What kinds of individuals are 'produced' by psychological practices of measurement, in the study and treatment of child sexual abuse?

2. How do techniques of measurement govern the activities of counsellors[1] in their work with children who have been sexually abused?

A major theme emerging from this research is that these practices of psychological measurement are centred around measuring the harm of sexual abuse. I will argue that such measurements produce this harm in a particular way, and require harm to be rendered in a measurable form in order to 'count'. Within a psychological discipline that commits to the scientific method, the harm of child sexual abuse is made visible through measurement. I trace the conditions that make this way of rendering the harm of child sexual abuse not only possible but to appear objective. In examining the mechanisms through which this harm is produced, the intent is not to argue that child sexual abuse is not harmful or abusive, but to interrogate the effects of the manner in which this harm is produced and treated through psychological measurement and statistical analyses.

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[1] The term 'counsellor' is the job title given to the professionals with whom part of this research was conducted. They included psychologists, social workers, an art therapist, and one who described their qualification simply as 'counsellor'. Beyond their employment which legitimates their work in this capacity, the specific educational qualification of each counsellor bears little consequence for this research.

Why focus on measurement and statistics?

As I suspect happens with many theses, the focus of this work has shifted in considerable and unexpected ways throughout the course of its production. My initial motivation (if I can refer to such a thoroughly psychological concept) emerged from a moral and ethical position about a very popular, specific aspect of knowledge about boys who had been sexually abused; namely, the notion of a 'victim to offender cycle', the idea that a boy who has been sexually abused would be at a relatively greater risk of acting abusively towards others when he reaches adolescence or adulthood. I was concerned by the fact that much research and therapeutic practice with boys who had been sexually abused seemed to accept this notion of a cycle, often under the rubric of prevention. A number of authors urged that addressing the potential for future acts of violence ought to form part of a responsible therapeutic intervention into the lives of some, if not all, boys who had been sexually abused (Ryan, 1989; Bentovim et al., 1998; Salter et al., 2003; Wilcox et al., 2004; Noll, 2005; Victorian Health Promotion Foundation, 2007).

I am not suggesting that all the literature cited posits a direct link between child sexual victimisation and adult/adolescent offending, or that it uniformly states that all boys who are sexually abused will go on to offend. Recent research has focussed on trying to identify specific circumstances or dynamics of childhood victimisation that are associated with a greater likelihood of later sexual offending. Examples of the variables studied include male v female perpetrator, familial v extra-familial perpetrator, severity and duration of the abuse, and responses to the abused child from within the support network (for a review see Thomas et al., 2009).

Nonetheless, the fact that research is devoted to such questions illustrates the persistence of the explanatory power of the 'victim to victimiser' cycle.

I believed (and still do) that this notion of a cycle—and its truth-effects—is deeply problematic for a range of reasons, not least because of the limitations such boys may be incited to place on their own lives. The victim to offender cycle is an insidious form of victim blaming and secondary victimisation, implicitly suspecting boys who have been sexually abused of being perpetrators who are yet to commit their crimes. I wanted to confront the cycle discourse and deprive it of its power to make counsellors, parents, and children (and adults abused as children) regard such children in this way. Thus, part of my motivation was to contribute to what I believed would be more liberatory and ethical forms of therapeutic practice with boys who had been sexually abused.

I also held objections to what this type of idea implied about why sexual abuse[2] happens. It seemed to me that 'the cycle' served to minimise the influence of broader political questions of gender and power. If it is thought that the cause of an individual's sexually abusive behaviour is a product of personal history, surely this invites analyses of acts of sexual violence and abuse which are individualistic and depoliticised. This grated against my commitment to the perspective informed by my reading of feminist theory and work on gender and masculinity, which broadly held that men's sexual violence is structured around inequalities of

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[2] I say 'sexual abuse' and not 'child sexual abuse' here because the 'cycle' posits these boys as potential perpetrators of a range of abusive sexual acts, not just child sexual abuse.

power between men, women and children, and the particular forms of masculinities that men and boys are incited to take up (see Boyd, 2007, for a more detailed examination of the lack of influence such theories hold in current regimes of treatment with men and boys who commit sexual abuse).

I wanted to know how the victim-to-offender cycle was able to be so influential even within those contexts where a broad feminist analysis of sexual abuse, and the importance of stressing the innocence of the child victim of abuse, was espoused. Here I am referring to the field of child sexual abuse counselling, in which I was employed at the time I began this thesis. One line of enquiry that was possible at that juncture, perhaps the most obvious one, would have been to follow *trauma*, particularly the notion of *traumatic sexuality* developed by child sexual abuse expert David Finkelhor (Finkelhor, 1988). Trauma has become the premier conceptual tool through which the effects of child sexual abuse are to be understood in research and therapeutic practice. Although the trauma path is not the one I have taken here, I believe that it would be extremely valuable to undertake an analysis of trauma's place in shaping understandings and practices in the field of child sexual abuse, perhaps taking the work of Ruth Leys (2000) as a starting point. This would stress the productivity of the concept of trauma, as opposed to establishing or debating the reality an objective referent, trauma.

I am now going to attempt to justify here why I did not take that path, by describing the path I have taken. My reading of the texts that supported the cycle discourse did not generally say that there is a one-to-one correlation between being subject to child sexual abuse and becoming a perpetrator. (There are some examples that

come close to this position; for example, Freda Briggs' *From victim to offender: How child sexual abuse victims become offenders* (1995)). More commonly, child sexual abuse was identified as a *risk factor* that may contribute to some individuals' later perpetration of sexual abuse onto other children, usually in combination with other risk factors. This is consistent with Castel's (1991) notion of the risky individual. The risky individual is distinct from the dangerous individual. Where the dangerous individual was a person deemed potentially violent or criminal based on an intimate knowledge of that person, the risky individual is more a result of a collection of various risk factors he/she has accumulated which are statistically correlated to the likelihood of offending.

'Strong' studies in this area are said to utilise "…measurement of other variables (e.g., "force") [that is] standardized, objective, and behaviourally specific" (Thomas et al., 2009, p. 383). In these texts, trauma often provides an explanation for this possible progression from victim to perpetrator, but it is the notion of risk that makes this explanation necessary. In other words, there was another technique at work, one that had already established the need for an explanation. This technique was *correlation*.

At its simplest, correlation means that if *a* happens, then *b* is more likely to happen than if *a* had not happened. If *b* happens to be a bad or undesirable thing, then the notion of *risk* might become relevant. Correlation *per* se is not a psychological concept, even though it appears everywhere in psychological texts. It is a *statistical* concept, as is risk. I became intrigued about how these statistical concepts found their way into psychology, to the extent that they seemed, not only to be everywhere in the research on the

effects of child sexual abuse, but tied to claims of objectivity. If I wanted to understand the influence of the victim to offender cycle, I would have to develop an understanding of the function of statistics. This is stated clearly in some of the research literature on how the risk of the victim-to-offender cycle should be approached for boys who have been sexually abused: "…a statistical model and, ultimately, an actuarial risk-assessment method should be aspired to" (Thomas et al., 2009, p. 385).

If statistical techniques are 'black boxes' (Latour, 1987) that are capable of producing calculations of risk (input---->[black box]------->output), where does the input come from? A black box is a metaphor for a tool for producing knowledge that, once established, becomes 'closed off' from the need for further explication. Gravity is a prime example of a black box in the field of physics. In much the same way, statistical processes work as black boxes in psychological research papers. One might find descriptions of or justifications for the use of a particular statistical technique, but it would generally be regarded as quite redundant to go back and 'open' a black box, to re-establish its legitimacy. This has become almost literal in that much statistical processing is done by computer programs; the BASC (Behavioural Assessment System for Children) which the counsellors in my research use comes with computer software.

Statistics is a number processing technology, taking the 'input' numbers and turning them into 'output' numbers. While the output numbers are a result of statistical processes or operations performed on the input, the input numbers must come from somewhere else. They come from another technique, that of

measurement. This is where things get interesting enough to form the basis of a thesis.

To recap my path so far: the victim-to-offender cycle is problematic. Why is it so influential despite these problems? Is it because of the power of the concept of trauma? This is too far down the track already- why is an explanation like trauma needed in the first place? Because statistical correlation has shown that there is an increased risk of victims becoming offenders. What kind of things are correlation and risk? They are statistical things. What do statistical things need in order to work? They need measurement.

At this point the reader may be asking why I am referring to correlation and risk as statistical 'things'. This is a central epistemological question that will get more detailed treatment in Chapter 2. At this point it might be useful to say simply that I will be treating tools for knowing as *material and constructed objects*. Statistical procedures are one such tool; psychological tests are another, as are theoretical concepts (Barad, 2007) such as trauma. None of these tools are invisible windows for seeing the effects of child sexual abuse 'as they really are', nor are they naturally occurring; they have all been built over time, either within the discipline of psychology or from other scientific disciplines.

I will go back one step: How did statistics manage to get themselves everywhere in the psychological research on the effects of child sexual abuse? By the time the effects of child sexual abuse

became a discrete area of psychological research[3], statistics were everywhere in psychology. Statistics were actively ushered in to the discipline, because statistics were established as a hallmark of science, and it was in psychology's interest to be a *science*. In terms of strategies of power, psychology historically needed statistics in order to make itself stronger in the face of doubts about the validity of the knowledge it produced (John, 1992; Michell, 1999). Statistics on the other hand would survive quite well without psychology (being firmly in place in other disciplines such as biology, physics, economics, demographics, etc.), but it would welcome the chance to expand its territory of influence. The alliance was and is productive for both psychology and statistics.

For statistics to enter psychology, it needed numbers to process. It is no use giving statistics qualitative descriptions; it needs numbers in order to work. Psychology needed to provide statistics with measurements of psychological attributes and phenomena (Comrey, 1968). The putting into practice of this requirement to measure psychological attributes was very much an *achievement*, in that it was not taken for granted or merely discovered that psychological attributes could be meaningfully measured. Michell (1999, 2011) argues that the debate over psychological measurement is still very much alive, despite the ubiquitous claims by psychology to be measuring psychological attributes.

These questions about psychology as a science, and the attendant need for measurement, led me to a range of literature falling broadly under the banner of 'science studies'. This included

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[3] The first edition of the *Journal of Child Sexual Abuse* in 1992 provides some indication of the establishment of this 'field', although research on child sexual abuse was growing throughout the 1980s. Finkelhor's *Child sexual abuse: new theory and research (1984)* provides another landmark.

feminist critiques of scientific principles such as objectivity and neutrality (e.g. (Harding, 1986); studies of the formation and construction of scientific facts and objects (Latour, 1986); Foucault's work on how some knowledge claims attain the status of objectivity or truth while others are subjugated (Foucault, 1970, 1980); and critiques of classical scientific ideals of measurement from 'newer' scientific disciplines such as quantum physics (Barad, 2007). These texts and others provided the tools to develop the first major question of this thesis:

1- How have techniques of psychological measurement and statistical analyses been made to function as objective methods for determining the effects of child sexual abuse?; and the attendant sub-questions:

1a. What relations of power/knowledge do practices of measurement require and produce? What are the 'conditions of possibility' for psychological measurement?

1b. What kinds of individuals are 'produced' by psychological practices of measurement, in the study and treatment of child sexual abuse?

On a simultaneous, secondary journey, my reading also took me into the realm that could be characterised as the "social construction" of sexual abuse. This lead to the formulation of questions such as: What does it mean for a child or an adult to be called a 'victim of child sexual abuse'? How did this come to have specific meanings, far beyond the idea of a victim as one who is assaulted? How did the victim of sexual abuse come to be constituted as an object of *psychological* expertise? I became

aware that the ability to speak about child sexual abuse as a domain of psychological expertise was contingent upon a number of historical categories. For example, there had to first be children and adults as distinct kinds of people (Ariés, 1960). There had to be a category of behaviour called 'abuse'.

There also has to exist the category of sexuality, and this domain had to be amenable to psychological knowledge. In fact, it has been argued that this category of sexuality was wholly produced by 'psy' disciplines—see Foucault (1978) and Davidson (2001). Measurement plays and has played a central role in establishing sexual norms in a scientific and objective manner, thereby producing 'abnormal' categories of sexuality to be corrected by therapeutic interventions (Downing, 2004).

There are many other categories that will be introduced throughout this thesis. Not all of these categories are exclusively psychological productions. It is unthinkable, for example, that our current discourse about child sexual abuse would be possible without the political activism of large numbers of feminist women. The framing of sexual abuse as an issue defined primarily as an abuse of male or patriarchal power remains an integral element of modern discourse that cannot be attributed to psychological categories alone: although, this definition of child sexual abuse is not always present in psychological research, and is sometimes explicitly eschewed (see, for example, Khalily et al., 2011). My interest in this thesis is primarily with those categories that allow psychological measurement and associated therapeutic practices to produce the effects of child sexual abuse as objects of scientific knowledge. Feminist writers of various persuasions have many differing things to say about psychological measurement in relation

to child sexual abuse, as well as raising questions of whether feminist discourses on child sexual abuse are 'part of the discipline' (Bell, 1993; Scott, 2001).

These insights about the historically contingent nature of child sexual abuse as an *achieved* category raised some ethical dilemmas. If one argues that the 'victim of child sexual abuse' is in some fundamental way a social achievement, does this imply the view that such people are not 'really' victims?; that the suffering instantiated by sexual abuse is merely a fiction of psychological discourses of development or trauma (or, indeed, feminist understandings of power)? This would be a deeply problematic position to take, and at no point in this thesis do I wish to minimise or dismiss the suffering experienced by individuals who have been subjected to sexual abuse. (Here is a positioning statement intended to persuade you, the reader, of that intent; during the time it has taken to produce this thesis, I have worked as a counsellor/advocate with children and adults who have been subjected to child sexual abuse, and have been a witness on a daily basis to their accounts of suffering and the subsequent legacies in their lives). Having said that, at times I have had to 'bracket' these concerns in order to pursue my enquiry into the effects of child sexual abuse as an *object of scientific enquiry*.

It is not my intention to argue for one correct or truthful frame for understanding how child sexual abuse influences a person's life. A crucial claim for the development of this thesis is that psychological studies of the effects of child sexual abuse makes these effects into *objects of science*. This claim is extremely productive for two major reasons:

1- it allows me to draw on the methodological insights of practitioners of science studies, such as Bruno Latour, Karen Barad and Sandra Harding among others, who provide guidelines as to how to study objects of science as practical (Latour), material (Barad) and political (Harding) achievements. Thus, I have referenced studies of such things as hormones, DNA and quantum physics, by following the principle of scientism (applying techniques from one area of scientific study to another- I hope that I will be allowed the play on the term 'scientific study' to incorporate the study *of* scientific activity); and

2- Child sexual abuse is contested territory in the sense that decidedly non- or anti- scientific elements of feminist practice and theory make competing claims about what child sexual abuse is and how best to understand and respond to it. This second point allows me 'somewhere to stand', a well worked out and relatively stable analytical ground through which to read these scientific practices and claims.

The purpose of the thesis

Given the sensitive nature of the topic of child sexual abuse, I want to make one immediate clarification. This research is not intended to support the idea that 'child sexual abuse' is simply a fictional construct of psychological discourse, or that (consequently) the abuse (or use) of children, by adults, for sexual purposes should be legitimated. Rather, the general purpose is to interrogate the disciplinary functions of contemporary forms of knowledge about child sexual abuse. This will involve questioning some taken-for-granted assumptions about child sexual abuse, its impacts and how these are researched, and therapeutic responses to sexually abused

children and adults who were sexually abused as children. It involves recognising that 'psy' knowledge about sexual abuse, and expert responses to it, are just as infused with relations of power as abuse itself, albeit of a different kind.

Another way of saying this is that I experience two co-existing responses to the problem of child sexual abuse. I hold a deep seated conviction that a whole range of actions named as child sexual abuse can contribute to immense distress, suffering and pain to a great number of individuals, sometimes for the course of a person's entire life. At the same time, I hold a radical uncertainty about how this ought to be understood and responded to by people such as myself whose professional positions authorise us to act in the name of therapy and/or research. In naming this uncertainty, it is not my aim to become more certain. In many ways, I believe radical uncertainty to be an ethically helpful position when considering how to act in realms that may entail real effects for people who experience ongoing suffering and distress. This connects to some of the substantive issues raised throughout this thesis. It is a truism stated by some feminist critiques of science; that attempts to impose understanding and order can be a form of violence in their own right, and often serve the interests of those doing the ordering. In this thesis, I am interested in producing problems, not answers. I attempt to de-order, or destabilise, ways of knowing and acting in response to child sexual abuse which operate in influential ways in the contemporary professional field.

The emergence of child sexual abuse

Child sexual abuse has not always been recognised as a problem in Australian society or other 'Western' societies. The 'uncovering' of child sexual abuse is usually attributed to both Freud (who has also been the focus of some strong attacks that claim he later backed away from his original 'seduction theory' to protect his professional status (Masson, 1984)), and to the women's movement, especially of the 1970s.[4]

I argue in this thesis that the effects of child sexual abuse have become an object of legitimate scientific study and therapeutic practice for professionals. Claims from professionals that child sexual abuse does not exist or is not harmful are rare in 2012. In contrast, in 1984 the Australian researcher Dr Jocelyn Scutt could say, in a report commissioned by the then South Australian Government Minister of Health J.R. Cornwall, that there was a very different professional response to child sexual abuse:

> *"...a deliberate refusal of those in positions of power to acknowledge the truth. A "truth" manufactured by psychologists, psychiatrists, lawyers, counsellors and other "experts" has made for more comfortable living—for them....It is easier for those in positions of power to believe that incest does not occur" (Scutt, 1984, p.iii).*

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[4] In Australia since white invasion, issues of sexual assault more broadly have a longer history. The management of adult male rape was an important concern for administrators of early penal colonies- see Foster, 2005.

Since that time, both research into child sexual abuse, and therapeutic services for those subject to such abuse, have become part of the professional landscape for social workers and psychologists in Australia. In Melbourne, Victoria, CASAs (Centres Against Sexual Assault) provided therapeutic services for children who were the victims of recent sexual abuse since the late 1970s, and today receive funding from the Victorian State government to provide this service in many regions across the state of Victoria, as well as services to adults who were sexually abused in their childhood. Also in Melbourne, in 1986 Australians Against Child Abuse (now the Australian Childhood Foundation- ACF), started providing counselling for children who had been sexually abused (Worth, 2008). The Child Sexual Abuse Treatment program was established by the Children's Protection Society in 1993 (Scott et al., 2002), now still operating as the Sexual Abuse Counselling and Prevention Program (SACPP), providing therapeutic services for children and adolescents[5]. The establishment of these services reflects that recognition of the problem of child sexual abuse was a joint effort of child protection advocates and the women's movement.

In terms of recognising child sexual abuse as a legitimate topic for research and evidence collection, the National Child Protection Clearinghouse (NCPC) was established by the Australian Government Department of Families, Housing, Community Services and Indigenous Affairs in 1995, with child sexual abuse being one of the areas of child abuse identified (http://www.aifs.gov.au/nch/about.html). In 2003, the Australian

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[5] Some CASA's, the SACPP program, and ACF now also provide therapeutic services to adolescents who have sexually abused other children, SACCP and ACF since the mid-late 1990's and CASA's from the mid 2000's (Worth, 2008).

Government, through the Office of the Status of Women, established the Australian Centre for the Study of Sexual Assault (ACSSA: http://www.aifs.gov.au/acssa/about.html). With a primary focus on issues related to the sexual assault of women and girls over the age of 15, ACSSA also identifies "adult survivors of childhood sexual abuse" as a "key area of interest" (Tomison, 2003). Both ACSSA and NCPC are situated in the Australian Institute of Family Studies. Internationally, the peer-reviewed *Journal of Child Sexual Abuse* was first published in 1992, although individual research papers were being published earlier than this in other psychology journals. The work of David Finkelhor is widely regarded as reference point for the establishment of the contemporary field of psychological research on child sexual abuse (Finkelhor, 1984, 1986).

These achievements are the result of both social/political activism (primarily the 'women's movement') to situate child sexual abuse as a crime and an abuse of adult (male) power over children, and the efforts of researchers to establish not only the prevalence of child sexual abuse, but the harm that it can cause.

In regards to the latter question, there is evidence of an epistemological shift in how the harm of child sexual abuse is to be established, and this shift involves psychological measurement and statistics. This shift has been accompanied by resistance and tensions within the field. Understanding these is the primary focus of this thesis.

Two controversies

To illustrate the problematic context of the role of measurement and statistics in child sexual abuse research, I will briefly discuss two exchanges that ran in two separate journals during the 1990s. Each event produces a slightly different problem regarding measuring the effects of child sexual abuse.

a- Russell and Levett

In the South African feminist journal *Agenda*, a series of articles appeared in response to an initial paper by established researcher Ann Levett[6] (Levett, 1990), which she stated was a "postpositivist deconstruction" (p. 38) of the ways that the trauma of childhood sexual abuse was commonly researched and understood, particularly in relation to normative ideas of development and the accompanying neglect of socio-political factors that shape identity and inform sexual abuse. She was concerned to question whether "…certain kinds of research, intended to be humanitarian and progressive, can also actually serve as an ideological tool which perpetuates oppressive social structure" (p. 38). She noted that "The notion that some children and adults are reported to be unaffected by the experience of sexual abuse is not popular and is unusual within the current literature" (p. 43), an observation that carries particular prescience in the light of the next controversy I discuss (Rind, Tromovitch and Bauserman, 1998).[7]

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[6] Ann Levett sadly passed away at the age of 70 in 2006, one year after I began this thesis.

[7] However, I would argue that Levett and Rind et al would have differed on the meaning they ascribed to this observation.

Levett's article prompted an 'outraged' response from Diana Russell (Russell, 1991), who argued that Levett's work 'trivialized' the suffering of women and children who had been sexually abused. Russell's response was based largely on her perception that Levett was arguing that many women who have been subjected to child sexual abuse are not harmed or damaged ('damaged' becomes a problematic word in this debate). Whether or not Russell's reading of Levett is correct is not my main concern here (although I tend to agree with Levett (Levett, 1992) that Russell is situated within an entirely different epistemological tradition which results in her having "…completely failed to grasp the central arguments of my research" (Levett, 1992, p. 68)). It is in this article that Russell began to define what constitutes 'quality' research from poor research, suggesting that Levett failed to discriminate between them. This begins to crystallise the relevance of this debate for my thesis. Russell stated: "I do not know of a single study in which an adequate control group is used which fails to find sexual abuse and those who did not (sic). In my own study, several statistically significant differences emerged when I compared those women who reported an experience of incestuous abuse with those who did not" (Russell, 1991, p. 49). Statistical concepts—control groups, statistical significance—are introduced as indicators of 'superior quality research'. Russell also mentions other statistically derived research tools she uses in her own research, (e.g. probability samples).

In her response, Levett (1992) unpacks the notion of 'facts' as they are presented in Russell's research, and unsettles the idea that facts are transparent, referring to real things and identifiable by empirical means. Part of this epistemological package includes committing to cause-effect relationships. "Establishing cause-

effect chains is an important focus of this framework: here is sexual abuse, there is the trauma. Such logical positivistic research has taken us a good way along the road in contemporary technological achievements and to statistically inferred relative 'truths' about social conditions" (Levett, 1992, p. 70). Levett names "objective rationality" as a part of this Enlightenment-tradition way of knowing, and links this to New Right conservatism. Citing Foucault, Levett characterises such empirical research as a dominant discourse, producing its own, unreflective regime of truth.[8]

Levett cites the critiques of feminist science studies authors such as Sandra Harding who "…have identified liberal, positivistic research as fundamentally patriarchal" (1992, p. 71), although stops short of saying that Russell's work itself is patriarchal (which would, obviously, be the harshest of insults). However, there is some suspicion regarding the use of these 'positivistic' methods for feminist purposes, and Russell (1993) reported her experience of a generalised tendency within strands of feminism to be suspicious of scientific and quantitative research (see also Oakley, 2000).

In the context of the *Agenda* debate, Ann Mayne interviewed prominent UK feminist Liz Kelly to provide a commentary on the exchange (Mayne, 1993), in which one of Kelly's criticism was Russell's reliance on statistical and quantitative research methods. As Russell (1993) points out, the criticism of her work was not that she made 'bad' or mistaken use of statistical methods, but simply

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[8] This itself becomes a source of further dispute- in 1992, was the claim that child sexual abuse is harmful a 'dominant discourse'? Levett clearly states it is, Russell would perhaps agree with Jocelyn Scutt's comments cited earlier.

that she used quantitative methods at all to research sexual abuse. Russell described encountering a "…virtual consensus that quantitative research is male and patriarchal" (Russell, 1993, f.n.5) within some feminist study circles.

I will leave Russell and Levett for now, having shown that debates about measurement and statistics in child sexual abuse research (and their implications for therapy) have something of a history within feminist-informed work. The issues I want to highlight here are: the association of quantitative methods with patriarchy; the presumption that questioning research about the harmful effects of child sexual abuse can lead to accusations of minimising or trivialising suffering; and the problematic place of facts, objectivity and cause-and-effect ontology.

b- Rind,Tromovitch and Bauserman (1998)

The second 'controversy' is in relation to the Rind et al. meta-analysis of child sexual abuse studies (Rind et al., 1998). This paper generated disputes explicitly centred on the themes of science, measurement and the impacts of child sexual abuse in psychological research. Rind and his colleagues conducted a meta-analysis of 59 studies using college samples, on the long-term impacts of child sexual abuse. Rind et al. found that statistically, across the 59 studies reviewed, the long term negative effects (harm) of 'childhood sexual encounters with adults' (sic) were not as inevitable or substantial as commonly stated. In addition, they found that much of the effect could be attributed to other problematic events or situations (e.g. having been subject to physical abuse as a child), not just child sexual abuse. The article

was published in a prestigious academic journal *Psychological Bulletin*, a journal of the American Psychological Association. Subsequently, after a rather long and intriguing set of events that makes for a story in its own right, the study was condemned by the United States Congress (see Lilienfeld, 2002 for an extended discussion; Davis, 2005, p. 285, footnote 20). The Rind et al. paper and the ensuing texts which commented on it constituted a significant moment in the use of scientific and statistical methods to research the harm of child sexual abuse.

In contrast to the debate between Russell and Levett, the Rind et al. study reverberated well beyond the pages of a specialist journal. They were not criticised for their use of patriarchal methods; however (like Levett), they were accused of minimising the long term harm of child sexual abuse, and even of attempting to justify child sexual abuse. In relation to this last point, it presumably did not reflect well on the authors when the North American Man-Boy Love Association promoted the study's conclusions (Lilienfeld, 2002). Rind et al. were also criticised from within the scientific community, where the focus was more on whether the methods used actually were scientific, rather than the contentious nature of the conclusions.

The perceived potential that the study could be used to argue for the legitimisation of child sexual abuse made it the subject of ethical and moral condemnation from within the discipline of psychology and from external sources, including political and religious bodies. Rind et al. had proposed that:

A willing encounter with positive reactions would be labelled simply *adult–child sex,* a value-neutral term. If a young person felt

that he or she did not freely participate in the encounter and if he or she experienced negative reactions to it, then *child sexual abuse,* a term that implies harm to the individual, would be valid (Rind et al., 1998, p. 46)

The informative phrase here is 'value-neutral term', placed alongside the phrase "adult-child sex". It seems odd to even encounter the suggestion that 'adult-child sex' would be read as a value neutral term, regardless of the child's apparent consent. Children cannot give consent to sexual acts with adults, whether from a discourse of morality, legality, or developmentalism. This highlights the problems of consequentialism or the scientific principle of causality; because the harm of the adult-child sexual encounter was found to be often minimal or sometimes positive in Rind et al.'s study (and the child 'consented'), they redefined the encounter as not abusive.

In response to the criticisms they received, Rind et al. cited the well-known child sexual abuse research pioneers Brown and Finkelhor (Brown and Finkelhor, 1986, cited in Rind et al., 2000), warning against the exaggeration of the effects of child sexual abuse. In the cited quotation, Brown and Finkelhor had explicitly advocated that child sexual researchers adopt a "…posture of *objectivity* and balance" (ibid., p. 4, my emphasis).

Rind et al.'s comments are based on a definition of abuse they claim is scientific:

> *"In science, abuse implies that particular actions or*
> *inactions of an intentional nature are likely to cause*
> *harm to an individual…Classifying a behaviour as*

abuse simply because it is generally viewed as immoral or defined as illegal is problematic, because such a classification may obscure the true nature of the behaviour and its actual causes and effects" (Rind et. al., 1998, p. 45).

This is consistent with Canguilhelm's identification of causality as a requisite feature in conventions of scientific explanation (Canguilhelm, 1978). Rind et al. go on to illustrate their point by highlighting historical examples of sexual behaviours that have been viewed as pathological: "…masturbation, homosexuality, fellatio, cunnilingus, and sexual promiscuity" (1998, p. 45) and mentioning that masturbation has been previously called 'self-abuse'. They asserted that the moral condemnation of these categories has been overcome by scientific research demonstrating that they do not have harmful effects.

The study attracted attention not only from within psychology; some of the criticisms of the study emerged from outside the field of psychology. Rind et al. (2000) point out that some of the loudest critics of this aspect of their study were socially conservative groups and individuals interested in protecting values associated with the nuclear family, who seemed primarily concerned with what they perceived as the apparent normalisation of pedophilia, and by association through the notion of perversion, homosexuality. However, it is mainly the within-discipline critiques that I am most interested in here.

The criticism from other psychologists and researchers centered on Rind et al.'s selection of samples, the psychological constructs they analyzed, and the statistical techniques employed (Dallam,

2001; Dallam et al., 2001). At stake was whether or not the Rind study was scientific. Stephanie Dallam concluded that the study was "…an advocacy article that inappropriately uses science in an attempt to legitimize its findings" (Dallam, 2001). Ironically, this is precisely what Rind et al. had claimed of previous studies which they argued over-stated the effects of child sexual abuse.

What interests me in this thesis is the claim that child sexual abuse research should be scientific. I will attempt to draw out what this claim means, and identify specific practices and principles that qualify as scientific. Through this, I present an argument about the privileged role of psychological measurement and statistical methods in child sexual abuse research.

Two main points are of interest, which I will outline briefly here. Firstly, Rind et al. (2000) claimed that consequentialism or *causality* is the only scientifically valid principle for assessing whether an action should be considered abusive or not. Another way of saying this is that scientific enquiry is in the business of establishing causal relationships between events or phenomena. If child sexual abuse does have harmful consequences, scientific enquiry should reveal a causal connection. Given that their study revealed limited long term negative consequences of child sexual abuse, Rind et al. advocated for a review of what constitutes child sexual abuse.

Secondly, *objectivity* is frequently evoked in child sexual abuse research. While objectivity is prized as a sign of validity and accuracy, it is rarely defined. I argue that the use of psychological measurement often serves as a proxy for objectivity in psychological child sexual abuse research. I intend to examine this

connection more closely, both in terms of the production of knowledge about child sexual abuse, and the conduct of researchers and practitioners in their work.

Somewhat unusually, given their commitment to scientific method, Rind et al. (2000) make the argument that "the true nature" of abuse cannot be decided upon by any objective measure, that such matters are always matters of political and social contestation. They cite a list of historically situated constructions of sexual behaviours and the knowledge/power relations that governed them (for example, homosexuality and masturbation). Unless the reader is invested in a narrative of scientific progress towards ever more truthful understanding (and perhaps Rind et al. assume this to be the case), their list simply evidences the contingent nature of scientific claims regarding sexuality, which would seem to undermine their project, which is precisely to establish objectively the true impacts of child sexual abuse.

Taking the Rind et al. controversy and the Russell/Levett debate together, the themes of objectivity and causality appear as important constructs. The perspectives on these constructs are slightly different across the two sets of texts. In the Rind et al. texts, objectivity (mostly) appears as a desirable scientific practice that should be adhered to by researchers, particularly from 'within discipline' (as opposed to those coming from religious groups). Yet, whilst emphasising the importance of objectivity, Rind et al. (2000) also state that child sexual abuse cannot be understood through objective means. Levett's and Russell's exchange adds another dimension to the problem of objectivity, in that objectivity and measurement are discussed in terms of whether or not they are patriarchal.

What interests me in this thesis is how objectivity is accorded such an important position. I argue that psychological measurement in the field of child sexual abuse research has flourished partly because of this commitment to objectivity.

The construct of causality likewise supports practices of measurement. If child sexual abuse causes harm, the way of establishing this cause-effect relationship is through measuring psychological attributes before and after, as it were. Given before and after measurements are not feasible in this case, substitute statistical means such as comparison to a norm or control group are used. The point of interest here is that commitment to, or resistance to, the scientific principle of causality shapes research practice.

The harm story

> *As we sift through and try to make sense of the suffering to which we are called on to respond, we implicitly and explicitly sort out, measure, and give shape to it (Spelman, 1997, p. 1)*

The developmental consequences of childhood sexual abuse have been the subject of enormous amounts of research. A meta-analysis by Paolucci and Genuis (2001) found 860 articles and conference papers published between 1976-1996 investigating the effects of childhood sexual abuse. Maniglio, in his review of reviews published between 1995 and 2008 on the health impacts of child sexual abuse, found reviews of 587 studies on the topic (Maniglio, 2009). A common focus of such studies is to 'document and

catalogue' the harmful effects of child sexual abuse, following the program suggested by David Finkelhor (cited in Davis, 2005).

O'Dell critiques the universalism implicit in what she dubs 'the harm story', arguing that it obliterates differences of class, gender, sexuality and ethnicity (and individual agency). She states: "[B]y storying the harmfulness of child sexual abuse through a discourse of 'development', women, children and men affected by child sexual abuse are positioned as remaining a product of their past abusive experiences" (O'Dell, 2003, p. 132). She argues that 'the harm story' frames all choices after abuse, especially choices about sexuality that are constructed as deviant (e.g. homosexuality, celibacy, promiscuity) as *effects of the abuse*, rather than as "conscious, informed decisions". Such choices become symptoms of pathology, and further, deviant sexuality may be constructed as being 'transmitted' by sexual abuse. Children can become stigmatised for their loss of 'innocence' (Kitzinger, 1992, cited in O'Dell, 2003, p. 138; Davis, 2005), as their experience of abuse marks them as having deviated from the course of normal, healthy development. Such children have historically been managed in child protection/welfare systems in terms of the risk they may pose to the innocence (read 'childhood') of other children (Smart, 1999).

It is possible that psychological discourse would find nothing in this analysis to object to, and would claim this deviance as the proper territory for its own practice. It is perhaps too broad to argue that psychological knowledge about children's development does not "highlight particularity and difference" (Burman, 1992, p. 48), as suggested by this critique. The reverse position is fruitful

for analysis; psychological techniques of measurement and assessment which establish the norm also *produce* difference (Meadmore, 1993). The difference produced by developmental psychology is not the same difference which is celebrated by O'Dell and other feminist authors influenced by various strands of post-structuralism or constructivism (e.g. Levett, 1990; Burman, 1992; Warner, 2009); it is a deviant difference, or an indication of a self that is in some way deficient (Gergen, 2007). However, this deviant/deficient difference is no less valued by developmental psychology, as it provides a legitimate object on which psychological expertise can operate. In this sense, particularity and difference are essential to the psychological enterprise. The effect of this psychological expertise is first to produce difference, to evaluate this difference as deficiency or deviance, then to normalise it; psychological expertise is necessary for both aspects of this work.

To reiterate, what is under contention in these two discourses about difference is the meaning to be made of difference. In O'Dell's narrative, psychology and the harm story obliterates a diversity which cannot (or ought not) be conceptualised in terms of pathology. In the 'psy' narrative, difference is produced in order to be normalised. It is important to state here that both the critique offered by O'Dell, and the analytic of developmentalism, posit (different) normative ideals of childhood. The distinction, however, is that the developmental approach attempts to naturalise its subject normal child, whereas the critical approach acknowledges childhood and the impacts of child sexual abuse as products of discourse, power and resistance.

It has been convincingly argued by some feminist authors that the field of child sexual abuse treatment, in its deployment of developmental psychology, has produced a discourse and practice of therapeutic concern lacking in critical political analysis. They state that the co-opting of feminist discourses of "choice, power and liberation" by therapeutic discourse has resulted in responses to child sexual abuse that are depoliticised (Armstrong, 1996; Lamb, 1996; O'Dell, 2003) and individualised (Warner, 2009). An analytics of discipline would complement this argument, by pointing to the manner in which political problems of control are recast as technical, scientific problems to be addressed by experts (Hook, 2007). A politicised approach to understanding child sexual abuse considers power relations between adults and children, highlighting the extent to which adults are considerably more powerful than children thus rendering sexual 'relations' inherently abusive. Child sexual abuse is problematic because it is an abuse of power. This makes room for analysing adult-child relations by bringing attention to the strategies that children employ to resist or utilise power relations. This position produces children who are active agents, as users of discourse and subjects within relations of power (Burman, 1992), while still leaving room to acknowledge that adult-child relations are contingently, though perhaps not essentially, relations of inequality.

The developmental paradigm and the harm story have effects beyond childhood; as well as attributing to childhood sexual abuse a causal status in current actions and lives of adults, there are broader political issues at stake. The harm story tends to exclude critical consideration of dynamics of power and gender.

If child sexual abuse continues to act as a causal
narrative in accounts of sexual violence and
problematic sexual relations in adult life, the very
organisation of heterosexuality remains unexamined,
and men and women's subject positions are
'naturalised' (Butler, 1993) within a regulatory notion
of heteronormativity (Reavey, 2003, p. 157)

The notion that adult women and men who fail to conform to such
powerful regulatory ideals must in some way be 'damaged' or
deficient is supported by the harm story. The irony for Reavey is
that it is precisely 'normal' arrangements of heterosexuality that
produce the gendered relations of dominance that are conducive to
sexualised violence and abuse.

Victim of child sexual abuse as a category

Sexually abused children and adults who had been sexually abused
in childhood surely existed prior to the psychological invention of
these categories or 'kinds' (Hacking, 1995). Yet when child sexual
abuse as a problem crossed the "threshold of scientificity"
(Foucault, 1972), a different way of conceptualising (and thus
governing) these children—and later, adults—became possible. In
much the same way that Davidson states that there were no
'perverts' prior to a psychiatric style of reasoning in the late
nineteenth century (Davidson, 2001), our contemporary
psychological subjects 'the child victim of sexual abuse', or 'the
adult victim/survivor of child sexual abuse' were not objects of
psychological knowledge prior to the psychologisation of child
sexual abuse. It is important to note that the survivor of sexual

abuse also became a political category produced through radical feminist theory and political activism on men's sexual violence towards women and children (Warner, 2009).

The harm story, which frames sexual abuse of children as harmful due to the adverse impact it has on the child's development (including into his/her adulthood), was strategically crucial to the aim of establishing the criminality and seriousness of child sexual abuse. Carol Smart, in her work on the historical discursive struggles over child sexual abuse within UK legal and medical discourses between 1910 and 1960 (Smart, 1999), shows how feminist legal and medical activists deployed the harm story to try and protect children from sexual abuse; these efforts met with resistance from the 'male establishment', within the legal profession particularly. Citing archival documents from the period, Smart shows how the claim that sexual abuse (or what we now consider to be sexual abuse) is harmful to children was not easily accepted. There were a number of competing discourses about girls and sexuality in particular, such that girls were often described as 'wicked' and inciting sexual attacks from boys and men. Another discourse about children was that childhood was not a particularly important or formative stage of life; "childhood was a phase of both resilience and insignificance" (Smart, 1999, p. 403). The effect of this was that children, and especially working-class girls (who were often the victims in cases of sexual crimes), did not matter a great deal, especially in relation to the rights of the breadwinning men accused of the crimes (Smart, 1999).

Paradoxically, whilst childhood itself was not seen to be of great significance, the sexually abused girl was regarded as a potential source of moral contagion to her peers, likened to a leper (Smart,

1999). It was not lawyers who expressed concern about contagion, but the 'rescue workers' concerned with the welfare of children.[9] Such children were no longer 'innocent' and so protecting them became less of a concern than protecting other children from them. "In this discourse, the abuse of the child was recognised as harmful, but the victim of the abuse became a non-person as a consequence of the harm done to her" (*ibid:* 404). This difficulty of understanding harm and innocence in the context of protecting children was of crucial concern in the 1920s-1930s amongst reformers. The evidence of this struggle is apparent in the contemporary context, with workers speaking about 'protecting the innocence' of the sexually abused children they work with, meaning that they attempt not to introduce sexual knowledge to children of which the child was previously unaware.

The social category and the psychological category were not the same thing, and in a process that Hacking refers to as the 'looping effect' (Hacking, 1995) the two categories (or, the kind and knowledge of the kind) mutually interact and change each other. Perhaps some sexually abused children are unaware of the social implications of their clinical status, although this is far from certain (Mudaly et al., 2006). But there are profound effects for such children in terms of the kinds of responses and treatment they elicit from others. For example, the parent or carer of the sexually abused child, or child who experiences or witnesses other forms of interpersonal violence (especially boys), is subject to a moral incitation to seek intervention in the interests of preventing future violence. The notion that the child sexual abuse victim is a risky individual (Castel, 1991) traverses this field in a myriad of ways,

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[9] Such concerns continue to circulate in current debates about children in residential homes.

including in the victim-to-offender cycle for boys. For a local example, see the document *Preventing violence before it occurs* (Victorian Health Promotion Foundation, 2007), where the parents of boy victims of child abuse (including sexual abuse) are incited to seek therapy with the goal of preventing the boy from committing violence against women in the future.

Additionally, adults who have been sexually abused as children have available to them an identity construction - the adult survivor of childhood sexual abuse - which offers its own life-shaping narrative of 'victim-survivor-thriver' (Armstrong, 1996; Davis, 2005). Along with this identity comes what Rose calls 'responsibilization' (Rose, 1999); the moral obligation to take up expert knowledge and act upon oneself.

Thus, the harm story has historical and current strategic importance in problematising child sexual abuse and establishing it as an issue of public concern. It also has problematic, if unintended, consequences of its own for children and adults, and for broader understandings of sexual abuse and sexuality.

Some initial comments regarding the trajectory of this thesis

In attempting this thesis I have tried to remain attentive to the shifts in my own analytic approach. I have not started with a solid, immovable structure or framework and proceeded to analyse my problems from a single viewpoint. Much of the theoretical material I have drawn upon demands being open to engagement, including the possibility of myself being changed. This is partly a feature of work often categorised under the banner of 'post-structuralism' or

constructivist theory, but it also derives more directly from my chosen topic. A key question I have sought to understand is regarding measurement, including scientific practices of measurement. Exploration of this question led me to the notion of 'diffractive reading' (Barad, 2007), which is a practice drawn from work in the field of physics. Physics is, in an historical sense, the disciplinary home of measurement, particularly for psychology. Diffractive reading demands an acknowledgement of the 'entanglement' inherent in measurement, but also demands the same of *any* attempt to understand the world. This necessarily includes my own attempts in this thesis. In this next section, I will outline some points regarding both constructivist theory around the problem of child sexual abuse, and the notion of diffractive reading. This is with the hope of preparing the reader for the sense of instability and movement that occurs throughout the body of the thesis.

Potential problems of constructivist approaches to the study of child sexual abuse

As Hacking (1999) points out, to say that something is a 'construction' is not necessarily to say it is bad or false (although this often is the agenda of constructivist work). Scientific knowledge is constructed, and this insight does not demand that the knowledge it produces be reject or opposed (Latour, 1986, 1987, 1999, 2005). Thus, as a social and scientific achievement, the recognition of child sexual abuse as a problem, as a traumatic experience, allows for the legitimation of claims of suffering by individuals subject to abuse (Davis, 2005). The fact that this has been achieved through activism and through research does not make it any less real or important.

This thesis does not seek to question whether this legitimacy is a 'good thing' (I do think *recognition* of abuse and the suffering it can contribute to is a good thing). Nonetheless, legitimation does come with potentially unforeseen and unintended consequences. Judith Herman makes a similar point in *Trauma and Recovery* (Herman, 2004)*,* when she points out that war veterans fought to have the harm of their experiences recognised through the legitimation of PTSD, at the cost of submitting to the authority of the expert gaze of psychiatry, and an acceptance of an identity as a 'sick' person (see also Young, 1995).

Hacking points out the distinction between child abuse and the *concept* of child abuse, one having always been around and the latter responsive to and constituted by what is said and done (Hacking, 1991). That is to say, children have no doubt always been (what is now called) sexually abused throughout history and across cultures, even though the term itself may be relatively recent; but what qualifies (in law, in sociology, in the 'psy' disciplines) as child sexual abuse, what is said and done about acts so categorised, and what is done to the victims and perpetrators of the acts, is manifestly subject to a great deal of change. It is one thing to say that children ought not to be abused by adults (providing some agreement can be made about what this means; for an example of the difficulty of this see (Archard, 1999)); it is something different to say that such children (and later, adults) ought to be the focus of psychological intervention for their own good (Armstrong, 1996; Rogerson, 2001; Warner, 2009).

In this thesis, I am interested in how the effects of child sexual abuse become available for scientific study. I have no interest in

questioning the fact that child sexual abuse occurs, or that it impacts on the lives of many people. Nonetheless, upon engaging with the constructivist literature around child sexual abuse, one is confronted with a jarring question: Why is it harmful or wrong for an adult to interact sexually with a child? What makes it 'abuse'? Indeed, what is child sexual abuse? While these questions seem almost too obvious to warrant an answer, they must be engaged with if one intends to utilise the theoretical insights of the constructivist literature. After all it is the calling into question of the 'taken for granted' that is one of the hallmarks of social constructivist criticism (Hacking, 2004).

There is more than one way of saying *why* child sexual abuse is a problem. I attempt to show in this thesis that within the scientific discipline of psychology, child sexual abuse is abuse if it *causes* harm and this harm can be demonstrated. This scientific principle of causality is similar to the ethical principle of consequentialism. In contrast, a deontological ethical position is concerned primarily with the act itself (Slaney, 2001; Lilienfeld, 2002). This invites interrogation of the 'harm story' (O'Dell, 2003) of sexual abuse. In this story, all child sexual abuse is always harmful because it disrupts or distorts the natural or normal development of a child (Anderson, 2008). A critical engagement with the harm story prioritises different issues.

When the historically contingent nature of current practice and knowledge (discourse) concerning child sexual abuse is recognised, some care is needed not to imply that the 'constructed' nature of all these categories renders them somehow less real. To quote Nicola Gavey's position on social constructionist arguments about sexual abuse,

*...I would counter that these constructionist
arguments do not imply that the suffering caused by
sexual abuse is not real; but they do suggest that the
particular shape of this suffering, and the form of its
ongoing incorporation into any person's identity is also
shaped by the cultural frameworks available for
making sense of the impacts of sexual abuse (Gavey,
2003, p. 203)*

I am interested in the 'also' of Gavey's statement. Does this imply
a *pre-discursive* suffering that functions prior to being thought or
made sense of ('shaped by cultural frameworks')? On what basis
(either ontologically or ethically) can this pre-discursive suffering
be distinguished from that 'shaped by cultural frameworks'?
Perhaps it is not a case of either/or, and in fact the suggestion of an
either/or may itself be a legacy of a constructed nature/culture
distinction (Butler, 1993; Oksala, 2005).

In summary, because this thesis joins with the problematisation of
this equation of normal with natural/healthy/unharmed, and
because deviance from the norm is the established method of
framing the harm of child sexual abuse, alternative theoretical and
ethical arguments against the sexual abuse of children are needed,
without recourse to naturalistic or essentialist ideas about the
nature of bodies, childhood and harm. To put it another way, the
question is how to understand the harm of sexual abuse, if what is
regarded as 'healthy' or 'normal' is, in first instance (i.e. 'pre-

abuse'), already founded on a kind of violence, the violence of discourse and regulatory norms.

Reflection and diffraction

Given that an important element of some feminist critiques of science is the perceived claim of a 'view from nowhere' inherent in scientific method, the identification of two distinct positions through which to understand child sexual abuse (that I, not unproblematically, call post-structuralist feminism and scientific psychology) allows for what Barad (2007) calls a 'diffractive reading'. The two traditions can be read through each other; post-structuralist feminist knowledge claims about child sexual abuse can be read through scientific practices and vice-versa. What I try to avoid is to claim for myself an 'objective' position (however, there are some useful re-workings of objectivity that will be explored which differ somewhat from the classical notion of objectivity).

Having just stated that there may be somewhere stable to stand, I need to immediately contradict myself and warn the reader that each attempt I make to stabilise a point of reference comes undone to some extent as this thesis proceeds. This can be disorientating and frustrating, however I will attempt here to give an explanation of this process. I have been influenced by the analytical metaphor/tool of diffraction, which entails ethical, epistemological and ontological implications (or, as Barad (2007) would have it, demands 'ethico-onto-epistemic' attention).

What do I mean by diffraction? The intra-actions between topics, styles of analysis, authors and points of reference produce what I have conceptualised as diffraction patterns (after Barad, 2007). With each encounter a series of effects is facilitated. My hope has been to remain open and alert to these effects, as opposed to the practice of imposing a fixed structure or framework upon a multiplicity of thoughts and practices. Diffraction insists that we acknowledge the entanglement of ourselves and our objects of study, within our attempts to investigate the world. My attempts to understand the problems I pose in this thesis change both me and the problems. According to an ethics of diffraction, I (and my frameworks for enquiry) cannot remain unchanged by my encounters, any more than the effects of child sexual abuse could remain stable regardless of the investigative methods used to produce them as an object of enquiry. The ethical imperative is to attempt to account for these changes.

For social workers in particular, a useful counter-point to the notion of diffraction is that of reflection. Reflective practice (or reflexivity)[10] is regarded as an essential social work skill. Reflection and diffraction are both optical/visual metaphors. However, diffraction also conveys a sense of embodiment, the felt instability produced by encounters with conflicting, competing, complementary and co-existing forces.   Reflexivity is widely held to be a distinguishing feature of the human sciences that demarcates it from the natural sciences. However, such a statement foregoes the extent to which reflexivity itself can be historicised, to be "reflexive about reflexivity itself" (Smith, 2005, p. 1), the task

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[10] It would be possible to delineate diverse meanings for these terms (reflective practice and reflexivity). I have chosen not to because; a) as noted by D'Cruz et al. (2007), the terms are often used interchangeably in the literature; and b) both terms convey the visual sense I am interested in here.

undertaken by Foucault in *The order of things* (Foucault, 1970; Smith, 2005). Morawski (2005) argues that despite some attempts by a small number of psychologists, the discipline in general has displayed a "disregard for the problems of reflexivity" (p. 78); although others argue that reflexivity has been an important part of psychological research through the history of the discipline (Cohen-Cole, 2005). Social work has its own traditions of reflexivity—even if it is not always clear precisely what this term might mean—such that reflexive practice is widely regarded as a fundamental social work skill (D'Cruz et al., 2007).

Reflective practice in the helping professions is sometimes described in contrast or opposition to the supposedly automaton-style of quantitative knowledge (e.g. Taylor et al., 2000). This would be one way to critique psychological measurement, however I suggest this would be inadequate. I suggest that it is inaccurate to state that psychological measurement is an unreflective practice, either in research or practice settings. This applies in a superficial way to the scrutiny which any instruments themselves undergo before becoming accepted, as well as to the practice of performing measurement. It is in fact only after lengthy and detailed scrutiny that any particular instrument becomes accepted in a field of research. Instruments used to measure children's trauma responses, for example, are scrutinised and compared to each other for their reliability and validity (Crouch et al., 1999). A close reading of the research papers that undertake quantitative knowledge making do, in fact, evince a kind of reflexivity in the conduct of measurement (I explore this more closely in Chapter 7).

