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**Memories are Made of This:  
Exploring Argumentation in Popular Texts.**

A thesis presented in partial  
fulfilment of the requirements for the degree  
of

Doctor of Philosophy  
in  
Psychology

at Massey University, Palmerston North,  
New Zealand.

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2004

## Abstract

The role of discourse in the construction of institutional and academic knowledge is now recognised within a wide variety of theoretical perspectives, including social constructionism, the sociology of scientific knowledge, the rhetoric of inquiry and discursive psychology. The purpose of this study was to examine the ways in which such discursive knowledge construction practices occur in relation to psychological phenomena. The site of this investigation was the highly contentious debate surrounding the reality of repressed/recovered memories of childhood sexual abuse. Ten 'popular' psychology texts (five supporting the concept of recovered memories and five questioning it) were discursively and rhetorically analysed in order to gain an understanding of how the authors of these texts deployed arguments to support their own positions and undermine those of their opponents.

Five broad rhetorical resources were identified as being prominent in the texts, each of which was examined in detail to determine more specifically the source of their persuasive power. The five resources included authorial credibility, definitions, science, history and personal experience. Despite the meta-rhetoric surrounding the debate, which suggests that it is essentially an argument between researchers (drawing on scientific evidence) and clinicians (drawing on clinical experience), what was apparent was that *all* of these resources were utilised to varying degrees to support both pro- and anti- recovered memory positions. This analysis suggested that a reasonably structured set of discursive resources were available for making arguments about the nature of psychological phenomena. Furthermore, when given the opportunity, rhetors utilised as many of these resources as possible in order to produce a convincing argument, even when this resulted in inconsistencies within their texts. It was concluded that in the memory debate, the demands of the rhetorical imperative (to persuade the audience) often appear to be paramount, and should not be discounted by those seeking to understand this difficult and often distressing topic.

## Acknowledgements

As anyone close to me knows, the process of producing this work has been a long and difficult one – a real test of my resources, not only intellectually, but emotionally as well. Throughout, I have been supported by many encouraging and understanding people.

In particular, I thank my supervisor, Dr Keith Tuffin, whose patience seems to have no limits, and whose comments were always pertinent! Thanks also to my second supervisor, Dr Mandy Morgan, who willingly provided advice and assistance when needed.

I am grateful to Massey University for providing the financial support which enabled me to undertake this project. I am also indebted to my current employer, The Open Polytechnic of New Zealand, and especially my manager, Dr Graeme Beaumont, who made time and resources available to assist me in the completion of this thesis, as well as providing daily reminders that I should be writing!

To all my fellow students, friends and colleagues at Massey and The Open Polytechnic – thank you! Often the only things which kept me going were your humour, helpful suggestions and assurances that I could do it and it would all be worth it in the end.

Finally, I want to express my love and gratitude to my parents, Dale and Maurice Howard, whose unfailing affection and support I appreciate now more than ever.

# Table of Contents

<b>Abstract</b> .....	<b>ii</b>
<b>Acknowledgements</b> .....	<b>iii</b>
<b>Table of Contents</b> .....	<b>iv</b>
<b>Chapter One: Introducing the Memory Debate</b> .....	<b>1</b>
A Brief and Partial History .....	3
From Past History to the Present Research .....	26
<b>Interlude: Recovered Memories and Me</b> .....	<b>28</b>
<b>Chapter Two: Alternative Approaches to the Memory Debate</b> .....	<b>30</b>
Re-thinking Memory, Confabulation and Repression .....	31
Feminism and False Memory Syndrome .....	37
Looking at Language: Analysing Accounts Instead of Ideas .....	43
Precedents and Opportunities .....	45
<b>Chapter Three: Epistemology, Methodology and Ethics</b> .....	<b>47</b>
Social Constructionism .....	47
Discourse Analysis .....	51
Discursive Psychology .....	54
Rhetoric in Various Guises .....	58
My Methodology .....	61
Memory, Truth, Reality and Social Constructionism.....	62
<b>Chapter Four: Doing the Research: Selection, Interpretation, Inscription</b> 72	
Refining the Focus of the Analysis .....	72
A Selection of Texts .....	75
Analysing the Texts .....	79
Writing the Research.....	81
<b>Interlude: That sounds very interesting ... but what do you really think?</b> 85	
<b>Chapter 5: Constructing and Undermining Authorial Credibility</b> .....	<b>89</b>
Discursive and Rhetorical Approaches to Authorial Credibility.....	89
Working Up Category Memberships.....	91
Managing Issues of Stake and Interest .....	94
Category Condemnations.....	98
<b>Chapter 6: The Strategic Deployment of Descriptions and Definitions</b> ... 103	
Reifying Recovered Memory .....	105
Ironising Recovered Memory .....	112
Reifying False Memory.....	125
Ironising False Memory .....	133

<b>Chapter 7: Science as a Rhetorical Resource.....</b>	<b>140</b>
Science on their Side .....	141
Lost in a Shopping Mall .....	154
Williams' Prospective Survey .....	162
Problems with Science .....	167
<b>Chapter 8: History as a Rhetorical Resource.....</b>	<b>172</b>
The Historical Origins of Repression .....	173
Repression on the Battlefield .....	184
The Cultural Context of Recovered Memory .....	188
Historical Events and History Itself .....	195
<b>Chapter 9: Personal Experience as a Rhetorical Resource.....</b>	<b>199</b>
Experience and <i>Ethos</i> .....	199
Experience and Emotion .....	205
Scientific Warranting and Experience.....	211
<b>Chapter 10: The Rhetorical Context of Recovered Memories .....</b>	<b>214</b>
The Analysis: An Evaluative Overview .....	214
What Makes Popular Texts Persuasive?.....	219
Producing Psychological Knowledge in a Rhetorical Context .....	222
Looking Back.....	224
Looking Forward.....	225
And finally ... ..	226
<b>References .....</b>	<b>228</b>

## Chapter One: Introducing the Memory Debate

For the last five years I have been immersed in the murky world of traumatic memories. I have read about memory, thought about memory, and written about memory. The purpose of my involvement has been to explore the ways in which knowledge about traumatic memory has been constructed and contested over the last two decades, and the 'memory debate' has provided the discursive site for this exploration. This debate, about whether memories of childhood trauma can be repressed and then later recovered, has been one of the most contentious issues in modern psychology, and many key issues have yet to be satisfactorily resolved. Despite this, I have never intended in this research to become an active participant in this dispute (by generating more knowledge about traumatic memory). Rather, my object has been to *examine* the conduct of the debate in order to gain some insight into the workings of a psychological controversy.

The aim of this introductory chapter is to familiarise the reader with the memory debate, in order to provide a context for the analysis which follows. As it has unfolded over the last twenty years or so, a vast amount of information about traumatic memory has been generated and disseminated in a wide variety of communicative contexts. A comprehensive survey of all of this material would be far beyond the scope of this thesis. Consequently, what is presented here is my attempt to concisely summarise the major developments in the debate, from the late 1970s through to recent years. For reasons of clarity and manageability, I have focused mainly on the academic literature and popular texts, with the intention of providing a description of the main issues and ideas in a more or less chronological sequence.

In the writing of this history, I found that one of the difficulties associated with producing a linear account of the memory debate is the existence of a number of related controversies, such as those surrounding the etiology of dissociative identity disorder (formerly known as multiple personality disorder), the prevalence and factuality of child abuse reports, and the existence of satanic or ritual abuse. As other observers have noted, there are many points of

interconnection between these various debates, which are all grounded in issues of child sexual abuse (Hacking, 1995; Webster, 1995). Concerns which originate in one area are likely to 'migrate' into another (Hacking, for example, links the reports of ritual abuse related to dissociative identity disorder to earlier child abuse cases). However, in the interests of simplicity and maintaining focus, I have elected to concentrate upon the memory debate *per se* in the following historical narrative, rather than following through on all of these tangentially related issues. Similarly, I have avoided discussing research which, while it has been generated as a result of the debate, is not actually central to the main issues (for example, studies of the malleability of children's memory, and research relating to legal issues such as juror decision making).

Furthermore, I want to stress that this is a history of the debate itself, rather than a history of repression or child abuse. The historical origins of repression and recovered memories are an important resource in the debate itself, and have been extensively explored by both recovered memory (rm) and false memory (fm) supporters (see, for example, Herman, 1992, and Pendergrast, 1995). Consequently, these histories are themselves treated as data for analysis in this thesis. Thus, it does not seem appropriate to elaborate on this material here.

In highlighting these restrictions and limitations of this history, I by no means wish to imply that what is provided is a complete or exhaustive summary of the remaining literature on traumatic memory. As I will argue in Chapter Eight, all histories are inevitably partial and selective, as authors construct the past to have certain rhetorical effects in the present. Thus, I acknowledge that the history which is presented here is only one of a myriad of possible ways of telling this particular story. As the reader might expect, my selection and ordering of this material is designed to provide a context in which the analysis which forms the heart of this thesis appears both consistent and convincing. As I have aimed to undertake a symmetrical analysis of the debate (as explained in Chapter Two), this history therefore attempts to present an even-handed recounting of the development of the arguments on both sides of the debate.



## A Brief and Partial History

### *A possible beginning*

In the mid-1970s, Harvard University psychiatrist Judith Herman began her work with the Women's Mental Health Collective in Sommerville, Massachusetts (Herman, 1997). Herman, whose training was psychoanalytically based (as was common for American psychiatrists at the time), worked mainly with women who had been sexually abused in childhood, often by male relatives. In 1977, she published, with co-author Lisa Hirschman, a paper outlining a feminist theoretical perspective on incest. In this paper the authors claimed that Freud's repudiation of his initial theory that hysteria was caused by sexual contact with the father was the most famous and important denial of the reality of incest (a similar argument can be found in Rush, 1977 and Masson, 1984). They also suggested that incest was far more widespread than was commonly believed. Furthermore, Herman and Hirschman argued that the reason the vast majority of incest is perpetrated by adult male caregivers on dependent female children is the patriarchal organisation of society. As well as outlining this novel (for the time) theoretical perspective, the paper also presented a clinical case study of incest survivors, some of whom were treated by the authors. They concluded from this case study that group therapy was likely to be useful for incest survivors, as being able to meet with other similar individuals and tell their stories would dispel the feelings of shame and secrecy surrounding abuse issues.

Herman and Hirschman's (1977) paper is highlighted here because it outlines the theoretical and clinical perspectives which appear to have formed the basis of the memory recovery movement. In this paper, however there is only one specific instance of repression mentioned – and that occurs incidentally, as part of a quotation which is included to show the general lack of empathy of male psychotherapists for incest survivors:

In analysis [the client] talked at first only of her good feelings toward him because she had blocked out the sexual episodes. When they were finally brought back into consciousness,

all the fury returned which she had experienced at the age of thirteen. (Stein, 1973 in Herman & Hirschman, 1977, p.753).

What is perhaps most striking about this reference is its incidental nature. It does not require explanation by Herman and Hirschman, or by the original author, Stein. This seems to indicate that the idea that memories of actual sexual abuse (rather than just Oedipal sexual fantasies) could be repressed and then later returned to consciousness was accepted at the time by at least some psychoanalytically orientated therapists (see also Schuker, 1979).

### *A developing movement*

As time went on, the phenomena of repression and the recovery of memories came to play a much more prominent role in Herman's writings about her work with incest survivors. In 1981 she published *Father-Daughter Incest*, which reiterated and expanded upon the material of her 1977 paper. This was followed by two papers in which Herman published accounts of treatment groups which she and a co-facilitator had provided for incest victims, acting on her earlier conclusion that group therapy would prove beneficial (Herman & Schatzow, 1984, 1987). These groups included not only women who had always remembered being abused, but also women who believed that they had been abused although they had no substantive memories. Herman and Schatzow describe how, over the course of the time limited treatment (10-12 weeks), the stimulation of hearing other women's stories allowed group members to retrieve memories where previously they had had none. In some cases, where recall had always been present but was incomplete, additional memories were recovered.

A version of Herman and Schatzow's 1984 paper had been presented at an American Psychiatric Association meeting two years earlier, indicating that this form of group therapy, incorporating recovery of repressed memories of sexual abuse for some participants, probably began sometime in the late 1970s or early 1980s. According to O'Hare and Taylor (1983) similar groups for incest survivors were also being set up in other parts of the United States around this time. Webster (1995) suggests that this type of therapy was quickly popularised through informal therapist networks, as well as through the more traditional

route of publications and conferences. Indeed, Herman, along with her co-authors, (1977, 1984, 1987, 1997) makes frequent reference to these networks, and credits them as being instrumental in the development of her group therapy model for incest survivors, noting that "Among practitioners working in this field, we have encountered a strong oral culture which supports the idea of group treatment" (1984, p. 606).

These networks undoubtedly contributed to the increasing popularity of memory recovery, but were not the only means by which these ideas were spread. The general public became well informed about repression and memory recovery through magazine and newspaper articles on the subject, and through television talk shows, documentaries and dramas (Pendergrast, 1995). Survivor autobiographies, such as Sylvia Fraser's *My Father's House* (1987), also began to describe experiences of memory repression and later retrieval. Throughout the 1980s the recovery movement flourished.

#### *Publication of the 'Bible'*

This flowering resulted in the 1988 publication of *The Courage to Heal*, which is often referred to as the 'Bible' of the recovered memory movement (usually by detractors – see Loftus, 1993; Pendergrast, 1995; Tavris, 1993; Webster, 1995). According to its authors, Ellen Bass and Laura Davis, work on the book began in 1984. Bass explains that she had been running creative writing workshops for women since the early 1970s, and from 1974 onwards an increasing number of women in her workshops were writing about their experiences of childhood sexual abuse. In the early 1980s a collection of these writings was published (Bass & Thornton, 1983). Following on from this Bass began to offer writing workshops designed specifically for sexual abuse survivors. She stressed that the material in *The Courage to Heal* was a result of this work: "The process described, the suggestions, the exercises, the analysis, the conclusions, all come from the experiences of survivors" (Bass & Davis, 1988, p. 14). *The Courage to Heal* was a self-help book, written for survivors, which explained the process that women go through in recovering from child sexual abuse, and offered advice on how to manage this process successfully. It addressed women who had always remembered their abuse, but it also took

for granted that some women might not remember their abuse occurring. The authors explained that “Children often cope with abuse by forgetting it ever happened. As a result you may have no conscious memory of being abused” (Bass & Davis, 1988, p. 22), but you should

Assume your feelings [that you were abused] are valid. So far, no one we've talked to thought she might have been abused, and then later discovered that she hadn't been. The progression always goes the other way, from suspicion to confirmation. If you think you were abused and your life shows the symptoms, then you were. (Bass & Davis, 1988, p. 22).

In a chapter entitled *Remembering* (pp. 70-86) they discussed why women may not remember being abused, and what the process of remembering feels like. They also offered advice on how to go about regaining memories, noting, however, that some women who were abused may never have any memories. But they assured readers that this lack of memories did not mean that they were not abused. Instead, they advised women to trust their intuition and ‘act as if’ they were survivors.

Following the publication of *The Courage to Heal*, memory recovery continued to grow in popularity. The movement was publicised by a number of high-profile celebrity cases of recovered memory which were given wide coverage in popular magazines and newspapers (Loftus, 1993). More self help books for abused women were published, including Blume’s *Secret Survivors* (1990; see also Farmer, 1989; Poston & Lison, 1990) and a volume that focused specifically on how to recover repressed memories of abuse (Fredrickson, 1992). Herman, the pioneer of repressed memories, published *Trauma and Recovery* in 1981. In this book she explicitly linked child abuse to other types of trauma (such as that caused by war or torture) and suggested that a new diagnosis of “complex post-traumatic stress disorder” (p. 121) was required for victims of such prolonged and repeated suffering. In addition, Herman also proposed that childhood dissociation might be responsible for adult amnesia for abuse memories. This idea was also proposed in some of the many articles on repression and recovery of abuse memories that began to appear regularly in clinical journals and texts (see, for example, Courtois, 1992; Laikan, Winston &

McCullough, 1991; Malmo, 1990; Reik, 1991; Shapiro & Dominiak, 1990; Siegal & Romig, 1990; van der Kolk & van der Hart, 1991).

### *Doubts arise*

In spite of the support that the recovery movement was receiving during this period, there were also signs that problems were on the horizon. One of the more disturbing aspects of the movement was the recovery of repressed memories of what came to be called 'Satanic Ritual Abuse' – sexual abuse and torture that was said to occur in the context of organised Satanic worship. According to Hacking (1995), during the late 1980s "Satan had become the star of American television talk shows" (p.114). Many people found these memories too fantastic to be believable, with the corollary that the credibility of the entire recovery movement began to be undermined.

A particularly disturbing case occurred in 1988, when Paul Ingram was accused of ritual abuse by his daughters on the basis of recovered memories. After an intense period of questioning, Ingram confessed to the abuse. Richard Ofshe, a professor of sociology and an acknowledged expert on cults, was called in to examine Ingram. In the course of his contact with Ingram, he came to believe that all of his confessions were coerced, even though Ingram himself believed them. Ofshe tested his theory by conducting a 'field experiment'. He created a specific abuse incident which he reported to Ingram, saying that the accusation had come from one of his children. Using the same techniques that had led to his earlier confessions, Ingram later produced a detailed written confession of the incident, which he refused to withdraw even when Ofshe revealed that he had invented the entire episode. This incident convinced Ofshe that all of Ingram's confessions were false (although sincere), along with the allegations which prompted them. Ingram, however, was not convinced. He pleaded guilty to the charges made against him, and was imprisoned. Not surprisingly, articles on this case which later appeared in both popular and academic forums began to cast doubt on the veracity of recovered memories (Ofshe, 1989; Ofshe, 1992; Watters, 1991).

Two years later another high profile legal case raised further questions about recovered memories (in spite of the conviction of the accused perpetrator). In 1990, George Franklin was accused of the 1969 murder of 8-year-old Susan Nason on the basis of the recovered memories of his daughter, Eileen Lipsker. During the trial, Dr Leonore Terr appeared as an expert witness for the prosecution, explaining how traumatic memories could be repressed and then recovered many years later. In contrast, Dr Elizabeth Loftus, appearing as an expert witness for the defence, stressed the malleability of memory, and the many ways in which eye witnesses can be mistaken as a result of misleading post-event information. Dr Loftus was a well respected cognitive psychologist who had devoted much of her professional life to examining the unreliability of eye witness memory, and had presented this material as an expert witness at many previous trials (Loftus & Ketcham, 1991). However, this was the first time she had ever been involved with a case that was based upon repressed memories, and, by her own account, her subsequent involvement in the recovered memory debate was prompted by her participation in the Franklin trial (Loftus & Ketcham, 1994).

#### *A pivotal year*

Both of these trials contributed to a growing debate in the media about the nature of recovered memories (Berliner & Loftus, 1992). This debate culminated in 1992 in several significant milestones, including the formation of the False Memory Syndrome Foundation, the publication of the first academic critiques of the validity of recovered memories (Baker, 1992; Ellis, 1992), and the establishment of a newsletter by and for 'retractors'. Kelly and Kelly (1994) believe that the appearance of retractors – people who had come to believe that their own repressed/recovered memories were fabrications – was a significant blow to the credibility of the recovered memory movement (though, as Mollon, 1996, points out, retraction of an accusation does not necessarily mean that the accusation was untrue). Retractors described intensively coercive individual and/or group therapy, being encouraged to confront parents with accusations of abuse, and of becoming more unhappy and unwell during their treatment, often resulting in hospitalisation for mental illness (Gavigan, 1992; Gondolf, 1992). Frequently, retractors compared their therapeutic experiences to being in a cult

(see Goldstein & Farmer, 1993). Many told their stories in the newly established newsletter *The Retractor*, responsibility for which was later assumed by the False Memory Syndrome Foundation (FMSF).

This organisation was established in 1992, by Pamela and Peter Freyd, as an advocacy group for parents who had been accused of abuse on the basis of recovered memories. The previous year, Pamela Freyd had written an account of her daughter Jennifer's incest accusations against her father, accusations that Pamela maintained were totally false and unfounded. This account was sent anonymously by Pamela to members of the psychology department at the University of Oregon, where Jennifer was employed as an associate professor (specialising in cognitive and perceptual psychology). Later in the year, the article was published in the journal *Issues in Child Abuse Accusations*, still under a pseudonym (Doe, 1991). In 1992, an entire issue of this journal (Volume 4, Number 4) was devoted to articles critical of repressed and recovered memories. The founders and editors of the journal, Ralph Underwager and Hollida Wakefield, were among the first members of the Scientific and Professional Advisory Board of the FMSF. However, Underwager resigned from the Board the following year (1993), after claims from opponents that he had made comments in a Dutch journal which indicated that he supported paedophilia. Underwager vigorously denied this, claiming that his comments were taken out of context. (For more on this controversy, see Lawrence, 1993 a, b, c, 1994; Underwager & Wakefield, 1994).

### *Loftus speaks out*

Also appointed to the Advisory Board was Elizabeth Loftus, who had recently given an invited address to the 100<sup>th</sup> Annual Convention of the American Psychological Association (APA) arguing that supposedly recovered memories could well be the product of therapeutic suggestion (Loftus, 1993). Loftus, however, was careful to note the logical limitations of her argument – several times she stressed that even if some recovered memories were the result of suggestion, it did not necessarily follow that they all were. This reasonable tone was also apparent in the article she had co-written with her friend Lucy Berliner (a proponent of recovered memory), which stressed that polarisation of the

debate was counterproductive, and that there were many points upon which they both agreed (Berliner & Loftus, 1992).

In 1993, Loftus published an article (based on her address to the APA) which was the first to articulate a comprehensive argument against the reality of repressed memories. As well as presenting information about the current 'state of play' regarding recovered memory, she offered a strong critique of the phenomena. Firstly, she claimed that the clinical evidence for repression was anecdotal, and therefore unconvincing. Neither was there any experimental support for repression that could stand up to scrutiny (citing Holmes, 1990). She undermined claims that repression was widespread by pointing out methodological flaws in research which produced high estimates of prevalence. She also noted that some people reported recovering memories from the first few years of life, in spite of the well known effect of infantile amnesia. As well as raising these criticisms, she offered several alternative explanations for recovered memories. They could be the result of material introduced from a variety of sources such as television programmes and popular literature. Another possibility was that they were caused by therapeutic suggestions (to support this Loftus supplied anecdotal accounts of suggestive therapeutic practices). Finally, she argued that the large body of experimental evidence showing that it is possible to create false memories by suggestion should be considered when assessing the validity of recovered memories (for an elaboration of this argument see Lindsay & Read, 1994). These criticisms articulated the basic premises of the false memory position.

### *Making monsters or meaning?*

Similar criticisms can be found in Ofshe and Watters' (1993) article *Making Monsters*, which details the way in which they believed false memories were implanted by therapists. However, in contrast to Loftus (1993), the article was polemical in tone, with the authors stating:

In this dispute about recovered memory therapy, there is no room for a middle ground. The mind either functions in the way the therapy demands or it does not. The techniques either uncover repressed memories or they create pseudo-memories. For PhDs and MDs on



opposite sides of the controversy, the fight now starting may become the therapy world's gunfight at the OK Corral. (Ofshe & Watters, 1994, p. 9)

The article linked recovered memory therapy with the performance of lobotomies as comparable examples of faddish mental health therapies which have caused terrible damage to patients. It also characterised recovered memory practitioners as foolish, arrogant and cruel. Not surprisingly, proponents of recovered memory therapy felt compelled to defend themselves, and a response by Olio and Cornell (1994) appeared the following year.

In their article, *Making Meaning not Monsters*, Olio and Cornell (1994) argued that Ofshe and Watters' (1993) comments were a gross oversimplification and distortion of what actually occurred in therapy with abuse survivors. Their main argument was that psychogenic amnesia for trauma was a well documented phenomenon, although the mechanism causing it (repression? dissociation?) was not well understood. They cited research supporting both psychogenic amnesia and corroborated recovered memories, including a study by Loftus, Polonsky and Fullilove (1994) in which nearly one fifth of respondents who reported being sexually abused said that they had had memories of the abuse return after a period of forgetting. Olio and Cornell claimed that Ofshe and Watters had wilfully ignored this contrary evidence in their own article. Furthermore, they argued that the false memory argument was based on misleading use of empirical evidence:

The cardinal rule of science is violated when research conclusions are applied to phenomena and/or subject populations different from the ones originally studied. Although numerous studies have examined the complex questions of suggestibility and accuracy of memory, there is no scientific evidence to indicate that false memories of sexual abuse have been or can be implanted in people who do not have trauma histories. (Olio & Cornell, p.85)

Thus, experimental research supporting memory malleability is not generalisable to the trauma therapy setting. They also criticised Ofshe and Watters presentation of the Ingram case, suggesting that it omitted material which pointed to Ingram's guilt, and that Ofshe's 'field experiment' was inconclusive (see also Olio, 1994; Olio & Cornell, 1998).

Ofshe's (1994) response to this critique was even more scathing than his original article. For example, he wrote that "Olio and Cornell demonstrate a characteristic confusion, sloppiness and lack of understanding of research methods ..." (p. 96) in their comments on the Ingram case. In addition, he argued that there were many 'demonstrations and examples' (p. 100) of false memories being created in therapy. Ofshe maintained that incorrect understandings (such as the belief that all of a person's life experiences are stored 'somewhere' in the brain) combined with suggestive techniques, such as hypnosis, resulted in confabulated memories of sexual abuse. He also suggested that many of the studies which Olio and Cornell cited in support of their position were methodologically flawed.

#### *Williams' prospective study*

One of the studies to which Ofshe was referring was that published by Williams in 1994 (following publication of the preliminary results in 1992). One of the problems with previous prevalence studies of traumatic memory repression (for example, Briere & Conte, 1993) was that they were retrospective, and relied on participants self-reports that they had been abused. Williams' study was quite different. Two interviewers spoke to 129 women with documented histories of childhood sexual abuse (the information had been gathered at a hospital emergency room as part of a larger study of the consequences of sexual assault). The abuse had occurred 15 to 18 years prior to the interviews. Williams found that 38% of these women did not report the documented sexual abuse to the interviewer, and concluded that this was likely to be because they did not actually recall the abuse. The reasoning behind this conclusion is discussed in detail in the article, organised as a series of responses to challenges such as "Is it likely that the women were embarrassed or just did not want to talk about such personal matters?" and "Is the high proportion of women who did not recall attributable to the young age of the children at the time of the abuse?". Williams concludes that "... having no memory of child sexual abuse is a common occurrence ..." (p. 1170), and her study is often cited by recovered memory supporters as undeniable proof of repressed memories or traumatic amnesia (see, for example, Terr, 1994; Bass & Davies, 1994).

However, there were many others who did not find the Williams study so compelling. Loftus, Garry and Feldman (1994) argued that it was a misinterpretation of the research to see it as supporting the belief that memories could be repressed and then later recovered. They introduced the idea that 'normal forgetting' may have been the reason for the lack of recall, without any special mechanism such as repression needing to be involved. Similarly unconvinced were Ofshe and Watters (1994), Pendergrast (1995), Wakefield and Underwager (1994) and Yapko (1994).

These authors all critiqued the Williams study in the books they published in the mid-1990s. These books could perhaps be categorised as self-help books, as they offer advice and support to parents who have been falsely accused. The volumes can also be read as a counterattack on the self-help books encouraging memory recovery, such as Bass and Davies (1988), Blume (1990) and Fredrickson (1992). In this context, their main purpose appears to be polemical, with a two pronged attack on recovered memory. Firstly, they undermine the status of repression as a 'real' entity by suggesting that there is no scientific basis for its existence, and claiming that it contradicts empirically derived psychological knowledge about memory. Secondly, the authors argue that a more convincing explanation for the source of these memories can be found by surveying the existing literature on memory suggestibility and malleability. They point out that many of the techniques used in memory recovery (such as hypnotherapy and guided visualisation) are known to produce confabulated memories. Similar arguments appeared in the volumes authored by Loftus and Ketcham (1994) and Wassil-Grimm (1995).

#### *A revised edition of the 'Bible'*

It was in the face of such criticism that Bass and Davis published the third edition of *The Courage to Heal* in 1994. The publication of a third edition attested to the enduring popularity of the book, in spite of the condemnation it was receiving from those opposed to recovered memories. The third edition was also significant, as it contained a number of changes and additions to the original text. The volume contained an entirely new chapter, *Honouring the Truth*, in which the authors described and responded to what they labelled the

'backlash' against survivors with repressed memories (see also Gedney, 1995 and Quirk & DePrince, 1995). Although they admitted that some false allegations had been made on the basis of recovered memories, they argued that the FMSF and its supporters lacked evidence and credibility. However, it is interesting to note that in the third edition of the book some of the more controversial statements were altered, to reflect "legitimate criticisms", according to the authors. For example, the infamous "If you think you were abused then you were" statement (see p. 5 for the full quote) was changed to read:

Assume your feelings are valid. It is rare that someone thinks she was abused and then discovers she wasn't. The progression usually goes the other way, from suspicion to confirmation. If you genuinely think you were abused and your life shows the symptoms, there's a strong likelihood that you were. If you're not sure, keep an open mind. Be patient with yourself. Over time you'll become more clear. (Bass & Davis, 1994, p. 26).

#### *Different rules for traumatic memories*

Another book supporting the recovered memory hypothesis also appeared in 1994, written by Dr Lenore Terr, the psychiatrist who acted as the expert witness for the prosecution at George Franklin's trial. Her account of her involvement in this trial was narrated in this book (just as Loftus presented her own perspective in Loftus & Ketcham, 1994). However, Terr's main goal appeared to be the restoration of some scientific respectability to the concept of repression. She developed the hypothesis that memory for prolonged and repeated trauma (*Type II trauma*) is substantively different to memory for a single traumatic event (*Type I trauma*). This explains why children might remember, for example, a single instance of being kidnapped, but could repress or dissociate a childhood of repeated sexual abuse. She also cited neurological evidence which she claimed supported her theory that memory for long term trauma was so different from other memory that the 'normal rules' did not apply.

Jennifer Freyd (1993, 1996) was also convinced that memory for childhood sexual abuse might operate differently to normal memory. But unlike Terr, Freyd's *betrayal trauma theory* did not focus on the number of traumatic events, but on their social context. As mentioned above, Jennifer Freyd is the daughter

of Pamela and Peter Freyd, founders of the FMSF. In her book she describes how her development of betrayal trauma theory began in 1991 as a way of making sense of her personal situation (returning memories of childhood sexual abuse) through her professional knowledge as a cognitive psychologist.

After her mother made their family situation public in 1991, Jennifer made no public comment until 1993, when she spoke at a conference about both her personal history and betrayal trauma theory (Freyd, 1993). In telling the story from her perspective, Freyd expressed puzzlement as to why her memories were considered unreliable and her father's were not, revealing his history of alcoholism and inappropriate sexualised behaviours in the family. She also disclosed that her parents had invited her to be a member of the FMSF advisory board, an invitation which she found incomprehensible. Freyd integrated this personal material with a description of betrayal trauma theory. She argued that when a child is abused by someone upon whom they are dependent (emotionally and/or physically), it is adaptive for the child to develop psychogenic amnesia for the abuse in order to maintain an attachment to the caregiver who is responsible for their survival. Of course, most intrafamilial abuse meets these conditions. According to Freyd, it is the elements of dependency and betrayal which make abuse memories different from normal ones.

### *Debate in the academy*

In spite of these arguments, many psychologists were unconvinced that traumatic memory was somehow 'different', making the research on 'normal' memory irrelevant to the issue of recovered memory (see, for example, Hembrooke & Ceci, 1995). As a result, the mid 1990s saw a sustained debate occurring in the academic literature. Entire issues of highly respected journals such as *Applied Cognitive Psychology* and *Consciousness and Cognition* were devoted to the question of repressed memories. At this point in the academic debate, an attempt was usually made to provide a balanced perspective (though Courtois, 1995, complained of an under-representation of clinicians and trauma researchers). In general, research and argument which supported the general phenomenon of delayed memories (Harvey & Herman, 1994; Pezdek,

1994) appeared alongside research and argument which challenged it (Garry, Loftus & Brown, 1994; Ceci & Loftus, 1994). Commentaries and responses to specific articles which expressed opposing viewpoints were often invited and published, such as the responses to Loftus (1993) by Gleaves, (1994), Gold, Hughes and Hohnacker (1994) and Peterson, (1994), and her subsequent reply (Loftus, 1994). Some authors even attempted to find a middle ground which "...acknowledged the possibility that both recovered and fabricated memories correspond to real phenomena" (Schooler, 1994, p. 452; see also Baars & McGovern, 1995; Kotre, 1995).

The complexity of the debate is evident in the wide variety of standpoints which can be found in the literature of this period. Although many authors characterise the debate as being intensely polarised (Alpert, 1995a; Corballis, 1995; Lindsay & Briere, 1997), in fact a broad spectrum of opinion is apparent. There were indeed authors who took up positions on the extreme ends of the continuum of opinion, refusing to concede that their opponents had any legitimacy. The articles by Crews, who felt that the recovered memory movement was a "...disgrace ... the most destructive episode in the entire history of professional psychotherapy" (1995, p.6) and the book by Champagne (1996) who was similarly incensed by false memory syndrome, are exemplars of these extreme positions. However, they were certainly not typical of the views expressed by the majority of participants in the debate. As well as the 'middle grounders' described above, there were many others who, while arguing strongly for one particular position, were willing to admit that there might be some validity to the arguments of the other side. For example, many recovered memory supporters were willing to admit that some memories of abuse recovered in therapy might not be historically veridical (including Berliner & Williams, 1994; Briere, 1995; Pezdek, 1994). On the other hand, many authors who generally supported the false memory explanation accepted (at least as a possibility) that some people might regain accurate memories of previously unremembered abuse (see, for example, Lindsay & Read, 1994; Schachter, 1996; Toglia, 1995). Indeed, by 1997, Lindsay was arguing that psychologists could be reasonably certain (on the available evidence) that memory recovery and memory confabulation were both real phenomena.

### *New developments in the mid-1990s*

However, although the debate was not as uniformly polarised as was often reported, it was still undoubtedly the most controversial issue in psychology at the time. As a result, it generated a wealth of research studies, articles, conferences, reviews, and books. Many new ideas and arguments were introduced on both sides. For those who supported recovered memory, attention turned away from repression and towards other possible explanations for loss of abuse memories (such as dissociation or possible neurobiological explanations - see Alpert, 1995b; Bremner, Krystal & Charney, 1996; Chu, Matthews, Frey & Ganzel, 1996). Arguments that the false memory position was flawed by errors such as over-generalisation and misrepresentation of evidence were elaborated (Courtois, 1995; Crook, 1995). And the validity of the false memory syndrome (a creation of the FMSF) was also questioned, in that the existence of a 'syndrome', in the medical sense, had not been empirically established (Pope, 1996).

On the other hand, authors such as Kihlstrom (1997) and Perry and Gold (1995) defended false memory syndrome, arguing that use of the word 'syndrome' was warranted, as it identified a pattern of behaviour. Other supporters of false memory continued to argue that recovered memories were mainly a product of suggestion (Zaragoza & Mitchell, 1996), although source amnesia was also posited as another possible cause (Ceci, 1995). Recovered memory therapy was also critiqued on the grounds that it often had a deleterious effect on the mental health of clients (McElroy & Keck, 1995). For example, according to research conducted for the U.S. Department of Labour and Industries (Parr, 1996, in Loftus, 1997), the wellbeing of women deteriorated notably after undergoing therapy for repressed memories. The numbers of women attempting suicide, requiring hospitalisation and engaging in self mutilation all increased considerably. As a corollary, the women also showed an increase in unemployment, broken relationships and family estrangement after being in therapy.

As the debate continued, more evidence, both for and against recovered memories, was published. The difficulty that this posed for observers of the

debate is evident in the conflicting findings of two review articles published in 1996. Reisner (1996) claimed that reviewing the literature in the area showed that recovered memories could be accurate (although false memories can also occur), and that assertions by critics that repression did not exist were not warranted. In contrast, Paris's (1996a, 1996b) review concluded that there was little evidence for the concept of repression, and that the recovery of repressed memories in therapy was not consistent with empirical research findings. These widely divergent interpretations of the available research and publications suggest that, in the mid-1990s, the evidence certainly did not 'speak for itself'. As a result, reviewers could (and did) draw widely divergent conclusions from the same literature.

### *Professional organisations investigate*

A similar difficulty faced professional organisations in mental health, expected by their members, and the public, to provide some leadership on the issue. Many organisations did issue position statements (see Robinson, 2001, 2002), some of which were produced by working parties specifically set up to examine the issues. In 1995, the British Psychological Society's working party presented its report, concluding that memory loss after trauma is common, that recovery of these memories does occur, and that false memories caused by suggestion are possible (British Psychological Society, 1996). The authors pointed out that most qualified psychologists believed in the accuracy of recovered memory, and stressed the credibility of these informants by pointing out the 'non-doctrinaire' nature of these beliefs (in that most were also willing to acknowledge the possibility of false memories). However, they also cautioned therapists not to impose their beliefs about childhood events on clients, or diagnose childhood abuse on the basis of adult symptoms. While at least one of its authors thought the report was balanced (Mollon, 1998), it was criticised by many fm supporters who felt that it gave recovered memories undeserved credibility (Kihlstrom, 1996).

As Mollon (1998) points out, although the British Psychological Society's report was not received uncritically, the working party members were, at least, able to reach a consensus on the issue. In contrast, he characterises the American



Psychological Association's report as a "... 239-page slanging match ..." (p. 4). Although the American Psychological Association's working group was set up in 1993, it did not produce a final report until February, 1996 (see American Psychological Association, 1998). The working group consisted of three clinicians (Judith Alpert, Laura Brown and Christine Courtois), and three memory researchers (Stephen Ceci, Elizabeth Loftus and Peter Ornstein). What is apparent from their report is that these two factions could find little common ground. The report was presented as two separate literature reviews (one by the clinicians, one by the researchers), each with a critique by the opposing faction and a response by the original authors. These were prefaced by the "final conclusions" of the working group which listed five points of agreement – that sexual abuse is prevalent and problematic, that most children who are abused don't forget it, that remembering of forgotten abuse is possible, that creation of false memories is also possible, and that knowledge of the memory processes leading to these outcomes is incomplete. Reading the report, however, makes it clear that while the theoretical possibility of accurate recovered memories was acknowledged by Ceci, Loftus and Ornstein, they were not convinced that recovered memories actually existed. On the other hand, Alpert, Brown and Courtois seemed to be slightly more open in accepting the existence of false memories, but they were not convinced that inappropriate therapeutic suggestion was as widespread and problematic as their working group colleagues claimed.

Although some saw the report as adversarial or divisive (Barnard & Fischer, 1998; Davies, Morton, Mollon & Robertson, 1998), it did reflect the dynamic and unresolved nature of the debate in the mid 1990s. This contrasts with the report produced by the working party of the Royal College of Psychiatrists. Like the American Psychological Association's report, the Royal College of Psychiatrists' report was submitted in 1996. However, it was never released, with the College choosing instead to publish two pages of practitioner guidelines the following year (1997). In 1998, members of the working group published what they called a "revised version" of the original report under their own imprimatur (Brandon, Boakes, Glaser & Green, 1998). Kendall (1998), a College representative, later explained that the paper was not published by the College because of its

controversial nature. Reading the Brandon et al. paper, the controversy appears to lie in its total dismissal of recovered memories (as being entirely unsupported by any evidence whatsoever), and in unequivocal statements such as “Memories of satanic abuse or other bizarre events, memories from before the age of four years and memories of repeated abuse over many years forgotten until recovered therapy [*sic*] are not credible” (p. 304). It is easy to see why the College would not want to be associated with such broad generalisations, which encourage psychiatrists to totally dismiss patient’s reports if they contain any of these elements.