More cogently, against the notion that measurement is not a reflective practice, within Barad's schema of reflection/diffraction,

measurement relies precisely on the metaphor on reflection. It requires that one stands at a distance from an object, and the use of a measuring device (in the case of this thesis, a psychometric test) is part of the enactment of this distance or cut (Barad, 2007), used in order to see things as they really are.

One of the meanings of reflexivity identified by D'Cruz, *et. al.* (2007) is "a critical approach to professional practice that questions how knowledge is generated and, further, how relations of power influence the process of knowledge generation" (p. 77). When professionals speak, they are actively 'making knowledge' (Taylor et al., 2000). This gets at part of the kind of analysis I am attempting here, but misses the question of how this knowledge/power nexus constitutes *counsellor* as a subject position. What a diffractive analysis of subjectification allows is to ask how the discursive rules or conditions of practice and research (whether these are called scientific or not) not only create or disrupt the proposed boundaries between researcher/practitioner and the natural world/human mind/client, but also, how these conditions and practices fabricate the possibility of a subject position like counsellor/psychologist/social worker/researcher at all. In other words, the counsellor is not taken as a pre-existing individual who then deploys scientific (or non-scientific) methods to discover their client or object of research, but it is through these practices themselves that the counsellor 'becomes'.

Barad's discussion of the implications of quantum physics (Barad, 2007) points us to the notion that a given phenomenon has determinate properties only by virtue of the apparatus used to measure or observe it. As all such apparatus necessarily *exclude* some other properties, then the phenomenon cannot be perfectly

knowable in its entirety; in fact Neils Bohr's theory of
complementarity states that the more knowable one property of an
object under examination, the less knowable other properties are.
According to Barad, thinking was Bohr's favourite exemplar of
this notion of complementarity.

> *"[Y]ou need to make a choice between two
> complementary situations: either you think about
> something, in which case that something is the object
> of your thoughts, or you examine your process of
> thinking about something, in which case your thoughts
> about what you are thinking (about something), and
> not the something itself, are the object of your
> thoughts"* (Bohr, cited in Barad, 2007, p. 21).

What marks Barad's arguments as being of particular interest is
that objects of knowledge themselves are constructed by the way
we know about them. The issues are ontological as well as
epistemological. In fact this very distinction starts to break down in
the notion of 'agential realism'. This distinction is sometimes held
to be at the centre of misrepresentations of social constructionist
theory (e.g. Hacking, 1999; Taylor et al., 2000), with the 'saving'
of relativism consisting of pointing out that social constructionists
do not believe that objects themselves are socially constructed or
not real. Whilst Barad is not a social constructivist in this sense,
she does argue that it is only in specific material arrangements that
objects are "disclosed" (2007, p.361), that scientific practices are
an intra-action with the world that inevitably contribute to the
marking and shaping of reality. The agential cuts between agents
and objects of knowledge *matter*.

I have encountered the notion of diffraction directly through engaging with the problem of measurement. This in itself is a good example of the phenomenon I am trying to account for. Rather than dismiss classical ideals of measurement—which includes, amongst other things, an insistence on the inherent separateness of the measuring agent and the measured object—a diffractive reading invites closer examination; of the 'cuts' between object and agent, of the genealogy of the measurement situation, of the material arrangements involved, of flows and relations of power, and so on. In being invited into these questions in relation to the measurement of the effects of child sexual abuse, I am also invited to interrogate my own investigative practice. The only consistency I can adhere to is to be consistently entangled with the material I engage with, and try to account as transparently as possible for the order that I impose on things in the process. This involves continuous destabilisation.

As a secondary effect, diffractive reading also provides a response to the methodological dilemma posed by genealogical and archaeological methods. Where these latter methods look to past events for historical epistemic conditions which cannot be perceived 'from within', diffractive reading asserts the existence of multiple, simultaneously existing, differentiated sources of knowledge production, while acknowledging that these sources still share a larger set of historical epistemological conditions. Thus, while there are no doubt aspects of my account that I cannot account for (Butler, 2005; Barad, 2007), I can attempt to identify these sources of knowledge which shape and are shaped by the problem of measuring the effects of child sexual abuse.

An outline of the chapters

Following this introductory chapter, I outline the key methodological and conceptual tools the thesis uses to develop and explore the main research questions. Chapter 2 involves outlining the key concepts that I draw upon to develop my research questions, based on Foucauldian concepts of discipline and governmentality. This follows Rose's suggestion that the focus of analysis ought to be the ways in which psychological testing produces truths about humanity, rather than simply as attempts to describe such truths.

> *Psychology is potent because it can appear to shift*
> *such judgments [about human difference] from a*
> *sphere of values, prejudice, or rule of thumb to the*
> *sphere of human truths, equality of standards, cogently*
> *justifiable choices and objective criteria of efficacy*
> *that should reign in a democracy (Rose 1998:90)*

I also attempt to trace some of the debates within feminist literature on the ethical quandaries posed by the work of Foucault. It is sometimes claimed that Foucault's work is not necessarily sensitive to problems of sexual violence and abuse, and even that it can be used to justify and enlarge the problem of men's sexual violence to women and children. Given that I am invested in the belief that 'sexualised interactions' between adults and children should be regarded as abusive, I attempt to justify my position whilst responding to the problems and questions thrown up by Foucault's critique of childhood, sexuality and power.

Chapter 3 outlines more specifically the analytic practice I have deployed in reading research texts, how to understand texts and speech as discourse, and developing a way of using both published research texts and the focus groups I conducted in a way that is consistent with these conceptual frameworks and tools. I document the way in which my original intention for 'focus groups' as an investigative practice become deformed and reassembled through my engagement with critical theories of a kind of discourse analysis and theories of measurement. This shift largely came about through the points I discussed above in relation to diffractive reading, particularly noting the parallels between the ethical, epistemological and ontological dilemmas involved in measurement and interpreting text and speech.

Chapter 4 interrogates measurement and statistics as scientific technologies. I will trace how statistical and measurement ideas from scientific fields other than psychology have been utilised in psychology, a movement made possible by the claim that psychology is a science and therefore deploys scientific methods (a notion sometimes called *scientism*). I will argue that what is called psychological measurement in the research on the effects of child sexual abuse is based on one specific model of scientific measurement, and discuss other available models of measurement (or more accurately, of understanding measurement). This chapter establishes that scientific measurement is in fact a heterogeneous concept formed by local practices, and is not a monolithic or stable entity.

Chapter 5 examines the role of the statistical production of *normal* in researching the effects of child sexual abuse. Through this analysis I identify the notion of normal as an important concept

performed by psychological measurement, and I analyse this in relation to child sexual abuse specifically. I examine how the statistical construction of a normal child or a normal developmental pathway is used to construct the abnormality of those subject to child sexual abuse. I also interrogate the relationship between therapy and calibration to the norm, and engage with the tension experienced by the counsellors I spoke with in regards to this problem.

Chapter 6 addresses the question of objectivity in measurement, sharpening the focus from psychological measurement generally to show how measurement operates as technique of power/knowledge in the field of child sexual abuse research and therapy. This will entail a detailed examination of psychological/scientific research literature on child sexual abuse, supported by extracts from two focus group discussions I conducted with a team of child sexual abuse counsellors. In counter-posing research literature with the talk of counsellors, I intend to highlight the lack of hegemony of psychological measurement in problems of child sexual abuse, whilst also attempting to identify the way measurement can shape what is visible and sayable, and what is elided. My intention is to examine what kinds of knowledge are produced when techniques of psychological measurement are applied to the problem of child sexual abuse. Under what conditions is objective knowledge about child sexual abuse possible, and what are the effects of these practices?

In Chapter 7 I discuss objectivity in relation to the conduct of research and therapy from the point of view of the researchers and counsellors themselves. I argue that objectivity, as a reified feature of scientific measurement, produces its object of knowledge while

also disciplining the conduct of researchers and counsellors. Objectivity itself is analysed as a constructed and contested practice, and I outline some competing versions of objectivity in contrast to the classic scientific model on which psychological measurement is based.

I conclude the thesis by recapping the main findings and exploring the productive elements of psychological measurement and statistics for knowing about and responding to child sexual abuse, and for alternative modes of understanding child sexual abuse that actively resist or marginalise the practice of psychological measurement.

Summary of Chapter 1

In this chapter I have outlined the key research questions and situated these in a historical context in relation to the field of child sexual abuse research. I have reviewed debates about the scientificity of child sexual abuse research, specifically identifying measurement, causality and objectivity as important concepts/practices. I have described some of the political and critical concerns raised by these practices, whilst acknowledging that presenting child sexual abuse research as scientific has strategic purposes for the acknowledgement of suffering.

Having established the relevance of the key questions raised in the thesis, in the next chapters I describe in greater detail the epistemological and analytical resources I draw upon.

Chapter 2- Epistemological and methodological considerations

My intentions in this chapter are twofold: 1- to provide a context for both the ostensible topic of this thesis (the importance of measurement and statistics in the study of child sexual abuse), and the style of analysis being attempted; and 2- to convey both the method of gathering and generating the 'data' I have analysed (including how I have defined data), and the conceptual framework I have employed to analyse the data. In both cases, I highlight that this framework has not been stable over the course of the thesis and this is reflected as the thesis progresses. There is a lot of criss-crossing or weaving in and out of discussions about the epistemology for this thesis and the research questions themselves. After all, in many ways this is a thesis largely about epistemology. I would ask the reader to bear this in mind when things seem 'a long way from home', when questions of psychological measurement or the effects of child sexual abuse seem far away. Of course, I will try to indicate the signposts/stepping stones/breadcrumbs I have followed as fully as possible.

I will begin by discussing the ways that my thesis has been informed and influenced by a number of aspects of the work of Foucault. I have chosen to do this to reflect something of the way that the conceptual framework has developed. Whilst the thesis draws upon a wide range of authors and perspectives, my encounters with Foucault's work played an influential role in choosing a research topic and formulating the primary research questions.

As the thesis has progressed and come to its conclusion, I found myself becoming less concerned with understanding all the intricacies of Foucault's theories and methods[11], and engaging with a wider range of authors in the somewhat fuzzy field of science studies. I have become more and more interested in the ways that claims about child sexual abuse get consolidated into scientific facts and the work of *fact construction* this entails on the part of researchers (Latour, 1986). Accordingly, I will elaborate on the conceptual and analytical tools I have borrowed from authors in the science studies field in the following chapter.

What does Foucault's work have to offer?

> *Foucault's work subverts and challenges a certain modern version of Enlightenment, made up of morally and intellectually validated schemes of social improvement, therapy and order, which operate by identifying and correcting various forms of individual deviation from a norm (Gordon, 2000, p. xvii)*

The invocation of the name of Michel Foucault is more or less obligatory in a contemporary thesis on issues regarding sexuality and the 'psy' disciplines. While Foucault's writing has been productive in thinking about the problem of child sexual abuse and its therapeutic management, and references to his work will appear throughout the text, there are important ways in which this thesis might be said to not be 'Foucauldian'. While I do not want to get caught up in attempting to follow a

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[11] Which can become a dangerous pre-occupation, leading to complex considerations of issues such as the use of quotation marks in Foucault's work on the 'human sciences' (Visker, 1995).

(non-existent) pure Foucauldian method (Graham, 2005), in what follows I will outline some key aspects of Foucault's writing that have informed this thesis, as well as discussing some of the difficulties involved.

I will now discuss the key features of Foucault's 'analytics of power' and the formulation of problemetisations that have informed this research.

Epistemology, archaeology, genealogy

Davidson (2001) provides a helpful clarification between epistemology, archaeology and genealogy as interdiscursive, intradiscursive and extradiscursive sites of analysis respectively. Epistemology is concerned with the internal logics of a scientific discipline, its own rules for producing statements which may be candidates for truth or falsity. Statements that do not conform to these discursive rules are classed as nonsensical- they do not even enter the scientific conversation within the discipline. I argue that the application of psychological measurements is an example of a rule for producing *scientific* truth claims about child sexual abuse in contemporary psychological discourse (see, for example, Freyd et al., 2005)[12].

Archaeology is concerned with broader 'conditions of possibility' of knowledge that cuts across any particular discipline. For example, in *The order of things*, Foucault (1970) described how the distinct scientific disciplines of biology, economics and linguistics shared the same *èpistèmé* of the classical era, which determined the rules for the formulation of

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[12] Of course, there are also non-scientific discursive practices about child sexual abuse that do not require measurement.

scientific statements. Scientific disciplines may manifest or articulate the *èpistèmé* in distinct ways, however all scientific disciplines will be governed by the same, generally unspoken, hidden or implicit, rules.

As for this thesis, it is this archaeological notion of the *èpistèmé* that leads me to explore practices of measurement from physics and how they relate to psychological measurement. As Hacking notes, practices and theories of measurement applied in diverse scientific disciplines may share a common formation of scientific discourse: "Social statistics and quantum mechanics look like apples and oranges…but they are, from the point of view of the archaeologist, part of the same formation" (Hacking, 2006, Introduction, no page number).

Both approaches are useful when undertaking a critical study of therapeutic and research practices linked to child sexual abuse. I will mostly be limited in this thesis to a more epistemological level of analysis. However, I will be trying to delineate two approaches to child sexual abuse (scientific psychology and post-structuralist feminism), and attempting to analyze how both discursive formations have a certain legitimacy in contemporary discussions of child sexual abuse.

The third approach, genealogy, is most concerned with how knowledge is put into practice, and therefore described by Davidson (2001, p. 205) as extra-discursive. I will now discuss genealogy in more detail as it is a key methodological resource for the latter part of this thesis.

Genealogy is a term used to describe Foucault's approach to research during a particular period. It is generally specified by commentators as the period following his 'archaeological' work. In this typology, the major genealogical works are said to include *Discipline and Punish* (Foucault, 1977a) and *The*

history of sexuality: Volume 1 (Foucault, 1978), while the archaeological works include *Madness and civilization* (Foucault, 1965) (which is a heavily abridged English translation of *Histoire de la folie*; it is only recently that the full book has been translated and published in English), *The birth of the clinic* (Foucault, 1973)*,* as well as the methodological works *The order of things* (Foucault, 1970) and of course *The archaeology of knowledge* (Foucault, 1972).

The differences and similarities between these approaches are the subject of much debate amongst Foucault scholars. Some make clear distinctions between these periods (as well as the later work concerned primarily with subjectivisation, a period of Foucault's commonly referred to as relating to the 'ethics of the self'), whilst others argue that the oeuvre of work must be understood as a whole (a position which Foucault was critical of- see *What is an author?* (Foucault, 1977c), although see also the article penned under Maurice Florence (1994), where it is apparent that Foucault is discussing his own work as a whole). In some instances Foucault seems to give archaeology a definition that resembles the definition that some commentators appear to give genealogy, in order to distinguish the two; for example: "The archaeology of the human sciences has to be established through studying the mechanism of power which have invested human bodies, acts and forms of behaviour" (Foucault, 1980, p. 61). This gives credence to the claim that the two methods are not exclusive or even necessarily distinct. Yet Foucault also states that archaeology is related to the analysis of 'local discursivities' and genealogy related to the 'tactics' whereby knowledges are brought into play (Foucault, 1980, p. 85; Rogerson, 2001). Elsewhere, just before he had coined the term 'genealogy', he stated that "in comparison to what I call archaeology, the discursive analysis of power would operate at a level…that would enable discursive practice to be

grasped at precisely the point where it is formed" (Foucault, 2006/1973-74, p. 13).

The difficulty of untangling these two terms (archaeology and genealogy) has produced a great deal of secondary commentary with a range of assertions about their differences and commonalities. Of interest here, however, (this complexity notwithstanding) is that 'archaeology' is generally concerned with the historically contingent 'conditions of possibility' associated with knowledge, while 'genealogy' is generally thought of as the approach that highlights the importance of practices (discursive or otherwise) involving power and knowledge, especially within the 'human sciences' (Davidson, 2001). Following Allen (1999), it can be said that generally, archaeology is concerned with knowledge, while genealogy is focused on power.

Another characteristic feature of genealogical enquiry is a certain style of historical enquiry- 'a history of the present'. Genealogy differs from traditional historical enquiry in that it does not take the present to be an inevitable or evolutionary outcome of the past (Foucault, 1977b).

Genealogy and social work

The contribution of the tradition of genealogical enquiry to social work is, partly, in examining the ways in which the expertise and interventions associated with the fixing of problems contributes to the very production of those problems. This is not meant in the traditional radical view that social work interventions are ineffective or create dependency amongst the targets of intervention, or are part of an administrative regime that reinforces rather than relieve oppression (Lavalette, 2011).

Rather, that the capacity of a problem to be 'thought', or 'rendered visible and knowable', relies upon the same discursive regimes from which their solutions are derived.

Genealogical enquiry is not necessarily directed at prescribing improvements in knowledge production or developing improved techniques for the implementation of the human sciences in practice. "What marks genealogy… is its inability to provide solutions" (Colwell, 1997). This is, indeed, a problem for genealogy in finding receptivity in the profession of social work, and perhaps part of the reason why the work of Foucault and other Foucauldians has been largely neglected until relatively recently in English language social work literature, despite its obvious thematic relevance. Recently a greater interest has become apparent.[13] Social work is increasingly in the business of being seen to fix problems (or, in the discourse of evidence-based practice, delivering outcomes). The concern with "keeping an eye on the practical", and being seen to do so, extends to critical social work theory (Lovelock et al., 2004). Theoretical work which does not contribute to the development of practical techniques to attain specified ends (whether such ends be managerialist outcomes or a liberatory social work practice) struggles for legitimacy in such an environment, but neither this orientation, nor a 'rational' argument with it, is the problem of the style of analysis practiced or informed by Foucault's work. If anything,

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[13] Social work writing drawing on Foucault is emerging, at a much slower rate than other disciplines such as cultural studies, feminist studies and philosophy. Some of the emerging literature crosses disciplinary boundaries. The Foucauldian analyses I have found most helpful, and the approaches which seem most epistemologically consistent, tend to take/constitute social work as the object of enquiry. Others attempt to use such enquiry as a method for developing or improving social work practice. Recent work on social work and Foucault includes: Chambon, Irving and Epstein (eds) 1999; Critchley, 2003; Foster, 2005; Joy, 2003; Lovelock and Powell., 2004; Margolin, 1997; Tew, 2006; Trainor and Jeffreys (eds)., 2003.

this approach tends to produce more problems, to problematise the taken for granted.

One cannot help but feel that the social work dictum (transplanted from medicine) 'First do no harm' is a paralyzing absurdity if social work practices are examined from a genealogical perspective. All knowledge, including social work knowledge, is, if not bad, at least dangerous (Dean, 1999). It comes as no surprise to learn that Foucault stated, on at least one occasion, that paralysis among social workers (specifically, those working in prisons) was an intentional effect of his work (Foucault, 1991c). He distinguishes between paralysis and anaesthesis, stating his intention that his work would contribute to the former, to a sense amongst social workers that they "…no longer know what to do, so that the acts, gestures, discourses which up until then had seemed to go without saying become problematic, difficult, dangerous. This effect is intentional" (Foucault, 1991c, p. 84). The very paralysis that Foucault celebrates is often decried in the social work literature as an obstacle to effective practice, linked to the perceived nihilistic relativism of post-modern theory (D'Cruz et al., 2007).

An interesting way to explore what is done in genealogical, as well as archaeological, enquiries is through an oft-asked question; is it critical? Can it be a methodology by which one can conclude that one way of doing things is better or worse than another? Visker (1995) explores the difficulties of using a genealogical approach for 'critical' purposes. It is often argued that genealogy, without a normative reference point, can only describe and in the end reinscribe and validate the existing conditions. If it does describe a normative reference point, this threatens the philosophical premise of genealogy. That is to say, the philosophical premise is that the individual is

historically constituted, and there is no 'human condition' prior to constitution. Therefore, one cannot say that one way of doing things or speaking of things is more 'true' to human nature than another; the very idea of 'human nature' itself is a historically specific achievement.

Thus, there appear to be no grounds within genealogy on which to justify resistance to any practice at all. This is one of the criticisms of Foucault particularly from liberal theorists; "…without introducing some normative reasons for why resistance is preferable to submission, Foucault cannot explain why anyone should resist" (Pickett, 2005, p. 34). Habermas puts it more directly- 'Why fight?' (cited in Pickett, 2005). Horowitz (1987) points out that this lack of a pre-constitutional (or essentialist) subject makes Foucault's work prone to colonisation by a liberal, democratic project. Citing particularly the work of William Connolly, Horowitz describes how such theorists are able to conclude that the best solution that Foucault can offer for societal change is to 'slacken' the order, to expand the range of behaviours and ways of being that fall outside the realm of governance and discipline. Pickett (2005) suggests that this liberal democratic 'colonisation' of Foucault is particularly prominent amongst American theorists, who champion the Enlightenment vision of the free, ungoverned, rational individual.

Visker (2005), in contrast, suggests that Foucault did have a preferred mode of 'subjectivization'. Visker notes a shift in emphasis in *History of sexuality volumes 2 &3*, from subjectification (linked to practices of domination) to 'subjectivization' (acts performed on the self) (see also Hook, 2007, Paras, 2006). Subjectivization is conceptualised as in response to power forces external to the individual, but

subjection and subjectivization cannot operate independently. It is "the difference between being accorded a subject-position, and what it might mean to take on, to assume or personalize, such a subject-position" (Hook, 2007, p. 31). Visker puts forward that, particularly in the *History of Sexuality volumes 2 and 3*, "Foucault wishes to release ethics from any form of legality and he believes that, to do this, he must break the hold of religious, scientific or metaphysical authority on the self" (Visker, 1995, p. 96). The self is not pre-given, it must be made; and the preferable mode of doing this would be as a work of art, as in classical Greece. This reference to the self as a work of art simply refers to the belief that there is no essential truth to be discovered about the self to which one ought to calibrate their being, but rather, the self is actively produced is thus able to be worked on as a project. It is helpful to think of an expansive attitude as opposed to an introspective one.

Without a normative framework, decisions about what to resist and in what fashion to resist them cannot help but be arbitrary (so goes the criticism). Pickett discerns in Foucault's writing that whilst he overtly eschews normative frameworks, he also depends upon some moral or political basis to inform involvement in particular political projects. The 'modern Foucault' is essentially aligned with the goals of the Enlightenment (freedom, autonomy, equality etc.) (Paras, 2006), but is constantly suspicious of how these values are practiced. Rather than seeing history as a steady progression towards these goals, he is constantly alert to the power-knowledge nexus involved in any truth claims.

Resistance

An objection to this line of criticism is that it assumes that the only basis for resistance would be a conviction that a particular practice (e.g. psychological measurement) distorts or misrepresents some authentic aspect of human nature (Joel Michell's (1999, 2000) work on the question of whether psychological attributes are quantitative, and therefore measurable, is of this order. I will refer to Michell's work later, particularly in Chapter 3). Todd May states that "Foucault nowhere argues that there is no value to a discourse or discipline being true. Instead, he shows that there are effects which discourses have regardless of their being true or false, so that the truth of a discourse is no longer a sufficient defence for it to claim in the face of criticism" (May, 1993, p. 27). The truth of psychological explanations is in a sense immaterial[14]. Thus, a genealogical approach is not concerned with establishing the truth or falsity of a discourse or practice, and this is not the basis for resistance to any particular practice.

Foucault offers an alternative version of resistance. For him, the basis of resistance is not some idea of an essential human nature, but precisely the opposite. Practices and discourses which limit or proscribe possibilities for human experience are to be resisted on the very basis of the exclusions they produce and require (Horowitz, 1987; see also Foucault, 1997a). Simultaneously, it is these exclusions that ensure resistance to any dominant discourse. Since exclusions are inevitable, so too is resistance (Hook, 2007).

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[14] Immaterial is perhaps not the best word here. The effects of 'psy' practices and regimes of knowledge are clearly concrete in the use to which they are put in the governance of bodies and lives. What is meant here is that a genealogical approach is not epistemologically orientated to the question (or the possibility) of whether a particular set of statements accurately describes its object in an extra-or pre-discursive sense.

Pickett (2005) draws a distinction between the 'Nietzchean Foucault' and the 'modern Foucault' (who emerges particularly in the later writings, but is discernible throughout Foucault's writings). The Neitzchean Foucault is the target of those critics who argue that Foucault cannot make any reasoned decisions about what one stands for, other than resistance to the way things are. They argue that to resist with no teleological framework is simply arbitrary and meaningless. The counter to this is that to resist does not necessarily imply how things ought to be, but simply the possibility things could be otherwise. Foucault advocated, to the frustration of numerous commentators, that critique be celebrated as a valid intellectual activity in its own right, that it need not be tied to particular visions of how things ought to be. Such a dictate was dismissed as 'blackmail' and as 'ministerial cabinet talk' (Foucault, 1991c, p. 84). One quote will suffice to illustrate the point here:

"Critique doesn't have to be the premise of a deduction which concludes: this then is what needs to be done. It should be an instrument for those who fight, those who resist and refuse what is. Its use should be in processes of conflict and confrontation, essays in refusal. It doesn't have to lay down the law for the law. It isn't a stage in programming. It is a challenge directed to what is" (Foucault, 1991c, p. 84).[15]

Foucault's resistance is not located in some essential notion of humanity which struggles to be liberated from oppressive regimes (Foucault, 1978). Rather, the powers of discourse are

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[15] This is not to say that Foucault was not politically active: he was publicly involved in a number of political campaigns during his life. However there was a general insistence that his involvement in these campaigns was based on his participation as a citizen, and that what he offered as an intellectual was not a general kind of wisdom but a limited expertise of thinking about particular problems (Gordon, 2000).

never total, there are always competing discourses and thus available subjectivities, and it is from there that resistance emanates. It is not, then, resistance *against* a particular regime of subjectivity, but rather that any regime of subjectivity will necessarily produce its own resistance (Pickett, 2005). It is not necessary (or in a sense, possible) for a subject to choose resistance- the multiplicity of discourses ensures resistance. Whilst clearly political, such a view of resistance does not require a moral decision. There is no necessity, in a grand theoretical sense, to argue that 'what is' is wrong; it encounters resistance regardless.

Some feminist authors have approached the question of resistance in Foucault's work from a somewhat different angle. Haber (1994) argued that as a result of the disruptions to identity in much 'post-modern' or post-structuralist theory, such theories fail to offer a satisfactory means of resistance to domination. Haber argues that Foucault, in developing his critique of modern identity, eschews effective subjectivity altogether, partly as a consequence of his individualistic conception of the subject. To this, Haber poses a solution in terms of 'solidarity', based on the understanding that identity is formed in community, and this interconnectedness provides the basis for collective action towards a shared end. In this notion of solidarity, essentialist kinds of identity are unnecessary. Similarly, Allen (1999), in her analysis of models of power in feminist and (broadly speaking) post-modernist theory, defines solidarity as "the ability of a collectivity to act together for the agreed-upon end of challenging, subverting, and, ultimately, overturning a system of domination" (p. 127). Similarly to Haber, she argues that this notion of solidarity (a sub-category of what she calls 'power-with') does not have to rely upon an essentialist notion of identity; rather, drawing on the work of

Arendt, she argues that identity categories are political facts in a given situation. Thus the kind of 'identity' required for 'power-with' actions is political, towards an agreed upon goal.[16]

While avoiding the essentialist identity problems of other models of resistance and political action in general, Allen's discussion does not, I suggest, make explicit the normative distinctions on which this model of 'solidarity' is based. Allen does make explicit that solidarity is a specific application of power-with; using the example of a military group, she points out that power-with can serve purposes of domination or injustice (although, she does not make explicit the normative statement in this instance). Her definition of solidarity (cited above) is potentially problematic; many military groups would define their actions in this way; as aimed towards "overturning a system of domination". Think also of extreme right Nationalist groups who claim that their position is one of being subjected to a relation of domination by immigrants—would their collective actions be called 'solidarity'? More to the point, what about so-called 'men's rights' groups who claim that they experience systematic discrimination in relation to women in the family court? They could also claim that their collective actions are instances of solidarity. The normative aspects of the distinction made between solidarity and other kinds of power-with remain unclear, other than the general "kind of solidarity in which feminists are interested" (Allen, 1994, p. 127), somehow assuming this is inherently known and agreed upon.

Butler provides a further way of thinking about resistance and identity. In *Psychic life of power* (1997a, especially chapter 5)*,*

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[16] Note the contrast with Foucault's more open-ended project of critique, which some would argue is nihilistic or pessimistic, but others (e.g. Deleuze, 1988) would see as less restrictive.

she suggests that a culturally dominant identity (gendered, heterosexual) depends upon a pre-emptive disavowal and internalisation of a barred object of desire. In terms of my discussion here, this implies that there is a sense in which a dominant subjectivity is always already founded upon a socially disallowed set of identifications. This would effectively reverse the relation between dominant and resistant; rather than resistance equating to the protest of a less dominant against a more dominant, the dominant is instead seen as a resistance to a less powerful subject position.

So, genealogy is useful in the sense that it raises these important questions regarding power, resistance and regimes of thought associated with specific therapeutic practices and practices of knowledge. In relation to the current study, then, it might be argued that there can be no 'objection' as such to psychological measurement, at least not on the grounds that such practices falsely represent or distort 'true' human nature or experience. One might, however, investigate (or problematise) psychological practices of measurement on the grounds that they not only shape, but actively produce the objects of expert knowledge- the victim of child sexual abuse, and the harm of child sexual abuse. It is also possible to investigate the ways in which measurement produces individual difference (Meadmore, 1993); i.e. it is not a matter solely of identifying what kinds of limitations are placed on lives, but reversing the question to ask what possibilities for subjectivity are enabled through psychological testing and measurement.

This is the value of a genealogical approach for this thesis. I am not aiming to argue that psychological measurement is 'wrong' and researchers and counsellors ought to adhere to 'better' ways of knowing and practicing. What I do attempt is to

identify that psychological measurement has produced this object of scientific knowledge- the harm of child sexual abuse. Thus, rather than take for granted the ontological and epistemological practices and tools which have established this scientific object, I investigate more closely the practices and tools themselves, and the effects of their work in the field of child sexual abuse research and therapy. In saying this, I do not argue that psychological measurement and the associated practices of enquiry are the only means through which this field has been produced, but that they are particularly influential.

Part of this thesis is to also ask this question in relation to counsellors; what subjectivities are made available to counsellors via psychological testing and measurement, and how are these possibilities taken up, resisted, and otherwise understood? How do the practices of psychological measurement produce and govern the actions of researchers and therapists/counsellors?

Disciplinary power

The form of power enacted through this nexus of knowledge/power is what is referred to as 'disciplinary power'. This is best understood in opposition to sovereign power, a model of power which emphasizes the repressive and inhibiting aspects of authority (most obviously, as in the right of a king to hold the power of death over his subjects). Disciplinary power is, by contrast, productive, in that it acts upon individuals so as to incite them to conduct themselves in particular ways. These preferred modes of conduct are frequently linked (through knowledge and expertise, especially that of the human sciences) to the betterment of the population and maximization of individual well-being (Foucault, 1978).

In other words, psychological testing does not simply *repress* possibilities for understanding and action by an individual (the child or adult client, the parent or teacher, the counsellor), but actively *produces* such possibilities. Furthermore, the knowledge produced through the practice of measurement provides a kind of lens by which those individuals can frame, understand, and act upon their own situation. It is not simply a matter of such change being demanded or forced by some oppressive external force (although this can, of course, be part of the situation); the crux of the power/knowledge formulation is that individuals take this knowledge and act upon themselves in the name of psychological health and well-being.

This conception of the individual subject is in contrast to the "rational, autonomous individual [that] is central to liberalism" (Ashenden, 2004, p. 42). In this liberal view of subjectivity, individuals are fully formed, atomistic entities upon which social forces generally act in a repressive manner. Individual subjects are held to exist 'prior' to power. This is especially so in regards to the area of sexuality, as Foucault argues at length in *The history of sexuality* (1978). In contrast to the liberal view of the individual subject, Rose (1998) argues for an approach in which no theory of the subject or metapsychology is necessary. Rose discusses the idea of infolding (taken from Deleuze). "The fold indicates a relation without an essential interior, one in which what is 'inside' is merely an infolding of an exterior" (Rose, 1998, p. 37). Thus the subjectification of persons occurs not through the shaping of an essential inner space, but the inner space itself is a result of infolding of external authorities (subjectivization). One of Rose's central concerns is to show that what he terms the 'psy' disciplines constitute an important

'external authority' through which individuals become subjects[17].

The implications for this thesis are quite clear. Rather than try to argue about why psychological measurement is repressive or a distortion, it is more helpful to ask what it produces, what it allows, and so on. Indeed, in important ways the social acknowledgement of child sexual abuse as harmful was made possible by psychological measurement and statistics. This is not to say that all 'productive' aspects must be embraced or celebrated; for example, psychological measurement and statistical techniques have also made it possible to make normative judgments about individuals who have been subjected to child sexual abuse (Warner, 2009). It is a question of analyzing the range of truth effects that are produced when scientific research (through the use of measurements and statistics) produces the effects of child sexual abuse through objective means of enquiry. The aim is to identify what makes such a mode of enquiry possible and desirable in the field of child sexual abuse research.

Power/knowledge

'Power/knowledge' is a concept that appears throughout the work of Michel Foucault (particularly *Discipline and Punish* (Foucault 1977a), and the writings included in the book titled *Power/Knowledge* (Foucault, 1980), especially in relation to what he calls the 'disciplines' or the 'human sciences'. Such

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[17] Although acknowledging Rose's use of 'the fold', Hook (2007) remains insistent that this interior must still be factored into an analysis of subjectivity. He argues that the processes of subject formation are not as rational as Rose depicts them to be, and that some account must be given of the irrational or unconscious processes of subject formation. A similar argument was advanced earlier by (Flax, 1990)

disciplines include social work, psychology and psychiatry. (Psychoanalysis is in some ways an exception, a challenge to psychiatry and the notion of the stable, coherent subject. However, even psychoanalysis performs a normalising function upon individuals and their relationships (Foucault, 1980)). To say that psychological measurement is a practice involving 'power/knowledge', is to rely on a specific meaning that this phrase signifies in the Foucauldian literature. It does not suggest that knowledge is deployed (by an oppressive State, for example) in order to exercise power, or as an act of oppression. Nor is it meant that "power is knowledge" (Ransom 1997). Rather, it is part of a mutually reinforcing process of individualization and expert intervention in the subjectification of troublesome and deviant individuals:

> *"Knowledge, as a modality of power… produc[es] profiles of troublesome persons and related behaviours, while simultaneously refining the techniques of measurement, comparison and surveillance able to render such problematic individuals in ever more detail" (Hook, 2007, p. 15).*

Issues of sexuality have historically been particularly dense focal points for the operation of power/knowledge in scientific constructions of normalcy and deviance. For example, both physical and psychological measurements have been deployed during the first half of the 20th century to research the 'constitutional factors' associated with homosexuality (Peterson, 1998) and paedophilia (Bowman, 2005).

The 'power/knowledge nexus' consolidated in the 'psy' disciplines can be understood as a means of governing

individuals. This is not to propose that individuals would be 'free' if they were not the objects of psychological discourses and practices; indeed, as Rose (1998) and Dean (1999) point out, the effectiveness of the 'techne' of governance is premised on the individual exercising their freedom in order to maximise their own life. The client of therapy is to be active in monitoring, evaluating and adjusting their thoughts, behaviours and feelings (Hook, 2007). That is, individuals are incited to use their freedom to act upon themselves, to 'calibrate' themselves to psychologised constructions of health and well-being.

The purpose of deploying the concept of governance in this thesis is not to propose a route of escape from the gaze of psychology or the quantitative social sciences for the problem of sexual abuse. There is no suggestion that one could be 'ungoverned'. The aim is rather to produce a problematisation of the victim of child sexual abuse, and the child sexual abuse counsellor, as constituted by specific regimes of measurement authorised by therapeutic (psychological, scientific) expertise. Again, this is not to deny that other practices or discourses of therapy are at work, but to open up for investigation the conditions and effects of psychological measurement as a scientific practice.

According to this idea of a power/knowledge nexus, knowledge is possible only because of the operations of power, particularly on the body. Foucault argued that power is necessary condition for knowledge, especially in the human sciences.

> *Far from preventing knowledge, power produces it.*
> *If it has been possible to constitute a knowledge of*
> *the body, that has been by way of an ensemble of*
> *military and educational disciplines. It is on the*

> *basis of power over the body that a physiological,*
> *organic knowledge of it became possible (Foucault,*
> *1980, p. 59).*

This is in contrast to the idea that power corrupts knowledge, or that true knowledge is innocent from the effects of power. The point of interest is in how individuals as subjects constitute themselves, are constituted through regimes of knowledge "…in one specific form or another, as a mad or healthy subject, as a delinquent or non-delinquent subject, through certain practices that were also games of truth, practices of power, and so on" (Foucault, 1997a, p. 290). Power's 'strength' derives from the fact that is does not simply operate to repress, but subjects take up knowledge as means of acting upon themselves (Foucault, 1980).

A key function of the concept of the power-knowledge nexus in Foucault's work is to dispute the status of knowledge claims as value-neutral and free of power relations (Visker, 1995). Again, the truth or otherwise of psychological explanations does not discount their significance. May argues that knowledge is always accompanied by operations of power: "What is important is that the emergence of this knowledge is entwined with the exercise of this power" (May, 1993, p. 44). In other words, it is not that there would be a 'real' truth about individuals if knowledge was divested of its connection with power. It is the operation of power that enables knowledge to be produced at all. The point of analysing psychological knowledge from this perspective is not to correct prior flaws in research and evolve towards a purer view of reality; it is to analyse the techniques and practices which "…support…the power and knowledge relations that invest human bodies and

subjugate them by turning them into objects of knowledge"
(Foucault, 1977, cited in McNay, 1992, p. 28).

Why engage in a problematisation of the psychological
measurement of individuals, specifically? Psychological
measurement of human attributes and problems is integral to
the 'art of governance'. According to Rose, the psychological
test:

> *…was the most important contribution of the*
> *psychological sciences to the human technologies*
> *of the first half of the twentieth century…It has*
> *become an indispensable part of any modern*
> *programme for the government of individual*
> *differences (Rose, 1999, p. 143).*

Psychological measurement is a prime example of examination:
the technique that produces the data to be quantified, the
measurement that I referred to in my introduction as a
requirement of statistics, a combination of hierarchical
observation and normalising judgement. As May states:

> *Both a ritual of power and a procedure for the*
> *establishment of truth, the examination is the*
> *culminating, if recurrent, event in the disciplinary*
> *process. Moreover, by introducing documentation*
> *into the normalising process, the examination both*
> *constituted individuals as describable objects and*
> *opened up populations for measurement and*
> *comparative study (May, 1993, p. 43).*

This alludes to the manner in which counsellors, in producing a
document based partly on the results of the measurements,

produce a text (an assessment report) which becomes a textual representation of the child. In addition, many psychological tests come equipped with software that produces a computer printout of a graph demonstrating exactly where an individual is within or out-of the bounds of statistical normality. Within the bounds of disciplinary knowledge, such textual fabrications are in a sense more 'real' than the actual children they purport to describe (Rose, 1999). It is these documents that are acted upon, revised, subject to examination and which inform the reflections and analysis of the counsellor as to the proper intervention. The counsellors I spoke with for this thesis utilised the results of psychological measurements to prepare their assessment reports on individual children (although this is a fraught and contested practice, as I will show later, especially in Chapter 7). Likewise, researchers conduct psychological measurements on their subjects to produce their findings which form the basis of their publications

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Problems and problematisations

The notion of 'problematisation' is derived from the tradition of genealogical enquiry, from the intellectual legacy of Foucault (who in turn was inspired by Nietzsche) (Foucault et al., 1997). Colwell provides a neat distinction between problems and problematisations, likening the latter to an awareness that a situation is an unfolding *event* rather than a fixed set of circumstances:

Problems, as such, always have specific and singular solutions. "A problem always gets the answer it deserves." Events, on the contrary, remain problematic; they do not have solutions or, more to the point, they do not have solutions except insofar as they are actualized. This is a function of their temporal nature

as …always in a state of becoming. The problem is a reduction of this becoming to the present tense, an actualization of the virtual, infinitive structure of the event which allows for a singular solution that can be repeated without (much) difference. (Colwell, 1997)

The situation being problematised here is the conversations that the counsellors were able to convene about their work, and the literature produced by researchers. Rather than taking for granted these conversations and publications, attention will be focussed on tracing the contingencies of knowledge production and circulation that 'make possible' such conversations.

This shifts the focus away from defining and defending a position in relation to any particular model of measuring the effects of child sexual abuse, or debating the accuracy or otherwise of any claims based on such measurements. What I will be trying to understand is the situation that allows—and perhaps even requires—psychological measurements in order to establish the harm of child sexual abuse.

Sexual abuse therapy/counselling can be understood as part of the 'power/knowledge nexus' discussed earlier, and thus as a means of governing individuals. It is not a question of pitting 'therapy' against 'power', as the argument being developed here is that therapy *is* a practice of governance, invariably involving the deployment of power and resistance. To say that there are deployments of power in therapy, is not to say that therapy can never be helpful, or contribute to a kind of freedom in people's lives. As has been discussed in relation to the concept of governance and disciplinary power, power does not always repress freedom; some practices of freedom can involve

(or even depend on) the exercise of certain kinds of power
(Rose, 1999).

The purpose of this thesis is not to propose a route of escape
from the gaze of psychology and social work for those sexually
abused as children; such a project would be futile, and may in
fact not be desirable. The aim is rather to produce a
problematisation of the 'victim of child sexual abuse', as
constituted within practices of measurement authorised by
scientific research and therapeutic expertise.

Statistics and governmentality

Statistics, the science of the state, emerged in 16th and 17th
century Europe where population became the object of
government, with 'apparatuses of security' (e.g. health, law,
welfare) the essential mechanism (Foucault, 1991a, 2007).
Where sovereign power is concerned with the continuation of
the sovereign's sovereignty (as it were), and discipline
concerned with the conduct of conduct, 'government' in this
emerging sense was concerned with the management of
population, framed as a problem of economics and health. (The
problems of sovereignty and discipline did not 'disappear', but
become crucial questions within the rationality of government).
A crucial shift involved the family; where the family had
served as a model for government, the notion of population
exceeded this model. The family becomes not a model of good
government, but a segment of population and an instrument in
government. The emergence of population, rendered
governable through statistics, is associated with an economic
rationality of government. 'Statistics' is literally knowledge
(*savior)* of the population, to be put to use towards the ends to
which government is directed (chiefly; prosperity, security,

health of the population). Importantly, the notion of governance here refers to the aim of maximizing the health and well-being of the population, and in this context Latour says that statistics were "the major science of the mid-nineteenth century" (1988, p. 21).

Numbers facilitate more efficient management of people, from the broad arenas of populations of nations, to the management of individual deficiency (Rose, 1991). This can be conceptualised as an administrative imperative, in contrast to a scientific one (Porter, 2003, p. 244; Hand, 2004). There are two related streams of statistics in this regard; that of populations— surveys and censuses—that feed into economics and sociology (Foucault's 'bio-politics'), and the more mathematical tools of statistical inference that feed into psychology and medicine ('anatomo-politics' of the individual). (Foucault, 1978; Gastaldo, 1997). Psychology has its own names for the distinction between measurements of populations (nomothetic) and of individuals (idiographic) (Hand, 2004). Governing these two 'poles' has been described as the "two pure functions of modern societies" (Deleuze, 1988, p. 61), and both are involved in measuring the societal and harm of sexual abuse.

The quantification of human problems and characteristics is important to the art of governance. One of the operations of disciplinary power is that it…

> …*measures in quantitative terms and hierarchizes*
> *in terms of value the abilities, the level, the 'nature'*
> *of individuals. It introduces, through this 'value-*
> *giving' measure, the constraint of a conformity that*
> *must be achieved. Lastly, it traces the limit that will*
> *define difference in relation to all other differences,*

> *the external frontier of the abnormal (Foucault 1977a, p. 183)*

Hierarchisation, segregation, and normalisation. Through history, these practices have been conceived in religious, legal, medical or psychological terms. However, whilst "the explanations may have changed…the process of creating structures with which to measure and contain human behaviour have not" (Hutton, 1988, p. 128). Individuals whose psychological measurements show them to be statistically different from the norm in a range of psychological attributes become legitimate subjects of therapeutic interventions. At the same time, such statistical differences are used to gather political support to make child sexual abuse a visible problem by scientifically proving its harmfulness and establishing its prevalence.

Numbers about sexual abuse

Measurement as a practice of power/knowledge has a significant historical role in contemporary productions of, and regimes of management of, problems of sexual violence and abuse. Statistics and psychological testing (of populations and individuals) have been implicated in the establishment and management of the social problem of sexual abuse, the proving of the harmfulness of sexual abuse, the ascription of motivation to individuals who sexually abuse, and the invention and practice of intimate interventions into the lives of both victims and offenders. The list of the types of numbers about sexual violence includes (but is certainly not limited to):

- the establishment of the numbers of victims of abuse in various populations (Australian Bureau of Statistics,

1996; Mouzos, 2004; Australian Bureau of Statistics, 2006);

- the measurement of the harm caused by sexual abuse (Spaccarelli, 1995; Rind et al., 1998; Maniglio, 2009), including the calculation of the likely long- term effects of victimisation on individuals, most notably predictions about the 'cycle of abuse' and other kinds of 'dysfunction' (e.g. Wilcox et al., 2004);

- forensic assessments of the risk posed by individuals who have sexually abused (for an overview, see Gelb, 2007);

- calculations of the economic costs of sexual violence (Mayhew et al., 2003);

- measuring the attitudes of populations to sexual violence (Victorian Health Promotion Foundation, 2006); and

- evaluations of the effectiveness of various models of treatment for both victims and offenders.

Statistics and measurement thus contribute to the ways that problems of sexual abuse are produced and discussed. These number-producing activities consequently influence research and therapeutic practice with sexually abused children and adults sexually abused as children.

Such numbers about sexual abuse as those referenced in the list above are often presented as truths about the problem, or as a step on the way to the truth. The numbers are said to reflect something about the reality of a problem which already exists. In contrast to this representationalist view of statistics, Gail Reekie (1998), in her study of social measurements of

illegitimacy, stated that her concern was not to use "…
statistics of illegitimate birth or single parenthood to signify the
presence of an underlying and deterministic demographic
reality, my concern is to analyse the contribution of statistical
and demographic discourses to the constitution of illegitimacy
as a social problem" (p. 18).

That is to say, the problem does not necessarily pre-exist the
measuring; rather, the measurements per-form the problem *as a
scientific object*. I stress this aspect of what I have called a
performative model of measurement (after Barad, 2007- see
Chapter 4) to make clear what this *does not mean*; it does not
mean that children were not sexually abused prior to this being
acknowledged through statistical research. I am suggesting that
techniques such as collecting and circulating statistics on child
sexual abuse, and psychological measurement, constitute the
effects of child sexual abuse as an object of scientific
(psychological) knowledge. This is not the same as saying that
children were not subjected to sexual abuse by adults before it
became an object of scientific enquiry, only that the effects
became produced through different styles of reasoning and
amenable to new forms of intervention.

Discussing the post-modernist critique of representationalism
(or the 'language-as-a-mirror of reality' practice of modernism-
analogous to the classical model of science)- Alvesson (2002)
states that "In pragmatic contexts, such as the composition and
use of train timetables and the *counting of people*, the view of
language as a mirror of reality has practical advantages and
functions" (p. 64, italics added).[18] This acknowledgment of

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[18] Clearly train-related phenomena have served as a paradigmatic case of
numbers that has endured for over 150 years. Some imaginative speculation
about the place of train accidents in the development of trauma theory

'practical advantages and functions' still begs the question of whether counting people is an innocent practice. For example, Hacking (2004) shows how the apparently straightforward act of counting people in census-taking requires that defined categories of people be 'made up' before the counting can take place. In a sense, types of people are thought of as such because they are counted, and counted because they are considered a type (or a 'kind') that can legitimately be represented by a number. Thus, the inclusion of groups in a census (such as individuals identifying as gay or lesbian) becomes a matter of political recognition (Henrickson et al., 2008).

More pertinent for my thesis, counting abused children is never a neutral practice (McCallum, 2008). Changes to systems of administrative recording of numbers of abused children, as well as to the definition of abuse, are implicated in sometimes dramatic changes in the ways problems are understood. This in turn involves new ways in which child sexual abuse is to be managed and responded to. For example, the often cited statistics—that 1 in 3 women and 1 in 6 men are subject to sexual abuse—enables sexual abuse to be constituted as a fundamental social or political problem, rather than a rare and aberrant misfortune that occurs to an unlucky few and is committed by individual perverts or deviants. Numbers, measurements and statistics do not operate as innocent or neutral representations about the world and the people who live and die upon it (or within it, or as part of it, depending on your point of view). The concept of governmentality requires recognizing just that 'counting people' is *not* a mirror-of-reality knowledge-practice, but it produces knowledge as well as kinds of people and kinds of things.

(Leys, 2000), and its connections to the current research questions could prove productive, but are beyond the scope of this thesis.

Nikolas Rose (1998) discusses the psychological test as a 'techne' of psychology that 'renders individuals calculable'. Rather than joining with the humanist objections to testing (which claim that testing reduces people to numbers and crushes their unique individuality), Rose suggests an alternative approach. He suggests that the focus of analysis ought to be the ways in which testing produces truths about humanity, rather than simply describe such truths.

> *Psychology is potent because it can appear to shift such judgments [about human difference] from a sphere of values, prejudice, or rule of thumb to the sphere of human truths, equality of standards, cogently justifiable choices and objective criteria of efficacy that should reign in a democracy" (Rose 1998, p. 90)*

Testing does not obliterate human difference and individuality, rather the converse; it produces it[19]. Further, this production of individuality and truths about human nature is legitimated because it is derived not from moral prerogatives or emotive prejudices, but scientific method.

To borrow from/paraphrase Rose (1999), a psychological test might usefully be thought of as a kind of 'census of the soul'. I find this metaphor productive in two senses. First, it allows the visual image of an individual as a space or territory that is populated by various psychological attributes, much as a census enumerates different kinds of people and attributes of a population to be governed towards the health, wealth and stability of that population (Foucault, 2007). Discussing the emergence of statistics as the science of state through 18th

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[19] See Meadmore (1993) for a discussion of this point in an educational context.

century Europe, Rose characterises population as a "…domain to be known and charted through statistics" (Rose, 1985, p.42). I argue that the development of psychological testing through the 20th century to the present regards the individual as a similarly calculable domain. Second, the numbers generated by the psychological measurement of any one individual only become meaningful or significant in reference to a larger population, primarily through reference to the norm. I want to emphasize this inseparability of psychological measurement and statistics of population. The scientific practice of measurement is influential only in the context of comparison to a population.

Here, it can be seen that practices of psychological testing and measurement are implicated in the government of victims of child sexual abuse (and their parents and partners). Further, these practices and forms of knowledge implicate the counsellors of these victims in certain ways of working. That is to say, the kinds of knowledge produced by psychological measurement also prescribe particular ways in which counsellors should understand their work. This includes engaging with clients through a relation of power associated with their expertise knowledge about the client, in relation to the latter's experience of sexual abuse. These truths, techniques, knowledges and practices are all directed towards the health and well-being of the child or adult who has been subjected to child sexual abuse. The notion of 'expertise' is important to this process.

> *These regimes of practices give rise to and are informed by and reshaped by various forms of knowledge and expertise such as medicine, criminology, social work, therapy, pedagogy and so on. Such forms of knowledge define the objects of*

> *such practices (the criminal, the unemployed, the*
> *mentally ill, etc.), codify appropriate ways of*
> *dealing with them, set the aims and objectives of*
> *practice, and define the professionals and*
> *institutional locus of authoritative agents of*
> *expertise (Dean, 1999, p. 22)*

A recent example of this position of expertise can be found in a 2011 article published in the *Journal of Child Sexual Abuse* (Khalily et al., 2011). The authors report the words of a young woman (the focus of their case study—itself a practice of establishing an 'agential cut' (Barad, 2007) between agent and object of knowledge) who had been subjected to child sexual abuse. The woman's description of her own experience is described as subjective, whilst the authors position themselves as having the capacity to perceive and describe her experience objectively: "Subjectively she described her mood as "down"; however, objectively, she presented as normothymic but anxious." (Khalily et al., 2011, p.340). Through a disciplinary analysis, such a statement is read as a relation of power authorised by expertise.

Thus, an aim of my thesis is to examine how psychological measurement, applied to victims of child sexual abuse, plays a productive role in the constitution of the effects of child sexual abuse as an objective form of knowledge. What forms of 'governance' of such effects are facilitated by such practices, and how do they govern the activities of counsellors? What are the broader effects of this power-knowledge regime for the problem of child sexual abuse?

Measurement and governmentality

In this chapter I am discussing a way of analysing measurement and statistics which is complementary to the scientific approaches outlined in the next chapter. This approach draws on the broader notion of numbers and quantitative forms of knowledge as instruments involved in governmentality (or the conduct of conduct). This approach is not so much concerned with whether measurement is accurate or truthful, but what it enables power to do. Although scientific and disciplinary analyses of measurement can be conceptually distinguished from each other, they are also entangled. Like the representationalist model of measurement, the performative model has been developed in other fields of scientific research (i.e. it has not been a product of psychology itself). Barad's development of a performative model of science (2007) explicitly draws upon insights from the disciplinary literature, particularly that of Foucault and Judith Butler. In Chapter 4 I will be arguing that a performative model of measurement can join up with the insights of a disciplinary analysis to produce some reconfigurations of how to understand statistics and psychological measurement in relation to child sexual abuse.

The analytical tools of disciplinary power and governmentality (or the conduct of conduct), allow me to develop the connections between statistics, psychological measurement, power, and the problem of child sexual abuse. One of my reasons for doing this is that power is central to contemporary understandings of sexual abuse. However, the understanding of what power is (or *how* power is, or what power *does*) is not consistent across the various discursive arenas I have been drawing upon (i.e. science studies, disciplinary analysis, child sexual abuse research).

I can summarise and simplify my starting point like this: the field of child sexual abuse asserts that power can be possessed by one individual over another, and that it harms and prohibits freedom when misused; a performative model of measurement asserts that power is inherent in *phenomenon* and scientific *arrangements* (not with individuals), and produces its own objects; whilst a disciplinary model of power contends that power is an action and that its primary feature is it's productivity. My intention here is to tease out these diverse powers, to attempt to develop a notion of power that will successfully move through all the parts of the problem I am constructing in this thesis. My touchstone will be, as throughout the thesis, the measurement of the effects of child sexual abuse.

Power or domination?

The regimes of knowledge through which individuals are fabricated (Rose, 1998), and understand themselves, open up particular possibilities in terms of how such individuals may be governed, or included in relations of power. As described by Dean (1999), the Foucauldian concept of governmentality describes:

> *… any more or less calculated and rational activity, undertaken by a multiplicity of authorities and agencies, employing a variety of techniques and forms of knowledge, that seeks to shape conduct by working through our desires, aspirations, interests, and beliefs, for definite but shifting ends and with a diverse set of relatively unpredictable consequences, effects and outcomes (Dean, 1999, p. 11).*

In this nexus of knowledge/power, psychological testing and measurement are implicated in the governance of child and adult victims of child sexual abuse, along with their parents and partners. The relations of power and knowledge constitute counsellors as 'authoritative practitioners of a technological truth regime' about their clients (Slaney 2001), however these practitioners are simultaneously bound to particular forms of conduct themselves.

To say that there are deployments of power in therapy is not to say that therapy can never be helpful, or contribute to a kind of freedom in the lives of children or adults (Lupton, 1997, in relation to medical intervention). Some practices of freedom can involve, or depend on, the exercise of certain kinds of power (Rose, 1999). In this thesis I will not be arguing 'against power'; as I hope I will make clear, there is no possibility of an absence of power. (Domination, as opposed to power, is another matter altogether which I will also discuss). I am instead interested in the ways that power is productive and generative, rather than solely a limiting force.

One way of thinking about the 'power differences' between child sexual abuse and psychological interventions as modes of governance, is to make a distinction between relations of 'domination' and 'power'. Several texts (e.g Allen, 1999) concerned with Foucault's analysis of power refer to an interview (Foucault, 1997b) in which he described domination as the state in which power relations are frozen, blocked, or congealed. "The former [situations of domination] allow no room for effective struggle, whereas the latter [power relations] are contestable and alterable" (Sawicki, 1994, p.312, n18). I would suggest that in child sexual abuse, especially perhaps in

intra-familial abuse, that this term 'domination' is a better one than the complexities involved in the Foucauldian concept of power. Some instances of child sexual abuse may best be understood as a situation of domination, because "…power is only power (rather than mere physical force or violence) when addressed to individuals who are free to act in one way or another" (Gordon, 1991, p. 5). Foucault described the necessity of freedom for the exercise of power as follows:

> *It should also be noted that power relations are possible only insofar as the subjects are free. If one of them were completely at the other's disposal and became his thing, an object on which he could wreak boundless and limitless violence, there wouldn't be any relations of power (Foucault, 1997a, p. 292).*

Bell (1993) highlights that domination is not a pre-given state of static power relations, but is 'built-up' by tactics and practices of power. For example, the adult-child relation, which is one of dominance, is built up and maintained by two modes of power; "the authority of the Father figure (juridico-discursive power) and the disciplinary tactics" (p. 72). This dominance is "…an asymmetry built upon the constant practice of power" (p. 72). Intra-familial child sexual abuse is in this sense a 'disciplinary tactic' that produces and maintains a situation of dominance. The child has little capacity to 'contest and alter' in such a situation, given their dependence on the adult. Such a situation can have the effect of naturalizing the child's subjectivity (including, for example, the adaptive strategies they may employ to survive) by obscuring the power-infused and constructed context in which they are required to per-form their selves (Warner, 2009). (It is commonplace for

sexual abuse counsellors to say things like "It is not you that is sick or crazy, it is the situation that you survived that is sick", in an attempt to contextualize a client's lived experience of him/herself as crazy).

An alternative possibility would be to think of child sexual abuse as an operation of sovereign, rather than disciplinary, power. The power that restricts, restrains, injures, and minimizes possibilities is the kind of power that is generally accorded to the state in political theory. The ability of one subject to harm another through the exercise of power in this way requires a kind of transfer of sovereign power (Butler, 1997b). This way of thinking about child abuse by adults would be supported by the notion that adults possess the power of life and death over children, which holds for individual instances as well as more broadly as a social phenomenon. Even Foucault noted that the workings of disciplinary power required the transfer of some of the functions of sovereign power to the site of the family, the father in particular (Foucault, 2006/1973-74). Thus it may be unhelpful to think of power as either being sovereign or disciplinary. They are perhaps two forms of power that say little about the nature of power itself and more about their concrete arrangements and operations.

In contrast, I think there is more room for resistance in children's interactions with counsellors. I am not proposing that children and their counsellors can share 'equal' relationships, despite the earnest and well-intentioned efforts of some counsellors to do so. But power, I suggest, works in a different way to the situation of domination or of outright sovereign power that I have identified with child sexual abuse. Indeed, it is this room for 'contestability and alterability' that many counsellors may work with; they rely on the child to assert an

active subjectivity (to 'practice their freedom' (Rose, 1998)). Psychological techniques, whether in the form of self-help manuals or direct therapy, tend not to work through a program of subordination, but through exercising 'free will' in order to achieve autonomous individuality (Maasen, 2007).

Powerlessness in child sexual abuse

The analysis of child sexual abuse as an abuse based on unequal power often runs alongside the 'harm story', but it is a different story. According to this analysis, the problem with adults and children having sexual interactions lies in the fact that these two social groups do not share equal power and status. (This analysis implies a relation of sovereign, rather than disciplinary, power. That is, power is spoken of as 'possessable'). In such a situation of inequality, there can be no authentic consent. The child's lack of power can be attributed to either (or both) her social position as a child *vis a vie* adults, or it can be understood as a dependency inherent in the state of childhood. The former view relies on a sociological view of adult/child relations, whilst the latter is derived from ideas about development. Some authors have questioned the effects of this view of children's powerlessness, raising the question of children's agency and to what extent this can or should be accounted for in child sexual abuse (Angelides, 2004). Angelides points out that, if sexual relations are to be deemed abusive on the basis of unequal power, then all hetero-sexual activity ought to be classed as sexual abuse. According to most strands of feminist theory, women and men are in positions of unequal power (sociologically viewed). Andrea Dworkin's classic *Intercourse* makes the claim that all sex between men and women is effectively rape (Dworkin, 1997).

Judith Butler (1997a) outlines this question when she argues that the very formation of a child's subjectivity necessarily emerges within a relation of dependence.[20] In being subjectivised through this very dependence, if it is possible to talk of a child's agency it must be recognised that a child may desire their own subordination[21], as a very condition of her psychic existence (or her subjectivity, or indeed survival). Thus, the model of child sexual abuse in which an adult imposes his adult sexuality unilaterally upon the child (who is innocent, powerless and completely asexual) is, according to Butler, incomplete. It is not that the child can be said to have brought the abuse upon herself; but that the child is not a passive object who has no subjective sense of agency; and further, the dependence inherent in the adult-child relation, which forms the condition of the abuse, is a dependence upon which the child's very subjectivity depends. In sum, Butler is suggesting that child sexual abuse involves relations of power, rather than domination, but it is not clear whether *any* situation of domination is conceivable from her analysis.

A useful contrast to Butler is the work of psychologist Jennifer Freyd, particularly her notion of betrayal trauma (Freyd, 1996; Freyd et al., 2007). For Freyd, it is not the conditions of dependence that are problematised, but the betrayal of the child by the adult who abuses her or him. The child is radically dependent on the familial adult for the material conditions of survival, as well as the psychic necessities (encapsulated by the notion of attachment). Thus, the abused child is in a position of

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[20] Butler uses the word 'attachment', but my reading is that she means this in a more common sense than the technical meaning it takes on in childhood developmental psychology.
[21] To say the child's very subjectivity depends on their relations of dependence with adults is not to say that the child wants to be hurt or exploited in any way; simply that the conditions which make such abuses possible are the very same conditions which make the child's subjectivity possible.

having to continue to attempt to elicit attachment-giving responses from the abusive adult. If they did not, they would have no way of meeting their basic survival needs. However, to do so requires 'forgetting' the abusiveness of the adult. This suggests, however, that the situation is not one of domination but of power relations, because the child is clearly an active agent in the context of the abusive relationship to the extent that they must 'forget' and attempt to get their survival needs met.

At stake between Butler and Freyd is the degree of contestability, or at least the possibility of asking broader political questions, about the conditions of dependence and subordination themselves. From a strictly developmental framework (such as Freyd's), the dependence of the child on the adult is an unalterable fact (a black box, if you like), whereas this dynamics of power is precisely what Butler seeks to open up for analyses. Despite this difference, both share an acknowledgement of the fact that children actively negotiate their survival in contexts of abuse, and therefore are involved in relations of power, not simply domination (using a Foucauldian/disciplinary notion of power). It is important to note that arguing for the presence of this power in no way justifies, minimizes the harm of, nor makes the child in any way responsible for the sexual abuse. To the contrary, some therapists stress the importance of acknowledging the ways in which individuals subject to abuse have resisted and survived as crucial to healing (Wade, 1997).

Angelides (2004) examines the consequences for child sexuality of the 'feminist' child sexual abuse discourse[22].

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[22] Angelides' paper focuses on a narrow range of 'dominant' feminist discourses of child sexual abuse. There are multiple feminist analyses of

According to Angelides, in the feminist analysis of child sexual abuse, by constructing children as totally powerless, the adult-child opposition is reinforced and confirmed. This is a product of the judicial view of power held in the feminist analysis of child sexual abuse; i.e. that adults 'possess' power and hold it over children, and that children are completely powerless. Angelides suggests a Foucauldian informed understanding of power as exercised through practice, and recognising that children do exercise power, or at least have a sense of exercising power, if their understandings of their own experiences of sexual abuse are listened to. He criticises the therapeutic maxim to convince the child of their complete innocence and powerlessness, which is aimed at minimising a sense of guilt or self-blame for the abuse, arguing that this inadvertently reinforces and normalises the child's sense of powerless, and diminishes children's capacity to make choices about sexuality.

To give Angelides' argument it's fuller context: Angelides reviews some of the pre-1980's psychological literature, which tends to highlight the child's complicity or agency in sexual contacts with adults. This was a possible position to hold because it was generally accepted, through Freud and psychoanalysis, that children had sexual impulses, urges and desires, in addition to sexual imagination and fantasy. Thus the proposition that children might not necessarily be harmed, or may even enjoy, aspects of what would now be considered sexual abuse, was a 'makeable' claim. In contrast, Angelides stresses that his argument is not against the problematisation of adult exploitation of childhood sexuality, but that the absolute emphasis on child sexual abuse as an abuse of power

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child sexual abuse, (e.g. the contributors to Warner & Reavey, 2003), some of which do not fall within Angelidies' apparent definition.

effectively had the consequence of precluding acknowledgement of children's sexuality per se.[23]

One of the shortcomings of the judicial view of power, and its deployment as an argument against child sexual abuse, is that there are few relations between individuals that are not infused with relations of power. Additionally, there are many domains of life where adult power over children is accepted and even expected to be exercised, even where children themselves may object to this deployment of adult power (school attendance must be a prime example). "…if inequalities of power are thought only to corrupt sexual and parent-child relations, then there can be no ethical sexual or parent-child relations" (Angelides, 2004, p.151). In other words, the 'feminist' (sic) analysis of child sexual abuse creates an impossible ethical *telos* where consent can only occur in unachievable situations of equality (understood as the absence of power relations). Angelides point is that there are no relations between *any* individuals where power is absent.

If I am going to be consistent and carry through on this analysis of power in the context of child sexual abuse, there are some implications for how to understand psychological measurement in a research or therapeutic situation. The classical model of measurement in research would be consistent with the idea of domination; there is only one powerful actor (the researcher in the research situation) who observes and describes, through the proficient use of a measuring instrument, properties of a

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[23] The use of the term 'erasure' in Angelides' title (*Feminism, child sexual abuse, and the erasure of child sexuality)* is potentially misleading, considering Foucault's general point in *History of Sexuality vol. 1*; neither the feminist nor psy discourses on child sexual abuse *erase* child sexuality so much as produce a body of knowledge that constitutes child sexuality in particular forms. To argue that such knowledge regimes *erase* child sexuality could be taken to imply that 'child sexuality' has a certain nature or form that is improperly (un)represented.

passive object of study. The entire arrangement is understood to be a direct result of the will of the one with power (Barad, 2007). In taking on board a disciplinary model of power, however, that recognises the entanglement of power relations and the agency (however minimal) of all actors in the arrangement, then such a clean split is not possible.

This articulation of the complexity of power relations enables me to recognise that it is possible to talk about power relations in research and therapy, and power as a defining feature of child sexual abuse. It is the complexity of these diverse powers that is the point, not the achievement of a single model of power I had hoped for at the beginning of the chapter!

Foucault and the regulation of 'adult-child sex'

To what extent does Foucault's work demonstrate any objection to the sexual (ab)use of children by adults, or sexual abuse in general? If it does not, or is ambiguous, this represents a significant methodological/ethical dilemma for my thesis. What is at stake is not the question of whether Michel Foucault was an uncaring person or a 'bad' philosopher, but whether the work of Foucault can be usefully turned to these questions of child sexual abuse. I make no apology for being cautious on this issue. Although I am questioning and being troubled by some practices of scientific knowledge production in relation to child sexual abuse, I want to ascertain that the tools I am using do not lead to the fabrication of a position that condones, or fails to object to, child sexual abuse *per se*. As I show in this chapter, doubts have been raised by many authors about whether Foucault's resources can be usefully turned to this kind of project. While it may seem like an unnecessary detour, I justify this based on the discussion to follow. I leave it to the

reader to decide at the end of this discussion whether the interlude was required.

One episode that raises some questions about using Foucault's work for investigating sexual abuse is related not only to his academic work but to a particular issue he publicly became active about. Foucault (along with many other French intellectuals of the time) advocated for the removal of prohibitions on 'consensual' adult-child sex. I do not mean to claim that Foucault's personal or civilian life determines whether his work is 'bad', however there were some identifiable Foucauldian analytics at work here. My question is: Does this mean that Foucauldian perspectives on childhood, sexuality, the family and so on inevitably come to this problematic position of endorsing adult-child 'sexual relations'?

Bell (1993) cites a 1977 submission signed by Foucault (along with many others) to decriminalise 'consensual' adult-child sexual relations. The argument was based on attempting to disrupt the naturalised 'childhood' on which such laws are founded. The petition (information on which is available at http://www.answers.com/topic/french-petitions-against-age-of-consent-laws, including a full list of the 69 signatories which included Deleuze, Guattari, and Simone de Beauvoir), and a 1978 radio interview given by Foucault, Guy Hocquenghem, and Jean Danet, have been taken up as texts, as objects of enquiry, by several feminist theorists (e.g. Bell, 1993; Alcoff, 1996) who find aspects of Foucault's work useful but problematic. (It is important to note here that although Foucault supported the petition, he was neither the instigator nor main figurehead. He was clearly part of a larger group bringing this question into the cultural arena for discussion. Nonetheless,

aspects of his analysis of sexuality and power are clearly present. If such analysis can be deployed in the service of arguments to legitimate what I want to call child sexual abuse, I argue that close scrutiny is warranted regarding how this deployment operates and the possible implications for this thesis).

Hocquenghem stated that the purpose of their petition was to protest "the traditional prohibition against sexual relations without violence, without money, without any form of prostitution, that may take place between adults and minors". He believes that such sexual relations are treated as 'worse' than situations where there is clear coercion or force. One of the key arguments made is that the laws against adult-child sexual relations act as a repressive technology of surveillance over the lives of children. The law is infused with psy knowledge that insists that adult-child sexual interactions are always traumatising for the child. They argue that the psy-legal nexus refuses to hear or misuses the words of children who say they consent to such relations (Foucault et al., 1988). This question of what children say, and the use to which it is put, also forms a major focus of many feminist/child-protection advocates. However, they insist that it is when children say *no* that they are not listened to (Taylor, 2004), in contrast to Foucault, Hocquenghem & Denet's stance that it is when children (apparently) say *yes* that is problematised.

Foucault and his fellow petitioners imagined that adult-child sexual relations can occur in a context of equality, much like Rind et al. (1998) but from a different perspective. Rind et al argued that their position 'backwards' from their finding that 'sexual interactions' with adults did not always harm the child's development, and this affirmed the developmentalist

paradigm. In contrast, the proposal of Foucault et al was to disrupt this developmentalist paradigm through the argument that psy-knowledge about children produces their inequality. They argued that their proposal was not about violent incidents or rape; in contrast, feminist analyses would consider adult-child sex to effectively *be* rape, but this does not mean that a feminist opposition to child sexual abuse *must* defer to developmental psychology (although it may, and often has been strategically useful to do so).

In other words, one cannot point to the "categories of 'sexual coercion, sexual assault and rape' as if these are unambiguously defined, in actual fact their scope of application is constantly being contested" (Alcoff, 1996, p. 114). Bell (1993) understands this feminist position to be based on unequal positions of economic, social and political power, rather than any naturalistic notion of development. Alcoff argues that the questions should be about the historical conditions that give rise to desires between adults and children (Alcoff, 1996). But, by arguing the line that children are unable to consent because of unequal power relations, the question arises- are any two individuals *ever* in a relationship of equality, given the feminist problematisation of the broader issue of consent between women and men (Angelides, 2004), and indeed Foucault's own analytics of power? For Bell, this is the interesting part of the challenge raised by Foucault's questions.

Bell points out that the petitioners seem to want to dismiss the fact that childhood and children's experience of childhood and sexuality is constructed (which is Foucault's argument in *The history of sexuality*), and in this construction they are less powerful in relation to adults. That is to say, the proposal seems to simply 'wish away' the fact that children (or, at least,

individuals subjectivised as children) are, in this current epoch of social relations of power, in a relatively powerless position with adults (regardless of instances in which they may resist such power), by replacing it with a situation in which children are legislatively regarded as capable of being equal with adults. In a sense this constructs the child as the free, rational citizen of liberal democracy unfairly repressed by the law, a position which Foucault himself so thoroughly critiqued. While this state of equality might be a theoretically utopian state of affairs, I agree with Bell's conclusion that in this case 'the proposal' constitutes a danger which ought to be opposed. In framing the law as repressive, the possibility that adult-child 'sexual relations' may have repressive effects is excluded (Alcoff, 1996). That is to say, "the repression of adult-child sex may effect a decrease in the constraints by which children's own sexual energies are policed, managed and deflected" (ibid, p. 111). The Foucauldian proposal would result in the 'liberation' of (primarily) adult men's sexual desires, despite the rhetoric given to children's empowerment to consent (a notion which, as noted, is contradictory to Foucault's analysis of the constitution of sexuality and freedom). Therefore it is hardly a radical proposal, effectively shoring up the sexual rights of adult men and legitimating their sexual abuse of children.

In a similar discussion, Carolyn Steedman (1994) discusses an identical proposal by James Kincaid. (It is interesting that in both cases male academics are arguing for the legitimation of sex with children, and women arguing against). She describes and critiques Kincaid's (1992) work (titled 'Child-loving. The erotic child and Victorian culture'), where he deploys the term 'child-loving' to refer to a range of practices undertaken by adults. Under this rubric, he includes both the molestation of children and helping children, as well as a range of other child-

focussed adult activities. That is, he collapses all adult interest in the happiness, well-being, and safety of children to this category of an eroticized 'child-loving'. He claims that the intense focus in contemporary culture on the sexual abuse of children is a kind of cultural denial, of the erotic pleasure that all adults find in children. Steedman finds Kincaid's analysis of paedophilia at the centre of Western cultural history 'illuminating' (p. 166), but takes serious issue with the proposal that he derives. Essentially, following a Foucauldian program of 'bodies and pleasure released from power' (Kincaid, 1992, p.386, Steedman, 1994, p.167), he advocates for the lifting of legal prohibitions of adult-child sexual relations.

Steedman's objections can be summarised as follows

- Kincaid ignores the evidence from children and child sexual abuse survivors that such abuse '*hurts*, either physically or psychically', even when as children they would not necessarily have understood it in this way[24].

- Throughout his book Kincaid discusses children as 'images, bodies, beings located in a field of adult desire' (Steedman, 1994, pp. 167-168). To then suggest

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[24] This is no straightforward point in the context of my argument. In one way, Steedman's use of the word 'hurts' is a helpful way to identify the 'harm' which I have attempted to problematise throughout this thesis. The point I am trying to illustrate is that a notion and experience of harm that has been produced through the psychological power/knowledge nexus, whilst perhaps dominant within therapeutic practices, is not the only discourse of harm available. Perhaps the word 'hurt' can signify these other possible ways of understanding unwanted, unpleasant, dangerous, or otherwise deleterious experiences. This is not to say that 'hurt' is a more immediate or phenomenological experience; it merely indicates the presence of other discourses (apart from psy knowledges) through which child sexual abuse can be understood. For example, one may be speaking of bodily harm such as bruising or tearing; or of having experienced an injustice. Of course, one must acknowledge that much of the evidence to which Steedman refers has been presented in a psy context, most obviously in relation to 'psychological' hurt. Statements by children or adults regarding harm or hurt caused by sexual abuse, whilst not statements one would wish to deny, can nonetheless be examined for the discursive regime in which they are embedded; the same applies to statements made by counsellors about their clients.

that the claims of these children to have warm 'feelings' towards the adult paedophile as a platform for his proposal is incongruous with the way childhood and children have been constituted throughout the rest of his book.

- He asks the wrong question- why children want sexual relations with adults, rather than the opposite, more obvious question.
- "The severer predicament in which the argument finds itself is due to thinking that history proffers any kind of solution at all to the weird problems it allows us to see taking shape and form during the recent past" (Steedman, 1994, p. 168)

Taken together, the work of Bell and Steedman constitutes a convincing argument against the proposal that the prohibition of adult-child 'sexual relations' be legitimated.

Bell (1993) argues that whilst the exposition of the discursive 'line' that Foucault's work has been used to reveal between adult and child sexuality is theoretically interesting and maybe even convincing, in practice its implications are likely to worsen rather than improve the legal situation regarding child sexual abuse. Bell argues that in relation to Foucault's statements on childhood sexuality and on adult-child 'sexual relations', one can follow his theoretical insights and line of questioning without agreeing with the "'ethico-political' decisions he offers" (p. 150), however it certainly has raised doubts for the usefulness of his writing for this project.

Having identified some limitations and cautions in using the work of Foucault to understand child sexual abuse, I will now

turn briefly to the same concerns in the broader context of gendered violence.

Foucault and gendered violence

Given that the feminist analyses I am drawing on share the claim that sexual abuse of children is a gendered issue, I want to briefly address this broader question. It has been suggested that Foucault's work was not particularly concerned with the situation of women (Plaza, 1981, cited in Marcus, 1992). Other feminist writers have drawn on Foucault's work, particularly his destabilising of essentialist notions of sex and sexuality, but the relationship between Foucault and feminism is marked by tension and ambivalence (Bell, 1993; Sawicki, 1994; Hekman, 1996). This is particularly marked where questions of sexual violence are concerned (Bell, 1993; Alcoff, 1996; Joy, 2003; Howe, 2008).

A couple of key quotes attributed to Foucault are frequently cited to affirm these suggestions that Foucault was unconcerned with the situation of women and minimised the problem of sexual abuse. Monique Plaza (1981, cited in Marcus, 1992; see also McNay, 1992) refers to a 1977 interview where Foucault discusses the problem of how best to legally respond to women who have been raped. His suggestion is that sexual violence should be regarded no differently to 'a punch in the mouth'; that is, as an act of violence, not specifically as an act of *sexual* violence. A problem is that this approach seems to betray his own analysis of the centrality of sexuality to modern understandings of the self (McNay, 1992). If subjectivity and identity have no essentialist basis, if our sense of ourself is discursively produced, and such discursive productions place sexuality at the core of our self, then it seems both

disingenuous and simplistic to resist this centrality by simply willing it away (Weeks, 1989). (This is the same problem in his proposal about the decriminalisation of adult-child sex). It also betrays a blindness to gender as a mechanism of oppression (McNay, 1992).

Despite the intention of Foucault's argument to free sexuality from disciplinary power, the general response of feminist critics is that this solution remains unconvincing at best. At worst, it would "further legitimize the sexual oppression of women" (McNay, 1992, p. 45). It has even been suggested that that some of Foucault's statements "…suggests that rape laws should protect the sexual expression of rapists before that of their victims" (Hengehold, 1994, p.89).

The second key quote is from *The History of Sexuality vol. 1.* Throughout this book, Foucault is pre-occupied with "…all those social controls, cropping up at the end of last century, which screened the sexuality of couples, parents and children, dangerous and endangered adolescents- undertaking to protect, separate, and forewarn, signalling perils everywhere, awakening people's attention, calling for diagnoses, piling up reports, organizing therapies" (Foucault, 1978, pp. 30-31). However, it appears that he is supremely indifferent to the fact some of these 'perils' may have resulted in suffering for those subjected to them (Howe, 2008). Discussing the apparent molestation of girl by a farm hand (in the village of Lapcourt, France) in 1867, Foucault marvels at the 'pettiness' of the fact that this was responded to by the authorities; according to Foucault, these were nothing more than "inconsequential bucolic pleasures" (p. 31). Clearly, Foucault sees no problem with an adult man paying a young girl for his sexual gratification. He neglects to consider the extent to which such a

sexualised interaction performs a kind of governance, or in some cases domination, in children's lives.

Summary of Chapter 2

From this discussion, it is clear why a straight-forward appropriation of Foucault is problematic for understanding the sexual abuse of children by adults. It is perhaps reflective of the time and context in which Foucault worked, more so than any callous disregard, that led him to focus on offenders rather than victims of sexualised crimes. It was the offender who was subject to the machinations and discipline of psy knowledges, more so than the victims (who may not have been considered much at all, or as a risk to other children's innocence, or deserving more of moral condemnation than therapeutic intervention (Smart, 1999), or of the crime of 'being in need of care and protection' (McCallum, 2008)).

Where I think the value of Foucault's work lies is in thinking about the way that the child victim of sexual abuse is 'disciplined' by the psy knowledges, and the way that the role of social workers and other counsellors is also produced by these disciplinary practices. Whereas Foucault's political concern seemed to be mainly with the subjection of adults categorised as deviant or abnormal (including not only adults who committed sexual crimes, but also those categorised as homosexual), this thesis proceeds on the claim that some of the categories and analysis produced by Foucault (discussed through this chapter) can be useful for thinking about those who have been subjected to child sexual abuse and the associated research and therapeutic practices. More generally, the insight that sexuality itself, as well as childhood, is produced through discourse provides productive ways for

thinking about child sexual abuse. It is these tools, rather than any way of defining abuse itself, that I intend to utilise from Foucault's work.

Having discussed these cautions regarding the use of Foucault's work (and perhaps having persuaded the reader of, if not the conclusion, then at least of the necessity of the discussion), I will now move on some of the other theoretical and analytical tools I draw upon for this thesis.

Chapter 3- Analytical practice

In this chapter I go into further detail about the use of focus groups and published research texts as sources of primary data for this thesis. This will require a problematisation of terms such as 'focus group', 'discourse' and 'data'. I am particularly concerned here with how to analyse texts; published research papers and focus groups discussions. As the chapter develops, I indicate the way that these analytical traditions disrupted and transformed my approach, especially in relation to the use of focus groups. By the end of the chapter, this research practice becomes destabilised, and I attempt to put together an analytical practice that suits the demands of my project. Of all the chapters, this chapter makes the least direct reference to matters of measurement *per se*. This is perhaps in response to the fact that much of the work or significance of numbers and measurement consists in what is said or written *about* them. Accordingly, I need a way to conceptualise not only how such written and said things (statements) have been produced, but also how I go about analysing these statements for this thesis.

Methods

In this section I outline my research method- how was the data produced and what conceptual tools were used to analyse it? The thesis rests on an analysis of two sets of texts (published research, and focus groups discussions). It is necessary to attempt to demonstrate to the reader how the texts have been produced, read, and put to work in this thesis. The method I have developed is, I argue, compatible with the overall theoretical orientation being developed, while trying to avoid

the dangers of becoming overly concerned with whether or not the method is Foucauldian (Graham, 2005).

It might be expected that this thesis would offer a definition of *child sexual abuse*. This expectation will not be met directly. This is mainly for the simple reason that the act or crime of child sexual abuse is not the focus here. The focus is on therapeutic and research statements on the effects of child sexual abuse. This can be put simply as follows: for the purpose of this thesis, child sexual abuse is defined as whatever a particular text or speaker says it is. Child sexual abuse is being *produced as an object of (scientific and/or therapeutic) discourse* by the speech or texts. That is to say, if a research paper discusses the effects of child sexual abuse on the research subjects, the research paper will be interrogated for the terms on which this understanding is made. Part of the thesis is to investigate how these discursive practices produce and manage child sexual abuse: to offer a definition here would be to already answer some of the question being posed.

Focus groups

Extracts from the focus groups discussions will appear in the following format:

> NANCY[25]   *So then it's really subjective, isn't it?*

> KATE   *Well of course it is.*

> NANCY   *Yeah.*

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[25] The names of focus group participants have been changed.

KATE   But your assessment's subjective.

NANCY   Hmm, yeah.

KATE   Your individual assessment will be subjective, probably, you know, we would hope…

NANCY   So what's the purpose of the test?

KATE   We would hope we would get significant…[turning to Nancy] are you the person or are you…[meaning 'are you the researcher?']

[all laugh]

CB This is why we have focus groups.

The first set of texts are transcriptions from two discussions that I convened with a group of individuals employed as child sexual abuse counsellors in a busy community-based organisation in a capital city of Australia. The team I spoke with was one part of a larger organisation offering a range of community based services. The team receives funding from the state government to provide counselling services to children and adolescents who have experienced sexual abuse, as well as to their family members. At the time of the focus groups it employed approximately ten workers to undertake this and related work in the field of child sexual abuse. The team also supervises social work and psychology students on placement.

I was previously employed as a counsellor in this team, but had ceased working there by the time the research discussions were held. Initially these groups were conceptualised as focus

groups, a standard method of data collection in social research. My role was presented as facilitating a discussion on the use of standardised psychological testing in therapy with child victims of sexual abuse.

Between conducting these groups and preparing the thesis as it appears now, however, I might be said to have undergone a discontinuity in subjectivity in the intervening period. In other words, this thesis has been animated by (at least) two different sets of propositions about what kind of data could be generated in such focus group discussions. In the following sections, an account is presented of the problematisation of the focus group as a method for this research. The speech of the participants will be interrogated, and the contributions of the researcher to the discussions will be regarded as fully implicated in the discussions, and thereby presented as a legitimate object of enquiry. The questions that will finally be asked of these discussions are: What are the conditions by which these specific discussions were made possible? What do these texts suggest about the discourses that animate or inhabit them (Wooffitt, 2005)? Secondly, I want to draw on the data to address my primary research questions, acknowledging the issues raised by the problematisation I just mentioned.

I intend to analyse the focus groups as achieving three functions: 1-producing and circulating knowledge; 2-constructing the object of knowledge (child sexual abuse, its effects, and relevant therapeutic interventions), and; 3: constituting the counsellors. In what follows, two ways of understanding focus groups as a research method will be developed, in an attempt to identify what can be productive about this research method whilst maintaining an epistemologically justifiable conceptual framework.

My initial conceptualisation of focus groups

Lunt and Livingstone (1996) credit Lazarsfield and Merton with the introduction of the focus group to the social sciences. Krueger and Casey outline the general characteristics of a focus group:

> *The purpose of a focus group is to listen and gather information. It is a way to better understand how people feel or think about an issue, product, or service. Participants are selected because they have certain characteristics in common that relate to the topic of the focus group (2000, p.3)*

Other methodologists claim that focus groups are useful for "exploring how points of view are constructed and expressed…the study of attitudes and experiences around specific topics….examining how knowledge, ideas, story-telling, self-presentation and linguistic exchanges operate within a given cultural context" (Barbour et al., 1999, p.5). It seems, then, that the focus group would be a useful method for identifying the discursive struggles over meaning that operate within the field of child sexual abuse counselling and the regimes of knowledge that inform these struggles, following the claims of some persuasive texts that discursive struggles are manifest in social interaction as well as in written texts (Barbour et al., 1999; Cameron, 2000).

An initial plan was envisaged for a series of focus groups, each consisting of 6-8 participants (Lunt et al., 1996 say 6-10; Krueger et al., 2000 suggest 6-8; Hamel, 2001 suggests 6-12 is

the optimum number of participants), who share the characteristic of working as a counsellor with children who have been subjected to sexual abuse. Green & Hart prefer the term "discussion group interviews" over 'focus groups' (1999, p. 21) to describe such groups where there are pre-existing relations between group members, as was the case in these groups. The two groups convened took place in one agency, with a team of counsellors employed to undertake therapy with child victims of sexual abuse. Nine individuals (not including myself) were involved in total, with some participating in both discussions while others participated in only one of the discussions.[26]

The participants were recruited by the researcher first approaching the management of the agency, with a brief description of the research questions and aims. After meeting and gaining permission from management, I sent an information letter outlining the research (see Appendix Two), along with an invitation to participate. After receiving notifications of interest from several staff members, a mutually suitable time and location was organised via correspondence between myself and a nominated member of the staff.

The primary professional groups were Psychologists and Social Workers, as these are the disciplines generally recognised as authorised to undertake this work. (I will refer to 'counsellors', specifying particular professions when needed). The focus groups were envisaged as a guided discussion, initiated by myself as a researcher/facilitator, of the theoretical and philosophical understandings that the participants perceived themselves as bringing to the work, especially in relation to the

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[26] I also attempted an online discussion group with three additional participants; however this was unsuccessful due to technical difficulties.

practice and theory of psychological measurement. As the facilitation of focus groups requires skills in group facilitation (Krueger et al., 2000), I proposed calling upon my experience as a Social Worker to conduct the focus groups. This would involve deploying technical know-how about facilitating a group discussion.

A number of themes for the focus group were developed and presented in advance to the participants. These were:

- Identifying particular psychological tests that counsellors use in their work with children who have been sexually abused (if, indeed they are used)
- Discussing the details of these tests, such as what traits they are used to measure, what kinds of clients they are used with and why they are used
- Explore the details of how these tests measure the traits they claim to measure
- Discuss how the process of testing is managed within therapy
- Discuss how the counsellors use the results of these tests in the formulation of therapeutic plans and goals with their clients
- Discuss counsellors' views on the ethics of testing
- Are there any particular organisational requirements related to the use of tests? (For example, is the use of some tests routine with certain clients?)
- Discuss the counsellors views on the philosophical dimensions of psychological testing through prompt questions such as:
  - Are psychological attributes quantitative in structure?

- o What power dynamics are involved in administering tests?
- o What is the relationship between the use of standardised testing and the exercise of clinical judgement in therapy?
- o In what ways do counsellors find the use of testing helpful or unhelpful in their practice as counsellors with child victims of sexual abuse?
- o What are counsellors' views on how children might experience the practice of psychological testing?

The above provides a productive point of departure to develop a critical perspective on focus groups, informed by the constructivist approach to knowledge that has been referred to throughout this thesis. That is, the above descriptions of what a 'focus group' is, and what might be done with the resulting data, have produced a kind of knowing which can now be unsettled.

Before proceeding, it is relevant to note that the focus groups consisted entirely of members of one organisation, in one location at one point in time, and therefore the resultant data is not generalizable. What I am interested in is precisely the specifics of the conversations, and more importantly, what made these conversations 'have-able' or possible. It is entirely possible that another group of workers would have opened up different points of enquiry and produced another set of problematisations to be examined.

Adjustment to the depth of field

What can be seen in a focus group? A helpful visual tool here is the 'depth of field', a concept from the field of optics applied in disciplines such as photography, cinematography and microscopy. In film and photography, the depth of field refers to the distance between the nearest and the furthest point in the portion of a scene which is held in acceptably sharp focus (Galer, 2007). As one photography text explains, with a shallow depth of field: "Subject matter behind and in front of the point of focus appears progressively out of focus" (ibid., p. 23). Although the lens can only focus on one point, the transition from in-focus to blurry may be gradual, so that although there may be some blurriness within the depth of field, this is not always easily perceived.

When one looks with a shallow depth of field, a small amount of text can be seen very sharply. "However, the only way that shallow depth of field is apparent is if there's something in the background that is noticeably soft and blurry" (Long, 2012, p.208). This is illustrated in Figure 1 below.

Figure 1- Depth of field[27]

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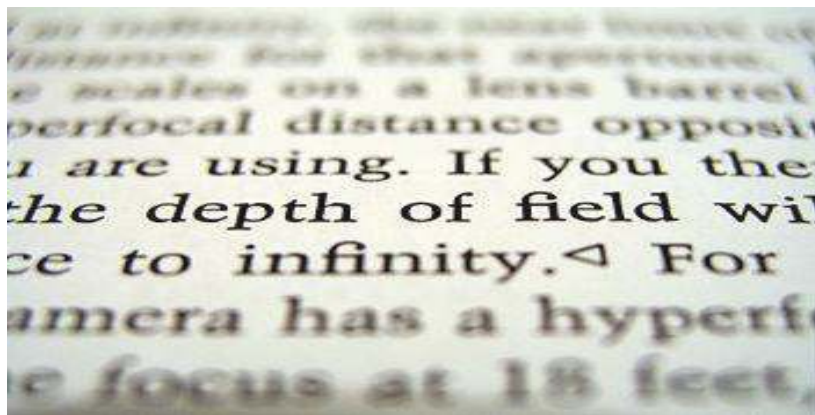
[27] DOF-ShallowDepthofField.jpg by PiccoloNamek, available under the Creative Commons Attribution-Share Alike 3.0 Unported license at http://en.wikipedia.org/wiki/File:DOF-ShallowDepthofField.jpg

So with the shallow depth of field implied by restricting the analysis to the level of the text from the focus groups, the capacity to see clearly around the edges and beyond is diminished. Yet, the blurriness of the surrounds is in danger of going unnoticed: "having larger, easier-to-see objects in the background will make your shallower depth of field more apparent" (Long, 2012, p.208). What 'conditions of viewing' might enable, not only the blurriness to become noticeable, but to also bring the distant objects into the field of visibility?[28]

> *ELIZABETH So on the BASC if you had the depression come back clinically significant you might say 'further stuff is warranted, further exploration is warranted' so you might give the Children's Depression Inventory. Which is, once again, really subjective and I certainly wouldn't take it as the gospel, I would use it, yeah effectively, so…*

By focussing on a comment such as Elizabeth's, I could ask how Elizabeth has 'brought into existence' the depression of a child, the clinical significance of this fact, the question of whether this conclusion is a subjective measure, and so forth. This could be broadened out to examine what this comment achieved in the social interaction of the focus group: what did other participants say, what had been said beforehand, was this comment challenged, etc.

None of these questions, however, can ask how such comments were made possible; the *statements* 'existed' before being uttered by her. In contrast to the idea that Elizabeth brought the

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[28] To reinforce the analogy, Carl Friedrich Gauss, who I cite later in relation to the error law, also developed Gaussian optics, useful in understanding and calibrating the focussing capacities of lenses.

statements into existence, this thesis will try to ask how a certain discourse brings both the object and the subject into existence. Elizabeth speaks *of* therapeutic objects (such as depression, clinical significance), and in doing so speaks *as* a therapist. In this sense, a therapeutic discourse has laid the conditions for Elizabeth to speak herself into this subject position.

The term *statement,* like *discourse*, is one that Foucault's work endows with a special meaning and purpose that is distinct from more common usages of that term. Whereas a statement might be thought of unproblematically as simply something that is said, in Foucault's usage the statement itself is not quite so immediately apparent. Statements are "…there and say everything", but "become readable or sayable only in relation to the conditions which make them so" (Deleuze, 1988, pp. 46-47). Entire works of Foucault are dedicated to describing these conditions; for example, in relation to madness (Foucault, 1965) or the 'human sciences' in general (Foucault, 1970). In order to be able to speak of statements in this sense, it is necessary to 'open up' the texts (Deleuze, 1988).
It is possible, for example, that one sentence might appear in two different contexts and serve completely different purposes and suggest different meanings. Davidson describes these contexts as 'styles of reasoning', making it possible that identical sentences can function as two different statements (Davidson, 1987); I will come back to this point shortly.

The use of speech as data

A potential difficulty with using Foucault is that this thesis takes as one of its primary data sources speech from participants in focus groups, transcribed as text, whereas for

some Foucault's methods were primarily 'archival' (Elden, 2001)[29]. Taking this archival approach would require having access to, for example, confidential counselling records, containing the results of psychological tests conducted in therapy, and the associated reports of counsellors. While such a study is imaginable, this thesis draws upon what counsellors have to say about their use of psychological tests, and the subject positions they produce in doing so. Some would say that this is not a 'Foucauldian' approach to research. It could be said that this thesis makes use of Foucault's work to develop a conceptual framework (Marshall et al., 1999), but must draw on other sources to develop the research method.

What is methodologically important for this thesis is not whether things are written or said, but that they are studied at the level of discourse, to study the conditions through which the statements exists, rather than attempting to attribute them to an "all-powerful subject" (Foucault, 1991b, p. 60) who speaks from with-out discourse, or posing psychological explanations about the motivations of the speaker or writer. In fact, the position or the role of the speaking subject *in* discourse is itself an object of enquiry.

The use of the speech of research participants has some precedence in other Foucauldian informed studies relevant to child sexual abuse (Slaney, 2001; Joy, 2003). Rayleigh Joy notes that her use of interviews with women presents an epistemological challenge to a Foucauldian research project-the practice of interviewing might be thought to presume a self

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[29] One key reason for this is related to the idea of an historical *episteme*, the set of conditions for knowledge that make some statements 'candidates for truth' (Foucault, 1970). If all possible thought is already embedded in the episteme of the times, then the episteme itself cannot be captured or described 'from within'. This is a methodological reason that Foucault tended to focus on historical problems.

that can disclose the truth of her experience, unmediated by discourse. However, she cites Foucault's later work on "…discourse as a non-vertical, unordered criss-crossing of forms of power and forms of knowledge, suggests that the power/knowledge nexus does indeed encounter selves and is in turn affected by them" (Joy, 2003, p. 124). I take this to mean that the use of interviews (or in my case, focus group discussions) is one way of drawing attention to the way in which subjects are both users of discourse, as well as being constituted by discourse. Subjects constitute themselves through discourse, as well as simultaneously reproduce, reshape and resist the discourses they deploy. Subjects are not "inactive dupes of the discursive field" (ibid., p. 124); nor is the researcher positioned outside of the discursive field, able to 'objectively' identify and get hold of the discursive field that his/her 'subjects' are merely passive conduits for.

Thus in this thesis, I use focus groups with the aim of engaging with the ways in which counsellors constitute themselves, with a particular emphasis on how the practice of psychological testing and measurement fits in with such moves.

'Collaborative' research and power

Focus groups inherently imply a collaborative element to the production of knowledge. As Trickett and Espino (2004) demonstrate, collaboration is by no means a simple or unitary construct. A range of epistemological, pragmatic and ideological rationales provide the foundation for the theory and practice of collaboration in social research. These rationales largely share a common assumption that there is a generalised power imbalance between the researcher and the participants, a particularly important concern for feminist research (Oakley,

2000). This assumption stems from the historical tendency for social science research to study marginalised groups, whether in the name of liberation, science, or social control. There is a more recent tradition of 'studying up' (i.e. studies of particularly powerful groups in society).

My thesis might be more aptly thought of within this angular mode of description as studying 'across'. In some regards, I have exercised power where the research itself is concerned: I have formulated the research topic and questions, I have invited the participants to the focus groups, I have recorded and analysed the data, and have final say over its presentation in my research (Wilkinson, 1998). However, in other important ways I am/was not in a position of relative power vis-à-vis the participants. I had worked in the same specialist field, and some of the participants would be regarded as more experienced in this field; I am dependant on the good will and interest of the participants to contribute to the focus groups; as I was relatively new to the geographical area, I did not have long-standing networks in which to ground and position my personal and professional views and practices; I did not have an established 'profile' in the field. These are all relevant considerations in facilitating focus groups with peers.

Still on the topic of power within focus groups, some authors suggest that the focus group is a more democratic site than the one-to-one interview or the survey. For example, Madriz states, that "…the group situation may reduce the influence of the interviewer on the research subjects by tilting the balance of power towards the group. Because focus groups emphasize the collective, rather than the individual, they foster free expression of ideas, encouraging the members of the group to speak up" (Madriz, 2000, p. 838). This statement could imply that the

group is homogenous, and that there is consensus and equality amongst group members.

Taking into consideration questions of professional status, networks, education, ability to coherently articulate an argument, reputation etc., this would be a problematic assumption to make for my focus groups. Madriz clarifies her position: "Although it can be argued that there is potential for power relations to surface among the participants, these relations, if they arise, are the participants *own* power relations, in their *own* constructed hierarchies. Indeed, observing and documenting the development of these hierarchies may provide the researcher with some very important data" (ibid, p. 840).

Even in recognising the potential for power differences between group members, the type of power that Madriz's statements seem to be referring to is a naturalised, juridical/sovereign mode of power, where power is possessed by one person or group and exercised over the less powerful group members. There is an implication that *ideally* there would be no relations of power between group members, and that when it is recognised it should be minimised or mitigated against. The attainability of such a goal has been questioned within feminist methodological debates. Claims of equality and a democratic process can serve simply to obscure power relations and make them less visible, particularly when there is a sense of relationship between researcher and participants (Stacey, 1988, cited in Oakley, 2000). This seems to be of particular importance in a study relating to child sexual abuse, where abuses of power routinely occur within very intimate or close relationships.

Drawing on a disciplinary model of power, I would argue that each speaker's very capacity to speak is constituted through operations of power that produce their subjectivity[30], subjectivity being the necessary condition for speech. In this view, power does not repress or prevent individuals from speaking, but it is what produces the very possibility of speech. Thus, whenever individuals are speaking (or writing), there is power at work. Power per se is not something to be avoided, however one can (and I argue, should) still try to account for one's entanglement in relations of power and practice (Butler, 2005; Barad, 2007).

Whatever power relations may be performed and reiterated through the focus groups themselves, I have not made any claims or declared any intention to democratise the interpretation or analysis of the focus group texts. I argue that I am not making any evaluation or interpretation of individual counsellors' practice or judging the validity of what they said in the groups. As I am interested primarily in the statements made at the level of therapeutic discourse, I am interested in how it was possible for the counsellors to say what they said within a professional context. Furthermore, the focus groups produced a problematisation of both the knowledge produced by psychological measurement, and the practice of actually conducting (one variety of) psychological measurement in the context of working with children who have been sexually abused. This second point proved particularly productive for this thesis in generating a set of problems around the theme of 'subjectivication' of the counsellors—in other words, how practices of psychological measurement govern their activities and what kinds of resistances emerged.

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[30] As has been discussed, power is also entwined in the production of knowledge about the object being spoken of.

Before drawing together these considerations on how to use the focus groups in this thesis, I will discuss the second group of texts that I draw upon as 'data'.

Published scientific research on child sexual abuse

In drawing on these texts, I attempt to retain the genealogical principle that the distinction between primary data and secondary (commentary) be collapsed. For example, some texts are published as commentary on the Rind et al. (1998) paper mentioned earlier, while others offer alternative findings based on different data, methodology, and statistical analysis. The possibility of some texts speaking from distance, from outside the regime of knowledge/power, is called into question; all such texts are to be located within the "field of power" (Bowman, 2005, p. 26). I will treat both kinds of texts as equivalent in terms of their circulation in the field of child sexual abuse research. Again, the questions will be asked of these texts: What are the conditions by which they were made possible? What do these texts suggest about the discourses that animate or inhabit them? And how do these texts produce the object of knowledge: the effects of child sexual abuse?

One phrase, two statements

Davidson (2001) provides some methodological guidance here, in his discussion of 'statements' and the 'field of stabilization' in which they occur. A field of stabilization is akin to a 'style of reasoning'; that is, the network of relations and rules that makes it possible to produce statements that reach the threshold of scientificity. It is not that all such statements are true, but

that they are candidates for truth or falsehood (as opposed to nonsensical). A statement is distinct from a phrase or a sentence. "…a series of recurrences of the same phrase can correspond to a multiplicity of different statements. Even the identical sentence need not constitute the same statement" (Davidson, 2001, p. 140). In other words, the same phrase might appear in two texts, but be produced by two distinct styles of reasoning (or 'fields of stabilization'), and thus entail entirely different concepts governed by different rules of usage and production.