*Caution is advised*

Indeed, many of the therapeutic guidelines produced in the mid 1990s recommended that clinicians be extremely cautious about making *any* pronouncements at all regarding the veracity (or otherwise) of clients’ memories. The British Psychological Society’s (1996) guidelines, for example, encouraged the therapist to tolerate uncertainty and ambiguity, to avoid drawing premature conclusions about the truth of a memory and to avoid imposing on the client any conclusions about what took place in their childhood. This advice was echoed by Pope and Brown (1996), who noted that cognitive biases can lead therapists to a premature assessment of their client’s problem, which is then reinforced by a search for confirmatory evidence. They urged therapists to avoid this by accepting ambivalence and confusion as part of the therapeutic process. More specifically, Reviere (1996) cautioned that the therapist should never conclusively inform the client that trauma has occurred, or use any ‘memory work’ techniques with clients who have no known trauma history, as did Lindsay and Briere (1997).

This cautious approach towards therapy may have been partly motivated by the desire to avoid malpractice lawsuits, such as the successful case (described by Johnston, 1999) brought by Gary Ramona against his daughter’s therapist in 1994 (Simon & Gutheil, 1997). However, it also appears to have been reflective of a growing appreciation by some therapists of the limitations of their knowledge. Prozan (1997) was a noticeable exception, believing that she could accurately reconstruct a client’s past through ‘clues’ such as dreams and sexual

fantasies. In contrast, Courtois (1996, 1997), for example, stressed that without corroborative evidence a therapist can never definitively know about what has taken place in a client's past. This seemed to be an issue of particular concern for some psychoanalytically trained therapists, several of whom drew on Spence's (1982) discussion of the differences between narrative and historical truth (see, for example, Alpert, 1995c; Mollon, 1998). Spence stressed the impossibility of the analyst ever being able to reconstruct the historical truth (reality) of a patient's past, claiming that a "narrative truth" (a story that orders and makes meaningful the "analytic material") is the most that can be achieved. However, other authors (Grand, 1995; van der Hart & Nijenhuis, 1999) felt that sustained uncertainty was unsatisfactory for dealing with issues of recovered memory, as it did not acknowledge the client's need to know (with some degree of certainty) whether or not they were abused (for further discussion of this issue, see Chapter Three). They believed that, at some point, therapist neutrality must be discarded in order to support the client's judgement about "what really happened".

#### *Corroborating recovered memories*

For those who supported recovered memory, cases in which "what really happened" was confirmed by corroborative evidence became an important part of the debate in the latter part of the 1990s. Some critics of recovered memories (such as Brandon et al., 1998) charged that there was no evidence for the idea that traumatic experiences could be 'lost' from consciousness, and therefore that all recovered memories lacked credibility. Highlighting cases of corroborated recovered memories, therefore, became a popular counter-strategy on behalf of recovered memory supporters, on the basis that even one accurate recovered memory proves that such a phenomena can exist (Brewin & Andrews, 1997; Kluft, 1998a).

The two main contributors to this project were Raymond Kluft and Ross Cheit. Kluft's (1995, 1997, 1998b) data were drawn from his own clinical practice, and he claimed that 56% of a sample of patients were found to have confirmed specific memories of abuse (while only 9% of patients had memories which were disconfirmed on investigation). Cheit's (1998) methodology was somewhat

different, in that he chronicled confirmed cases of recovered memories (including his own) from a variety of sources. By 2004 (when last updated), Cheit's on-line archive detailed 96 cases with some form of corroborating evidence, including 25 cases drawn from the academic literature (for further corroborated cases see Schooler, 2001; Schooler, Bendikson & Ambadar, 1997; Shobe & Schooler, 2001).

Not everyone found this evidence persuasive, however. Piper (1998) claimed that Cheit's cases were insufficiently documented and that his methodology lacked scientific rigor. More generally, Wagenaar (1997) made the claim that case histories cannot logically be used to test theories, including theories of memory recovery. In contrast, Brewin and Andrews (1997) wonder why individual case studies are not accepted as a valid form of evidence in the debate, when they are in disciplines such as neuropsychology. They argue for recognising case studies as a valid form of scientific data, and note that these cases have been accepted as substantiating recovered memories by some commentators.

Perhaps the most controversial of the single case studies presented as support for recovered memories is the one published by Corwin and Olafson in 1997. The authors described how 17 year old 'Jane Doe' recovered a memory of her mother abusing her after viewing tapes of herself at age six reporting the abuse. Although many found this case convincing, one who did not was Elizabeth Loftus. She was sceptical about many aspects of the case, and began to investigate them, in collaboration with Melvin Guyer (Loftus & Guyer, 2002a, b). They concluded that it was likely that Jane's childhood allegations were the product of suggestion (on behalf of her father and step-mother), in order to gain sole custody of Jane. Furthermore, upon interviewing Jane's foster-mother (who cared for her after the death of her father), they found that Jane had not forgotten or repressed memories of the accusations, but had (prior to viewing the tapes) begun to question whether the abuse actually occurred.

In order to gain this information, Loftus and Guyer (2002a, 2002b) had located and interviewed Jane's mother, step-mother and foster mother (though not Jane

herself). They did not contact Corwin, the psychologist who originally videotaped Jane, and who had contacted her again at age 17 (to gain her permission to continue using her tapes in teaching). Although the authors contacted various people for advice about the ethical issues surrounding their activities, Loftus did not complete the full ethics review procedure at the University of Washington, where she was employed (Kelleher, 2003; Wilson, 2002). Jane Doe subsequently complained to the University, claiming invasion of privacy. This complaint resulted in a lengthy investigation, during which time Loftus and Guyer were unable to publish their findings. Although eventually cleared of ethical breaches, she left the University of Washington soon after to take up a new position.

#### *Accusations of bad behaviour*

While allegations of unethical behaviour have always been present in the memory debate (the situation of the Freyd family, discussed above, is a prime example), they did seem to come to the fore in the late 1990s. Many accusations of unethical and unscholarly behaviour (such as *ad hominem* attacks) were made. Before Loftus came under fire about the Jane Doe case, for instance, she was accused of various ethical breaches in relation to her famous "Lost in a shopping mall" study (Crook & Dean, 1999a). This study involved using suggestions from a family member to attempt to create fictitious memories of being lost in their research subjects (Loftus & Pickerell, 1995). Loftus (1999) responded by claiming that her work was deliberately misrepresented by Crook and Dean, who were motivated by personal animosity. In reply, Crook and Dean (1999b) accuse Loftus of *ad hominem* attacks, and stress the legitimacy of their concerns. Perhaps more disturbingly, Calof (1998) and Salter (1998) report harassment (including invasions of privacy, intimidation of family and employees and multiple law suits) as a result of their professional involvement in the memory debate (as supporters of recovered memory).

A further ethical controversy in the late 1990s resulted from the publication of a study by Rind, Tromovitch and Bauserman in 1998. Their research, a meta-analysis of college student samples, found that those who had experienced

child sexual abuse were less well adjusted than controls, but that this difference was small, and disappeared entirely when family environment was controlled for. They concluded that claims that sexual abuse inevitably causes harmful long term effects were not supported. This study provoked many negative critiques, both in the literature (Dallam et al., 1998; Dallam, 2001; Ondersma, Chaffin & Berliner, 1999; Ondersma et al. 2001; Tice, Whittenburg, Baker & Lemmey, 2001; Whittenburg, Tice, Baker & Lemmey, 2001) and in the media (see Tavis, 2000), and was even condemned by a unanimous vote in the United States Congress. Most of the criticisms focused on both the methodology and the morality of the study, with critics concerned that this research would be used to minimise or justify sexual abuse in court cases or in public policy making. Rind, Tromovitch and Bauserman (2000, 2001) responded to these criticisms at length, justifying their methodology (for example, they responded to criticisms of sampling bias and external validity) and maintaining that concerns about morality were 'extrascientific'.

#### *The controversy continues*

These recent controversies suggest that, at the start of the twenty-first century the memory debate was far from over. This was also illustrated by the discord that resulted when Elizabeth Loftus was invited to be a keynote speaker at the New Zealand Psychological Society's conference in 2000 (Corballis, 2003). Many members of the society objected, and one of the Society's Board members (Dr John Read), even resigned in protest. Although the volume of publications reduced considerably after 1999, new research and commentary was still being produced by both fm and rm supporters. For example, articles by Loftus (2000) and Garry and Polaschek (2000) examined the role of "imagination inflation" in the formation of false memories, while Ost, Costall and Bull (2001, 2002) studied retractors of recovered memory in order to understand the processes involved in the formation and then repudiation of abuse memories. The view was even advanced that false memory syndrome should be further studied with a view to establishing valid criteria for including it as a diagnosis under 'factitious disorders' in the DSM (Kaplan & Manicavasagar, 2001). Recovered memory supporters also continued to argue their case. Pezdek (2001) questioned whether suggestibility effects were actually strong

enough to cause false memories, and Leavitt (2002) re-examined Loftus' (1993) seminal article, suggesting that many of the claims made were lacking in scientific rigor. Additionally, the contributors to Freyd and DePrince's (2001) volume "Trauma and Cognitive Science" presented possible cognitive explanations for the forgetting of traumatic experiences.

*And the winner is ...?*

In spite of this continued activity, some commentators have made claims regarding the final outcome of the memory debate. As early as 1998, Lipian was claiming in *The Lancet* that the 'madness' of repressed memories in the UK was fading. Somewhat surprisingly (given the absence of such explanations in the literature), he suggested that the American experience had shown that repressed memories were the result of patients with personality disorders (borderline, narcissistic, histrionic) receiving inept therapy which gave them the opportunity to blame all of their problems on a childhood abuser. In contrast, Brown, Goldstein and Bjorklund (2000) argued that the preponderance of scientific evidence against recovered memory was what was causing a decline in its status. Robinson (2003) also believed that the recovered memory movement was declining in popularity, as a result of it being exposed as a psychological fad, with no scientific basis. Since there were never any reliable figures as to the number of practitioners of 'recovered memory therapy' or the number of clients claiming to have recovered memories, the extent to which there has been an actual decline in popularity is difficult to gauge. However, what does seem apparent is that recovered memories are no longer as *publicised* as they were in the 1990s.

In any case, as many of the issues in the debate still remain hotly contested by memory recovery supporters (as noted above), claims that the memory debate itself has been resolved seem premature. It is true that some commentators feel that there is an emerging 'middle ground' in the memory debate (Davies & Dalgleish, 2001; Lindsay & Briere, 1997), which acknowledges that both false memories and accurate recovered memories do occur. Similarly, Knapp and Van de Creek (2000) and Courtois (2001) suggest that there is a consensus amongst professionals as to the most appropriate way to deal with memory

issues in therapy (which involves the therapist taking up a stance of 'supportive neutrality'). However, there is still fundamental disagreement regarding the nature of traumatic memories, including whether traumatic memories are 'different' from normal memories and if suggestion can actually cause false memories of sexual abuse to be implanted. Thus, a discipline-wide consensus on the issue seems unlikely, especially when so many of the main protagonists in the controversy continue to maintain positions which appear to have changed little since the debate began.

### From Past History to the Present Research

As I read through the books and articles summarised in the history presented above, what became apparent to me was that the main participants, while divided on many issues, were united in their reliance upon traditional psychological understandings of memory. Most contributors seemed to agree that memory exists as a property of an individual's internal cognitive functioning, that it is possible to gain knowledge about how memory functions through scientific research, and that the accumulation of such scientific evidence will be the key to eventually resolving the memory debate (Conway, 1997). To date, however, this resolution has yet to be achieved. Despite over a decade of intense research activity (by both sides), neither perspective has emerged as the uncontested 'winner'. In fact, it is beginning to appear doubtful that further research *will* provide the definitive answer which both sides desire, since they seem unable to agree on what counts as valid research (American Psychological Association, 1998; Davies, Morton, Mollon & Robertson, 1998; Schooler, Bendiksen & Ambadar, 1997). It may well be that the question of whether recovered memories are valid is one that the empirical apparatus of psychology is unable to address.

This inability to reach a satisfactory conclusion has reinforced, for me, my decision to step outside the positivist, empiricist assumptions of traditional psychology and take up an alternative approach to the memory debate. The prevailing understandings seem to make it unlikely that the debate will ever be



resolved on its own terms (through the application of the scientific method). Instead the debate appears to have come to the point where the participants, realising that they possess no arguments capable of convincing their opponents, simply assert “I’m right” – countered by the equally convincing, “No, you’re wrong and I’m right” (see, for example, the exchange of views sparked by Karon & Widener, 1997, which extended over three years, through nine articles and responses). At this point it seems worth exploring alternative approaches that allow researchers to engage with the issues from different perspectives, and to ask new questions about the memory debate.

Not surprisingly, a number of other researchers have also come to this conclusion, with the result that there already exists a body of critical literature addressing various aspects of this debate. This literature has been produced by authors from a broad range of disciplinary traditions, who profess a wide variety of theoretical perspectives. As a result there are a plethora of alternative approaches available to the researcher interested in studying the memory debate from a critical perspective. In the next chapter, I review this critical literature and discuss how it has influenced my own analytic approach to the memory debate.

But first, a brief interlude ...

## Interlude: Recovered Memories and Me

I first became interested in the issues surrounding recovered memories as an undergraduate student of psychology. While browsing the book shelves of my local public library I came across Webster's (1995) unambiguously titled volume, *Why Freud Was Wrong*. Like many female students, I suspect, I had learned enough about Freud to know that I didn't like him, but not enough to know why. Reading the book, I found Webster's arguments about Freud's errors convincing. And in his book I was first introduced to the debate about recovered memories. At that point, I already had a vague idea about memory repression. I can remember, in my late teens and early twenties (around the time when recovered memory first began to have currency) having fleeting concerns that I might have been sexually abused by my father, grandfather or uncle, and *not even know it*. I cannot remember what prompted these concerns (I assume it was something that I heard or read), but they seemed to me to be so unlikely that I never seriously considered them. However, it was reading Webster's book that first alerted me to the fact that these ideas were controversial.

Later, as a postgraduate student (with a new found interest in discourse analysis) searching for ideas for a thesis topic, the recovered memory controversy came to mind. I knew from reading Webster that various self-help and 'pop psychology' books had been published about the issues, and I thought that it could be interesting to examine these from a discursive perspective. Accordingly, I characterise my interest in the debate as academic, rather than personal. I have not been in therapy and recovered memories myself, and I do not know of anyone who has, or of anyone who has been accused on the basis of such memories. However, from 1994 to 1998 I was employed as a social worker in the New Zealand Government's child protection agency, Child, Youth and Family Services (CYFS).

Social workers are often identified by critics of recovered memory as contributing to the 'sex abuse hysteria' which they feel has led to both false allegations (as a result of coercive interviewing of children) and false memories of sexual abuse (Goodyear-Smith, 1993; Pendergrast, 1995). The type of

inappropriate interview techniques identified by these authors include repeated interviews, leading questions, suggestions and other manipulations, such as promising the child food or rest if they answer the questions 'correctly'. While I cannot comment on whether such techniques were used by CYFS prior to 1994, I do know that by the time I was employed they were not. While I was not employed as an investigative social worker, I was involved with a number of cases which made me aware of the departmental standards requiring extremely careful interviewing of any child who was suspected of being abused. Interviews were kept to a minimum, and questions had to be non-leading (almost to the point of obliqueness in some cases). Parents were also instructed not to question the children about the abuse in any way. I understood that the purpose of these strictures was to ensure that the child's evidence would be credible in the event of a prosecution being brought against an offender.

Accordingly, I came to my study of the memory debate with no personal involvement, yet with some professional awareness of child sexual abuse issues. As I discuss in the following chapter, the type of analysis that I am undertaking requires the researcher to take a "symmetrical stance" in relation to contested knowledge claims (Potter, 1996a). This has often been a difficult task. The nature of the material discussed (and the way it is presented) in the debate is frequently shocking and unpleasant. I found it impossible not to be emotionally affected by the stories of both recovered memory survivors and of accused parents that are told in the popular texts which I studied. At times I felt that my own research was trivial and inadequate because it could not provide a solution or remedy for the problems and pain of those involved most directly in the debate. Although I eventually resolved this concern (as I describe in Chapter Three), I am still acutely aware that this study will not help those who are desperate to know the truth about their memories. However, in exploring some of the ways in which facts about memory are constructed and contested, I believe that this research offers some pertinent lessons regarding the nature and limitations of psychological knowledge in this area.

## Chapter Two: Alternative Approaches to the Memory Debate

The memory debate has generated a huge amount of research and writing by a diverse authorship, including therapists, researchers and legal experts. The vast majority of this material is grounded in a realist epistemology where the most important issue is whether recovered memories are true or false recollections of past events, and in which the resolution of this issue is assumed to be through conducting more (and better) scientific research. However, there are some writers who have approached the memory debate from different standpoints. These authors make a wide variety of claims about their own epistemological grounding, informed by their varying disciplinary traditions (philosophy, sociology, feminism, critical psychology). In fact, their claims are so diverse that these authors seem similar only in their desire to ask questions about the memory debate which *cannot* be answered by undertaking scientific research.

What all of these authors demonstrate is the feasibility of studying the memory controversy from a critical perspective. The various methodologies employed provide a variety of insights into the conduct of the debate, and offer new ways of thinking about recovered and false memories. Reading and engaging with this material has been very important in shaping my own analytic approach. My aim here is to offer an overview and appraisal of the major critical approaches to the memory debate, and to discuss how these approaches have influenced the development of my own epistemological grounding, research aims and analytic method. For the purpose of this discussion, I have divided the literature into three broad categories: alternative (non-positivist) theories regarding the nature of recovered and/or false memories; feminist writings on recovered memory and false memory syndrome; and language-based analyses which focus on discourse produced within the debate. I am aware that this categorisation is also, inevitably, a simplification – and excuse it by claiming that it serves here as an organising device, rather than a critical assessment.

## Re-thinking Memory, Confabulation and Repression

The memory debate has attracted the attention of some major critical scholars over the past ten years. Michael Billig, Ian Hacking and Theodore Sarbin have all written or edited books that address the issues at the heart of this controversy. Despite their very different theoretical orientations, what they have in common is their attempts to reframe the central concepts of the memory debate within a non-positivist context. For example, Billig's work suggests that the phenomenon of memory repression can be explained within the framework of discursive psychology, obviating the need to invoke Freudian notions of the unconscious. In contrast, Sarbin focuses on false memories, theorising them as highly valued self-narratives. And Hacking's work contextualises memory science within a historio-cultural framework.

Indeed, Hacking was one of the first people to engage with the issues raised by the memory debate in a non-empiricist fashion. Although his work focuses on multiple personality disorder (MPD), one of the tangential controversies which I noted in Chapter One, much of his analysis is relevant to the memory debate. In two papers (1992, 1994), and later in his book *Rewriting the Soul*, Hacking (a philosopher) investigated MPD as a "microcosm of thinking-and-talking-about-memory" (1995, p. 4). He characterises his research methodology as a Foucauldian style archaeology; a history of the making of a knowledge – in this case, knowledge about memory in general, and multiple personality in particular. Although his research is clearly critical, Hacking does not consider himself a social constructionist. In fact, he describes social constructionism as obvious, overworked, and often sloppy. For Hacking the only "point" of constructionist research is "consciousness-raising" through the challenging of taken for granted assumptions, showing how they are products of their historical and cultural context. He claims "My topics in the past have tended to be too complex, and to demand too much nuance, as well as conceptual analysis, for the simplistic phrase 'social construction' to be serviceable" (1997, p.14). Lynch (1995) is not entirely convinced by this claim, seeing close parallels between Hacking's work on memory and that of constructionist writers. However, he also praises Hacking's approach for avoiding the "tired polemics of realism and

constructivism (sic)" (Lynch, 1995, p. 123). The relationship between realism and social constructionism is a complex one, especially within the context of the memory debate. It is an issue which I will address in some depth in the next chapter.

However, at this point I want to concentrate on discussing Hacking's (1995, 1996) thoughts on the development of modern concepts of memory. As noted above, although Hacking uses MPD as a focus for his research, his broader interest is in the science and politics of memory. Drawing on ideas of depth and surface knowledges (after Foucauld's *connaissance* and *savoir*), he has developed four interconnected theses relating to memory. Firstly, he claims that the sciences of memory first came into existence in the latter part of the nineteenth century, when psychological pioneers such as Freud, Janet and Ebbinghaus began to study memory in a systematic way. Secondly, he maintains that this new orientation to memory was a way for the scientific establishment to "conquer the spiritual domain of the soul, and replace it by a surrogate, knowledge about memory" (1995, p.198). Thus, science replaced religion as the arbiter of the human condition – questions about the nature of the human condition could be answered through studies of memory, rather than through recourse to spiritual doctrines. Thirdly, Hacking states that memory science has both depth and surface knowledges: "The facts that are discovered in this or that science of memory are a surface knowledge; beneath them is the depth knowledge, that there are facts about memory to be found out" (1995, p.198). Finally, he claims that debates which would previously seen to be about morality could now be argued in terms of facts about memory, and that this depth knowledge also underlies the debate about recovered memory.

For me, the third of Hacking's propositions has proved the most analytically useful – the idea that underlying memory science is a very basic assumption that there are actually facts about memory to be found out. While this assertion may seem incontrovertible, Hacking's analysis shows that until about 1850 the phenomena now scrutinised in psychological studies of memory were considered the province of religion rather than science. However, since the mid-nineteenth century science has successfully constructed memory as a suitable

subject of study. This success is evident in the current debate, where the belief that memory is amenable to scientific investigation and that science is the ultimate arbiter when memories are contested is so fundamental that it is unquestioned. While fm and rm supporters may differ on the kind of scientific evidence that they believe is relevant (laboratory vs. clinical studies), both sides maintain a belief in the ability of science to eventually reveal the truth about memory – which is, of course, the truth of their own particular position. This is discussed further in Chapter Seven, where I explore the important role of science in the rhetoric of both rm and fm supporters.

Despite the near universal acceptance of the positivist scientific paradigm in memory research, it does have some major limitations. One of these is related to what Hacking calls the *looping effect of human kinds* (1995). His claim is that a kind of feed-back loop operates when people are classified as being of certain kinds, and knowledge is produced about them. The looping effect occurs because this knowledge has the potential to change its subject. This is an almost identical position to the one articulated by Gergen (1973), who argues that humans, as self-aware agents, are consumers of, as well as subjects of, psychological knowledge. In this instance, what psychologists know from their scientific studies of memory has the potential to be incorporated into lay understandings of memory, which in turn may prompt changes or alterations to the way that people remember and forget. Thus, the very phenomena about which scientific knowledge is supposedly gained are potentially transformed by interaction with this knowledge. The consequence of this interaction is a disruption of one of the foundational assumptions of the scientific project – the cumulative nature of knowledge. Further critiques of the deployment of science as the arbiter of truth in the memory debate are discussed in the section on feminist research below.

Hacking's critical analysis of the development of the sciences of memory contrasts with Sarbin's (1998a) nostalgia for the 'gentlemanly tradition' of scientific enquiry, which he sees as having all but disappeared from the conduct of the current memory debate. Sarbin is well known in critical psychology as the originator of the proposition that narrative theory provides the best explanation

for human conduct. He terms this "... the narratory principle: that human beings think, perceive, imagine, and make moral choices according to narrative structures." (1986, p. 8). Sarbin entered the frame of the memory debate in 1995 when he suggested that narrative theory provided a useful way of understanding recovered/false memories. He claimed that they should be conceived as *believed-in imaginings*, a concept drawn from his earlier work on hypnotic suggestion (Sarbin & Coe, 1972 in Kirsch, 1998). This formulation is clearly predicated on the assumption that when people recover memories in the course of therapy, those memories are of events that never happened (Sarbin, 1995). Given that this is the case, Sarbin is concerned with providing some kind of explanation of *how* it happens. He suggests that these memories begin as imaginary stories authored by the individual (and often co-authored by a therapist). These stories help the individual make sense of the world and of themselves. As the individual becomes more and more involved with their narrative, they engage in a process of self deception, whereby they selectively attend to material that supports their narrative, while ignoring any disconfirming evidence. This level of involvement results in a narrative which is highly valued by the individual, at which point it ceases to be a story and becomes, instead, a history. These beliefs about the past then take on the power to inform the individual's behaviour (Sarbin, 1995, 1997, 1998b, 1998c).

The main way that this narrative concept of *believed-in imaginings* has been utilised is in de Rivera's (1997, 1998a, 1998b, 2000) studies of individuals who have made accusations based on recovered memories and then later retracted them. However, somewhat confusingly, de Rivera's own analytic approach is not clearly grounded in narrative theory. His initial research does involve an analysis of the self-narratives of retractors, along with a collaborative attempt at meaning making through a conceptual encounter interview (1997). This involves offering the participants several theoretical accounts and working with them to decide which account(s) best fit their experience of developing false memories. However, possibly in response to critiques that this work lacks scientific rigour (Coons, 1997; Gudjonsson, 1997; Kassin, 1997; Lynn, Stafford, Malinoski & Pintar, 1997; Qin, Tyda & Goodman, 1997), his next research project is a quantitative questionnaire analysis aimed at ascertaining which characteristics



of the individual and their circumstances determine the theoretical account(s) they utilise. In this paper, while he has taken on the language of narrative (the term *believed-in imaginings* is used instead of false memories), the self-narrative account appears as only one of three possibilities which might explain memory construction (alongside mind control and role playing).

Nevertheless, de Rivera (1998b) and Sarbin (1998c) both agree that the use of the concept *believed-in imaginings* is intended to provide a neutral, non-judgemental and non-pathological way of talking about people's experiences of remembering events that never really happened. In particular, Sarbin claims, it allows investigators to avoid "...employing the implicit moral verdicts contained in the medically inspired vocabulary of hallucination and delusion" (p. 298). I find this argument somewhat inexplicable, in that, in my extensive reading of the literature, even the most ardent supporters of false memory syndrome (FMS) rarely resort to such pathologising (in fact, they are generally at pains to point out that even the most sane individuals are vulnerable to the processes of suggestion which result in false memories). Thus, I read this claim as a rhetorical move on the part of Sarbin to defend the term against its critics.

For as these critics point out, labelling any memory an "imagining" seems to involve an *a priori* assumption that it does not refer to an actual past event (Scheibe, 1998; Vinden, 1998; Wiener, 1998). So the term clearly involves making a judgement that the memory is inaccurate or false, even if it does not imply that the person is bad or sick. In fact, calling false memories "believed-in imaginings" seems to be simply a semantic substitution, which is what allows de Rivera to co-opt the concept into his positivist framework with minimal difficulty. Indeed, Sarbin himself seems to have some difficulty integrating the concept with his avowed social constructionism, and his final solution to this problem – a version of bottom-line relativism – seems less than satisfactory (this point is discussed further in Chapter Three).

Thus, the major inconsistency in Sarbin's approach appears to be his basic assumption that all recovered memories are necessarily false ones. This inevitably involves him as a participant in the memory debate (indeed, like

Loftus and Wakefield, he is a member of the FMSF advisory board), rather than allowing him to take the critical stance to this area of psychological 'knowledge' that a social constructionist epistemology would seem to require (Burr, 1995). In contrast, Billig (1999, p.145) avoids this pitfall by taking up the position that there is evidence to support both sides in the debate, and maintaining that "no blanket answers can be given ..." to questions of memory veracity. This allows him to neatly side-step involvement in the debate, in order to concentrate on the development of his argument that repression certainly does occur, but not in the way that Freud (or modern recovered memory supporters) would have us believe.

In a comprehensive series of publications, Billig (1997b, 1997c, 1998, 1999, 2000) offers an alternative explanation for repression that does not require Freudian conceptions of biological drives and mental structures. In fact, he points out that even Freud himself admitted that he was unclear as to how exactly repression occurred, which suggests that psychodynamic theory is not equal to the task of explanation (1999). Instead, Billig suggests that repression makes more sense if considered within the framework of discursive psychology. He argues that "... language is not only expressive, but also repressive" (1998, p.12), and that all speakers learn the rhetorical skills required to close down or avoid topics of conversation, which is the basis of what he terms "dialogic repression". According to Billig, dialogic repression can occur both in self-talk (thinking) and in conversation. In addition, he claims that the rhetorical skills of dialogic repression are so subtle that they are often employed without either the listener *or the speaker* being aware of their employment (1999). Indeed, their very utility depends upon this subtlety, to the point that they cannot be taught explicitly without undermining their usefulness. Billig uses the reported conversations between Little Hans and his father (one of Freud's famous cases) to show how these skills are taught and learnt implicitly as a child learns the social practices of language (1998, 1999).

To further develop the concept of dialogic repression, Billig (1999, 2000) also examines Freud's various reports of part of his own self-analysis, showing how repression can be accomplished through the telling and retelling of

autobiographical stories. In this analysis he makes the point that forgetting (and thus repression) is often accomplished through remembering. As he explains:

Such memory stories – whether personal, professional or cultural – can be suspected for being partial in their patterns of inclusion and omission. It is not so much that the past is censored, but that the past is being recruited, indeed created, to serve present purposes. What is inconvenient or painful to remember is overlooked in this remembering. (1999, p.170).

Thus, remembering and forgetting can be accomplished simultaneously. In fact, when considered from this perspective they can be seen as two aspects of the same social practice, rather than as opposing mental phenomena (which is their status in traditional psychological approaches). Furthermore, Billig's formulation allows repression to be conceptualised as a form of forgetting – an extension of the normal abbreviating and excluding practices involved in all remembering – differentiated by the motivation to avoid unpleasantness and to support current self constructs. As I will explain later, Billig's concept of dialogical repression has been pivotal in the formulation of my own understanding of recovered and false memories.

Billig, Sarbin and Hacking have all provided alternative ways of understanding the psychological phenomena central to the memory debate. Engaging with their analyses has proved extremely helpful in the development of my own approach to researching this issue. Hacking and Sarbin's work has challenged me to clarify and refine my own epistemological standpoint in relation to truth claims, and to grapple with the ethics of taking up a relativist position in relation to this debate, while Billig's approach has provided me with the conceptual resources to resolve some of these issues in what I believe is a theoretically consistent and ethically principled way, as I will discuss further in the next chapter. For now, I turn to a consideration of the feminist literature in relation to the memory debate.

### Feminism and False Memory Syndrome

There are a wide variety of feminist analyses of the issues contested in the memory debate (Burman, 2001a; 2001b). While the majority of these analyses

seem to have been mobilised in reaction to the claims of the False Memory Syndrome Foundation, it would be erroneous to maintain that there is a single unified 'feminist position' on recovered/false memories. Rather, there appears to be a spectrum of opinion, at one end of which are writers who are unreserved in their support for women's recovered memories and deeply antagonistic towards any criticism, which they perceive as an attempt to minimise the problem of child sexual abuse. Champagne (1996) exemplifies this stance, claiming

Denial of the prevalence of incest and its damaging effects on people and communities, a position advocated by the seemingly rational and scientific discourse of the FMSF and its disciples, is a vestige of anti-feminism and needs to be publicly critiqued and excoriated for small-minded sexism. (p. 1)

In contrast, many other feminist writers are deeply concerned by false memory arguments and the publicity surrounding them, but still accept the possibility that some reports of recovered memories may be inaccurate (Brown & Burman, 1997). Campbell (1997), for example, is worried that these possible false memory claims will undermine the status of women as rememberers, and therefore as persons, entrenching stereotypes of women as lacking a stable sense of identity (see also N. Potter, 1996). Follini (1997), on the other hand, feels that the most problematic aspect of the false memory argument is that its focus on falsely accused parents diverts attention away from the continuing problem of child abuse. This concern is echoed by Saraga and MacLeod (1997), who are particularly critical of the way false memory has been created as a 'syndrome', which they feel implies a level of prevalence of false claims that effectively undercuts public concern for child victims. In general, these feminist authors tend to look beyond the arguments about whether recovered memories are true or false, being more concerned with the broader societal implications of wide-spread acceptance of false memory syndrome.

In addition, many feminist authors are extremely suspicious of the psychological research that is offered as evidence in support of false memory syndrome. They are particularly critical of the way that fm supporters tend to present themselves and their findings as objective and without personal or political bias (Brown, 1995; Kristiansen, Haslip & Kelly, 1997; Burman, 2002). These authors point out that science is inherently subjective – it is inevitable that scientists' personal

beliefs will affect the way they undertake research and interpret their findings. A related issue is the selective reporting of empirical studies identified by Brown, and Kristiansen, Haslip and Kelly. They suggest that fm supporters routinely ignore research which supports the existence of recovered memories, while claiming that this partial presentation of the evidence represents an objective scientific view of memory.

Despite these problems with ways that science is rhetorically invoked by fm supporters, some feminist authors do look to psychological research as a way to make sense of the competing claims within the memory debate. Enns, McNeilly, Corkery and Gilbert (1995) argue that their survey of the empirical literature supports their claim that memories are likely to be repressed and recovered (though they also acknowledge that there is some empirical support for false memory creation). Similarly, Jennifer Freyd (1996, 1997), who positions herself as a feminist psychologist, maintains that several sound empirical studies support the idea that abuse memories are sometimes forgotten and later remembered. As noted in Chapter One, she has developed the concept of *betrayal trauma theory*, as a cognitive explanation for such forgetting and remembering.

However, appeals to science also underlie the critiques of rm made by some feminists. For example, Tavis (1993) feels that the authors and therapists promoting recovered memory have minimal knowledge of the psychological literature on memory, and that their approach is unscientific. She urges readers to be cautious about the self-help books and therapies which she claims create abuse memories, but does not go so far as saying that all recovered memories are false. Tavis also argues that the recovered memory movement encourages women to blame all their problems on abuse, and that this is an overly simplistic response to the complexity of women's lives. She is concerned that the focus on individual recovery and healing diverts attention away from efforts to change social systems that implicitly condone abuse. This concern about rm is echoed by Guy (1996). Writing from a New Zealand perspective, she is particularly concerned about the prevalence of false accusations of sexual abuse, including those based on fms. According to Guy, this is a serious problem which has not

been acknowledged by a monolithic feminist 'establishment' who are ideologically invested in the belief that all accusations must be believed. As evidence, she points to the account of the scientific evidence offered by Goodyear-Smith (1993), which shows how misguided therapeutic interventions can produce false allegations.

In contrast to the more or less partisan positions discussed so far, some feminist authors feel that *both* the rm and fm movements are problematic for women. For example, Park (1997) argues that "the dissemination of the ideas of the FMSF among the general populace simultaneously (re)marginalises the biologically female, the culturally feminine, and the politically feminist." (p.10). She explains that fm not only throws all claims of abuse into doubt (making it even more difficult for abused women and children to talk about what is happening to them), it also attacks the predominantly female profession of psychotherapy, thus devaluing women's work. Furthermore, fm supporters also directly attack feminism itself, explicitly blaming feminists for creating a public climate of 'sex abuse hysteria' which has encouraged false accusations.

However, Park (1997) also critiques the recovered memory movement for relying on out-moded 'storehouse' models of memory, and believing that memories unproblematically reflect the reality of past events. She suggests that these beliefs are based on philosophical assumptions which are no longer tenable in light of empirical studies which clearly show the constructed nature of memories. In ignoring this evidence, the recovered memory movement has created a situation in which great harm may be caused to not only the client and her family, but also to individual therapists and their profession. Most significantly, it also harms the credibility of people who have actually been abused. In light of this, Park suggests that a principled feminist response is to

... acknowledge that in some (but not all) cases, women' experiences of "remembering" may be misleading and iatrogenically caused (that is therapist induced), but that in other (but not all) cases, the experiences are genuine memories caused by earlier life events – although the catalyst for now recalling these previously repressed episodes is, in part, the therapeutic technique. (p.31)

Thus, rather than making a generalisation about all recovered memories being true or false, each case must be examined on its own merits, and in its own context. Both therapist and client must be responsible for establishing, as much as is possible, the epistemological basis of memory claims. Park feels that both sides of the recovered memory controversy have been remiss in their construction of the (female) client as lacking in responsibility and agency – she is either an ignorant victim of child sexual abuse, suffering from the effects of a trauma she cannot even remember, or she is a passive victim of suggestion, unaware that the memories which torment her are not really her own.

While Park sees both rm and fm as merely disempowering, Schuman and Galvez (1996) highlight the active pathologising of the client by both movements. They argue that the concepts of post-traumatic stress disorder, and multiple personality disorder (favoured by rm proponents), and false memory syndrome (on the fm side) actually serve a similar purpose, allowing the client to be labelled as 'sick', so that individual, rather than institutional, transformation is required to solve the problem. They also draw attention to how the argument that false memory syndrome is not a 'real' disorder serves to obscure the constructed nature of all mental illnesses. In fact, they suggest that feminist rm therapists are actually complicit in maintaining patriarchal structures, by not questioning the basic assumptions of the therapeutic enterprise. They are similarly critical of the fm arguments, which they see as arising predominantly in response to feminist challenges and as generally serving to "... oppress women patients and unwanted [female] therapists while safeguarding the mental health profession's most instrumental irrationalities" (p. 21).

Schuman and Galvez (1996) also grapple with several issues raised by considering the debate in a postmodern context. They argue that the rhetoric of fm helps to reassert psychology's status as a science in the wake of challenges to this status from feminists and social constructionists. They also discuss how the true/false memory binary, and knowledge claims regarding the reality of memories are problematic in a postmodern context. They point out that fm proponents are in a particularly difficult position, in that they argue for the constructed nature of memory, while simultaneously maintaining that the 'truth'

about what 'really' happened can be ascertained. Finally, the authors ask (but don't answer) the most difficult question of all – does taking up a constructionist position on memory and reality deny the importance of women's material, corporeal experiences of abuse?

Just as there are a variety of feminist positions on recovered memory, there are a variety of responses to this question. Some psychoanalytically inclined feminist writers, such as Burman (1996/97, 1997, 1998, 2001a, 2001b, 2002), Haaken, (1998) and A. Scott, (1996,1997), have taken up broadly postmodern and constructionist approaches in their analyses. These writers maintain that there is always an element of uncertainty with regard to the historical veridicality of memories. However, they see this uncertainty as potentially positive, as it allows exploration of the processes of memory re/construction, which may have a therapeutic benefit for the client. Burman (2001a) claims that the view that memories are constructed accounts produced for various audiences is now widely accepted, but that " ...attending to the contextual character of the production of accounts of memory does not mean that what is being said is fiction" (p.23). So she argues that accepting the constructed nature of memories does not require a denial of material experience. And Atmore (1997, 1999) argues that postmodernist analyses can be very effective in exposing how the dominant discourses deployed in these debates are linked with the material conditions of women's oppression (see Atmore 1991, 1996).

However, not all feminist writers share this view. S. E. Scott (1997) claims that "postmodern paralysis" is responsible for the lack of a strong, publicly articulated feminist critique of fm claims. As she sees it, postmodernism has made all memory claims contestable, leaving feminists feeling unable to forthrightly assert the veridicality of survivors' stories. She feels that postmodern concern with discursive production must be tempered with a recognition that survivor stories are " ... part of embodied human lives" (p.37). A similar concern is voiced by Crossley (2000), as she reflects on her earlier constructionist analysis of one survivor's story (writing as Davies, 1995). This earlier study, which she characterises as "ethnomethodological/discourse analytic", focused on how Sylvia Fraser's (1987) account of memory recovery incorporated two



very different ways of understanding her experience – a “socialist-feminist” narrative and a “healing/therapeutic” narrative. The cultural production and political implications of each of these narratives was explored in the analysis. Crossley now suggests that this type of analysis has some flaws. Drawing on Parker (1991), she maintains that her work shares a common fault with many discursive approaches – a tendency to ignore the subjective and reflexive aspects of human experience, and to disregard the agency of the individual. She feels that these limitations are particularly problematic in analysing personal accounts of trauma, where it is important to acknowledge that the individual is struggling with existential and moral questions that will affect how they live their lives. In order to do this, she argues that analysts must be willing to go beyond a narrow concern with language and social structure and, in the true spirit of postmodernism, engage fully with the complexities of subjectivity.

These feminist perspectives on the ethics and utility of postmodern and constructionist analyses of the memory debate have had an impact upon the development of my own analytic standpoint. In particular, they have challenged me to refine and elucidate my thinking on the relationship between experiential reality and social constructionism (see Chapter Three). My reading of these feminist authors has crystallised some of my concerns in relation to taking up a constructionist standpoint. However, it has also provided some ways of resolving these issues in a way that does not ignore the importance of lived experiences, even when taking up (in common with the authors in the next section) an analytic standpoint that focuses on language.