This allows a comparison of phrases found in different texts, to try and discern distinct styles of reasoning which produce them. If I remember the principle of diffractive reading at this juncture, I must acknowledge that I have already been engaged in actively producing two styles of reasoning in the act of naming scientific and post-structuralist feminist knowledges about child sexual abuse. I justify this naming of post-structuralist feminism and scientific psychology by referring directly to the texts themselves; that is to say, I am not interpreting these claims about styles of reasoning, simply citing them as they appear in the respective texts (Kendall et al., 1999). In doing so I am reiterating and so actively constructing 'post-structuralist feminism' and 'scientific research'.

Here are two quotes from the two traditions:

> *We do not deny that child sexual abuse is prevalent nor that it can have devastating effects, but we do challenge the too-ready presumption of inevitable harm and the narrowing of concern that this has given rise to (Reavey and Warner, 2003, p. 4)*

The findings of the current review should not be construed to imply that CSA never causes intense harm for men or women—clinical research has well documented that in specific cases it can. What the findings do imply is that the negative potential of CSA for most individuals who have experienced it have been overstated (Rind et al., 1998, p. 42).

I will hope the reader allows that the two quotes above are literally similar enough to be analysed as a recurrence of one phrase, but that they are two different statements in Davidson's sense. My task here is to mark out the respective styles of reasoning that have produced these statements. Why can one quote appear in a self-proclaimed post-structuralist feminist text on child sexual abuse, whilst the other appears in a prestigious mainstream psychology journal by self-identified scientific researchers (and be subsequently maligned for minimising the harm of child sexual abuse)?

The table below (Table 1, on next page) offers a rough summarisation of the two styles of reasoning that have produced the above quotes. The table shows that while the two sets of authors draw on apparently radically different ideas about how knowledge of child sexual abuse is produced, they can also both produce near-identical sentences or phrases. The point is that the analytical tool of 'styles of reasoning' shows that statements are not the same as sentences. The two quotes above cannot be understood to mean the same things.

Table 1

	Rind et al.	Reavey et al.
Causality	Child sexual abuse can cause harmful effects, but not inevitably and not necessarily intense. Expert knowledge can and should identify 'what and how much' harm.	Child sexual abuse can cause harmful effects, but not inevitably and not necessarily intense. Expert knowledge about child sexual abuse constructs (although not exclusively) identities of survivors.
Object-subject relation	Iatrogenic effects- incorrect research can incite individuals to evince symptoms.	'Looping effects'- individual take up identities but also 'push back' against expert knowledge.
Objectivity	Desirable, not necessarily achievable but should be attempted. Child sexual abuse can be studied as a scientific object. Objectivity guards against ideology, morals, etc.	Neither possible nor desirable. Claims to objectivity serve to privilege some knowledge claims over those made by less powerful groups, typically survivors themselves.
Epistemology	Statistical, scientific method	Reflexive, discursive
Norms	Statistically verifiable standard against which the effects of	Disciplinary tool of power-knowledge which effectively casts

	abuse can be measured. Can be useful for identifying helpful therapeutic and policy guidelines to assist survivors of sexual abuse.	sexual abuse survivors as abnormal and therefore deficient, and in need of intervention.
Difference	To be corrected	To be embraced

So, my analytical task for this thesis is to attempt to identify the styles of reasoning that produce the particular sentences, phrases, words, etc. in the focus groups and the research texts.

Discourse?

The question of Foucauldian discourse analysis has implications for the coherency and theoretical rigour for social work research (Garrity, 2010). The question of what is, and what is not (Burman, 1991), discourse, and how it ought to be analysed, has been a controversial question in the methodological literature. This is particularly so in reference to the work of Foucault (Graham, 2005), where the use of the term itself (discourse) can mean something quite different from other texts that discuss 'discourse analysis'. This can lead to some misleading criticisms of 'Foucauldian discourse analysis', especially that a) such work focuses on texts to the exclusion of practices, and b) that such analyses imply an overly deterministic relationship between knowledge, power and subjectivity (Lupton, 1997).

The notion of the statement (see above) is implicated in a notion of discourse that is not immediately apparent. In a

Foucauldian sense, discourse cannot be taken to mean the exchange of words and ideas that it seems to mean in some methodological approaches, including social work research by prominent authors such as Karen Healy and Jan Fook (Garrity, 2010). Rather, this is a discourse that consists of a 'great murmur', an 'anonymous murmur' "in which positions are laid out for possible subjects" (Deleuze, 1988, p. 47). In this conception it is not possible to imagine a subject that *then* produces or uses discourse; rather the subject position from which 'one' speaks is one possible position (not the only possible position) laid out by discourse. Text and speech can be animated or inhabited by discourse (Wooffitt, 2005), but they are neither exhaustive of or synonymous with discourse. To this extent this thesis seeks to locate the discursive formations that 'animate' the texts (the focus group data and the research publications) in regards to the nature of child sexual abuse as a problem for and production of therapeutic treatment and expertise.

Following Hook (2007), drawing on Potter and Wetherell's (1987) work on discourse analysis can put into relief an approach to this research that is more consistent with the general methodological and epistemological framework being developed here. In particular, it is worthwhile noting the distinction between the 'interpretive repertoires' of Potter & Wetherell, and the 'conditions of possibility' associated with Foucault's archaeology. In the first, an active subject draws upon a range of possible ways of understanding a situation, object or event; in the second, it is the *epistemè* that produce the subject.

It is not necessarily the case that the approaches to discourse analysis are incompatible, and applying them in a

complementary fashion may be productive. In particular, a focus on face-to-face and small group interactions (such as the focus groups) can assist with asking how forms of discourse become part of the technology of a discipline or institution, while attention to the historical formation of formative structures and institutions brings attention to their contingency (Hacking, 2004). Nonetheless, care must be taken regarding what the analysis implies or assumes about the *intentionality* of the speaking or writing subject

Intentionality

Earlier I quoted Krueger and Casey (2000) who stated that the "*... focus group… is a way to better understand how people feel or think about an issue, product, or service"* (p.3). By this stage in my thesis, attention is immediately drawn to the epistemological implications of the suggestion that focus group participants could unproblematically express how they "feel or think" about psychological measurement, as if this was some stable opinion that the individual held inside their head, ready to be produced on demand in any given situation. Reifying statements as stable opinions to illustrate a theme of the research can "undervalue and even distort the data produced by discussion groups" (Green et al., 1999, p, 25). A more productive orientation might be to ask what speech acts or statements do or achieve in their context, how successful they are, what "repertoires of knowledge" (ibid. p. 34) do they draw upon, what contesting or concurring statements do they elicit, and so on. The focus groups are not attempts to uncover what counsellors really think about psychological measurement and testing; much less to discover the truth of these practices themselves.

Potter and Wetherell might say that focus groups are a specific context in which speech acts are performed. This context must have a profound influence on what is being achieved or attempted in the speech of participants. The focus groups in this research might be said to be 'naturally occurring'[31], where the issues under discussion are commonly talked about (or evaded) in this timeslot in the organisation's routine. Pre-existing groups may have norms around what can and cannot be said[32]. The aim would be to identify the normative rules of conversation being followed by the speakers, rather than assuming they are expressing underlying/consistent psychological states, desires, preferences etc. The speech that occurs in focus groups can illustrate the 'interpretive repertoire' (Potter et al., 1987) available when counsellors speak about the use of psychological testing in their work, and how these resources are deployed in the specific context. "The interpretive repertoire is basically the lexicon or register of terms and metaphors drawn upon to characterise and evaluate actions and events" (*ibid.* p. 138). In this sense one might refer to a therapeutic interpretive repertoire.

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[31] By 'naturally occurring', I do not mean that the setting does not influence the focus group- this buys into the myth of neutrality. "Researchers should consider, instead, the different messages that are being given to participants when we select different venues" (Barbour et al., 1999, p.11). In this research, the venue and time of the face to face focus group was suggested by the group members in initial conversations as the most convenient and accessible- it is doubtful that they could have participated elsewhere. The groups were held at the agency during 'case-review' time, when such discussions relating to the work of counselling children who have been sexually abused is accepted and part of the weekly routine of the organisation. As Hacking (2004) notes, such setting and interactions help to understand how ways of categorising people, the circulation of knowledge, can occur in institutions.

[32] A hypothetical example might be that one can discuss kicking a soccer ball with a young client as a rapport building activity; to describe the same activity being about improving the child's passing skills would not be an acceptable statement, and even less acceptable would be to frame it as improving the counselor's soccer skills.

Here, I want to outline a methodological guideline for this thesis described in contrast to the above. In distinction to the idea that a speaker can 'use' or even 'draw upon' an interpretive repertoire, the locus of discursive strategy here does not lie with the speaker; the speaker is rather caught up in or constituted by strategy of discourse, although this does not necessarily mean the speaker is a passive participant in the play of discourse (such as Elizabeth in the example given earlier, on page 79).

Judith Butler (unsurprisingly) complicates this practice of 'giving an account of oneself' (Butler, 2005). In *Psychic Life of Power* (1997a), Butler explains Nietzsche's idea that the subject is always grounded in 'bad conscience'. In this version of subject formation, it is only in the installation of a set of social or moral norms that the subject can recognize oneself as a 'self'. Thus the originary scene of subject formation is always already a justification. That is, it is not that a subject *is*, and then is called upon to explain oneself; this is the shortcoming that Butler identifies in Althusser's *interpellation*. (This is the famous scene in which the individual is hailed in the street by a policeman: "Hey, you". At this moment the individual, being recognised by authority, recognises oneself as potentially in breach of a societal or legal norm). It is the call to account for oneself, by some external authority (which may well become internalised but whose origins precede the subject), that initiates the 'turn' which founds the subject itself.

In her later book (2005), Butler revisits and revises this scene of subjectivation; it is not only an experience of a demand for justification that compels one to provide an account of oneself. It is also a desire for recognition; to be recognized and to recognize others. This recognition (of oneself) requires others

to (hopefully) receive and validate it, and this requires a set of norms (at the very least, linguistic or symbolic norms) to provide the material with which such an account can be constructed, offered and received. Thus, the conditions of subjectivation precede the subject (and remain 'opaque' to the subject, so that such accounts are inevitably incomplete), but the desire to be recognized may be an equally compelling catalyst for giving an account of oneself as the justification born of bad conscience or interpellation.

What can be done with these elaborations of the 'accounting subject' in the current thesis? Given Butler's discussion is largely an abstract discussion of moral philosophy and psychoanalytic theory, how might it be put to work in the very specific and local context of the focus group discussions? At the risk of oversimplifying, the focus groups are situations in which the participants are recognised as particular kinds of subjects; as counsellors, therapists, social workers, art therapists, psychologists, and so on. To what extent might this notion of recognition frame the conditions in which each participant 'gives an account of themself'? In writing and in speaking of psychological subjects, professionals construct themselves through language and conceptual tools of their discipline, or else risk losing credibility or, in the extreme case, becoming unintelligible to their colleagues (Gergen, 1998). If this is the case, analyses of the focus groups links up with the genealogical principles described earlier. What is said in the focus groups can be understood in reference to the 'sayable' as a counsellor. There are a finite (although potentially large) number of utterances one may make 'as a counsellor'.

Summary of Chapter 3

In these two chapters I have attempted to describe the epistemological tools I will use in the thesis, and the specific forms of information I will be using. I have provided an account of a movement in my use of the focus groups over the life of the thesis. The position I have described here is best conceptualised as a pragmatic one, in the sense that I do not claim I have come to the best or ultimate position, just one that I can use for the present text. The initial conception of focus groups as relatively unproblematic forums in which participants could express opinions and views on psychological measurement in their counselling work in the field of child sexual abuse, has been modified in response to the demands of the questions around discourse and subjectivity. I have also outlined two influential traditions of knowledge in the field of child sexual abuse, and attended to their differences as well as their shared concerns. In line with the practice of diffractive reading, I have tried to suggest that these two traditions are both productive of ways of knowing about and responding to the effects of child sexual abuse.

Chapter 4- Measurement and science

In this chapter I will discuss scientific approaches to analysing psychological measurement as a separate analytical approach to the administrative or disciplinary analysis outlined in Chapter 2. However, they are more entangled than distinct. In order to produce an understanding of how psychological measurement and statistics discipline the problem of child sexual abuse, throughout this thesis I will be analysing measurement as both a scientific technique and an administrative technology. In the rest of this chapter I will discuss scientific measurement in more detail. I will argue measurement in science is a historically contingent practice, and refer to some of the feminist critiques of these practices. I will describe three general models of measurement: representational, interactionist and performative. The first two are embedded in the epistemological/ontological framework associated with many classical ideas of science, while a performative model draws on ideas and practices associated with quantum physics. I will make some connections between a performative model measurement and a disciplinary analysis of power, and start to apply these ideas to understanding the function of psychological measurement. I finish the chapter by considering the contemporary intra-action of scientific measurement and psychological measurement in brain imaging technology.

The popularity and power of numbers in psychology, and the social sciences more generally, is often thought to derive from the association of quantification with scientific method. "…the recourse to mathematics, in one form or another, has always been the simplest way of providing positive knowledge about man with a scientific style, form and justification" (Foucault, 1973, p. 351 , cited in Reekie, 1998, p. 45). The importance of

measurement in psychology is due in large part to the emphasis placed on psychology being regarded as scientific, both by its own practitioners and the broader community (Porter, 1995; Michell, 1999; Porter, 2003).

As well as this rhetorical importance, there is also a strong historical connection between physics and psychology and the use of statistics and measurement. Gustav Fechner, often cited as the founder of quantitative psychology with the publication of his book *Elemente der Psychophysik* in 1860 (Boring, 1961; Hacking, 1991; Stigler, 1999), was himself a physicist interested in how he could apply the statistics he had learned from physics to the study of human psychology. This is more than an interesting anecdote, and Michell (1999) shows how profoundly this connection has shaped psychology (c/f Green, 2003). I have been utilising the connections between physics and psychological measurement throughout this thesis.

Some important statistical ideas in psychology derive from calculations initially made in other fields of science. This is called 'scientism'- "…the view that methods successful in certain 'paradigmatic' sciences must also apply to others" (Michell, 1999, p. xii). There is perhaps no more powerful example of scientism than the Gaussian distribution of errors from the field of astronomy, and renamed by Pearson in the 1890's as the *normal* curve (Hacking, 1991). This curve (also called the Bell curve due to its shape when represented visually in a graph) is now deployed in a myriad of contexts in the government of lives through psychological assessments and interventions[33]. The Bell curve is not a product of the discipline

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[33] The Bell curve is a statistical device for understanding the distribution of random errors, and not of human traits at all (Sartori, 2006) Nonetheless, the bell curve continues to be utilized as a scientific tool in powerful (if

of psychology, but of another field of science, so it can be transported across various *scientific* disciplines. This curve was present in my focus group discussions:

> *CB And…um… I guess…'cos the actual tests if you look at graphs and stuff say well this is normal range*
>
> *ELIZABETH Graffs [pronounced differently, all laugh]*
>
> *CB So, what's you're understanding of, in terms of the test, they talk about norm groups, they say 'well this is how we define what normal is', so what's your understanding of what normal means in the context of a graph?*
>
> *KATE The bell shaped curve, how they fit in, that's how they work it out, isn't it? That's what they say, if it fits within that range*

Despite the importance of psychology's claims to scientificity, not all practices of quantification derive from the natural sciences. There is also a tradition of measuring and collecting statistics that is more properly thought of as administrative, deployed not for the purpose of discovering truth but explicitly for classification, planning, and regulation (Porter, 2003).[34] It is notable that Alfred Binet, the late nineteenth-century pioneer of

---

sometimes controversial) ways, perhaps most infamously in the case of race and intelligence testing in (Herrnstein et al., 1994)

[34] I might make an analogy to Hacking's distinction between 'representing' and 'intervening' in relation to the functions of these two styles. (Hacking, 1983) Rose suggests two histories of numbers: "a benign American history and a less optimistic European history" (Rose, 1999, p.203), roughly equating to numbers as truth vs numbers as political instrument.

intelligence testing (arguably the most measured of all psychological constructs), was quite explicit that his tests did not measure, but classified (Zenderland, 1998, p. 96). Statistics of this kind are closely implicated in what has been called the "science of the state" (Foucault, 2007, p. 101)—the language of 'governmentality' refers to the use of statistics in this context (as discussed in Chapter 2). The relevance of this tradition for analysing psychological measurement is evident especially in relation to the establishment of population norms. Whilst psychological measurement is a scientific practice, it relies upon statistical ideas about population to be effective.

Scientific measurement

It is difficult for us to imagine a practice of science that does not involve measuring things. However, measurement was not always readily accepted as suitable for understanding Nature, the historical object of science or 'natural philosophy' (Oakley, 2000). Prior to the influence of what is now called the Enlightenment during the 18th century, although systematic enquiry into Nature was undertaken, understanding how the world worked was primarily done through appeal to authorities of a religious kind. Scientific explanations often appealed to God as an irreducible cause or creator, or to universal laws of mathematics accessible by logic and reason. Mere human observation and experience was not considered adequate to the task of such knowledge. It was not until "…the nineteenth century…[that] it had finally become a task of the natural scientist to measure" (Hacking, 1991, p. 186).

The classification of plants and flowers during the 17th and 18th centuries was a significant context for the emergence of scientific measurement, which related to debates about holism

and atomism as ontological principles. For example, during the mid-to-late 1600s and into the 1700s, the Paris Académie Royale des Sciences worked to catalogue plant species, with great care and attention devoted to differentiating varieties based on colour, drawing on the language of painters, dyers and weavers for their descriptive tools. Later, in 1737, Linnaeus complained that such verbosity was not only unnecessary but wasteful, and that only "Number, Shape, Position and Proportion" provided scientific description of plants (Daston, 2005). At the centre of this shift was the way in which a scientific fact ought to be constituted, with a movement away from detailed particularism to repeatable generalisation.

Discussing the early scientific measurements of natural phenomenon such as temperature and electricity, Porter notes the dissatisfaction that was expressed by some philosophers of the time about the infatuation with numbers and measuring.

> *Diderot, in his more romantic moods, complained of the alienation from nature implied by mathematics. In the 1830s, the Hegelian natural philosopher Georg Friedrich Pohl compared Georg Simon Ohm's mathematical treatment of the electrical circuit to a travel book that ignored a charming landscape and its inhabitants in favour of recording precisely the times of arrival and departure of trains (Porter, 1995, p. 18)*

Contrast the view that mathematics represents an 'alienation' from nature, with the oft-cited Galilean claim that mathematics is 'the language of nature' (Hacking, 1991). Measurement, let alone broader methods of observation and collection of evidence, has not always been uniformly accepted as the ideal

method of understanding things. Nor is the meaning of measurement itself uniformly conceptualised. Nonetheless, measurement is a central feature of contemporary scientific practice. It is generally agreed upon that by 1860, with the publication of Fechner's book mentioned earlier, that measurement was a condition of a discipline being regarded as a science (Boring, 1961).

What is scientific measurement?

I will outline three ways to understand what measurement is, or what it does, in science. The first is a representationalist view of measurement; there are objects with properties, and measuring these does not dramatically change, alter, or produce the objects being measured. Second is an inter-actionist model, which holds that although objects do ostensibly pre-exist the act of being measured, there is ultimately no way of separating what is known from the practice of knowing: measuring something changes it in ways that are not entirely able to be accounted for, so 'pure' knowledge of objects is impossible. Third, I will discuss a performative or intra-actionist model of measurement, which essentially argues that the material arrangements by which we measure things do not simply alter or distort them, but effectively produces them. Each of these models has broad implications which apply to research and therapeutic practices with the problem of child sexual abuse.

1- Representationalism, classical science and measurement

In classical science, 'measurement' has a precise meaning. A practice does not qualify as measurement simply by the use of numbers. Michell provides an outline of the basic features of

the classical concept of measurement in the natural sciences (1999, pp. 1-23). The first important issue at stake is that of *commensurability*. An attribute only has a *quantitative structure* if it is qualitatively the same regardless of where it occurs. Thus, *length* is the same attribute (or 'property') whether it is the length of a plank of wood, the circumference of the Earth, or a distance that a particle travels in a nanosecond in a physics laboratory. The units used to express this distance may differ by orders of magnitude, but length itself is the same thing in all these instances. In this model of measurement, the attribute 'length' found in the classical physics lab is the same attribute 'length' measured by a carpenter; length is commensurable in the two instances.

Suppose for a moment that anxiety is measurable (I am choosing anxiety as it commonly appears in instruments of psychological measurement, including the BASC—Behavioral Assessment System for Children—the main assessment instrument used by the counsellors in my focus groups). Commensurability would mean that person A's anxiety is qualitatively the same as person B's. It would also mean that person A's anxiety is always qualitatively identical to itself, differing only in amounts across time and space. (If it were not the same in each situation, we would need to use different names for each instance). Under this model, it is possible, given the right instruments, to work out how much anxiety any individual person has, both in absolute terms (this is called a ratio or cardinal scale) and relative to the rest of the population (an ordinal or interval scale). If something is measured using the ratio or cardinal scale, it refers to an actual amount or quantity of anxiety; anxiety is considered to be a quantitative attribute, and the measurement is meaningful in and of itself. With an ordinal or interval scale, the anxiety itself may not be

exactly a quantitative attribute because it is only rating person A's anxiety relative to person B's; A may have more or less than B, but this does not have to mean we know anything about the absolute amount of anxiety. Ordinal and interval scales are ordering devices, but do not entail measurement in the strict scientific sense defined by Michell (1999).

Some further formal features are required for an attribute to be quantitative and therefore measurable within the representationalist model.

1: Measurement requires that the attribute in question contains a structure which supports consistency of ratios; when some thing '*a*' is measured as being 2x, it bears a precise and meaningful relationship to x (i.e. it is meaningful to say that *a* is equal to twice the value of x). The ratio remains consistent regardless of the quantity represented by x (e.g. be it centimetres, furlongs or miles). x here is a unit of measurement, and all quantities of that type (e.g. length) can be expressed as a magnitude of x. The ratio to x of any two given lengths will remain consistent, regardless of the actual unit in which x is expressed (e.g. all lengths can be expressed as a ratio relative to a metre, although it is often impractical). Similarly, we can conceive of the length 'zero' even if we could not actually produce or point to such a thing. With our example of Anxiety, it would mean that a measurement called *2x* (where *x* is a unit of anxiety, analogous to a metre or centimetre of length[35]) is twice as much anxiety as *x.* It was this lack of an apparent standard reference unit for sensations that led some of Wundt's and Fechner's contemporaries to argue that 'mental processes'

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[35] To my knowledge, there is no standardised unit of anxiety proposed in the literature.

could not be measured in the same sense as physical properties could be (Darrigol, 2003).

2: A further question as to whether or not an attribute is measurable is- how can we tell if the attribute possesses an additive structure? The possession of an additive structure is why ratio is so important; with our symbols above, an attribute x with an additive structure means that 2x is precisely twice as much as x. With the attributes of 'length' we can say that 2 metres is twice as much length as one metre, by the practice of concatenation (adding two like quantities together). If the attribute does not have an additive structure, it is nonsensical to add them together.

This question can be further elaborated by drawing a distinction between 'intensive' and 'extensive' properties (Comrey, 1968; Delanda, 2002; Michell, 2006)[36]. Although this is a fairly complex matter, there are a few points that are relevant for my purposes here. First, while extensive properties (such as length or volume) are directly empirically measurable, intensive properties (e.g. temperature, density) are generally measurable only through the way they affect things. So while the volume of a glass of water is directly measurable, the temperature can be measured only be the affect it has on the water (or the mercury in a thermometer). It is possible that intensive properties are measurable through the effects they have on extensive ones. If this is the case, then the differences between intensive and extensive properties lie only in our capacity to perceive them, and are not inherent in the properties themselves.

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[36] Michell (2006) actually claims that the distinction is now only of historical interest in philosophy of science, whilst Delanda (2002) constructs an entire Deluezean ontological framework based on the distinction. In any case, I have found it a useful way to conceptualize aspects of the scientific problem of psychological measurement.

The second distinguishing feature between extensive and intensive properties has to do with their continuity. While ever larger *volumes* of water will always remain water, changing the *temperature* will actually change the water into a qualitatively different kind of thing (ice or steam). Intensive properties can produce qualitative state-changes, whilst extensive properties cannot. Intensive properties can be thought of as states rather than amounts (Essex et al., 1999).

The third difference is whether the property can sustain division or addition (concatenation). If you pour half the glass of water into a smaller glass, the volume of water in each glass is halved. However, in each glass the temperature remains the same as it was originally.

It is possible for both intensive and extensive properties to be *quantitative* in structure (or 'fundamentally measurable' as one mid-20th century writer put it (Comrey, 1968)). Although an intensive property (anxiety, for example[37]) can be experienced as more, less than or equal to, it does not necessarily mean that these differences are measurable in the strict scientific sense (Michell, 2006). Michell argues that, while psychological attributes might be quantitative, this in fact has not been proven; it is simply assumed. Here is the outline of the scientific problem of measuring psychological attributes. Considering the properties that an attribute must possess to be quantitative in structure, and therefore scientifically measurable, it is far from certain that psychological attributes possess such a structure. Michell's argument is that psychological attributes have not been shown to possess this structure, and that this has not been acknowledged within the

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[37] I am jumping ahead slightly here; for now I will assume that anxiety may be thought of as an intensive property.

discipline (1999). His argument makes an implicit assertion that either the classical-representationalist model or the interactionist models (see next section) of measurement are the only correct models for scientific measurement[38].

In this model of measurement, an external observer (e.g. the scientist) can 'stand apart' from the objects being measured and does not influence the object or its attributes through the actual measurement act. The instruments used may not be entirely accurate, but they do not fundamentally alter the object or its attributes. This is the notion of 'separatism', meaning that there are definable points of connection between ostensibly discrete systems; the measured object and the observing agent. Measuring instruments act as a mediator between the two systems, but the instruments themselves are (ideally) transparent and neutral. In this separatist model, psychological measurement consists of a (more or less) neutral instrument (e.g. a standardised questionnaire, or an experimental laboratory situation) that can measure the traits it sets out to measure, the 'cut' between the object and the researcher is complete, and the process of measurement fundamentally alters neither participant. It is the will and action of the observer that structures and drives this process, with the object of investigation being more or less passive (Barad, 2007).

2- Interactionist model of measurement.

The representationalist model is an ideal notion of measurement that provides an aspirational reference point in classical physics but is generally recognised as unachievable.

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[38] There is also another possibility mentioned by Michell (2006, see p.422), that psychological measurement does not require attributes themselves to be quantitative, but that the difference between two instances of the attribute are. He suggests that this was the dominant view of psychophysics at the start of the 20th century.

There is general recognition, even in the 'hard' science of classical physics but more so in the human sciences, that measurement constitutes an intervention, however much one wishes to avoid disruption of the observed object. The implication of this recognition is to try and account for and minimise the disruption, in order to more accurately detect the 'true' nature of the object under study.

The distinction between the representationalist model and interactionist model of measurement has methodological and epistemological implications, but there is no great ontological issue at stake. Both models hold that there is an independently existing reality, that objects possess attributes that are part of their essence or nature. In one sense, the interactionist model simply acknowledges that the pre-existing reality may not be directly accessible to human observation or measurement. The methodological imperative is to devise means of measurement that produce the least disturbance or distortion possible; or less ideally, to be able to fully account for whatever disturbance is involved so that the pre-measurement state can be deduced.

3- Performative model of measurement

In this model, neither the object, subject, nor instruments of measurement can be understood apart from their engagement in the 'phenomena' of measuring (Barad, 2007). A key point here is that there is no independently existing Cartesian 'cut' between the object of knowledge and the agent of knowledge. The cut is in fact a "constructed, agentially enacted, materially conditioned and embodied, contingent Bohrian cut between an object and the agencies of observation" (Barad, 2007, p. 115). The issue is an ontological one, rather than an epistemological one.

Barad stresses the importance of this distinction: as she points out, classical physics (and because of this to a large extent, common sense) understands that measuring something makes it behave differently. Her (and Bohr's) claim is a stronger one of indeterminacy, not uncertainty. "The issue is not one of unknowability per se; rather, it is a question of what can be said to simultaneously exist". (p. 118). That is to say, depending on the apparatus and conditions of measurement, only momentum *or* position (of a particle in a physics experiment) can be said to materially exist at the same time, but not both: "…the values of complementary variables (such as position and momentum) are not simultaneously determinate". In a performative model, "…measurements do not represent measurement-independent states of being" (Barad, 2007, p.138). In a performative model, scientific methods, instruments and techniques are constitutive of the theories they are used to elaborate. "Contemporary scientific reality- and this goes for a science like psychology as much as any other- is the inescapable outcome of the categories we use to think it, the techniques and procedures we use to evidence it, the statistical tools and modes of proof we use to justify it" (Rose, 1998, p. 52).

This needs to be distinguished from the recognition (the kind familiar to social workers) that "other observers may determine a different reality and, similarly, the observational text is likely to hold a multiplicity of meanings for the reader" (Le Riche, 1998, p. 31). A performative model places much more emphasis on material arrangements of measurement and observation, and de-emphasizes the intentions or will of the human observer. It is a 'post-humanist' model. The contribution of the human observer is partially (not completely) determined by the epistemic arrangements of the situation, in contrast to the classical scientific model where the entire

arrangement of observation is understood as a manifestation of the will of the human observer (Crary, 1990; Barad, 2007).

The majority of debates and critique about psychological measurement revolves around the first two models of measurement from the natural sciences. Key questions in this literature are whether psychological attributes are quantitative in structure, and more so, what kinds of instruments might be used to measure such attributes. There are significant questions about the applicability of methods from the natural sciences for the study of people. There is the inescapable fact that measurement of psychological attributes involves an interaction between two or more people, both of whom possess a degree of agency thus introducing tricky questions of subjectivity and the shaping of interaction by social, political and cultural forces.

In this thesis, I am interested in how a performative model of measurement can be usefully deployed to understand psychological measurement. As Barad (2007) makes explicit, there are important implications for our understanding of the power/knowledge nexus in this model. Importantly, the performative model shifts attention away from the results of measurement, to a genealogy of the measurement situation itself. The connections with a Foucauldian formulation of power provide a potentially rich source of material for this thesis. In the next section I explore some of these implications more fully, before reviewing some of the major critiques of the dominant models of measurement.

The importance of practices

Although I will reference the arguments over the disputed scientific status of psychological measurement (i.e. is psychological measurement an authentically scientific practice?), I want to state that it is not my aim to come to any conclusions on that question. In a sense, that question will be sidestepped by taking seriously the claim that psychology is a science, and treating psychological measurement as a material practice of enquiry and knowledge production. The authors I draw upon in following this tradition tend to adopt an approach that shares many of the epistemological and ontological facets of Foucauldian analysis I outlined in Chapter 2. This approach follows from recent work in science studies that seeks to centre questions of how knowledge, the objects of knowledge, and the knower are produced in scientific work (e.g. Barad, 2007; Daston et al., 2007). Central to this work is the interrogation of representationalism; the idea that there is a reality that exists independently of our (human) knowledge of it, and that our descriptions accord with that reality to a greater or lesser degree. There are a number of possible takes on this idea. Indeed, classical scientific method itself rests on the assumption that researchers do not have immediate access to this reality through the senses, and that only by carefully identifying and eliminating and/or minimizing subjective distortions can scientific work arrive at better and closer representations. Other more critical positions include relativism (all efforts to obtain knowledge of the world are limited by the local and specific position of the enquirer) and constructivism (our efforts to produce knowledge of objects actually produce those very objects).

Performative approaches to these questions focus on how not only representations but scientific objects (or more correctly, phenomenon: Barad, 2007) themselves are produced through scientific practices, and the conditions under which these achievements are possible. "A performative understanding of scientific practices…takes account of the fact that knowing does not come from standing at a distance and representing but rather from a direct material engagement with the world" (Barad, 2007, p. 49). Observation of things is an engagement with them; knowledge and objects ('words and things') are entangled. In analysing scientific practices of knowledge production, including practices of psychological measurement, one also has to account for the potential that these practices alter the researcher.

This understanding informs my investigation of the quantitative research texts on child sexual abuse and the practices of knowledge production they entail. However, as Barad (2007) is at pains to remind her readers, practices and theories derived from experimental physics do not translate smoothly into discourses about the human sciences, as productive as these insights might be. At the same time, as I have already mentioned, physics and psychology do not have entirely discontinuous histories. It is a matter of taking some care not to argue by metaphor or analogy, but to apply insights from one discipline in an empirical and thorough manner (parallel to scientific measurement itself). Further, if child sexual abuse research presents itself as scientific, then an analytical approach drawn from the field of science studies is appropriate.

One particular difference that I am aware of in the use of some of these ideas from science studies for this thesis, is that the practice of psychological measurement with sexually abused

children or adults is not an experimental situation of the kind that Barad takes as her preferred 'phenomenon'. In a strictly experimental research situation (usually in a physics lab, for example), researchers control and adjust the independent variable(s) in order to investigate how they affect the object under investigation (the dependant variable).

Experimental research of this kind does have a solid tradition within psychology, in research on stimulus, response and perception (Danziger, 1990, 1997), with the language of dependent and independent variables emerging around the 1930's (Winston, 2005). This is clearly not the model for research on child sexual abuse. Researchers obviously do not take their measurements before sexual abuse occurs and then subject the research participant to sexual abuse and take a second set of measurements. However, more broadly, research on the physiology of trauma (a central construct for child sexual abuse research and therapy) has subjected animals to traumatic events in laboratory and field research (McNally, 2003).

Sexual abuse researchers try to discover differences based on the variable of interest (child sexual abuse) through other means, including a raft of statistical procedures. One means of attempting this is through the production of the normal range of a particular trait, in which a population of non-sexually abused research subjects are compared on a set of measures to sexually abused research subjects. There are difficulties to be overcome with this approach. Even in the case of prospective studies (where non-sexually abused subjects would be followed for a period of time such that some percentage of them would be expected to have been abused in the interim), any changes in measurements could not be attributed to sexual abuse alone as

there would be so many other possible uncontrolled variables. There are also issues with the construction of normality, which is often naturalised in the research.

In terms of the epistemological issues I am concerned with in this chapter, I am keenly aware that the situation of psychological measurement and measurement in experimental physics are not identical. However, my contention will be that there are important ethical, epistemological and perhaps ontological insights from a performative model of scientific measurement that will be useful for developing an understanding of psychological measurement in research and therapeutic contexts with child sexual abuse.

Critiques of quantification

Measurement is an empirical practice, compared to the logical methods of mathematics and statistics. However, measurement is often 'joined up' with these other styles of number-producing ways of knowing, and these are all sometimes congealed together as scientific methods. As in the natural sciences (perhaps more so), the idea that numbers and statistics are a suitable means for understanding people is contentious. This has manifested itself in methodological debates in various disciplines of study concerning people, as well as popular debates. In his 1854 novel *Hard Times,* Charles Dickens satirised the idea that humans could be known through measurement and quantification (Oakley, 2000).

> *Inhumanity, math, and social measurement are*
> *combined in the character Thomas Gradgrind: A*
> *man of realities. A man of facts and calculations. A*
> *man who proceeds on the principle that two and*

> *two are four, and nothing over. . . With a rule and a*
> *pair of scales, and the multiplication table always*
> *in his pocket, sir, ready to weigh and measure any*
> *parcel of human nature, and tell you exactly what it*
> *comes to (cited in Peters, 2001, p. 437)*

This passage recalls the famous dictum of Lord Kelvin,

> *When you can measure what you are speaking*
> *about and express it in numbers you know*
> *something about it; but when you cannot measure it*
> *in numbers, your knowledge is of a meagre and*
> *unsatisfactory kind (cited in Porter, 1995, p. 72)*

Dickens' attack on such ways of knowing was likely motivated by notions of elitism. HG Wells was another who argued against scientific methods of understanding humans, as these were 'deceitful' because they disregarded the "'objective truth' of individual experience" (cited in Oakley, 2000, p. 132). Note Wells' use of the term 'objective' to describe individual experience, precisely what present day psychological researchers tend to call 'subjective'.

It is interesting to note here that these objections are also constitutive of other ways of knowing, which produce different normative understanding of humans. That is to say, criticisms of measurement specifically, or scientific method in general, are themselves based on a set of assumptions about what kind of things humans are and the best way to know about them. Already evident in the Wells' quote above, for example, is the humanist notion that each individual person is a unique being whose essential self comes from within them; a standardized method of enquiry is incapable of portraying this uniqueness of

being. This would potentially give rise to more phenomenological ways of knowing about people. This theme – that criticisms of any truth claim necessarily include their own competing truth claims—which runs through much of the Foucauldian literature, revolves around these questions; what is the basis of resistance to any regime of knowing, and what normative assumptions and values do these resistances in turn produce?

In other words, why critique measurement, and what happens when we do? I mention this point here to remind myself and the reader that my intention is not to expose psychological measurement as a false and inaccurate method of knowing and that there is another truer way of knowing, free from power and other corrupting conditions. Rather, criticisms of 'quantification' are themselves produced by networks of knowledge, power and claims to truth, just as much as quantification itself.

Quantitative is to qualitative, as patriarchal is to feminist?

Well after Dickens' time, a robust feminist critique of quantification in the social sciences has been developed. Various feminist critiques of science and objectivity, and quantification in particular, have been developed over the last 25-30 years (Keller, 1985; Harding, 1986; Haraway, 1991; Keller, 1992; Duran, 1998; see Oakley, 2000 for a critical discussion)[39]. The common argument of these criticisms is that

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[39] I have not delineated the respective contributions of these individual authors. I acknowledge that there are divergences and specificities amongst their work. My purpose here is primarily to indicate the existence of a well-established critique of scientific quantification developed from feminist frameworks. My later discussions of the practices of psychological

scientific method (of which measurement is a crucial component) represents a masculinist or male-centred world view, which is presented as objective, ahistorical and universal. This is conceptually linked to the Cartesian break between reason and matter, and the whole familiar list of gendered dualisms associated with each (nature/culture, body/soul, emotional/rational, and so on). Experiences and accounts of the world which are not presented within the rules or discourse of the scientific method are marginalised, along with the individuals and groups which produce such non-scientific accounts (including women and children, as well as Indigenous peoples (Connell, 2007)); Foucault's 'subjugated knowledges'. While this remains a popular critique of science, there are also feminist 'defences' of quantitative approaches to knowledge.

Experiments in knowing, Ann Oakley's (2000) examination of the history of research methodologies in the social sciences (and how the term 'science' came to be applied to research of the social world), takes as its starting point the long running debates between quantitative and qualitative approaches to social research, and the ideological alignments that have become associated with each camp. In particular, she is interested in the equation of qualitative research with feminism and so-called 'female' ways of knowing, and the associations between masculinism and quantitative approaches to research (Oakley, 2000)[40]. I showed evidence of this in the field of child sexual abuse research in my account of the Levett and Russel debate in Chapter 1.

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measurement highlight the implications of this body of work in greater detail.

[40] It is worth noting that a reverse sentiment exists on this division: "There is more than a germ of truth in the suggestion that, in a society where statisticians thrive, liberty and individuality are likely to be emasculated" (Moroney, cited as the opening quote in (Hopkins, 1973)

This polarisation between proponents of quantitative and qualitative approaches to knowledge has generated much heated debate. On the one hand are the opponents of quantification, who argue that such ways of knowing are destructive and overly rational, thereby implicitly aligned with 'masculine' ways of knowing[41]. Oakley cites Dorothy Ross as an example of this critical position. In this argument, scientism has political implications precisely due to the claim that scientific methods are objective. This has resulted in the use of such knowledge…

> *…to manipulate such things as the money supply, consumer choices, votes, and remedial social therapies…Blind to what cannot be measured, they are often blind to the human and social consequences of their use. The manipulators of social scientific technique, intent on instrumental rationality, cannot notice the qualitative human world their techniques are constructing and destroying* (Ross, 1991, cited in Oakley, 2000, p. 197).

It could be said that in some ways, Ross's statement is not really even a criticism, to the extent that it simply repeats the claims of advocates of scientific measurement. That is, it is precisely the capacity of measurement to enable control that advocates take as a virtue, not a problem. For example:

> *Control of nature has been particularly facilitated by the invention of instruments of measurement. Similarly, the control of human conduct and*

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[41] This tendency, in critical literature on gender, to equate such destructive terms with masculinity in a kind of circular ritual of definition of masculinity is discussed by Frey (2004).

> *education depends on the development of more*
> *exact methods of describing human conduct. The*
> *exact description of human conduct can be*
> *rendered most efficient when it is reduced to a form*
> *of measurement, for then small differences are most*
> *accurately portrayed and small differences most*
> *accurately noted (P.M. Symonds, 1931:v, cited in*
> *Danziger, 1997, p. 147)*

While declaring some sympathy for views such as Ross's, Oakley presents a different story about the development of quantitative scientific methods. She traces the origins of science and scientific approaches to knowledge as being born of democratic impulses, with the vision of enabling any person to have access to knowledge provided they can apply the correct methods[42]. This was a liberatory advancement upon theological ways of knowing which held that knowledge and truth were the domain of divine providence, and the nature of reality was mysterious and inaccessible to 'man'. The intent of the pioneers of scientific method was not to exclude a class of people from knowledge, but quite the reverse. Any person in possession of the correct method, equipment and enquiring mind could access the truth of things. Consistent with this democratic impulse, Oakley argues that quantitative approaches to knowledge are not necessarily 'masculinist'. Indeed, some feminist researchers have dedicated their efforts to developing scales to measure feminist attitudes themselves (e.g. Henley et al., 1998). More pertinently, I argue in this thesis that

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[42] The same point can be made more generally about numerous Enlightenment concepts. Autonomy, for example, has been subject to much feminist criticism as venerating a detached masculine individualism, yet this concept can also mean the capacity to become aware of the external limits and influences which inform one's desires and interests. This capacity allows one to 'refuse to submit' to such operations of power which may otherwise appear natural. As McNay (1992) argues, this aspect of autonomy ought to retain some usefulness for emancipatory feminist projects.

measurement of the effects of child sexual abuse has been a crucial component in feminist efforts to have the harm of sexual abuse socially recognised (Russell, 1991).

As science and scientific methods came to be applied to the study of society and individuals, so it was necessary for the behaviours and characteristics of individuals to be rendered calculable, in order for knowledge about them to have the status of science (Porter, 1986). As the quantification of natural phenomenon became synonymous with what was considered legitimate science, Condercet's 'social mathematics' was the apt naming of the application of mathematical methods to the study of society (Oakley, 2000, p. 103). The appeal of numbers was that they were considered to be objective, and not susceptible to the vagaries of human subjectivity. Again, this was seen as an advance towards democratic forms of knowing, free from personal prejudice and political despotism (Rose, 1999).

The field of child sexual abuse is a highly contested site of knowledge claims (being constituted by those same claims). The involvement of feminist activists, and the subsequent theoretical and political resources on which they draw, often sit uneasily alongside those who regard child sexual abuse research as an area of objective scientific (psychological) study.

This thesis will argue that it is not necessarily productive to categorically dismiss measurement and quantification as an oppressive, dehumanising and masculinist practice. Neither is it helpful or accurate to dismiss as emotional and subjective those knowledge claims that do not conform to scientific methods. Important social justice aims have been served through the feminist production and deployment of statistics, including,

importantly, the problem of child sexual abuse. An obvious and fundamental example to point out here is the deployment of statistics regarding the gender of the majority of perpetrators. The statistic that the vast majority of sexual offenders are men is a crucial support for the argument that sexual abuse is a gendered crime. It is difficult to reconcile the proliferation of this statistic with the argument that quantification *per se* is necessarily a way of knowing that serves patriarchal interests. Even feminist post-structuralists who contest the way that statistical norms and psychological measurements can be used to pathologise women survivors of child sexual abuse, nonetheless recognise the political and strategic use of citing statistically significant trends such as "men make up about 95% of sexual offenders" (Warner, 2009, p.15. Consistent with her framework, Warner is explicit about the unstable nature and strategic deployment of statistics). Abandoning measurement and statistics altogether would seem neither possible nor, I will argue, desirable in relation to the problem of child sexual abuse. Following Gail Reekie's claim cited at the beginning of this thesis, we need statistics in order for child sexual abuse to be a discussable social problem. However, I argue it is valuable to analyse how measurement and statistics *discipline* the problem of child sexual abuse. The performative model of measurement outlined above provides some valuable tools for undertaking this disciplinary analysis.

In the next section, I will narrow down from science, social science and measurement to focus on the practice of psychological measurement in more detail.

A brief historical sketch of psychological measurement

Despite the ongoing debates related to measurement and statistics in the social sciences generally, I would argue that quantification *has* come to be synonymous with scientific method in the psy-disciplines. This is made explicit in the field of psychiatry in the title of the key diagnostic reference text, the *Diagnostic and Statistical Manual.* Psychological measurement plays a central part in the claim that psychology is a *science*. In this section I will present a brief history of psychological measurement, largely based on Joel Michell's work on this topic.

Michell (1999) investigates how measurement as a practice came to be a central tool of *psychology as a science*. Michell suggests that the impetus for psychology's historical concern with measurement was to present itself as both 'scientific' and as an 'applied science' (1999, p. xiii). That is to say, psychology as a discipline needed to establish both intellectual credibility and practical purpose. Descartes' radical separation of material and non-material properties set the ground work for the early separation of psychology from the natural sciences (Michell, 1999: 40-42). The separation of physical and mental phenomenon leaves no possibility for mental attributes (i.e. those functions associated with 'the mind') to be the proper subject of measurement, which is exclusively concerned with the material world. Non-material objects are not considered measurable under this division (Boring, 1961; Darrigol, 2003). This is one of the bases of the 'quantity objection' to measuring psychological attributes.

The quantity objection has two sides. The first is conceptual: mental phenomena cannot meaningfully be said to be

quantitative. The other is more empirical: the hypothesis that mental phenomena are quantitative is meaningful, is just that, as a matter of fact, mental phenomena do not possess quantitative structure (Michell, 1999, p. 44)

Michell (1999, pp. 24-25) identifies three intellectual trends as the context in which quantitative psychology had to establish itself:

1- The Classical concept of measurement- all measurable attributes must be quantitative
2- The Measurability Thesis- Some psychological attributes are measurable
3- The Quantity Objection- No psychological attributes are measurable

In Michell's account, the idea of applying measurement and calculation to psychology was directly modelled on and inspired by the natural sciences (p. 34). In the natural sciences, there is an association of measurement with exactness. However, measurement is not meaningful, let alone exact, if the thing it purports to measure is not quantitative (see earlier discussion); Michell argues that it is has not been established that psychological attributes are quantitative. Despite this scientific lack, psychologists nonetheless accepted and promoted the idea that psychological attributes are measurable for at least five key reasons:

i   the tendency to model psychology upon quantitative natural science;

ii   the belief that pursuit of the goals of precision and exactness required measurement;

iii   Pythagoreanism (the idea that all entities are quantitative- as opposed to Aristotle's metaphysics which held that some attributes are qualitative);

iv   the 'quantitative imperative'; and

v   the perceived need to 'sell' psychology as quantitative (p. 39)

Rather than take the more familiar (though perhaps more self-consciously radical) humanist objection that understandings of human psychology derived from scientific methods *per se* are flawed, Michell argues that it is the *unquestioned* suitability of psychological attributes for measurement which is unscientific. His project is to resuscitate what he sees as a critical scientific failing on the part of quantitative psychology, a two–level 'pathology of science' (Michell, 2000): 1- the hypothesis that psychological attributes are quantifiable has not been adequately tested; and 2- this failure has not been acknowledged. There is no problem with the notion of psychological measurement; it is just that it has not yet been proven to be scientifically justified. Michell lays the blame for this situation largely at the feet of one S.S. Stevens.

Stevens' definition of measurement: Assigning numbers to objects

Michell contrasts the focus on ratios in the classical definition of measurement, with Steven's (1948) definition of measurement that has become the accepted definition in psychology[43]. Stevens' definition was simply that measurement is the assigning of numerals to objects according to a rule. The

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[43] Statistical methods (probability based modelling and inference), as opposed to measurement of psychological characteristics *per se*, entered psychology much earlier, through the work of Gustav Fechner around 1860. (Stigler, 1999, p, 187).

assigning of numbers to objects does not constitute measurement in the classical scientific sense; it is more like a method for categorisation or classification. In measurement-theory terms, this assignation of numbers to objects would be called 'nominal' if the purpose is to categorise or distinguish one class of objects from another (the number is simply a symbol with no inherent value), or 'ordinal' if numbers are used to create a series ordered by some notion of an attribute being less-to-more, yet the numbers not necessarily indicating any absolute quantity. Nonetheless, Stevens' definition became the benchmark for measurement in psychology, and contemporary texts on psychological measurement still refer to it approvingly and without qualification (e.g. McCartney et al., 2006; Strack et al., 2007). That is to say, the numbers produced by cardinal or ordinal measurements are generally regarded as if they were absolute measurements of quantity.

Michell stresses that the acceptance by psychologists of Stevens' definition of measurement is a rejection of the classical definition. This acceptance was productive in that it allowed for the quantity objection to be overcome; "it made the quantity objection seem quite irrelevant". The quantity objection is essentially the claim that psychological attributes are not quantitative in nature. It was not a genuine resolution of the scientific problem, but allowed the business of psychological measurement to proceed[44]. So psychologists were able to proceed as if psychological attributes were quantitative in structure, and used the term 'measurement' to describe what they did. The use of this term allowed the impression that this was a scientific activity. Specific practices like the standardisation of questions and instructions for

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[44] There is a sense in which my thesis makes a productive play on Steven's definition, in that I consider a range of 'number-generating activities' in relation to sexual abuse.

interviewers in questionnaires and surveys facilitated this perception of equivalence with standardized measurement in the natural sciences (Strack et al., 2007).

By the late 1940s, with Stevens' definition, measurement had become accepted practice within psychology and was considered a necessary aspect of psychology as a science; "…a wide class of number-generating operations were routinely accepted as measurement procedures" (Michell, 1999, p. 78). In effect, the assignment of numbers to attributes was thought to constitute scientific measurement, with no real consideration given to the question of whether the attributes in question were quantitative in structure. Michell (2000) argues that this remains the case to the current day.

It is worth noting that some of Stevens' statements are strikingly resonant with what might now be considered current critiques of science associated with the likes of Latour and Foucault (Matheson, 2006). For example, Matheson cites the following quote from Stevens: "…all that passes for scientific truth is conditioned upon its social acceptance…In fact science can speak of truth only as of a certain place and date, for what is true today was not true yesterday and may not be true tomorrow…Knowledge, or truth…is dynamic, restless and relative" (Stevens, 1939, cited in Matheson, 2006, p. 72). Although Foucault conceptualised truth and knowledge in terms of historically situated discursive regimes (i.e. that which conditions the socially acceptable and the scientifically valid), rather than simply social acceptance, the quote suggests that the work of both shared a suspicion (or an outright rejection) of the claim that scientific truth did or could represent a fixed and timeless reality.

Michell's account is enlightening and provides something of a critical 'insider's' history of the practice of psychological measurement. Importantly, his account does not dismiss altogether the *potential* of psychological attributes to be a proper object of scientific measurement. His claim is simply that it has not been adequately proven that psychological attributes are quantitative in structure. He does not object to or critique the idea that measurable, scientific knowledge about the psychological attributes of individuals is possible or desirable. He does not object to the practice of scientific psychological measurement, he just does not think such a practice yet exists.

As a result, other than commenting on the professionally and economically lucrative advantages that the adoption of measurement as a psychological practice generated for its practitioners, Michell does not pursue the constitutive aspects of the problem. That is, he represents the findings of such practices as objectionable because he does not believe that it has been proven that psychological attributes are actually measurable. Therefore, for him such knowledge may present an inaccurate picture of the true nature of individuals. This implies that if the correct methods could be developed (a project to which his work is directed), then psychology could discover, could represent, some truths about humans. Thus, while Michell argues that measurement often alters the object measured (Michell, 2011), his argument commits to an inter-actionist model of measurement, with the caveat that psychological attributes have not yet been shown to be quantitative. This then requires that I look elsewhere for accounts of psychological measurement for this thesis, as I am interested in following a performative model of measurement.

Historicising psychological measurement

Danziger (1990) proposes a history of psychology that takes as
its basis not the 'rational' approach of consecutive discoveries
or instruments, nor the 'irrational' view of the individual
psychologists and the circumstances of their lives. He
emphasises the constructed nature of psychological knowledge,
and the imperative upon psychologists to construct their
research and its presentation in agreed upon forms to a
community of other psychologists, in order to be regarded as
scientifically valid knowledge. Further, he proposes that it is
not only the methods of psychological research that should be
studied, but also that the proposed object of psychological
study can be studied historically as this is also liable to change.
To be more precise, the objects of psychological research
(people and their characteristics) are constituted by the
historically specific methods used to produce/describe them. So
for example, the notion of personality, and the ability to
categorize personalities as normal or pathological, depends
upon (rather than proves) an additive model of the person,
along with the assumption that the numbers applied to the
various aspects actually refer to something measurable
(McCallum, 1997). While this insight is shared by both
Danziger and Michell, Michell's concern is with the
misrepresentation and lack of scientific rigour this involves,
while the constructivist understanding focuses on the 'truth
effects' of these practices, the contingent historical
circumstances of their production and circulation, and the
practices of governance they make possible.

Danziger argues that the form of the early psychological
experiments was in many ways a continuation of the link
between psychology and physiology. The physiological

experiment, with its concern with response times, and the perceptions of sensations like vision and touch, gave practices of measurement a central role. There was in fact significant crossover between the two disciplines in the early-mid 1800s, and it was only in the late 1800s that the distinction between them became clear (Danziger, 1997, p. 51).

Many instruments used for research in the earlier 'psychophysics' and sensory perception research were also used in psychological research experiments (Gundlach, 2007). These research practices of measurement created a demand for technical equipment, with some research laboratories even employing or contracting specialists to manufacture psychological instruments for these purposes (Gundlach, 2007). At this practical level then, it seems that psychology was bound up with particular forms of measurement from its beginnings (Hand, 2004; Borsboom, 2005).

A distinction here is helpful: whilst physiological research was concerned with directly measuring bodily reactions and sensations, psychological research became more interested in the (human) subject's perception of these sensations. So the phrase 'just noticeable difference' indicates that small differences between weights, for example, are not necessarily perceived by the human subject. The measurement of these 'jnd's' is an example of a type of problem taken up by psychological research, implying the existence of a conceptual space in which actual differences are subject to psychological processes that cannot be explained at the level of physiology or biology. Early psychological researchers were aware of this space as a problem to be managed; for example, weights with different values were manufactured to appear visually identical (i.e. the same size), called 'deceptive weights', to minimize the influence that visual stimuli may have on the subject's

perception of the difference in actual weight. As Gundlach notes, "You will probably find this kind of weight only in psychological laboratories. The Bureau of Standards and your local farmer's market have no need of them" (2007, p. 203). It is worth noting that the very concept of jnd's is premised on the idea that human perception is not accurate- otherwise all differences would be noticeable.

Despite the disciplinary separation of psychology from physiology noted by Danziger, contemporary studies into the traumatic impacts of child sexual abuse continue to combine the psychological and physiological. Post-Traumatic Stress Disorder is a widely agreed upon consequence of sexual abuse, with measurable 'psychophysiological' indicators. Researchers measure phenomena such as forehead muscle tension, electrodermal activity, and heart rate whilst the research subject listens to a trauma script, comparing sexually abused subjects to controls without such a history (Orr et al., 1998; McDonagh-Coyle et al., 2001). Some commentators view this reflective of a larger process of 'embodiment' as an historical event integral to the establishment of PTSD as a 'real' entity with scientific validity (Baldwin et al., 2004).

Three types of psychological experiment

Danziger (1990) discusses three different types of psychological experiments (or models of 'investigative practice') that were important to the historical development of the discipline of psychology, each producing contrasting relationships between the experimenter/psychologist and the 'subject' (although the subject of a psychological experiment is in fact the object of knowledge, the object of the enquiry; or more specifically, it may be not so much the person who is the

object of enquiry, but whatever particular psychological attribute or phenomenon is being studied via the person). Each of these models will be briefly discussed in turn. The salient point here is to illustrate that divergent investigative practices both assume and produce different relationships between researchers and their objects of knowledge. Practices of psychological measurement require specific 'agential cuts' (Barad, 2007) to be enacted. I am asking the reader to hold this in mind, while also holding in mind that relations of power between agents are of central concern in the professional field of child sexual abuse. How we—in our professional capacities—know about the effects of child sexual abuse, and how we respond to those who have been abused, also involve such 'cuts'; I am arguing for an awareness of these cuts that extends to accounting for the emergence of the very practice of psychological measurement in research and therapeutic situations.

1- The Leipzig model

The Leipzig model, associated with the German experimental psychologist Wundt (working in the mid to late 19th century), was typified by a collaborative partnership between subjects and experimenters, often students and teachers. The roles, while distinct, were also changeable, so that each person could play different roles in different experiments or even in the course of one experiment. In this kind of experiment, the object of investigation was taken to be the 'normal, mature' mind. The aim was to discover psychological characteristics that were common amongst 'normal' individuals; psychologists were the same kind of being as the subjects of the experiment (Danziger, 1990). This understanding of the psychologist as like 'Everyman' (sic) has been evident in other contexts, for

example the shift from behaviourism to cognitive science in American psychology during the 1960's (Cohen-Cole, 2005).

2- The clinical experiment

The clinical experiment (common in hospitals) had a much more strictly differentiated structure, with the role of (typically male) experimenters being clearly in control and in the authoritative position in relation to their (typically female) subjects. No switching of roles occurred in this context. The object of investigation here was the 'illness' or difference of the subject (typically the 'hysteric' or the 'somnambulist'). Healthy individuals were used chiefly for the purpose of comparison, to better identify the nature of the primary subjects' deviance/difference. Commenting on the contrast between these two styles of practice, Danziger states: "In the one case the object of investigation presupposed the asymmetry of the experimenter-subject relation, but in the other case it did not" (Danziger, 1990, p. 54).

3- Galton's model

The third model, that of Francis Galton (in his laboratories in the UK around 1884), involved a kind of contractual relationship between experimenter and subject. Individuals paid for the privilege of having their mental faculties measured. The social acceptability of this practice may have been due, in part, to the popularity of phrenology a generation earlier. Galton's services were marketable to individuals: "In a society in which the social career of individuals depended on their marketable skills any "scientific" (i.e. believed to be objective and reliable) information pertaining to these skills was not only of possible

instrumental value to the possessors of those skills but also likely to be relevant to their self-image and their desire for self-improvement" (Danziger, 1990, p. 56)[45]. Galton's primary interest was the development of a large data base which could be used to advance his interest in a program of eugenics (Rose, 1985).

Galton's model constructed results of experiments as individual performances in a test, reflecting the stable and unalterable abilities of that individual. Their social significance lay only in their comparison to the scores of other individuals. (Galton's type of experiment is classified as 'anthropometry'). In this way a set of performance norms were established against which individuals would be compared. The statistical purpose and nature of Galtonian investigations made the individual subject very different from the 'case' of the clinical experiment and the generalisable model of the human mind in the Leipzig approach.

The social relationships inherent in the three different approaches were entangled with the type of knowledge being sought and produced in each. The different approaches were reflective of different traditions of both social relations and knowledge production. The social aspects of each kind of experimentation produced its own unintended, as well as intended, consequences on the conduct of the experiment and therefore on the data produced by those experiments (e.g. in the clinical model, unequal power relations may result in subjects producing the kind of performance they believe the experimenter desires).

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[45] Similar developments are occurring currently in the field of genetics, with self-administered tests available for purchase over the internet (Rose, 2007).

There is no method which does not have such effects, as any human interaction involves social dynamics. In this thesis, practices of measurement, the production of statistical norms, and the function of comparison are of greatest interest from these models. It is enough to note that Danziger's account of these three models reinforces the validity of the performative model for analysing psychological measurement, by illustrating that the model of practice is never a neutral tool for transparently revealing the truth of the situation under investigation.

Traits

In psychology, the attributes being measured are usually called traits. An assumption of psychological measurement is that people's behaviour can be explained by certain traits or attributes they may possess. Too much or too little of this or that trait may have observable behavioural outcomes[46]. Whether acquired through childhood experiences or through some biological/genetic mechanism, trait theory holds that behaviour is determined or influenced by this conglomerate of psychological objects (attributes). In child sexual abuse research, the assumption is that the experience of abuse is one factor accounting for the state of the relevant traits. Dominant streams of developmental psychology generally hold—to a greater or lesser degree—that once acquired, such traits are relatively stable (although of course they are changeable- this is the very purpose of psychological intervention). The influence of particular contexts in which people act varies according to

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[46] This kind of idea existed at least as far back as classical Greek medicine, namely the idea that the four 'humours'—black bile, yellow bile, blood, and phlegm—existed in varying quantities and intensity from individual to individual and climate to climate, and that the balance of the four was the key to diagnosing both illness and types of personalities- (Arikha, 2007)

schools of psychological thought; they may be thought to be secondary to the person's basic personality, or play a formative role in the development of traits (Morss, 1996). Some schools of psychological thought do call into question the primacy (or the very existence) of stable internal traits relative to the person's context (cf. Potter and Wetherell, 1987); however these schools also generally reject the designation of their psychology as scientific. For now I am interested in just those versions of psychology that claim to be scientific.

Traits emerged as a theme in the following extract from the focus group:

> *CB: ...with the BASC (Behavioural Assessment System for Children) test, can someone talk me through the process of how it works?*
>
> *ELIZABETH: I don't want to do all the talking. [whispers 'Go Kate']*
>
> *KATE: Oh OK. Um...*
>
> *CB: So if you had to explain to someone who had no idea what it was..*
>
> *KATE: OK. I think it's up to 8 years of age... Is it 8?*
>
> *ELIZABETH: Uh-huh.*
>
> *KATE: You get a parent to fill out the form, um, and once the child's over 8, then you can use the self-report. You can also get a parent-rating scale*

to coincide with that, but you wouldn't get a child to use…there's not a scale for the child to self-report on until, um 8 years of age. Um, and you would use it during the assessment period, um, and it's made up of 152 questions.

ELIZABETH: 186 on the adolescents.

KATE: Yeah. Um and they're true or false, the child's asked to answer true or false to a number of, to those number of statements, um, which are really about behaviours, thoughts, feelings…

ELIZABETH: Yep

KATE: Pretty much. And um, yep so we do that during the assessment phase, which would be probably during the first 6 to 8 to 10 weeks that we would be seeing a child. And I try to get the parent to do that at the same time as when I get the child to do that as well.

CB: And is the parent report identical to the children's report?

ELIZABETH, Kate: No, no.

ELIZABETH: There's um, four, four scale, never, sometimes, (Kate joins in) often, always.

CB: and are they the same questions?

ELIZABETH, Kate: No.

KATE: Nuh…and then we've got the computer database program and you basically enter in all the answers and it prints you out a…

ELIZABETH: printout.

KATE: A printout of the results

ELIZABETH: a report.

KATE: And measures them on scales, and then it has…cause all the questions are related to sub-scales and stuff, and then it comes out on areas around um…clinical manifestations, um…

ELIZABETH: Internalising, externalising, maladaptive and adaptive behaviours.

CB: So are these what the scales are? Or, what are the scales…when you talk about the scales and sub-scales, what are they?

ELIZABETH: so for example under the, um, Internalizing composite, that consists of several, um scales, which include like Atypicality, Anxiety, um, Somatization, Depression and anx…did I say anxiety?

KATE: Yep, depression.

ELIZABETH: Yeah, depression, depression is not part of it, um, but…so you can either start by

looking at the composite, the overall, like an upside down pyramid. You start with the composite, then you go into 'right, what are the sub-scales saying?', within the sub-scales, what might be the individual items that are of…that the [composite corresponds to? Recording unclear]

KATE: Yeah So you can narrow it down to what might have been the responses that have elicited that kind of thing, and it puts it, it rates it within everything within, you know, an average range

ELIZABETH: of same age peers

KATE: of same age peers. Based on the normative testing and then, there's at risk and critical items, and on the front page of the print out it will tell you what might be clinical, or at-risk- items that might be things that you might want to look into further and sometimes—I don't know if we're getting to that bit yet—sometimes, I don't always, necessarily, am not guided by that. Like if it says that's at-risk there might be a very good reason

ELIZABETH: hmmm

KATE: and you know the child, you know the family, so that might be exactly what you're expecting at that point. So it might say you want to further testing but at that stage I don't…you know, that's not a process I would necessarily always undertake. Um, and especially if you've got a child that's at the start of therapy so, obviously, you

know, it wouldn't be surprising necessarily that there might be something

ELIZABETH: hmmm

KATE:...if you know that about the child already

DAVID One thing that I emphasize when I explain either to the child or the parent is that what this test measures is your perception…

KATE: hmm

DAVID …of your own behaviour, or your child's behaviour. Umm, and particularly when I get two parents to do it, emphasize that they do it separately, because their perceptions of their child's behaviour may be quite different. Not to do it together. Like some parents might just automatically [inaudible]

CB hmm. So what's your reasoning, why emphasize that it's their perception?

DAVID Umm, I think, because, um, I think having a perception of something is very different from it being a concrete, actual thing. 'Cause perceptions change and are fluid. Um, so [linked to that??] it's not used to diagnose, it's not used to make something that's real. You know, that your perceptions of your child's behaviour can change and can vary depending on how you are right now.

And I think it's also that to make it a dynamic thing
that it can change

Understandings of illness or disease have historically oscillated between two basic models. Either illness is caused by some foreign element or pathogen that is alien to the nature of the organism, or else it is caused by an upset in the healthy equilibrium of natural elements (Canguilhelm, 1978). Trait theory is clearly an example of the latter. That is, it is not that Aggression or Anxiety are inherently unhealthy, but simply that one may have too much (or perhaps too little) and that this causes the person to be psychologically unwell. In the logic— or style of reasoning (Davidson, 2001)—employed by measurement instruments such as the BASC, the problem is one of quantitative imbalance, not qualitative error.

Constructing traits

The contingent nature of psychological traits is implicitly (but rarely explicitly) acknowledged when psychologists refer to traits as 'constructs'. For example, Reynolds and Kamphaus (2002), authors of the Behavior Assessment System for Children (BASC), state that the BASC is based upon a history of scientific research that has established constructs such as Anxiety, Depression, and so on. In this sense, the term 'construct' refers to the fact that Anxiety is not directly accessible to the researcher/clinician, but is an assemblage of behaviours and other self-reported phenomenon. Over time, through an accumulation of psychological research, some of these behaviours are put together to assess the existence and amount of Anxiety in a particular individual.

The BASC clinician's guide (Reynolds et al., 2002) references the existence of psychological traits as explanations for behaviour. The BASC is divided into various scales for psychological constructs such as anxiety and depression. "The scales are based on a half century, or more, of psychological and medical science" (ibid. p. 19). The authors assert that this history provides the evidence needed by BASC users "to draw inferences related to individual scale scores" (ibid. p. 19); i.e. to connect behaviours to psychological constructs.

I suggest that the history of research to which they refer in fact highlights the 'constructed' nature of psychological constructs, whereas these authors take this history as an accumulation of evidence that such traits exist and are merely being discovered. Although the word 'construct' is often used interchangeably with 'attribute' or 'trait' in psychology literature, its use in this context provides a stark contrast to its use in studies such as Danziger's book *Constructing the subject* (Danziger, 1990). The two uses of the word construct could not be more different—it is a fascinating example of the 'one sentence, two statements' phenomenon (Davidson, 2001). In Reynolds and Kamphaus, constructs have a solidity and permanence which the history of scientific research has revealed as truths. Danziger's reference to construction in psychology aims to unsettle just this sense of permanence associated with psychological objects.

In summary, psychological research using measurement of traits as an explanation for behaviour establishes kinds of attributes of which people are made up. The truth of these attributes is literally constructed, crystallized over time through the accumulation of produced knowledge.

Any particular instrument of measurement produces only those attributes which it is constructed to measure. There are two critical approaches to this situation: 1- a humanist criticism which argues that such practices *break down* the individual into the component parts, thereby distorting or losing the unique and essential human essence of each individual; or 2: a performative critique which holds that, seeing as there is no pre-existent human condition prior to knowledge, in the measurement situation the objects are produced and assembled to *make up* the person. This thesis has been developing the second line of investigation.

As the lists of attributes from child sexual abuse research show, the production of knowledge about victims of child sexual abuse makes-up the very same individuals being researched. This can be demonstrated visually in the graph produced by the BASC analysis (). This graph lists the attributes along the horizontal axis, with normal, at-risk, and clinically significant amounts indicated along the vertical axis.

Referring back to Barad's (2007) analysis of scientific measurement, any knowledge that can be gained from such a measuring instrument is produced by the research situation, including (but certainly not limited to) the instrument itself. This is not only because of the epistemological limitations of measuring tools, but that the very objects being measured are only brought about by the research situation.

In the case of the BASC, the objects of investigation are ultimately not the child, but the various traits (anxiety, hyperactivity, depression, etc.). However, these traits are not directly measured by the BASC. It is not an experimental situation like that found in physics. What I suggest the BASC

does do, in a clinical context, is to act as an apparatus to *order* the vision of the observers, in a similar way to other technical devices intended for observation (Crary, 1990). Certain specific behaviours have been identified and linked to psychological traits (constructed through years of psychological research). The observers (who are not the clinician, but teachers and/or parents, as well as children over the age of 8 who are enlisted as observers of their own behaviours) are disciplined in the sense that they are incited to watch the child and reflect on the frequency of certain behaviours, while others are necessarily excluded.

There is no feasible way that these observations can be said to constitute measurement, let alone precise measurements, in the classical sense; they are observer's judgements, guided by the particular questions asked on the BASC forms. This is most obvious in the instruction to parents, not to count the number of times a child does a behaviour, but to rate them as 'never, sometimes, often', and children simply to provide 'true or false' responses to the questions. What this process produces is a range of limited possibilities for the 'kind' of child being assessed. On each attribute being measured, the child has a chance of being assessed as 'normal, at-risk, or clinically significant' (indicated by green, orange, and red circles respectively; see figure 2, on p.219). Although this does not meet the classical scientific criteria for measurement I described earlier in this chapter, it is nonetheless regarded as measurement in psychological research.

**Classical scientific models of measurement meet
contemporary scientific models of measurement**

The distinction between permanent traits and changeable states
was referred to and reiterated in the discussions. In the extract
above, David points out two important things: firstly, that
psychological testing (especially where children are the object)
is often based on the perceptions of others, and secondly, that
these perceptions themselves are fluid. The child and their
psychological attributes are produced, as it were, by a
measurement of these observations at a
particular moment in time by the interventions of others around
them. These interventions are "not used to make something
that's real", even though they certainly can have material
effects.

This concept of fluid perceptions vs. stable traits is a useful
way to illustrate an important example of how a contemporary
science (quantum physics) constructs measurement as a
practice. In the classical scientific sense of measurement
described above (associated with Newtonian physics), when an
object in nature is measured, is taken to say something about
that object (even if the measurement itself is subject to human
error). A measurement of a child's anxiety would be reflective
of an actual property (attribute) of the child that exists
independently of being measured.

With quantum physics (in contrast to classical physics), the
matter of measurement is different. For example, the state of a
photon wave's particles only becomes realized by our
intervention in measuring them at a particular moment. Other
probable states effectively 'vanish' at that moment in time, but
this is not a reflection of the nature of the thing being

measured, it is an artefact of measurement itself (Barad, 2007). The research situation (which Barad calls a 'phenomenon') consists of specific material arrangements. The properties being measured only become meaningful within that arrangement. All the material elements of the phenomenon intra-act to produce the measurement. Crucially, even the observer/object split is a product of the arrangements; Barad calls this an 'agential cut'. This is one important basis of the performative model of measurement.

Applying this to the case of psychological measurement, it is necessary to take into account the various apparatus that construct the situation. This includes those elements that are internal to the research situation, such as the room, the pieces of paper and the words written on them, the spatial arrangement of seating and tables, the presence of the researcher/counsellor and the child, and the words used to describe or explain the procedure. But a diffractive reading/performative model also requires us to provide a genealogical account of the arrangement. An explanation is needed for how this specific arrangement is possible as a material practice. Barad argues that one could in fact provide an objective account of this situation, if the phenomenon of the measuring, and not the object of measurement, is maintained as the analytical object. In other words, it might be possible to objectively describe the measuring, but not the measured object. (This involves a reworking of the notion of objectivity). I am hesitant to claim any aspirations of objectivity for this thesis, but I do hope to provide an account of the 'phenomenon' of the psychological measurement of people who have been subjected to child sexual abuse.

"The crucial feature of quantum theory is that the observer is not only necessary to observe the properties of an atomic phenomenon, but is necessary to even bring about these properties" (Signorile, 1989, p84, citing Capra, 1982). This helps to disrupt not only the notion of a *stable* trait, but also draws attention to the effective fabrication of traits themselves through intervening. In a sense, then, the results of our intervention into this system of perceptions and change might be an act of "participation in chance events" (Signorile, 1989, p. 81, citing Polanyi). At a point in time, a child or parent is asked to answer a series of questions, the results of which are subject to quantitative analysis in order to produce an account of the child's traits; or better, to produce the child's traits. Other possible results exist, but only one can be actualized.

This brief detour has been for the purpose of establishing that whilst the practice of psychological measurement frequently claims to be embedded within a classical scientific model, there are other contemporary models of scientific measurement which would not endorse some key assumptions of the former. Perhaps ironically, by the mid-late 1950's behaviourist psychology and the model of science that it attempted to affiliate itself with was already being characterised as "outdated…and …rejected by logicians of science" (Cohen-Cole, 2005, p. 116, citing Koch, 1956, 1959), even being associated with authoritarian politics. Some authors such as Kvale (1992) argued quite strongly, 20 years ago, that the entire discipline of psychology as a science was born of, and remained hopelessly entrenched in the age of modernism, and thus held little relevance to the so-called 'post-modern' era.

I argue, then, that the so-called 'hard sciences' such as physics have engendered and responded to the ontological and

epistemological challenges raised by 'post-modern' critiques (e.g. Barad, 2007), focussing particularly on measurement. Later in this thesis I will be examining recent psychological texts concerned with measuring the effects of child sexual abuse, to assess the extent of any awareness of the view that these strictly hypothetical-deductive models of science are seen as outdated[47] even today.

The continuing hold of classical science in psychological research on child sexual abuse

This opens up another productive question: if the classical model of science *is* considered outdated, how can the success of psychological measurement be explained? If scientific theorizing itself, not to mention the post-modern and feminist critiques of science, quantification and scientific method, have been so comprehensive in unsettling classical scientific notions of measurement, I would argue that this is not reflected in current psychological research into child sexual abuse. Academic or intellectual refutation of the classical scientific foundations of psychological measurement has barely registered in the pages of journals such as *Child maltreatment, Journal of interpersonal violence, Child abuse and neglect,* or *Journal of child sexual abuse*. I will cite just one recent text (from the latter journal) to make clear that child sexual abuse researchers do, in fact, say that they are measuring. It is not a question of me having to interpret or dig away at a deeper meaning of the text, but simply to present what the text itself says. The authors here describe the instruments they use to

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[47] In using the term 'outdated', I do not mean that ways of knowing about things necessarily improve or always change for the better. I mean only to refer to scientific discourse itself, and to examine the status of the kind of science that psychological measurement depends upon for legitimacy.

collect the data for their case study of a young woman who had been subjected to child sexual abuse. "Beck Anxiety Inventory (BAI): The BAI (Beck & Steer, 1990) is a subjective rating instrument that measures anxiety symptoms in adult and adolescents. Beck Depression Inventory (BDI-II): The BDI (Beck, Steer, & Brown, 1996) is a subjective rating instrument that measures depressive symptoms in adults and adolescents" (Khalily et al., 2011, p. 342).

While noting the claims to measurement, I am also struck by an interesting juxtaposition. The instruments are described as both subjective *and* as conducting measurement. How can this be, given the insistence that measurement is desirable because it is objective? How can a measure be subjective and objective? Paying closer attention, the statement is clear that the *rating* is subjective (being performed by the research participant- usually in the form of a Likert scale of 1-5 describing the frequency of specific thoughts or behaviours). These subjective ratings are transformed into measurements by the instruments (including the expertise of the psychologist/researcher). The numbers generated by the instruments are then treated as objective measurements in the statistical procedures that follow the initial test-taking and scoring.

One further quote illustrates—and complicates—the point: "It is not enough to know that CSA subjects are more poorly adjusted than controls; it is also important to know by how much" (Rind et al., 2000, p. 9. The reader is left to ask why this is important as no specific reason is given). The very question of 'how much' assumes that differences in the attributes which are used to measure adjustment are not just ordinal but are indeed measurable.

This kind of insistence that whatever exists (such as 'harm', or poor adjustment) must exist in some amount echoes the sentiments expressed in the early 1900s by leading figure of the time E.L Thorndike (cited in Green, 2003). However, other psychology researchers noted that not all properties or attributes can be properly subjected to the kind of measurement that produces a 'how much':

> *Hardness and softness, like temperature, shape, density, intelligence, courtesy, are non-additive qualities. Such qualities are frequently called intensive. They can be ''measured'' only in the sense that the different degrees of the quality may be arranged in a series. Concerning them, questions of how much or how many times are meaningless''. (Cohen & Nagel, 1934, p. 296, cited in Michell, 2006, p. 419).*

Theodore Porter suggests that "[t]o understand the circumstances under which quantitative objectivity has come into demand, we need to look not only at the intellectual formation of experts, but even more importantly at the social basis of authority" (Porter, 1995, p. 6). Why, Porter asks, do we 'trust in numbers' so readily, even when they may not represent a very relevant means for knowing about a specific kind of object?

Despite ongoing contestation within psychology of the validity of using practices of quantification in understanding human thought and behaviour (Hezewijk, 2004), tools of measurement

have proliferated[48]. Some contemporary commentators from within the discipline of psychology do take this use of measurement, and it's uncertain scientific status, as a phenomenon to be explained rather than taken for granted (e.g. Michell, 1999; Hand, 2004; Borsboom, 2005), and some of this work informs much of my own thinking here. However, as an analysis of research texts demonstrates, much scientific research on the effects of child sexual abuse assumes the importance and necessity of measurement, and this will be cited to argue that measurement practices 'order' contemporary understandings of child sexual abuse. My intention is that the perceived inevitability and taken-for-granted-ness of psychological measurement be disrupted (Hacking, 1999), and made to seem 'strange' (Kendall et al., 1999).

I want to briefly discuss an example of a contemporary form of psychological measurement, before concluding this chapter.

Brain imaging technology

I want to turn briefly to a relatively recent development in psychological measurement; brain imaging. This was mentioned by one of the focus group participants:

> *JASON Is there just not a sufficient test to show that brain development and brain functioning, if you have insecure attachment grows one way…*

> *ELIZABETH I think there's lots of theory, but I don't know if there's an actual test..*

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[48] The electronic database *Mental Measurements Yearbook* lists no less than 393 tests that can be used for the assessment of children (accessed 09/05/2008).

> *JASON You could have an actual physical test that says "Hey, your brain is developed this way…*
>
> *KATE There is brain…*
>
> *JASON Which shows that you have had insecure attachment. Your brain is, by this scan it shows your brain is like this.*

Medical technology is becoming increasingly implicated in the measurement of psychological processes, through the practice of brain imaging. Images produced through brain scanning (Positron Emission Tomography- PET- and Magnetic Resonance Imaging- MRI) are becoming increasingly familiar, and one notable context for such images is the covers of psychology textbooks (Beaulieu, 2002). There are three justifications for spending some time on this subject here: firstly, as Jason indicated above, counsellors in this field are interested in the possibility of using such technology. Second, brain imaging is a form of psychological measurement in its own right. The third justification is to illustrate the methodological point mentioned earlier. Although PET and MRI scans are becoming more and more familiar, they are novel enough to retain some degree of strangeness. They remind us not only of the way that technology can produce scientific knowledge, but also that any practice of knowledge can be both familiar and strange. They may also help to explore what is meant by Foucault's notion that knowledge is a relation between the sayable and the visible (Deleuze, 1988).

Although at first such images might seem like a visual medium (and of course, they are partly that), Anne Beaulieu's (2002,

2003) research with brain image researchers and professionals shows that these images are not really read as visual *per se* by those involved in their production. These professionals interact with the images as simply representations, admittedly ones that hold some popular appeal and are available to a wide audience as engaging pictures (Dumit, 2004). According to Beaulieu's research participants, the images *represent* something meaningful, but are not meaningful in and of themselves. What they represent is a *measurement* of the brain; they are "pictures of numbers" (Beaulieu, 2002, p. 57). As one imaging researcher put it: "They're not pictures, they're statistical maps. So you're showing hard evidence" (ibid, p. 60).

Brain imaging has been used to measure both physical features (e.g. volume), and activity of brains. Measuring brains often concerns mental processes in 'normal' brains, such as cognition (Bösel, 2007), while other research is concerned with measuring changes that are wrought on the brain by psychologically traumatic events (as distinct from physical injuries caused by accidents, although this has been an historically important area for the study of brain functioning; *ibid.*). For example, Stein and colleagues (1997), using MRI technology, found that women who had experienced childhood sexual abuse had significantly reduced (5% smaller) left-sided hippocampus volume compared to non-victimized women. Later studies using both MRI (to measure volume) and PET (to measure activity) technologies found smaller (up to 22% smaller) and less active hippocampus regions (right and left) for women who had been sexually abused and suffered from PTSD, compared to controls who had either been sexually abused and suffered no PTSD, or who had neither been abused nor suffered from PTSD (Bremner et al., 2003, cited in Roth et al., 2007). Recent Victorian Department of Human Services

training material for child protection workers includes brain scan images, contrasting the brain of a 'non-abused child' to the much smaller and evidently less active brain of a 'traumatised child'.

These techniques render the question of psychological measurement in different ways, producing different possibilities for conceptualising how such measurement might be undertaken. For one thing, brain imaging renders the Cartesian mind/matter split irrelevant by taking the brain (matter) as its direct object. Being *matter*, the brain is directly observable and thus measurable in ways that the *mind* could never be. Through these technological possibilities, psychological attributes become extrinsic (directly observable and measureable), and no longer intrinsic (measurable only via their effects on behaviour). The quantity objection outlined by Michell (1999; see the earlier discussion) disappears when psychological processes are taken to be chemical/biological and thus directly observable and measurable.

These are possibilities that some of the participants in my research were alive to. In the context of discussing whether the effects of trauma and insecure attachments in childhood could be measured, Jason (in the above extract) was doubtful that pen-and paper psychological testing could produce a meaningful measurement, but turned to brain scanning technology as holding this possibility.

The possibilities of this technology are clearly an exciting development for some counsellors and researchers. While such developments are debated and sometimes criticised as a tactic of biological reductionism, they are also productive processes that open up new possibilities for understanding and

management of the self (Beaulieu, 2003). In this sense, then, brain imaging technology can be read as a continuation of the practice of psychological measurement, which can direct counsellors in the therapeutic management of their clients' lives. Resource restraints prevent brain imaging from being an everyday practice with clients, but research in brain functioning is becoming increasingly incorporated into contemporary knowledge production about people who have suffered trauma and abuse.

This entails a noticeable reconfiguration in how we think about psychological attributes and processes. Speaking about changing conceptions of atoms from Democritus through to the physicist Feynman, Barad says: "Not only has our image of the atom changed, but our practices of imaging and imagining and intra-acting with them have changed, and so have we" (2007, p.354). I suggest that the word 'atom' could be replaced with 'psychological attributes and processes' without losing the meaning of Barad's argument. A new technology not only changes the way we understand the object, it also changes the object, and just as importantly, through our intra-actions, changes us. As counsellors and/or researchers in the field of child sexual abuse, our 'selves' become different across different material relations of engagement and entanglement.

Summary of Chapter 4

In this chapter I have attempted to establish the instability of psychological measurement as a scientific practice, by showing that what is called psychological measurement is based on one specific model of scientific measurement. I have begun to develop an alternative model of analysing measurement, a performative model, based on the argument that scientific

measurement is in fact a heterogeneous concept formed by local material practices.

In the previous chapter, I focussed not on the scientific dimensions of psychological measurement, but what might be called a disciplinary analysis. The two approaches to analysis of psychological measurement will together form the basis of the rest of the thesis, focussing specifically on the psychological measurement of the effects of child sexual abuse.

I have noted that measurement and statistics are produced within both scientific and administrative (or disciplinary) discourses. I have teased out the connection between these ways of producing and conceptualising measurement and statistics and the practices of psychological measurement. I intend to show that scientific and disciplinary practices of measurement are entangled, and that it is necessary to consider both elements. I have described some divergent understandings of what measurement is and what it does. I have also stated that practices of psychological measurement are influential in understandings of child sexual abuse. In the next chapters I want to investigate more closely practices of knowledge production, tying this more explicitly to psychological measurement in research and therapeutic settings with people who have been subjected to child sexual abuse. In Chapter 5 I examine the function of 'normal' within these practices.

Chapter 5- Normal

In this chapter I begin with an extract from the focus group that is mainly concerned with the issue of *normal* and how to understand this idea in the context of child sexual abuse therapy. While the counsellors did not endorse the idea that the aim of therapy is to normalise their clients, there is nonetheless a difficulty in the relationship between the concepts of normal, harm, and well-being. This is particularly complex in light of the fact that psychological measurements generally take *normal* as the point of reference for establishing degrees of harm of child sexual abuse and classifying behaviours and traits as 'clinically significant' or not.

The tensions and difficulties with the potentially regulative functions of normal, especially within developmental psychology, have been established by a substantial tradition of critical literature over the last two decades or more (e.g. Walkerdine, 1988; Morss, 1996; Rose, 1999; Ashenden, 2004; Burman, 2007). In this chapter, I do not focus on going back and re-establishing these claims[49]. I am more interested in historicising normality's privileged position in research and therapy on the effects of child sexual abuse. I am also interested in the difficulties of the critical claims themselves. Resistance to normative ideas of development raises further questions about how, then, we go about understanding the effects of child sexual abuse. I will note here that this approach is informed by the diffractive practice I am attempting to perform. Rather than establishing or rejecting the

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[49] I could say this is one black box I am leaving relatively untouched.

truth of claims about norms and normality, I try to account for both their 'conditions of possibility' and their effects.

This chapter is structured around developing an account of how the following conversations in the focus group were possible. How were certain things sayable? Upon what epistemological conditions, material arrangements and relations of power does this conversation depend? What effects do these difficulties produce for understanding the effects of child sexual abuse?

> *CB What's therapy's role in relation to the idea of, you know, kids being normal, or not normal*

> *ELIZABETH Hmmm, cause I know you don't like the word normal[50]*

> *CB Do you? [laughs]*

> *ELIZABETH Yeah. You always like 'expected'. What might be expected? Yeah*

> *CB Yeah, what do you reckon, what's the difference? Between using those words?*

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[50] This statement, demonstrating that I have influenced Elizabeth, could be problematic if I were claiming that my research approach was objective in a classical sense. However, as I have made clear, I do not make such claims (refer to Chapter 3). Rather, I try to acknowledge and account for my 'entanglement' in the research. I am of the view that challenging and disrupting the notion of normalcy is a useful thing, and am pleased that my discussions with my colleague seem to have had some effect in this direction. Obviously, in this focus group I was invested in asking her to articulate this further. I would also note that Elizabeth has made her own interpretation and use of our past conversations that do not necessarily accord with my own.

ELIZABETH Well ,normal, what's normal? But I think in certain ages, etc. there's things that would be, personality traits, social skills etc. that would be expected. And so…yeah, I'd rather use the word 'expected'…oh, I'd write the word 'normal' in my reports, but I mean 'expected' [laughs]

CB Yeah. But um….and normal is the word that the test itself uses

ELIZABETH Yeah

CB It says, this is the normal range, so why do you prefer, or why do you mean 'expected' as opposed to normal?

ELIZABETH Expected, because what is normal? And I remember you used to say that- 'what does normal mean?' you can say something's expected, you know or it's normal, it's a normal reaction for a victim of sexual abuse, mm…how, normal? I don't know, but is it something that might be expected, for a victim of sexual abuse that they might regress in behaviours. I just think umm, I don't know, normal more puts people into 'normal/abnormal', there's those two categories

KATE Yeah, you can, if you're not normal, then there's something wrong

ELIZABETH Yeah, that's right, where as if it's something that's expected …

KATE If you say that's not expected, that's not necessarily saying there's something wrong with that

ELIZABETH No

KATE That wasn't expected, or that's extra-ordinary in some way, but it's not an abnormal trait which I suppose that's a societal, kind of negative

ELIZABETH That's right. The connotations I think it brings up, you know 'you're normal', or well 'you're a weirdo' you're crazy, you know just those things I think it can raise for people, even in trainings I use the word expected, what would be expected in terms of child development. So…anyway that's just me.

CB Yeah

ELIZABETH   So when I compare it to like the BASC to a group of same age peers, we would expect, you know, you would expect that they'd have a certain degree of social skills. But when it's really high and says 'well no, that's actually clinically significant', their social skills are quite, um, delayed compared to their peers and you know, they can't do general encouragement, they can't share with others, etc., well that's not something that's expected with that age group, so why are they engaging in such behaviours? So, anyway, that's why I like expected.

CB   And…um… I guess…'cos the actual tests if you look at graphs and stuff say well this is normal range

ELIZABETH Graffs [pronounced differently, all laugh]

CB   So, what's you're understanding of, in terms of the test? They talk about norm groups, they say 'well this is how we define what normal is', so what's your understanding of what normal means in the context of a graph?

KATE The bell shaped curve, how they fit in, that's how they work it out, isn't it? That's what they say, if it fits within that range

In the above conversation, how were such questions and comments possible, and how was the notion of 'normal' produced as a contentious and problematic term? At first blush, the attempt to displace the term 'normal' with 'expected' may appear as a purely semantic distinction. This reading would be in line with narrative therapist Jane Hutton's (2008) suggestion that expectations are closely tied up with norms, that the two tend to team up in a regulatory assemblage. There would also be a distinction to be made between *normal* as referring to a standard, which may not be explicitly social in nature, and *expectation* as cultural or social pressure to conform to[51].

However, the counsellors in the focus group attempted to produce a different distinction between what is *normal* and what is

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[51] Ron Frey, personal communication, 2012.

expected. Normal implies a "*kind of negative*" for those deemed abnormal. In contrast, the notion of expectations facilitates the therapeutic-assessment question- "*Why are they engaging in such behaviours*?" Normal is regarded as a statement of judgement, whilst expectation is a statement of enquiry and explanation.

Again, this thesis asks not whether such a distinction is valid, but about the grounds upon which such a problematisation occurs. In short, what were the contingencies, or the conditions of possibility, that made the conversation 'have-able'? I am interested in exploring what enabled the counsellors to be critical of the notion of normal, when the normal child is so pervasive and taken-for-granted in quantitative research on child sexual abuse. In this chapter I argue that *normal* is a foundational reference for establishing the harm of child sexual abuse, yet its' very normativity produces resistance and discomfort in therapeutic practice.

I want to reiterate the extent to which *normal* is contested. The following quote is offered by Burman as something of a critical statement, a critique of dominant discourses about children and child abuse:

> *...the dominant naturalised discourse of ('normal' or 'typical') development ...has helped to produce the position of the abused child as different, abnormal and outside prevailing discourses...of childhood (Burman, 2003, p. 37)*

As the focus group extract above suggests, such concerns are not limited to the theoretical or academic, but inform the thinking and

practice of counsellors who simultaneously employ and contest the discourses of developmental psychology which produce the very categories of normal/abnormal. Thus, in articulating a position regarding the usefulness of the norm, the counsellors (myself as researcher included) speak themselves into variously refined subject positions as counsellors, positions which both engage and contest the place of *normal*. The productions of such subject positions are contingent upon the possibility of the norm *and* its problematisation in the field of child sexual abuse therapy. I want to avoid the suggestion that the counsellors uncritically embraced the notion of normality in these discussions.

Normality and well-being

Scientific enquiry has generally equated health with being a 'normal' state, and illness as a pathological state (Canguilhelm, 1978). This scientific reification has contributed to the capacity of *normal* to perform as a powerful technique of governance (Hacking, 1995). It is in reference to the norm that the individual subject of disciplinary power is produced and governed (Foucault, 1977a)[52]. To be normal is a goal towards which individuals are incited to orient themselves as an ethical ideal of health and well-being, and deviations from the norm ought to be worked on.

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[52] It is worth noting that Foucault argues in *Discipline and punish* that disciplinary power effects greater individualization towards the bottom of the hierarchy (see p. 193), with the child as the most individualized subject of disciplinary power. However this evocation of a hierarchy is uncomfortable, not only because it maintains elements of a sovereign model of power, but also because it would suggest in the current study that the counsellor should be more anonymous or less individualized than the child. This is a position which I will be attempting to disrupt in the current study, where I will be arguing that the subjectivization of the counsellor is as much an effect of disciplinary power as the subjectification of the child.

Paradoxically, although 'normal' is morally conflated with what is natural, expertise is frequently sought in the quest for normality (Rose, 1998, 1999).

> *The normal stands indifferently for what is typical, the unenthusiastic objective average, but it also stands for what has been, good health, and for what shall be, our chosen destiny. That is why the benign and sterile-sounding word 'normal' has become one of the most powerful ideological tools of the twentieth century (Hacking, 1990, p. 169).*

Scientific discourse on sexuality in general has been very closely governed by notions of normality and deviance; Krafft-Ebbing's *Psychopathia sexaulis* (1965), first published in 1886, is widely cited as influential in this regard, with its concern with 'aberrations' and 'deviations'. Perhaps more widely known, to general Western audiences, is the work of Alfred Kinsey, which did not so much question the normal/deviant binary but expanded the range of what could be considered normal[53].

Statistical norms

In order to grapple with theme of *normal* in understanding the effects of child sexual abuse, its privileged place as a point of reference and comparison has to be put in context. Following the demand of diffractive reading—i.e. that I attempt to provide a

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[53] Kinsey has also been accused of minimising the extent and the potential harm suffered through child sexual abuse- Mayne 1993.

genealogical account of the phenomenon under investigation—this requires an examination of the statistical concept of normal and how it used as a reference point for understanding an individual in the context of a population. The earlier extract indicated the ethical dilemma faced by the counsellors in regards to the regulatory and judgemental functions of the normal. If this were the only factor at work, we might expect a relatively easy dismissal of the importance of the normal. However, the following extract suggests the complex productivity of statistical norms for understanding the effects of child sexual abuse.

> *ELIZABETH The only thing I look at is the percentile rank, I find that quite informative. ....on the sheet, so it's, it gives you a number, and I just remember from stats, so if it's got 35 what you actually do is, that means that 65% of same age peers display more problematic…*

> *KATE …Yep.*

> *ELIZABETH …Challenging behaviours than your client. So I think, 'that's pretty good then', so [laughs]*

> *NANCY …{inaudible] for Indigenous clients?*

> *ELIZABETH… I'm not saying, I'm not saying the [inaudible] stuff isn't there,*

> *NANCY …No*

ELIZABETH… I 'm just saying I find percentile ranks…standard deviations have got no…when it's got .05 in terms of, what do you call that, frequency?

[inaudible. Group talking at once]

ELIZABETH …The thing tells you, shoots out at you if something's clinically significant, but yeah, I don't get all that. Percentile rank I found very useful.

SALLY But the percentile rank is based on the mean

KATE: It's based on the mean, and those…

ELIZABETH Yeah

SALLY The 50th percentile is the mean of that sample population

ELIZABETH OK.

CB Can you give an example of how it's been useful to look at the…

ELIZABETH I just find it useful, I don't know, I find it interesting to be able to put it in context with same age peers

CB Yep..

> *ELIZABETH So even if it was like a behavioural thing or whatever comes back, you know you might have a high number and you think 'oh my God that's gonna be really bad', you know. But when you do the percentile ranking you go, you know what; there's 80% of same age peers who display more, um, withdrawal than what they do so I actually think they're going pretty well, in context of everything. SO yeah, I dunno, that's what I like.*

For Elizabeth, the issue is not whether the measurements of attributes or behaviours have any intrinsic meaning; the numbers only take on relevance when compared to the mean (average) among the child's peers. This provides her with a means of contextualisng the child's observed or perceived behavioural difficulties and struggles.

Statistical laws and populations

During the mid-1800s, the French statistician Adolphe Quetelet devised the idea of a 'statistical law'; that social phenomenon would continue to occur at regular rates due to the…

> *underlying stability of the "state of society". Using statistics, it seemed to be possible to uncover general truths about mass phenomena even though the causes of each individual action were unknown and might be wholly inaccessible (Porter, 1986, pp. 5-6).*

Individual causes were of no relevance; what mattered were statistical regularities, or 'laws' (Cole, 2000). Applied to human and social phenomenon, the idea of statistical laws led to the possibility that human behaviour was at least partly dictated by underlying laws of society or human behaviour, laws which were not subject to human will. One of the more famous works in this vein would be Durkheim's pioneering study of suicide (Durkheim, 1952, first French edition 1897), in which he used statistics to argue that social conditions determined rates of suicide (Oakley, 2000).

This type of reasoning presents a problem for the discipline of psychology (or psychological styles of reasoning). In other words, it is difficult to justify individualized interventions into people's lives if their 'problems' are the result of more less deterministic statistical laws at the level of society. There is, however, a different possible use of statistical regularity, one which guides the bulk of psychological research and intervention. This is the reification of normality and the problematisation of individual deviance. Individuals could be incited to take responsibility for their own deviation from the norm, with the help of expertise. Rose refers to this as 'responsibilization' (Rose, 1999). Individual cases of deviation from the statistical norm are understood as errors to be corrected, with the help of psychological expertise if necessary.

In the field of child sexual abuse, an example of this is performed through the notion of 'sexualised behaviours' caused by the trauma of child sexual abuse (Friedrich et al., 2001), where the abused child's sexual behaviours are measurably different from the normal child. Within a psychological style of reasoning, intervening with

the individual child is a possible response because of her/his difference from the norm.

Yet, as the focus group extracts in this chapter demonstrate, the counsellors' relationship with notions of normal, average and deviance are more complex than this analysis suggests. There was considerable resistance to the idea that the role of therapy is to normalise deviance. At the same time, some counsellors found it helpful to compare their client's scores on psychological measures to the average, but stressed that this was not intended as a normative judgement. Difference from the norm was framed by members of the group as an expected response to child sexual abuse, but still a difference that warrants individualised therapeutic intervention.

The 'error law'/ the normal curve

Another statistical device implicated with the normal is the curve of the 'error law', initially developed as a way of understanding and controlling errors made by different astronomers in their celestial observations. Astronomers noticed that their observations of planets differed from observer to observer. But how could this be, given that the planets must be of the same size or in the same position regardless of who observes them (Cole, 2000)? The difference must be due to human error. The error law was devised by Gauss as a tool to understand and control these differences in observations. Each individual's observation was to be understood as a deviation from the truth. The larger the number of individual observations, the more the errors could be controlled for, effectively cancelling each other out, with the aggregate

representing something closer to the truth than any individual observation.

It has to be noted that, while Gauss's curve is intended as a statistical tool, the same principle of truth aggregating around an average is also influential in the qualitative research notion of triangulation. In this process, the results of two or more sources of data (e.g. interviews and surveys) are compared, and where there is difference, the truth is assumed to somewhere in between the two results (Oakley, 2000). There is an ontological and epistemological contrast between this notion of triangulation and Barad's (2007) practice of 'diffractive reading'. This contrast consists primarily in the fact that triangulation assumes there is some form of extrinsic truth, perceived differently from different perspectives (Oakley, 2000), whilst diffraction assumes that each position/practice/discourse is productive of truth effects, with no necessary common object that exists independently of them.

Returning to the error law; Quetelet, while working on the preparation of the Belgian census of 1840, had "…convinced the world that that Gauss' bell-shaped 'law of errors' was precisely the type of law for the distribution of human, social and biological traits" (Hacking, 1991, p. 188). In 1835, Quetelet published *Sur l'homme et la développement de ses facultés*, and with it the introduction of the 'average man'. The average man was the result of measurements of large numbers of individuals (in his example, military recruits) on a range of characteristics (height, weight, etc.). Each individual could be placed in a curve distributed around the average man (Cole, 2000). The average man was put to work in a range of contexts, including as a means of governing the labour of convicts in Australian penal colonies. Convicts sentences were

measured not in chronological years, but by comparing their labour to the amount of work the average man could achieve in a year (Foster, 2005). The distribution of individuals in relation to the average enabled any difference[54] to be rendered as a deviation from the norm, as error.

…the 'error law' finally found its place in 1844 as the formula governing deviations from an idealized "average man". Quetelet interpreted the applicability of this law as confirmation that human variability was fundamentally error, but the effect of his discovery was to begin the process by which the error law became a distribution formula governing variation which was itself seen to have far greater interest than any mean value (Porter, 1986, p. 7)

The notion of human difference-as-error is profound. In his discussion of the 'making up' up of human kinds, Ian Hacking (1995) describes how scientific ideas about people have shifted from a focus on human nature and the 'memorable' person, to the idea of normal and the calculable person, and that this shift was made possible, in large part, by the emergence of statistics. "For well over a hundred years the most powerful second-order kind used in conjunction with people has been normalcy" (Hacking, 1995, p. 371). Foucault describes how Lombroso (around the 1870s) was able to use physiology and psychiatry to discredit political movements to which he was opposed, by scientifically proving (measuring) the deviance or inferiority of the individuals involved (Foucault, 2003, p. 154). Another example of this process of moral judgment through measurement is provided by Reekie (1998): "By the late 1930s unmarried mothers and their children

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[54] Differences which are inevitable, seeing as the average person does not correspond to any 'actual' person.

were being subjected to a battery of intelligence, educational achievement and aptitude tests. As psychological techniques became more refined social investigators attempted to measure the 'emotional maturity' of illegitimate subjects" (p. 126). In fact, statistically calculated norms allow any kind of behaviour or characteristic to be measured and distributed, with averages and relative deviations identified (Hook, 2007).

Foucault's term 'bio-politics' refers to the way in which statistics form part of a strategy of normalization, in which people are understood, see themselves, govern and are governed in relation to such norms (Foucault, 1978, p. 139; Hacking, 2004). It is not simply that the average is 'there', it works as an ethical ideal, "the telos of history" (Cole, 2000, p. 81)[55]. It is crucial to note that in this concept of bio-politics, the norms of the population towards which conduct is directed are those of health, prosperity, and security (Foucault, 2007). The aim of normalization is not oppression, but the incitement of each individual to maximize the quality of their life and the prosperity of the population as a whole. In this way psychology has harnessed the power of statistics, whereas the sociological concept of 'statistical laws' (e.g. Durkheim's studies of suicide) could threaten the legitimacy of psychiatric or psychological 'styles of reasoning' which are based on the notion of individual deviancy (Davidson, 1987, 2001).