#### Looking at Language: Analysing Accounts Instead of Ideas

The authors discussed so far have all tended to focus on providing analyses of the broader psychological, cultural, historical and social aspects of the memory debate. In contrast, only a few authors have attempted language-based analyses of texts and discourse produced within the debate. One of the few who has is Davies (1995), whose discursive analysis of *Healing Sylvia* (1989) is discussed in the previous section. Davies’ analysis focused on one woman’s

account of recovering her memories. In contrast, Davis (2000) explores a number of the 'retractor' accounts that have appeared in the FMSF literature. Although he recognises that questions of accuracy are important (especially for those families directly affected by abuse allegations based on recovered memories), he argues that the accounts of recovered memory retractors are "...important for what they do, a matter to be analyzed in its own right and pursued independently of the question of accuracy or truth-value" (p. 50). Davis, a sociologist, believes that existing accounts research (by social psychologists) focuses too narrowly on the individual account producer. Thus, his research explores the role of the FMSF in developing and publicising a paradigmatic account of recovered memories which can be utilised by women who wish to restore family relationships after making an abuse allegation. He suggests that the retractor accounts promoted in the fm literature revolve around constructing both accused parent and accusing child as victims of unethical or misguided therapists. This account, when taken up by individuals, works on two levels, both to repair the familial relationships disrupted by the abuse allegation, and to provide an explanatory self-narrative which allows the accusing child to make sense of her own experiences and behaviour.

Similarly, McMartin and Yarmey (1998, 1999) take up a neutral stance in relation to the reality or truth of recovered/false memories. However, while Davis's (2000) research focuses on the way in which recovered/false memory phenomena can be constructed to achieve interpersonal business for retractors, McMartin and Yarmey's discursive research focuses on how these phenomena are constructed as argumentative resources by protagonists in the memory debate. Their approach recognises the rhetorical character of the discourse produced by supporters of both rm and fm. Specifically, their papers explore how the concepts of repression and dissociation are differentially constructed in the writings of pro-rm and pro-fm authors. Their analyses highlight the processes of fact construction (and deconstruction) engaged in by these authors in the effort to have their versions of repression/dissociation accepted as the 'real' ones. Their discourse analysis approach incorporates insights from linguistic pragmatics, rhetorical psychology, and discursive psychology, allowing them to investigate the dynamic relationship between the arguments and

counter-arguments produced to support and undermine positions in the debate. However, they do concede that in their analysis they have focused on “ ... the content of the memory debate at the expense of detailed coverage of linguistic form” (1999, p. 345). Nonetheless, McMartin and Yarmey’s analysis provides some fascinating initial insights into the rhetorical strategies and resources utilised by participants in the memory debate.

### Precedents and Opportunities

This overview of the existing critical literature has highlighted two key points relevant to the development of my research project. The first is the utility of stepping outside of the boundaries of positivist psychology to consider the memory debate from different perspectives. Although the ethics of taking up postmodern standpoints in relation to this debate have been queried, alternative approaches have provided researchers with the opportunity to ask and answer new and interesting questions about the debate. In fact, in the very act of foregrounding the ethical implications of epistemological commitments, postmodernist authors have added an important, and otherwise unconsidered, dimension. The second point is that most of the critical literature to date has discussed broad psychological, cultural, historical and social aspects of the debate. Very little of this material has focussed on the *actual language practices* utilised by rm and fm supporters engaged in this debate. Given that arguing and debating are inherently discursive activities, this seems to constitute a major gap in the critical literature.

Thus, the existing critical literature creates both precedents and opportunities for me to further explore the memory debate from an analytic standpoint outside of the entrenched dichotomies of the debate. More specifically, McMartin and Yarmey’s (1998, 1999) discursive analysis of the rhetorical construction of repression and dissociation has provided both a grounding and a “jumping off” point for this research project. Although I had already formulated the intention of undertaking some kind of discursive analysis of the memory debate before I read their work, their articles showed me just how such an analysis could

proceed. Therefore, in the analysis which follows, I will be both building and expanding on many of the insights offered in McMartin and Yarmey's work, including the role of definitions, first person accounts, science and history in the memory debate. I have also designed this project to take account of some of the limitations which they acknowledge in relation to their own analysis, including the need for a more detailed textual analysis of rhetorical strategies, identification of a broader range of discursive resources and analysis of texts outside of the 'scientific domain'.

### *My research aims*

At this point, it seems appropriate to delineate the research aims developed over the course of this preliminary exploration of the memory debate literature. My intention in this thesis is to examine the rhetorical practices utilised by protagonists in the memory debate. The purpose of this examination is to both identify the available rhetorical resources and strategies which are drawn upon by debaters, and to provide an explanation as to how these rhetorical strategies actually work – that is, how aspects of their construction contribute to their persuasive function. From this analysis, I hope to gain some understanding as to how the rhetorical context of the debate has shaped the production of psychological knowledge about recovered and false memories.

As well as leading to the formulation of these research aims, my survey of the critical literature in this chapter has highlighted some important concerns related to epistemology and ethics. In particular, it has raised questions about what taking up a social constructionist standpoint means when considering the issues of truth and reality that are central to this debate. In the following chapter, I consider these issues in some depth, after first describing the constructionist analytic standpoint and methodological approach which I have taken up for the purposes of this research.

## Chapter Three: Epistemology, Methodology and Ethics

The purpose of this chapter is to describe the theoretical underpinnings of the analytic approach I have utilised in this project. There are many complex points of interconnection in the epistemological and methodological approaches which inform my thesis, which I have attempted to explicate here. I begin by discussing social constructionism as an epistemological approach. Then I discuss discourse analysis in its incarnations in both the sociology of scientific knowledge and social psychology. Next I explore discursive and rhetorical psychology, and various other forms of rhetoric and rhetorical analysis. Finally, I explain how I have attempted to incorporate and synthesise these approaches in my own analytic method, before discussing some of the challenges which arise from taking up a stance of methodological relativism when studying the memory debate.

### Social Constructionism

Gergen (1999) situates social constructionism as part of a widespread cultural and academic turn towards alternative understandings of human life; what he calls the 'posts' – post modernism, post structuralism and so on. As one of the most prominent champions of social constructionist ideas, Gergen was himself responsible for their introduction into psychology in the early 1970s (Burr, 1995). Since then, increasing numbers of psychologists have taken up these ideas, developing alternative, critical approaches to social psychology (Billig, 1997a). As this work has proliferated, social constructionist theory has been variously interpreted, modified and extended, resulting in a network of linked, but not identical, approaches (Burr, 1995; Potter, 1996a; Danzinger, 1997). The existence of so many different types of social constructionism creates a potential for confusion, so in this section my aim is to explicate the network of related theoretical assumptions which inform my social constructionist position.

Epistemological issues are at the core of my understanding of social constructionism. In particular, I am persuaded by what social constructionism has to say about the nature of knowledge, and consequently about our

knowledge of natures. Social constructionism challenges the assumption that the knowledge we have about the world is a product of objective observation and rational deduction (Burr, 1995; Gergen, 1999; Tuffin, 2004). The belief that, under strict conditions, observation can produce a reasonably accurate and unmediated picture of independent reality underpins the scientific method (Danzinger, 1997). In contrast, social constructionism suggests that knowledge is more correctly conceptualised as a product of social processes (Edwards, 1997). At the most basic level, observation can never be unmediated, because the very categories which we have for organising our perceptions are socio-linguistic artefacts. Additionally, knowledge cannot be produced independent of social context, and this context will influence all aspects of the knowledge production process. Thus, from a social constructionist perspective, knowledge can be considered a community creation, rather than the result of individual observation (Gergen, 1999).

Clearly, this view of knowledge challenges many of the taken-for-granted assumptions of psychology, such as the belief that our existing knowledge production processes are capable of revealing universal truths about human nature (Burr, 1995). On the contrary, social constructionists argue that psychological knowledge is always a product of its historical and cultural context, just as psychological 'subjects' are themselves (Gergen, 1973). As Cherry (1995) points out, research and theory development are driven by the preoccupations of researchers, which are themselves shaped by cultural mores. The search for universal laws of human behaviour is further complicated by the potential for human subjects to become aware of and assimilate the knowledge produced about them, creating a kind of 'feedback loop' (as discussed in Chapter One – see Gergen, 1973; Hacking, 1995). Social constructionism thus highlights the contingent and situated aspects of psychological knowledge, arguing that it can never be abstracted from the social context of its production, and that claims to universality must always be regarded with scepticism.

Social constructionists also differ from traditional psychologists in their understanding of the nature of psychological phenomena. Psychological phenomena such as emotions, memories and attitudes are no longer

considered to be possessions of individual minds. Rather, they are conceptualised as constituted through social interactions, processes and relationships (Butt, 1999). As Gergen (1985) argues “The explanatory locus of human action shifts from the interior region of the mind to the processes and structure of human interaction” (p. 271). This reorientation is a result of the recognition that psychology’s traditional understanding of the role of language is fundamentally flawed. Wittgenstein’s (1953) private language argument demonstrates that it is logically impossible for words to act as labels for internal experiences. Children cannot learn the meaning of jealousy, for example, by being shown the internal experience of the emotion. Rather, they learn the vocabulary of jealousy in the situated context of social relationships, and by having their own experiences and utterances identified by others as indicative of an underlying feeling of jealousy (see Billig, 1999). Thus, jealousy (and indeed, all ‘interior’ phenomena) can be seen to be situated and produced in social interaction. Rather than being mental phenomena reflected by language, they are social phenomena constituted in language.

This understanding of language as constituting rather than reflecting or representing reality is a central feature of social constructionism (Gergen, 1985; Burr, 1995; Pujol & Montenegro, 1999). For a constructionist, the communicative properties of language allow realities to be constructed in social interaction, while the culturally and historically situated nature of language provides the boundaries for how we perceive the world and how we can act in it (Howard, Tuffin & Stephens, 2000). Thus, language becomes an object of study in its own right. It is important to note, however, that most social constructionists are not particularly interested in studying language as a static symbolic and logical system, in the mode of a linguist. Rather, they are concerned with ‘language in action’ – language employed in social interaction, as a communicative medium, and as the site and tool of meaning making.

This dynamic version of language seems to me to be at the heart of what is meant when the term ‘discourse’ is invoked by social constructionists. The concept of discourse incorporates both language and social relationship as inextricably linked and co-creating. However, there are some marked

differences in the way that different constructionist psychologists understand and work with this relationship. The most obvious of these divisions is between the Foucauldian-inspired analytic perspective pioneered by Parker, and the more functionally oriented style developed by Potter, Wetherell, Edwards and Billig. Initially these approaches were mainly differentiated by Parker's (1990a, 1990b) attention to the relationship between discourse and power, in contrast to Potter and Wetherell's (1987; see also Potter, Wetherell, Gill & Edwards, 1990) focus on the functional, performative nature of discourse. More recently, these approaches have diverged even further as questions of epistemology have come to the fore. Parker (1996, 1999a, 1999b) and other sympathetic colleagues (see the collection of articles edited by Nightingale & Cromby, 1999) ground their constructionist analyses in a critical realist epistemology, in contrast to Edwards, Ashmore and Potter (1995; see also Potter, Edwards & Ashmore, 1999) who champion relativism as the most appropriate foundation for constructionist enquiry. I explore the implications of this issue for my own research at the end of this chapter.

While both the modes of discourse analysis described above are rooted in social constructionism, their increasing divergence has created two quite distinct styles of analytic practice. For the purposes of this research, I have utilised the more functionally oriented version of discourse analysis, with a specific focus on rhetoric and argument (Antaki, 1994; Billig, 1987). McMartin and Yarmey's (1998, 1999) study has demonstrated the utility of this approach for analysing the memory debate as a site of contested processes of knowledge production. Questions about the discursive and rhetorical practices through which versions of recovered memory and false memory syndrome are produced as factual accounts, and undermined or countered, are at the heart of my research. These issues are well theorised in the functional discourse analysis literature, but have yet to be widely explored in relation to the memory debate. In contrast, as discussed in the previous chapter, many authors have already considered the issues of power and ideology which are part of the arguments surrounding repression and false memory. Thus, my choice of analytic methodology is a response to both the existing literature and to my own particular interests – which are, no doubt, influenced by my familiarity with and



affinity for the analytic methodology I have chosen. An explication of this methodology is my next task.

### Discourse Analysis

Discourse analysis as a coherent analytic methodology was first developed in Gilbert and Mulkay's (1980, 1984) study of a controversial issue in biochemistry. While the practice of analysing scientific discourse was not unknown, Gilbert and Mulkay were the first to offer a comprehensive explanation of the way in which talk and text could be examined for patterns of utilisation (Antaki, 1994; Woofitt, 1992). Working from a disciplinary background in sociology, Gilbert and Mulkay developed this new approach at a time when sociological studies of science were being transformed by the emergence of a new and radical understanding of the nature of science (Edge, 1995). Traditional sociological approaches tended to consider scientific "facts" as unmediated representations of external reality, and thus not amenable to sociological analysis. In contrast, this new understanding, articulated by Bloor (1976) as the 'strong programme', argued that *both* scientific practice and scientific knowledge should be treated as social accomplishments. Furthermore, in studying science, sociologists should be impartial in regard to the truth status of accounts (Woolgar, 1980), by taking up a standpoint of methodological relativism. When studying scientific controversies, for example, the strong programme enjoined researchers to examine the construction and production of both positions as social accounts, rather than assuming that the 'winning' side represented unmediated scientific truth while the errors of the 'losing' side could be explained in terms of social factors (Scott, Richards & Martin, 1990). Adoption and expansion of the strong programme by researchers resulted in the body of work and theoretical approach known today as the sociology of scientific knowledge (SSK) (Edge, 1995).

Influenced by the development of these new ideas, Gilbert and Mulkay (1980, 1984) developed discourse analysis as a way of better understanding the variability in the discursive data which they were collecting from scientists. They argued that traditional approaches to science studies required the analyst to

regard participants' discourse as a resource which they could use to work out 'what was really going on'. Generally, they claimed, this meant that analysts looked for similarities in the participants' statements, and then decided that these similarities were an accurate reflection of social reality. Gilbert and Mulkay felt that this was problematic because it ignored the context-dependent and variable nature of participants' discourse, and rested on the assumption that what participants say is "... simply descriptive of the social action to which it ostensibly refers ..." (1984, p. 7).

In contrast, Gilbert and Mulkay's (1984) version of discourse analysis involved identifying the variability in participants' accounts and considering how this variability related to the linguistic context in which it was produced. They concluded that scientists' talk was organised, in that it drew on different *interpretative repertoires* depending on the situation. In formal contexts (for example, journal articles), the scientists drew almost exclusively on an *empiricist repertoire*, which "... portrays scientists actions and beliefs as following unproblematically and inescapably from the empirical characteristics of an impersonal natural world" (p. 56). However, in less formal settings, such as interviews, the scientists also drew on a *contingent repertoire*, which acknowledged the personal and social influences impacting on scientific research. This repertoire was used asymmetrically to account for the errors of scientists who held an opposing position in the scientific debate. They also identified what they called a special interpretative pattern, which was utilised to resolve the tensions between the two repertoires, called *the truth will out device* (over time, the special structure of science means that the true facts of the matter will become apparent). This analysis was achieved through a detailed study of the way the scientists actually talked and wrote, which highlighted the functional aspects of their discourse.

Like Gilbert and Mulkay (1984), Potter and Wetherell (1987) felt that there were some fundamental problems with the theorisation of language in their discipline (social psychology). In particular, they questioned the assumption that language was a neutral medium through which people's internal states and characteristics were expressed. They adopted discourse analysis as an approach which

allowed the researcher to focus on the performative, contextual and constructive aspects of language use. Potter and Wetherell trace their interest in these features of language to a number of sources, including speech act theory, ethnomethodology and semiotics. A fusion of these intellectual traditions provides the basis for their elaboration of discourse analysis.

Potter and Wetherell's (1987) approach to discourse analysis is based around four basic concepts: function, variability, construction and interpretative repertoires. The notion of function refers to the performative aspect of language – that is, the ability of language to *do* things as well as to say things. For example, language may be used to explain, request, justify or blame (among many other activities). Sometimes the function of particular linguistic productions will be explicit (“I excuse you”), but often the function is both complex and unintentional (individuals may not necessarily be aware of the functional nature of their utterances, or the resulting consequences). Accordingly, one of the aims of discourse analysis is to understand the functional aspects of talk.

Wetherell and Potter (1988) suggest that one of the ways in which functions can be elucidated is through the consideration of variability within and between individuals' accounts. People commonly produce a wide variety of different (and often contradictory) linguistic formulations depending on the context of their talk and the functions which they wish it to perform. The existence of variability in accounts is important because it challenges traditional psychological assumptions about the unitary and coherent nature of the self. Rather than producing consistent formulations which reflect 'underlying' attitudes and traits, people can be seen to produce variable and inconsistent accounts which do different work in different situations.

Construction, the third concept discussed by Potter and Wetherell, refers to the way in which versions of reality are constructed through language. As noted above, social constructionists consider that language provides the boundaries of the ways in which people can think, perceive and act in the world. However, speakers do have the ability to choose among the linguistic resources available

to them to construct the world in a particular way at a particular time. These linguistic resources include the interpretative repertoire. Interpretative repertoires have been described as a communal “tool kit” of linguistic resources available to people who share a language and culture (Burr, 1995). Potter (1996b) defines interpretative repertoires as “... systematically related sets of terms that are often used with stylistic and grammatical coherence and often organised around one or more central metaphors” (p. 131). The idea is that systems of understanding and meaning are properties of a particular culture and society. While variability can often be identified within accounts, there is also consistency between accounts as speakers draw upon the interpretative repertoires which are socially and culturally available to them.

Antaki (1994) positions this style of discourse analysis as lying between the broader, content based analyses in the constructionist tradition, and the more tightly focused studies of interactional organisation produced by conversation analysts. Discourse analysis draws on both approaches, in the hope of providing an analysis which illuminates the way in which content and organisation are inextricably linked in discursive productions. Antaki also notes that this type of research relies heavily on the cultural competence of the analyst, who must be able to draw persuasively upon the cultural resources which s/he shares with readers, in order to construct an interpretation which makes sense of the data.

### Discursive Psychology

For some social psychologists, the adoption of discourse analysis as a methodological approach has provided a new, discursive way of thinking about the nature of psychological knowledge. The claim of discursive psychology is that psychological concepts such as attitudes, attribution, memory and personality can best be understood in discursive, rather than cognitive, terms (Edwards & Potter, 1992; Potter, Edwards & Wetherell, 1993). What discourse analysts noticed was that when they started paying attention to the active and constructive nature of talk, concepts such as ‘attitudes’ and ‘memories’ became

impossible to pin down at the level of the individual. The variability of people's talk seemed to suggest that such phenomena did not reside within the mind of a single person, but were instead produced in social interaction, and the purpose of the interaction thus became influential in shaping the 'attitude' or 'memory' phenomena that were produced within it.

As a result, Edwards and Potter (1992) developed the *Discursive Action Model* to summarise what they believed were the key features of discursive practices. These features are organised around the themes of action, fact and interest, and accountability. They suggest that analysts should focus on the actions achieved through discourse (which are sequentially organised), rather than on the cognitive activities that are assumed in the traditional model to underlie discourse. Edwards and Potter argue that in the course of analysing these actions, the researcher should pay attention to how participants attend to questions about their motivations (stake), and suggest that this is often achieved by constituting their accounts as factual reports of an extant reality. These factual reports are often concerned with issues of agency and accountability (what is the cause of events and who is responsible for them?).

These ideas have been developed further by both Edwards (1997) and Potter (1996a). The relationship between the management of stake and interest and the construction of factual accounts is explored in Potter's book. He argues that all descriptions have both an action orientation and an epistemological orientation:

On the one hand, a description will be orientated to action. That is, it will be used to accomplish an action, and it can be analysed to see how it is constructed so as to accomplish that action. On the other, a description will build its own status as a factual version. For the most part, the concern is to produce descriptions which will be treated as *mere* descriptions, reports which *tell it how it is*. (Potter, 1996a, p. 108)

Thus, a description derives part of its persuasive force from the successful construction of it as a straightforward and factual account of events. If this facticity is accomplished, the description is accepted as a neutral reflection of reality, rather than as relating to the speaker's motives. Conversely, one of the ways to undermine the facticity of a particular description is to point out how it

reflects the interests of the speaker. Potter describes in detail many of the linguistic techniques that can be drawn upon to construct an account as factual, as well as the ways in which these techniques can be countered and undermined. He also discusses in detail the concept of *methodological relativism*. He argues that methodological relativism (suspending judgement about the truth or falsity of knowledge claims) is a necessary starting point for studies of fact construction in general, and studies of science in particular.

Methodological relativism is just one of the principles which Edwards (1997) derives from the sociology of scientific knowledge, to apply to his discursive study of cognition. The others are “the counts-as principle” (focusing study on what participants count as important, real, factual), “the mutually constitutive mind-reality relationship” (considering how participants establish the reality of psychological phenomena in talk), “the merits of close empirical investigation”, and “the usefulness of focusing on error accounts” (the discourse invoked in accounting for error can illuminate the processes of reality and fact construction). In his project, Edwards is interested in taking some of the phenomena of psychology usually conceptualised in cognitivist terms and examining them through a discursive lens. He provides discursive accounts of scripts, emotions and autobiographical memory, among other concepts.

Although discursive psychology is a relatively new field, its approach to the study of memory is well established. Edwards and Middleton (1986a, 1986b, 1986c) were among the first to begin to pay attention to the social and discursive aspects of memory. They noted the central role of language and social context for activities of remembering, highlighting the variety of functions that memory serves in everyday interactions (as opposed to the rather limited ‘reproductive’ goals of laboratory studies). They claimed that Bartlett’s studies of narrative remembering had indicated that memories were reconstructive, rather than reproductive. Now, using the techniques of discourse analysis, memory could be examined in a new way, highlighting the means by which versions of past events were reconstructed in relation to the present business of the rememberers (Middleton & Edwards, 1990).

Edwards and Potter (1992) have developed this approach to memory even further, with their studies of the situated and occasioned memories of John Dean and Chancellor Lawson. One of the issues on which they have focused is the status of 'truth' in the discursive study of memories. They suggest that truth should enter into such studies as a participants' concern, rather than as something to which the researcher has privileged access (because they have some record of the past event which is unavailable to the participants). As they point out, in everyday memory 'truth' is something that is *established* in the construction of past events, through the discursive production of the memory, and may be contested discursively by others. In real life, there is generally no objective record with which to compare people's memories to discern their accuracy. Rather, establishing (and challenging) accuracy is part of the social process of remembering (Edwards, Potter & Middleton, 1992). Engel (1999) calls this process *consensual truth*, and notes that the criteria for establishing truth vary according to the social context in which the memory is produced, and the function which it is performing.

A further feature of the discursive psychology approach to memory is the way in which memory and attribution are conceptualised as being "mutually constitutive". Edwards and Potter (1992) suggest that reports of past events are often invoked and constructed in everyday interactions to deal with issues of responsibility, mitigation and blame. Therefore, one of the ways of understanding the discursive function of memory reports is to consider what attributional business is being attended to in the account. This suggests that reports of past events are never uninterested accounts, but are constructed to work in quite specific and local interactional contexts. Thus, memories are a resource which people can draw upon to achieve interactional business.

This discursive formulation of memory informs my understanding of the psychological phenomena at issue in the memory debate. However, as I have made clear, my own research does not actually focus on memory processes. Instead, I intend to examine how debate participants discursively construct these processes in a rhetorical context. As a student of psychology, I have come to discourse analysis through the work of Potter, Wetherell and Edwards

discussed above. But my interest in studying the memory debate has also been heavily influenced by the analytic approach to scientific controversies which has been developed within SSK. I have also incorporated into my analytic methodology concepts and ideas from the various rhetorical approaches described and discussed in the next section.

### Rhetoric in Various Guises

In recent times, the utility of a rhetorical approach has been rediscovered by many scholars across a wide variety of disciplines. This interdisciplinary interest in rhetoric has seen the development of a new field of academic activity, the rhetoric of inquiry (Nelson, Megill & McCloskey, 1987). The rhetoric of inquiry recognises the central role of persuasive discourse in all scientific and scholarly disciplines which require the interpretation and presentation of research. As Simons notes "... virtually all scholarly discourse is rhetorical in the sense that issues need to be named and framed, facts interpreted and conclusions justified ..." (1990, p. 9). Rather than being the antithesis of rhetoric, scientific discourse is considered to be inescapably rhetorical. Studies in the rhetoric of inquiry focus on analysing the ways that knowledge claims are made and challenged within academic disciplines such as anthropology, economics, and mathematics (and many others; see the volumes edited by Nelson, Megill & McCloskey, 1987; Simons, 1989; 1990a).

The rhetoric of inquiry bears many similarities to the various theoretical and methodological perspectives discussed in this chapter. In common with social constructionism, the rhetoric of inquiry rejects the notion that facts speak for themselves, and focuses instead on how facts are constructed in discourse, and in particular, on how these constructions are made persuasive (Simons, 1990b). In common with discourse analysis, the rhetorical study of inquiry privileges actual talk and text as the focus of analytic attention (Nelson, Megill & McCloskey, 1987). And in common with rhetorical psychology (discussed below), the rhetoric of inquiry acknowledges the central role of argumentation in the business of knowledge production (Nelson, Megill & McCloskey, 1987). All



of these approaches are connected by a common rejection of the objectivist and logical positivist assumptions underlying the scientific method. Indeed, the similarities are such that Simons (1990) claims that rhetorical psychology, social constructionism and discursive studies of science could all be named 'contemporary rhetorics of inquiry'. These points of interconnection form the theoretical foundation for the rhetorically focused discourse analytic methodology which I have utilised in this project. In developing this methodology I have adopted and synthesised ideas from classical and modern rhetoric, discourse analysis and rhetorical psychology.

Billig published his treatise on rhetorical psychology in 1987, the same year that Potter and Wetherell's *Discourse and Social Psychology* appeared. Rhetorical psychology was another attempt to reframe social psychology, by reconsidering central concepts in light of rhetorical theory, first developed in ancient Greece. Billig suggests that cognitivist concepts such as attitudes stress consistency, ignoring the dialogical, argumentative nature of thinking and speaking. He argued for the replacement of the notion of the 'contemplative thinker' with the 'rhetorical thinker' and introduced the idea that thinking is contingent upon dialogue. That is, thinking is essentially talking to (and arguing with) yourself. Billig stressed argumentation as the basis of thought, and borrowed from Protagoras the idea that because there are always (at least) two sides to every argument, any statement is always oriented towards the possibility of refutation.

It is in this sense that the concept of rhetoric has been co-opted into discourse analysis. Edwards and Potter claim that the rhetorical organisation of everyday talk is one of the central concerns of discourse analysis, and that "... in order to understand the nature and function of any version of events, we need to consider whatever real or potential alternative version it may be designed to counter" (1992, p. 28). Potter (1996a) develops this further suggesting that descriptions can be analysed in terms of offensive and defensive rhetoric, where offensive rhetoric is designed to undermine possible alternative versions, and defensive rhetoric works to resist such undermining. Billig (1996) himself has expressed the view that discursive and rhetorical psychology have an "obvious affinity", in their criticisms of cognitivism and focus on language.

Billig (1987) explains that the rhetoricians of ancient Greece, such as Protagoras, Gorgias and Aristotle, were interested in all of the characteristics of convincing oratory, studying both the form and the content of persuasive arguments. However, although rhetoric, as a scholarly discipline, continued to be part of Western culture, the study of the content of arguments was neglected. Rhetoric became little more than a prescribed collection of stylistic devices and 'adornments' which were supposed to add to the eloquence of speeches. In the development of rhetorical psychology, Billig reversed this focus. He was interested in what the model of argument and counter-argument suggested about psychological processes, rather than in the specific presentation of these arguments. However, taking up a discursive approach to scientific controversies requires the analyst to pay attention to both the content and the form of arguments. Indeed, a discourse analyst would consider these two elements to be inextricably linked in a dynamic relationship, with the function of the argument (persuasion) inextricably linked to its construction. In this way, the discourse analyst has much in common with both classical and modern rhetoricians.

Despite classical rhetoric's loss of intellectual status, it has retained a position in the academy, mainly in academic disciplines concerned with written and oral composition. In recent times, the attention of some of these modern scholars of rhetoric has turned to scientific discourse and controversy (Bazerman, 1989; Harris, 1991). The reflexive narrator of Ashmore, Myers and Potter's (1995) review article is somewhat dismissive of much of this work, apparently on the basis that many of these studies go no further than merely identifying the rhetorical devices in scientists' talk and text, which leaves her asking "So what?": Does this simply demonstrate that "... given a long tradition of lists, terms, and broadly defined features, a trained analyst can find them anywhere?" (p. 328). This criticism is a valid one, but it has not deterred me from attempting to integrate concepts from classical and modern rhetoric (such as the broad concepts of ethical, logical and emotional appeals, as well as more specific stylistic devices) into my discourse analytic methodology. It seems to me that although modern rhetoric and discourse analysis are the product of different intellectual traditions, they are both concerned with the fine detail of language

use, as well as broader themes (see, for example, Fahnestock, 1989). I believe that my analysis shows how rhetorical concepts both enrich, and are enriched by a discourse analytic framework which is concerned with explaining exactly how talk and text is constructed to achieve its persuasive objectives.

### My Methodology

To summarise, the analytic methodology which I have developed is grounded in social constructionism, and draws on material from the sociology of scientific knowledge, discourse analysis, discursive and rhetorical psychology, classical and modern rhetoric and the rhetoric of inquiry. The underlying thread which connects all of these approaches is a concern with the constitutive and functional properties of language in action. My basic analytic tool is discourse analysis, specifically focussed upon examining the rhetorical aspects of the memory debate. That is, I am concerned with understanding how arguments are constructed to persuade, how arguments are countered by opponents and how arguments are both oriented to, and a product of, their cultural and communicative context. In order to do this I identify both broad themes (the rhetorical resources available to participants), as well as specific textual devices (the way in which these resources are linguistically constructed to achieve their persuasive purposes).

The specifics of my approach are outlined in the next chapter, but before I move on, I want to explore some of the ethical challenges which have been made to authors taking up a social constructionist approach to the memory debate. These challenges seem to arise from the methodological relativism required by constructionist modes of inquiry. Although there has been some argument in SSK as to whether methodological relativism is even possible, let alone desirable, in studies of scientific controversies (see, for example, the special issue of *Social Studies of Science* edited by Richards & Ashmore, 1996), I have nonetheless attempted to take up a neutral stance in relation to the existence (or otherwise) of recovered memories. The next section is my attempt to resolve the ethical and theoretical issues which have arisen out of this decision.

As I noted in the previous chapter, there is a complex relationship between the ontological commitments of social constructionism and the issues of reality and truth central to the memory debate. Most of the authors mentioned in Chapter Three have engaged with this issue to some degree, and I have similarly had to resolve this complexity in order to develop a rigorous and principled foundation for my analytic approach. From my perspective, there are three major challenges to be answered by social constructionists wishing to study the memory debate. Firstly, it has been argued that social constructionism's relativist ontology provides no basis for judging the relative merits of truth claims, resulting in an "anything goes" attitude towards truth. In relation to the memory debate, where questions of truth are central for participants, this makes social constructionist analysis unethical and/or irrelevant. The second challenge also stems from perceived problems with relativism, specifically the belief that a relativist ontology necessarily denies or disregards the reality of victims' pain, which is regarded as both immoral and obviously nonsensical. Finally, there has also been some concern that taking up a social constructionist stance automatically positions the analyst on the false memory side of the debate, as fm proponents also argue for the constructed nature of memories. The following section addresses each of these challenges in turn, and shows how resolving them has contributed to the development of my own theoretical approach to this research.

### *Truth and social constructionism*

Social constructionists are often characterised (and indeed, characterise themselves) as relativists, rejecting the idea of an objective reality which exists independently of the knower (Held, 1998). In the absence of an objective reality, the common sense understanding of truth (a statement is true if it corresponds with objective reality) becomes problematic (Burr, 1995; Gergen, 1985). As Gill (1995) points out (in her feminist critique of relativism), the implications of this position are that "All knowledge claims are treated with scepticism, and all truth claims as being in principle undecidable, since there is no transcendent standpoint from which 'the Real' can be directly apprehended" (p. 169). It is

argued that as a result of rejecting the concept of an accessible objective reality, relativists are left with no basis for judging the relative merits of truth claims. They are thus compelled to accept all accounts and claims as equally (in)valid (Adshead, 2001). Instead of searching for the truth, the relativist must acknowledge the existence of many truths – this is known as pluralism (Burr, 1995; Sass, 1992).

This pluralist understanding of truth underpins many social constructionist analyses, and has both enabled and required researchers to reconceptualise truth. One approach is to treat truth making as a social process which can be investigated discursively (Edwards & Potter, 1992). In this type of analysis, the researcher is expected, for the purpose of the analysis, to take a neutral (symmetrical) position regarding the veracity of truth claims (Potter, 1996a). As Grint and Woolgar (1995) point out, this does not mean claiming that all truths are equally valid. But constructionist analyses do assess the relative success of competing truth claims by investigating the social, discursive processes involved in their construction, rather than assuming that success is a product of a simple correspondence between the claim and an objective reality. This symmetrical stance towards truth is demonstrated in the work of Davis (2000) and McMartin and Yarmey (1998, 1999) described in the previous chapter.

However, some authors find this understanding of truth problematic in relation to studies of the memory debate. Their objections seem to be based upon concerns that, in this case, it 'really does matter' whether or not recovered memories are true or false (real or illusory) – it is the prime concern for participants, and should also be the prime concern for researchers. Not surprisingly, this concern is most likely to be articulated by authors who have an *a priori* commitment to a position in the debate. For example, S. E. Scott (1997) is concerned that the influence of postmodern pluralism has undermined the ability of feminist academics to make a strong critique of false memory syndrome, a critique which she sees as necessary to prevent the reestablishment of a culture of disbelief surrounding child abuse. In contrast, Kihlstrom (1998) and de Rivera (1998b, 1998c) are concerned about the

suffering that is caused when people believe things about their past that are not true:

At this moment there are thousands of families trying to contact children who believe they were incestuously abused. We cannot in good conscience simply say that reality is constructed. One of the realities is realer and we must act. (de Rivera, 1998c, p. 161).

All of these authors are concerned that taking up a relativist position in relation to this debate is ethically unsound because of the practical consequences which they believe will follow – an inability to act in the world to protect those in need of protection (of course, the membership of this category varies depending upon *your* positioning in the debate).

One possible, though problematic, way to deal with this ethical challenge is to take up a position which Edwards, Ashmore and Potter (1995) have labelled “bottom-line relativism”. This involves conceding that while many things are a product of social construction (and thus require a relativist understanding), there is a certain category of things (on the other side of the bottom line) which are objectively real. Often this line is drawn between cultural and natural artefacts. For example, Hacking (1997, 1999) places his bottom line between ideas/concepts on the one hand and objects/practices on the other, though he does acknowledge that ideas and objects interact to such an extent that it is often difficult to differentiate between them. This highlights one of the major problems with bottom line relativism – that someone has to decide where to draw the line. Without that person having direct access to objective reality how can they be sure that their line is in the correct place (Edwards, Ashmore & Potter, 1995)?

It is possible to deal with this problem by treating some categories of things as being self evidently real, as in this passage from Sarbin, where he articulates his version of the bottom line (which is similar, but not identical, to Hacking’s):

As ambiguous as it is, human beings (at least in the Western world) need an ontological construction such as “reality” against which to assess fiction or imaginings. But we cannot get away from the premise that reality is a social construction ... Having said this, it would be fatuous, for example, to say that the brick wall is only a social construction. One quickly learns that you cannot walk through brick walls. However, the meanings we assign are social constructions: Is the brick wall a form of protection, or is it a prison? (1998c, p. 306).

In other words, reality is social constructed, except for the things that you cannot walk through. Invoking the 'brick wall' – the material object whose reality is undeniable – is a classic bottom line argument (which has been elegantly dismantled by Edwards, Ashmore and Potter, 1995). However, it seems odd that Sarbin makes such an appeal in this context, where presumably the reality which he is wanting to assert is the reality of past events (“against which to assess fiction or imaginings”), which are phenomena of a very different kind from brick walls.

This brings us to the critical limitation of bottom line arguments in relation to the memory debate – they do not take into account the indeterminate nature of past events (Hacking, 1995). Sarbin, for example, wants to maintain an objective and knowable reality of past events in order to make a judgement about the accuracy of recovered memories. This is critical for his entire argument, because saying that a person's recovered memory can more accurately be described as a 'believed-in imagining' implies the ability on behalf of the researcher to differentiate between the two (which is impossible without believing that you have access to an objective version of past events). Unfortunately, the temporal nature of past events in real life precludes this kind of objective knowledge (as Edwards & Potter, 1992, point out). As Reviere (1997) notes, neither therapists or researchers have any way of infallibly determining the truth about an individual's personal past (see also Adshead, 1997).

At the most basic level, there is rarely any kind of record of most of the personal events of people's lives (Singer, 1997). This is particularly true of events such as child sexual abuse, where in most cases perpetrators go to great lengths to avoid leaving any evidence that would result in detection (Saraga & MacLeod, 1997). However, as Hacking (1995) points out, even if such a record did exist, it would still be open to interpretation and redescription (he also suggests that this “retroactive redescription” can actually change the past; for a critique of this argument see Sharrock & Leudar, 2002). This kind of interpretation occurs whenever corroboration of past events is sought - the evidence rarely (if ever) speaks for itself (Burman, 2001a).

Given that no-one has any way of definitively knowing the objective truth of an individual's personal past, Bowers and Farvolden (1996a, 1996b) question whether it is actually ethical to pursue the goal of truth in relation to memories. They are particularly concerned that the idea of 'Truth' does more harm than good in therapeutic relationships, because of the propensity for 'Truth' to be determined by the therapist's own prior theoretical commitments, in spite of the therapist's inability to determine the client's past with any measure of certainty.

Although this criticism is directed mainly at clinicians, it also raises relevant issues for researchers involved in the memory debate. As Bekerian and Goodrich (1995) note, in spite of their general knowledge about the mechanisms of memory, it is impossible for any psychologist to determine who is telling the truth in an individual case. Therefore, it could be argued that taking up a relativist stance in relation to memories is actually *more* ethical than the alternative realist positions, because such a stance honestly acknowledges that the researcher does not have any privileged access to the truth about the past. This position does not deny that "what really happened" is an important issue for participants and researchers. However, it recognises that the indeterminate nature of past events defines an epistemological boundary, with the result that knowledge claims about the past are, by their nature, always open to challenge. Investigating the ways that these challenges are made and refuted is one of the ways that social constructionist researchers can make a relevant contribution to this debate (see Atmore, 1999).

### *Experiential reality and social constructionism*

The second major critique of relativism in relation to the memory debate is that committing to a relativist ontology means denying the pain and suffering of the victims directly affected by recovered memory/false memory syndrome. For example, Schuman and Galvez (1996) are concerned that in taking up such a position "... there is a danger of denying women's corporeal trauma. If the postmodern discourse argues that we have no real selves, no core intuition, then, perhaps, we have no real pain, or we have not really experienced sexual



abuses" (p. 26). Here a relativist ontology is linked to a rejection of the real – the real self, the real pain, the real experience.

The importance of the real event is similarly affirmed by de Rivera (1998) who argues that "[s]ome experienced realities are realer and anyone who has suffered an injustice knows how important 'real' Reality is" (p. 154). He goes on to challenge relativists to tell various oppressed individuals (Holocaust survivors, citizens of tyrannical military governments, rape victims, parents of children who have recovered memories of abuse) that truth is constructed, and therefore their experiences may not have really happened. Clearly, even the most committed relativist would shrink from such a task. Consequently, (according to de Rivera's logic) relativism is an indefensible moral position in such cases.