To summarise the argument following from the above material; statistical laws linking individual problems to social conditions do

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[55] The average is put to work in this teleological way at the level of governing the individual, but Quetelet also argued that humans would develop over time to become closer and closer to this ideal, and that this would be to the overall benefit of society. This proved to be a very productive idea for the project of eugenics (Cole, 2000).

not provide a rationale for individualised intervention into people's lives when they deviate from the norm of health and well-being. Statistical laws must be joined up with a way of understanding individual differences which not only render those differences problematic, but incite the individual themselves to regard them as such (as errors to be corrected). Psychological measurement makes explicit the practice of the normalising judgement, but also enables the recognition of the harm of child sexual abuse. The phrase *normalising judgement* suggests that some human actions are understood to be caused by an individuals' deviation from the norm on particular psychological traits or attributes, and thus are explainable in reference to the norm. An ontologically different reading is equally possible; that the traumatic experiences of sexual abuse have caused the individuals' traits to differ from the norm (I will develop this theme later in the chapter).

I suggest that the catch, then, or the trouble with normal-the reason the counsellors in the focus groups did not want to completely abandon *normal* despite its objectionable normative possibilities - is two-fold: 1) normality is readable as a sign of health and well-being; and 2) difference from the norm can be understood as an effect of child sexual abuse, making harm visible. I admit to being troubled here by what may be a shortcoming of simply critiquing normal as regulative. I am no longer certain why there is anything necessarily wrong with making visible the harm by showing the difference to the norm. I need remind myself that the normal child is itself a fabrication of psychological measurement (along with other regulative discourses about childhood), that there are relations of power involved in declaring a person or a group to be different from the norm, and that such designations of difference

have consequences for the person or group which may in itself be experienced as problematic.

Development and the normal child[56]

The notion in Western thought that individuals develop, as opposed to simply get bigger, has been traced by historians to the late 18th century (Burman, 2007). The natural sciences, particularly biology, physiology and the philosophical realm of *Naturphilosophen*, focused on two contrasting ways of understanding why things grew (or the question of 'what is life?') (Steedman, 1994; Reill, 2005). These two explanatory frameworks are signified by the terms 'mechanism' and 'vitalism' or 'hermeticism' (Keller, 1985). Mechanism meant that the causes of the body's movements and functions could be explained purely in terms of its material features. Once this 'machine' had been started, "…all the activities of the human body could be explained in terms of its material composition, and the interaction of its parts at the physico-chemical level" (Steedman, 1994, p. 52).

Vitalism or hermeticism proposed that living matter possessed an active principle of some kind that continually animated it. This active force was not an observable thing but an irreducible

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[56] Perhaps the contested nature of the term (normal) should be highlighted by the use of single quotation marks. However, I am cognizant of the point made by (Visker, 1995) In discussing Foucault's use of quotation marks to unsettle the 'human sciences', there is an implication that if done correctly there could be a correct, scientific method of studying humans; and such a method would have no need of the offending quotation marks. Similarly, to discuss a 'normal' child could be read as implying that although the 'normal' child of developmental psychology is a discursive construct, there is an actual normal child waiting to be discovered through other means. This goes against the constructivist reading I am trying to develop here, in which any object of knowledge is a product of the discourses surrounding it.

property of living matter that could not be subject to investigation or explanation (Steedman, 1994). The notion of vitalism, the driving life force, provided the foundation for the idea that individuals have 'interiority' (Steedman, 1994). This point warrants special attention. The notion that individuals have an interior, a space 'inside' themselves that directs one's actions and way of being in the world, is clearly one condition of possibility for the production of psychological attributes as objects of scientific enquiry. Once such a space is established, it becomes possible to produce knowledge about it; the nature of the space, the relationship of the inside space to the observable exterior, and crucially, the ability to devise techniques for its proper management. The interior space is able to be described as one amenable to calculation and measurement, able to be understood in terms of, if not Mathematics, at least in terms of numbers. This was the subject of debate between Enlightenment mechanists and vitalists- the latter arguing that "…active life forces could not be seen directly, nor could they be measured" (Reill, 2005, p. 158), in opposition to the Newtonian program of quantifying the 'life forces' within individuals (Reill, 2005). Both such traditions can be seen at work in different and sometimes conflicting ways in contemporary literature on child development. The development of the individual was closely supported by the notion of evolution of the species.

The notion of a normal pattern or progression of childhood development is an example of the operation of normalcy in the lives of children. Past texts on child psychology with titles such as *Normality and pathology in childhood: Assessments of development* (Freud, 1965) and *The normal child and some of his abnormalities: A general introduction to the psychology of*

childhood (Valentine, 1956) demonstrate and reproduce this structure.

Such concerns continue to be found in titles such as *The normal child* (Bellman et al., 2006). However, there is also a concern with the consequences of defining a normal child. Witness the preface to the just-mentioned text:

> *We accept the dangers inherent in talking about the 'normal child': we might be understood to imply the existence of the 'abnormal child'. Far from it:…we emphasize that every child has norms and that deviations from the 'standard' patterns are often unclear and controversial (ibid, p. v)*

This dilemma was also a concern for Elizabeth in the focus groups: "Normal more puts people into 'normal/abnormal', there's those two categories".

The operations of normality and abnormality or pathology have especially profound effects for sexually abused children, and structure (sometimes critically) therapeutic and research practices. Levett argued that the field of child sexual abuse is characterised by "…an over-riding preoccupation with norms: development, sex and sexual practices" (Levett, 1990, p. 42).

 "'[C]hild abuse' is an idea established in relation to the distinction between the normal and the pathological, that is, it is an idea

generated within discourses of science and resting upon an understanding of 'normal' childhood development" (Ashenden, 2004, p. 17). O'Dell (2003) argues that the 'analytic of developmentalism' (p.135) is the key way in which children and childhood is/are constructed within contemporary psychological (and popular) discourse.

Sexual abuse is seen as an experience which positions abused children outside of the 'normal' childhood experience. This abnormality 'sticks' (Ahmed, 2004) to the child in a socio-cultural sense: the child herself becomes abnormal. This is partly connected to the association of childhood with innocence, which renders the 'knowing' child not only problematic, but perhaps also culpable (Burman, 1991, citing Kitzinger 1988). But more pertinent to my interests here is that this enables a scientific production of the effects of sexual abuse as it impacts upon the expected normal development of the child. The primary concern within this paradigm of developmentalism is identifying and correcting the pathological effects of sexual abuse upon the child's development (Khalily et al., 2011); thus the therapeutic phrase, *a corrective experience*. While I concur with Burman (1994) that the normal child "is…a fiction or myth. No individual or real child lies at its basis. It is an abstraction, a fantasy, a fiction, a production of the testing apparatus that incorporates, that constructs the child, by virtue of its gaze" (p. 16), the effects of the construction of the normal child are concrete rather than mythical. In other words, the focus here is not on looking for the child that developmental psychology attempts to capture and describe, but the children that it produces and governs.

Developmental psychology is a science. A common claim as to what constitutes a scientific approach to child sexual abuse is in relation to causality. Child sexual abuse has harmful effects, and scientific research aims to identify and measure these effects and their size. An important point must be made here; statistical research alone is not intended to demonstrate causality, only correlation. In order to prove or establish causality, statistical methods must be linked up with psychological theory and etiological models which attempt to explain why two or more factors are statistically correlated (Maniglio, 2009). In the classical scientific method research is about testing hypotheses. The researcher posits that an independent variable will bear a causal relation to one or more dependent variables. A commonly found example is when researchers have utilised the notion of traumatic sexualisation (Finkelhor et al., 1985) to posit a cause-effect relationship between sexualised behaviour and sexual abuse among children (Friedrich, 1993; Matorin et al., 1998; Friedrich et al., 2001).

In mainstream developmental psychology, the 'doctrine of continuity' (Magai and McFadden, 1995, cited in Miltenburg and Singer, 2000) requires this notion of linear causality. Harmful experiences in childhood are thought to necessarily result in a raft of psychological and relational problems. This notion of continuity cannot easily account for the fact that many abused children do form compassionate and meaningful relationships during the course of their lives. Miltenburg and Singer (2000) argue that these developments are best understood as moral commitments, rather than as strictly scientific or psychological outcomes, and therefore less amenable to measurement.

Definitions of child abuse have largely revolved around two poles-
the intention of the abuser and the harm suffered by the victim
(Buchanan, 1996).[57] Within an ethic of consequentialism, sexual
abuse is held to be morally problematic because it can be
scientifically demonstrated that it has harmful effects (Slaney
2001; Davis 2005). It could be suggested that the majority of
'effects' studies rely upon and reinforce this notion, 'cataloguing'
and 'documenting' the consequences of child sexual abuse
(Finkelhor, 1988, cited in Davis 2005:112).[58]

Consequentialism removes emphasis from the act of abuse, and
focuses on the effects of such abuse (Lilienfeld, 2002). In other
words, consequentialism is the moral corollary to the scientific
principle of causality, and so is consistent with the classical
scientific premise of cause-and-effect. Measurement provides a
demonstration of a cause-effect relationship between sexual abuse
and harm. This relationship is demanded by scientific practice,
which is not necessarily required by a *moral* objection to child
sexual abuse. Bowman (2005) notes that in cases of child sexual
abuse reported in apartheid South Africa in the 1940-50's, the
concern was with the moral transgression of the offender with little
regard given to the child. He notes that in many of these cases
"[t]here was no apparent quantification of the damage to the child"

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[57] Buchanan is discussing child abuse as a general phenomenon, and not child
sexual abuse specifically. In relation to the question of intentions, I will note that
adults who have sexually abused children frequently do not construe their
intentions (nor their behaviour) as harmful. Psychologists tend to refer these
kinds of thoughts as 'cognitive distortions'; this in itself is a fascinating example
of the power-knowledge nexus in operation around the issue of child sexual
abuse, but one beyond the scope of the current project.

[58] There is a further dimension to this debate that marks any adult sexual interest
in children as problematic because it is a kind of illness (pedophilia, a type of
paraphilia). Here the problem is the pathology of the adult pedophile, quite apart
from any consideration of whether the child was harmed. The emergence of the
pedophile as an object of psy knowledge has been taken up by numerous
Foucauldian inspired writers, for example (Davidson, 1987; Slaney, 2001)

(p. 109) and the child was not referred to social workers or psychologists for counselling.

Ashenden (2004) shows how, in a modern liberal democratic system of governance, state intervention into the 'privacy' of the family requires justification based on scientific, rational knowledge, in order to operate under the banner of objectivity. Whilst the concern with the abuse of children may well be founded on moral concerns, this is insufficient to justify overriding the moral primacy of the private family[59].

Child sexual abuse researchers themselves are often careful to present their findings as scientific, and therefore not as moral. In the magazine *Science,* a group of established child abuse researchers published a short paper titled *The science of child sexual abuse* (Freyd et al., 2005). Here they lamented that the state of research on child sexual abuse was 'fragmented', and "often infused with unstated value judgements". They recommended that advancements in the prevention and treatment of child sexual abuse could be made only by furthering the possibility of

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[59] The requirement of scientific legitimacy is not the only barrier to be negotiated for intervening into men's familial violence against women and children. Wendy Brown (1995) suggests that classic liberal constructions of 'rights' depend upon an unstated exclusion of women and children from the status of citizen. State ratified rights are designed to protect equal citizens from other equal citizens who might attempt to use violence and present other impediments to the pursuit of their interests. "Liberal state-of-nature theory presumes that violence inheres among equals, not between dominant and subordinate persons" (p. 150). The liberal rights effort to protect women and children from men's familial violence flounders, according to Brown, because liberal rights were never meant for the private sphere of the family. Violence and domination within these spheres is naturalized, barring its treatment as political and thus a legitimate object of state intervention. Furthermore, the formative ground on which the liberal subject/citizen depends upon this disavowed zone of exclusion (the 'haven' of hearth and home). Extending rights to these zones would politicize precisely what liberalism requires to be naturalized, in the process upsetting the very grounds upon which (an already masculine) citizenship depends.

conducting scientific research. There is a place for morality and politics here, in so far as funding is required to facilitate scientific research. However the authors qualify even this potentially moral dimension, by justifying the need for research in terms of the economic costs of the impacts of child sexual abuse.

The elaboration of scientific knowledge, which can be seen to objectively establish the necessity of intervention, is required. "In this sense, medicine must find on the body some form of violation in order to substantiate a moral transgression scientifically" (Bowman, 2005, p. 110). In the case of interventions into child sexual abuse, the ethic of consequentialism links up with the requirement of the law to find the irregularity in the body or the psychology of the child victim (ibid.). Bowman refers to this as a "crisis of damage" (p, 140), where the moral offence might be seen to outweigh the physical harm caused in some cases of child sexual abuse. Developmental child psychology acts here as a legitimating form of knowledge to substantiate that abuse has not only occurred, but more importantly, caused harm. Researchers within the developmentalist tradition make purposeful decisions about defining child sexual abuse as a matter centred on the "developmentally immature child" rather than "adults' advantage of authority and power over the child" (e.g. Khalily et al., 2011, p. 339).

While the physical examination acts as both forensic verification and establishes the need for medical intervention, the psychological assessment establishes the necessity of (usually longer term) psychological treatment. In some jurisdictions, victims of sexual abuse may be entitled to forms of compensation to assist with their recovery. Claims are assessed not on the act or

acts abuse per se, but on the psychological harm suffered; harm which must be assessed and verified by an authorised professional such as a clinical psychologist (Gavey, 2003). It is not that physical harm, the "anchoring point of sexual damage" (Bowman, 2005, p. 112) becomes unimportant-any such injuries come to hold a psychological significance. However, psychological research tends to work in terms of psychological constructs that may not have a physical manifestation. In the language of scientific measurement, there is a distinction between directly observable (and therefore measurable) *extensive* attributes, and *intensive* attributes which are only detectable via their effects on extensive attributes. I discussed this in Chapter 3.

Slaney (2001) describes how the ethics of consequentialism (causality) have led to the search for the corroboration of abuse with psychological and behavioural problems to demonstrate why abuse is 'bad'. Consequentialist (or utilitarian) ethics are "...based exclusively on the consequences of an action", in contrast to deontological (or intuitinist) ethics which are "…based on deep-seated beliefs concerning an action's wrongfulness irrespective of its consequences" (Lilienfeld, 2002, p. 182, citing Hacking 1995). For Slaney, an ethics of consequentialism is unnecessary as abuse is unacceptable in and of itself, as it is defined by the issue of power, not harm. In making such a claim, one knowingly either a) challenges the idea that science ought to be solely concerned with establishing cause-effect relationships, or b) eschews scientific approaches as the only legitimate option. It is entirely theoretically possible to propose that 'adult-child sexual contact' is not necessarily *harmful* to the child *without* condoning or excusing such behaviour (a point made by Dorais, 2002, p. 8, but not a position he himself, a therapist, condones).

However, the pervasiveness of the consequentialist ethic, especially the scientific commitment to causality, makes this difficult; thus the insistence that adult-child sexual contact is always, necessarily harmful. To object to child sexual abuse on grounds other than its harmfulness is to challenge the consequentialist ethic. Davis (2005, p. 11) objects to the reliance on a consequentialist ethic for moral wrongness to be established. That is, even if research (such as the Rind et. al. study) claiming that the effects of childhood sexual abuse are not so disastrous as often thought is taken into account, this does not provide grounds for challenging the illegality or immoral wrong of the acts. Davis suggests that such consequentialism, via psychological knowledge, has in fact constrained the lives of adults sexually abused as children. "Indeed, the tendency to equate wrongness with psychological harm has certainly contributed to the tendency to define victims in terms of psychological problems" (Davis, 2005, p. 274: footnote 17).[60]

Critical perspectives on child developmental psychology

The very idea of development is not natural and universal, but extremely specific, and, in its specificity, occludes other marginalised stories, subsumed as they are within the bigger story. The big story is a European patriarchal story, a story from the

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[60] Another obvious implication of the focus on harm is that the seriousness of child sexual abuse may be minimised in cases where children who do not display signs of harm. In a report in *The Age* newspaper (Child sex abuse sentence 'soft', Christine Kellett, August 29, 2007), it was reported that Judge Botting, in his sentencing comments, said there was "scant" evidence to suggest the victim of a man found guilty of child sexual abuse charges had suffered psychological harm as a result of the abuse. This lack of harm was referred to in relation to the sentence handed down.

centre which describes the periphery in terms of the abnormal, difference as deficiency (Walkerdine, 1993, p. 455, cited in Morss, 1996, p.50)

Burman (1992) illustrates how the tendency to understand children through the discourse of developmental theory obscures or neglects the power dynamics that operate in children's experiences, especially with adults. Developmental theory interprets children's speech (or behaviour) as a (de-contextualised) product of their developmental stage (e.g. capacity to utilise logic, rational use of concepts). This excludes considerations of the power relations in children's lives (adult/child, interviewer/child). Burman brings attention to the discursive strategies that children employ to resist or utilise power relations. This brings forth children's agency, especially as users of discourse and subjects within relations of power. These aspects of children's subjectivity are foreclosed in the analytic of developmentalism. This also illustrates that other, competing regimes of description are available for the fabrication of children which produce other possibilities for children (or, indeed, produce other children).

In the statistics of (developmental) psychology, abnormality is literally thought of as a significant deviation from established norms (Pilgrim and Rogers, 1993, cited in Warner, 2009). In measures of psychological characteristics a statistical deviation of more than two standard deviations from the normal range is considered to be 'clinically significant', undoubtedly calling for the expertise of professional intervention. Quetelet's conception of human difference as error is not a metaphor–it is literally applied to real people seeking to understand the ways that sexual abuse may have influenced their lives. This alludes to the manner in

which psychological measurement is simultaneously a means of producing knowledge about people, and a means of governance.

Psychological testing and measurement has been integral to the scientific endeavour of developmental psychology, and psychology in general, and as such, to our ways of thinking about children. As Burman (1994) has observed, "…tools of measurement produce research objects and research subjects. Developmental psychology has been driven by the demand to produce technologies of measurement" (p.3). Rose (1999) shows how these normative expectations of developmental psychology—what children should be like as they grow up—were established in clinical laboratories. Looking at photos of such observations being undertaken (see Rose, 1999, p.146), it is evident that what the adults were involved with was very much perceived (at least by themselves) as 'proper science'. As with all good science, events and observations were measured and recorded. Rose illustrates how the 'normal' child in these studies was first represented through photographs, then illustrations, then sparse line drawings, and finally, tables of numbers. Ultimately, the numbers became more real measures of childhood than actual children.

Harm and the norm

Quantitative research on child sexual abuse frames the question of harm in terms of difference from a) non-abused children, and relatedly b) the norm. The non-abused child is synonymous with the normal child. Distinguishing the normal child from the abused child becomes a focus. For example, Stephanie Dallam and her

colleagues (Dallam et al., 2001) argue that some of the choices of variables studied and statistical techniques used by Rind et. al. under-calculated the difference between abused and non-abused subjects. Dallam et al also argue that the variable 'child sexual abuse' accounts for more of the difference between abused and non-abused individuals than Rind et. al. claim. While what is at stake is establishing the extent of harm caused by child sexual abuse, in their disagreement both sets of authors establish that normality is the prime reference point for measuring the effects of child sexual abuse.

In developmental psychological research, both the normal child and the sexually abused child are examples of the construction of a scientific fact (Latour, 1986). This is not unique to the field of child sexual abuse or psychology in general, but occurs across different scientific disciplines. The making of scientific facts by comparing two sets of numbers, often represented in a graph or table is a trans-disciplinary technique, made possible by the use of statistics: a normal/control group versus a comparative/experimental group. For example, in his study of the discovery of the substance called TRF in the field of endocrinology, Latour argues that scientific objects are constructed as the difference between a control curve and an experimental curve. In other words, results from experiments on a 'pure' substance are calculated and inscribed, then represented in a curve on a graph- this is the control curve (this is taken to be a representation of how the substance 'behaves'). A second experiment is then conducted on a 'purified fraction' (i.e. a substance similar to the first except it contains the object under study, TRF) and similarly inscribed. When the two curves are compared, the discrepancy can be interpreted as a result of the

action of the object under study (TRF). Latour's point is that, as opposed to the internal scientific explanation that the substance caused the discrepancy between the curves, "…the object was constructed out of the *difference* between peaks on two curves" (Latour, 1986, p. 125). TRF could not be said to have existed *as a scientific object* without the two curves and their discrepancies.

In the field of child sexual abuse, I suggest that 'harm' is a scientific fact or object that has been constructed out of the difference between sets of numbers. The superimposition of the curves produced by studying normal (non-abused) children and sexually abused children allows researchers to see—to make visible—the difference between the two groups. This difference is the harm caused by child sexual abuse. Scientifically, statistically, the harm is only visible after these graphs are available for comparison. I stress again that this is not the same as saying that child sexual abuse would not be harmful if it was not studied by psychological research, only that when these effects are cast as objects of scientific enquiry, they become objects of a different kind (compared to, say, political, moral or religious constructions of the effects of child sexual abuse).

Figure 2 (see next page) shows that on a BASC test, the normal child should score in the area just above the bold line (at 50) on a range of attributes (circled in green). The band between the numbers 50 and 60 on the vertical axis indicates the average range for each attribute listed on the horizontal axis; 60-70 indicates the 'at-risk' range (technically, more than one standard deviation outside the mean range); and above 70 is the clinically significant range.

Figure 1- Sample of BASC graph[61]
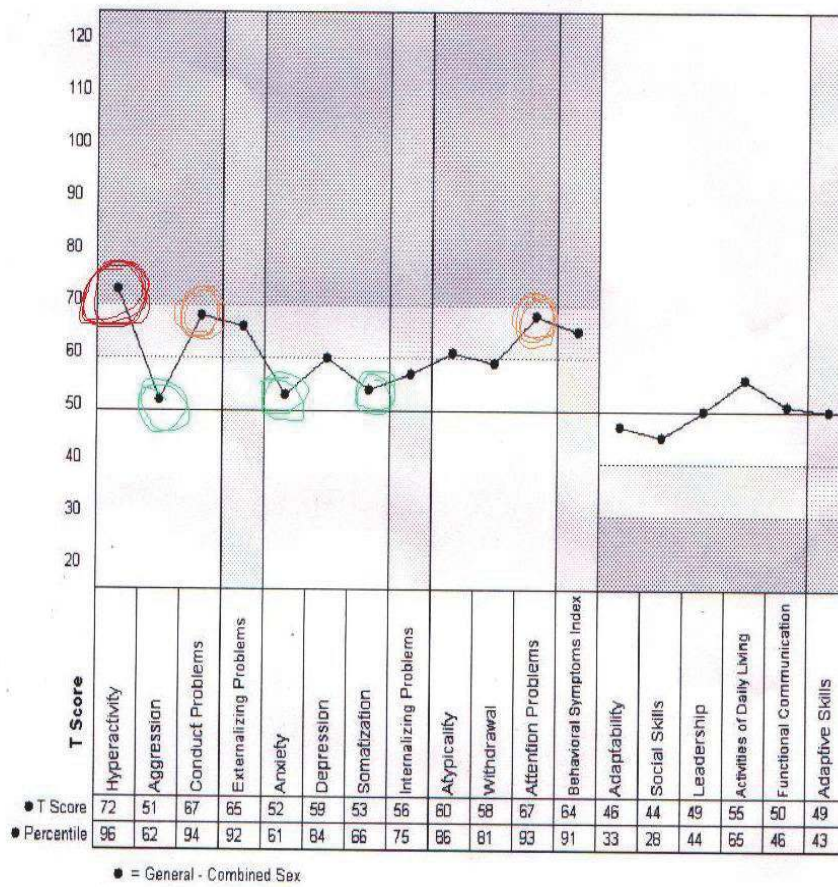
[61] Behavior Assessment System for Children, Second Edition (BASC-2).
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http://www.pearsonclinical.co.uk/Psychology/ChildMentalHealth/ChildADDAD
HDBehaviour/BehaviorAssessmentSystemforChildrenSecondEdition%28BASC-
2%29/Resources/bascsamplereports/basc2_assist_plus_standard_rpt_sample.pdf

'Poorly adjusted'?

Returning again to Rind and his colleagues: "It is not enough to know that CSA subjects are more poorly adjusted than controls; it is also important to know by how much" (Rind et al., 2000, p. 9).

Picking the phrase 'poorly adjusted' out of the quotation cited above indicates how deviation from the norm is problematic in quantitative research on child sexual abuse. Poorly adjusted would mean a score on the BASC, for example, outside of the normal range. However, the word 'adjusted' also provides a clue as to alternative ways to consider relations to the norm. Deviation from a norm need not be thought of as necessarily a poor or deficient state of affairs. Writing in 1933 on veterans of World War I suffering from shell shock, Kurt Goldstein (1995) challenged the conceptualisation of such individuals as simply damaged. Using the biological concept of the adaptive organism, Goldstein instead understood the limited functioning of distressed or traumatised beings as adaptive responses to experiences of the environment. By seemingly curtailing the level of one's functioning on a behavioural, physiological and neurobiological level, the traumatised 'organism' actually enhanced its capacity to survive. Difference from the norm was in fact beneficial.

I think this is what the members of my focus group were getting at. If a child has been sexually abused, they may well be different from the norm, but this is to be expected as an adaptation. The adaptation cannot be understood as a deficiency, because it is an adaptation that enables survival. Yet it is precisely this adaptation that produces the difference between numbers, the psychological measurements of abused vs. non-abused people, and in the

language of psychological testing, the difference is classified as 'clinically significant' or 'at-risk'. This leaves a tense dilemma for the counsellor. What is the role of a therapeutic intervention in the case of an individual (adult or child) whose deviation from the norm has enabled them to survive an abusive environment?

Canguilhem took up Goldstein's ideas (Canguilhelm, 1978) and argued for the importance of *order* to understand processes of cure and recovery- a new order can be established that is beneficial to the organisms changed capacities, situation, etc. It is of no value to compare evaluatively the new order compared to the old order because circumstances and/or the organism have changed- the conditions of survival and functioning have changed.

Canguilhem warned against attempting to correct the 'new order'. "We must not attempt to interfere with these new constants, because we would thus create new disorders" (1978, p. 114). Taking this idea into the field of child sexual abuse would mean that traumatised children should be understood as behaving adaptively, and not as deviant. From this perspective, referring and comparing to the non-abused (or normal) child is worse than irrelevant; in fact it would be harmful to try and normalise the abused child.

In the focus group, Kate was well aware of the problems presented by this tension between, on one hand, an individual child in therapy seemingly suffering pronounced anxiety, and on the other, an appreciation of the context in which this anxiety functions.

> *KATE: well developmentally, but OK you've got Jonny*
> *come in, well he might be a kid that is very anxious*

*about this, in his character traits in general so, I think
you're right, when you look at impacts and indicators
and see yes, you're trying to look at a reduction in that
you know, nightmares, intrusive thoughts, blah blah
blah, you'd be hoping that your therapy would do that
but I guess, the thing is, is that we start with a child
after they've had a traumatic event so, you don't know
them a year ago, you don't know them prior the them
having met Uncle Billy, you know what I mean, so…*

My point here is that while Kate is aware that, as a counsellor,
there is some expectation that she might assist Jonny achieve a
reduction in anxiety, she also struggles with question of how the
anxiety is indicative not simply of an individual deviation from the
norm, but a contextually adaptive response.

The conversation continued:

*ELIZABETH: but anxiety can be, is about feeling
unsafe so if after 12 months of counselling there's, you
know, the child now real….the support system's more
stronger and has a greater sense about how to keep
little Johnny safe, well there's a huge anxiety gone,
and sometimes it can just be about that, that, yeah I
dunno*

*JASON: Yeah I don't think it really matters what it was
because if it's enough to get it to the high end of the
scale then it's come down, obviously it was significant
to get it, do you say clinically significant, yeah, I would*

have thought that'd be important. I'd take that as a positive.

ELIZABETH: hmm. And you'd be worried about the long term impacts if you, you know if you did it in 12 months and then your kid, you're still seeing little Johnny at 2 years and it's clinically significant, I'd be worried about the long-term impacts on little Johnny's mental health having such high anxiety…

KATE: if he's ranked up there constantly…

ELIZABETH: that could lead to phobias or obsessive compulsive, or, I dunno, but I'd be worried about, I'd probably say 'He needs to go to a CAMHS'[62], you know, I dunno. It might help inform whether we're the right place for him given his anxiety is so great…

KATE: yeah, has the right work been done, it might make you question…

ELIZABETH: that's right, I dunno

KATE: have you not…maybe, 'cos that would be something, if you saw that and it was clinically significant and you'd go 'Oh Ok', and it would be hard and like Nancy said that would be highly unlikely if you got something like that that would be surprise to you, I would hope, in our assessment, If you get a high

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[62] Child and Adolescent Mental Health Service

anxiety rating you go 'yeah, that makes sense to me'.
Yep, I get it.

JASON: and I think the question of 'normal' is really
important, because like culturally, I mean I don't know
the exact figures but, apparently Western culture is the
most stressed, the most mental health, the most, all of
these…

KATE: hmmm

JASON: the wealthiest are the most unhappiest so
where does that…what's normal, what are we
considering normal?

CB: yeah.

JASON: as opposed to some of these other cultures
where all of these, not all of them, but a lot of these
issues aren't even present, so what's, you know, you
might question what's normal in all that.

I want to focus here on Kate's comment: "*has the right work been done, it might make you question*". The therapeutic context produces a particular construction of deviation from the norm (in this case, Jonny's anxiety), with an attendant expectation that therapy should address this problem. If therapy fails to achieve this outcome, the counsellor is invited to question her own practice, even while articulating the view that Jonny's anxiety is related to his life circumstances.

Significantly, Canguilhem states that in the context of adaptation to a changed environment that "the concept of normal is not susceptible to objective measurement" (1978, p. 119). However, this is precisely, and literally, what psychological measurement does- it objectively produces normality. The implication of this is that in the context of trauma, an individual functions in a qualitatively different way than they did before, and this qualitative difference is not pathological but in fact adaptive.

These different interpretations of difference, adaptation and deviation, seem to be fundamentally opposed. How does the adaptive interpretation impact upon the therapeutic imperative to normalise deviance? While counsellors experience their entanglement with these questions, when researchers call for improved techniques of treatment for victim/survivors of child sexual abuse (Freyd et al., 2005), what do they mean, if not to restore normality? One text provides an answer quite explicitly: "A better understanding of the pathophysiology associated with exposure to childhood maltreatment will lead to improved psychotherapeutic and psychopharmacologic treatment interventions with a goal of ultimately leading to a lower prevalence of associated morbidities" (De Bellis et al., 2011, p. 550). The goal is to treat affected individuals in order to reduce the prevalence of morbidities; no mention is made here of possible adaptive purposes of these responses. Further, it is worth noting, echoing the earlier concern of Louise Armstrong (1996), that the focus is on treating and preventing the prevalence of 'morbidities' among victims, devoid of any political or contextual positioning of the problem of child sexual abuse.

Based on my reading of the above extracts, I argue that neither the disciplinary view of normal as a regulatory construct, nor the equation of normal with healthy, is adequate to understanding the production of these problems. The counsellors are moved, are influenced, by their engagements with all of these ideas. It is not a sufficient response for the counsellors, to say 'well, Jonny is anxious because of the sexual abuse he suffered'. The subject position of counsellor demands a therapeutic response; at the same time there is an awareness that an individualised response is not necessarily congruent with the situation. The counsellors are left to struggle to account for their own agency within this situation.

Summary of Chapter 5

Statistical techniques alone, whilst pivotal to the "work of modern government" (Tyler, 1997, p. 79), are not inherently psychological. They are means of recording and producing the traits of a population; "they become 'psychological' only at the moment of their application to the inner, emotional life of future citizens" (*ibid.,* p. 79). These numbers, in tandem with the incitement to the *normal* and the goal of maximization of the health of the population (for evidence of this rationality applied to child victims of violence, see Victorian Health Promotion Foundation, 2007), work as measures against which it is "possible to calculate success or failure at the work of being a child" (Tyler, 1997, p. 77).

The results of individual measurements can only take on effective meaning when the results are compared to a general population, with particular reference to where the individual is calculated to sit

relative to the statistical norm. In fact, some authors suggest that this comparative function of psychological tests *is* measurement: "*Measuring* in this case means finding where this degree [to which an individual exhibits a certain attribute] and thus this individual fit into the distribution of that attribute across a defined population" (Gundlach, 2007, p. 214). Such a practice entirely sidesteps the issues raised by Michell (discussed earlier), namely the question of whether psychological attributes have to be quantitative in order to be meaningfully measurable, but raises new issues in the light of a disciplinary analysis of power/knowledge, and critical perspectives of *normal*.

In this chapter I have attempted to unsettle the notion of normal, and also to make visible the tensions that counsellors can experience in relation to normal. While the normal child does sit at the centre of developmental psychology in important ways, this is also contested in practice. The statistical device of normality does not, I argue, entirely succeed in presenting itself as objective and neutral, as evidenced by the critical concerns documented in the literature and the focus groups. There is special concern about the potential for individuals who have been subjected to sexual abuse to be stigmatised or deemed abnormal. At the same time, making difference from the norm visible has been an important strategic achievement in producing the problem of child sexual abuse.

Chapter 6- Objective knowledge about child sexual abuse

The practice of clinical assessment utilises psychological measurement for different purposes than in research contexts (Anastasi, 1993). As part of these distinct arrangements, the object of enquiry as well as the 'test user' are subject to different technologies of the self and modes of subjectification. For example, in test manuals, repeated advice is given to the clinical practitioner that measurements should not be the sole source of information, and that any results of such measurements should always be understood in context of information gathered through other sources such as the 'clinical interview';

> *"...it is important to keep in mind that individual children are not predictable in their manifestation of trauma, despite the trends that are evident among groups of traumatized children. Results of broad-based measures, such as the BASC, must be integrated with the history, the interview and observations of the child, and possibly the narrow-band trauma scales that focus specifically on the traumatic event" (Reynolds & Kamphaus, 2002, p. 233).*

Such advice would be irrelevant for researchers intending to establish the very trends among groups referred to. This chapter focuses on child sexual abuse as an object of scientific research, with the issue of the clinical context and counsellor subjectivity being taken up in the next chapter.

Child sexual abuse as an object of scientific enquiry

What do quantitative approaches to studying child sexual abuse, including meta-analyses like the Rind et al. paper, and research papers deploying instruments of psychological measurement, attempt to achieve? In general, they claim that the effects of child sexual abuse are a proper object of scientific enquiry, and as such should be subject to the methods of scientific enquiry. This specifically entails *objectivity,* and a commitment to *consequentialism (or causality)*. The claim that research into child sexual abuse should be scientific is by no means unique to Rind et. al.. In a short paper published in the prestigious journal *Science*, a group of established sexual abuse researchers make a strong call for inter-disciplinary research into child sexual abuse to unify under the banner of science (Freyd et al., 2005). They lament that "much of the research on CSA has been plagued by nonrepresentative sampling, deficient controls, and *limited statistical power*" (p. 501, my emphasis), as well as the "unstated value judgements" that "infuse" the field.

Note that these are precisely the shortfalls that Rind et. al. identified and attempted to address. Yet the Freyd et. al. paper claims that CSA has long term and widespread harm despite their admission that there is a lack of scientific evidence to support this claim (that is the point of their paper). The call from Freyd et. al. is based on the assumption that such scientific research will better demonstrate the harm of sexual abuse. Yet Rind et al., using rigorously reviewed scientific (statistical) methods, claimed that

the evidence does not lead to this conclusion.[63] Despite their differences, both sets of authors agree that child sexual abuse is a proper object of scientific study; it has crossed the 'threshold of scientificity' (Foucault, 1970).

Latour (1993) described the sorting of *ideology* from *science* as a key feature of modernist knowledge, and the papers cited above seem to confirm Latour's vision of 'the modern' as "waiting for that dawn and thrilled to its promise" of dispelling the "traps of naturalization and scientific ideology" (p. 36). In other words, both sets of authors above maintain that the truth about the effects of child sexual abuse will be discovered by the gradual accumulation of scientifically attained information, with current efforts always building and improving upon the past. When this state of pure knowledge has been achieved, ideology and morality will be revealed for what it is and only the pure truth will remain.

What model of science?

The features that I have identified in the research texts (objectivity, causality) are used in those texts to establish the research on child sexual abuse as scientific. Taking that claim seriously, and in reference to the previous discussion of scientific measurement, I can draw on some of the recent literature in science studies to ask what kind of science this research is modelled on. In short, I will argue that these are features of a classical model of science that has in fact been usurped in the very fields of scientific research from

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[63] It is notable that the Freyd et al. (2005) paper did not contain reference to the Rind et al. (1998) study. It does not seem likely that the 2005 authors could have been unaware of the 1998 paper, given the controversy it generated.

which psychology originally adapted them. More recent reworkings of these classical concepts, particularly in the field of physics, may generate alternative understandings of psychological measurement.

It should be noted that other authors in the wider field of developmental psychology are more ambivalent about the extent to which this classical model of science 'fits' their discipline. (After all, developmental psychology, like any other construct, is not a unitary discipline). White and Pillemer (2005), for example, are quite explicit about the importance of *values* in developmental psychology work, and seek to embrace this aspect of the knowledge they produce. The commitment to causality in the child abuse literature is questioned by Miltenburg and Singer (2000)[64] for different reasons. As they point out, causal explanations of the harm caused by child abuse generally tend to explain negative or deleterious consequences. They propose that the reason for the general neglect, in the research literature, of how many abused children go on to live satisfactory lives and build close relationships, is that such achievements are based on moral agency (see also Mayne, 1993). Such acts and decisions are difficult to capture within an ontology committed to causality.

So what makes child sexual abuse research scientific? "In the strict sense of the term…the science of an object exists only if this object allows *measurement and causal explanation*, in short, analysis. Every science tends toward metrical determination through establishing constants or invariants" (Canguilhelm, 1978, p. 131). The strictest version of causality is determinism, that event *a* will

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[64] The paper refers to the study of all forms of child abuse, not only sexual abuse.

lead to event *b*. Scientific research, in this model, should uncover deterministic relations. When there is variability, classically-informed models of psychology put this down to the ignorance of the researcher or the inadequacy of the research tools, rather than any indeterminacy inherent in the phenomenon under study (Gigerenzer, 1987).

The notion of direct causality, a one-to-one linear model of a cause and effect, has been somewhat displaced, if not entirely replaced, by the notion of risk factors in the social sciences. The language of risk factors can be traced historically to a shift in scientific thinking from determinism to probability during the 19th century (Hacking, 2006). In this model of risk, child sexual abuse is understood as one of a number of potential factors that may contribute to poor outcomes. Thus Maniglio (2009) can state in his meta-analysis of child sexual abuse research that "…being a victim of child sexual abuse should be considered as a general, non-specific, risk factor for psychopathology, but not the only important one" (p. 655). Nonetheless, "further research should elucidate the causal mechanisms and processes that contribute to the adverse consequences associated with child sexual abuse" (*ibid*). This statement contains elements of a Laplacian idea about probability (Hacking, 1991); that although a one-to-one correlation between sexual abuse and any particular set of outcomes cannot be demonstrated, this may be due to the ignorance of researchers rather than any indeterminacy inherent in the phenomenon itself; there are still 'causal mechanisms' awaiting discovery.

This statement makes it clear that there is still a commitment to causality that is not ontologically very different from determinism. It is only that the number of causal factors (independent variables)

is now larger than one, requiring more sophisticated statistical techniques to analyse. The notion of causality is still important, and an 'individual factor' like child sexual abuse remains influential, however it must be placed in a network of other factors that all contribute to an overall outcome (along with a caution about the limitations of our knowledge). Within this network of factors, the influence of child sexual abuse can be calculated by comparison to the norm. For example, one group of researchers calculated that for individuals who were subjected to child sexual abuse, this accounted for "approximately" 7.83% of their contact with mental health services over their lifetime (Cutajar et al., 2010).

The tight connection between cause and effect is a requirement of scientific explanations, but Latour reminds of the reflexive nature of notions of causality. Scientific explanations are built up over time, through work, materials and associations. "Causes and effects are only a retrospective way of interpreting *events*" (Latour, 2005, p. 39. f.n. 30). The cause is posited as such after the effects have already happened; this is the work of scientific explanation. Adherence to this ontology harnesses the rhetorical power and persuasive authority of scientific psychology, even if it requires the elision of voluntary responses and decisions that individuals may make (Gergen, 1998).

A good example of Latour's point about the retrospective attribution of causes can be found in Matorin and Lynn's (1998) discussion of sexualised behaviour among children who have been sexually abused. They note the finding of other researchers that sexually abused children tend to display "…sexual preoccupations, compulsive sex play, frequent masturbation, precocious sexual

activity and knowledge, and aggressive sexual behavior" (p. 262), that adolescents and adults can also display troubling sexual behaviours, and they state that these behaviours are the "behavioral manifestations of traumatic sexualisation". Viewed from Latour's position, traumatic sexualisation has been posited after the fact as an explanation for the problematic behaviours. Where Matorin and Lynn argue that traumatic sexualisation causes the behaviour (and that traumatic sexualisation, in turn, is caused by sexual abuse), Latour's position would be that the very concept of traumatic sexualisation is formed by the behaviours.

The scientific research on child sexual abuse generally proceeds by defining particular psychological constructs (sometimes called traits) that previous clinical and research literature has identified as being effected by child sexual abuse. Rind et al. delineated 18 categories of such constructs from the studies they looked at: alcohol problems, anxiety, depression, dissociation, eating disorders, hostility, interpersonal sensitivity, locus of control, obsessive-compulsive symptomatology, paranoia, phobia, psychotic symptoms, self-esteem, sexual adjustment, social adjustment, somatization, suicidal ideation and behaviour, and wide adjustment (Rind et al., 1998, p. 28). Rind et al. were criticised for excluding a specific category of Post Traumatic Stress Disorder, commonly considered to be an effect of child sexual abuse (e.g. Dallam et al, 2001. However, these authors also noted that one reason that PTSD was not included in the Rind et al. meta-analysis was the lack of relevant studies with non-clinical populations). Other researchers generally include either PTSD or specifically identified symptoms of PTSD in their effects studies. For example, Paolucci et al. (2001), in their meta-analysis, used 6 categories: PTSD, depression, suicide, sexual promiscuity, victim-

perpetrator cycle, and academic performance. Another category of increasing concern amongst researchers has been developmentally inappropriate sexualised behaviour as a consequence of child sexual abuse (Friedrich, 1993; Friedrich et al., 2001; e.g. Chromy, 2003).

Warner notes the representational assumption inherent in attempts to scientifically measure psychological attributes and bracketing of critique this can entail:

> *Psychology also too often relies upon the idea of an objective world and psychologists' expert ability to decipher problems within it. Critique is avoided by restricting the terms of debate. It is not whether the problem exists, but how good is this test or this technique at measuring it" (Warner, 2009, p.25).*

The above list of measured psychological attributes[65] illustrates the assumption contained in meta-analyses that these characteristics are in fact measurable. However, this is not to say that the measuring instruments themselves go unquestioned. Questioning, critiquing, assessing and evaluating the applicability and validity of the measuring instrument to the task are standard scientific procedures. This is expressed in Schrodinger's caution to scientists about the use of measuring devices: "Any old playing around with an indicating instrument in the vicinity of another body, whereby at any old time one takes a reading, can hardly be called a measurement of that body" (Schrodinger, 1935, cited in Barad,

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[65] Of course, not all those listed are strictly psychological attributes (e.g. alcohol problems); however in the context of the research, problems in these areas are theoretically posited as symptoms of psychological traits.

2007, p. 281). It is routine for research papers to include a section entitled 'Measures' to describe the questionnaires or tools (instruments) used to assess the variables under investigation. These discussions frequently investigate the reliability and validity of the instruments used. Some literature focuses specifically on which psychometric instruments provide the best measurements of the traumatic impacts of child sexual abuse (Crouch et al., 1999; Gilbert, 2004). Others produce further related constructs as scientific objects that can be measured; for example, the degree of severity of sexual abuse itself can be treated as a measurable, independent variable to be statistically accounted for when assessing the harm of child sexual abuse (Loeb et al., 2011; Young et al., 2011).

When is a measure not a measure?

Interesting anomalies can be found on closer examination of the 'Measures' sections of published articles. A close reading may reveal that many so-called measures do not claim to do any measuring whatsoever. To choose a recent example from the *Journal of Child Sexual Abuse*: Deering and Mellor's (2011) 'Measures' section describes the *asking of open-ended questions* asking about the respondents' history of sexual abuse, asking respondents to *describe* the long-term impact of the abuse experiences on aspects of their functioning, and *inquiring* into aspects of family history. In fact, the very title of the article specifies this is a qualitative study. Similarly, a study into the psychophysiological impacts of child sexual abuse includes as a 'measure' the Early Trauma Interview, consisting of 'broad prompts' and 'open ended questions' (McDonagh-Coyle et al.,

2001). By what convention, then, is a discussion of *measures* deemed necessary or even accurate?

In the above instances I think it is reasonable to assume that the section title 'measures' is a convention of the journals the articles are published in. It could be argued that the term *measure* in this context is intended as a catch-all for any investigatory tool or instrument used in a study, and that I am missing the point by drawing attention to this. My point here is precisely that the term *measure* is used to describe procedures that clearly do not fall within any scientifically defensible definition of this term. The fact that the heading of 'measure' is a convention indicates the preferred style of investigative practice (Danziger, 1990). I suggest that even if the term is simply a poor choice of words, it implies some commitment to the epistemological requirements of measurement as a mode of scientific enquiry, even if the mode of enquiry is not technically measurement. The example of triangulation I mentioned earlier works here (Oakley, 2000). It is about a particular, objective relationship between knower and object. The use of standardised questions in an interview format is another example of structuring research practice around an ideal of objectivity (Gundlach, 2007).

But, back to the more clear-cut instances where measures are used with the intention to measure. As Michell has argued, the implication that psychology measures things is an important part of psychology's claim for scientific status (Michell, 1999, 2000). Michell (1999) would describe the current situation as attesting to the fact that whilst the *instrumental* tasks of quantification have been duly undertaken, the logically prior *scientific* task of

establishing that the measured objects are indeed quantitative is ignored.

Michell's concern can be explored by considering an instrument such as the Child Sexual Behavior Inventory (CSBI) (Friedrich et al., 2001). The CSBI, like many psychological instruments, is not a 'direct' measure of traits but a measure of behaviours and their frequency, based on the reports of parents on their child's behaviour. There is no metaphysical question involved in whether the frequency of certain behaviours can be counted by an observer (the reliability and accuracy of the observer is another question). Like the BASC and other similar psychological tests, however, the CSBI does not instruct anyone to precisely count anything. Rather, in the case of the BASC the reporter is asked to state the frequency of specific behaviours using a Likert scale, on points from 1 (never) to 5 (very often). The CSBI is more specific (e.g. less than once a month, 3 times a week).

Friedrich and colleagues found that "the CSBI-1 total score delineated sexually aggressive from physically aggressive and non-aggressive children" (Friedrich et al., 2001, p. 38). This should hardly be surprising, given that the CSBI is simply a checklist of sexual behaviours. In this claim, the CSBI only 'measures' behaviours, with no underlying assumption that these are related to another, un-observable attribute (i.e. the logic is that of behaviourism).

A more interesting claim for the CSBI is that it discriminates sexually abused children from non-sexually abused children. The behaviours are *theoretically* linked to the individual's attributes or traits. This is theoretically founded in Finkelhor & Browne's

notion of traumatic sexualisation as an outcome of sexual abuse (see Finkelhor, 1988). In this theory, children subjected to developmentally inappropriate, frightening, or otherwise distressing sexualised acts (i.e. sexual abuse) are said to suffer from traumatic sexualisation, which manifests in observable sexualised behaviour.

The issue is in the assumptions of the theoretical link between the sexual victimisation of the child and their subsequent sexualised behaviour. It is one thing to say that a child "Makes sexual sounds (sighs, moans, heavy breathing, etc.)" (an item on the CSBI- see the Appendix to Friedrich et al., 2001) less than once a month or more than 3 times a week. (There is the question of how the observer interprets exactly what constitutes a 'sexual sound'). It is two more things to say; 1- there is a normal amount that a child of certain age should exhibit this behaviour, and 2- a deviation from this norm is caused by the child's traumatized sexuality (or any other discernible psychological trait or attribute).

The kinds of research described above turn on establishing differences between sexually abused research participants and statistically normal populations, and using standardised techniques intended to achieve a modicum of objectivity of some kind. There is an implicit assertion of causal effects of child sexual abuse. Especially when conducted in statistical terms, this commitment to causality fails to account for individual agency of the research participants. These issues have been identified as problematic by researchers interested in making visible the harm of child sexual abuse, without wanting to invest in these ontological, epistemological and ethical commitments. In the remainder of this chapter, I re-iterate some of these alternatives and then try to

account for some of the problematisations these attempts produce in turn.

Developing alternatives

The production of normality renders the sexually abused child *abnormal* to the extent the sexual abuse has caused their development to be thrown off the normal and healthy course. This way of framing child sexual abuse has been strategically effective in establishing the unacceptability of adults using children for sexual purposes, and therefore it has obvious usefulness. However, this discourse of developmentalism has a number of limitations and unexpected consequences of its own, which may in turn be limiting to the lives of children and of adults abused as children. One response to this is to try and suggest a way of thinking about child sexual abuse that does not rely on the power/knowledge nexus in which developmental psychology is embedded. In doing so, the productive possibilities of developmentalism are concurrently articulated. That is, developmental psychology, like any body of knowledge that is invested in governing lives, produces as well as limits possibilities for the living of lives and the production of subjectivities.

The 'wrongness' of child sexual abuse (or 'adult-child sexual relations') [66] has been established within the paradigm of

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[66] The terms used to describe sexual interactions between children and adults (and older children and adolescents) are themselves constitutive of how such behaviour is understood. My preference will be for the term 'child sexual abuse' to describe any sexual interaction between children and adults (or significantly older children), as I think it keeps issues of power and exploitation as the focus. However in doing so I acknowledge that this term (child sexual abuse) is potentially problematic for a range of reasons: it forecloses on questions such as

developmental psychology[67], where measurement practices have played a pivotal part in establishing the parameters of the normal child (Burman, 1994) and the normal adult. If one follows the critique of normal I have been tracing here, if *normal* is regarded as a problematic, regulative ideal, one must respond in different ways to the question of why child sexual abuse is unacceptable or unethical.

In order to begin on this question, it might be helpful to consider that the clinical literature identifies both physical and psychological harm to sexually abused children. In both cases, a normal, healthy child is counter-posed to the sexually abused child, the former being characterised by its presumably unharmed psyche *and* body (e.g. Adams, 2011). In raising this, I intend to draw a fundamental analogy between the discursive constitution of the body, and the construction of the normal and sexually abused child through developmental psychology. This analogy draws on questions regarding the nature of the body, and the extent to which

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what do we mean by 'sexual interactions'?; what do we mean by 'child' and 'adult'?; how do we respond to children and young people who claim to have initiated and/or consented to sexual interactions? What are the consequences for children when they are identified as having been sexually abused? Despite these limitations, I will use the term strategically, as the available alternatives appear to minimize the operation of power. For example, "children who have experienced sex", (Joy, 2003), or children who have had "sexual encounters with adults" (Angelides, 2004, p. 143). Here I am influenced by Sharon Marcus's (1992) argument (although I may be misappropriating her position for a different purpose) that the languages we use to discuss sexual violence and rape are founded on "neither real nor objective criteria, but political decisions to exclude certain interpretations and to privilege others" (p. 387). I attempt to draw attention to these exclusions where possible.