On closer examination, it appears that this type of argument is based on a fundamental misunderstanding of the relativist/social constructionist position. Burr (1998) suggests that this misunderstanding results from the fact that "reality" has not one but three meanings (and thus antonyms) in the English language. Reality can mean truth (as opposed to falsehood), materiality (as opposed to illusion) and essence (as opposed to construction). She notes that when social constructionists argue against an essential reality (and for a constructed one), they are heard (or read) as also rejecting truth and materiality, even though this is not part of their claim. Most constructionists do not argue against the existence of reality (this is a quite separate philosophical position, known as "idealism"), but about the nature of reality (socially constructed rather than observer-independent). Material reality is thus not rejected, although it is problematised.

This problematisation arises from an understanding of the material and the discursive as inseparably linked (Brown & Pujol with Curt, 1998). Events and practices are enmeshed and embedded in a discursive context (and vice versa). This makes constructionist analysis of all kinds of material realities not only possible, but appropriate. As Hepburn (2000) notes, even awful experiences such as child abuse are not independent of discursive practices, but " ... to

claim that such things are not simple freestanding objects is not to treat them as any less important or shocking” (p. 98).

### *False memories and social constructionism*

The third and final challenge is related to the apparent similarity between the postmodern/social constructionist understanding of memory and that espoused by fm proponents. As discussed above, discursive psychologists, working from a social constructionist perspective, have argued that memory is a socially mediated process which is constructive, rather than representative.

Concurrently, experimental psychologists such as Loftus (1992, 1993) have developed an understanding of memory as malleable; that is, vulnerable to the effects of suggestion and post-event information. The fm argument draws on this notion of the malleability of memory when it claims that recovered memories are really a product of therapeutic suggestion. Since both groups claim that memories do not simply ‘reflect’ past events, but are constructed within a social context, it appears to some observers that the social constructionist and fm understandings of memory are one and the same. Clearly this could be problematic for the constructionist researcher who does not wish to be positioned as being on the side of the false memories – or any side at all for that matter (although the ability of the analyst to avoid being ‘captured’ in this way may be limited – see Scott, Richards & Martin, 1990).

This potential dilemma is noted by Davies (1995), who believes (erroneously) that “... the image of memory put forward by advocates of FMS is culled from social constructionist forms of thought” (p. 15). It is also touched upon by S. E. Scott (1997), Schuman and Galvez (1997) and Burman (2001a) who all note what they see as the troubling tendency of fm supporters to exploit the conceptual possibilities opened up by post-modernism. The common response of these authors is to point out that the fm position is not constructionist *enough* (to be authentically constructionist), in that it maintains a commitment to some kind of fundamental truth.

Extending this argument, I would suggest that, despite appearances, the fm position is not constructionist *at all*, in that it is based not upon constructionist notions of socially mediated remembering, but upon experimental studies of memory malleability. This distinction is important because it highlights the fundamental epistemological differences in the two positions. Laboratory studies of memory malleability, as Edwards and Potter (1992) explain, are intrinsically different from constructionist studies of everyday remembering. In the lab, the experimenter acts as the validator of the accuracy of subjects' memories by retaining privileged access to the original memory input (for example, a list of memory words, or a video of a car accident). Thus, the experimenter is able to make a judgement about the truth or falsity of memory. In contrast, in everyday memory (the kind that goes on in the 'real world', and that discursive psychologists study), no one has access to the original input (the past event). So in everyday remembering, a 'true' or 'accurate' memory is a memory that is an "acceptable, agreed or communicatively successful version of what happened" (Edwards & Potter, 1992, p. 75).

Given these different approaches to the study of memory, it should be clear that the constructionist understanding of memory is quite distinct from the fm position. In order to make a judgement that a memory is false, someone has to retain a privileged position in relation to past events – to be the possessor of the 'real story' about what went on. In the fm understanding, this person is the accused parent(s). While the adult child's memories of abuse are considered to be the constructed result of social influences, the parent's memories of blameless behaviour are considered to be an accurate reflection of 'what really happened'. In contrast, a constructionist approach to memory acknowledges that *all* memories are socially mediated constructions, that access to the 'real event' is impossible and that researchers should therefore maintain a neutral stance with regard to the accuracy of any specific memory.

### *Echoes of critical realism*

Those who have been following the critical realism vs. relativism argument in constructionist psychology will recognise the echoes of this debate in the challenges discussed above. In this debate, critical realism is suggested as an

alternative to a relativist epistemology, making issues of embodiment, subjectivity, materiality and power central. Grounded in relativism, social constructionism is accused of ignoring or excluding these important issues, and thus producing a flawed and impoverished understanding of the human condition (Burr, 1999; Cromby & Nightingale, 1999; Harré, 1999). The objections to social constructionist analysis of the memory debate noted here are motivated by similar concerns. For example, when the memory debate is understood as a struggle for the power to define how memory works, a relativist problematisation of the rhetoric of truth and reality threatens to undermine these concerns. Similarly, some critics are concerned that social constructionism, which they see as ignoring materiality, denies the experience of abuse and in effect tells victims that “it didn’t really happen”.

However, as I have argued in my responses to these objections, I do not believe that taking up a relativist social constructionist standpoint requires a denial of truth, reality or materiality. What relativism does highlight, though, is the way in which judgements about what is true and real are discursively accomplished, and the way in which material experiences and discourse are inextricably linked. It is true that a social constructionism grounded in relativism denies me, as a researcher, the ability to ascertain whether recovered memories are true or false. But as I have noted, it is arguable whether it is possible to do this from within any perspective, due to the epistemological boundary imposed by the indeterminate nature of past events. Certainly, it is apparent that a definitive conclusion has not yet been reached within the traditional positivist framework of psychology, despite years of research and debate.

The only suggestion for a resolution so far is a compromise position which accepts that both memory recovery and false memory construction occur, and proposes that the truth of each case be decided on its own merits (Terr, 1994). Of course, these judgements cannot be made in relation to “what really happened” – that information is not available. Instead these judgements about the accuracy of memory must be made on the basis of evidence which is (inevitably) discursively produced, and open to discursive challenges. Given

that all claims about memory (both memory processes and individual memories) are never decided outside of language, I believe that a constructionist-based discursive approach is uniquely suited to the study of the memory debate. The indeterminate nature of past events poses a real problem for positivist researchers of memory. The fact that debate continues unresolved is probably attributable to this very problem, because it makes all research findings (and all individual memories) potentially contestable. However, taking up a constructionist stance allows me, as a researcher, to consider such issues in a way that is both relevant and ethical.

In this chapter I have outlined the theoretical and methodological underpinnings of my project, outlined my analytic approach and discussed some of the ethical challenges resulting from the standpoint I have taken up. In the next chapter I discuss my project in more detail, including a description of the data, an explication of my analysis, and a discussion of reflexivity.

## **Chapter Four: Doing the Research: Selection, Interpretation, Inscription**

In this chapter, I introduce the data and analytic process which I have used in this study of the memory debate. Firstly, I discuss the reasons why I decided to make 'pop psychology' texts the focus of my investigation, and how I went about making a selection of specific books to analyse. I then introduce these texts, providing a brief précis of each. How I went about the process of interpreting these texts is then explained, and finally I discuss the form in which I have chosen to present this interpretation to the reader.

### **Refining the Focus of the Analysis**

A central task in any discourse analytic project is generating or selecting the discourse to be analysed. This task is both guided and constrained by the researcher's aims. For example, as I explained in Chapter Two, my intention in this thesis is to identify and describe the major rhetorical resources and strategies utilised by protagonists in the memory debate. What this means is that my analytic attention is directed to studying the debate itself – the actual talk and text produced by a wide variety of interlocutors as they have argued over the status of recovered memories. This excludes certain categories of data from consideration. For example, it would make little sense, given this goal, to collect data by interviewing debate participants – when what I want to examine is their actual rhetorical practice in the debate, rather than what they might tell a researcher about it. I want to explore naturalistic data from the debate, not oriented to the context of a research interview, but in the original context of its production (see Potter & Wetherell, 1987; Edwards, 1997).

However, the problem remains that over the last twenty years, a vast amount of such rhetorical material has been generated – far more than even the most diligent researcher could attempt to engage with at the level of detail required for a discursive analysis. Furthermore, this material has been produced in and for a wide variety of communicative contexts, including (but not limited to): academic journals, books and conference presentations; self-help and "pop

psychology” books; court cases; television documentaries and talk shows; radio interviews and talk-back; internet web pages, notice boards, user groups and discussion forums. All of the people producing such material in these various contexts could therefore be classed as protagonists in this debate.

Indeed, one of the key features of the memory debate is the way in which it has played out in both academic and public arenas. Like other recent scientific controversies with significant social implications, it has blurred the boundaries between professional, academic and public debate (Beacco, Claudel, Doury, Petit & Reboul-Touré, 2002), and between the public and private domains. For example, several prominent academic and clinical scholars have published ‘pop psychology’ books supporting recovered or false memories, and have featured prominently in the many media reports and discussions of the issue (including appearances on day time talk shows). Conversely, a number of the key protagonists in the debate are not professionally or academically qualified in psychology. In addition, the personal stories of some key figures (such as the Freyd family) have become part of the wider debate.

It is this cross-contextual feature of the debate which has directed my selection of texts for this analysis. McMartin and Yarmey (1998, 1999) state that the texts they analysed were mainly from the “scientific domain” (although they do claim to have examined some lay texts – unfortunately their sources are not listed in their articles). They note this as a limitation of their study, in that the academic context undoubtedly constrained the use of rhetorical devices that would be present in other discursive genres. Thus, in the present study I was eager to engage with material that was produced for a wider audience than the community of academic and practicing psychologists. In order to gain a reasonably comprehensive view of the debate, I also wanted to examine material which presented a large number of arguments and counter-arguments, and get an idea of how these had developed over the course of the debate (which has spanned at least a decade). Finally, it was also important that this material be available and accessible to me from my chronological perspective (see Potter & Wetherell, 1987, who note that the availability of data often provides an “analytic starting point” for discursive research) . Since I have come

to the study of this debate after much of the argumentative activity has already taken place, many of the more ephemeral forms of public discourse would be difficult, if not impossible, to locate and access.

With these criteria in mind, I decided to focus my investigation on the self-help and 'pop psychology' books which have been published about recovered and false memories. These texts are particularly intriguing because they represent the intersection of academic and lay interests in the memory debate. They are authored for a general audience, yet reflect, utilise and influence many of the arguments in the academic literature. This characteristic of popular texts thus allows authors to employ and develop a wide range of arguments. When publishing in academic journals, authors are constrained not only by the requirement that articles be reasonably concise, but also by the conventions of scientific writing (Gilbert & Mulkay, 1984; McMartin & Yarmey, 1999). On the other hand, situations such as interviews for radio, television or other publications are often not conducive to presenting technical research information or statistical analyses. But in a book written for "the intelligent general reader" (Pendergrast, 1996, p. 18), there are few such limitations. This does not necessarily mean that 'anything goes' in these texts of course. Like all discursive contexts, this one has its own specific constraints, moderated by editors' and publishers' desires to produce texts which they believe will appeal to the public's interest, and thus produce a profit. However, it does mean that the author can include almost any material which they feel will be persuasive to their intended audience, including research evidence, anecdotes, criticism of opponents and so on. This allows for the analysis of a wider range of rhetorical strategies in this debate than has previously been undertaken.

A further advantage of analysing popular texts is their relative accessibility. I had no difficulty in obtaining copies of the texts for the purposes of my research. All of these books were published in the public domain, and thus constitute naturalistic data. When conducting a discursive analysis of publicly available texts the question of whether the author's consent is required is not often considered. Nonetheless, this issue is clarified by the American Psychological Association's (APA) ethical principles and code of conduct (2002) which



exempts researchers from gaining informed consent where the method involves naturalistic observations or archival research. Thus, informed consent was not required or obtained for this research. However, I have attempted at all times to respect the intellectual property rights of authors by providing full citations for all quoted material.

### A Selection of Texts

My decision to analyse popular texts raises the question of categorisation – how can such texts be recognised and differentiated. This topic is not well theorised in the psychological literature, and definitions tend to be broad, with ‘pop psychology’ usually meaning any discussion of psychological matters directed to a general audience, rather than to other psychologists (Fried, 1998). However, despite the lack of academic attention accorded to them, the publication of books about psychology for lay audiences has a long standing history (Fried). Many of the psychological books published for a general audience also have a therapeutic focus – they are aimed at providing the reader with skills to improve their own lives by dealing with a specific problem, and are thus known as ‘self-help’ books (Plummer, 1995; Sommer, 2003).

There is no set of rules for the classification of books into these genres, and as a result I have made the selection of my titles for analysis purely on my own judgement of their intended audience. I based this judgement on a number of features of the texts such as the publisher (non-academic), absence of detailed experimental findings and inferential statistics (few or no tables/graphs), title and cover (descriptive or emotive), and also, where available, on the authors’ and/or publishers’ characterisations of their intended audience. For example, on the inside cover of Yapko’s (1994) text, the reader is told that the book addresses the issue of recovered and false memories in “plain language”, indicating that it is intended, and suitable, for an audience unacquainted with psychological jargon.

Rather than attempting to analyse all of the popular texts available (for the reasons of time and capacity discussed above) I have chosen ten texts published between 1988 and 1997. I felt this would provide enough data to produce a broad range of arguments, and thus facilitate the recognition of patterns in the utilisation of rhetorical strategies, while still remaining manageable (see Potter & Wetherell, 1987 for a discussion of sample size in relation to discursive analyses). The specific texts were chosen both for the importance of their contribution to the debate and for their degree of referentiality; that is, the extent to which the authors engage with each other's arguments (see McMartin & Yarmey, 1999). Five of these texts were written by proponents of recovered memory: *The Courage to Heal* (Bass and Davis, 1988, 3rd edn 1994), *Secret Survivors* (Blume, 1990), *Repressed Memories* (Fredrickson, 1992), *Trauma and Recovery* (Herman, 1992, 2<sup>nd</sup> ed. 1997) and *Unchained Memories* (Terr, 1994). The first three titles could all be described as self-help books, while Herman and Terr's texts present broader analyses of trauma and memory. The other five texts all provide arguments against the recovered memory movement, and some also include self-help advice for falsely accused parents: *The Myth of Repressed Memory* (Loftus and Ketcham, 1994), *Making Monsters* (Ofshe and Watters, 1994), *Return of the Furies* (Wakefield and Underwager, 1994), *Suggestions of Abuse* (Yapko, 1994) and *Victims of Memory* (Pendergrast, 1995, 2<sup>nd</sup> edn 1996, British edn 1997). In order to give the reader a general sense of the style and content of each of these texts, I present a brief description of each book here.

*The Courage to Heal: A Guide for Women Survivors of Child Sexual Abuse*  
(by Ellen Bass & Laura Davis)

As explained in Chapter One, this book was one of the first self-help books to be published (in 1988) which told women that they might have repressed memories of abuse, and how to go about recovering them. The text is organised as a guide to healing from the effects of abuse (both remembered and repressed), and draws its mandate from the personal experiences of survivors. Bass identifies herself as a writer, teacher and counselor; Davis as a sexual abuse survivor (on the basis of her recovered memories).

*Secret Survivors: Uncovering Incest and its Aftereffects in Women* (by E. Sue Blume)

According to the biographical note at the end of her book Blume is a social worker, private therapist and lecturer/trainer. Her text focuses on describing and analysing the consequences of incest, including repression of memories, with two final chapters offering suggestions for dealing with these consequences. Her text seems to be directed at helping women identify themselves as sexual abuse survivors, especially through the use of her Incest Survivors Aftereffects Checklist, developed in her clinical practice.

*Repressed Memories: A Journey to Recovery from Sexual Abuse* (by Renee Fredrickson)

Fredrickson is a PhD qualified psychologist, and writes from her clinical experience. The book is a self-help manual which explains the concept of memory repression and how the reader can know if they may be repressing memories. It offers many suggestions and specific techniques to encourage the recovery of these repressed memories, on the basis that remembering is healing in and of itself.

*Trauma and Recovery* (by Judith Herman)

Herman is a psychiatrist and Harvard University professor, and her book is slightly different from the other texts selected, in that it does not focus specifically on repressed memories, or even childhood sexual abuse. Rather, she presents an argument that a wide range of traumas (including sexual abuse) have similar psychological effects, and then presents a process for healing from these effects. However, Herman's clinical research and experience with memory repression and recovery (as outlined in Chapter One) forms the basis of her work on trauma.

*Unchained Memories: True Stories of Traumatic Memories, Lost and Found* (by Lenore Terr)

Terr, also a psychiatrist and professor at the University of California, has written her book as a series of short stories about various kinds of memory repression.

Most (but not all) of these are stories about adults recovering traumatic memories from childhood. Interestingly, one story illustrates the formation of what Terr says is a false memory (in a twelve-year-old girl). She also discusses her involvement in the Franklin trial as an expert witness.

*The Myth of Repressed Memory* (by Elizabeth Loftus and Katherine Ketcham)

This text is a comprehensive problematisation of the concept of repressed memory. It includes stories of accused parents and retractors, and information on what psychologists 'know' about how memory works, as well as outlining how Loftus (a highly respected memory researcher and psychology professor, employed at the time at the University of Washington) became involved in the memory debate as an expert witness in the Franklin trial, and the development of the "Lost in the Shopping Mall" experiment. Katherine Ketcham, her co-author, is a professional author of self-help and pop psychology texts.

*Making Monsters: False Memories, Psychotherapy, and Sexual Hysteria* (by Richard Ofshe and Ethan Watters)

In this book, the authors focus on how false memories can be created, utilising information from recovered memory texts to demonstrate their claim that suggestive techniques are employed inappropriately by rm therapists. They also argue that satanic ritual abuse and multiple personality disorder are the result of similar suggestive practices. The involvement of Ofshe (a cult expert, and professor of social psychology at the University of California) in the Paul Ingram case is also outlined by his co-author, Watters (a freelance magazine writer).

*Return of the Furies: An Investigation into Recovered Memory Therapy* (by Hollida Wakefield and Richard Underwager)

These authors are both psychologists who have worked extensively in the area of child sexual abuse, and acted as expert witnesses in court cases. Their text situates recovered memory in the context of a wider "child abuse system", which they claim is founded on anti-sexuality, paranoia and a lack of reasoned thinking about abuse accusations. Their text includes scientific arguments against repression, stories of parents and retractors, data collected from FMSF members, and suggestions for evaluating 'good' and 'bad' therapists.

*Suggestions of Abuse: True and False Memories of Childhood Sexual Trauma*  
(by Michael Yapko)

Yapko, a clinical psychologist and hypnotherapist, organises his text around the results of a survey he conducted which indicated that many therapists had a poor understanding of the nature of memory and the suggestive powers of hypnosis. He uses this data to argue that many (but not all) of the memories recovered in therapy are false. He also offers advice to falsely accused parents and other family members on how best to cope with the accusations and maintain contact with their child.

*Victims of Memory: Incest Accusations and Shattered Lives* (by Mark Pendergrast)

The centrepiece of Pendergrast's book is a series of interviews with therapists, survivors, accused parents and retractors. Pendergrast provides commentary on these interviews, and flanks them with chapters in which he presents scientific and historical evidence to argue that recovered memories of abuse are likely to be false. Pendergrast is a professional writer (journalist and author), who also identifies himself as a falsely accused parent.

### Analysing the Texts

Once the ten texts were selected, the first task was to begin to acquaint myself with their content, as a preliminary to the 'close reading' that is required for analysis. My first reading of these texts was designed to familiarise myself with the arguments made, and to gain a broad sense of the ways in which they were differentially deployed by authors. As I read through the texts I made brief summaries of the content, in an effort to help me retain an overview of the material (these summaries were not designed or used as analytic tools).

After my first read through, I returned to the texts to begin the process of coding. In coding these texts, I followed the general procedure outlined in Tuffin and Howard (2001), but informed specifically by my research aims. As I was

interested in analysing a range of persuasive devices and arguments utilised by authors, I considered all the text contained in my ten books (3,414 pages of text) as potentially relevant to my analysis. Thus the practice of preliminary coding and discarding of irrelevant material was not appropriate here, apart from the obvious measure of excluding indexes and bibliographies from consideration.

In coding the texts, I followed the practice, advocated in Tuffin and Howard (2001), of allowing coding categories to *emerge* from the data, rather than imposing them at the outset. As I read the material, I was interested, in the first instance, to identify the broad rhetorical resources that I felt the authors were deploying to persuade their readers. That is, I was interested in finding out what the authors were *actually doing*, rather than trying to impose the framework of classical rhetoric onto the texts – a style of rhetorical analysis which appears to contribute to Ashmore, Myers and Potter's (1995) less than enthusiastic assessment of the utility of rhetorical approaches to studying scientific discourse.

My ability to do this was aided by my ignorance, at the point of initial coding, of classical rhetorical theory. It was only after I completed this initial stage, and began undertaking the finer work of analysis, that I turned to the rhetorical literature in the hope that it would help me 'make sense' of the practices of argument which I was encountering. At this point I had decided (after some initial false starts and reorganisations) that the main argumentative strategies utilised by the authors could be organised into five broad categories, which I decided to call 'rhetorical resources'. The first of these resources was concerned with issues of authorial credibility. The second focussed on descriptions and definitions of memory phenomena. The third resource was oriented around science, including both the utilisation of specific scientific evidence and broader claims about the nature of science and scientific values. The fourth resource was the presentation of evidence from previous historical eras. And the final resource which I included in my categorisation was the recounting of experiential narratives.

After this initial process of coding was completed, I began the task of analysis. Firstly, I noted that within each of these broad categories there could be found examples of both offensive and defensive rhetoric (Potter, 1996). Sometimes this rhetoric was oriented towards arguments made in other texts and forums, while at others it was oriented towards anticipated, rather than actual, criticisms and arguments (see McMartin & Yarmey, 1999). The dialogic nature of this material helped to structure my analysis of these resources, as I considered how arguments were constructed to counter and resist possible or actual opposition (Edwards & Potter, 1992). Secondly, since the texts had been produced in the context of a debate, I approached the textual examples in each category with the assumption that their function was to persuade the reader of the rightness of the particular author's position. With this in mind, I attempted to analyse exactly how the text was constructed to achieve this persuasive goal. I was interested in identifying the discursive features of the text which made it seem factual, true and correct to the reader. I found utilising concepts from both discursive and rhetorical analysis useful at this point, and where appropriate I tried to synthesise these two approaches to provide a more complete explanation as to how the dialogical and persuasive functions of the text were achieved. Obviously I was not able to examine every sentence in every text at this level of detail, so in my analysis I have chosen excerpts which exemplify the particular features of interest to support my arguments, and have analysed many of these excerpts in more detail to highlight specific linguistic constructions.

### Writing the Research

Finally, I turn to a consideration of an important issue in my research – that is, the textual form in which I have chosen to present my analysis in this thesis. A consideration of this issue is necessary given the disciplinary groundings of my methodology, and my adoption of a relativist social constructionist epistemology. In many disciplines where a relativist stance has been taken up by researchers, and most particularly in SSK, there has been a recognition that the knowledge produced by relativist research is just as much a social

construction as the knowledge produced by more conventional, realist research methodologies (Ashmore, 1989; Woolgar & Ashmore, 1988). This recognition has led to a concern with reflexivity – that is, producers of such knowledge are expected to recognise its constructed nature, and their own role in its construction. For example, Woolgar (1989) suggests that rhetoricians studying science should acknowledge the rhetorical nature of their own analyses. One of the ways which researchers have taken up the challenge to be reflexive is by highlighting the constructed nature of their analytic texts through the use of unconventional styles and forms of writing. These ‘new literary forms’ include, but are not limited to dialogues, parallel texts, and various fictional forms (see Ashmore, 1989, for a more complete list and discussion of the issue of reflexivity). By writing in different ways, researchers can disrupt the expectations of the reader, so that they are continually confronted by the constructed nature of the text, rather than slipping into the realist mode of reading which is prompted by the use of conventional academic writing styles.

Another advantage of reflexive analysis, suggested by Ashmore (1996), is that it may deal with the problem of ‘capture’ of symmetrical SSK analyses raised by Scott, Richards and Martin (1990). These authors noted that in their experience, the analyses which they produced from a symmetrical, impartial SSK stance ended up becoming part of the very controversies which they had intended to analyse. This was because, in highlighting the constructed nature of scientific positions, they inevitably provided argumentative ammunition that the ‘underdogs’ in these controversies could use to undermine the scientific authority of their opponents. Thus, Scott, Richards and Martin concluded that the neutrality and symmetry of research required by the strong programme, was, in practice, impossible to achieve because the research so produced always tended to be more ‘useful’ to proponents of the less scientifically credible position in the controversy (the underdog). Ashmore (1986a) was not convinced of the inevitability of symmetrical, relativist analyses being more useful to the ‘underdogs’ than to the more orthodox position (as is perhaps evidenced in the memory debate, where some commentators are concerned that relativist approaches to truth and authenticity of memories actually supports the more scientifically orthodox position). However, he argued that if this



possibility of 'capture' is a concern to researchers, then a possible answer is to provide a thoroughly reflexive analysis as "... a reflexive study will seem *self-undermining* to all concerned and therefore of no use to anyone" (Ashmore, 1996, p. 307).

While the employment of new literary forms in writing analyses is a useful way of demonstrating a researchers' reflexive commitment, there are also possible disadvantages of using these forms. For example, Woolgar and Ashmore (1988), and Pinch and Pinch (1988) all note the potential for readers to be annoyed by the adoption of unconventional writing forms, especially if they are unfamiliar with the practice. Pinch and Pinch (1988) also notes the potential of these new literary forms to undermine the serious findings of the researcher, as they raise issues regarding the authenticity of the text. In this regard, Pinch is particularly critical of the "second voice device" (in which a notional interlocutor is introduced to provide tension and critique), which he sees as artificial, and as obscuring the (actual) univocality of the texts which employ it.

These potential problems are of particular concern to me as the producer of a doctoral thesis. I am aware that I am producing this text as an academic exercise, and that at least three of my readers will be reading with the purpose of making a judgement as to whether the quality of my work warrants the award of a PhD. However, I do not know to what extent each of these readers will be familiar with and comfortable with reflexive writing. I am particularly concerned with not 'annoying' readers whose final judgement of the text will determine the academic success or failure of this thesis. Furthermore, in deciding how to present this work, I was also concerned that the 'playful' tone of some reflexive forms of writing seemed inappropriate to employ in a thesis in psychology (where reflexivity is less well established than in SSK). However, in saying this, I was also cognizant that it was important that I show my recognition of the constructed nature of my own interpretations in this analysis. I was also aware of Potter's (1988) argument that the writing up of a discursive analysis is, by its nature, a reflexive form, in that it lays bare the processes of interpretation engaged in by the researcher and thus draws attention to the constructed nature of the analytic reading.

Given all these considerations, I have dealt with issues of reflexivity in this thesis in a number of ways. At the most basic level, I have attempted to situate myself in the text through the use of the first person. I have also tried to provide (following Wynne, 1988) occasional commentary on my text by discussing some of the analytic issues, questions and complexities which I have grappled with in the production of this thesis. However, I have integrated these commentaries into the body of writing (occasionally in the form of textual 'interludes'), rather than using any kind of 'multi-voicing' device, and by doing so, hopefully preserved the authenticity of my text. I have also, on occasion, self-consciously employed the rhetorical strategies of persuasion which I have pointed up in my analysis (see, for example, the interlude following this chapter). I have also included this discussion of reflexivity here, making the reader aware of my own awareness of the constructed nature of my analysis, and thus hoping that they will excuse any lapses into the 'traditional' analytic language of 'discovery' that may appear later in the text (Gross, 1990). Given these considerations, I ask that the reader consider my decision not to employ a new literary form in the production of this thesis as a failure of imagination and courage on my behalf, rather than as a conspicuous lack of reflexivity!

At this point, having explicated the historical background, theoretical grounding, methodological antecedents and research process of my study, it is time to introduce the actual analysis. The following five chapters explore the five rhetorical resources discussed above, presenting my interpretations of the construction and function of the author's texts. I begin this analysis with a discussion of the resource of authorial credibility, just as most of the popular texts begin with an attempt to establish the integrity and authority of their producers.

But first, a brief interlude ...

### **Interlude: That sounds very interesting ... but what do you really think ?**

During the time I have been involved with this project I have had numerous opportunities to discuss my research with interested (or sometimes just polite!) colleagues, friends and acquaintances. When I explain what I have been doing, the response which I most frequently hear is a version of - "Well, that all sounds very interesting ... but tell me what you really think about recovered memories"! Even those of my fellow students working within similar critical, constructionist frameworks, while showing an appropriate degree of interest in my stated research topic, have asked this question. The underlying assumption appears to be that the symmetrical approach which I have taken in relation to the issues of the debate, while being a necessary element of the research persona, is inauthentic at a personal level. I must have *some* opinion about what is really going on here. In other words, like the positivist, empiricist researchers I have spent so long critiquing, I have subjugated my subjective responses to the debate in order to perpetuate the myth of my neutrality. The irony of this is not lost on me.

Nevertheless, it would be inauthentic of me not to admit that, over the course of the research, I have held a number of opinions about repressed and false memories. These opinions have not been static, though, and have developed and changed over time. As I noted earlier, I did not come to this research with any personal investment in the issue. Indeed, I suspect that conducting research of this type would be next to impossible for a researcher with any strongly held personal commitments on this topic. However, through the path my reading had taken prior to beginning this study, I was initially more acquainted with the fm arguments in the debate. Furthermore, given my background in discursive psychology, I found the idea that memories were constructed in social settings to be quite unremarkable. Also, many experiences in my own life had given me an appreciation of my own memory's malleability and inaccuracy.

Thus, I was reasonably convinced that it was possible for memories to emerge in a therapeutic setting that were totally convincing to the rememberer, and yet

might not correspond to actual events in the person's past. However, I felt that it was, in principle, impossible for an outside observer to state with any degree of confidence whether this was the case in any specific instance. That is, I did not believe that anyone (therapist, researcher, friend or family member) could make a definitive judgement on the 'reality' of another person's memories. Thus, I also remained open to the possibility that memories could be repressed and then later recovered, though I found it hard to understand how exactly this would occur, as on a personal level I felt convinced that I would remember any traumatic event that occurred in my own life.

However, a specific incident that occurred about two years into my research served to alter my opinion in this regard. As a PhD student, I supplemented my student income (from a university scholarship) with various part-time jobs over the years. For about 18 months I worked as a writing consultant at Massey University's Student Learning Centre. Students would bring me drafts of their essays and reports for comments and feedback about how they could improve their writing skills. One afternoon a young woman from the veterinary nursing course came to see me, with her report of a practical animal behaviour modification assignment. Her study involved working on extinguishing undesirable behaviours in the family cat, including his tendency to run away and hide when it was time for him to be put out when the family was going to bed. As a long term, and somewhat doting cat owner myself, I must admit that as I read her work, my sympathies were entirely with the cat!

That evening I discussed this with Murray (my partner at the time and also a cat lover), commenting in a light-hearted way that I didn't know why people had cats if they were going to put them out at night. "Why have cats if you don't want them to be part of the family, if you're just going to treat them like animals" I joked. He looked at me for a moment, then said "I can't believe you are saying that. Don't you remember that you used to put out the cats every night when you first got them. You didn't stop doing that till Moppet got run over ...".

At that moment I remembered. Remembered the old, run-down flat I was living in when I adopted the kittens. Remembered the back door that I used to open

every night to put them out (because that's what we had always done at home). Remembered waking up one morning and not finding Moppet at the back door, waiting for her breakfast, and searching and calling for her until I found her stiff, cold body on the front path. Remembered the vet's conclusion that she had probably been hit by a car as she crossed the road, and had died of internal injuries.

Of course, I had always remembered that Moppet had been run over. But what I had entirely forgotten was my feelings of guilt and culpability over her death. Perhaps more significantly, I had even forgotten the fact that I had *ever* been a 'put the cats out at night' type of person. Until I was reminded, by someone who was there at the time and had seen it happen, my memory of that event had been moulded in such a way as to avoid that unpleasant truth about myself – that my actions had, however indirectly, led to the death of a beloved pet.

This incident gave me a new and very personal appreciation for the recovered memory position. For the first time, I was able to understand how it might be possible for someone to lose memory for an event or action which they found to be too difficult to contemplate, and then later have this memory return. At around the same time, reading Billig's (1999) *Freudian Repression* gave me an intellectual framework that enabled me to theoretically integrate my beliefs that both memory recovery and memory confabulation could occur. In his book, Billig reconceptualised repression as a dialogic phenomenon, accomplished in and through the conversational practices of our culture which are learnt at the same time as language. Given the power of language to shape our consciousness, these linguistic practices of avoidance also create a 'dialogic unconscious' which generates the possibility of repression. Billig's argument thus goes further than simply claiming that traumatic events may not be talked about. Instead he argues that the same practices that can be used to forestall talking about an issue to others (avoidance and replacement), can be used to avoid talking (thinking) about it to one's self.

By providing a dialogic theory of repression, Billig opens up the possibility of placing both memory repression and memory confabulation within a language-

based framework. In other words, linguistic processes can both create and destroy memories. The fm position claims that false memories are created by therapeutic suggestions which alter a person's reality to the extent that they remember that events have happened to them, which in reality *have not occurred*. However, they neglect the logical corollary of this argument – that suggestion could also then be powerful enough to alter a person's reality such that they *do not* remember things that have happened to them, even though the events *have actually occurred*. In relation to child sexual abuse, which often goes undetected, and thus undiscussed, the possibility of dialogic repression must be considered. This can be further reinforced by the linguistic messages which may be utilised by the abuser (for example, "that never happened", "you're crazy", "no-one will believe you") which are specifically intended to undermine the victim's sense of reality and thus protect the abuser (Mertz & Lonsway, 1998). Furthermore, when 'spaces for speech' are finally opened up to such victims, memories which have previously been dialogically repressed may find opportunity for reconstruction.

So, for those who are interested, what I 'really think' about recovered and false memories is this: All memories are discursive constructions, produced in and through the stories we tell ourselves and others about our past experiences. This process of discursive construction both allows for the repression (and later recovery) of memories of past events, as well as the creation of memories for events which may not have actually occurred. The ability of psychologists (and, indeed, people in general) to differentiate between these two types of memory is limited by the indeterminacy of past events. Thus, a very careful approach, and an understanding of the conversationally constructed nature of all memories is important in any therapeutic or research setting.

However, this understanding, while it inevitably informs my analytic interpretations, merely forms the backdrop to my research. So I must return, back to the (symmetrical, neutral) analysis ...

## Chapter 5: Constructing and Undermining Authorial Credibility

In this first analytic chapter I consider some of the ways in which discursive and rhetorical approaches to the construction of authorial credibility converge, and use both of the concepts as a route to understanding the specific textual constructions provided in the texts of Bass and Davis, and Pendergrast (the reader should note here that I have quoted all the textual examples in this analysis exactly as they appear in their original texts, including any errors or variants in spelling, grammar and so on). I structure this analysis around the ideas of category membership and stake management, arguing that while category membership is useful in establishing credibility, claiming certain memberships may also create dilemmas of stake which need to be carefully managed by authors. Finally, I develop the concept of 'category condemnation' to describe the working up of category memberships in order to undermine the collective credibility of rhetorical opponents.

### Discursive and Rhetorical Approaches to Authorial Credibility

McMartin and Yarmey (1999) note that in the recovered memory debate, establishing the credibility of an account producer (through category entitlements and interest management) helps to construct the facticity of their account. Similarly, the persuasive power of an author's (or speaker's) character has long been recognised in classical rhetorical theory. Aristotle believed that persuasion was accomplished in three main ways - by appealing to the audience's reason (*logos*), their emotion (*pathos*) or to the character (*ethos*) of the rhetor (Corbett, 1990). Aristotle claimed that a speaker's *ethos* was either 'situated' or 'invented' (Crowley & Hawhee, 1999). A rhetor may draw upon situated *ethos* when there is a pre-existing relationship with the audience, who already know the character of the speaker/writer. In contrast, when the rhetor is unknown to their audience, they must 'invent' an *ethos* in their discourse. That is, they need to (and are free to) construct their own character for their listeners or readers.

In order to be persuasive, this character must be carefully developed. Aristotle considered that in order to be successful, a speaker had to portray three qualities: intelligence, virtue and goodwill (Crowley & Hawhee, 1999). These qualities should be demonstrated by the rhetor throughout their discourse. However, as Corbett (1990) notes, authors often make a special effort to do this in the opening stages of their discourse. This strategy is definitely evident in the texts which form the material of this analysis. All of the authors do some discursive work around presenting themselves as knowledgeable and reliable sources of information on memories of abuse, and the first few pages of each book is generally where this work is begun (often in a preface, prologue or introduction to the text). The authors all appear to be aware that establishing themselves as credible in the eyes of the reader is a necessary pre-requisite to the production of a persuasive argument.

Before beginning this analysis of these texts, I want to discuss some interesting parallels between the rhetorical concept of the invented *ethos* and the discursive understanding of category entitlement and interest management. As already noted, intelligence is considered to be an important component of a successful *ethos*, and Crowley and Hawhee (1999) suggest that possessing a thorough knowledge of the subject at hand is one way that a rhetor can demonstrate this intelligence. In discursive psychology, thorough and expert knowledge is something which can be tied to membership of social categories; this is known as a “category entitlement” (Potter, 1996a). However, as Potter points out, participants must often do some discursive work to establish their category membership, and thus persuade others that they are knowledgeable – that is, they must “work up” their category entitlement. However, if membership is successfully established, it can provide a kind of discursive short-cut to knowledge and authority, and thus to the development of an intelligent *ethos*.

Similarly, there is a relationship between the virtue and goodwill components of *ethos*, and the discursive understanding of stake management. From a discursive perspective, one of the ways in which a speaker’s credibility can be undermined is by pointing out the stake or interest of the speaker – that is, the idea that an individual’s account is coloured by their personality (lack of virtue)



and/or interests (lack of goodwill), rather than being an objective report. Therefore, in order to maintain credibility, a speaker must find ways of managing or defending against accusations of interestedness, much as Aristotle's rhetors were required to establish their virtue and goodwill in order to successfully persuade their listeners. The authors of my texts utilise a number of rhetorical strategies in order to achieve these goals.

Firstly, in order to develop a credible *ethos*, authors of pop psychology texts work up a variety of category entitlements to establish their knowledge and authority in relation to memory and abuse. These categories include 'victim/survivor', 'clinician', 'researcher/scientist', 'expert witness' and even 'investigative journalist'. I suggest that while each of these categories confer certain knowledge entitlements, they may also create specific dilemmas of stake which are dealt with by processes of interest management and/or by claiming multiple category memberships (for example Herman, Terr, Yapko and Wakefield & Underwager all identify themselves as both 'clinicians' and 'researchers'). The complex relationship between category membership, entitlement and dilemmas of stake is illustrated in this chapter by examples drawn from *The Courage to Heal*, *Victims of Memory* and *Making Monsters*.

### Working Up Category Memberships

In the preface to *The Courage to Heal* (1988), co-authors Bass and Davis each write a section developing their particular category memberships through the recounting of a personal narrative. Each has specific difficulties relating to their membership which they must negotiate. Bass faces the task of establishing her credibility even though she cannot claim *a priori* membership of any of the relevant categories mentioned in the previous paragraph. She is not a survivor (unlike her co-author), but neither does she have the professional training of a clinician or the academic credentials of a researcher. And yet she must convince the reader that she has the credibility to author a self help book for incest survivors. She does this through a careful, sequential recounting of her

work with abuse victims, and by drawing links between this work and her established category entitlement as a teacher of creative writing:

I first heard that children were abused in 1974, when a young woman in my creative writing workshop pulled a crumpled half-sheet of paper out of her jeans pocket. Her writing was so vague, so tentative that I wasn't sure what she was trying to say, but I sensed that it was important. Gently, I encouraged her to write more. Slowly she revealed her story. In pieces, on bits of paper, she shared the pain of her father's assaults, and I listened. (Bass & Davis, 1988, p. 13)

In the opening sentence, Bass begins by admitting that up until 1974 she was unaware that children were abused. At first reading this confession may seem a little odd. How can admitting ignorance work to develop a category membership? However, it is important to realise that this initial admission (contained in the first clause of the first sentence) acts mainly as a contrast to the following one and a half pages of Bass's account. Her initial ignorance acts as a foil for the rest of her story, which recounts systematically the growth of her knowledge and experience in the 14 years between her first exposure to the reality of abuse, and the publication of *The Courage to Heal*.