[67] Of course, the field of developmental psychology does not have exclusive claims to the problematisation of child sexual abuse. Feminist activism, the law, as well as broader discourses regarding the family and the innocence of children can all be identified as discursive operants constituting the problem of child sexual abuse. My focus here, however, is on the contribution of developmental psychology as the premier mode of understanding child sexual abuse within therapeutic practices, acknowledging that it operates in conjunction with these other knowledges.

all bodies are discursively produced and maintained. This is the subject of long-standing debate, especially amongst contemporary feminist academics. To pre-empt my argument slightly, I suspect that the question of whether or not the harm of sexual abuse is solely 'discursive', as opposed to 'real', is ultimately a misleading one, to the extent that children's bodies *and* psychological interiority are always already produced as objects of scientific (medical, psychological) discourse. If this is the case (as I will argue), then the harm associated with sexual abuse can only be said to act upon a discursively constituted body; it follows that the harm itself can only act 'through' discourse. That is to say, if a body is made up of what is said, written and thought about it, and through the practices incited as an enactment of these discourses, then any harm to that body must also be produced through things said, written and thought, and the consequent practices[68].

The experience of 'pain' is a site of similar contestation. According to the International Association for the Study of Pain, one defining part of pain is that "the attribution of meaning to the unpleasant sensory events is an intrinsic part of the experience of pain" (cited in Ahmed, 2004, p.23). Professional understanding of (even physical) pain and suffering is historically constituted. For example, surgeons in 19thC America had very different definitions

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[68] Of course this is not to suggest that bodies would not exist if there was no discourse. Nor, that the physical effects of child sexual abuse, where present, such as bruising or tearing, only exist because we talk or write about them. But, what we say and write about them does play a formative part in the meaning that is made of them. Taking the example of bruising; bruises which appear identical will be thought of and responded to differently depending on the explanation given for its appearance and the context in which the explanation is given. A bruise on the arm of a 10 year old boy forcibly held down by an adult in an act of sexual aggression is different from the 'same' bruise acquired through a game of football. The latter may not even be considered an injury, whilst the former may take on medical, legal/forensic and psychological significance. The physical injuries of sexual abuse take on their significance via discourse.

to their modern day counterparts of what constituted 'minor' surgery when considering when and to whom to administer anaesthetics. Their understandings were not based on a primitive brutalism (no matter how incomprehensible their views may appear to modern sensibilities), but on careful considerations befitting the professional climate of their time. This included debates over if, how and what kind of pain could be experienced by children, and what the consequences of this might be for the child (Pernick, 1985). Some felt that children actually "lacked the mental capacity to suffer" (Pernick, 1985, p. 150), while others believed children to be especially sensitive to pain. However, this is most definitely not to say that the pain, the suffering or the harm is not 'real'- there is no necessary binary opposition between the notions of discourse and material reality (Hall, 2001), and to say that something is the product of social/historical construction is not to say that it exists only in imagination (Butler, 1993; Hacking, 1999; Butler, 1999).

This presumed binarism opposing a pre-discursive reality (or body) to a socially or historically constituted one is related to long-standing Western understandings of free-will versus determinism. Philosophical positions that emphasise the social construction of reality are often associated with determinism, whilst those positions that privilege the free rational individual (i.e. pre-discursive, ahistorical) subject are seen to be aligned with free will. However I follow Sawicki's (1994, see also Hacking, 1999) argument that the process of social construction is not necessarily deterministic. I will use the phenomenon of gender as an example, as it seems particularly germane to the issue of child sexual abuse.

Judith Butler's *Gender Trouble* (1999, first edition published 1990) is probably the best-known contemporary feminist critique of the Western philosophical debates over the extent to which gender is socially produced by discourse or reflective of deeper, underlying characteristics of individuals and their differences (see also Grosz, 1994 for a summary of these debates, and also Butler (1993), for a more focussed discussion of the relation between discourse and the materiality of bodies). Directly addressing the feminist antagonism between post-structuralism and 'the body', Butler summarises the questions/warnings of concern: "If everything is discourse, what happens to the body? If everything is a text, what about violence and bodily injury? Does anything *matter* in or for poststructuralism?" (Butler, 1993, p. 28). In general terms, Butler states that feminism requires a grounding of some kind in the 'sexed specificity of the female body', even if this is constructed and is already inscribed as gender.

> *We may seek to return to matter as prior to discourse*
> *to ground our claims about sexual difference only to*
> *discover that matter is fully sedimented with discourses*
> *on sex and sexuality that prefigure and constrain the*
> *uses to which that term can be put. Moreover, we may*
> *seek recourse to matter in order to ground or to verify*
> *a set of injuries or violations only to find that matter*
> *itself is founded through a set of violations, ones which*
> *are unwittingly repeated in the contemporary*
> *invocation (Butler, 1993, p. 29)*

In other words, one cannot state unproblematically that sexual violence causes harm to bodies, without considering the extent to

which bodies do not simply exist as matter, but are experienced, configured and constituted through discourse[69].

Butler makes the radical argument that not only gender, but biological sex differences themselves are produced by discourses about the body. Medical, legal and psychological discourses have been particularly influential in this achievement. By reifying certain capabilities of some bodies (e.g. the capacity to conceive and give birth), and emphasising links and connections between certain groups of body organs and parts, a particular understanding of bodies as fundamentally 'sexed' is achieved. Monique Wittig, feminist lesbian philosopher, once stated in response to a question in a seminar, that she did not have a vagina (cited in Butler, 1999, p. 201 footnote 54). This surprising response makes sense in the light of this understanding of bodies as being produced and 'sexed' through discourse. Wittig's statement stems from the argument that particular parts of women's bodies have been linguistically 'carved up' and grouped together to form the organ known as the vagina, but there is nothing naturalistic or necessary about this grouping or assemblage of the body's capabilities. This is not dissimilar to Deleuze and Guattari's claim regarding the "originary theft" of the girl's body into an assemblage of passive womanhood, "…achieved through the transcendental organization of her organs into a functional form, a receptacle for male desire and progeny" (Gatens, 2000, p. 68). These types of formulations can be seen as

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[69] A similar tension exists in the psychoanalytic literature on trauma and the notion of 'splitting'. Briefly, some kind of splitting of consciousness is seen to be necessary as the condition of subjectivity; the primary address by the Other is both overwhelming and necessary (Butler, 2005) Individuals who experience profound trauma are also thought to undergo a further split, in order to fabricate a kind of space in consciousness where one can still function as a coherent subject. The tension, then, is around what kinds of differences there might be in these two traumas, and why one is necessary and the other problematic (or why they might be both simultaneously). See (Leys, 2000)

belonging to a Nietzschean tradition of understanding the relationship between knowledge and the things that are known: "…there can be no relation of natural continuity between knowledge and the things that knowledge must know. There can only be a relation of violence, domination, power, and force, a relation of violation. Knowledge can only be a violation of the things to be known, and not a perception, a recognition, an identification of or with those things" (Foucault, 2000, p. 9).

However 'radical' this may seem, Butler goes on to argue that Wittig's view still presupposes a pre-discursive body, upon which violence is done by language and other discursive practices (including sexual violence, which is an extreme act of enforcing the category of 'sex'). This is what Foucault (1978) characterised as a juridical model of power, the 'repressive hypothesis' in *History of Sexuality vol. 1*. In a juridical model of power, 'sex' or sexuality would be seen as an entity that pre-exists the operation of power through discourse (which includes both text and practices). In the juridical model this pre-discursive sexuality is repressed through acts of power which restrict its true expression. There are several examples of Butler's point in Wittig's theoretical and political writings. For instance: "Language casts sheaves of reality upon the social body, stamping it and violently shaping it. For example, the bodies of social actors are fashioned by abstract language (as well as by nonabstract languages). For there is a plasticity of the real to language" (Wittig, 1992, pp. 43-44). The key terms here are the images of 'stamping and violently shaping it', implying a pre-discursive body upon which discourse acts, as opposed to the constitution of bodies of which Butler writes.

Foucault offered an alternative understanding of power as operating discursively and productively, a 'discursive' model of power. Bodies, in this discursive model of power, only 'exist' to the extent that they are produced through discursive practices. Bodies are not repressed or mutilated by discourse, but actually produced. (In this sense, power is productive, not repressive). Butler would argue that the constitution of bodies is not just gendered, but *dependent upon gender*. "The mark of gender appears to 'qualify' bodies as human bodies; the moment in which an infant becomes humanized is when the question, "is it a boy or a girl?" is answered" (Butler, 1999, p. 142). The practice of gender assignment of ambiguous babies illustrates this; in fact it is often presented as an act of compassion because it bestows on the child a definite sex/gender, and therefore humanity. In a similar argument, one that Butler attributes to Wittig, sexual abuse is a violent practice of gendering; it enforces the categories of sex and gender. In an awful process of simultaneous annihilation and affirmation, then, the sexual abuse of girls both confirms their status as female, and via this 'legitimate' gendered status, as human. For boy children, sexual victimization sets in place a doubting of their own subject position as masculine. The clinical literature on boy victims provides many examples of how sexual victimization disrupts their gendered identity. In turn, men and boys are often quoted as saying they feel they 'are nothing', as if by taking away gender, so too is humanity itself forsaken. It is perhaps not necessarily that masculinity per se has been called into doubt, but the possession of a sex/gender at all.

Discourse, bodies and children

Although there are important points in Butler and Wittig's arguments that emphasize the gendered nature of the discursive violence inflicted upon women's bodies, and the particular ways in which lesbians (in Wittig's work) disrupt the binary gender division which the 'heterosexual matrix' reproduces, the framework is also a productive one for thinking about children. I have already referred to Butler's argument that children do not 'become human' until they are assigned a gender. I would like to suggest there is a kind of analogy of the argument when looking at the clinical and research literature about sexually abused children. The documentation in the psychological and social work literature of the harm caused by sexual abuse relies on this view of children as being harmed by practices of adult power, not only in regards to their sexuality but their psychological, social and physical 'well-being' generally. If, in this literature, sexual abuse of a child is understood as an act of violence inflicted on a child, then the consequences of this violence are framed as a thwarting of the healthy development of the child. Of course it does not necessarily follow that the child would have developed 'normally', but I think this is the effect of comparison to the statistical norm. The child is restrained from expressing its 'true self'. Thus, just as for Wittig language and other discursive practices inflict violence upon women's (pre-discursive) bodies which result in their disfiguration from their 'true' nature, so too are children restrained from expressing their true nature as a consequence of sexual abuse. In both cases, language and abuse are practices of power understood in terms of the extent to which they thwart the true and 'free' expression of the individual.

Sexual abuse sets the abused child apart from the normal child. This normal child is sometimes especially apparent when it is used in contrast with versions of childhood against which one objects. A recent example may help clarify this point. An Australian study into representations of children in corporate advertising material argued that many depictions of children are overly 'sexualised' and as such incite both adults and children themselves to understand children as sexual beings in a manner that is usually associated with adult sexuality (Rush et al., 2006). Of the many interesting and worthwhile points raised in the research, my interest here is on the way the normal child (or more accurately, a normal childhood) is used to problematise the childhood depicted in the advertising material. The normal child can have exclusionary effects on the sexually abused child.

A discursive model of power regarding childhood would hold that the child (and for that matter, childhood sexuality) is already constructed through discourse, even to the extent that the notion of 'childhood' is a social achievement (Ariés, 1973). That is to say, childhood itself is a relatively recent historical category of being human; the historical emergence of our productions of childhood can be documented. As a product or fabrication of construction in this way, 'the child' is already imbued with relations of power that are productive, not just oppressive. For example, the documentation produced by psychological, medical and legal discourses work together to produce a 'normal' child (Rose, 1999). This works to fabricate a particular understanding and way of relating to individuals constituted as 'children', in turn inciting particular ways in which they experience themselves and their own capacities, limitations, obligations and so on (i.e. their subjectivity).

Radicalising further Beauvoir's famous statement that one is not born, but becomes, a woman, Butler argues that one is always in the process of becoming a sex/gender. Gender is not a state to be achieved but "…an incessant and repeated action of some sort" (Butler, 1999, p. 143). As described by Butler, Wittig argued that 'sex' is always gendered, to the extent that only women can be said to be 'sexed', in relation to the male who is not sexed. Contemporary work on masculinity has argued convincingly that masculinities are diverse and the result of a set of ongoing gendered practices. That is, there is not one masculinity that can be achieved and secured, but a multiplicity of masculinities that are always performed through specific actions in localised contexts. The concept of 'hegemonic masculinity' (Connell, 1987) is an influential explanatory framework in understanding how men as a social group achieve and maintain power through the reproduction of oppressive gender identities.

Nonetheless, the language that some children use to discuss sexual abuse is illuminating here. Some children use the term "sexed" as a verb, as in 'he sexed me'[70]. In this sense, and in keeping with Wittig's argument, the child who is sexed (i.e. sexually abused) is made female within the modality of heterosexuality, whether the child in question is considered a boy or a girl. But such identifications are performative, neither artificial nor fully stable. 'For the imperative to be or get "sexed" requires a differentiated production and regulation of masculine and feminine identification that does not fully hold and cannot be fully exhaustive' (Butler, 1993, pp. 187-188).

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[70] Here I am citing my own clinical experience.

So far I have been struggling with the question of how to understand the discourse surrounding the harmful effects of sexual abuse of children, and trying to disrupt some of the taken for granted psychological knowledges of development, and naturalised assumptions about children's bodies. The problem with these modes of thinking the harm done by child sexual abuse is in the extent to which they posit a normative model of being for individual children, a normativity which I have been arguing is a power-laden means for governance of children in general; but especially the sexually abused child. One of the philosophical assumptions underlying these questions is the Cartesian mind/body dualism. The debates have been essentially staked on the material difference between thought and body; that what is thought about bodies affects bodies, bodies which are of a different kind of substance or essence to that of thought.

Some feminist philosophers, when faced with this question, have turned to the philosopher Spinoza (often through Deleuze). Spinoza's concept of 'monism' refers to the "fundamental assumption…of an absolute and infinite substance, singular in both kind and number. If substance is infinite and nondivisible, it cannot be identified with or reduced to finite substances or things. Finite things are not substances but are modifications or affections of the one substance, modes or specifications of substance" (Grosz, 1994, p. 10). That is to say that reason, mind, body, thought, feeling, or matter (human or non-human) are not different substances, but are expressions of the one substance, referred to by Spinoza as simultaneously God and nature (Gatens, 2000)[71].

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[71] Spinoza's references to God are generally thought to be referring to nature; it was not really possible to exclude God altogether in the political climate of Spinoza's time (personal communication, Ron Frey).

Spinoza offered a novel way of thinking the body that was not reliant on the mind/body dualism of Cartesian philosophy. For him a body can be defined not in terms of what it *is*, or what makes it, or its essence; but, put simply, in terms of what it can *do*. The body, according to Spinoza, is defined not by a pre-determined set of limits imposed by nature, or the inherent properties of the body itself (which are after all of the one substance as all other things), but in terms of its possibilities of connections with other bodies. The limits of such possibilities of human bodies are not known, indeed may not be capable of being known. Deleuze and Guattari wrote of these connections (or 'assemblages') as forming machines, emphasizing what they can achieve (1983). Therefore one may speak of writing machines, for example, and especially desiring machines.

Elaborating on this conception of bodies, Gatens offers an idea of how harm might be defined in this ontological framework:

> *The human body is permanently open to its surroundings and can be composed, recomposed or decomposed by other bodies. From the perspective of modal existence, such encounters with other bodies are conceived as good or bad depending on whether they aid or harm our characteristic constitution…Human freedom…amounts to the power that one possesses actively to select one's encounters rather than always being the plaything of chance associations (Gatens, 2000, p. 61).*

Connections and relations with other bodies are judged according
to whether they enhance or diminish the potential for the
realisation of possibilities. Relations dependant on "order-words"
(commands, demands, insistences) (Gatens, 2000) diminish such
possibilities because they insist upon the acceptance of one
possible situation as *the actual* situation. Gatens specifies rape as
an instance of the operation of "order-words". The dynamics of
child sexual abuse is another example of such a diminishing
situation.

Psychological assessment as a radical practice?

Having examined some of the alternatives to what I have called
scientific psychology as means for making sense of the effects of
child sexual abuse, I want to attempt to continue the diffractive
reading and read these points of productivity through each other. I
am not aiming to start from the position that one is more truthful or
liberatory than the other, but to acknowledge both as influential
forces in the production of this unstable scientific object: the harm
of child sexual abuse.

One of the key points made by Wittig is that the violence that
shapes and naturalises the social experiences of sex and gender (or
the workings of subjection in general), is hidden by these very
effects. If sex and gender are made to appear as natural
social/biological/cultural entities, then it is not thought or made
conscious that these entities were/are violently forged. Returning
to the analogy made earlier, that knowledge and sexual abuse are
both forms of violence, I am reminded of the historical and
contemporary silences that surround the sexual abuse of children.

As Liz Kelly pointed out, "It simply is not the case that the 'damage discourse' has always been dominant. In the late seventies and early eighties, both researchers and practitioners were struggling against an orthodoxy which asserted that abuse was not damaging" (Mayne, 1993, p. 19). Russell argued that "Feminist researchers have focused on investigating the destructive consequences of child sexual abuse… because there has been so much sexist research which has both minimised the effects and accused the child of asking for it by her so-called seductive behaviour" (Russell, 1991, p. 53)

An opening here appears for a radical interpretation of clinical descriptions of the effects of sexual abuse. If (for example) the 'unruly' behaviour of a child, or the difficulties in life experienced by an adult, can be said to be an effect of violence inflicted upon them, rather than being a trait inherent in themselves, this presents a potential exposé of the violence that has been inflicted. So the BASC clinician's guide states that, when assessing children who have experienced traumatic events, that "…they often show aggression, overactivity, and even rule-breaking behavior as they act out the intense feelings experienced in response to the traumatic life event(s)" (Reynolds and Kamphaus, 2002, p. 233). As the BASC measures behaviours that are linked through psychological theory (specifically, through Post-Traumatic Stress Disorder) to constructs such as aggression, anxiety, etc., it 'shows' the effects of the trauma, enabling the traumatic event(s) themselves to be revealed through further clinical investigation.

In light of Wittig's position regarding the 'covering up' of discursive violence in relation to its effects, there is an argument to be made that psychological statistics function as a radical

'showing' of the violence of child sexual abuse. The Foucauldian literature tends to focus on the power/knowledge matrix produced by psychological discourse, positioning the 'psy' professions as a kind of dominant practice governing the lives of individuals, especially children (Rose, 1998, 1999). I do not object to this analysis. However, a different analysis is made available if, following Wittig's general observation that sexual violence is a radical enforcing of sex/gender that obscures its own violence, I would consider sexual abuse to be among the practices that most powerfully produces these power/knowledge effects. Sexual abuse produces effects, the silence surrounding the abuse naturalises the effects and obscures the violence. In this light it is possible to see psychological statistics as a counter-discourse, by exposing this violence through measuring its effects. The difficulty with this line of argument is that the 'showing' depends upon the kind of normative identity that Wittig herself argued is a product of violence.

I would suggest that the clinical literature goes, in a sense, 'half-way' to this end of making visible the harm. The trauma of sexual abuse may be identified as a cause of the problem; but the traits are then reconfigured as those of the child, who (with their parents and the expertise of the counsellor) become morally charged with correcting the deficiencies. This is what Louise Armstrong was identifying when she wrote *Rocking the cradle of sexual politics* (1996). A clear contemporary example of this incitement to responsibilization of abused boys especially can be found in the Victorian Health Promotion Foundation's violence prevention document *Preventing violence before it occurs* (Victorian Health Promotion Foundation, 2007). In that document, statistical evidence is drawn on to suggest that boys who have been abused

are at higher risk of committing violence against women in the future, and their parents are encouraged to access therapeutic services to prevent this from occurring.

The language of 'measurable symptoms" (e.g. Crouch et al., 1999) produces this disciplinary function. It is not uncommon for researchers to call for improved tools for measuring the impacts of child sexual abuse. For example, "most child maltreatment researchers agree that work in this area has been hampered by inadequate instrumentation for measuring relevant variables" (Crouch et al., 1999, p. 255). In wanting to identify the harms caused by child sexual abuse, one kind of power, or practice of governance, gets superseded with another; relations of power are both discontinuous and continuous. It might seem incongruous to propose any kind of similarity between the power involved in child sexual abuse and the disciplinary function of psychological research. However, I argue that each exercises power in relation to the child, or adult who was abused as a child, in its own way. In the psychological research, at stake is the identification of what kind and 'how much' harm is caused by child sexual abuse, measured in relation to the normal child (Friedrich et al., 2001). It is the very operation of power that encourages the therapeutic normalization of sexually abused children in the interest of their and their community's well-being.

While sexual abuse is clearly an oppressive and diminishing experience, it does not follow that psychological measurement is a liberatory practice. Both can be understood as means of governing the individual child (in the sense that they produce possible subjectivities while excluding others). Bell (1993) addresses this issue in the context of a discussion on feminist knowledges about

'incest'. Is feminist knowledge of child sexual abuse ('incest') itself a disciplinary discourse? In reference to Foucault's analysis of the deployment of sexuality within families as a site of alliance, Bell states that…

> [t]he statistics that have and are being produced due to feminist investigation of the issue of incestuous abuse can be understood as a bio-political knowledge. At first blush, therefore, it may seem that feminism is part of the deployment of sexuality. But is feminist knowledge of incest a knowledge that works within the power/knowledge networks of the deployment of sexuality? And if it does, does that mean that feminism, paradoxically, is part of a discursive incitement to incest? (Bell, 1993, pp.100-101)

This comes in the context of a discussion, based on Foucault's *The history of sexuality: Vol. 1* (1976), of the two systems of power deployed around incest; alliance within the family, and the deployment of sexuality. Incest is at the intersections of these two strategies. Bell identifies two aspects of feminist work on incest that could be considered "mechanisms and effects of the deployment of sexuality" (p.101). These are 'the confession', and the "disciplinary power which feminism has arguably set in place both within and around the family" (p101). Unfortunately Bell does not go on to discuss the feminist statistics specifically in further detail, but her point is germane for my purpose here, to explore psychology as practice of governance. I will also return to the notion of the confession, for as Armstrong (1986) points out, "all elements of human measurement, including the survey, the questionnaire and the interview seek to capture the essence of the

confession" (cited in Bowman, 2005, p. 118), thus contributing to mechanisms of surveillance.

Scott (2001) argues that an account of feminist knowledge of sexual abuse as simply emerging from personal testimonies of abuse incorporated into an analysis of patriarchal society, thus constituting a subjugated knowledge, is incomplete. Thus, while some have characterised feminist knowledge of child sexual abuse as below the threshold of scientificity— "Such knowledges are not 'discourses' for they have little authority, no institutional backing and none of the accepted forms such as statistics, clinical instruments or case studies which legitimate the unions of power/knowledge (Scott, 2001, p. 357). Scott reminds us that in fact "…feminists have not simply listened to, and retold, survivors' stories. We have analysed, edited, counted, listed and in countless other ways 'worked up' 'subjugated stories' into 'evidence', 'theory' and 'professional practice'" (ibid, p. 359). Feminist knowledge of child sexual abuse indeed holds considerable authority, institutional backing[72] and produces statistics. "The problematic assumption here is that feminists are by definition located outside the disciplinary professions" (p. 357)

This has not only historical but also methodological stakes for my project. Referring to Barad's practice of 'diffractive reading' ("reading insights through one another in attending to and responding to the details and specificities of relations of difference and how they matter" (Barad, 2007, p. 71)) requires first that feminist and scientific-psychological ('psy') insights are in fact

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[72] For example, the Victorian CASA standards of practice for working with survivors of sexual abuse explicitly indicate the need for a feminist understanding of gendered power dynamics.

distinct. I want to assess the idea that feminist and psy knowledges are potential systems of knowledge production, each definable by a set of distinct and specific conditions (e.g. assumptions, standards of evidence, technologies and practices of research, etc.). Barad would not suggest that each system is radically separate, but rather that they are entangled though specific material arrangements (notably, their shared production of child sexual abuse as an object of knowledge). Nonetheless, for a diffractive reading to be possible, there must be at least some sense of the systems being 'different'. If they are not, I find myself in the Foucauldian trap of having no contrasting epistemological standpoint from which to view any knowledge claims—feminist and psy knowledges, if they are indistinct, can only analyse knowledge from within a closed and self-referential 'knowledge-power' nexus.

Sexual abuse and liberal subjectivity

A further angle on the recognition of the subject 'sexually abused child' can be taken from the work of theorists of recognition and subjectivity. Wendy Brown argues that attempts to ensure rights for identified groups of subjects through legislative and disciplinary discourse (e.g. based on race, religion, culture, gender, sexuality, ability) also work to reify, to solidify, to 'empiricize' identity, "as if their existence were intrinsic and factual, rather than effects of discourse and institutional power…the language of recognition becomes the language of unfreedom, how articulation in language, in the context of liberal and disciplinary discourse, becomes a vehicle of subordination through individualization, normalization, and regulation, even as it strives to produce visibility and acceptance" (Brown, 1995, p. 66).

This parallels the paradox identified by certain political and psychoanalytic thinkers (explored at length in Butler, 1997a, 2005), which can be summarised as follows. As subjects, we are required to conform to a minimal set of social and linguistic norms in order to be recognized by, and to recognize, other subjects as legitimate. While these norms limit and constrain possibilities for how to be, they are the necessary ground on which subjectivity is possible at all. Or rather, they are means through which the (re)iteration of identity and subject positions are per-formed. Thus it is not possible to 'free' the subject by abandoning normativity per se; this would mean the dissolution of the subject entirely. Within liberal rights discourse, potentially political abuses of power become "matters of individualized, dehistoricized injury and entitlement, into matters into which *there is no harm if there is no agent and no tangibly violated subject*" (Brown, 1995, p. 124, my emphasis).

From this, it could be argued that the statistical configuring of the harm of child sexual abuse, through the measurement/production of child sexual abuse victims, is the attempt of a scientific psychology to confer legitimacy on such a subject. This also points to the inadequacy of a protest against the use of numbers and statistics in research. For example, at a seminar on "Narrative methods in research", a speaker righteously proclaimed that "whenever you reduce a person to a number or statistic, you reduce a person to a number or a statistic" (Chamberlain, 2008). The implication being, that it is better to describe the person using words, preferably those uttered by the 'research subject' themselves. However, in the context of the work of Brown and Butler (and others), the equation of statistics with untruth and words with truth does not bear up under scrutiny; both are the

material of disciplinary power. But as such, they also play a part in the circuitry of recognition. "Language, essentially, objectifies experience and, in so doing, enables it to be shared" (Le Riche, 1998, pp. 29-30). Numbers and statistics hold out the same promise of shareable, communicable knowledge, on a universal scale. If psychological measurement (as a practice of disciplinary power) individualizes and normalizes, it says: "Here is a place for you, a place in which you are recognizable as a subject, and a place which locates you amongst other individuals by comparing you with them". The price of recognition is a normalizing judgement in a field of hierarchization, but (notwithstanding some of the critiques of quantification), language also involves the flow of power and plays of hierarchy (Le Riche, 1998).

Summary of Chapter 6

In this chapter, I began by developing an account of how the effects of child sexual abuse are produced as objects of scientific enquiry, through modes of research that privilege causality and objectivity as ontological and epistemological commitments. I then put these ways of knowing 'into play' with some critical alternatives that call into question these ethico-onto-epistemic (Barad, 2007) positions. I tried to retain my own commitment to a diffractive reading, which is interested not in the truth or falsity of either set of claims, but the effects they have in producing the effects of child sexual abuse as objects of enquiry. I suggested that read in interaction with each other, it is possible to regard psychological measurement as a more helpful practice than otherwise might be possible. By helpful, I mean it makes visible the harm of child sexual abuse, harm which may otherwise remain

naturalised. At the same time, it also individualises this harm as a property of the abused person, inciting such persons to access therapeutic treatment. This in turn raises further problems in regards to disciplinary power, and the extent to which this is both restrictive and productive.

In the next chapter I intend to explore how practices of objectivity entailed in psychological measurement can govern the activities of counsellors in their therapeutic work.

Chapter 7- Practices of objectivity for researchers and counsellors

But we should not think of these practices that make individuals calculable purely as technologies of domination, for they can also be technologies of autonomization and responsibilization. Numbers, and the techniques of calculation in terms of numbers, have a role in subjectification- they turn the individual into a calculating self endowed with a range of ways of thinking about, calculating about, predicting and judging their own activities and those of others (Rose, 1999, p. 214)

If numbers are about "objectivity", then where does this leave subjectivity? (Oakley, 2000, p. 102)

In this chapter I will pay greater attention to the subject positions that are possible for researchers and counsellors. The theme of objectivity will be prominent here, considered through the lens of an analysis of subjectivization and 'technologies of the self' or 'care of the self' (Foucault, 1988a, 2005). I will argue that practices of psychological measurement can be studied in reference to techniques of the self: what type of self is a condition of conducting psychological measurement? Further to this, any practices which are formulated in resistance to psychological measurement as a therapeutic activity or research tool constitute technologies of the self in their own right.

Rind et al. (1998, 2000) state repeatedly their view that qualitative reviews of child sexual abuse research are problematic because they are subjective and imprecise, and that quantitative reviews have the advantage of "reduced subjectivity, and increased precision" (*ibid*., p. 42). This accords with most accounts of both measurement and statistical methods in general; they are valuable for their objectivity and their precision. Meta-analyses make particularly strong (yet contestable) claims to scientific objectivity (Shercliffe et al., 2009). I have already noted Michell's objection to the precision assumption; if psychological attributes are not quantitative in nature, then investigating and/or representing them using quantitative methods is in fact imprecise (Michell, 1999). I have also mentioned the critique of scientific objectivity that states that the belief in a 'God's eye view' actually leads to a less, rather than more, complete understanding than approaches which take the investigators own subject-position and the 'entanglements' of the research situation into account (Harding, 1986; Barad, 2007; Daston et al., 2007). Such views demand a blurring of the objective/subjective opposition and call into question the 'cut' between objects and subjects of knowledge.

These debates aside, the review of clinical and research literature and the focus groups provides empirical evidence that practices of objectivity and precision can be influential in terms of the 'technologies of self' of researchers and counsellors in their work with sexually abused children. But saying that objectivity is influential is not to say what objectivity is, or that it is ever actualised in practice. I need to look more closely at the statement: Objectivity is way of conducting oneself.

Technologies of the self

Although Foucault's concerns were quite different in his development of the notion of technologies of the self, I suggest it is a useful frame for my purposes here. In this section, I will discuss the idea of technologies of the self, to provide a conceptual context for the notion that objectivity can function as a way of conducting oneself as a counsellor or researcher in the field of child sexual abuse. Objectivity is an important practice to investigate because of its privileged position in instructions and claims for the conduct of psychological measurements.

"Let's say very briefly that through studying madness and psychiatry, crime and punishment, I have tried to show how we have indirectly constituted ourselves through the exclusion of some others: criminals, mad people, and so on. And now my present work deals with the question: how did we directly constitute our identity through some ethical techniques of the self which developed through antiquity down to now?" (Foucault, 1988b, p. 146)

Foucault's later works marked a shift in emphasis from the disciplinary apparatus of institutions such as asylums and prisons, towards an examination of the means through which individuals actively engaged in the constitution of their 'self' (Hutton, 1988; McNay, 1992; Paras, 2006). In *The hermeneutics of the subject,* Foucault (2005) traces the practices of care of the self from early Greek philosophy (in Plato), through to their transformations in later Christian traditions. There are a few themes I want to draw upon for the current research. Firstly, although the concern for the self does not remain constant in its meanings, methods or

objectives over different historical periods, nonetheless the theme of caring for the self emerges in different forms across societies. In the Socratic dialogues, for example, care for the self was a concern for the young and privileged male youth who were in a position to harbour ambitions in public life and politics. Caring for the self was a kind of training to prepare for these responsibilities of governing. In contrast, around the first and second centuries A.D., this concern for the self had expanded its scope both in terms of the range of the population to whom it applied (it became a more widespread or democratised cultural phenomenon), as well as shifting emphasis from youth to more of a life-long process directed towards old-age.

Some of these technologies of the self, such as verbalization, "have been reinserted in a different context by the so-called human sciences" (Foucault, 1988a, p. 49). Whereas the practice of confession, for example, was used in some Christian traditions for the purpose of renunciation of the self, in the human sciences such techniques of verbalization affect an active constitution of the self. In therapy around child sexual abuse, talking about the abuse may be seen as necessary to gain insight into how it has impacted on the self; that is, how the self has been shaped by the abuse. The therapeutic project then is to understand this impact and undertake a new or preferable shaping of the self. Counsellors informed by an understanding of the normative potential of confessional practices may also contest this imperative to talk about the abuse, for example by acknowledging that silence may be a way for clients to resist the intrusion of unwanted therapeutic intervention in their lives (Warner, 2009).

Hutton (1988) spells out the historical relevance of these ancient and Classical practices of the self for contemporary 'psy' professions:

> *Like the therapists of our own society, these earlier practitioners regarded the care of the self as a serious and salutary pursuit, even if they expressed their commitment to it in an ethical or religious rather than a medical vocabulary. The question that Foucault asks of Freud is why he seeks to discover the truth about the self through these techniques, whereas his predecessors had been content to search for a method of self-care (p. 135).*

Indeed, in *Hermeneutics of the subject*, Foucault (2005) argues that within the dominant Western tradition of philosophy, the principal of 'know thyself' has taken prominence, but that in the earlier practices of the self, this principal was secondary to the wider edict to 'care for the self' (effectively, to care for one's soul). That is, the ethical reason for knowing oneself was so that one could better undertake the task of caring for one's soul, rather than discover the truth (of oneself, of humanity, of the world). The kind of self that is concerned primarily with caring for the soul may be a different kind to the one concerned with truth.

This does not sound too distant from the rationale that drives psychological and social work research; we need to know about the problem of child sexual abuse in order to better care for abused children. Yet the relationship between ethics and science, between care and knowledge, in this field problematises such a straight-

forward understanding. In light of this, it is interesting to note the comment made by Elizabeth in the focus group:

> ELIZABETH   But I think in response to your question [to JASON], you know, what is the therapeutic point [of administering standardized psychological tests] for the child, I'd say none.
>
> CB In doing tests?
>
> ELIZABETH   Yep. I would say it's to assist the therapist. I think parents might get, feel a bit more settled or something about hearing well this test says that you, you know, your child's emotionally and behaviourally going along OK,
>
> KATE   Hmm, hmm
>
> ELIZABETH   So you know, you can have parents out there that search for a diag…not even a diag…, yeah well a diagnosis, but you know, just to, that might help settle some parental anxieties. But I would say therapeutically there's probably none for the child.

"Therapeutically there's probably none [benefit] for the child". There is a practice of knowledge occurring (the psychological test), and Elizabeth suggest it may have some value for the therapist, and perhaps for the parents. I am presenting this as evidence for my argument that the use of psychological testing can be thought of as a 'practice of the self' for counsellors.

I suggest that contemporary practices of the self within therapeutic and research milieux contain evidence of ethical *and* medical-scientific discourses. The texts I examine and the counsellors I spoke with constitute the self via the deployment of both a truth about *and* a care of the self; at least that is what I contend. In addition, the discourses of knowledge and care through which the counsellors constitute their clients, also produce a relationship to themselves as ethical and knowing subjects. As McNay states, these techniques of subjectivization inevitably entail the operation of power relations that are of particular importance for counsellors: "Practices of the self are situated at this level of power relations [as opposed to domination where relations are asymmetrical and irreversible], at the point where individuals autonomously order their own lives and, in doing so, attempt to influence other individuals" (McNay, 1992, p. 67).

It is the practice of caring for others to which therapeutic knowledge is deployed; however, a certain kind of self is needed to perform this disciplinary function. The technique of training, especially when used in the group, is another example of a technology of the self, aimed at reshaping the subjectivities of helping professionals. This technique enables the dispersion of psy expertise over vast distances and into the lives of both the clients of the professionals, and the professionals themselves. Using as a case study the UK Tavistock clinic, Miller and Rose (2008) point out that the training 'group' becomes the transformative experience itself; it is through attending training and participating in groups that professionals learn not only about 'group processes' and dynamics, but these training events also become sites for the subjectivization of professional identities. Professionals expect not only to learn something about how their clients experience groups,

but also to learn something about themselves on both personal and professional levels. Thus, in terms of the professionals themselves, the training group can be thought of as a 'technology of subjectivization'.

Along these lines, the focus groups I conducted with counsellors might be analysed similarly, as an event or occasion in which the participants subjectivized themselves as counsellors by 'being performed' through therapeutic discourse. Using the notion of 'iterability' developed by Butler (Butler, 1997b, 2005), I can analyse the text of the focus groups by asking what strategies of subjectivization are evident in the fabrication of the subject, child sexual abuse counsellor? Of course, I am also claiming that what the counsellors have to say about their relation to testing, and also the texts on conducting psychological measurement, offer prescriptions for a proper ethical subjectivization of the counsellor-self. I should point out that I have just included the focus groups, alongside the conduct of psychological measurement, as a site for investigating measurement as a practice of the self.

Professionals very much engage with the question of what it is that gives them the authority to intervene in the lives of others (Miller et al., 2008), a point confirmed in my focus groups. The Tavistock clinic succeeded in making this authority linked to technical expertise, by promoting the helping professions as a vocation, as something of a way of life which entailed obligations upon those engaged in the 'conduct of conduct' to undergo individual transfigurations of their own subjectivities. The notion of the 'scientist practitioner' is instructive here. This phrase is frequently used by psychologists in particular to describe just this interrelation between a certain kind of knowledge and a practice of

care through which one constitutes oneself as psychologist or, more frequently in the domain of evidence based practice, social worker. By producing and using knowledge in a technical way, researchers and counsellors are incited to become 'scientific selves' (Morawski, 2007).

Just as the 'technologies of the self' notion can include both knowledge and care, it can also refer to heterogeneous ends of such care. For example, Foucault (2005) contrasts conversion as a Christian practice of the self with earlier, more overtly political forms. In the conversion idea, the self is turned only to the self as both object and subject of knowledge; in earlier forms (in the Socratic dialogues, for example), the care of the self is required in order to govern others as an explicit objective. Through conversion, the subject is always being turned inward towards oneself; there is no goal to change or direct the actions of others, except perhaps in the role of assisting a student to turn *their* self to their self. To give another example of the lack of unity amongst the different forms of care of the self, different 'ethics' (schools of thought, traditions of philosophy, etc.) produce different relations between knowledge and the self.

 I offer the above comments as a justification for my use of the language of technologies of the self in the present text. This follows the use of this aspect of Foucault's later writings by writers such as McNay who, despite these studies being centred on historical cultures, see "…practices of the self as a suitable analytical category to understand the way in which people act in modern society" (McNay, 1992, p. 61).

Some social work texts have sought to utilise this aspect of Foucault's research to resuscitate a critical social work practice. For example, Rogerson's thesis (2001, concerned with the production of the 'best interests of the child' in child protection texts), proposed that the practice of *parrhēsia* (an example of a 'technology of the self') could produce the kind of ethical subjectivity suitable for social work practice. Parrhēsia was a topic discussed during Foucault's last courses at the College de France before his death (see Flynn, 1988 for a further discussion; Foucault, 2005). According to this practice, located in ancient Greek culture (but possibly, presumably, having yet earlier historical antecedents - see Hutton, 1988 for a brief discussion of the methodology of Foucault's research in classical and ancient 'technologies of the self'), the *parrhēsiast* was a truth-teller who speaks 'freely' despite the danger this might precipitate to themselves from someone in authority. The parrhēsiast thus not only speaks the truth[73] in spite of danger, but through this act also shows himself or herself to be 'true' in the sense of "independence and even authenticity: the individual has constructed her own truth-self bond that stands outside the dominant one" (Ransom, 1997, p. 164). Foucault elaborates on this quality:

> *Parrhēsia is a quality, or rather a technique, used in*
> *the relationship between a doctor and patient, between*
> *master and disciple: it is the free hand, if you like,*
> *which ensures one's ability to select from the field of*
> *true knowledge that which is relevant for the subject's*

---

[73] Of course, this begs the question of 'truth' itself. Briefly, the suggestion is that the parrhēsiast establishes or maintains a commitment to a 'truth' regime that runs against dominant discourse, that operates according to a different regime of knowledge/power, and in speaking this truth "puts herself on the line" (see Ransom, 1997, pp. 164-165 for further discussion).

transformation, change and improvement (Foucault, 2005, p. 242)

Rogerson (2001) argues that "familiar social work skills including empathy, community work, political activism, social assessment, negotiation and mediation" (2001, pp. 194-195) can be deployed in this kind of practice of truth-telling. Rogerson's suggestions resonate with other similar proposals for a social work based on 'virtue' (Webb, 2006).

My use of 'technologies of the self'

My project differs slightly from Rogerson's, although saying this is in no way intended to dispute the usefulness of that text. Given the extensive use of the work of Foucault and commentators, and the interest in the topic of child abuse, it may be useful to outline some differences. First difference; I am interested here not so much in the particular exercises and *techné* of self-care from the classical and ancient periods (such as parrhēsia), and ways in which they may be useful for social work practice. My interest is on the general metaphor of counsellors undertaking various kinds of activities as continual and considered acts of self-constitution, as technologies of the self. In particular, 1) in what ways do practices of psychological measurement (objectivity) constitute subject positions for counsellors, and, crucially 2) how do counsellors use these practices, OR, their resistance to these practices, to produce an ethical self? By examining the texts of child sexual abuse research and the focus groups with counsellors, a limited set of practices centred on objectivity associated with psychological measurement have been identified. I suggest that these are

practices that can be analysed as a kind of practice of the self through which social workers and psychologists can become ethical subjects.

Second difference; while Rogerson's (2001) text explicitly aims to produce a 'post-structural' social work practice based on the deconstructive skills of a discursive practice—for example, by resisting social work's grand narrative, in the belief that "social work can resuscitate itself" (see pp. 195-196)—the intentions here are more limited. I am more sceptical of the value and possibility of prescribing a way of doing social work that would make it more virtuous (c/f Webb, 2006). I am more influenced by the position of authors such as Luepnitz, who, writing about family therapy, states: "A Foucauldian 'impact' on family therapy would not have to do with practising different techniques, nor would it mean necessarily the end of practice. The point would be to understand ourselves differently as practitioners" (Luepnitz, 1992, p. 284). While I would agree with Rogerson that the techniques of deconstruction and 'post-structural' practices of the self might offer productive possibilities for social work, my research limits itself to analysing what is said in the texts, and attempting to avoid prescriptive suggestions on how (or if) social work should be done.

The objective tester?

> *CB So um, when you were talking about the manual*
> *saying you're not supposed to do things like unpack or*
> *ask the questions, what are people's understandings of*
> *the rationale for why the manual says that?*

SALLY Well because you might influence one way or another

KATE Influence the answer

CB And is that seen as something that you're meant to avoid?

ELIZABETH Hmm.

SALLY Yeah, My understanding of standardised testing is that that's an important thing to avoid in order to do it properly.

ELIZABETH It would be like doing the [work?] and saying 'No you're doing it wrong, let me put in the sequence for you, like, you're not, yeah.

DAVID OR you're going hmmmm [shaking head 'no'] [laughter- others join in saying hmm, mmmm, etc]

NANCY But how can it, how CAN it be avoided, how can the environment and you sitting in the room be avoided? It will always have an impact….

DAVID,KATE Hmm

NANCY On what the child puts down in the test. They're coming in here [to the counselling centre], they might have had a shitty day, blah blah blah,

> *you're sitting there, you're watching them, it's gonna*
> *have an impact.*
>
> *LAURA Yeah, they'd have to be in the room, by*
> *themselves, completely isolated, in order to have…*
>
> *DAVID And maybe not even … here. [laughs]*
>
> *LAURA Not even in the room. In a sensory deprivation*
> *tank.*
>
> *NANCY But still the notion of TESTING comes…*

McClure (1999) refers to the 'vanishing subject', the idea that human subjectivity is not the correct 'method' for attaining knowledge. Given that psychological tests claim to be scientific tools of measurement, the technically competent 'tester' should have minimal impact on the results of a test, just as the objective sociological observer would be assumed to have no impact on the social phenomenon being observed. This has been a problem addressed by psychologists since at least the start of the 20th century (Morawski, 2007). The extract above documents some of the research participants discussing how they might need to conduct themselves in the process of administering a psychological test.

Although the word 'objective' does not appear here, I have interpreted the above discussion as a problematisation of objectivity, in the sense of the observer having no influence on the results. What I wish to point out is that the counsellors do not necessarily believe that (this ideal of) objectivity is attainable, but

it may nonetheless act as a regulative ideal for some (not all). In this, they are joined in a long tradition of scientific practices where "objectivity was an ideal…never perfectly attained but consequential" (Daston et al., 2007, p. 143).

Objectivity

Véronique Voruz (2005) argues that key intention of genealogical analysis is to demonstrate "how the *contingent* has become 'objective', unquestionable 'truth'" (p. 167). Furthermore, the (subject) interpreter is also "constituted through the historical processes within which he (*sic*) is inscribed", rather than "a self-conscious, reflexive subject that stands outside of the field of discourse which he interprets" (p. 167). This consideration can apply equally to the researcher/author as to the participants in the focus groups, who are in effect constituting their 'selves' by producing knowledge-claims about child sexual abuse therapy. If, as Voruz states (following Foucault), "the degree to which a subjective interpretation appears objective is in fact a measure of the success achieved by a given hegemonic discourse in its construction of the interpreting subject" (p. 167), I will be arguing here that if—and it's a big if—the 'measurability thesis' (Michell, 1999) constitutes a dominant discourse within the field of child sexual abuse therapy and research, it nonetheless fails to achieve hegemony. This, of course, is not to argue that it is not influential.

Furthermore, the very notion of objectivity itself is questioned. If it is possible to be 'reflexive about reflexivity', there is also a possibility of being 'objective about objectivity', where the practice of objectivity is understood as dependent upon historical

contingencies and epistemological assumptions. One might say, for example, that objectivity *exists*, but understand that "objectivity has been found to entail a masculine subjectivity, obfuscated by sanctioned proclamations of neutrality" (Hawes, 1998, p. 95). One can still speak of objectivity, but in an ironic manner which subverts the intended meaning and authority normally entailed. The pursuit of objectivity might, in fact, be openly disavowed as a way of knowing about the world. Alternatively, reconfigurations of objectivity might be pursued which privilege accountability, entanglement and embodiment rather than distance and separation (Haraway, 1991; Barad, 2007).

In their book titled simply *Objectivity*, Daston & Galison (2007) treat objectivity as an historically situated concept formed by scientific and epistemological practices. They argue that from our modern vantage point, the concepts of science and objectivity are so close to being synonymous that the formation of objectivity as a particular approach to knowledge is all but obscured. They demonstrate their point by showing how what might now be called objectivity has a fairly recent history, which they trace to nineteenth century science and to Kant in the field of epistemology. Yet, recognizable science was being practiced many centuries before this notion of objectivity was developed. The point is simply that science did not have to 'wait around' for objectivity before it got started.

Daston and Galison delineate four styles of scientific objectivity, or relationships between science and objectivity: 1- truth-to-nature, 2- mechanical objectivity, 3- structural objectivity and 4- trained judgement. Although emerging in historical succession, the paradigms inform, merge and re-define each other rather than

replace, so that traces of truth-to-nature might continue to inform some practices of structural objectivity, for example. The authors look at scientific atlases to interrogate the styles of representation current at various historical periods from the 1700s through to late twentieth century. In the truth-to-nature paradigm, naturalists (or other scientists) collaborated with artists to produce illustrations of scientific objects (e.g. leaves, plants, skeletons). These collaborations were rife with political, social as well as epistemological implications. The authors call this collaborative way of seeing 'four eyed sight'. The scientist knew that the artist's skills were indispensable, but imagined that their job was to put onto paper what the scientist had learned to see by their training in observations. The illustrations would be based on an actual example or specimen, but would be 'smoothed out' to represent an ideal type, an image of which was held in the mind of the scientist on the basis of their extensive observations of numerous individual examples of that type. Thus, the image produced by the artist represented truth-to-nature, nature being the ideal type rather than any actual specimen. (This is resonant with Plato's ideas about Truth, Beauty, etc). Intervention between object and image is valued. Implicitly, a certain kind of scientific self is produced, one that is actively trying to produce objectivity.

In the 'mechanical objectivity' paradigm (emerging mid-nineteenth century, and firmly established by the end of that century), intervention is to be avoided. This style of objectivity is defined as follows: "By *mechanical objectivity* we mean the insistent drive to repress the wilful intervention of the artist-author, and to put in its stead a set of procedures that would, as it were, move nature to the page through a strict protocol, if not automatically" (Daston & Galison, 2009, p. 121). The move was consequently away from

types and towards individual examples. "Depicting individual objects "objectively" required a specific, *procedural* use of image technologies" (p. 121). Mechanical objectivity also meant a "newly disciplined scientific self bound to a highly restrained way of seeing" (p. 122), one that was committed to restraint from intervention. The way of seeing here is described as 'blind sight' (rather than the 'four eyed sight' of truth-to-nature). Thus, the emerging scientific self on the late 1800s was a self that could be eliminated from the image or measurement. (Recall McClure's 'vanishing subject' (1999)). The virtue of restraint neatly illustrates the way that "mastery of scientific practices is inevitably linked to self-mastery, the assiduous cultivation of a certain kind of self" (Daston and Galison., 2007, p. 40).

Daston and Galison identify two divergent trajectories in response to mechanical objectivity. Structural objectivity was uncomfortable with the use of images *tout court*, as they are inevitably reliant on visual sensory perception. As this is held to be an innately individual and therefore subjective way of knowing about the world, whose independent existence can never be finally established, the visual medium is unsatisfactory as a means of both comprehending and communicating about the external world. Structural objectivity holds particular relevance for the topic of psychological measurement, as much early psychological experimentation was concerned with the measurement of perception. Along with the work of ethnologists during the mid-late 1800s, such research established that sensory perception was quite variable from individual to individual, and across cultures.

Structural objectivity led to a search for Truth not in the essence or nature of objects themselves, since these were inaccessible to the

senses, but in relations between objects. Truth here was to be discovered in stable and universal structures that remained intact despite differences in content. This entailed a turn away from images, and towards a way of communicating that was not dependent upon individual or culturally specific perception, that would be non-empirical. What was needed was a kind of language that could be universal, even enabling communication between humans and yet-to-be-encountered alien species. Numbers, specifically the laws of arithmetic, promised such a structural language. Frege outlined his intention in creating such a universal language:

> *Whereas each [person] can only feel his pain, his desire, his hunger, can only have his sound and color sensations, numbers can be the common object for many, and indeed are exactly the same for all, not just more or less similar inner states from various [people] (Daston et al., 2007, p. 281, citing Frege, 1884).*

 For Frege, it was precisely the fact that reason did not require empirical data that made arithmetic objective. The kind of self that this ideal produces was parallel to the world it described. Only reason itself was valued, the self and the world both "stripped down to skeletal relations, nodes in a network, knower and known admirably adapted to each other" (Daston et al., 2007, p. 302).[74] The irony of Frege's project of creating such a universal language is that it was found to be inaccessible, even to his learned contemporaries such as Bertand Russell.

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[74] This image recalls Deleuze's use of the rhizome as an image for thought, although he claimed he was an empiricist and not a structuralist.