The provision of a date in the early 1970s also serves to contextualise her lack of knowledge. As she points out later:

There were no groups for survivors of sexual abuse then. The word 'survivor' was not yet in our vocabulary. (Bass & Davis, 1988, p. 13)

Bass's ignorance is normalised in relation to society as a whole. Not only were there no groups for survivors, there was not even a word with which to identify them. Bass cannot be faulted for not initially knowing about child sexual abuse, because no one else knew about it either. This historic lack of knowledge and acknowledgement of abuse issues is a common theme utilised by pro-rm writers in working up their category memberships.

Returning to Bass's opening paragraph, there are some further features which are worthy of comment. In this short narrative the reader is subtly introduced to Bass as both a member of a specific category of experts (teachers) and as an individual who possesses special personal qualities which allow her to extend her expertise into a new area – helping victims of abuse. The young woman at the centre of the story is a student in a workshop that Bass is teaching. In her

role as a teacher, Bass is able to help this young woman, whose initial attempts to tell her story are so unsuccessful (vague, tentative, difficult to understand). Bass intuitively understands the importance of the story and by listening to and gently encouraging her student, she helps her to “share the pain”. Thus we see how Bass’s role as a teacher modulates into that of a healer. This connection is further developed in the following passage:

By 1983 ... I had learned a great deal about the healing process. One of the things that I had learned was that writing itself was healing. (Bass & Davis, 1988, p. 14)

The implication is clear: if writing is healing, and Bass is an expert in writing, then by extension she can also become an expert in healing.

However, Bass acknowledges the fragility of this category membership in an interesting piece of defensive rhetoric which occurs later in the preface:

I am not academically educated as a psychologist. I have acquired counseling skills primarily through practice. Since 1970, when I began working as a counselor and group facilitator, I’ve had the opportunity to train with a number of excellent therapists. But none of what is presented here is based on psychological theories. The process described, the suggestions, the exercises, the analysis, the conclusions, all come from the experiences of survivors. (Bass & Davis, 1988, p. 14)

Bass is clearly aware that, in spite of the work she has done to establish her expertise, she still lacks academic credentials, and that this is a possible criticism which may undermine her credibility as an author. She chooses to deal with this by confessing her deficiency (similar to the ‘stake confession’ identified by Potter, 1996a), but making this confession in such a way that it can be seen to be inconsequential. It is not important that she is not a trained psychologist, because “*none* of what is presented here is based on psychological theories” This extreme case formulation is reinforced in the next sentence where Bass enumerates the elements of the book (process, suggestions, exercises, analysis and conclusions) and identifies them *all* as coming from the experiences of survivors. Bass is working here from within a feminist understanding, in which women’s experience provides a powerful warrant, particularly when it is combined with the feminist critique of psychology (Humm, 1995). The survivor is the ultimate expert on her own experience, and psychological ‘experts’ are thus viewed with suspicion. This position is restated more explicitly in the preface to the third edition of *The Courage to Heal* (1992):

As authors, we have been criticized for our lack of academic credentials. But you do not have to have a Ph.D. to listen carefully and compassionately to another human being. In fact, our perspective as laypeople helped us to take the suffering of survivors out of the realm of pathology – and instead to present them as strong, capable people who'd been hurt. (Bass & Davis, 1994, p.14).

Bass's original defensive rhetoric has obviously not been entirely successful in deflecting criticism of her credibility, so she reiterates her argument in an even more positive formulation. Not only is a lack of qualifications not a hindrance, it is an advantage, because of the tendency of the academic establishment to pathologise the experience of survivors.

### Managing Issues of Stake and Interest

Although Bass appears to view membership of the 'survivor' as an important route to establishing authorial credibility, Davis's (Bass & Davis, 1988) preface illustrates that such a membership does not necessarily provide an unproblematic entitlement. Membership of the survivor category must be authenticated, as claims of abuse are always potentially contestable. Furthermore, there are specific issues of stake and interest which are inherent to membership, and must be managed. In Davis's account, issues of authenticity are addressed by stressing how terrible the experience of being a survivor has been. Her preface begins with a short narrative in which Davis remembers a phone call she made to Bass shortly after she "first remembered the incest". Her thoughts and feelings are described in explicit and jarring detail:

I cannot stand another moment of this pain. My heart hurts and I can't take any more ... I can't sleep, and when I do, it's all I dream about. I can't think about anything else. Every child I see on the street reminds me of incest. I can't make love, I can't eat, my whole body feels like a giant piece of rubber. I'm crying all the time. My whole life is flashbacks, going to therapy, and talking about incest. Half the time I don't even believe it happened, and the other half I'm sure it was my fault. (Bass & Davis, 1994, p.15)

In Davis's narrative, she reports Bass as responding:

It did happen, Laura. Look at what you're going through. Would anyone willingly choose to go through this torture? Why would you ever want to invent something this bad? (Bass & Davis, 1994, p.15)

Doubts about the authenticity of her experience (and the category membership deriving from it) are expressed here by Davis herself, and later in the account she reports those of her mother, who accuses Davis of “making it up” and “jumping on the incest bandwagon” (p. 17). In both instances, these doubts are countered by the reality and awfulness of Davis’s feelings (which may explain why they are recounted so explicitly). And, as she has Bass explain, since it would be illogical for a person to inflict such pain upon themselves unnecessarily, these feelings must therefore derive from a real experience (a claim which is strenuously contested by fm proponents).

Having (hopefully) established her authenticity as a survivor, Davis must then go on to deal with the issues of stake and interest which go hand in hand with this category membership. I believe that the issue is this: as a survivor she may be assumed to have some personal motivation in relation to her authorship of the book, which may undermine her credibility as an author. Davis deals with this possibility by combining stake confession and inoculation. She admits that her motivations for writing the book were initially selfish:

I wanted to write this book for probably the same reason you are picking it up now – I felt a tremendous amount of pain in my life, and I wanted it to stop ... I needed to understand what was happening to me. I needed to talk to other women who had been through it. Out of that need, my desire to write this book was born. (Bass & Davis, 1994, pp. 15-16)

While Davis ‘confesses’ that she was initially interested in writing the book as a way to deal with her own issues, the reader is invited to see this goal as an entirely natural one; one, in fact, that they might have themselves (she started writing the book for the same reason “you” are reading it). But once she has revealed this initial motivation, the reader is then reassured:

As I moved along in my own healing, my relationship to the book changed. The acuteness of my own needs began to fade. It became increasingly important to me to communicate what I was learning. (Bass & Davis, 1994, p. 16).

Davis’s motivation is transformed into the more benevolent (and credible) goal of communicating beneficial healing information to her readers. In Aristotle’s terms, she is demonstrating goodwill towards her audience. They can be assured that she now has their best interests, rather than her own, at heart.

Membership of the 'survivor' category proves to be even more problematic for Pendergrast (1995, 1996), who is a survivor not of incest, but of false accusations of incest. In the first edition of his book, *Victims of Memory* (1995), Pendergrast works up this category membership in some detail, devoting an entire chapter (*Chapter Two: Daughters Lost*) to his own experiences as an accused father. However, highlighting this particular category membership proved to be problematic for Pendergrast, as he explains in the second edition of his book:

One regret that I have is that the media attention to my book has centred inordinately on my personal situation. Even favorable reviews often perceived the book as an effort to exonerate myself, to defend myself against unfair allegations (Pendergrast, 1996, p. 21).

Pendergrast is clearly unhappy that the issues of stake inherent in his category membership (that his motivations for writing are personal ones) are so salient for readers. His response to this is interesting. In the second edition of the book, he removes much of the personal material which focused attention on his 'survivor' identity (including "Daughters Lost", as well as a section in the first chapter where he talks about publishers' responses to his status as an accused parent writing about false memories, and an epilogue in the form of an open letter to his daughters), explaining:

... I have eliminated the personal chapter and the letter from this edition of the book. That shouldn't detract from the value of the book. My contribution to the debate on recovered memory is my investigative journalism and scholarship, not my personal story, which, though tragic, is unfortunately not unusual. (Pendergrast, 1996, p. 23)

This passage also illustrates the second component of Pendergrast's response, which is to emphasise his membership of an alternative category, that of 'investigative journalist'. In the first edition of the book, there are only two (oblique) references to his status as a journalist (1995, pp. 15, 16). In the second edition, there are six references (1996, pp. 15 [x2], 19, 22 [x2], 23).

By highlighting his membership of this category, Pendergrast attempts to counter some of the stake attributions which have concerned him. For example, in describing what motivated him to write his book, he confesses his personal involvement in the controversy, but identifies his prime motivation as originating from his professional identity:

I finally realised that what has been termed "false memory syndrome" (FMS) was destroying not only my children's very identities and my relationship with them, but millions of other families as well ... As an investigative journalist, it seemed clear to me that this was an issue that needed investigation, and I needed to pursue it to the best of my ability before re-focusing on other issues. (1996, p. 14-15).

In this paragraph, Pendergrast implies that while his personal situation was distressing, it was not until he realised that it was part of a wider social issue that deserved the attention of an investigative journalist that he was motivated to write about false memory syndrome. If his reasons for writing the book are not personal, but rooted in a more objective, journalistic concern, he becomes less vulnerable to charges of interestedness.

In Pendergrast's account, being an investigative journalist 'works' to counter the interestedness of being a survivor because of the professional standards and ethics which are invoked. For example, he argues that

I was a professional journalist long before my children cut off contact, and using a book to try to exonerate myself would be quite unprofessional. (1996, p 22).

In other words, his pre-existing status as a professional prevented him from engaging in the unprofessional activity of self-absolution which might be attributed to other falsely accused parents. Pendergrast also suggests that his identity as an investigative journalist allowed him to overcome the subjectivity and bias that would normally be attributed to an accused parent. In an ingenious paragraph combining both stake confession and inoculation he explains:

My primary motive in telling my own family's story was intellectual honesty. The book is a work of investigative journalism, which needs to be judged on its own merits. I interviewed people in all situations, with a wide range of informed opinions, and tried to represent them all fairly and honestly. I did not begin my research as a dispassionate observer, however, but began looking into this issues in order to understand what had happened to my own family. Readers had a right to know that. (Pendergrast, 1996, p. 22).

Potter (1996a) suggests that the effectiveness of stake confession may lie in the way it allows the individual to present themselves as honest, objective and self-aware, and Pendergrast certainly appears to be making the most of all of these possibilities in this passage. Honesty is what motivates the recounting of his personal history, a desire to tell the reader what they "have a right to know", rather than attempting to deceive them regarding his personal situation. He

claims that this honesty also became a feature of his approach to the controversy, which is balanced and fair (“I interviewed people in all situations ...”). By noting that he “did not begin his research as a dispassionate observer, however, ...” he is able to confess his initial bias, while simultaneously claiming the later development of an objective viewpoint. Although his personal situation may have initially motivated his interest in the controversy, making it a “work of investigative journalism” allowed him to become the “dispassionate observer” which he was at first unable to be.

### Category Condemnations

The work that Bass, Davies, Pendergrast and the other authors do to establish their own credibility can be categorised as defensive rhetoric. It is oriented towards deflecting possible, as well as actual, criticisms (conforming to Billig’s, 1987, suggestion that all arguments are oriented to the possibility of counter-arguments). Mirroring this defensive activity is the offensive rhetoric which is employed to undermine the credibility of opponents. Offensive rhetoric of this kind has been a prominent feature of the memory debate (see Chapter One). It can be directed towards specific individuals, or towards entire categories of individuals. In the ‘pop psychology’ texts which I have analysed, attacks on the credibility of specific individuals, while present, are not as prominent as they are in other discursive contexts (see, for example, the articles and letters in Crews, 1995). Instead, the criticisms are mainly levelled at groupings such as ‘recovered memory therapists’ or ‘False Memory Syndrome Foundation supporters’.

Ofshe and Watters (1994) demonstrate this strategy in the introductory chapter of *Making Monsters*, where they explain to the reader:

We will show that recovered memory therapists are not, as they portray themselves, brave healers but professionals who have built a pseudoscience out of an unfounded consensus about how the mind reacts to sexual trauma. In the process they have slipped the ties that bind their profession to scientific method and sound research. Free from any burden of proof, these therapists have created an Alice-in-Wonderland world in which opinion, metaphor, and



ideological preference substitute for objective evidence ... We believe that there is now sufficient evidence – within the therapists' own accounts of their techniques – to show that a significant cadre of poorly trained, overzealous, or ideologically driven psychotherapists have pursued a series of pseudoscientific notions that have ultimately damaged the patients who have come to them for help. (Ofshe & Watters, 1994, p. 5-6)

The criticisms in this extract are directed at “recovered memory therapists” as a group, rather than targeting specific individuals. The effectiveness of this strategy relies on constructing the group as homogeneous and unified. Ofshe and Watters achieve this by claiming, firstly, that all recovered memory therapists portray themselves in the same fashion (as “brave healers”), which suggests a high level of similarity among group members (this is also interesting as ‘rhetoric about rhetoric’, as many rm authors do portray themselves as pioneers of healing). Secondly, they describe rm therapists as a “cadre”, a word which implies a closely knit organisation with a unified belief and purpose.

Establishing the unity and cohesiveness of the group is a useful strategy, as it means that all available criticisms are potentially applicable to all members of the group. The utility of this strategy is evident if the alternative is considered. Challenging recovered memory supporters individually would not only be extremely cumbersome, but it would also be less effective, because only a specific subset of criticisms would apply to each individual (for example, it would be difficult to argue that Dr Judith Herman, a psychiatrist employed by Harvard University was “poorly trained”). However, such a restraint does not apply when referring to a group, which allows all possible criticisms to be listed together. This produces a much stronger challenge, as the impression created is that each criticism potentially applies to each group member. I refer to this rhetorical strategy as a ‘category condemnation’. In a sense it is the antithesis of a category entitlement, as category membership is worked up (by opponents) for the purposes of undermining credibility rather than augmenting it.

The criticisms themselves are also interesting, as they attack the credibility of recovered memory therapists on several counts. The intellectual credibility (intelligence) of the therapists is challenged by claims that they are “poorly trained” and no longer operating within the boundaries of accepted knowledge

production practices (they have “slipped the ties that bind their profession to scientific method and sound research”). Ofshe and Watters (1994) also call into question the virtue and goodwill of therapists who are described as being driven by ideology rather than concern for their patients, who they are damaging, rather than helping. This particular criticism is one which the authors go on to elaborate and extend in the following extract:

Perhaps most disturbingly, recovered memory therapists often train their patients to “reexperience” the emotional pain of rape, sexual abuse and other horrors that they “discover” in therapy. Believing it therapeutic, therapists encourage abreactions ... If their treatments are invalid and unnecessary, as we have concluded, then these reports of the pain induced by therapy are in reality a documentation of the brutalization and psychological torture of the people who have come to them for help. Although we don’t suggest that these recovered memory therapists take sexual pleasure from these abuse “re-creations”, some recovered memory therapists perhaps deserve recognition as a new class of sexual predator. If, for no defensible reason, some therapists are causing the same emotional and psychological trauma as an actual rape or sexual assault, then they, like those who physically victimize people, deserve moral condemnation. (Ofshe & Watters, 1994, p. 7)

Perhaps the most notable feature of this extract is how the authors use a disclaimer (“Although we don’t suggest ...”) to suggest the very thing which they claim they are not suggesting (see Pinch & Pinch, 1988)! While “these recovered memory therapists” (presumably those who believe abreactions are therapeutic) are ostensibly absolved by the disclaimer, the vague designation of “some recovered memory therapists” as “sexual predators” works in a similar manner to the category condemnation described above. By not specifying which particular therapists “deserve moral condemnation”, this criticism applies, at least potentially, to all group members.

Bass and Davis also employ a version of the category condemnation strategy, in the response to the fm argument which they added to the third edition of *The Courage to Heal*. They write:

Among professionals, a number of those who endorse the “false memory” theory have little or no clinical experience with survivors. Some have specialized in unrelated areas and are uninformed about the impact of trauma or the patterns of perpetrators. Others have been moved by the obvious pain of accused parents who, whether or not they abused their children, are clearly suffering. Still others, concerned with issues of suggestibility, have erroneously gone on to challenge the credibility of all survivors who remember their abuse as

adults. And finally, those who serve as expert witnesses for the defense of accused perpetrators have a clear professional and financial stake in discrediting survivors. (Bass & Davis, 1994, p. 484)

As in the earlier example, the authors list criticisms which apply to an entire group of individuals, this time “professionals who endorse the false memory theory”. Although they suggest that different sub-groups (“a number”, “some”) have different motivations, the very vagueness of these designations mean that the criticisms work in a similar way to other category condemnations. By not specifying the membership of these subgroups, the criticisms could potentially apply to any group member. There is also a similarity in the kind of criticisms which are made. Bass and Davis make attacks on the intelligence of their opponents, claiming they lack the clinical experience and/or relevant knowledge to make a valid contribution to the debate. They also undermine the virtue of those who act as expert witnesses (such as Loftus, Ofshe, Wakefield and Underwager) by claiming that they are motivated by personal interests such as money and professional prestige – which, as McMartin and Yarmey (1999) note, is in turn a criticism that Pendergrast (1995) levels against recovered memory therapists.

The persuasive power of the ‘category condemnation’ strategy is such that even when an individual’s credibility is challenged, it is often done in such a way that the individual is constructed as an exemplar of their category, thus undermining the group as a whole. For example, when Bass and Davis specifically criticise Ralph Underwager in their book, they also emphasise his relationship to the FMSF (see also pp. 493-494):

The FMS Foundation has an advisory board of well-credentialed professionals from prominent universities. However, a founding member of this board (who has since resigned) has engaged in questionable practices designed to intimidate therapists, and squelch academic debate<sup>32</sup>. (Bass & Davis, 1994, p. 488).

Footnote 32 identifies Ralph Underwager as this board member, and describes these questionable practices in more detail. This description appears to be constructed to cast doubt on the credibility of the entire FMSF advisory board by virtue of their association with Underwager. Furthermore, readers are cautioned that the professional credentials and status of board members are not necessarily guarantees of credibility, since at least one of the members has

engaged in “questionable practices”. Similarly, Ofshe and Watters link their criticism of Herman to rm therapists as a group:

That recovered memory therapists like Herman should tout Freud's early work as “brilliant” and “compassionate” demonstrates how willingly these experts ignore the coercive nature of any procedure so long as the resulting memories confirm their beliefs. ... To tout Freud's early works as “closely reasoned” requires the same sort of blindness as Herman and others employ today to ignore current gross mistakes in therapy. (Ofshe & Watters, 1994, p. 292).

Herman's views on Freud, and her “blindness” in ignoring both his mistakes and the “coercive nature” of therapy, are constructed as being typical of recovered memory experts as a whole, thus undermining their collective credibility.

The attention which is paid to both constructing and attacking authorial credibility highlights the importance of *ethos* as a route to persuasion. By working up category entitlements, and carefully managing issues of stake and interest, the authors present themselves as caring, credible experts whose views on the memory controversy are worthy of consideration. Conversely, by employing the strategy of category condemnation, and focusing on the personal interests and motivations of their opponents, their credibility can be damaged. Personal credibility – a successfully constructed *ethos* – underpins the persuasive power of an author's argument. Thus, establishing credibility is an essential component of successful rhetoric in the memory debate. If an author is considered to be credible, then the other arguments and evidence which they present are more likely to be persuasive. The next of these argumentative strategies to be considered is the construction (and deconstruction) of definitions and descriptions.

## Chapter 6: The Strategic Deployment of Descriptions and Definitions

What I have claimed in the preceding chapter is that authorial credibility is highly contestable (and contested) in the memory debate. It could be argued that this is unsurprising, given the essentially subjective nature of any assessment of personal or professional integrity. However, it is worth noting that the existence of the psychological phenomena at the centre of the debate is equally disputed. Recent language-based critiques of psychology have stressed the socially constructed nature of psychological phenomena, including memory processes. For example, Shotter (1990) has argued that the concepts employed by cognitive psychologists when studying memory (such as 'memory traces', 'sensory registers' and 'retrieval processes') are essentially discursive constructs. As he points out, these concepts do not correspond to actual things (material objects or processes) which make memory possible. Rather, they are simply ways of describing experiences "... in such a way as to constitute and sustain one or another kind of social order" (p. 123). Similarly the phenomena at the centre of the memory debate – repressed memories, dissociation and false memory syndrome – are created in and through language. Their nature is constructed by authors through a variety of descriptive and definitional processes which are employed rhetorically. A number of these processes are examined in this chapter.

In the memory debate, both rm and fm supporters have an interest in establishing that their phenomena of interest are 'real'. One of the ways they do this is by defining and/or describing the phenomena in particular ways. As Potter (1996a) explains, such descriptions have an "epistemological orientation", constructing the "out-there-ness" of phenomena in order to establish their existence as independent of the producer of the description. Often these descriptions involve constructing the nature or essence or character of the phenomena in particular ways. However, descriptions are potentially infinite (which can be a disadvantage), so at times definitions are invoked. Definitions are a specific type of description – an exclusionary description. A definition proscribes category membership, claiming "the phenomena must be like this" in order to belong to this category or be given this name.

While definitions can be used to facilitate productive discussion by ensuring shared understanding (Corbett, 1990), they also have potent rhetorical and persuasive potential (Crowley & Hawhee, 1999). In an argumentative exchange, definitions and descriptions can be deployed strategically – not to make sure that every one is arguing about the same thing, but to undermine the position of an opponent. Billig (1996) notes that definitions, when employed in the discourse of social scientists, always have a rhetorical purpose: “The function of such a definition is to locate the essence of the matter in a direction which accords with the theoretical leanings of the writer ...” (p. 178). In other words, processes of definition and description of a phenomenon are not preludes to argument. Rather, they are intrinsically argumentative.

MacMartin and Yarmey's (1998, 1999) analysis of the construction of 'repression' and 'dissociation' show that the rhetorical deployment of descriptions is a strategy which is extensively utilised by protagonists in the memory debate, as a brief summary of their findings indicates. They show how rm proponents describe repression as a process which is different from 'normal' forgetting (with special properties which allow repressed memories to retain clarity and accuracy). However, proponents of repression also describe it as a relatively common and normal response to trauma, which is experienced by many people. In reply, opponents of repression employ the concept of 'robust repression', the formulation of which is so extreme as to appear absurd. Furthermore, they claim that the lack of definitional consistency on the part of proponents renders the concept invalid. MacMartin and Yarmey suggest that rm proponents support their definitions of repression with first person accounts and historical evidence, while rm opponents argue that repression is unsupported by scientific evidence. In response, rm proponents counter this argument by invoking concepts of psychogenic amnesia and dissociation (whose status as 'real' psychological phenomena is well established) to explain memory loss, claiming that these phenomena are well supported by current cognitive science. Opponents of rm cannot (and do not) deny the existence of dissociation, but instead claim that the concept is misapplied in this case, and that it has been cynically and rhetorically employed as a way of trying to maintain the viability of the recovered memory concept.

In the following analysis I build upon and extend aspects of MacMartin and Yarmey's observations, and undertake a complementary analysis of the descriptions and definitions of false memories. Other issues raised by MacMartin and Yarmey, such as the use of history, science and first person accounts to support (or dispute) particular positions, are dealt with in more detail in the following chapters.

### Reifying Recovered Memory

In order to develop its status as a 'real' psychological phenomenon, proponents of recovered memory describe and define the processes involved in memory repression and recovery in a variety of ways. In general, the authors in my sample construct repression as a natural and rational response to the pain of traumatic childhood events. In addition, most of these authors also develop systematised descriptions of the effects of childhood sexual abuse, drawing upon the rhetorical power of the 'syndrome' and the 'diagnosis' to reify repression as a psychological entity.

#### *Repression is logical*

A number of different psychological processes are considered by rm supporters to be potentially involved in the forgetting of childhood trauma, including amnesia, repression and dissociation/splitting. What all of these processes have in common is that they are presented by rm proponents as being a logical, rational, and thus normal, response to a traumatic event which might otherwise threaten to overwhelm the mind of a child. MacMartin and Yarmey suggest that "the reification of concepts like repression is accomplished through the entrance of Freudian terminology into everyday discourse and its link to taken-for-granted, ubiquitous experience ..." (1999, p. 347). While the normalisation of repression is no doubt assisted by the relative cultural currency and familiarity of psychoanalytic discourse, I believe that this is not the only, or even the most important, normalising strategy utilised in the debate. Rather, I propose that the main technique used to normalise and reify the forgetting of traumatic childhood events is the presentation of such forgetting as a logical response to

overwhelming terror, based on a central assumption that the mind is able to act to protect itself, and drawing upon the discourse of human rationality. For example, consider the following extracts from Renee Fredrickson's *Repressed Memories* (1992):

Your mind has played a trick on you, but it is a trick to help you rather than hurt you. When you were abused you were too young and too fragile to retain the memory, or you may have undergone torment too appalling to handle it in any other way. You needed your strength for play, for learning, for seeking and holding on to whatever love you could find in your world. So your wonderful, powerful mind hid some or all of the abuse from you until you were strong enough to face it. (p. 24)

Powerful forces coalesce during the abuse to pressure children to forget. Even if they remember some of what happens, they will certainly forget other parts of the experience. Survival mechanisms kick in to protect them as their senses are bombarded with pain, shock and sexual stimuli they are unprepared to experience. (p. 57)

Abuse that is either too painful or too bizarre for the child to cope with necessitates the protection of amnesia. (p. 58)

Dissociation gets you through a brutal experience, letting your basic survival skills operate unimpeded. (p. 59)

Dissociation, a form of hypnotic trance, helps children survive abuse, and it is key to memory repression. The abuse takes on a dream-like, surreal quality, and deadened feelings and altered perceptions add to the strangeness. The whole scene does not fit into the "real world." It is simple to forget, easy to believe nothing happened. (p. 60)

In these extracts, the logic of repression is constructed, step by step. First, Fredrickson constructs the child's psyche as innocent and delicate ("too young and too fragile", "you needed your strength for play and for learning"), contrasting it with the torment and brutality of abuse. This contrast highlights the total inadequacy of the child's normal mental resources for coping with such trauma, along with their inability to protect themselves in any physical way. Luckily, however, there exist "powerful forces" and "survival mechanisms" which "kick in" to protect the child by helping her to forget – the only possible means the mind has to protect itself from the terror and pain of the abuse, and to allow the child to continue functioning normally in daily life. There is a sense that dissociation and amnesia are processes which are automatically triggered when



the trauma becomes overwhelming, in much the same way as a circuit breaker is triggered when the load on an electrical circuit becomes too great.

Thus, the repression of abuse memories, rather than being something odd and implausible, is constructed by rm authors as an eminently sensible and rational response to an extreme situation:

The cognitive techniques described in this and the preceding chapter [amnesia, dissociation etc.], while they may seem like symptoms of a mental disturbance, are specifically designed to control the flow of information about the abuse into the consciousness of the incest survivor and thereby protect her. (Blume, 1990, p. 105)

Children who go through a number of terrors protect themselves in this way. They are able to muster massive defenses against remembering, because this is the only way they can get through a frightening childhood. (Terr, 1994, p. 12)

All of the abused child's psychological adaptations serve the fundamental purpose of preserving her primary attachment to her parents in the face of daily evidence of their malice, helplessness, or indifference. To accomplish this purpose, the child resorts to a wide array of psychological defenses. By virtue of these defenses, the abuse is either walled off from conscious awareness and memory, so that it did not really happen, or minimized, rationalized, and excused, so that whatever did happen was not really abuse. Unable to escape or alter the unbearable reality in fact, the child alters it in her mind. (Herman, 1992, p. 102)

Blume and Terr both emphasise the protective function of forgetting. Herman also sees repression as protective and adaptive, but she develops the argument in a slightly different way. She suggests that forgetting the abuse is necessary because it enables the child to maintain a relationship with her parents; a relationship which is essential because of the child's dependent status. This extra layer of explanation serves to provide an even more compelling rationale for repression. Not only does it enable children to retain their sanity, but also the primary relationship on which their physical safety and sustenance depends. Interestingly, this is also the main thesis of Freyd's Betrayal Trauma Theory. The 1996 book in which she explicated this theory is subtitled "The logic of forgetting childhood abuse". By constructing the processes of memory repression as logical, authors attempt to establish its ontological status. But

they also gain the benefit of linking repression with logical, rational (masculine) thought, rather than emotional, irrational (feminine) responses.

### *Repression is common*

Establishing forgetting as a rational response to abuse serves to normalise the concept of repressed memories. This provides the argumentative scaffolding which supports the claims of rm advocates that repression is relatively common. If repression is a logical and plausible reaction to abuse, then it can be expected to be widespread among abuse survivors. The claims made in the following extracts gain credibility from such expectations:

I have found that most incest survivors have limited recall about the abuse. Indeed, so few incest survivors in my experience have identified themselves as abused in the beginning of therapy that I have concluded that *perhaps half of all incest survivors do not remember that the abuse occurred.* (Blume, 1990, p. 81)

Finally I realized the size of the problem. Millions of people have blocked out frightening episodes of abuse, years of their life, or their entire childhood. (Fredrickson, 1992, p. 15)

Such claims establish repressed memories as common and widespread without relying upon specific statistics. According to the authors “many”, “most” or “millions” of survivors have forgotten being abused. These claims of widespread occurrence rely on the ‘rationality’ of repression at the same time as they reinforce it. Claims that repression is ‘logical’ and that it is ‘common’ exist in a reciprocal relationship. One reinforces the other, and both contribute to the reification of repressed memories.

### *Repression is systematised*

Descriptions which characterise processes of traumatic forgetting as rational, common and, thus, relatively normal responses to childhood trauma work to establish the ‘out-thereeness’ of repression. However, this is not the only reifying strategy which pro-rm authors use. Another useful tactic which is also utilised is the systematisation of the repressed memory phenomenon into a syndrome or disorder. In medical discourse, a syndrome refers to a set of symptoms which, when they occur together, suggest the diagnosis of a particular underlying disease or condition. Systematised diagnoses of this kind have been a part of the practice of clinical psychology since the publication of the third edition of the

Diagnostic and Statistical Manual of Mental Disorders (DSM) in 1980, and have become increasingly influential. Indeed, Halling and Goldfarb (1996) suggest that without the imprimatur of the DSM the very existence of a psychological disorder is likely to be in doubt. On the other hand, the inclusion of a disorder or syndrome in the DSM is an assurance that it is a genuine psychological ailment. The pre-eminence of the DSM as a diagnostic tool in clinical psychology means that it provides the model for the creation/identification of new disorders. Psychologists expect that 'real' psychological disorders can be labelled, defined and identified via a list of symptoms (because this is how they are presented in the DSM).

All of the pro-rm authors appear to be attuned to this expectation, in that they make some effort to systematise individual responses to childhood trauma in their books. Terr, for example, interweaves her own theory of childhood trauma into the stories in her book. This theory was first proposed by Terr in 1987 (and subsequently published in 1991), and states that a single traumatic incident in childhood (a Type I trauma) will have a different psychological impact than repeated traumas (Type II). Type I trauma results in an indelible and often intrusive memory of the event, whereas Type II traumas cause repression, as Terr explains in this excerpt from her discussion of the Eileen Franklin case:

If the perpetrator commits the crimes without any disguises, the repeatedly traumatized child, when memory returns, almost always knows who did it. As a Type II child who had witnessed and experienced ongoing violence in her family, Eileen Franklin would have had to accomplish an enormous mind-bending trick in order to confuse her own father with someone else who had really killed Susan Nason. If they are repeatedly traumatised, children can be expected to "forget" much of what happened, however. Holes in memory are created by defensive operations, such as the very common defense of repression. When repression lifts, the memories may come back relatively intact. (Terr, 1994, p. 30)

While Terr (1991) presents her theory as simply an attempt to organise and schematise thinking about childhood trauma (and not a revision of the DSM), three of the other rm authors go one step further and actually propose new syndromes or disorders. Blume and Fredrickson both propose syndromes which specifically describe the effects of child sexual abuse, while Herman's formulation covers harm resulting from any prolonged trauma (such as child

sexual abuse, domestic violence, torture or incarceration). These authors all frame their syndromes as improvements upon the existing (and DSM endorsed) diagnosis of Post Traumatic Stress Disorder:

Yet while the use of post-traumatic stress disorder represents an enormous improvement from previous diagnoses, this diagnosis fails to acknowledge the admirable survival spirit and inevitability of the emotional and behavioral consequences it describes. It also is not entirely accurate in describing the post-incest experience. ... The label of Post-Incest Syndrome combines trauma theory and bereavement theory with the specifics of sexual abuse. (Blume, 1990, pp. 78-79)

As it is currently defined, however, PTSD does not adequately describe the effects of memory repression in cases of chronic child abuse. A new category must be developed to describe survivors with repressed memories of abuse. The repressed memory syndrome was developed to describe those who have no memory of the abuse, as well as those who remember but have a significant amount of amnesia. (Fredrickson, 1992, p. 40)

The current formulation of post-traumatic stress disorder fails to capture either the protean symptomatic manifestations of prolonged, repeated trauma or the profound deformations of personality that occur in captivity. The syndrome that follows upon prolonged, repeated trauma needs its own name. I propose to call it "complex post-traumatic stress disorder." (Herman, 1992, p. 119)

The effect of this framing is to suggest to the reader that these new syndromes are the same 'type of thing' as PTSD (which is a 'genuine' psychological disorder certified by its inclusion in the DSM). In a sense it is a kind of 'authenticity by association'. More specifically, the attempt to extend the legitimate status of PTSD to the new syndromes is subtly augmented by the suggestion that the new syndromes actually *improve upon* the current diagnosis by remedying its deficiencies. The reader could assume that if PTSD (with all its flaws) is a 'real' psychological entity, recognised in the DSM, then the new, 'improved' diagnoses must be even more deserving of this endorsement. Indeed, Herman (1992) tells the reader that her new diagnosis (complex post-traumatic stress disorder) may well be included in the DSM IV:

Currently, the complex post-traumatic stress disorder is under consideration for inclusion in the fourth edition of the diagnostic manual of the American Psychiatric Association, based on seven diagnostic criteria ... Empirical field trials are underway to determine whether such a syndrome can be diagnosed reliably in chronically traumatized people. The degree of

scientific and intellectual rigor in this process is considerably higher than that which occurred in the pitiable debates over "masochistic personality disorder". (Herman, 1992, p. 120).

As Herman notes, part of the systematic description of disorders which is a feature of the DSM is the provision of a list of symptoms (diagnostic criteria). Grouping symptoms together in a systematic way suggests an underlying pattern of psychopathology which is the cause of these symptoms. Therefore, it is not surprising to find that symptom lists are a prominent feature of the pro-rm literature (see Herman, 1992, p. 121; Fredrickson, 1992, pp. 48-51). Even authors such as Bass and Davis (1988) and Blume (1990), whose theoretical grounding in a feminist model of experiential knowledge make them critical of the 'psychiatric establishment', include comprehensive symptom lists in their books. Bass and Davis phrase their list as a series of questions. Their advice is for women to use these questions to assess how the abuse has affected them in areas such as self esteem, feelings, intimacy, sexuality, parenting and families (pp. 34-39).

For Blume (1990), the symptom list is even more important. Her "Incest Survivors' Aftereffects Checklist", which contains 34 items, is the central feature of *Secret Survivors*. However, Blume specifically denies that the formulation of the checklist, which she calls her "tool for tracing Post-Incest syndrome" is intended to be used for establishing a "new psychiatric diagnosis". In fact, Blume claims that Post-Incest Syndrome is not a disorder at all, but a description of the negative side effects of coping strategies used by individuals to deal with the trauma of incest (p. 75). According to Blume, the validity and credibility of the checklist and syndrome she has developed come from survivors:

My most sincere appreciation, of course, goes to those incest survivors, female and male, clients and workshop participants and strangers, who shared with me their stories, their anguish and their victories. From their truths was borne the checklist, and this book; their ongoing feedback ... energized and reinforced me. (Blume, 1990, p. xvii).

The Incest Survivors' Aftereffects checklist has spread from its downstate New York origins to international distribution ... the feedback I have received from these diverse and unrelated survivors and therapists has validated the universality of the profile. (Blume, 1990, p. xv)

Through these claims Blume maintains her category membership as a feminist therapist, the source of her authorial *ethos*. However, I suggest that despite her explicit repudiation of any desire to develop a new diagnosis, by invoking the listing strategies associated with DSM diagnoses, she still benefits from the reifying functions of the discourse of psychological systematisation (syndromes and symptoms).

My analysis suggests that rationalisation, normalisation and systematisation are all important definitional strategies for establishing the repression of traumatic memories as a real phenomenon. The relative success of these strategies is something which must be judged by the reader. In the following section, I examine some of the definitional counter-strategies (offensive rhetoric) employed by opponents of rm. These counterstrategies naturally rely upon constructing rather different definitions and descriptions of repressed and recovered memories.

### Ironising Recovered Memory

If the goal of rm supporters is to establish the reality and 'out-thereeness' of repression, then it is not surprising to find that rm detractors try to do the opposite. Their aim is to show that repressed memories are definitely not the same 'kind of thing' as real psychological phenomena or disorders. The fm authors in my sample draw upon a number of related definitional and descriptive arguments to achieve this goal. Firstly, they attack rm supporters for definitional inconsistency, which is constructed as a sign of scientific inadequacy (McMartin & Yarmey, 1998). They also characterise repression (and repressed memories) as pseudoscientific, contrasting descriptions of what 'scientists know' about how memory works, with the 'empirically unsupported' claims of repressed memory advocates. Having argued that repression has no scientific basis, rm detractors also depict belief in repression as a matter of faith rather than logic. To reinforce this, repressed memories are constructed as the same 'type of thing' as extraterrestrial encounters and other implausible events.

The final rhetorical strategy which I examine is perhaps the most audacious, but potentially effective. It involves attempting to define repression out of existence altogether. This is achieved through the construction of the concept of 'robust' or 'massive' repression in the writing of rm detractors. As McMartin and Yarmey (1999) note, this version of repression is so extreme that it is demonstrably absurd. This absurdity is then taken as evidence that repression cannot be a real phenomenon.

### *Definitional inconsistency*

In psychology, science is the arbiter of authenticity. One of the foundational assumptions of the discipline is that systematic utilisation of the scientific method allows psychologists to differentiate 'real' effects, phenomena and processes from those that are the result of confounding variables, experimenter effects or subjective interpretation (Heiman, 2002). In consequence, claims that a particular phenomenon lacks scientific validity or empirical support can be an effective ironising strategy in psychological debate. While this is most obviously achieved by the marshalling of arguments based upon empirical studies, it is also possible to do this in a more understated way. Rather than quoting results and statistics, repression is simply defined or described as unscientific in a variety of ways – by characterising repression as pseudoscientific, by contrasting scientific knowledge with rm 'beliefs', by defining the 'essential nature' of memory as inimical to repression and through claims of definitional inconsistency.

One of the most common criticisms of repression is that the concept lacks a precise definition (see Holmes, 1990). Its detractors claim that there is no standardised definition of repression, and that there are a wide variety of theories which attempt to explain how the process of repression occurs:

Beyond the shared belief in a powerful repression mechanism., the movement's literature is rife with a seemingly endless number of ideas and explanations for how this repression mechanism works and how memories can be recovered. Trying to examine the many theories that explain this concept of robust repression is like trying to get a grip on fog. While these experts would clearly like to believe they are all referring to the same phenomenon, once one gets past the idea that the mind has the ability to hide vast

knowledge of trauma in the unconscious, the theories about why repression happens vary widely from expert to expert. (Ofshe & Watters, 1994, p. 27)

This lack of a single, agreed definition and explanation of repression is clearly problematic in itself – the literature is “rife” with “a seemingly endless number of ideas” (note the extreme case formulation utilised here). The implication is that the existence of multiple theoretical explanations casts doubt upon the scientific and ontological validity of the phenomenon. As McMartin and Yarmey note, “This attack is grounded in the expectation that definitions must be consistent and precise in order to meet the standards of science” (1994, p. 348).

Furthermore, the formulation of so many different explanations implies a measure of desperation. If even the ‘experts’ on recovered memory cannot decide how it actually works, then there must surely be a problem with the concept itself. Ofshe and Watters infer this through their choice of analogy – trying to understand all of these theories is like “trying to get a grip on fog”, which suggests that the concept of repression may well turn out to be equally ephemeral and insubstantial.

However, there is more than one rhetorical point to be scored here. Not only is this lack of theoretical and definitional consistency construed as a sign of intellectual confusion, it is also constructed as a cynical ploy which is utilised to maintain a viable explanation for the recovery of childhood abuse memories:

In truth, the theories behind repression move and change as necessary to defend the observable results of the therapy. (Ofshe & Watters, 1994, p. 30)

Here Ofshe and Watters claim to have uncovered the ‘truth’ behind the multitude of theoretical explanations for recovered memory. This claim – that the authors are exposing the truth – suggests an attempted deceit on the part of those who are employing these multiple definitions. This duplicity is apparently motivated by a need to “defend” the concept of repression against the “observable results” (reality), which suggests that rm supporters are aware on some level that repression is problematic, but are still unwilling to publicly acknowledge this fact, as this would undermine the validity of recovered memories.



The desire to maintain the legitimacy of these memories has even led some rm supporters to dispense with the term “repression” altogether, according to the following authors:

Following the scientific criticisms of repression, showing the weakness of the evidence for it, some recovered memory therapists have abandoned ‘repression’ and are now using other concepts to explain how memories of traumatic events can be completely banished from consciousness but later accurately recovered. This is an attempt to accommodate the disconfirming evidence rather than admit a mistake and abandon the claims of recovered memory. ‘Dissociation’ is now most often used to describe the process by which memories not known about before can somehow surface and be claimed to be true and accurate. (Wakefield & Underwager, 1994, p. 257)

Because the controversy over repression has become so heated and contentious, clinicians and child-protection advocates often use synonyms such as “lost”, “buried,” or dissociated” to describe repressed memories. But, as psychologist David Holmes writes, “repression by any other name is still repression and the absence of evidence applies to all of the synonyms.” (Loftus & Ketcham, 1994, p. 141)

As McMartin and Yarmey (1998, 1999) have discussed the deployment and ironisation of dissociation thoroughly in their own work, I will confine myself to a discussion of only two relevant points here. Firstly, in both extracts, a sequential utilisation of terms is suggested. That is, rm detractors claim that repression was the concept initially used to explain the existence of recovered memories, and that this term has now been replaced or supplanted by other terms such as dissociation (although in my own reading of pro-rm texts, I noticed that the terms were often used concurrently, as synonyms or as ‘alternative routes’ to forgetting). Secondly, the suggested motivation for the use of these alternative terms and explanations is that rm supporters wish to maintain the validity of recovered memories even in the face of the ‘evidence’ (or lack of it), or that they wish to avoid the “heated” and “contentious” debate that is provoked by the term repression. Both of these motives imply a desire to avoid being confronted with the ‘truth’ about repression via a rhetorical sleight of hand which is distinctly unscientific.

### *Pseudoscience*

In addition, making explicit or implicit claims that repression is pseudoscientific is another popular strategy employed by rm detractors. It is alleged that rm

advocates are deliberating trying to make an essentially unscientific phenomenon seem more valid by appropriating or imitating scientific status. This is a very clever strategy, because it has the potential to effectively neutralise any features of the rm argument which may appear to be scientifically valid. It suggests that such claims do not have to be taken seriously, because they are not based upon factual evidence, but upon unsubstantiated assertions. 'Pseudoscience' imitates science, but does not produce reliable data :

There is no exercise of critical acumen to challenge any of these notions [of the recovered memory movement]. Rather, in the manner pioneered long ago by Freud himself, the mantle of science is wrapped around a pile of nonsense by the simple expedient of declaring it to be science and repeating it over and over. (Wakefield & Underwager, 1994, p. 72)

The entire psychoanalytic movement was founded ... on pseudoscience. Freud and his followers established a mythology of the mind that strove to imitate the medical advances of the time. The concepts of repression and dissociation have never been scientifically demonstrated, however, and tossing around words such as 'abreaction' or 'endogenous opiates' does not make manufactured memories any more factual. (Pendergrast, 1997, p. 544).

Recovered memory therapists in many respects have operated within Freud's pseudoscientific paradigm. In doing so they have become to the study of the mind and behavior what astrologers are to the scientific study of the stars and planets. They have engaged in an enterprise based not on science but on impressionistic insight, myth, metaphor, and the powerful persuasive nature of the therapy setting. (Ofshe & Watters, 1994, p. 295)

All of these descriptions make a link between the rm movement and Freudian psychoanalysis – the definitive psychological pseudoscience, according to some commentators (see Crews, 1995; Webster, 1995). This association further strengthens the assertion that the theory of repressed memories is invalid, because Freudian psychoanalysis has already been comprehensively critiqued and found wanting from a scientific standpoint. Freud's role in the memory debate will be discussed in more detail in Chapter Eight – here it is sufficient to note that he provides a useful rhetorical resource for authors wishing to undermine the scientific status of repression.

A second undermining strategy that is often utilised involves contrasting what psychologists 'know' about memory with what rm memory advocates 'believe'

about repression. Knowledge has a privileged status, as it is evidence based, while beliefs are what people hold in the absence of substantiation, as the following excerpts illustrate:

The belief that almost all memory is stored permanently in the brain is pervasive in the recovered memory movement, and it comes with a corollary idea: that all failures at recall are abnormal. (Ofshe & Watters, 1994, p. 37)

While it's comforting to believe that all our experiences (or, at the very least, the important ones) are stored somewhere in our minds, this model of memory runs directly counter to almost all scientific studies and experiments on the topic. Reviewing the scientific research, a much less perfect, more malleable, and ultimately more troubling picture of memory is formed. (Ofshe & Watters, 1994, p. 38)

Here, Ofshe and Watters contrast the accurate knowledge about and understanding of memory possessed by scientists (a result of "scientific studies and experiments") with the unfounded, subjective beliefs of the rm movement - which appear to be motivated more by wishful thinking ("it's comforting to believe") than by evidence. This suggestion that rm proponents are happier taking refuge in the "comfort" of false "beliefs" about memory, rather than confronting the reality of what memory is 'really like' also works to further undermine their credibility.

The privileged status of scientific understandings of memory is also the basis of a third rhetorical strategy employed by rm opponents. This strategy involves a claim that the essential nature of memory (what it is 'really like') is understood by scientists, and that this nature is incompatible with repression. The argument is made that repression cannot be a real phenomenon, because memory does not (and cannot) work in a way that would allow repression to occur. This argument is developed fully by Wakefield and Underwager (1994), who devote an entire chapter to explaining "The Nature of Human Memory and Forgetting", which opens with the following paragraph:

The nature of memory itself is central to the issue of recovered repressed memories of childhood sexual abuse. The current popular view of memory is that it operates like a videotape or a computer. According to this view, everything that happens to us is precisely recorded and stored somewhere in our brain. Memory consists of pushing the play-back button and retrieving the event. Although events may be forgotten, if the correct button can just be found, the memory can be retrieved. This assumption, which is basic to the belief in

recovered memories, is widely accepted by both lay persons and professionals (Wakefield & Underwager, 1994, p. 165)

Wakefield and Underwager construct their argument carefully, initially alerting the reader to the significance of the 'nature of memory', which according to them is at the heart of the debate over recovered memory. This is followed by a description of the "current popular view of memory" – a phrase which works to suggest the transience and subjectivity of this particular notion of how memory works. This view is then characterised as an "assumption" (not based on evidence, but on "hasty introspection" as we see below) which supports the "belief" in recovered memories.

But beliefs, assumptions and views are not the stuff of which reality is made, as the authors inform us in the next few pages:

#### **The Reconstructive Nature of Memory**

Although people often suppose, on the basis of hasty introspection, that their memories are a process of dredging up what actually happened, in reality our memories are largely determined by our current beliefs and feelings. This conclusion is solidly established in psychology and is supported by laboratory studies and surveys (Dawes 1988; Goodman and Hahn 1987; Loftus et al. 1989; Loftus and Ketcham 1991). What we remember is much more than the original encoding of an event. It is also affected by everything that has happened to us since that event occurred. Even if we accurately perceive and encode an experience, the original memory will be subjected to contamination over time. (Wakefield & Underwager, 1994, p. 167)

The "current popular view" of memory discussed above (that memories are replays of actual events) is characterised here as the product of "hasty introspection" – hardly a sound way of understanding psychological phenomena. On the other hand, the 'reality' of how memory works (reconstructing past events in the light of current beliefs and feelings) is a "solidly established" conclusion which is arrived at on the basis of "laboratory studies and surveys" – it is a general rather than an individual conclusion, deriving its power from consensus. Not only are memories not recordings of original events, they are actually "contaminated" by "everything that has happened to us" after that event (an extreme case formulation). Since scientific research has established that memory "in reality" is reconstructive, there is no way that repressed memories (which rely on a 'reproductive' model of memory) could actually occur.

All of the definitional strategies discussed in this section involve claims that repressed memories lack scientific validity. Characterising repression in such a way is a bold move, but an effective one. Without scientific foundations, the rm argument is unlikely to be successful in psychological circles, where science is the ultimate arbiter of truth. The utilisation of such arguments in books aimed at a mass market suggests that their authors believe that science holds a similar status in the public arena.

### *Faith in repression*

Why then do rm therapists themselves believe in the process of repression, and the veracity of recovered memories, in the absence of scientific support for these concepts? Many of the answers to this question supplied by fm authors are grounded in the motivation and personalities of the therapists themselves. Wakefield and Underwager (1994, p. 157), for example, suggest that there is evidence that rm therapists suffer from 'paranoid thinking'. Historical and cultural explanations are also invoked, and these will be discussed in more detail in Chapter Eight. What is most relevant here, however, is the way in which belief in recovered memory is characterised or described by some authors as being the same 'type of thing' as a religious conviction. In her book, Loftus describes how she comes to this realisation during the course of her involvement in the Franklin trial:

As I sat in the witness box answering the prosecutor's questions, I began to sense the power of this thing called repression. I felt as if I were in a church arguing with the minister about the existence of God. ... I was beginning to realize that repression was a philosophical entity, requiring a leap of faith in order to believe. For those willing to take that leap, no amount of "scientific" discussion would persuade them otherwise. Science, with its innate need to quantify and substantiate, stood helpless next to the mythic powers of repression. (Loftus & Ketcham, 1994, p. 64)

Ofshe and Watters make a similar point:

The more of the literature one reads, the more these women and men begin to sound like religious converts giving witness to the joys of salvation. It becomes unclear sometimes whether these therapists and groups are offering secular healing or a sort of quasi-religious deliverance from a difficult world – deliverance that rests upon one's faith, not in a deity, but in a therapist and the validity of recovered memories. (Ofshe & Watters, 1994, p. 114)

The religious analogy is a useful one because it allows authors to emphasise that RM supporters do not require scientific proof of repression, because their belief in the phenomenon is actually based on faith, rather than logic. Loftus capitalises on this association by labelling RM advocates as “True Believers” (p. 31, in contrast to “Skeptics” like herself), a designation which she then utilises regularly throughout her book. In fact, I would argue that the central theme of Loftus’ book (encapsulated in the title – *The myth of repressed memory*) involves characterising repression as a rather mystical, quasi-spiritual phenomenon, in contrast to the hard, evidence-based science of memory research.

Associating repression and religion is also useful because it helps to provide a reason for belief. Pendergrast (1998) devotes an entire chapter of his book (*Survivorship as religion*, pp. 515-549) to developing this association, beginning by explaining that:

Human beings are religious animals. We cannot exist, it seems, without finding a higher meaning for our lives. (Pendergrast, 1988, p. 515)

The search for meaning through religion is constructed as an innate part of human nature (note the similarity to the construction of ‘suggestibility’ explored in the previous section). Pendergrast then goes on to explain that religion is a universal of human cultures, but that religion can also have a ‘dark side’, and that

It is this darker aspect of the religious impulse that I will explore in this chapter in regard to the Incest Survivor Movement. Survivorship, I believe, has become a pseudo-religion that provides intensity and meaning to people’s lives in a destructive manner. (Pendergrast, 1997, p. 516)

In the rest of the chapter he discusses (among other things) how the recovery of memories in therapy is similar to a religious conversion (pp. 519-523). He then explores the question of whether the ‘Survivor Movement’ is a cult, explaining that he prefers to refer to it as a sect, because he is uncomfortable with the “negative connotations” of the “cult” label (p. 541). However, following this disclaimer, Pendergrast goes on to note that “some self-contained psychotherapy groups qualify for the term ‘cult’” (p. 541-542) and that “even run-of-the-mill recovered memory therapy, conducted one-on-one in a private therapy setting, shares certain characteristics with a cult” (p. 542). This move

from the idea of survivorship (based on recovered memories) as a religion, to a sect, and then finally a cult, is an interesting and clever rhetorical strategy. While the religious impulse that provides the reason for people's belief may be benign, by progressively moving the characterisation towards the negative extreme, the reader is subtly led towards associating recovered memories with cults, and all of the bizarre and potentially detrimental associations which the author claims to be wishing to avoid.

### *Going to extremes – bizarre beliefs and robust repression*

This kind of extrematisation is a useful discrediting strategy which is employed by rm detractors in two main ways. The first is the linking of repression with a variety of bizarre, extreme and 'obviously' unbelievable phenomena in order to suggest that recovered memories of child sexual abuse are the 'same type of thing' as UFO sightings, memories of past lives, memories of satanic ritual abuse (SRA), possession of multiple personalities and so on. All of the fm texts analysed in this study use this strategy to some extent, which attests to its usefulness.

The common factor linking these bizarre beliefs and recovered memories is claimed to be the therapeutic setting and techniques employed. Ofshe and Watters (1994), for example, construct both SRA allegations and MPD diagnoses as natural extensions of recovered memory therapy (p. 177 & 205). In their discussions of these phenomena, they explore in detail the conspiracy theories of rm therapists such as Cory Hammond (pp. 187-198) and Colin Ross (pp. 223-224), both of whom appear to believe in different world wide conspiracies (strangely enough, both involving the CIA) which torture and abuse children in order to 'program' them into becoming useful tools for the cult. The details of these beliefs (as presented by Ofshe & Watters) do indeed seem extremely odd, involving satanic plots to rule the world, and complex and intricate mind control programming that can nonetheless be neutralised by therapists utilising the following method:

"[As you deprogram patients] sometimes robots will block your way", Hammond told his audience. "You can say to the patient, go around and look at the back of the head of the robot and tell me what you see. What they will see are wires or maybe a switch. I tell them to

pull the wires or flip the switch and it will immobilize the robot. Then I tell them to look inside the robot and tell me what you find. Generally they will find several children. I tell them to remove the children and then tell them to vaporize the robot. They are usually amazed this works." As indeed they should be. (Ofshe & Watters, 1994, p. 190)

Ofshe and Watters quote Hammond's own words extensively, perhaps to indicate that in spite of the bizarre nature of this material, they are not embellishing it or making it up themselves in order to cast Hammond in a negative light (this effect of quoting your opponents own words against them is also noted by Antaki & Leudar, 2001). Instead, he is 'condemned out of his own mouth', the credibility of his theories destroyed by their own absurdity, with Ofshe and Watters subtle sarcasm ("As indeed they should be") simply reinforcing the reader's (presumed) disbelief.

While Ofshe and Watters (and the other authors) are not creating this material, they definitely utilise it with rhetorical intent. By focusing on these extreme examples, they create the impression that most (if not all) recovered memories contain bizarre elements which undermine their credibility. And by explicitly linking the origin of satanic-cult stories with recovered memory, they are also laying the foundations for what they claim is an even more compelling attack on the reality of *all* recovered memories:

The accounts of satanic-cult abuse are the Achilles' heel of the recovered memory movement. With no supporting evidence, most reasonable people will eventually question the validity of satanic-abuse stories. The skeptical recovered memory therapist is stuck, for if he questions even a single account of abuse, he challenges the entire structure of the therapy and thereby jeopardises his or her standing within the recovered memory community. ...Repressed memory promoters are boxed into another corner. If they admit that these stories of satanic abuse aren't true, they would have to admit that their therapy methods have produced false accounts that clients have mistaken for memory. By doing so, they would call into question all memories, believable or unbelievable, satanic or not, that have been recovered through the very particular procedures of this type of treatment. (Ofshe & Watters, 1994, p. 194-195 )

I believe that this is the reason why references to satanic ritual abuse and alien abductions are so prevalent in pro-fm texts – because they create 'reasonable doubt'. If some memories are so bizarre as to be obviously untrue, then it is always possible that the other, seemingly more believable, memories are also untrue. However, this creates only the possibility of error (if those memories are



untrue, then the others *might* be), leaving the determination of the truth or falsity of such memories at the individual level. Although opening up the possibility that recovered memories may be fictitious is a significant gain, it does not provide a total refutation of recovered memories. This kind of comprehensive argument is provided, however, by another kind of 'extrematisation' strategy – the creation of 'robust repression'.

Robust repression is a concept coined by Ofshe and Watters (1993), which is essentially a characterisation or description of 'what repressed memory advocates believe repression is'. McMartin and Yarmey (1999) believe that the creation of 'robust repression' is an example of rm proponents' own extrematising strategies (utilised in their descriptions of the repression mechanism, as an attempt to legitimise it) being used against them. Notwithstanding, this strategy is a prime example of the rhetorical deployment of definitions discussed by Billig (1996), in which definitions are specifically designed in order to support the position of the interlocutor.

Ofshe and Watters provide the following description of robust repression in *Making Monsters*:

... the robust repression mechanism supposedly allows the subconscious to wilfully steal away from our conscious mind virtually any traumatic memory. With this mechanism, our mind not only avoids unpleasant ideas and memories, it blocks them completely from consciousness. Not only are the memories of trauma hidden, but they lie frozen in pristine form, awaiting a time when the person is emotionally prepared to remember. The final magic of this new robust repression mechanism is the belief among recovered memory therapists that these traumatic events can be "relived" during therapy. (Ofshe & Watters, 1994, p. 25)

The extreme character of this definition is evident in a number of features, from the anthropomorphisation of the subconscious (which "wilfully steals" memories), and extreme cases ("virtually any traumatic memory", "blocks them completely"), to the "not only – but" construction of the second and third sentences. All of these discursive tactics combine to suggest the unbelievability of the concept, confirmed by the allegation that these properties of the "robust repression mechanism" are so unrealistic that they can be ironically described as "magic". The crowning absurdity, as pointed out by McMartin and Yarmey

(1999, p. 348), is that "The endorsement of such a repressive mechanism means that a person cannot rely on an absence of traumatic recollections to assume that he or she was never victimised". That is, accepting the existence of robust repression means giving up any certainty about your past, as an absence of memory does not necessarily mean an absence of trauma.

While 'robust repression' is Ofshe and Watters' own creation, other authors have either utilised it explicitly themselves (Wakefield & Underwager, 1994, p. 234) or worked up similar concepts. Loftus and Ketcham, for example offer this definition of repression, which has much in common with 'robust repression':

When we use the word "repression", we are referring to more than "ordinary forgetting", which is the act of not thinking about an event or experience for a period of time and then having the memory come back to mind. Repression refers to the active banishment into the unconscious of a traumatic event or series of traumas. (Loftus & Ketcham, 1994, p. 141)

Of particular interest is the way that these authors specifically differentiate repression from "ordinary forgetting". Repression is a totally different phenomenon involving the "active banishment" of memories. The reason for this particular exclusion is apparent later in the book where Loftus recreates her meeting and conversation with Ellen Bass, in the course of which she claims:

"I am only interested in this isolated subset of memories that are labelled 'repressed'. All I want to discuss, all I have the right to examine, is this relatively unexplored part of the survivor/recovery movement concerned with repression".

"But why do we even have to talk about repression?" Ellen asked. "Why can't we just get rid of that word? What if a person simply forgets about an abusive event and then remembers it later, in therapy? She's in a safe place; she feels, perhaps for the first time, that someone will believe her and validate her experience, and the memory suddenly returns. Isn't that a valid experience?"

"Of course it is," I said, "but that's simple forgetting and remembering, it's not this magical homunculus in the unconscious mind that periodically ventures out into the light of day, grabs hold of a memory, scurries underground, and stores it away in a dark corner of the insensible self, within a few decades before digging it up and tossing it back out again."

"But isn't it possible to redefine repression so that it falls more in line with the normal, scientifically accepted mechanisms of memory?" Ellen asked.

"But then it isn't repression," I said, "because repression isn't normal memory". (Loftus & Ketcham, 1994, p. 214).

While Loftus's "magical homunculus" who grabs memories and scurries away to store them in a dark corner owes much to Ofshe and Watters, the specific virtue

of Loftus and Ketcham's argument is that there is no way that repression can ever be 'brought into line' with science. No such compromise can ever be reached, because repression, by definition, is not normal memory. Furthermore, any "valid experience" of forgetting trauma and then remembering in therapy can now be explained as "simple forgetting and remembering", and thus can never be offered as proof that repression has occurred. Repression is, in effect, defined right out of existence.

### Reifying False Memory

Despite this definitional master stroke, my analysis suggests that negative argumentation strategies, while an essential part of the recovered memory debate, are not sufficiently persuasive in and of themselves. Those who are opposed to recovered memories cannot just undermine the reality of rm, because the very existence of these memory claims requires some kind of alternative explanation (If they are not repressed memories of real events, then what are they?). This explanation is offered by the development of the concept of 'false memories' by rm opponents. Somewhat ironically, they have to work just as hard to establish the 'reality' of false memories, as rm advocates do to construct the existence of repression. And, perhaps unsurprisingly, they use very similar descriptive and definitional strategies to do so. False memories are constructed as 'iatrogenic', a term borrowed from medicine, where it is used to identify any illness or injury resulting from a physician's treatment (Martin, 2002). In the memory debate, identifying memories as iatrogenic suggests that they are 'implanted' by the therapist in the course of treatment. This is claimed to be a natural and common consequence of the innate malleability of memory, when combined with the suggestive techniques employed in rm therapy. These techniques are often described as being the same 'kind of thing' as those employed in hypnosis, an established method of enhancing suggestibility. In addition, some of these authors also draw upon the concept of 'False Memory Syndrome', a systematised description of the effects of rm therapy developed by the False Memory Syndrome Foundation.

### *The iatrogenic origin of recovered memories*

The importance of iatrogenesis in the memory debate should not be overlooked by any analyst wishing to examine the structure of this dispute. Iatrogenesis, by offering an alternative causal mechanism for recovered memories, provides a potent refutation to the argument that the very existence of these memories 'proves' that repression must occur. Without iatrogenesis, rm detractors would be forced to find some other explanation as to why so many women (and some men) claim to have memories of sexual violation. They would probably have had to rely on the culturally available rhetoric which constructs such claims as the product of either madness (they are suffering from hallucinations caused by a mental disorder) or badness (they are lying with malicious intent). However, these explanations would have been unlikely to be persuasive in the memory debate, because feminist analyses provide counterarguments that these claims are based on myths and stereotypes designed to perpetuate patriarchal power (Kelly, 1988).

Iatrogenesis, on the other hand, provides an explanation for recovered memories which does not depend upon negative characterisations of the rememberer. On the contrary, iatrogenic memories are simply a product of the intrinsic suggestibility of the human mind, as Yapko (1994) explains:

People can and routinely do accept others' suggestions, and then build their lives around them. Their responsiveness to others' beliefs, perceptions, ideas, behaviors, feelings, attitudes, and values is known as suggestibility. It is an inherent human trait that can predispose us to accept suggestions of all sorts, including suggestions of abuse. (Yapko, 1994, p. 96)

In this excerpt, the author stresses that suggestibility is something which all humans possess. He begins by explaining that "people" (a general descriptor) "routinely" live their lives based upon the suggestions of others. To underline the power of suggestibility, the author claims it is an "inherent human trait" – something which no one can claim to be uninfluenced by, even the author and his audience ("us"). Suggestibility is so pervasive that even you or I might be susceptible to suggestions of abuse!

Establishing suggestibility as a normal part of the human experience removes any implication of fault from the rememberer. Instead, the blame is assigned to the person who is doing the suggesting:

In all the cases in which we have been involved and in the stories from the retractors, the effects of recovered memory therapy are iatrogenic. (Wakefield & Underwager, 1994, p. 116)

Wakefield and Underwager make a global claim here – in all the cases which they have heard of, or in which they have been personally involved, the therapy process has been iatrogenic. The instigators of recovered memories are always the therapists, not the rememberers. Targeting therapists, rather than their patients, means that rm detractors cannot be countered by claims that they are merely engaged in ‘victim blaming’. On the contrary, therapists are people who are in a position of power and trust, yet they engage in questionable therapeutic practices which lead their patients astray:

If the memories aren't real, where do they come from and why are patients so willing to believe them? In his talk, Ganaway blamed false memories on the overuse and misapplication of hypnosis. He held therapists accountable, expressing amazement at the large number of experienced therapists who do not understand the innate suggestibility of their own patients. ... Poorly trained therapists and therapists who operate under a fixed belief system ... are at greatest risk for confusing fact and fiction. Through tone of voice, phrasing of questions, and expressions of belief or disbelief, a therapist can unwittingly encourage a patient to accept the emerging "memories" as real, thus reinforcing the patient's delusions or even implanting false memories in the patients mind. (Loftus & Ketcham, 1994, pp. 85-86)

As this excerpt suggests, therapists are people who should know better . Unlike their patients, who are victims of their inherent suggestibility, therapists should have the benefit of training and knowledge which would give them greater insight into not only their patients, but their own therapeutic practices. By setting themselves up as ‘experts’ who are capable of helping others with their problems, they become vulnerable to censure if they are in error. Harsh criticisms are then justified, such as those made by Ofshe and Watters (1994) who go so far as to explicitly compare recovered memory therapists with lobotomists:

Recovered memory therapy, like lobotomy, illustrates the worst of all possible medical or psychiatric mistakes. Unlike useless treatments that merely do patients no good, the

practitioners of this therapy harm patients and cause them to suffer. (Ofshe & Watters, 1994, p. 303)

As well as providing a plausible alternative explanation for recovered memories, iatrogenesis also offers rm detractors a way to avoid aspersions of victim blaming by allowing them to focus their criticisms on the therapists who they claim are the origin of these memories. The innate suggestibility and malleability of human memory provides a reason for why patients may come to develop false memories in therapy.

In order to firmly establish the iatrogenic origin of recovered memories, all the authors examined in my sample spend a considerable amount of time describing the techniques of recovered memory therapy and examining how these techniques result in the implantation of false memories in the mind of the patient. Because these memories are claimed to be a product of the therapeutic invention, it is necessary for rm detractors to explain convincingly exactly how these therapeutic methods work to convince patients that they have memories of being abused. This is achieved by asserting the power of the therapeutic setting (including the therapist's expectations), and the potential of hypnosis and other related procedures to create false memories.

As discussed above, in order to explain just how suggestion can create memories in therapy, rm opponents construct the nature of the therapy setting as one in which vulnerable patients are influenced by powerful therapists, as shown in the following extracts:

Just being in therapy creates a vulnerability to directly stated or merely implied beliefs and suggestions. (Loftus & Ketcham, 1994, p. 166)

Given the suggestible nature of the therapy client – by definition – the therapist's use of suggestion (which is inevitable) must be especially skilful and exact. (Yapko, 1994, p. 105)

To fully appreciate the power of a recovered memory therapist to change a client's beliefs in this regard, one must analyze the tremendous investment patients make in therapy. The unspoken therapist-patient contract confers great power to the therapist. ... Much of the curative power of talk therapy comes from the therapist's carefully exploiting his or her power over the client. Because clients seek the approval of the therapist, the therapist's

expectations about how the client will behave can have a dramatic impact on the client's actions. (Ofshe & Watters, 1994, p. 111)

What is interesting is that rather than focusing specifically on rm therapy here, these authors are making claims about "talk therapy" in general. They assert that suggestion is inherent to the therapeutic enterprise, and that the power structure of the therapeutic relationship makes it inevitable that the therapist's beliefs, expectations and suggestions will impinge upon the client in some way. Constructing the nature of therapy in this way creates a normalising context for their claims that rm therapy creates false memories – this memory creation can now be seen as merely an extension of the suggestive influencing that occurs as a matter of course in therapy.

Having established the nature of therapy as inherently suggestive, rm opponents go on to explain in more detail exactly how the process of false memory creation occurs. These explanations are necessary to accomplish the reification of the iatrogenic origin of recovered memories. The claim that false memories can be implanted in a person's mind in such a way that they cannot be distinguished from memories of real events requires some form of justification – the mechanisms of memory creation must be shown to be plausible (in much the same way as recovered memory proponents aim to establish the plausibility of memory repression).

These mechanisms encompass a variety of techniques, which are examined in some detail by rm opponents. Some authors suggest that it is not even necessary to create episodic style memories in order to persuade a client into the belief that they were abused. Ofshe and Watters (1994), for example, claim that

Perhaps the simplest way that therapists create a "memory" is by declaration. ... First, the therapist assures [the client] that a symptom is a reliable indicator of abuse. Then he or she proceeds to change the client's definition of memory so that the indicator can be defined as a memory. All this process requires to succeed is a modicum of faith in the therapist's greater knowledge of human behavior and the manifestations of memory. (Ofshe & Watters, 1994, p. 83)

Here we find the implantation of a false memory represented as an entirely unremarkable feat. Through a straightforward process of redefinition, a client

can be persuaded they have a memory of abuse and “all that is required” is a “modicum of faith” in the therapist. This minimisation of the process works to make the implantation of false memories seem relatively easy and straightforward, and thus more plausible. Indeed, as Ofshe and Watters comment later:

The creation of this sort of abuse belief requires no coercion, complex influence, or thought-reform procedure. All that is necessary is that the client believe and trust the therapist. (Ofshe & Watters, 1994, p. 85)

And this belief and trust is constructed as being an inherent part of the therapeutic relationship, because the client enters therapy confident that the therapist is an expert, and is there to help them:

Those who have come to therapy because they believe therapy can help them are predisposed to believe that therapists can interpret and define problems and behaviors, just as a medical patient is inclined to believe that a doctor can interpret the shadows on X-ray negatives. (Ofshe & Watters, 1984, p. 85)

While the use of such simple ‘reframing’ techniques provide one explanation for how false memories can be created, it does not account for the development of the very detailed visual and narrative memories that many survivors seem to possess. According to rm opponents, these memories are probably the product of either formal hypnosis, or other procedures which induce a similar state of heightened suggestibility, all of which are claimed to be common in recovered memory therapy:

Defined broadly as any intentionally or unintentionally induced trance state, the hypnosis can include Amytal interviews and age regression as well as relaxation and guided imagery exercises. In their enthusiasm for their work, recovered memory therapists often uncritically promote hypnosis as one of the most effective methods for finding memories of abuse. (Ofshe & Watters, 1994, p. 139)

Defining guided imagery, age regression and other techniques as the ‘same kind of thing’ as hypnosis is useful because any criticisms and weaknesses of hypnosis can then also be applied to these other techniques, as Loftus and Ketcham (1994) are aware:

... forensic psychologists have concluded that guided imagery promotes a dissociative state similar to that produced by hypnosis; as a result, it may be equally unreliable as a tool for recovering memories. (p. 158)



And the value of hypnosis to rm opponents is that there is an extensive and long standing body of research which suggests that false memories can be created via hypnosis. This research is drawn upon (and sometimes discussed in detail) in order to validate claims that many of the techniques of rm therapy have the potential to produce memories which, although they have no basis in reality, are entirely convincing to the client, as the following passages demonstrate:

The literature on the dangers of the use of hypnosis is not an obscure body of research that a reasonably competent therapist might have simply missed. Warnings about the suggestibility of a hypnotized patient, and the likelihood that he or she will classify what is imagined during hypnosis as memory, have come from any number of sources, including the courts, the *Journal of the American Medical Association*, and prudent hypnotherapists. (Ofshe & Watters, 1994, p. 151)

... present day experimental techniques consistently [verify] that formal hypnosis creates a highly suggestible state in which visualizations, hallucinations, and dreams can be confused with real events. To complicate reality even further, hypnotized patients tend to be extremely confident that such pseudomemories represent real events and experiences. (Loftus & Ketcham, 1994, p. 254)

Claiming that hypnosis, and hypnotic-like techniques are common in rm therapy, and constructing the nature of therapy as inherently suggestive, are two strategies which rm opponents use to make the iatrogenic origins of recovered memories seem plausible to their readers. Without such explanations, claims of memory implantation could seem far fetched or bizarre. But by normalizing the process of memory creation, and drawing on an established body of research in relation to hypnosis, these assertions become much more persuasive.

#### *The False Memory Syndrome (Foundation)*

Like recovered memories, false memories have not only been normalised, but also systematised by their proponents. As Wakefield and Underwager (1994) explain it, "False Memory Syndrome" is inextricably interlinked with the False Memory Syndrome Foundation, as the term was developed in the process of forming this support and advocacy group to help accused parents:

The term 'false memory syndrome' (FMS), was chosen after much discussion as to what would best describe the set of behaviors the FMS Foundation was seeing in the families who contacted them. (Wakefield & Underwager, 1994, p. 96)

These authors go on to provide a detailed definition of False Memory Syndrome (pp. 96-97), which includes elements such as exposure to suggestive influences and a lack of corroborating evidence), and to defend its status as a real syndrome, even going so far as to draw upon the rhetorical power of the DSM themselves:

We believe it is quite likely, as information accumulates on the behavior patterns described by the false memory syndrome, that it will eventually find its way into one of the Diagnostic and Statistical Manual revisions, assuming that the recovered memory phenomenon continues. Until then, it cannot be said to have less support than many other 'syndromes' commonly used today. (Wakefield & Underwager, 1994, p. 99)

This strategy has been discussed above in relation to the various syndromes constructed around recovered memories, and its concomitant utilisation by the 'opposition' only serves to reinforce its reifying power.

However, unlike pro-rm authors in my sample, most of whom utilise the systematisation strategy quite readily, there seems to be a general reluctance amongst other pro-fms authors (apart from Wakefield and Underwager) to draw upon "False Memory Syndrome" as a rhetorical resource in the reification of false memories. Yapko (1994, p. 213), Loftus and Ketcham (1994, pp. 207-208) and Ofshe and Watters (1994, p. 13) all refer to the concept only in passing and in relation to the False Memory Syndrome Foundation, which is recommended as a support group for accused parents. This lack of attention is particularly puzzling in light of the fact that both Ofshe and Loftus (along with Wakefield, and, until 1993, Underwager) have been members of the False Memory Syndrome Foundation's Scientific and Professional Advisory Board since the early 1990s (FMSF Newsletter 1992; 1993) – a fact which they both omit to mention in their books. Perhaps an explanation for this can be found in what Pendergrast (1997) has to say about his connection to the FMSF:

I am tremendously grateful to the FMS Foundation, and I am a member of the organization. When I discovered its existence late in 1992, it provided a lifeline for me. ... However, neither the FMS Foundation, nor its counterpart, the British False Memory Society, sponsored this book, nor did they have any control over its contents. ... Nonetheless, it is quite clear that these organizations and their excellent newsletters have provided an

invaluable service. It is unfortunate that the Foundation has been extensively demonised and vilified by those who believe firmly in repressed memories. (p. xxix)

While Pendergrast acknowledges his membership of the Foundation, he attempts to divorce himself from any suggestion of influence that might be inferred from his association with them. It appears that being identified with the Foundation is a potential threat to authorial credibility (as it may suggest partisanship, and a less than objective interest on the part of the author). As the concept of False Memory Syndrome is so inextricably linked to the FMSF, conceivably it too is considered to be best avoided by many pro-fm authors.

### Ironising False Memory

These attempts by pro-fm authors to reify the concept of false memories have, inevitably, elicited a strong response from recovered memory advocates. Because both Blume and Fredrickson published their books in 1992, prior to the widespread dissemination of the false memory argument, they make no reference to it. However, Terr (1994) briefly addresses the issue, as does Herman (1997) in a new afterword to *Trauma and Recovery* (first published in 1992). And Bass and Davis (1994) include a comprehensive discussion and refutation of false memories in the third edition of *Courage to Heal*, allowing an analysis of some of the rhetorical strategies which pro-rm authors have used to undermine the false memory argument. These include refuting the iatrogenic origin of memories, and the existence of false memory syndrome, and characterising opposition to recovered memories as a politicised 'backlash'.

### *Deconstructing Iatrogenesis*

As discussed above, the arguments of pro-fm advocates work to make the implantation of false memories in therapy seem a relatively simple process, which requires no special coercive powers on the part of the therapist. The 'ingredients' for iatrogenesis lie in the innate malleability of the human mind, and the suggestiveness and imbalance of power inherent in the therapeutic setting. To counter these claims, one of the strategies employed by pro-rm authors is to represent the iatrogenic explanation in quite a different light, as Herman (1997) does in the following passage:

In response, advocates of accused perpetrators have argued that complaints based on delayed recall should be dismissed out of hand, because recovered memories can not possibly be true. They have maintained, rather, that such memories must be wholesale fabrications, invented by psychotherapists, and implanted in gullible minds by means of coercive persuasion. Survivors who come forward to reveal their childhood recollections have been portrayed as the pawns of a malignant cabal of psychotherapists with extraordinary suggestive powers. (Herman, 1997, p. 245)

This description of iatrogenesis evokes a very different impression from the rational plausibility of the pro-fm argument. Herman claims that in order for memories to be implanted, clients must be “gullible” “pawns”, and that “coercive persuasion” must be employed, along with “extraordinary suggestive powers”. Furthermore, the therapists involved are not only “malignant”, but are part of a “cabal”. This extremely negative characterisation is clearly designed to undermine the credibility of the iatrogenic explanation, by making it seem difficult to accomplish and too bizarre to be believable, as Herman goes on to explain:

When these arguments were first proposed several years ago, I found them almost ludicrously implausible, and thought that their frank appeal to prejudice would be transparent at once. The women’s movement had just spent twenty years deconstructing the presumption that women and children are prone to lie, fantasize or fabricate stories of sexual violation. If any principle had been established, surely it was that victims are competent to testify to their own experience. Yet once again, here were eminent authorities proclaiming that victims are too weak and foolish to know their own minds. (p. 245)

By constructing the iatrogenesis explanation as relying upon stereotypes of women as weak, foolish and gullible, the author attempts to further undermine the credibility of the argument. Surely any explanation for these memories which relies on the premise that women don’t “know their own minds” should be dismissed out of hand? This description of the nature of iatrogenesis as a product of patriarchal prejudice, rather than innate human suggestibility, suggests that this should be the case.

A similar argument is advanced by Bass and Davis (1994), though their approach is a little more moderate. They too characterise the iatrogenic explanation as relying upon susceptible clients and unethical therapists, claiming that:

**It is unusual for therapists to convince their clients that abuse took place when it didn't.** [emphasis in original] The core of the "false memory" argument is that fictitious memories of child sexual abuse are implanted in the minds of impressionable patients by overeager, manipulative or greedy therapists, and that they use coercive mind control techniques to do so. This is not how responsible therapists work. ... Good therapists don't lead – they follow their clients into the difficult and painful places they need to go. (p. 508)

The characterisation is similar to Herman's: patients are "impressionable", therapists are "overeager, manipulative or greedy" and "coercive mind control techniques" are required to implant memories. But, Bass and Davis claim "good", "responsible" therapists don't work in this way. And as they later go on to explain, while some therapists are unethical, they are "... not representative of the mental health profession as a whole..." (p. 508) – who are presumably good and responsible.

Unlike Herman, who finds the whole idea of false memory implantation "ludicrously implausible", Bass and Davis are willing to accept the possibility that occasionally even a good therapist will make the mistake of assuming that a client was abused. However, they believe that even if this does occur

... it is very unusual for clients to take on every suggestion a therapist makes. (Bass & Davis, 1994, p. 508).

Unlike pro-fm authors, they characterise the therapy setting as one in which the patient retains some power. They do not automatically take on the suggestions of their therapist, and they are the ones who take the lead in exploring "the difficult and painful places they need to go". Investing therapy clients with this degree of autonomy makes the iatrogenic origin of recovered memories seem much less likely.