Nonetheless, it is possible to suggest here a kind of emergence of an epistemological condition that would later make possible Steven's definition of measurement (assigning numbers to objects according to a rule). Measurement produces relations between objects. The existence of the relations themselves depends upon the practice of measurement, the technologies and rules used, but in no way relies upon fixing the nature of the objects themselves. For instance, when measuring aggression, an instrument (such as the BASC) only has to maintain that the rule it uses to assign numbers (i.e. the questions it poses, the answers it receives, and the relationship between these and the construct 'aggression') is internally valid as a procedure of measurement. It does not have to establish whether or not person A's aggression is qualitatively the same as person B's, only establish a relation between them. The practice of measurement literally objectifies aggression so that each person's individual experience of this construct is irrelevant. What it does produce is a relation between person A and B, through the relation of each to the norm.

The fourth kind of objectivity, also a response to the perceived failures of mechanical objectivity, is *trained judgement*. Rather than fleeing ever further from empiricism and individual interpretation, as with the structural objectivists, trained judgement sought a way to combine the virtues of mechanistic image making with the training and expertise of the scientist. Images are not necessarily idealised, as in truth-to-nature, nor left to speak for themselves, as in mechanistic objectivity. Trained judgement resonates with the notion of clinical expertise; scientific data is not rejected, but incorporated into a skilled judgement.

These four kinds of objectivity in representation hold some parallels with Rose's description of images of children in developmental psychology. While in Rose's account (1996) the graph becomes the ultimate image for representing the child (structural objectivity), there is a very current resurgence of the image; specifically, images produced through brain imaging technology and interpreted for the practitioner by the neurobiological expert (trained judgement).

Daston and Galison (2007) convincingly demonstrate that objectivity is a scientific *practice*, and not a unitary or fixed abstract ideal. Furthermore, objectivity involves very particular ways of conducting the self. This recognition ties in with the idea that objectivity, rather than being a matter of a disembodied vantage point which subsequently entails no responsibility for how one sees, can instead be reworked as a 'situated knowledge' (Haraway, 1991, Ch. 9, pp. 183-201). The acknowledgment of the entanglement of the subject and object of knowledge, and subsequently the intra-action of each with the other (as parts of the one system), demands accountability for one's practices within those entanglements (Barad, 2007). When this entanglement involves quantification, those "…who use these statistical and calculative methods are themselves constrained by the calculative apparatus they use. And this means that quantification produces a certain kind of objectivity" (Rose, 1999, p. 207).

Having established that objectivity is neither unitary nor abstract, but is produced through material practices in specific situations, I am now entitled to ask: what kind of objectivity does psychological measurement produce in the case of child sexual abuse research, and in counselling?

Knowing subjects and objects

One of the epistemological dilemmas raised by scientific knowledge about humans is that the knower becomes both subject and object of knowledge: the knower as well as the known. In the natural sciences (e.g. biology) the scientist can remain conceptually removed and distant from the 'object' of study. This breach is clearly more difficult to maintain in the human sciences. In the case of a psychological measuring device such as the Behaviour Assessment System for Children (BASC), this becomes a problem to be managed. (For a history of the development of the BASC, see (Rescorla, 2009). The BASC is the main test used by the counsellors in my focus groups. Its use with children who have been sexually abused is recommended in practice guides (e.g. Johnson et al., 2007). This test asks the child and/or caregiver to rate the frequency with which the child engages in a list of concerning behaviours. Children report on themselves, and are also reported on by their parent/caregivers, and teachers. (Children under a certain age are reported on only by their parents, not themselves).

Once these reports are completed, the BASC software calculates in each case the likelihood that the report is *reliable*, or whether it is unduly tainted by factors such as a parent's negative disposition towards the child. The child and/or adult report the data; their reports may be deemed more or less reliable. The expert, in both cases, is left to interpret the results, the veracity of this interpretation is ensured not only by the expertise and authority of the counsellor, but importantly in conjunction with the processes of calculation themselves, performed by the BASC software.

There is actually a chain of observer/observed relations with the BASC. The clinician (counsellor) is not called upon to observe the child directly. These observations are made by the child and their parents and teachers. This chain is productive for the counsellor, because they are able to maintain a degree of separation from the child's behaviour (which is the object being observed), as well as maintain a distance from the observers. In effect, the counsellor using a BASC assessment is working with the observations of observers.

The BASC authors credit parents and teachers with being the "behavioral experts" regarding the specific child (Reynolds et al., 2002, p. 2). This is significant in light of the fact that observation itself is considered a technical skill in the discipline of social work (Le Riche et al., 1998). In the BASC, the expertise of the counsellor lies elsewhere, in the interpretation and application of these observations. Yet I want to stress that despite Reynolds and Kamphaus claim, the 'behavioral experts' are not actually granted expertise in observing the child, and are in need of a disciplinary instrument (i.e. a checklist and questionnaire) in order to observe scientifically. Furthermore, the website for the (revised) BASC II advises teachers that:

> *A score in a shaded area of the table or chart does not necessarily mean that your child has a problem that is unusual or that requires treatment. Such a conclusion must be made by a psychologist or other qualified clinician, or by a treatment team (*http://www.pearsonassessments.com/hai/images/PA/Products/BASC-2/BASC2ParentFeedbackReport.pdf*. Cited 03/07/2012).*

Who is a measurer?

> *NANCY: [laughs] Like I mean I just think questions like '----------------?' and '------------'[75] really disturb me in the BASC testing, some of the individual questions. And the repetition of questions also disturbs me as well, and um, is um, quite perplexing to some of the clients, around, you know 'I've already answered this question, this is the same as the other one'. Also the notion of true or false, and there's no in between.*

> *KATE: I think and that's where, yeah, a lot of the kids it become like a right or wrong thing, like it's a…I mean I try and spend a bit of time before I give it, because- and explaining that- because [they might think?] 'If I do that, that's wrong'. And you know, what's a right or wrong, and then you're creating this anxiety around something which is…*

> *NANCY: Hmmm*

> *KATE: …Not about that and you know they think 'oh this is a test',*

> *NANCY: Hmmm*

> *KATE: Again, I'm gonna be right or wrong, I'm gonna get a mark out of this, and what does that mean? So…*

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[75] Due to copyright restrictions, it is not possible to reproduce the wording of test items used in the BASC.

NANCY: Yeah.

KATE: Yeah, it's an issue.

*CB And, I think that 'true or false' is really interesting.
Is that something else that people think about or talk to
their clients about? About why [they answer?] true or
false?*

*NANCY: Well, you know, I mean I have had a couple
of little boys who said that 'I can't…you know, it's
both'. It's true AND false, and so I can't push them
into either one*

KATE: Hhmmm

*NANCY: And so I just leave it, and then I circle one of
them later…*

I want to try and delineate what qualifies one as a measurer. In
light of the analysis that has been unfolding in this thesis, I can
examine a number of statements:

a- A measurer must utilise a measuring device- without this,
there is no measurement.

b- The device must be practicable in the situation (or
phenomenon, to use Barad's term)

c-   The phenomenon must *produce* measureable traits or
objects

d- The *agential cut* designates one subject as the measurer.

If these statements are right, I argue that identifying the measurer within any particular phenomenon depends very little on the will or intention of the subject. She or he must become entangled within the situation and follow the order of things set out for them, or else cease to be a measurer. The agential cut designates relations of power/knowledge involving all actors (human and non-human) in the situation.

And here is the dilemma facing researchers and counsellors in the field of child sexual abuse. In this field, unequal power relations are highly problematic. This lies behind the feminist objections to the use of measurement and statistics, for example. Yet in order to participate in the practice of psychological measurement, one has no choice but to enter into these relations of power with children. The above extract demonstrates the counsellors' engagement with this problematisation.

It can be argued that measurement is a way of seeing. The terms 'observation' and 'measurement' are almost synonymous in physics. In this context, to see is not to passively perceive, but to actively observe (Crary, 1990). My emphasis here is on the specific kind of agency and subjectivity demanded of the observer in order to see what is made visible by the research or therapeutic situation. This includes taking into account the assemblage of technologies, objects, institutions, bodies, and other materials entangled in the observation/measurement.

The BASC clinician's guide is clear about this intention that the BASC be used as an instrument of vision, stated clearly in this extract:

> *The SRP [Self-Report of Personality]* ***provides a view***
> *of how the child experiences his or her behavior,*
> *feelings, attitudes and cognitions. The PRS [Parent*
> *Rating Scales] and TRS [Teacher Ratings Scales]*
> *provide impressionistic, holistic summaries of behavior*
> *as* ***seen through the eyes*** *of behavioural experts*
> *specific to the child in question. The SOS [Student*
> *Observation System] provides* ***direct observation*** *and*
> *counting of behavior, believed by many to be the sine*
> *qua non of behavioral assessment (Reynolds et al.,*
> *2002, p. 2, all emphases added).*

The observer must link up with the various forms and instructions of the BASC to form what Deleuze and Guattari might call a 'measurement machine'. I think the researcher or clinician (the BASC user) is more of an 'orderer' of vision, including the observations of others who complete the forms. The professional BASC user calculates the observations of others, while never having to directly observe anything the child does.

The question to be asked of this arrangement is how this could be regarded as objective. As I have argued, the use of measurement instruments in the research on child sexual abuse and in therapeutic contexts, are lauded primarily for their capacity to provide objective knowledge. I suggest that it is a particular kind of objectivity at work that most closely resembles the trained judgement described by Daston and Galison (2007), with elements of structural objectivity. This kind of objectivity functions as a regulatory ideal for the conduct of the enlisted observers as well as the clinician. That is, it is not that objectivity means having no influence on the measurement situation; the impossibility of this

was clearly articulated by the counsellors in the focus groups. Rather, objectivity calls for close attention to how one conducts oneself in order to produce both a particular context of knowledge production, and a particular kind of knowledge.

Subjects and objects of knowledge

This enquiry also allows us to ask about the different kind of relationships that are possible between a knower (the subject of knowledge; the researcher/counsellor) and the known (the object of knowledge; the child victim of sexual abuse). Specifically, the possibility of objectivity as a feature of classical scientific method in the natural sciences depended on a dichotomy between the scientist and the natural world (Keller, 1985). In the social sciences, there is a strong feminist tradition of critiquing the notion of objectivity as "…an excuse for a power relationship every bit as obscene as the power relationship that leads women to be sexually assaulted, murdered and otherwise treated as mere objects" (Stanley and Wise, 1983, p. 169, cited in Oakley, 2000, p.35)

However, work in science studies, particularly since Latour, have stressed the constructed nature of this dichotomy between scientist and the natural world; "…applied to history of psychology, this work would imply the analytic value of breaking down distinctions between the natural world described by psychology (the human mind) and the minds and social worlds of psychologists themselves" (Cohen-Cole, 2005, p. 108). While some traditions of *scientific* psychological enquiry inherited the ideal of this style of relationship—psychologists were to be 'scientific selves' (Morawski, 2007)—other traditions of psychology maintained a

commitment to scientific method, but through their method disrupted the absolute separation between subject and object (for example, by a team of researchers interchanging roles in an experiment; Danziger, 1990). Social work, with its historical emphasis on styles of relationships between worker and client, offers further possibilities for 'subjectivization', as do the various schools of psychoanalytic practice where the therapist-patient relationship *is* the means of insight and change.

To make this inquiry specific to the problem of psychological measurement, one way of analysing this process is through understanding tester's instructions as attempts to standardize the tester. This is a key point of a disciplinary analysis of psychological testing of clients or research subjects. Through presenting a standardised field of possibilities (which can be presented in the visual form of a graph) in which any individual may be located relative to the norm and the rest of the population, psychological measurement and testing can be seen as a disciplinary tactic of standardisation. Just as such an analysis can be challenged through examining the resistance to such attempts at standardisation by research subjects or clients, the present text aims to investigate the means available to counsellors to resist (or indeed to enact) standardisation of their practice or subjectivity as counsellors. In saying this, it is important to make clear that resisting one form of power/knowledge is not synonymous with freedom from disciplinarity in its entirety (Rose, 1999). It is argued here that the focus group discussions provide empirical evidence that resisting one mode of disciplinary practice depends upon the working of other discourses (which may or may not constitute 'subjugated knowledges' within a discipline).

The knowledges and practices of psychological measurement both produce and require particular kinds of relationships between the subject and object of knowledge. Of particular interest is the ideal of objectivity. Standard texts and manuals for practitioners using psychological tests stress the importance of the tester having the minimal influence possible on the results, which is often called objectivity. To take a local example, the study guide on Psychological Testing and Measurement produced by Deakin University Faculty of Health, Medicine, Nursing and Behavioural Sciences (Miller, 2007) states that the human interaction between examiner and examinee can produce error in the results when administering psychological tests. Thus, the examiner should "put examinees at ease and establish rapport before launching into testing" (p. 4). The presence of the 'examiner' should have a particular effect, rather than a minimal effect.

Viewed from my analysis of subjectivation of the counsellor, this directive calls for anything but 'invisibility' (McClure, 1999), as the examiner should be active in seeking to create conditions that minimise the client's "feelings of apprehension and uncertainty that can degrade performance" (Miller, 2007, p. 4). Put another way, the 'examiner' is called upon not so much to be objective, but to use their subjectivity in such a way as to promote a certain, preferred kind of testing environment.

In their 'clinicians guide' to the BASC, Reynolds and Kamphaus (2002) advise that a combination of "measurement science, common sense, and accumulated experience" should inform the use of this piece of "child assessment technology", "with the flaws of each dimension fully recognized" (all quotes, p. 1).

In their case study, Khalily and Hallahan (2011) documented that filling in psychometric tests contributed to the research participant[76] becoming "agitated and tremulous" (p. 340). These authors did not offer any information about their own responses to their research participant's distress, how the conduct of the researchers may have contributed to these feelings, or how these feelings may have played their part in the research participant's responses to the psychometric instruments. One possible reading would be that the researchers failed to adhere to proper procedures. However, the researchers, by noting the effects of the measurement process on the young women, presumably intend to convey some kind of significance to the audience.

Note the difference between the case study reported above, a research context, and Kate's account of using a test in a therapeutic context. The intention of standardised testing instructions and practices to minimize the influence of the counsellor paradoxically heightens counsellor's awareness of their 'use of self'. Kate's account (below) demonstrates the extent to which the use of testing can produce the counsellor's awareness of their influence and responsibility in conducting a therapeutic counselling session:

> *CB … I just want to go back to something Kate said,*
> *she said that it can be a torturous process. Do you*
> *mean just for the child, or as a therapist?*

> *KATE: No. Both. I was feeling, by the end of it, I was, I*
> *felt, I felt bad, 'oh come on' you know and I thought*
> *'this is not what this is supposed to be about', and that*

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[76] Their participant was an 18 year old woman who had been subject to childhood sexual abuse when she was considerably younger.

> *was not her experience of previous sessions with me*
> *either, so it was back to that, the difference between*
> *the way you run your sessions normally as opposed to*
> *when you're trying to do that testing process, because*
> *she was like, you know, 'really, what is the deal with*
> *this?' We don't, you know, normally we do do a*
> *number of different things, because that's the way I've*
> *been able to structure, the way for her to able to be*
> *with me within that therapeutic space. So it was*
> *torturous for her, she was bouncing off the walls, and*
> *then it was a bit, like you know, it was hard work on*
> *my part to try and get her to do that, but then I was*
> *also feeling kind of responsible for getting to that point*
> *too. Umm, yeah, so probably all of those things, I*
> *think.*

This capacity for reflection calls into question Foucault's reference to psychologists as "servants of moral orthopaedics" (in *Discipline and Punish*, cited in Hook, 2007, p. 40). It is surprising that Foucault would characterise the deployment of psychological expertise in such a manner that effectively whitewashes the internal contradictions and struggles involved. This whitewashing obscures not only the subjectivizing effect of expertise on experts, but as Michell (1999) details, the discursive battles involved in establishing the legitimacy of practices like psychological measurement. Regarding the former issue, it is this simplistic portrayal of psychologists and social workers that I want to question in this section (the latter issue of the politics of legitimacy have already been discussed). Such a project can draw upon (among others, of course) the later work of Foucault on 'ethics', where much more attention is given to the ways in which

individuals take up bodies of knowledge and systems of ethics in order to actively work on their 'self'; specifically, to work at becoming an ethical self, a phrase that holds some resonance for the ways in which counsellors attempt to conduct themselves.

Kate's comments say something about objectivity as well as reflexivity, showing them to be entangled practices. Barad's (2007) notion of objectivity sets itself against a classical scientific image of an external object (to be measured) and an internalised and contained subject (to observe). Barad's 'agential realism' reworks objectivity from the Borhian insight that it is the apparatus that enacts and produces such 'cuts' between objects and subjects. Such relations between the observer and observed exist, but they are not pre-existent outside of the particular phenomenon being observed. Barad argues that "*Objectivity means being accountable for marks on bodies, that is, specific materializations in their differential mattering.* We are responsible for the cuts we help enact…because we are an agential part of the material becoming of the universe" (*ibid*., p. 178, italics in original). Barad argues that it is possible to understand phenomenon objectively, but not objects and properties; "…the objective referent is phenomena, not the seeming 'immediate givenness' of the world" (Barad, 2007, p. 244).

Remembering again that Barad bases her theory on physics[77], this notion of objectivity nonetheless holds some resonance for Kate's dilemma. I am taking on the implications for measurement as a practice, not any direct correlation between children and particles

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[77] In linking measurement in physics to measurement in psychology, I am doing no more than following the lead of the founder of quantitative psychology, Fechner, a physicist. (Michell, 1999).

as objects of scientific research. I would argue that one way of understanding Kate's comments, using Barad's objectivity, is as an attempt to grapple with her ethical responsibility for the manner in which this cut between herself and her client is being enacted and the apparent effect this has on both herself and her client.

The question of whether a psychological test can be objective or subjective appears throughout the focus group discussions (see also p.38). The following extract flows from a discussion of a scenario in which the re-application of a BASC test shows that anxiety levels for a child have decreased over a 12 months period of therapy.

> *KATE This was really high, you know, by closure it's not because, you know, at the time this, this, and this were happening, the writer notes now that, you know these things are no longer present, however there are still concerns around x, y and z that might explain the high score, you know that's how, and I would, yeah. And I think that's the important part, you can do a test for anything, but if you don't, the important part is the interpretation and what you put to that, so…*
>
> *NANCY   So then it's really subjective, isn't it?*
>
> *KATE   Well of course it is.*
>
> *NANCY   Yeah.*
>
> *KATE   But your assessment's subjective.*

NANCY Hmmm, yeah.

KATE   Your individual assessment will be subjective, probably, you know, we would hope…

NANCY So what's the purpose of the test?

KATE   we would hope we would get significant…are you the person or are you…[meaning 'are you the researcher?']

[all laugh]

CB This is why we have focus groups.

NANCY   No, but really, you know if it's so subjective, if we're making….I'm not arguing against you, I'm just asking, saying that if it's so subjective…

KATE   I think it's part of that, I think it [couples?] part of those things and I think sometimes that using it, it's a part of, it's not, it's not a, I don't think it's a thing in and of itself

JASON   Not the be all and end all

KATE   It's not the be all and end all and it's not, um, you know. Like you get those pieces of clay, well we could have decided many things of what some of those clay things could have, you know

ELIZABETH   We could have

NANCY   That's very subjective too

*KATE   That's what I'm just thinking like, you know
you get those things and you're interpreting them and,
you know, some of those things we were looking at
were…*

*JASON   So, is it about interpretations? You know, like
what's the best therapeutic value for a child in the 50
hours that you see them, and is a test, however long
that takes, um, what does that do…what benefit is that
for the child therapeutically? [pause] if you're saying
that's subjective and that's subjective, if everything's
subjective well let's just do, spend time with something
that works for the kid, then.*

*CB   Yeah, that's one of the, I think one of the most
interesting points, is that the whole purpose of people
designing and making tests is to supposedly achieve
some kind of objectivity or scientific…I mean these
guys talk about this work as being scientific, and based
on scientific method because it's supposedly objective.
Um, but what I'm hearing quite strongly, and last time
as well, is that people don't really treat them as
objective. Like, people don't treat these as an impartial
kind of truth about the child.*

ELIZABETH   Hmmm, yep yep. [others agree]

ELIZABETH But I think in response to your question [to JASON], you know, what is the therapeutic point for the child, I'd say none.

CB In doing tests?

ELIZABETH Yep. I would say it's to assist the therapist. I think parents might get, feel a bit more settled or something about hearing well this test says that you, you know, your child's emotionally and behaviourally going along OK,

KATE Hmm, hmm

ELIZABETH So you know, you can have parents out there that search for a diag…not even a diag…, yeah well a diagnosis, but you know, just to, that might help settle some parental anxieties. But I would say therapeutically there's probably none for the child.

The above extract evinces quite a different attitude towards the importance of objectivity than that articulated by Rind et al.. Rind et al. privileged their quantitative meta-analysis of the harm of child sexual abuse in large part because qualitative approaches are subjective. The extract, in contrast, seems to suggest the productivity of *not* seeing the practice of psychological measurement as objective (even if some version of classical objectivity is at work as a regulative ideal). In fact, what is being articulated is precisely the therapeutic practice issues entailed in the *process*, rather than the outcomes, of measurement. The process of measurement is what the classical notion of objectivity

wants to account for only on the basis that there would still be an object to measure without the process. If the classical scientific purpose of measuring is to 'mark bodies' in a way that takes the mark and the object as representing something about the world that would be true if the measurement had not taken place, here attention is being paid to the way the process of measuring itself can mark both children and counsellors. To reiterate a point made above, the counsellors are deeply engaged in wrestling with their agential responsibility for the situation in which such 'marks' are made possible.

Resisting one kind of expertise

In hearing Nancy, in particular, describe a general disregard for the usefulness of psychological testing and measurement for her therapeutic practice, I became interested in trying to understand how her position is also produced by a set of conditions for knowledge. In rejecting the subject-position of 'measurer', Nancy inscribes her practice within a different set of priorities and statements about knowledge and expertise.

> *CB to NANCY   Can I ask you, cause you've expressed*
> *that you hardly use it or place a great deal of faith in*
> *the testing,*

> *NANCY   No*

> *CB What do you do with the results of the report that*
> *comes back when you've finished a test?*

NANCY   I put them in the computer, and they spill them out for me, and then I look at them, and then I go "oh yeah. OK". Sometimes I bring KATE   over [laughter], but she's getting sick of me doing that so I don't bring her over any more. [more laughter].

ELIZABETH   So you get David [more laughter]

KATE   No no no, I just, I read it through and I think "oh yeah"….[laughs]

CB and do you share it with the child or the parents?

ELIZABETH   What, the 'oh yeah'? [laughter]

KATE   'We did the test. yeah'.

ELIZABETH   'I had a glance. Nn..oh yeah'

CB 'Thanks for filling out those 152 questions' [laughter]

ELIZABETH For taking the time.

NANCY   Well, yeah I know, it sounds interesting. I do have actually other things that I draw on, apart from the tests, [laughs] and yeah, so, look I look at it, KATE's helped me understand that, um, with a few of the ones I've done there's you know, there's nothing that's um, clinically worrying, or whatever, Um, so, I think I just…I really don't place a lot of emphasis on it,

I suppose if I saw there was a star up in the area where it wasn't meant to be I'd have a look at it, and I'd probably think I'd typed in the wrong numbers, that are not right, because I don't really, I have a lot of faith in my own attunement to the child, so um, and you know, but, I must say that once I read out a few of the questions to one of the little boys that I was seeing, and then that provoked a whole lot of discussion. So some of the questions were quite interesting, so that provoked discussion but that's not in any way you know like a, you know a proper administration of the test really.

Because it's, I mean I took it my way, and went off on an area and started talking to him about it "oh you know so, tell me a little bit about that" and so that kind of promoted some discussion, but um, yeah, basically…there was another test that I did there was a father who did the test and the child, the young woman who did the test, and you know she said 'yes I have thoughts about harming myself', he said 'she's never harmed herself in her whole entire life', so that just reinforced that they're poles apart. But that was in the test, um, and that said something to me about what I already knew, but I suppose it was reinforced in the test. Yeah. but I don't go back and give them feedback. Sometimes in the assessment part where I met with the parents, I might say that, you know, we filled out the forms, those tests, and um, you know I might highlight a couple of things that came out of that but only if that's been reinforced by what I've seen.

CB So, you're first thought that if it was very different to what you'd thought before, that something had gone wrong,…

NANCY   The wrong numbers for the wrong thing. Yeah, yeah. I'd be shocked if anything that came out of the test was different to my own perception….I mean, that might sound, you know, but it's, yeah, and I have my own theoretical framework of how I'd support the experience that I have with the child, that goes quite counter to the test, I suppose.

JASON So why do you do the test then?

NANCY   Because, uh, I have to. [laughter]

The technical notion of expertise embraced by the evidence based movement practice, and the associated therapeutic modality of cognitive behavioural therapy (CBT), is challenged by different notions of expertise. There is also a productive distinction between expertise as a researcher and expertise as a counsellor. In the field of child sexual abuse treatment, it has been argued that there is no current accepted definition of what qualifies anyone to be an expert (Oz, 2010).

Resistance to quantification

There is evidence of a certain discomfort with the representation of people with numbers throughout history. Even in the Old Testament, "numbering the people" is a great sin committed by David, resulting in the death of 70 000 people by plague (Cohen, 2005). Various professional groups claiming expertise in particular social problems have espoused resistance to the quantification of their work at various historical junctions. Certainly the idea that psychological attributes are suitable for measurement is not, and has not been, without controversy: one theorist even coined the term 'quantofrenia' as a kind of satirical diagnosis of the tendency in psychological research to "quantify all and everything" (Sartori, 2006, p. 409, citing Sorokin, 1956). A range of professions concerned with the conduct of human affairs have resisted quantification in various ways. I will outline some of these here to demonstrate that although quantification and measurement may constitute hegemonic discourses in many areas of the human sciences, they are rarely, if ever, totalizing.

The resistance of doctors

Stigler (1992) identifies resistance to statistical methods in European medical texts as far back as the 1690s. The medical profession's objections to the quantification of understandings of human illness also appear during the 1800s, where they "… maintained a case that looks very like the later feminist one: that each patient is an individual, whose situation and history can only be taken into account during a clinical interview; thus the value of 'aggregative thinking' destroys the essential, individualized basis of the medical art" (Oakley, 2000, p. 110). Statistical aggregates

obscured the mechanics of cause and effect in individual cases (Cole, 2000). Some medical practitioners objected to the collection of statistics from their patients, as this was irrelevant to the treatment of the individual patient being treated (Hacking, 1990, p, 86). Debates between French doctors and statisticians in 1837 revolved around the two issues of 'minority facts'[78] and defence of the medical practitioner as an expert subject of knowledge (Cole, 2000); it was the possibility of their expert judgment being rendered unnecessary, or worse, as unreliable and prone to error, that prompted the resistance of some doctors. They perceived the danger that, in a knowledge regime ruled by statistics, they might 'vanish' as subjects of knowledge (McClure, 1999).

Porter (1995) stresses this dimension to the resistance of doctors (discussing specifically US doctors in the 1940s-50s) to statistics as a basis for decision making. They feared that their expert clinical judgement would be displaced. "…the ideal of objectivity, as the statisticians conceived it, was difficult to reconcile with clinical judgement" (p. 204, referring to Austin Bradford Hill's reflections on his lectures at medical schools during that time. Hill contributed statistical expertise to some of the first large scale controlled clinical trials, especially on the relation between smoking and lung cancer). Statistics were related to a notion of objectivity that minimised (ideally, did away with) the need for the exercise of judgement.

According to Porter the purpose of double-blind methodology in medical research was mainly to "neutralize the effects of expert

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[78] Minority facts were those cases that were inconsistent with statistical aggregate. This debate pointed also to political concerns related the dangers of a democracy based on majority opinion (Cole, 2000, p. 88)

discretion without disbarring it" (Porter, 1995, p. 205). That is, when new treatment approaches were being trialled, and doctors were instructed as to what course of treatment to prescribe to their patients, doctors felt that their professional expertise was neutralised.

Porter (1995, p. 202) suggests it was also the imposition on the trust with patients that led doctors to be resistant to numbers and the use of instruments in treatment, especially those that left a record of their measurements. The title of Porter's book, *Trust in Numbers*, points to this displacement of trust, from the relationship between doctor and patient to the claimed objectivity of quantitative methods (although the argument in Porter's book reaches farther than the medical profession alone). Doctors' view of their work was not compatible with a mechanised application of interventions. They shared a privileged relationship involving a significant degree of trust with their patients, which they felt contributed to a thorough understanding of their patient's needs and circumstances. This was indispensable to an accurate diagnosis and treatment.

Of course, with trust came authority, and some 19th century British doctors argued for the necessity of the 'gentlemanly' standing of physicians as necessary for exercising the requisite authority over patients, not to mention defending their own elite status in society (Pernick, 1985; Daston et al., 2007).

With contemporary developments in biomedicine and its technological apparatus, challenges to the authority of the clinical judgement of medical practitioners have intensified over recent years. "Doctors have lost the monopoly of the diagnostic gaze and of the therapeutic calculation: the clinical judgement of the

practicing physician is hemmed in and constrained by the demands of evidence-based medicine and the requirements for the use of standardized, corporately framed diagnostic and prescribing procedures" (Rose, 2007, p. 11). However another practice of objective knowledge, 'trained judgement' (Daston et al., 2007), does not directly oppose such technologies, but embraces them in combination with the expertise required to make accurate and proper use of the information so garnered.

It is worthwhile noting D'Amico's (1989) discussion of Foucault's *Birth of the Clinic* regarding the conceptual schema of classificatory (as opposed to clinical) models of disease in the classical era. According to the classificatory schema, disease belonged to a familial 'space', in which disease has properties proper to itself, and not restricted to locations or manifestations in particular patients. In fact, manifestations in individual patients are more like distortions of the disease itself, as an entity. Thus, in order to understand disease in this schema, case histories of individuals are not part of the diagnostic 'gaze'. "If one wishes to know the illness from which he is suffering, one must subtract the individual, with his particular qualities" (Foucault 1963 p. 14, cited in D'Amico, 1989, p. 14). This suggests the linking between statistical analysis of psychological traits and the classical classificatory schema of disease. Borsboom's (2005) description of classical test theory is resonant here. In classical test theory, the concern is with populations, not the 'idiosyncrasies' of individuals. "So, the individual subject must receive a probability distribution, but only in order to make him disappear from the analysis as smoothly as possible" (Borsboom, 2005, p. 17).

Actuaries

One might think at first that actuaries would be amongst the most fervent advocates of quantitative approaches to their subject. Actuaries were employed by life insurance companies. Their job was to calculate the appropriate premiums that should be paid by individuals in a fund so that the fund could remain solvent. They were also responsible for choosing 'selected lives'–those lives that the insurance agency would consider a sound investment. Actuaries were responsible for developing tables of calculations indicating the appropriate premiums for a variety of lives that insurance agencies might insure.

However, Porter (1995) traces the resistance of actuaries to the strict quantification of their profession. They objected to a wholly routinised practice, which would involve a denial of trust in their positions as gentlemen of integrity and judgement. They also argued that the 'selection of lives' for insurance policies required 'real world' experience and judgement not possible with mechanically applied statistical laws.

Part of the actuaries resistance to a completely technical mathematical approach to their profession stemmed from the belief that "knowledge is local, and that even general rules are useless except to those who understand the conditions under which they should be applied" (Porter, 1995). The resonance of this sentiment (the period Porter is discussing is around the 1860's) with contemporary post-modernist and feminist critiques of scientific method is striking.

Part of their resistance was based on their perception of their position as trustworthy and gentlemanly members of the community, whose integrity ought to be guarantee enough that they did their job accurately and honestly. There was a definite elitist sentiment to their resistance. Their message was that a good actuary is better than a good set of rules for calculation. There is an argument to be made that this was an instance where quantification may have aided the democratic demand for transparency, and was resisted on the basis of distinctly elitist motivations.

Counsellors

There is an analogy to be made with the ostensible focus of this paper. Many may feel that a good counsellor is better than an empirically proven intervention. According to Hand (2004), the debate over clinical vs. actuarial approaches in psychology can be traced back at least to 1925 (citing papers by Freyd and Viteles of that year). These debates resemble that outlined above in the discussions of doctors: statistical methods were fine for understanding populations of people, but the treatment of the individual patient or client required nuanced clinical skills. In fact, contemporary counsellors could even cite a body of quantitative research which indicates that it is these very relational practices which are necessary for effective therapy (Campbell, 2002). Whilst there is a strong tendency in some child sexual abuse research to argue for the increased precision offered by statistical and quantitative knowledges (Rind et al., 1998; Freyd et al., 2005; Tonmyr et al., 2011), others counter that techno-scientific modes of knowledge are actually less likely to produce accurate understandings in welfare decision making (Taylor et al., 2000); a

position that echoes the critique of scientific objectivity developed by feminist writers such as Harding (1986).

Charges of elitism (such as those levelled at Porter's 19th century actuaries), one feels, would not sit well with counsellors in general and social workers in particular, who tend to privilege the espousing of egalitarian values. The arguments put by the doctors, about the importance of trust and the intimacy of the relationship with the patient/client would, it might be expected, sit better with most counsellors than the distinctly superior tone of the insurance actuaries. Evidence of this kind of statement is at hand. The following extract from the 'focus groups' shows that NANCY does not eschew the notion that she possesses expertise as a counsellor, but puts forward a clinical notion of expertise that is 'counter' to measurement (the group is discussing the problem of a child who asks for advice in answering a question in a test):

> *CB So how, when a child says that, like 'I can't say if that's true or false', what… do you talk to them about that, how do you respond to them?*

> *NANCY I try and unpack it with them, but if they can't come to a, you know, a either/or, then I let it go, 'cause that's where they're at. I don't want to be putting them into a true or a false when I, well…I don't believe, I don't like the testing, so I'm not wedded to the testing, and um,*

> *SALLY? Kind of runs counter to the rest of the stuff you might be doing in the room…*

NANCY It's totally counter, and I'm a process orientated counsellor and when we've just got a content, you know, 152 questions that are content, a lot of content based questions, then philosophically it's difficult for me to give the tests, so that's why I'm not, you know…I might, sometimes I might even read out the questions and get them to tell me what the answer is so I can engage with them in some way.

Again, later during a discussion about the evaluative connotations of the word 'normal' in psychological testing, Nancy cites her clinical skills as enabling her to understand the child she is working with:

NANCY I don't go for the graph myself

KATE But if we're talking about a graph

NANCY I kind of,…sorry?

KATE No but if you're talking about a graph

NANCY Yeah, and when I do the BASC I look at the graph and I see that it's all in the lines, well [laughs] I just

KATE I don't see that there's something wrong with that, I mean if you're saying Ok it's within that, and you know that's where there's concern that there might be, that that level of anxiety is….higher than expected

> *NANCY I can't, well…for me I think that I would pick*
> *it up anyway when I'm working with the child. I don't*
> *need a, I don't need the test to be able to support my*
> *understanding of what the child is like.*

Note that in relation to Nancy's last statement above that psychometrics has a concept of 'incremental validity' to describe this situation, where a test does not furnish any new information over and above the clinical assessment (Lilienfeld, 2006). Thus, one could interpret Nancy's statement as an indication that for her, the BASC generally lacks incremental validity.

For the current purposes, however, two general points can be made in relation to these discussions. The first is simply that therapy, or more specifically child sexual abuse counselling, is a heterogeneous collection of discourses and practices that participate in the production of an object of knowledge and subject of intervention: the child victim of sexual abuse. Neither Nancy, nor Elizabeth, nor Kate claims a monopoly on therapeutic knowledge per se. The second point is methodologically instructive. Following the notion that power is not a possession or a substance, but is more a flow of relations, of forces, one can speak of various 'apparatus' through which power is dispersed; a rationality, a network of 'broader political logics' (Hook, 2007, p. 234). The use of electrical current metaphors for the flow of power can be found in Foucault's lectures on psychiatric power (2006/1973-74). Within disciplinary apparatus of therapy, the counsellor is a kind of 'node' or relay point through which

power/knowledge flows[79]. Whilst relay points are necessary components of the networks of disciplinary power, they also represents points of potential resistance or disruption; whilst the image of a power that 'flows' is useful, the flow is not always smooth. In relation to this point, consider that Foucault explicitly stated that he intended his work to promote 'paralysis' in social workers (Foucault, 1991c; this discussion will be referred to later in relation to a different point). Rather than being a simple conduit of disciplinary power (as suggested in the term 'anaesthetised'), the paralysed social worker clearly represents a point of disruption for the flow of power. This notion has implications for the ways in which the subjectivity of counsellors can be analysed; if it is possible for a social worker to be paralysed (in the sense, described by Foucault, of not knowing what to do), it is clear that subjectivity is not a straightforward or rational process of either infolding or transmitting knowledge/power relations.

Summary of Chapter 7

In this chapter I have examined more closely the effects of the claim that psychological measurement is an objective practice for investigating the effects of child sexual abuse. I have attempted to demonstrate that objectivity is not a homogenous concept, but is historically constituted in material scientific arrangements. I suggest that objectivity has a specific meaning in the practice of

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[79] Hook (2007) uses the term 'micro-sovereignties' to describe these 'officers or agents' (p. 244). Yet the term 'sovereignty' would appear to run contrary to the point he is making, in the sense that, while stressing the agentic subjectivity of these agents, he nonetheless refers to them as what "makes the broader architecture of state control possible" (p. 245), which would appear to imply a more straightforwardly sovereign model of power than he has been advancing in his argument.

psychological measurement that calls upon researchers and counsellors to conduct themselves in particular ways to create a specific context in relation to research subjects. My emphasis has been on the entanglement of the researcher/counsellors—and the measurement instrument itself—in the measurement phenomenon, rather than their separation as espoused in some classical versions of objectivity.

I have also provided some historical context for the resistance to or discomfort with objectivity, measurement and statistics in the professions of medicine, insurance and counselling. I did this to try and suggest a sense of both continuity and specificity in the difficulties faced by the counsellors in the focus groups. Resisting one kind of expertise necessarily entails another preferred set of expert claims, as was evident in the historical examples and the focus groups.

An ending

The end of this thesis is a kind of enacted, agential cut (Barad, 2007). I am in a position to take responsibility for this decision, in the context of the pragmatic demand that this text be wrapped up, bounded and passed on as a fixed document. I am aware at this moment of the threads I have not picked up, or not followed as far as perhaps they might have been. To give one example; I would like to explore further the ways in which statistics have enabled the actualisation of elements of 'societies of control' (Deleuze, 1992) in the name of preventing men's violence against women and children. The technologies of risk management described by Castel (1991) are becoming more evident in the field of violence prevention, with particular consequences for children who have been subject to abuse (including sexual abuse); such children, particularly boys, are configured as at-risk for committing future violence (Victorian Health Promotion Foundation, 2007). This depends on the privileging of statistical ways of knowing. This angle of analysis would provide a way of approaching the question about the 'cycle of abuse' I posed at the very beginning of the process (as I discussed in Chapter 1).

Another thread to follow would be the ways in which scientific practices of measurement are being implicated in the contemporary efforts to locate trauma 'in the body' (Baldwin et al., 2004). I briefly discussed brain imaging technology as one example of this. Research in the fields of neurobiology and physiology is increasingly being taken up by professionals in the treatment of those who have been subjected to child sexual abuse. Much of this research is concerned with measuring substances in the body, such

as the flow of blood in the various regions of the brain, or of particular chemicals in the bloodstream itself.

Despite what I may be leaving 'undone', I think I am in a position to provide some tentative answers to the questions I posed at the start:

How have techniques of psychological measurement and statistical analyses been made to function as objective methods for determining the effects of child sexual abuse?

Appeals to objectivity are repeated throughout the research literature on the effects of child sexual abuse, including early work by the likes of David Finkelhor (Finkelhor, 1986). Objectivity is widely regarded as an essential trait of scientific work, and science is regarded as the premier means through which knowledge claims establish legitimacy. The linking of measurement and science has played an important part in the proliferation of psychological measurement, including in the field of child sexual abuse. This is despite a number of obstacles, including: 1) the contested claims around whether or not psychological attributes are technically amenable to measurement; and 2) criticisms of the very notion of objectivity as inherently problematic.

Even at this point, having investigated this very question, I admit to struggling to understand the imperative that research in the field of child sexual abuse should be objective, if 'objective' means dispassionate and non-influential. Surely *compassion,* not dispassion, is called for? It seems even stranger to me now that there are calls to specify precisely 'how much' harm child sexual abuse might cause. At the same time, it has been important to

acknowledge that these very efforts, to establish how much harm, have constituted an integral component of the professional response to the suffering endured by those who have been subjected to abuse. Whatever the shortcomings of claims to objectivity and practices of measurement, and however successfully or otherwise these ideals and practices have been performed, they have been crucial elements of establishing the legitimacy of this suffering.

What relations of power/knowledge do practices of measurement require and produce? What are the 'conditions of possibility' for psychological measurement?

The question of power has been made extremely complex and slippery through the work of Foucault and the development of an analytics of power. There can be no scientific knowledge of child sexual abuse without relations of power, and these relations are made relatively visible in psychological measurement. This raises difficulties in the field of child sexual abuse, where the understanding that power relations can be harmful is influential.

Any practice of measurement in the classical sciences contains a set of implicit assumptions: the separation of agent and object; the existence of discreet properties and traits; the presumption that properties found through the phenomenon of measurement pre-exist the measuring; and the ability of the measuring device to measure what it is intended to. The contributions of authors in the field of science studies (I have drawn particularly on Karen Barad) question these assumptions while offering productive alternatives as to how to analyse practices of measurement. The notion of entanglement (Barad, 2007) has been particularly useful in

analysing the attempts to measure the harm of child sexual abuse, and how this implicates researchers and counsellors, as well as children and adults who have been subjected to sexual abuse, in relations of power. If it is feasible to suggest that all attempts to gain knowledge depend upon relations of power, I think the notion of entanglement provides a helpful way to investigate how counsellors try to account for their involvement in situations that are sometimes in conflict with their preferred ethics or practice as professionals in the field of child sexual abuse.

This leaves the problem of whether power relations themselves should always be contested, as they are implicated in abuse; or whether some relations of power are legitimate, either for ontological reasons (e.g. child development perspectives), for therapeutic reasons (therapists exercise power benignly), or for epistemological reasons (relations of power are justified on the grounds of better knowledge). Or, if power is understood more as a productive force that enables rather than oppresses, there is no need to oppose power *per se*. In this case, one is left to grapple with the complex debates around power and domination, which has the effect (for me, at least, in this thesis), of unsettling the grounds upon which many claims about the harm of child sexual abuse are made in therapeutic and research settings.

What kinds of individuals are 'produced' by psychological practices of measurement, in the study and treatment of child sexual abuse?

The two main kinds of people that emerged through this investigation are, simply, those who are in the category of normal, and those who are not. Although statistical normality technically

has no moral judgement attached to it, statistics do not exist in a vacuum. The measuring research on child sexual abuse constantly compares the abused child (or the adult abused as a child) to the statistically normal child (or adult). Historically, sexually abused children have been deemed abnormal in varying ways via legal, medical, child protection and 'psy' disciplines, with responses ranging from punishment and separation (Armstrong, 1996; Smart, 1999), to caring and compassionate efforts to ameliorate their suffering (as evidenced in my focus groups). Although of course there are range of forces and discourses at work here (other than psychology and measurement), I suggest it is helpful to revisit the historical context whereby statistical differences from the norm were conceptualised as errors to be corrected.

A second way of describing kinds of people, following Hacking (1995, 2004), is to look at the attributes that a particular instrument uses to define people. People are anxious or not anxious, people are depressed or not depressed, children suffer from traumatised sexuality or they do not. Through measuring a conglomerate of attributes, people are produced in the terms of such research not as 'memorable persons', but as a set of calculable traits locatable in reference to the norm (Hacking, 1991, 1995). Identifying people along these lines, within a scientific 'style of reasoning' (Davidson, 2001) makes it possible to legitimise therapeutic intervention into their lives, whether this is sought by the individual themselves (Rose, 1998) or not (Ashenden, 2004; Warner, 2009).

How do techniques of measurement govern the activities of counsellors in their work with children who have been sexually abused?

As I just mentioned, objectivity is a commonly made claim in support of the use of psychological measurement as research practice. While it is possible to be critical of this claim by undermining the notion of objectivity as separatism, I have suggested in this thesis that objectivity can be understood as a way of conducting oneself. Furthermore, by following the suggestion that objectivity itself is an historically constituted material practice (Daston et al., 2007), rather than a timeless, placeless ideal, it is possible to identify how objectivity *works* in specific settings (such as conducting a psychological assessment using a standardised instrument in a counselling setting).

As my focus groups demonstrate, objectivity is sometimes an uncomfortable way of conducting oneself. This is especially so when this contributes to the apparent distress of the child sitting in the counselling room. Yet, in order to utilise the measuring instruments, the counsellor must engage in some way with the directive to be objective. This may take the form of outright resistance by constituting one's self through a different set of knowledge claims about good therapy, or it may entail attempting to grapple with the tensions inherent in the phenomenon, perhaps making compromises here and there while understanding that such relations of power may be productive for therapeutic work. What I have tried to contest is the view that the counsellors I spoke with are simply "servants of moral orthopaedics" (Foucault, 1977, cited in Hook, 2007, p. 40). I have attempted to convey a much greater

sense of agency and ethical commitment than this characterisation would suggest.

By following the principle of trying to account for the phenomenon of psychological measurement as a legitimate way of knowing about the effects of child sexual abuse, very little can be taken for granted. Surprising continuities and discontinuities appeared throughout the course of my investigations. I have argued that it is possible to identify two theoretical systems with the effects of child sexual abuse as their object (post-structural feminism and scientific psychology), enabling Barad's *diffractive reading* (2007). At the same time, it is not possible to step outside the *èpistèmé* that is capable of producing both these systems (confirming Foucault's position that one cannot grasp the current *èpistèmé*). Both systems are influential and legitimate ways of producing and speaking about child sexual abuse, despite being in disagreement over some significant questions. As I have traced in this thesis, a key area of difference is regarding the importance of measurement. The ongoing engagements between these systems will continue to produce and shape the field of child sexual abuse.

However, having reached this juncture, I should stress here that I do not claim to have established any fixed answers about the 'best' way to understand and respond to the effects of child sexual abuse, but I rather hope to have produced some unsettling yet relevant questions about the ways this work is currently understood and done.

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APPENDIX ONE- LIST OF CONSTRUCTS IN THE BASC 2, AND BRIEF OUTLINE OF THE DEVELOPMENT OF THE BASC

The original BASC (Behavior Assessment System for Children) was first released in 1992 by Reynolds & Kamphaus. It consists of several different sets of questions regarding a child's behaviour: a Parent Rating Scale (PRS), a Teacher Rating Scale (TRS), a Self-Report of Personality (SRP), a Structured Developmental History form (SDH), and a Student Observation System (SOS), to be completed by the appropriate person in each case (the parent, the child, the teacher, etc.). The Self-Report of Personality, for example, has 152-186 questions (depending on the age of the child), with each question to be rated on a 1-5 Likert scale corresponding to the frequency of the particular behaviour or thought. The revised BASC 2 appeared in 2004, with some changes to the various scales and questions. (See Rescorla, 2009 for more on the development of the BASC).

Each question is connected to a scale, listed below. Each scale is considered as either an externalising problem, an internalising problem, or an adaptive skill. The total score on each scale will indicat whether the child will be considered to be in the normal, at-risk, or clinically significant range for each construct. Statistical techniques are utilised to check the answers of the various completed questionairres for both internal validity (i.e. similar answers are given for similar questions), and for inter-rater correspondance.

One reason for the use of the BASC in this agency is it's ability to be administered by workers other than clinical psychologists.

However there was some dispute among the team as to whether other professionals (e.g. social workers) might be regarded as unqualified to adminster the test if it were to become a contested issue in a court case.

Externalizing Problems	Internalizing Problems	Adaptive Skills
Hyperactivity	Anxiety	Adaptability
Aggression	Depression	Social Skills
Conduct Problems	Somatization	Leadership
	Atypicality	Study Skills
	Withdrawal	Functional Communication
	Attention Problems	Activities of Daily Living

Further detailed information on the BASC2 is freely available through the 'Sample Reports' section on the publisher's website: http://www.pearsonclinical.co.uk/Psychology/ChildMentalHealth/ChildADDADHDBehaviour/BehaviorAssessmentSystemforChildrenSecondEdition%28BASC-2%29/BehaviorAssessmentSystemforChildrenSecondEdition%28BASC-2%29.aspx (accessed 17 Jan, 2013)

APPENDIX TWO: INFORMATION PROVIDED TO FOCUS GROUP PARTICIPANTS

The research project is an investigation into how practices of measurement (i.e. the use of psychological/psychometric tests) are used in counselling or treatment in the field of child sexual abuse. The research will investigate how (if at all) practitioners link these types of measurement to their clinical work with child victims of sexual abuse.

The results of the research are intended to be a theoretical contribution to the field in the context of growing demand for quantitative data about therapeutic interventions (e.g. evidence based practice, the increasing use of standardised testing in treatment).

Invitations to participate in the focus groups will be extended to people currently working as counsellors, therapists or clinicians in the field of child sexual abuse. That is, purposive sampling will be the approach to recruitment of participants. A small token of appreciation (a book voucher) will be offered to participants.

The plan is to conduct focus groups of 4-6 participants for sessions of 60-90 minutes.

The focus group participants will be asked to explore the following questions and topics:

- Identifying particular psychological tests that therapists use in their work with child victims of sexual abuse

- Discussing the details of these tests, such as what 'traits' they are used to measure, what kinds of clients they are used with and why they are used
- Explore the details of how these tests measure the traits they claim to measure
- Discuss how the process of testing is managed within therapy
- Discuss how the therapists use the results of these tests in the formulation of therapeutic plans and goals with their clients
- Discuss therapists views on the ethics of testing, for example, what considerations must one be attuned to in order to properly conduct psychological testing
- Are there any particular organisational requirements related to the use of tests (for example, are the use of some tests routine with certain clients?)
- Discuss the therapists views on the philosophical dimensions of psychological testing through prompt questions such as:
    - Are psychological attributes quantitative in structure?
    - What power dynamics are involved in administering tests?
    - What is the relationship between the use of standardised testing and the exercise of clinical judgement in therapy?
    - In what ways do therapists find the use of testing helpful or unhelpful in their practice as therapists with child victims of sexual abuse?
    - What are therapists views on how children might experience the practice of psychological testing?

The nature of the information and opinions being sought is professional; no disclosures of personal experiences (i.e. outside of the professionals context) will be requested or expected. The names of individuals and organisations will be changed or excluded in any published material.

The data from the focus groups will be analysed to identify the discourses available to counsellors, therapists and clinicians in their use of, or resistance to the use of, psychological/psychometric tests in their work. Thus, the intent is not to draw out certain points of view in order to criticise them, but rather to attempt to identify the various frameworks (be they theoretical, ethical, pragmatic, or administrative) that are called upon by counsellors, therapists and clinicians in their use of such tests. In other words, the style of data collection and analysis falls broadly under a 'discourse analysis' approach.

The focus groups will be recorded with a mini-disc recorder, and the discs stored in accordance with Deakin University guidelines (which requires retaining the material for a minimum of 6 years). The focus group discussions will be transcribed by the research candidate, and distributed to participants to check for accuracy. When a correct transcription is achieved, the data will be analysed to identify themes which will be used to develop and support the overall theoretical position of the thesis. Quotes from the focus groups will be used, and in the thesis will be attributed to fictitious names.

Participants are quite free to participate or not to any extent, and quite free to withdraw at any time. If participation is withdrawn, information gathered will not be used and either destroyed or returned or destroyed within a reasonable time thereafter. Participants will be given the opportunity, if so desired, to view the transcriptions of the focus groups and make any corrections regarding their contributions (e.g. misheard comments, inaccurate portrayal of comments made).