This is reinforced by Bass and Davis's next claim – that most repressed memories of sexual abuse are recovered prior to and/or outside of the therapy setting:

**Therapy is rarely the sole or primary trigger for memories of child sexual abuse.**

[emphasis in original] Proponents of the "false memory syndrome" promote the belief that most adult survivors regain their memories of child sexual abuse at the instigation of a therapist. This is not true. Many survivors never see a therapist at all. ... Of those who do seek therapy, many already know about the abuse. Either they've always remembered it and

they now are ready to seek help, or something else has triggered the memories and they need help coping with the fallout. (Bass & Davis, 1994, p. 510)

One interesting feature of this argument is the way that the authors construct the fm position. Bass and Davis suggest that this position involves claiming that “most adult survivors” don’t have memories until they are suggested by a therapist. They then go on to refute this claim by pointing out that many survivors never enter therapy. The reason this position is relatively easy to refute is that it is a ‘straw man’ argument – a version of the fm position which is intentionally constructed to be vulnerable to their critique (Edwards & Potter, 1992). In fact, the fm authors in my sample all make a point of clearly differentiating between people who have always known they are abused and those who recover memories in therapy (see, for example, Loftus & Ketcham, p. 214; Yapko, p. 31). Any claims about iatrogenesis are only aimed at this second group, not at ‘adult survivors’ in general. However, for rhetorical effect, Bass and Davis choose to construct these claims quite differently.

Apart from those who have ‘always remembered’ their abuse, Bass and Davis also argue that, rather than recovering memories after entering therapy, many women enter therapy *because* they have recovered memories. These memories have been “triggered” – a term which implies quite a different process of recovery than those used by fm advocates (such as ‘implanted’, ‘suggested’ or ‘constructed’). Triggering implies an immediate activation; the memory comes back without effort or struggle. The authors then go on to list a wide variety of events that may ‘trigger’ memories of abuse, including “life transitions”, “medical treatment”, and “adult experiences of victimisation”, and conclude their argument by maintaining that

Of course any of these life events may occur while a survivor is in therapy. And a survivor may seek therapy for help in coping with such events. But a therapist’s question, or even suggestion, about sexual abuse is rarely the only or most significant reason for memories to emerge. (Bass & Davis, 1994, p. 510)

By constructing “life events” as the main stimulus for memory recovery, the role of the therapist becomes almost incidental. The survivor may already be in therapy when these events happen, or seek therapy after they occur, but this does not imply a causal connection between therapy and memory recovery. By

offering an alternative explanation as to how and why memories return, the authors attempt to challenge the plausibility of the iatrogenic explanation.

### *The syndrome that doesn't exist*

Although false memory syndrome is only discussed in two of the rm books in my sample (Terr, 1994; Bass & Davis, 1994), the authors of these texts deal with it in exactly the same way – by a straightforward declaration of its non-existence:

The cornerstone of the current backlash is the promotion of the concept of a “false memory syndrome”. In fact, no such syndrome exists, as psychotherapist Karen Olio makes clear: “Syndrome” usually refers to a documented group of signs and symptoms that characterize a particular abnormality. In this case, however, there have been no clinical trials, no scientifically controlled comparison groups, no research to document or quantify the phenomena. “Syndrome” is used simply to create an aura of scientific legitimacy ... (Bass & Davis, 1994, pp. 485-486)

Here the existence of the syndrome is challenged on the basis that it lacks any scientific foundation. A syndrome is defined as requiring “documentation” – but as there is no evidence (in the form of research or clinical trials) to back up false memory syndrome, then it cannot be said to exist. Furthermore, without the actual (rather than predicted) endorsement of inclusion in the DSM, the use of the word ‘syndrome’ seems to invite dismissal out of hand. Claiming diagnostic status before it has been conferred creates an opportunity for your opponents to expose this as “simply” a rhetorical ploy, designed to appropriate legitimacy where it is not deserved.

Terr’s approach is similarly direct, though her argument is slightly different:

Political advocacy groups, however, such as the False Memory Syndrome Foundation, have garnered so much publicity for their cause that this cause is being confused today with a diagnosis. A therapist asked me at a recent meeting whether a patient we had discussed suffered from “false memory syndrome”. There is no such disorder. It is just a name taken by an organization representing parents, uncles, grandfathers, and siblings who believe they have been falsely accused. (Terr, 1994, pp. 164-165)

According to this author, false memory syndrome doesn’t exist as a disorder, but only as a name for a “political advocacy group”. This easy attribution of ‘interestedness’ is, as I suggested above, one of the major shortcomings of the false memory syndrome as a rhetorical resource (and perhaps the reason why

most pro-fm authors avoid utilising it). And by characterising the FMS Foundation as a “political” advocacy group, rather than simply a support group, this interestedness is further reinforced.

*It's (only) a backlash*

Arguing that false memory arguments are politically motivated is, in fact, the overarching strategy employed by Bass and Davis in their response to the false memory position. They characterise any critique or questioning of recovered memories as being part of a generalised ‘backlash’ against

... survivors of child sexual abuse, their supporters, and the significant social progress they have made. (Bass & Davis, 1994, p. 477)

The term “backlash” (which they used repeatedly ) is one which implies a lack of considered and rational thought on behalf of those engaging in it – like a ‘knee jerk reaction’, it minimises the significance of the response by suggesting that it is reflexive rather than reflective. The backlash is personalised (against survivors), but also politicised (against “social progress”). This progress, according to Bass and Davis, includes a willingness to protect children, hear the voices of adult survivors and even change laws. But, they caution,

This is a revolutionary change, and such change does not come without opposition. There is a documented history of backlash against every progressive movement to redress the rights of the disenfranchised and oppressed. ... The current backlash is in direct response to the activism of survivors. It was not until survivors started challenging and changing the laws regarding the accountability of perpetrators – and suing their abusers – that claims of “false memory syndrome” started to appear. (Bass & Davis, 1994, p. 483)

In this passage, the “backlash” is situated in a socio-political sphere through the claim that it is a “direct response to the activism of survivors”. And this response is constructed as being motivated by a desire to allow perpetrators of abuse to continue to avoid accountability for their actions – another example of ‘interestedness’ being invoked. Bass and Davis further minimise the false memory position by constructing it as just one of many such responses to other “progressive movements”. The implication is that acknowledging and accepting recovered memories moves us forward into a more equal and caring society, while accepting the false memory position is regressive, as it gives abuse perpetrators a way to escape justice (an outcome which surely no one can desire?).



In this chapter I have examined the ways in which definitions and descriptions of memory phenomena are rhetorically constructed and undermined. Not surprisingly, the rhetorical strategies employed by both sides are often quite similar (though employed toward diametrically opposite goals). In fact, there appears to be a level of broad (if unspoken) agreement among the authors in my sample about the kinds of arguments (such as normalisation and systematisation) that will be persuasive to the general audience at which these books are aimed. These shared understandings of what 'works' in argument are also apparent in the other common resources drawn upon to support the recovered memory and false memory positions, such as the scientific arguments examined in the following chapter.

## Chapter 7: Science as a Rhetorical Resource

In addition to the issues of authorial credibility and definition discussed in the previous two chapters, authors deploy a wide range of *evidence* to back up their positions. This evidence is drawn from a variety of sources, and I have chosen the three most prominent of these (those which feature most strongly in the texts) to examine in more detail in the next three chapters. These three sources are: scientific research and values; historical events; and personal experiences.

Perhaps the most conventional of these three sources of evidence is scientific research and values. As noted in the previous chapter, arguments based on scientific evidence hold a privileged status within psychology. Knowledge that is the result of systematic observation undertaken within the parameters of the scientific method is considered much more authoritative than knowledge derived from other sources (such as personal or clinical experience). Although the authority of scientific empiricism has been challenged by new perspectives (Myerson, 1994), on the whole these critiques have yet to make much impact on mainstream psychological thinking. Therefore, it is not surprising to find that science features as an important resource in the memory debate. In this study I have examined the discursive construction and utilisation of scientific evidence from a rhetoric of inquiry perspective, which recognises that science is both a rhetorical construction and a rhetorical resource (Gross, 1990; Nelson, Megill & McCloskey, 1987).

In this chapter, I expand upon the deployment of scientific claims and evidence which were touched on in the previous chapter. Firstly, I examine how proponents establish the scientific status of their positions by the construction of versions of science based upon values and principles such as scepticism, verifiability, falsifiability, disinterestedness and generalisability. This prefaces an analysis of two specific studies, the Loftus 'Lost in a Shopping Mall' experiment, and Linda Williams' prospective study of trauma victims. These studies are of central importance to the fm and rm positions respectively, and I examine how they are deployed and undermined in argument. Finally, I consider the rhetorical utility of constructing science itself as problematic.

## Science on their Side

In the previous chapter, I examined how various phenomena within the debate (such as repression and false memory syndrome) were described as being 'unscientific' as a way of undermining their objective existence. These descriptions of specific psychological phenomena are situated within the broad claims made by both sides about the scientific validity of their own positions, in contrast to their opponents'. The central importance of science as a warranting device is highlighted by the way in which both sides attempt to lay claim to scientific status, while undermining the claims of their adversaries. These claims are revealing, because they demonstrate the ways in which the scientific method itself can be recruited as a rhetorical resource. In this section I examine these warrants, and how they are embedded in, and derive their persuasive power from, particular constructions of the nature of science. In particular, following Prelli (1989), I consider how Merton's (1973) institutional norms (and other scientific values) are invoked as rhetorical resources.

In the rm and fm texts, claims about science are often made as if there is universal agreement on what constitutes the 'scientific method'. However, studies of science from philosophical and sociological perspectives indicate that understandings of what makes science 'scientific' (an accurate route to true knowledge of the world) have always been subject to revision and change (Riggs, 1992). Many theories and descriptions of science have been developed, with successive theories not necessarily replacing previous ones, but often standing alongside them. I suggest that this multiplicity of descriptive possibilities is one of the reasons that science is such a useful resource for both rm and fm proponents.

### *FM Science - Scepticism, Proof and Falsification*

At this point, the reader should not be surprised to find fm proponents claiming that only their position is exemplified by scientific values, values which are lacking in the opposing rm position. Take, for example, the following excerpt in which Loftus and Ketcham briefly sketch their version of the positions in the debate:

On one side are the "True Believers", who insist that the mind is capable of repressing memories and who accept without reservation or question the authenticity of recovered memories. On the other side are the "Skeptics", who argue that the notion of repression is purely hypothetical and essentially untestable, based as it is on unsubstantiated speculation and anecdotes that are impossible to confirm or deny. (Loftus & Ketcham, 1994, p. 31)

Even within this short extract, at least three different principles of scientific investigation are invoked. The first of these is "organised scepticism", one of the four "institutional imperatives" identified by Merton (1973) in his analysis of science as a social system. Organised scepticism refers to the responsibility of scientists to not only rigorously critique their own assumptions, but to expose their research to the appraisal of their peers. Because rm proponents "accept without reservation or question the authenticity of recovered memories" they are clearly not measuring up to this standard. The stance of fm proponents, on the other hand, exemplifies scepticism to such a degree that Loftus and Ketcham can apply it to their position as an identifying label.

Ofshe and Watters also invoke the value of organised scepticism when they construct rm proponents as being disinclined to engage with their critics:

As the criticism grows, those inside the recovered memory movement are becoming increasingly unwilling to discuss, much less admit, any possible problems wrought by their methods. (Ofshe & Watters, 1994, p. 13)

By refusing to even consider objections or questions raised by others, the rm position is shown to lack the necessary openness and rigour required if it is to be considered truly scientific. However, according to these authors, even when rm proponents do open themselves to critique (through the publication of research papers), they do not stand up to scrutiny:

On analysis, these papers not only fail to show what they purport to show but, perhaps more important, attest to the poor quality of what is accepted as legitimate research in this segment of the mental-health community. (Ofshe & Watters, 1994, p. 305)

The claim is that rm proponents accept "poor quality" research as legitimate – presumably because they are insufficiently rigorous in the standards of scepticism they apply. This scepticism is not lacking in the fm position, however, because the authors have been able to utilise their critical acumen to expose the faults of both method and interpretation in the research which "purports" to provide supporting evidence for repression.

In addition to organised scepticism, Loftus and Ketcham invoke two other scientific principles in the excerpt quoted at the beginning of this section. In characterising the rm position as founded on “unsubstantiated speculation and anecdotes that are impossible to confirm or deny”, the authors are drawing upon the verifiability principle which, according to Riggs (1992), is the central tenet of the logical positivist philosophy of science. This principle demands that any claims to scientific validity must be based upon systematic empirical observation. If a theory or phenomena cannot be corroborated by sensory evidence (proof), then it cannot be considered scientific. Loftus and Ketcham place this principle at centre stage by prefacing *The Myth of Repressed Memory* with the following dedication:

Dedicated to the principles of science, which demand that any claim to “truth” be accompanied by proof (Loftus & Ketcham, 1994, p. ii)

This dedication constructs the nature of science as being specifically concerned with the verifiability of truth claims. It also serves the purpose of suggesting to the reader that the authors (and, by extension, the position they espouse) exemplify these ideals in their search for ‘truth’.

Ofshe and Watters also draw on logical positivist understandings of science, especially in their criticisms of the rm position. This can be seen in the following extract, where they claim that rm proponents have

... slipped the ties that bind their professions to scientific method and sound research. Free from any burden of proof, these therapists have created an Alice-in-Wonderland world in which opinion, metaphor, and ideological preference substitute for objective evidence. (Ofshe & Watters, 1994, p. 5).

This critique of the rm position draws its persuasive power from contrast, utilising an oppositional model of rhetorical construction (Cockroft & Cockroft, 1992). Indeed, it is part of a much longer passage (Ofshe & Watters, 1994, p. 5-7) in which the authors provide very detailed, contrasting descriptions of the rm and fm positions. In this short excerpt, the comparison is made between what ‘counts’ as supporting evidence for rm proponents (opinion, metaphor, ideological preference) and what *should* count if they were operating from the standpoint of a logical positivist version of science (objective evidence). By invoking the notion of ‘burden of proof’ (a legal, rather than a scientific, concept) the authors assert that the responsibility for verifying truth claims in science lies

with those who are making such claims. According to Ofshe and Watters, rm supporters fail to offer the required type of proof ('objective evidence') and this failure undermines any claim they may have to scientific credibility. In fact, their lack of credibility is so extreme that for rm supporters normal standards of proof are subverted – 'scientific method and sound research' are replaced by an 'Alice-in Wonderland world'. Careful readers may note the irony of the authors using a metaphorical construction to critique their opponents' use of metaphor. However, this is a point which seems not to trouble Ofshe and Watters, perhaps because they believe that the persuasive effect of their rhetoric will overwhelm such considerations.

Returning to the original Loftus and Ketcham passage above, the third scientific principle invoked (in the criticism of "the notion of repression" as being "essentially untestable") is that of falsification. This principle proposes that the process of science operates by rejecting theories whose predictions are not supported by observation. Thus, what makes a theory scientific is its capacity to be falsified (contradicted) by observational evidence. The criterion of falsifiability was initially proposed by Popper (1934/1972) as an *alternative* to logical positivism. His suggestion was that science works not by a process of verification, but by a process of refutation. Thus, only a theory which is capable of being tested (refuted by observations contrary to predictions) can be considered to be truly scientific. However, in Loftus and Ketcham's account of science, verifiability and falsifiability are often linked and made to work in partnership:

But where was the proof to support these outlandish theories? Ofshe wondered. Any scientist worth his salt would demand proof before he permitted himself to accept something that couldn't be seen. Science demands facts; science requires a hypothesis that can be disproven ... (Loftus & Ketcham, 1994, p. 251)

In science everything hinges on proof and evidence. We call this the scientific method. Scientists can't just pronounce that the earth is round or the force of gravity keeps our feet on the ground without offering evidence in support of their theories (not, at least, if they want to call themselves scientists.) A scientific theory has to be falsifiable, which means that, in principle at least, some other scientist can come along and create an experiment designed to prove that the earth isn't round or that gravity doesn't keep us grounded. But ... how can a

scientist prove or disprove that a spontaneously recovered memory represents the whole truth and nothing but the truth rather than some creative blending of reality and imagination or, perhaps, just plain and pure invention? (Loftus & Ketcham, 1994, p. 65)

In these excerpts, the authors are invoking the verifiability and falsifiability principles in tandem. Even though these two principles are logically divergent, this distinction seems to be obscure enough to pose little problem for the authors. On the contrary, this version of science allows them to mount a stronger, two pronged attack on the rm position – not only is there no evidence or proof to verify it, but it is also unable to be falsified. Verifiability is understandably important to Loftus because, arguably, her most significant contribution to the memory debate has been the design of an experiment which ‘proves’ that false memories of childhood events can be implanted in adults (this study will be examined in more detail below). It is also possible that the authors feel that while the notion of falsifiability may be somewhat esoteric (in this form, it is a logical, rather than an empirical criterion), the assertion that theories be verified by ‘hard facts’ (observational data) is perhaps more likely to resonate with the ‘general reader’.

Like Loftus and Ketcham, Wakefield and Underwager also take advantage of the descriptive possibilities to construct a rhetorically useful version of science. Their version of falsifiability, for example, is particularly flexible. In the passages below, Wakefield and Underwager use this principle in a similar way to Loftus and Ketcham; that is, to assert that the rm position cannot be scientific because it is not falsifiable. Firstly, they explain the principle to the reader, thus:

Good scientific theories are not those that prove something by inductive reasoning. ... Scientific theories must always be able to make predictions in such a way that they can be falsified – shown to be wrong. Good candidates for theories are those that can be shown to be wrong, while bad theories are those that explain everything. Science advances by the process of producing hypotheses about how things work, finding a way to test them, and discarding those that don't work out. It keeps only those that are not proven wrong.

(Wakefield & Underwager, 1994, pp. 254-255)

This seems very straight-forward, and the authors go on to explain why the rm position fails this simple test of falsifiability:

Science continually challenges accepted beliefs and tests them to find out which are wrong. This is how there is progress. If everything is already explained, there is no progress. This is the position the recovered memory proponents are in. They explain away all contrary

evidence and refuse to accept that any of their concepts have been falsified. (Wakefield & Underwager, 1994, p. 255).

Although the logic of Wakefield and Underwager's argument is that rm is a "bad theory" (because it "explains everything", and therefore impedes the progress of science), things begin to get a little more complicated in the final sentence of this excerpt. Here the focus changes to rm proponents, rather than the theory itself. The problem with rm is now explained as residing with them; they are the ones impeding scientific progress by "explaining away" the falsification provided by "contrary evidence". So in addition to claiming that rm is a "bad theory" because it cannot be falsified, these authors also claim that rm is a bad theory because it has been falsified – despite the fact that rm proponents refuse to accept this falsification!

So why do fm authors like Wakefield, Underwager, Loftus and Ketcham make so much of the falsifiability principle? One possible answer is provided by Hull (1981, in Gross, 1990), who suggests (in relation to the field of taxonomy) that the reason scientists pay so much attention to falsification is because they believe it can show that their particular theories are 'truly scientific', while those of their opponents are not. The usefulness of falsifiability for this purpose is, in fact, helpfully explained by Wakefield and Underwager who claim that:

Falsification ... means that the considerable support there may be for a theory is not nearly as crucial as one validated instance which contradicts the theory. ... The quality of the research must always be examined. There are good, credible studies and bad, flawed studies. Only those that meet the minimum standards for methodology should be attended to. Also, the number of times a theory has been supported is not the critical element. There can be a large number of supportive studies, but a single instance of credible falsification means that the theory ought to be discarded. (Wakefield & Underwager, 1994, p. 255)

So, falsification provides for the wholesale dismissal of a theory on the basis of a "single credible falsification", which may consist of only "one validated instance". This construction of the principle of falsifiability provides a simple and direct route to the discrediting of recovered memory, since only one instance of contradictory evidence is required (see, for example, pp. 62-63 where they claim that "numerous reports" in the "literature on the effects of sexual abuse" show that the experience is not always traumatic, which "falsif[ies] the major premise of the theoretical scheme for recovered memories"). On the other hand,



it protects, to some extent, the status of the false memory position, because that instance needs to be “credible” or “verified”, a status which can always be contested by attacking the “quality of the research” produced by rm proponents.

*RM science – disinterestedness and ecological validity*

On the whole, fm authors characterise rm proponents as either wilfully ignoring scientific evidence (see Ofshe & Watters, pp. 5 & 10) or failing to understanding basic scientific principles, for example, Wakefield and Underwager (1994) claim that “only those who have no understanding of the principle of falsification” (p. 256) will be persuaded by rm arguments. While it is accurate that the early texts of the rm movement (such as the first edition of *The Courage to Heal* and Fredrickson's *Repressed Memories*) rely more on experiential rather than scientific warranting to establish their authority, a careful examination of the other texts in my sample, particularly those published more recently, shows that the rhetorical power of science, far from being ignored by rm proponents, is actively recruited by them. They, too, draw upon the scientific principles of scepticism and verification (although their approach is generally more subtle than that of their opponents). They also bolster their arguments by bringing to the fore additional principles, such as objectivity and generalisability, both to support their own position and undermine that of their opponents.

Blume, for example, demonstrates the rhetorical utility of science by drawing upon it as a warrant for her “Incest Survivors’ Aftereffects Checklist”, in addition to her use of feminist models of knowledge discussed in the previous chapter.

She states that:

Current research studies are validating the observations on the checklist. As often as the truth of this checklist has been validated, however, I am aware that the information it contains is “skewed”: it is based on the experience of incest survivors whose problems drove them to seek help, be it professional or peer-related. (Blume, 1990, p. xxiv)

By claiming that the “truth” of her checklist has been “validated” by “research studies”, Blume invokes a version of science based upon the verifiability principle. Her own “observations” (note the empiricism implied by this lexical choice) have been replicated by others, lending the weight of consensus to her claims. Her subsequent critique of her checklist on the grounds that it is biased

because of the “skewed” nature of her sample, actually adds to its scientific credibility by drawing upon the principle of organised scepticism. Blume is demonstrating her scientific credentials by showing that she is able to critically appraise her own research, and identify possible errors. The subtle effect of this particular strategy (invoking organised scepticism) is that the reader is reassured, rather than discomfited, by this revelation. If the author is scientifically rigorous enough to critique her own findings, then we can have more, rather than less, confidence in her knowledge claims.

Herman and Terr draw even more heavily upon science than Blume does. Both of these authors position themselves as scientists – they are working from within the dominant paradigm, rather than outside of it, as can be seen in this excerpt from the prologue of *Unchained Memories*:

I began to realize that I knew something about memories of childhood trauma from three research studies on traumatized children which I had already completed. ... I knew, too, that in the scientific literature on psychology and brain research there was a great deal of information about memory that could be integrated with what we observe in adults who were traumatized as children. (Terr, 1994, p. xiv)

Terr establishes her scientific credentials by highlighting both her status as a practising researcher and through her knowledge of the “scientific literature on psychology and brain research”. That is, she implies that she can draw upon not only her own research, but the research of others to provide support for her position – thus providing scientific verification of her theories. Similarly, Herman uses the opening pages of her book to highlight her status as a scientist:

Chris Perry has inspired me with his researcher’s generosity and integrity. As principal investigator of an ongoing study of people with personality disorders, he was initially skeptical about the importance of childhood trauma, yet he made all of his resources available to put the trauma hypothesis to a rigorous test. Though we started out as unlikely collaborators, we have grown together and influenced one another in unexpected ways. My thinking has been deepened and enriched by our partnership. (Herman, 1994, p. x)

The scientific principles of organised scepticism and falsifiability are both drawn upon in this passage. Herman specifically describes her colleague as “initially sceptical”, indicating that he approached the issue with the correct scientific attitude, thus making his eventual persuasion more convincing. Furthermore, he was able to help her to “put the trauma hypothesis to a rigorous test”, and since

the logic of hypothesis testing rests upon the principle of falsifiability, it too is invoked in support of her position. Finally, Herman is able to portray herself as partaking of these scientific values by emphasising their status as colleagues and partners.

Both Herman and Terr use research from neurobiological studies of memory to support their positions. Terr draws more heavily upon animal studies, including studies of fruit flies (pp. 99-100), and marine snails (pp. 103-103). She links these studies to her trauma theory in passages such as the following:

A traumatic memory established in childhood remains an important influence into adulthood. It is possible to prove how lasting and influential childhood memories can be, in lower forms of life, at least. Using his shock-aversion technique on fruit-fly larvae, Tim Tully of Cold Spring Harbor has shown that childhood memories last through something as strikingly disruptive as metamorphosis. Once put through eight shock cycles, the larvae will retain their memories over the five days that it takes them to metamorphose into adults. These experiments carry important implications for human children. And they fit in with a clinical research study I published in 1988 concerning twenty youngsters who had been traumatized in various ways under the age of five. (Terr, 1994, p. 109)

Firstly, Terr claims that the fruit fly experiments she is discussing have “proven” that the “childhood memories” of larval fruit flies are retained throughout the process of metamorphosis. Rather than stating that these experiments ‘prove’ a similar process in humans (which would be difficult to argue), Terr merely claims they have “important implications” (without stating just exactly what these implications are), and justifies this by linking them with a study of her own, in which she observed the persistence of traumatic memories in children.

Herman, on the other hand, focuses more upon studies of human neurobiology (see, for example, pp. 36, 38-39, 238, 239 & 240). In documenting the chemical and structural changes that occur in the brain as a result of trauma, Herman presents these studies as evidence of the link between trauma and dissociation, and of the long term effects of trauma, as she explains below:

Some of the most exciting recent advances in the field [of trauma study] derive from highly technical laboratory studies of PTSD. It has become clear that traumatic exposure can produce lasting alterations in the endocrine, autonomic, and central nervous systems.<sup>8</sup> New lines of investigation are delineating complex changes in the regulation of stress hormones,<sup>9</sup> and in the function and even the structure of specific areas of the brain. Abnormalities have

been found particularly in the amygdala and the hippocampus, brain structures that create a link between fear and memory.<sup>10</sup> (Herman, 1997, p. 238)

Taking the study of trauma into the controlled environment of the laboratory, where “highly technical” studies can be conducted, allows the scientific credibility of the rm position to be established much more readily. These studies contribute towards the weight of evidence in its favour (according to the verifiability principle). However, they also contribute in another way, as Herman goes on to explain:

As evidence of the central importance of dissociation in traumatic stress disorders has continued to accumulate, it has also become apparent that dissociation offers a window into consciousness, memory, and the links between body and mind. Posttraumatic and dissociative phenomena have therefore begun to attract the attention of a new generation of basic researchers, whose interest does not stem primarily from engagement with traumatized people, but rather from a more abstract scientific curiosity. This development constitutes a welcome sign that traumatic stress studies are moving into a status of full legitimacy within the mainstream of scientific investigations. (Herman, 1997, p. 240)

In this passage, Herman is invoking another of the institutional imperatives of science identified by Merton (1973) – disinterestedness. This imperative demands that the scientist be personally detached from their research. A principled scientist pursues knowledge for its own sake, rather than from a desire to satisfy a particular personal interest (financial or otherwise). In this case, we find that the producers of this new “basic research” are not motivated by their personal concern for traumatised people (as researchers such as Herman herself have been), but rather an “abstract scientific curiosity”, a phrase which perfectly captures disinterest.

However, while laboratory evidence can be useful for supporting rm claims, it can also be problematic when the evidence produced does not support the rm position. For example, while Terr is happy to claim that neurobiological studies of insects and snails have “important implications” for her theories of childhood trauma, she is not convinced that experimental studies of memory (which contradict her position) are relevant:

What comes from the memory lab does not apply well to the perceptions, storage, and retrieval of such things as childhood murders, rapes, or kidnappings. Trauma sets up new rules for memory. You can't replicate trauma in an experimental lab. You can't simulate murders without terrorizing your research subjects. Experiments on college students do not

simulate clinical instances of trauma. And they have little to do with childhood itself. (Terr, 1994, pp. 51-52)

In this passage, Terr highlights one of the major issues in the memory debate: the ethical issues involved in conducting generalisable experiments in relation to the effects of trauma. And according to Terr, because “trauma sets up new rules for memory”, other memory research is not relevant to the issue. Here Terr is drawing upon the principle of ecological validity, which demands that the experimental conditions in which a phenomenon is studied must bear as close a relation as possible to its real -world occurrence, in order for results to be generalisable. Schmuckler (2001) notes that ecological validity has historically been a concern of psychologists, as the complexity of real life is extremely difficult to replicate in the controlled laboratory conditions required for rigorous experimentation. According to Terr, when the ethical requirements that subjects not be harmed in the course of research are added to these concerns, the impossibility of conducting ecologically valid experimental research on trauma memory becomes evident.

The principle of ecological validity is also invoked by Bass and Davis in the following passage:

The research cited to validate the existence of a “false memory syndrome” consists largely of studies designed to learn about variations in normal memory and to assess the accuracy of eyewitness accounts of incidents such as car accidents. Such studies show that memory is fallible and that people are often in error when they report what happened. However, this research does not apply to traumatic amnesia. To say that the errors in ordinary memory prove that fictitious memories of child sexual abuse can be implanted by therapists violates a basic tenet of science: Findings pertaining to one specific set of circumstances and one population cannot be assumed to hold true for a different set of circumstances and a different population. (Bass & Davis, 1994, p. 511)

Here the authors follow the strategy which, as we have seen above, is popular with fm authors; they explicitly construct science as an enterprise based upon or governed by principles or rules, rules which are broken by their opponents. The “basic tenet” which they describe here is a version of ecological validity, in that it has to do with generalisability – the ability of research findings to be applied to situations different from the ones in which they were produced. Like Terr, the authors are making the point that “normal memory” is different from “traumatic

amnesia”, and accordingly that the studies cited by fm supporters are irrelevant to the debate.

Pro-fm authors are, understandably, unwilling to acknowledge that this is the case. While Loftus and Ketcham (1994, p. 58) admit that “The traumatic situations we induce in the laboratory are mild when compared to many real life traumas” they simply assert (without supporting argument) that “our documented, replicable findings can be generalized to real-life situations”.

Pendergrast also asserts the relevance of memory studies by researchers such as Loftus, Neisser and Spanos (1997, p. 60), and makes the point that the ecological validity of some studies quoted as supporting evidence for the rm position is questionable:

In the same chapter, Terr cites a shock-aversion study conducted on fruit fly larvae. Five days following their aversive shocks, after their metamorphosis into flies, they still remembered their training. What, one may inquire, does this have to do with repressed memories? ‘Eight electroshock cycles to a fruit fly larva suffice to alter the behavior of the adult fly,’ Terr writes. ‘One drowning attempt can change a child’s life’. I don’t think most of us need a fruit fly experiment to prove to us that attempting to drown an infant is a truly horrible thing to do. Because the adult flies weren’t hypnotized to determine whether they could recall their abuse as larvae, the study says nothing about repressed memories of human beings. (Pendergrast, 1997, p. 70)

Pendergrast’s reference to hypnosis here is clearly sarcastic (elsewhere he is extremely critical of its use), echoing the tone of the entire passage, which is carefully constructed to make Terr’s generalisations from fruit flies to humans seem truly ridiculous. The construction of the passage assumes that the reader will be unable to see the relevance of the study – “what one may inquire, does this have to do with repressed memories”? – and that Terr’s explanation (or rather, Pendergrast’s version of it) will be unconvincing – “I don’t think most of us need a fruit fly experiment to prove to us that attempting to drown a child is a truly horrible thing to do”. Pendergrast then positions himself and the reader (the right-thinking ‘us’ who are able to independently exercise our moral judgement), against Terr, who quotes irrelevant fruit fly research in an effort to convince us of something which should be self-evident .

Ofshe and Watters' response to the ecological validity criticism is a clever attempt to turn it into an argument which actually supports their position. They cite a number of laboratory studies indicating that false memories can be created in subjects under hypnosis, but note that these studies are dismissed by rm supporters as irrelevant to "real life". In response, they suggest that

... Herman's objection that memories retrieved in the laboratory are distinct from those retrieved in a clinical setting should, perhaps not be dismissed so quickly. No one in the laboratory has ever attempted what we believe is happening in therapy settings. Like all good scientific tests, what the experiments surrounding hypnosis are clearly intended to do is establish unambiguous causation. This requires that the experiment be boiled down to very small, controllable variables. Done well, however, these experiments allow the researcher to say with assurance that one variable was responsible for changing another. To apply these results to the rest of the world requires careful extrapolation. While these experiments do not prove that complex memories of childhood can be implanted in patients, they do seem to prove that singular beliefs about memory can be influenced. While laboratory experiments have only intended to prove that a single subtle suggestion can be implanted and internalised, the potential for such influence in therapy is manifold. (Ofshe & Watters, 1994, p. 150)

The authors offer some interesting insights here. Firstly, they characterise science as the search for certainty. Experiments are designed with one goal in mind – to "establish unambiguous causation", so that the researcher can make knowledge claims with "assurance". But in order to do this, the complexity of real life must be simplified, with any results produced then requiring "careful extrapolation" back into real world situations. Thus, they specifically state that that the studies they have cited cannot prove that the false memory position is correct. By acknowledging the tension that exists between the scientific imperatives of experimental control and ecological validity, Ofshe and Watters present themselves as reasonable and sophisticated thinkers about these issues. Therefore, the reader is more likely to give serious consideration to their final point – that if a single hypnotic suggestion, under controlled conditions, can be turned into a memory in the laboratory, then there is an even greater opportunity for this to happen in real life, because there are no restrictions on the suggestive influences of the therapist. In other words, the authors imply that the principle established in the laboratory is sound (because the experimental method gives us "assurance" of "unambiguous causation"), and that only the extent of the suggestive influence varies from the laboratory, where it is

minimised for the sake of control, to the therapy setting, where it is “potentially ... manifold”.

In contrast to these strategies, Yapko counters the rm criticisms of laboratory research with his claim that it is possible to carry out ecologically valid experiments, by introducing the concept of the “field experiment”:

... a number of studies over the years have demonstrated that false memories can be implanted. Critics of these experiments, however, invariably claim they are not “ecologically valid”, meaning that they may be interesting laboratory findings, but don’t necessarily apply to the “real” world.

What happens in the “real” world? A trusted person – the therapist - creates a context of exploration and discussion of the client’s inner experiences, including all his or her arbitrary beliefs and gaps in understanding. How can laboratory experiments approximate such conditions. One way is to make use of what is known as the field experiment – a study conducted in the real world.

While it would be unethical to create abuse situations with the goal of later seeing what happens, research psychologist and memory expert Elizabeth F. Loftus, Ph.D., did a field experiment that more closely approximates what happens in therapy. (Yapko, 1994, p.125)

In this passage it is interesting to see Yapko ironising the concept of the real world (by placing quotation marks around the word real) when he reports the criticisms made by rm advocates. This alerts the reader to the possibility that the rm version of the real world may be problematic in some way. What Yapko then does is to describe for us what *actually* happens in the real world of recovered memory (what the therapist does). Once this description of the real world is established, reality can be invoked without irony, because the reader is now aware of this true and accurate version of the real world – the one in which Loftus’s field experiment is carried out. This “field experiment”, known as the Lost in a Shopping Mall study, is regarded by many fm proponents as a key piece of scientific evidence supporting their position, and is examined in more detail in the next section.

### Lost in a Shopping Mall

The backbone of the ‘scientific’ arguments provided by fm proponents are the numerous studies which indicate the fallibility and malleability of human



memory. For example, references to Neisser and Harsch's (1992) study of the unreliability of flashbulb memories and Loftus's (1992) many experiments showing the vulnerability of memory to post event information are ubiquitous in the fm texts. However, at the time the books in my sample were published, there was virtually no research that spoke directly to the issues at the centre of the debate: Could traumatic memories of childhood be repressed and recovered? Could false memories of childhood trauma be implanted by suggestion?

As a researcher who had been drawn into the debate by her role as an expert witness, Elizabeth Loftus was the first to develop and conduct a study designed specifically to answer one of these questions. In *The Myth of Repressed Memory*, Loftus spends ten pages (pp. 90-100) describing how and why she developed the Lost in a Shopping Mall study. According to her narrative, after she testified for the defence in the George Franklin trial, she was inundated with requests for help and information from parents whose daughters had recovered memories of abuse. She describes feeling overwhelmed by the problem of repressed memories ("I felt insulated and emotionally numb" p. 84), but at the same time they were all she could think about. In this state, she attended the 1991 APA conference "with hopes of gaining some insight into the problem" (p. 84). These hopes were not to be disappointed. At the conference she attended a presentation by George Ganaway (director of a dissociative-disorders clinic at a psychiatric hospital). In his talk, he offered an alternative origin for the satanic ritual abuse memories that were being reported by many MPD patients, suggesting that these were 'pseudomemories' implanted by therapists who were misusing hypnotic techniques. Loftus describes her response to his ideas in the following passage

I was enthralled. Ganaway was stating clearly, forcefully, and without equivocation that therapists can unintentionally plant suggestions that lead to the creation and rapid growth of false memories of abuse. A respected scholar and esteemed psychiatrist was openly and freely admitting that he had seen evidence in his clinical practice of the same processes of memory distortion and implantation that I had observed in my laboratory. He agreed that memories can be distorted, even created, in susceptible minds ... (Loftus & Ketcham, 1994, p. 88)

The exultant quality of this passage, in contrast to Loftus's previous characterisation of her emotional state ("confused absorption", "emotional numbness" p. 84) is evident. The plentiful use of adverbs and adjectives in the first two sentences (clearly, forcefully, without equivocation; respected, esteemed, openly, freely) contributes to the almost 'breathless' tone. This can perhaps be considered a variant of the rhetorical figure known as *synathrismos* (using many nouns or verbs within a sentence), which, according to Cockcroft and Cockcroft (1992), creates an emotional or intellectual effect on the audience through a sense of 'overwhelming accumulation'. As well as conveying this sense of emotional epiphany, this passage also explains the reason behind it. Loftus has finally found an ally – a clinician who believes that memories could be distorted in therapy in much the same way as they can in the laboratory; suggesting that her memory studies *do* have some ecological validity and generalisability to the problem of repressed memories.

This question of generalisability is one which Loftus raises early, and in her account it is the driving force behind the creation of the Lost in a Shopping Mall experiment. Earlier in her book, she spends some time describing 'what scientists know' about memory (as discussed in Chapter 5), concluding:

From these and many other experiments, psychologists specializing in memory distortion conclude that memories are reconstructed using bits of fact and fiction and that false recollections can be induced by expectation and suggestion. But how could we, as scientists, convince anyone beyond our own inner circle that these studies were relevant to the phenomenon of repressed memories and recovered memory therapy? (Loftus & Ketcham, 1994, p. 79).

The problem, according to Loftus, is not *whether* these experiments are generalisable to recovered memory (because those who 'specialise' in this area know they are), but how to *persuade* others that this is the case. This passage throws some light on what Loftus sees as the significance of her experimental creation – not only will its findings be directly applicable to recovered memories, it is also the vehicle through which the findings of previous experimental research on memory can be linked to the debate. Thus, in her eyes, the study is of central significance, and this significance is reinforced by the detailed description of the development of the experiment which she provides. Her

account is a chronological one, which is focussed around problems and solutions. Loftus describes the first problem she faces in the following passage:

I flew back to Seattle with a sense of purpose and direction. Ganaway's clinical observations supplied all the pieces – vulnerable clients, gullible therapists, a credulous society, pervasive fear of sexual abuse. Now all I had to do was figure out how to put these pieces together in a psychological experiment. The major difficulty confronting me was how to pierce to the heart of the problem – the central question about the authenticity of these lost-and-found memories. While I couldn't prove that a particular memory emerging in therapy was false, perhaps I could step around to the other side of the problem. Through careful experimental design and controlled studies, perhaps I could provide a theoretical framework for the creation of false memories, showing that it is possible to create an entire memory for a traumatic event that never happened. (Loftus & Ketcham, 1994, p. 90)

This passage is interesting because it shows Loftus using Ganaway's "clinical observations" to work up the generalisability of her experiment. She characterises the process which she will go through as one of putting together a puzzle – the pieces are provided by Ganaway, while she has the job of assembling them into an experiment. However, what we find in the course of this paragraph is that this will not be as straightforward as could be hoped. Loftus wants to design an experiment that will deal with the central question of the authenticity of memories. For reasons unspecified here (but briefly discussed pp. 55-56), she admits that she has no way of ascertaining the authenticity of any specific memory recovered in a therapeutic setting. So her answer is to "step around to the other side of the problem" – this clever link acts as the pivot around which the paragraph is constructed. It provides the connection which maintains the relationship between her experiment, which will involve starting with a memory known to be false, and trying to implant it ("the creation of false memories"), and Ganaway's "clinical observations". While she will no longer be dealing with 'real' recovered memories, her study is still relevant (and generalisable) because it is simply another way of approaching the problem – the issue is still the same, only the viewpoint has changed. Loftus acknowledges that she cannot prove that a memory is false, but that she hopes to prove that there is a *possibility* that it could be false. However, this would be sufficient to provide a "theoretical framework" for false memory creation, supporting the iatrogenesis argument which is central to the fm position.

The problem of generalisability continues to be an issue as the experiment is developed, with the help of students, friends and colleagues. The difficulty of designing an experiment that is both ecologically valid and ethically permissible is one that initially seems almost insuperable:

As dozens of ideas were generated and then discarded, we realized that we were confronted with several seemingly insurmountable problems. First, the implanted memory had to be at least mildly traumatic, because if we succeeded in injecting a pleasant memory, or even an ambiguously unpleasant one, into our subjects' minds, critics would argue that our findings were not generalizable to recovered memories of sexual abuse.

Second, in order to parallel the therapeutic process, the memory had to be implanted by someone the subject trusted and admired ... But we couldn't be blatantly manipulative and risk endangering the relationship between the subject and "implanter". Nor could we cause our subjects undue emotional stress ... The trick was to design a study powerful enough to prove that it is possible to implant a false memory while also winning the approval of the university's Human Subjects Committee ... I began to wonder if it was even possible to design a false-memory experiment. (Loftus & Ketcham, 1994, p. 91)

This description of the difficulties encountered in attempting to design the experiment, and the competing demands upon the experimenter provide the background against which the eventual experimental design can be assessed. The chosen method will be one which takes all of these factors into consideration, and meets the criteria set out in these paragraphs – it will be generalisable, yet "powerful enough to prove" that false memories can be created.

And this is, indeed, shown to be the case with the Lost in the Shopping Mall study. The initial suggestion for the memory (of being lost) comes from a fellow cognitive psychologist, and is settled upon because Loftus considers that it is traumatic, but not overly distressing (p. 94). The suggestion of the memory by an older family member is achieved successfully when a friend apparently succeeds in convincing his 8-year-old daughter she that she was lost three years earlier ("I couldn't believe what I had just witnessed. In five minutes, with a few suggestions and minor prods from her father, Jenny had accepted a false memory and embellished it with details of her own" p. 95). The actual procedure of the experiment is developed by one of her students. As part of his course work for one of Loftus's classes, Jim Coan successfully convinces his teenage brother Chris that he was lost in a shopping mall as a child (pp. 97-98: another

student also succeeds in convincing her young daughter that she was lost in a housing complex). Coan's procedure involves subjects being presented with written scenarios of childhood experiences, one of which is false, and then being asked to write about their memories of all these experiences once a day over the following five days. A few weeks later, subjects are interviewed and asked to rate the clarity of each event. Subjects are then informed of the deception, and asked to choose which of the events they believe is the false one. This procedure is trialled with two other subjects to make sure that it 'works' on adults, as well as children and adolescents (p. 99), and is finally approved by the university's ethics committee, after some requested modifications to protect the emotional state of participants are made (p. 100).

This is only a brief summary of a process which Loftus spends ten pages describing in great depth. This level of description can, perhaps, be attributed to the fact that the development of the study is part of Loftus's personal narrative, and such narratives tend to be replete with evocative detail (as will be discussed further in Chapter Nine). As already noted, Loftus also clearly views this study as being of central importance to her argument – it is, after all, her hope that a "new theoretical" framework for false memories can be built upon the results produced. Explaining the process of experimental design in depth may assure the reader of the validity of the experimental procedure which is eventually developed. Another factor which may contribute here is that Loftus's narrative, presumably due to the time constraints imposed by her publisher's deadline, ends before the study has actually been run. She has, as we discover, no actual results to report! This lack of results (empirical observations) is potentially problematic for an author who has constructed her credibility so entirely upon her status as a scientist. I suggest that it is this absence of empirical observations (from a sample of sufficient size to provide statistically significant data) which also contributes to Loftus's choice to provide such a full account of the development of this study. The five 'test cases' which she describes are, in fact, the only results which she has to report at the time of writing. By explaining these in detail, she increases their salience and potential impact on the reader. This interpretation is supported by the following passage, where she explains that:

These five cases [summarised above] offered proof – what scientists call existence proof, which is simply proof that something exists or is possible – for the fact that it is possible to create false memories for childhood events. (Loftus & Ketcham, 1994, p. 99)

By introducing the concept of “existence proof” (endorsed by “scientists”), Loftus is arguing that these five cases are logically sufficient to prove her hypothesis. Since her initial aim was to design a study which ‘... show[s] that it is possible to create an entire memory ...’ (p. 90), and she now has the ‘existence proof’ demonstrating that it is indeed possible, the absence of results from a larger sample is, to all intents and purposes, inconsequential. ‘Existence proof’ thus shows itself (in Loftus’s hands) to be an extremely powerful construct, as it appears to be able to turn what could otherwise be described as anecdotal evidence into a scientifically proven hypothesis.

Similarly, this lack of actual results does not prevent Yapko from citing the Lost in a Shopping mall study as evidence in his text. He quotes a long passage from Loftus’s 1993 article describing the implantation of Chris’s false memory of being lost, and then comments that:

Dr Loftus has repeated this experiment with at least five different individuals. While this is not a large enough sample to be considered statistically significant, it is an important experiment that approximates “real life”. ... Those are the “real” world conditions for therapy. Granted, the trauma of being lost is not the same as the trauma of being abused. But, the fact that people can be convinced of something untrue by someone they trust who has no apparent motive to deceive is at the heart of the controversy. (Yapko, 1994, p. 125)

Although he acknowledges the small sample size results in a lack of statistically significant results, like Loftus he still claims that this is an “important” and, more importantly, generalisable experiment. Even though being lost is not the same as being sexually abused, the experimental conditions are close enough to the ‘real world’ conditions of therapy for this experiment to be considered relevant. Like Loftus (Loftus & Ketcham, 1994, p. 90), Yapko claims that this study addresses the “heart” of this debate – which they both claim is centred around the question of whether false memories can be created. This claim, is of course, a rhetorical one which draws upon the power of definitions discussed in the previous chapter. By defining this particular issue as the central question which must be resolved, and then providing the answer in the form of Loftus’s

existence proof, these authors construct the nature of the debate itself in a way which is most advantageous to their (fm) position.

Pendergrast, too, includes the story of Chris's induced memory in his description of the shopping mall study. In his version, Coan becomes Loftus's "research assistant" rather than her student (does this make him a more credible experimenter, perhaps?). Due to the fact that Pendergrast's book is published a few years later, he is able to include the results which were finally obtained after the study had been undertaken. He reports that

Six subjects – 25 percent of the pilot sample, ranging in age from 18 to 53, developed memories of being lost in a mall at the age of five (Pendergrast, 1997, p. 80-81)

These results were published in an article by Loftus and Pickerell in 1995. What is somewhat puzzling is why Pendergrast refers to this data as coming from a "pilot sample", when the authors themselves do not. Perhaps it is the fact that ultimately the Lost in a Shopping Mall Study (which has not been replicated, although other similar studies have been conducted – see Loftus, 2003) was conducted with only 24 participants – not enough to produce statistically significant, generalisable results. Loftus and Pickerell acknowledge this in their article, but still claim that they have provided "existence proof" of false memory creation.

As Pendergrast (1997, p. 91) notes, the main critique of the shopping mall study that is made by rm supporters is that the situation of being lost is in no way comparable to sexual abuse, and therefore the study is irrelevant to the memory debate. Of course, this is the very criticism (lack of generalisability) which Loftus was trying to circumvent in the design of her study. The problem, according to Bass and Davis is that

... being briefly lost is in the realm of common childhood experience. There is no evidence that memories of something foreign, traumatic, and repugnant to us, like sexual abuse, could be similarly implanted. ...

In a letter to Karen Olio about the relevance of this study to survivors of child sexual abuse, Loftus herself wrote, "Being lost in a shopping mall is completely different from being sexually abused. I've never tried to say they were the same." Loftus's research, like the other studies cited as evidence of a "false memory syndrome," have little relevance to traumatic memories of child sexual abuse. (Bass & Davis, 1994, pp. 511-512)

By claiming that even “Loftus herself” acknowledges the dissimilarity between the two situations, the authors add her own authority to their criticism (see Ofshe & Watters, 1994, p. 307, quoted below, for another example of this strategy). This use of a researcher’s self critique as oppositional rhetoric illustrates that utilising ‘organised scepticism’ can have disadvantages. Although it aids in the construction of scientific credibility, it does provide opponents with a potential rhetorical resource. What can be more persuasive than a researcher’s own admission of their study’s deficiencies?

In the final analysis, the generalisability argument places fm supporters in an impossible position. Given the ethical issues that would be inherent in any experiment designed to implant a memory of something that was “foreign, traumatic and repugnant”, it is unlikely that it will ever be possible for fm proponents to perform an experiment that rm supporters would consider to be ecologically valid. Constructing the generalisability of their experimental evidence in a way that is sufficiently persuasive thus proves to be one of the major rhetorical challenges for fm supporters.

#### Williams’ Prospective Survey

In common with their opponents, rm supporters also have a collection of published studies which are regularly drawn upon to support their position. Studies by Briere and Conte (1993), and Herman and Schatzow (1987) are commonly cited as evidence that memory repression and later recovery are normal sequelae of childhood sexual abuse. Both of these studies, however, are critiqued by fm supporters on the grounds that their subjects are self (or therapist) identified abuse survivors – there is no ‘objective’ evidence that the abuse that is remembered ever occurred, therefore no valid conclusions can be drawn from this research (Ofshe & Watters, 1994; Pendergrast, 1997; Wakefield & Underwager, 1994).

The study conducted by Williams avoids both this criticism (and the ethical concerns which are inherent in any attempt to study this issue experimentally)



by surveying women with known histories of childhood abuse. Her methodology and results are described by Terr thus:

The sexual-abuse researcher Linda Meyer Williams surveyed a group of a hundred women, who as girls under twelve had had examinations in the emergency room of a large city hospital because they or their families had reported sexual abuse to the authorities. She found that thirty-eight of the women had no memories of such an incident. Rather than demonstrating reluctance to discuss something personal and perhaps embarrassing, these women seemed completely unable to remember even the emergency-room visit, for which the investigators had records in their hands. This was repression or some other extreme "forgetting" defense in action. (Terr, 1994, p. 53)

In this account, the facticity of the original abuse is actively constructed by the inclusion of a number of verifying factors - unspecified "authorities" are informed of the abuse, examinations are carried out in a "large city hospital", and families are sometimes the abuse reporters. It is important that readers are convinced that the original trauma actually took place, in order for their later lack of memory to be evidence of repression. For Terr, the concreteness of the original trauma provides incontrovertible proof that some form of psychological defense is demonstrated by this study. Not only is the trauma not remembered, but not even the hospital visit is recollected by the affected women – and the reality of the hospital visit is an established historical event which cannot be challenged. The authenticity of this event is constructed by the tactile metaphor that Terr uses, evoking an almost physical image of actual hospital records being grasped by the researchers as they collect their data.

The existence of objective evidence of the trauma is also highlighted by Bass and Davis in their account:

Linda Meyer Williams, research associate professor at the Family Violence Research Laboratory at the University of New Hampshire, studied traumatic amnesia by interviewing 129 women who as children had reported sexual abuse and had been brought to the hospital emergency room for treatment and for the collection of forensic evidence. At the time of the original abuse, the girls and their families were all interviewed and information about the abuse was carefully documented. Seventeen years later, 38 percent, more than one in three, did not remember the abuse or chose not to disclose it. (Bass & Davis, 1994, p. 514)

Here we find "careful documentation" and "forensic evidence" attesting to the reality of the abuse suffered by these women, and providing the objective corroboration that is required by their critics. Bass and Davis, however, are not

as confident as Terr that this study provides incontrovertible proof of repression. The women either “did not remember the abuse or chose not to disclose it”. Here the authors are acknowledging one of the obvious criticisms of Williams’s study (as we shall see below) – that not reporting abuse to an interviewer does not mean it is not remembered. While this seems puzzling, it could perhaps be an example of Bass and Davis utilising the persuasive power of organised scepticism. By showing that they are capable of recognising that a possible alternative interpretation of Williams’s results exists, they are demonstrating their status as sophisticated and fair minded evaluators of scientific research. Then, by including a long footnote in which they explain why Williams believes that repression is the most likely explanation for non-reports (because most of the non-reporting participants disclosed other incidents of abuse, p. 515), they also provide an effective counter to this criticism, which has the potential to neutralise it as an opposing argument.

The fact that Ofshe and Watters, Wakefield and Underwager and Pendergrast all provide detailed critiques of the Williams study (as well as the other rm studies referred to above) attests to how seriously they take these attempts by rm proponents to utilise science as a rhetorical resource. These authors are reluctant to concede that there is any scientific validity at all to the rm position. They do this by claiming that there are only a small number of studies cited by rm supporters, and then systematically critiquing these studies to show their total inadequacy as evidence of repression (see Ofshe & Watters, pp. 305-312; Pendergrast, pp. 63-68; Wakefield & Underwager, pp. 243-256).

It is interesting to note that criticisms of Williams’s study centre around her analysis and interpretation of her data (with a few minor exceptions – Ofshe & Watters note the absence of a control group, .p. 306, while Wakefield & Underwager claim that important details are missing from her method section, p. 248). Little or no attempt is made to challenge the historical reality of the women’s abuse (though see Wakefield & Underwager, p. 248), which is surely a testament to the power of “documentation”. In fact, the design of her study is acknowledged by Wakefield and Underwager (p. 249) as being “an improvement” on other rm research, and Pendergrast (p. 67) similarly notes that

it is the “most credible” of the rm studies. However, what *are* strenuously challenged are the conclusions which Williams draws from her data:

Williams interprets her results to support the contention that a large proportion of sexually abused women are amnesic for the abuse as adults. There are several problems with this interpretation and conclusion. (Wakefield & Underwager, 1994, p. 247)

From her results [Williams] draws a number of broad conclusions supporting the predisposition of recovered memory therapists to hunt for abuse. ... However, if we look at her methods and results, the picture becomes much less clear. (Ofshe & Watters, 1994, pp. 305-306)

Williams, who calls her findings ‘quite astonishing,’ clearly thinks that she has found evidence of repression. ... There are, however, other ways to interpret her data. (Pendergrast, 1997, p. 65)

These authors then go on to offer a number of alternative interpretations of the data, such as: participants who did not report may have been too young to remember anyway (infantile amnesia); they may have simply forgotten the specific incident of abuse, possibly because it was not experienced as traumatic at the time, or perhaps because it had merged with other incidences if the abuse was ongoing; or, they may have remembered the abuse, but chosen not to disclose it to the interviewer. The elegance and utility of this rhetorical strategy is that it does not rely upon identifying problems with the experimental design or execution. In order to be effective, these alternative interpretations need only be provided (not necessarily proven). Simply by showing that other possible explanations for her data exist, Williams’s critics can cast doubt upon her conclusions. As Gross (1990, p. 11) points out, this is because science is essentially rhetorical ; “uninterpreted brute facts” are not what prove or disprove theories, but rather it is these facts “under a certain description” which do this job. This is why it is possible for authors on *both* sides (Terr, 1994, p. 238 and Wakefield & Underwager, 1994, p. 169) to cite Linton’s (1979) autobiographical memory study in support of their respective positions.

The inherent ‘interpretability’ of Williams’s data is demonstrated most audaciously by Ofshe and Watters. For example, they claim that the argument that she uses to support her contention that the target incident (for which the hospital visit occurred) was repressed, rather than non-disclosed (that many of

the participants who didn't report this specific incident did report others) is *actually* an argument against the rm position, because it is "... compelling evidence that these women lacked any special ability to force disturbing events from their consciousness" (p. 306). Furthermore, they argue:

No one questions the fact that almost all experiences – especially those of young children – are eventually forgotten. Looked at in this light, the most dramatic conclusion of the Williams study might be that close to 90 percent of women abused as children know as adults that they were abused, although they may have forgotten one or more specific instances. (Ofshe & Watters, 1994, p. 306)

A simple assertion ("No one questions ...") sets up the radical change in perspective that allows Ofshe and Watters to interpret Williams's findings as proof, not of repression, but of remembering (in spite of the inherent 'forgettability' of experience). While this assertion might be difficult to maintain in the face of counter arguments which would be raised in more interactive argumentative fora, within the pages of a book it can serve its rhetorical purpose relatively free from challenges, unless these are raised by the reader themselves.

In the end, fm supporters agree that the existence of possible alternative interpretations and explanations of her data means that Williams' study is not relevant to the central question of the repression and recovery of memories:

There is not sufficient information to evaluate her assertion that the "vast majority" of the women who failed to report the abuse were amnesic as opposed to simply choosing not to report the abuse to the interviewer. There is no information to support a claim that this study demonstrates repression, dissociation or traumatic amnesia. (Wakefield & Underwager, 1994, p. 249)

Because she does not document the recovery of any forgotten memories, Williams admits, in the end, that her study does not address the "validity of 'recovered' or recalled memories of once-forgotten abuse or the association of such memories with adult symptomatology". Williams proves only that traumatic events suffered by children can sometimes be forgotten. Despite its constant use by recovered memory therapists as proof of their assertions, this flawed study tells us ... nothing about the repression mechanism as it is postulated by many of today's therapists. (Ofshe & Watters, 1994, p. 307)

This 'interpretation' argument serves a similar purpose for fm supporters as the rhetoric of 'generalisability' does for rm supporters – it makes it virtually

impossible for them to provide a convincing scientific argument for their position. Just as fm supporters cannot, for ethical reasons, design a study which would be ecologically valid enough to persuade their opponents, rm supporters are hampered by the 'inaccessibility' of mental processes, which cannot be directly observed but only inferred, leaving open a range of possibilities as to just what a failure to report a memory might actually mean. Therefore, the major rhetorical challenge faced by rm supporters is to find ways to construct their theoretical interpretations (that lack of memory is due to repression rather than forgetting) as the most convincing explanation.

What is apparent from this detailed examination of Williams and Loftus's studies is that the 'best' examples of scientific evidence that are available to rm and fm proponents are still not good enough to persuade their opponents. It seems likely that each new piece of research which appears to support one position or the other will be scrutinised just as closely by those on the opposing 'side' of the debate, and that enough flaws will be found in the design, method, analysis or conclusions of the study to render it unpersuasive to those who are already entrenched in an opposing position.

### Problems with Science

Despite the central importance of science as a warranting resource in the memory debate, the authors in my sample do, at times, construct science as problematic. Of course, upon examination these constructions can be found to have a rhetorical purpose. In general, they are used to explain how it is that other scientists or scientific findings can appear to support an opposing position. These explanations are necessary because of science's status in psychology as the source of 'true' knowledge. As noted in the previous chapter, the scientific method is a potent validator of knowledge claims because knowledge derived from its application is considered to depict reality unmediated by the subjectivity of the knower. So when scientists and/or their findings disagree, these disagreements demand explanation. Clearly, only one position can be the 'correct' one, and that is the position with which the rhetor is aligned. Their task,

therefore, is to problematise the science of their opponents in such a way that it can be seen to not be 'truly' scientific at all.

One of the ways in which this task is achieved is by constructing the knowledge produced by opposition scientists as not necessarily being scientific. For example, Pendergrast notes that

...although some scientists believe in the concepts of massive repression, dissociation, and body memories, there is no good evidence for these concepts. ... Unfortunately, scientists are human beings, too, and they sometimes jump to seductive conclusions, especially if the conclusions fit their hypotheses and place them on the cutting edge ... (Pendergrast, 1997, p. 91)

The explanation as to why some scientists would "believe" in the concept of repression is constructed as being a result of their (unfortunate!) humanity. A true scientist would display the value of disinterestedness, but it is the personal needs of the rm scientists (for status – to be "on the cutting edge") that lead them to "jump to seductive conclusions". The implication is that these scientists *want to believe* in repression, and are willing to 'take a leap of faith', in spite of a lack of evidence, in order to do so. Thus, for rm scientists the "human" desire to be right supersedes the scepticism and disinterest with which they should approach their data, rendering their conclusions dubious.

However, personal interestedness is not the only human failing which scientists may display. According to both Pendergrast and Wakefield and Underwager, scientists are also prone to becoming entrenched in their own views:

Contrary to popular belief, scientists may be more prone to stubborn, dogmatic beliefs than other humans, because they often spend their entire professional lives devoted to a single hypothesis. (Pendergrast, 1997, p. 92)

When scientists believe there is a scientific consensus, many will refuse to accept any research challenging the perceived consensus (West 1992), so this mistaken concept of [recovered memory] therapy may appear to be the scientific consensus when it is not. Blind adherence to outdated or superseded models is one of the principal causes for scientists to resist scientific advances and new research (Barber 1961). (Wakefield & Underwager, 1994, p. 355)

Both of these authors suggest that just because an individual is a scientist, that is no guarantee that they will subscribe to scientific values (in this case,

scepticism). According to Wakefield and Underwager, their decision making is often influenced by factors such as perceived consensus and familiarity. In fact, argues Pendergrast, who has earlier claimed that scientists are 'only human', they may be even more prone to the human failings of stubbornness and dogmatism than other people! Constructing the essential humanity of opposition scientists is a useful strategy, because it is undeniable – all scientists are human. In this way, the failings of rm scientists can be seen to be understandable, if regrettable, failures to live up to scientific ideals. This is a good example of the use of Gilbert and Mulkay's (1984) 'contingent repertoire' (the concept of empiricist and contingent repertoires was discussed in Chapter Three, and will appear again in the next chapter). What is important to note here is that the scientists who are portrayed in this way are always those who subscribe to views which are different from those of the author, never those who produce research which supports the author's own position.

According to both Pendergrast and Ofshe and Watters, one of the factors which allows rm scientists to arrive at their faulty conclusions is the lack of reliable data in the memory debate. Pendergrast (1997, p. 91) quotes Stephen Jay Gould and Peter Huber, who both argue that when 'hard' data is lacking or difficult to obtain, the 'wishful thinking' of scientists can transform the information that is available into support for their preconceived opinions (resulting, according to Huber in 'junk science', a label which is applied, by implication, to rm theory). Ofshe and Watters also draw on Gould's argument, as we see in the following extract:

... If the scientist holds an assumption at the outset of the research, Gould theorizes, and there are enough pieces of data to manipulate or selectively choose among, the conclusions found will often simply mirror the scientist's assumptions, and by extension, reflect the conclusions of whatever political, ideological, or social movement the scientist subscribes to. ... Unfortunately some areas of study don't lend themselves well to the collections of hard data or provable hypothesis [sic] and end up, therefore, more at the will of the social currents than others. ... Our review of the recovered memory literature leads us to the conclusion that the science surrounding recovered memory therapy suffers from precisely these problems with one caveat: the mistakes made in this therapy are not due to the lack of reliable information but are largely the result of reliable information being ignored. (Ofshe & Watters, 1994, pp. 9-10)

Though they seem unclear as to whether the problem is having too much data or not enough, Ofshe and Watters describe Gould's contention that the failings of the data can combine with those of the scientist, to produce 'science' which is merely a manifestation of current social trends, rather than any underlying reality. This, they claim, is what has occurred in the rm movement – except that rm scientists don't lack relevant data, they simply ignore it. This qualification allows them to draw upon the authority of Gould's argument, while still maintaining their position that 'normal' memory research is generalisable and relevant to the rm problem.

Gould's argument is rhetorically useful because it substantiates the role of cultural and social influences on science, and this is an issue which fm authors are keen to highlight in relation to the rm position. As Ofshe and Watters note

... recovered memory therapy illustrates how easily the "science" of psychotherapy changes to confirm our society's current phobias and beliefs. Even the experts of the recovered memory therapy movement acknowledge that the field has been driven by political and social forces. ... But there is now much evidence to show that the line between political advocacy and science has become blurred. (Ofshe & Watters, 1994, p. 9)

The societal trends which affect science are described here as "phobias and beliefs", implying that they are based on unreasoning fears rather than rational considerations, an entirely inappropriate basis for scientific knowledge production. By introducing the idea of "political forces" Ofshe and Watters begin to work up the notion of 'interestedness' – not of individuals, but of particular groups in society – and this is strengthened by their identification of "political advocacy", an activity which is strongly aligned with group interests, as affecting or influencing science to an unacceptable degree (crossing the notional line which should separate them). In a similar vein, Pendergrast warns that

History has repeatedly taught us that scientists are not immune to cultural and social influences. ... We must therefore be wary of accepting the latest 'scientific' hypothesis 'proving' the reality of repressed memory. (Pendergrast, 1997, p. 92)

While scientists, in general, are identified as being susceptible to social pressures, what is clear is that as far as Pendergrast and Ofshe and Watters are concerned, it is only rm scientists and science which are affected. Science which appears to support rm must be greeted with scepticism – but no similar influences or warnings are required in relation to fm researchers or studies.



Despite its obvious asymmetry, this type of rhetorical strategy (applying a general critique only to one's opponents, and ignoring the possibility that it could apply equally to one's own position) is one which is surprisingly common, as we shall see in the next chapter, which explores the use of history as a rhetorical resource in the memory debate.

## Chapter 8: History as a Rhetorical Resource

Although science clearly holds a privileged position as a rhetorical resource in psychology, it is far from being the only one drawn upon by authors in the recovered memory debate. As I have argued elsewhere (Howard & Tuffin, 2002), history is another resource which is regularly utilised by both rm and fm supporters in their arguments. Although critical social psychologists have, for several decades, been drawing attention to the relationship between psychology and history, this relationship still tends to be ignored by mainstream psychologists (Billig, 1987; Gergen, 1997). However, to social constructionist psychologists, a greater understanding of history is important because they see human behaviour (and thus psychological 'knowledge') as epistemologically contingent, a product of its historical and cultural context. Thus, Gergen (1973) believes that psychology's disciplinary detachment from history is both short-sighted and limiting.

This detachment can be seen as developing from psychology's desire to align itself with what are seen to be 'conventional' scientific values (as illustrated in the previous chapter) and philosophies, which are part of what Riggs (1992) describes as the 'standard view' of science. One of the key features of this standard view is an understanding of scientific knowledge as being progressive and cumulative. The scientific method is seen as allowing researchers to make successively closer approximations to objective truth. In this view, the most recent theory or the latest research is considered to portray the most accurate picture of the 'real' world (Gergen, 1997). Old theories are thus discarded and replaced with new ones, and research evidence (apart from a few 'classic' studies) has a rhetorical value which is inversely proportional to the number of years since its publication. As a result, arguments drawn from history are a rarity in the psychological literature.

Given this general lack of retrospection, it is somewhat intriguing to find history coming to the fore in a contested and polarized area of psychological enquiry such as the recovered memory debate. One interesting feature of this complex debate is that opponents deploy historical justifications for their position

alongside more familiar arguments based on contemporary research and theory (MacMartin & Yarmey, 1999). Historians have long recognized that historical events are routinely interpreted and invoked in different ways, depending on the business that the historical account is designed to accomplish (Carr, 1964), and this practice is clearly evident in the memory debate. This chapter provides an analysis of how historical evidence is deployed to strengthen the positions of both opponents and of proponents of recovered memory. The analysis focuses on two highly contested historical 'sites' in this debate: the origins of repression in the late 19th century (and specifically the work of Freud), and the repression of battlefield trauma in major 20<sup>th</sup> century conflicts (such as World Wars I & II, and the Vietnam conflict). These sites were chosen because they were utilized both by rm and fm authors, and my primary interest was in comparing the way these historical events were constructed by the opposing sides. This analysis is complemented by an examination of the accounts given in the texts of the contemporary social context which has nurtured the development of the recovered memory movement, and how these accounts derive their meaning from the various understandings of history that are discussed in the preceding analysis.

### The Historical Origins of Repression

Hacking suggests that the memory debate owes its shape to the "...emergence of a structure of the possible knowledge of memory and forgetting..." between 1875 and 1895 (1994, p. 51). During this period, work on the nature of memory was linked closely to the study of hysteria. Hysteria was the name given to a group of diverse symptoms suffered by women. Typically, this disorder was characterized by a sensation that the uterus was rising up through the body, eventually causing a choking sensation in the throat. A multitude of other symptoms (including convulsions, paralyses, sleep-walking, trance states and amnesia) also prompted diagnoses of hysteria (Webster, 1995). Stories of these early investigations into hysteria are told by several of the authors in my sample. The construction of these accounts differs markedly depending on the

theoretical orientation of the author, as the following descriptions of the work of Emile Charcot clearly show.

In the course of his clinical work, Charcot (a neurologist), came to believe that hysteria was caused by the experience of a traumatic incident or accident. This could cause an 'emotional shock' which would lodge in an unconscious part of the patient's mind, resulting in the appearance of hysterical symptoms (Webster, 1995). According to both Herman (1997) and Pendergrast (1997), Charcot was the first researcher to suggest that the basis of hysteria was psychological rather than physiological. However, their characterisations of Charcot and his work are strikingly different. For example, Herman (1992) introduces him thus:

The patriarch of the study of hysteria was the great French neurologist Jean-Martin Charcot. His kingdom was the Salpêtrière, an ancient, expansive hospital complex which had long been an asylum for the most wretched of the Parisian proletariat: beggars, prostitutes, and the insane. Charcot transformed this neglected facility into a temple of modern science, and the most gifted and ambitious men in the new disciplines of neurology and psychiatry journeyed to Paris to study with the master. ( Herman, 1997, p. 10)

Herman describes Charcot in glowing terms: a "great neurologist", the "patriarch" of a "kingdom" and a "master" of neurology and psychiatry. The overall tone of this description is extremely positive. She constructs Charcot as a social reformer, who takes on the task of raising the living standards of the most vulnerable of society's outcasts. Furthermore, Charcot's academic credibility is established by casting him as the creator of a "temple of modern science", who attracts the most talented students, indicating that his work was widely accepted and highly regarded by his peers .

In contrast, Pendergrast's description presents Charcot in quite a different light:

No history of psychosomatic ailments would be complete without examining the psychiatric circus created by Jean-Martin Charcot at his Salpêtrière, a combination poorhouse, home for the aged, and asylum for insane women. Charcot was a great neurological systemizer who, in the 1870s and 1880s, became fascinated by what he termed 'hysteria'. (1997, p.457)

Here Charcot's creation is initially described as a "psychiatric circus", which immediately calls into question his credibility and alerts the reader to be dubious of both his motives and his conclusions. The circus metaphor paints Charcot as

a showman or charlatan, rather than a scientist. Although Pendergrast describes him as “a great neurological systemizer”, this does little to rehabilitate him in the eyes of the reader. Rather, it suggests that he is stepping outside of the boundaries of his expertise when he becomes “fascinated” by hysteria, a word which indicates a significant degree of emotional involvement. His interest in the area is thus characterised as motivated more by sentiment than objective intellectual curiosity. Charcot is also credited with the invention of the term (‘hysteria’) alerting the reader to the nebulous status of the disorder. Subsequently in his text, Pendergrast negatively critiques Charcot for ignoring the organic “real ailments” of his patients in the process of creating the “mythical disease” of hysteria.

These two short passages illustrate the power of language and metaphor to dis/credit one of the earliest investigators of the relationship between memory and psychological disturbance. The descriptions of Charcot vary in striking ways which depend on the authors’ positioning within the memory debate. Herman’s use of the ‘temple’ metaphor invokes credibility by suggesting that Charcot and his fellow researchers were devoted to the sacredness of scientific truth. Pendergrast’s ‘psychiatric circus’ suggests a focus on theatricality and performance rather than substance. Before they even begin to describe what Charcot *did* with his patients, these authors have presented varying histories which frame any subsequent evaluations of his contribution.

A similar, though more complex, pattern is also observable in description of the work of Sigmund Freud, the most significant historical figure in the memory debate. Supporters of the fm position claim that much (if not all) of Freud’s work was flawed and unreliable. They undermine Freud both professionally and personally, and employ a variety of strategies to achieve this end. On the other hand, proponents of rm must manage the difficult task of discrediting Freud’s later psychoanalytic theories (which suggest that reports of childhood abuse are Oedipal fantasies), while simultaneously maintaining his credibility as the discoverer of the psychological defenses of repression and dissociation.

Hacking claims that although Freud is important because he "... symbolizes the importation of repressed memory or trauma into the western mind...", in the end he proves to be an "encumbrance" to the supporters of rm (1994, p. 41).

However, a careful examination of the texts reveals that responses to Freud among the rm authors are not uniformly negative. Although Blume (1990) dismisses Freud as a liar and betrayer of women, Herman (1997) characterizes Freud as a scientific pioneer whose progressive theories were simply too advanced to find acceptance in his social and political milieu.

These variable constructions of Freud are both achieved using similar interpretative resources. Freud's early work, which posited childhood sexual trauma as the cause of hysteria, is presented as a concrete scientific discovery. Conversely, the later psychoanalytic theories (including the Oedipus complex) are seen as imaginative creations. This distinction is reinforced by the way in which Freud's changing ideas are explained, as a result of subjective pressures rather than an objective re-evaluation of the evidence.

Freud's own publications and letters document the way in which his theories changed over time. 'Studies on Hysteria' (1895/1955), which Freud co-authored with Josef Breuer, presents a theory of hysteria based on the repression of traumatic childhood memories. The book was based on their clinical work with hysterical patients in the 1880s. Subsequently, Freud refined this theory in 'The Aetiology of Hysteria', claiming that hysterics were all repressing similar memories, of 'premature sexual experience' (1896/1962, p. 203) – what is now referred to as child sexual abuse. However, soon after he published this theory (known as 'the seduction hypothesis'), Freud privately repudiated it. In a letter to Wilhelm Fliess in 1897, he explained that he no longer thought that the memories were of actual events, believing instead that they were memories of childhood fantasies (Freud, 1955/1966). This belief formed the basis of Freud's theory of infantile sexuality, which suggested that a normal stage of child development involved fantasising about sexual contact with parents (the Oedipus complex). These 'fantasies' were what his patients repressed, with later harmful effects (Freud, 1905/1953).

### *Freud in rm texts*

For those who support the recovered memory movement, the explanation for this sequence of events is very clear. Freud's initial claims about the origins of hysteria are hailed as pioneering and scientific. Herman (1997) and Bass and Davis (1994) both stress that Freud developed the seduction hypothesis as a result of *listening* to his female patients:

[Freud's] case histories reveal a man possessed of such passionate curiosity that he was willing to overcome his own defensiveness, and willing to listen. What he heard was appalling. Repeatedly his patients told him of sexual assault, abuse, and incest. (Herman, 1997, p. 13)

Early in his career, Sigmund Freud identified child sexual abuse as the cause of much mental and emotional illness in adulthood. By listening to his patients (a revolutionary idea in itself), he learned that many of the women and men he was treating had been sexually traumatized. (Bass & Davis, 1994, p.479)

In both of these extracts Freud's methodology is presented as empirically sound, as it involved simply listening to and recording women's own uncensored reports. At no point is Freud considered to have any pre-existing theoretical framework which may have shaped his interactions with his patients, or his record of their reports. Rather, Blume writes that Freud simply "*acknowledged the presence of incest among most of his early female patients*" (1990, p. 22, my italics). Similarly, he is credited with "discovering" the link between incest and hysteria (Herman, 1997; Bass & Davis, 1994). As Gross notes, the word 'discovery', when used in relation to science, is "... a hidden metaphor that begs the question of the certainty of scientific knowledge. Discovered knowledge is certain because ... it was always there" (1990, p. 7). These descriptions therefore construct the incest as existing independently, quite apart from Freud's theorising about it.

In contrast, Freud is said to have "constructed" (Bass & Davis, 1988, p. 347), "invented" (Blume, 1990, p. 21), or "created" (Herman, 1997, p. 14) his later psychoanalytic theories. The Oedipus complex, for example, is presented as a product of Freud's imagination:

[Freud] disavowed the seduction theory, his original discovery that sexual abuse was at the root of hysteria. In its place he substituted the Oedipal theory, thus turning the reality of

abuse into a child's fantasy. There was no evidence to support his switch; Freud was simply unwilling to believe that so many fathers – possibly including his own – could abuse their children. (Bass & Davis, 1988, p. 347)

Out of the ruins of the traumatic theory of hysteria, Freud created psychoanalysis. The dominant psychological theory of the next century was founded in the denial of women's reality. ... By the first decade of the twentieth century, without ever offering any clinical documentation of false complaints, Freud had concluded that his hysterical patients' accounts of childhood sexual abuse were untrue. (Herman, 1997, p.14).

The argument here is that psychoanalytic theory, unlike the seduction hypothesis, was not developed from empirical observations, and thus it cannot be regarded as scientifically valid. Both excerpts stress that Freud's new ideas were based in a denial, rather than a recognition, of (women's) reality – with the obvious implication that a theory which is not based in reality is seriously flawed.

Recovered memory supporters further challenge the validity of psychoanalytic theory in the explanations which they offer concerning Freud's change of heart. They suggest that this change was the product of several highly personal motivations. Bass and Davis claim that Freud "had reason to believe that incest had taken place in his own family" and that he was unable to face this "painful truth" (1988, p. 347). In addition, Herman (1992) quotes Freud's personal correspondence to show that he was simply unwilling to believe that sexual abuse could be widespread among his own social milieu. Thus Freud's rejection of the seduction hypothesis is attributed to his need to believe in the upstanding moral character of his fellow men, in the face of compelling evidence to the contrary.

Furthermore, Freud's own investment in the success of his theories is made explicit. The seduction hypothesis is represented as being too "radical" and "threatening" (Bass & Davis, 1988, p. 347) for his professional colleagues, who "criticized" and "ridiculed" him (Bass & Davis, 1994, p. 480). The implication is that Freud reworked the seduction hypothesis into psychoanalysis as a way of maintaining his own professional status. Herman (1997) also offers this explanation, but explicitly argues that Freud should not personally be held to



account. She claims that the social and political climate of the day totally precluded the acceptance of the seduction hypothesis. Even if Freud had continued to support his patients' stories, this would have had little or no impact upon society. Thus his "recantation" (Herman, 1992, p. 18) becomes an entirely understandable response to a hostile intellectual environment. It is interesting to note that in her text Herman draws heavily on Freud to support her central thesis, while Bass and Davis do not. Accordingly, it is important to Herman to maintain Freud's overall credibility. This may explain why she rehabilitates Freud's reputation by offering a systemic rather than a personal explanation for his recantation.

The excerpts above show how the variable accounts of Freud presented by rm authors are organized around claims of scientific veridicality. These claims are very similar to those made by the biologists studied by Gilbert and Mulkay (1984). These authors found that scientists tended to talk about their own findings (or the findings of other colleagues with whom they agreed) as simply reflecting the state of affairs in the natural world (the empiricist repertoire). On the other hand, contradictory findings were constructed as resulting from personal and subjective factors related to the experimenter (the contingent repertoire). These repertoires were found to work together almost seamlessly in scientists' talk. Similarly, the rm authors studied here draw upon an empiricist repertoire when discussing the Freudian theories with which they agree, and conversely utilize a contingent repertoire to explain what they consider to be his errors. That is, they suggest that when Freud is 'correct', he is simply presenting an objective and unproblematic picture of the world, untainted by any subjective interpretation. Conversely, his later theories, which tend to contradict the rm position, are seen to be contingent upon Freud's personality and social circumstances.

#### *Freud in fm texts*

Unsurprisingly, authors writing in support of false memory mainly utilise the contingent repertoire when describing the work of Freud. He is identified as being essentially responsible for the widespread belief in recovered memories, as a result of his postulation of repression as a psychological defense

mechanism (Pendergrast, 1997). The methods that Freud initially used to develop this theory are critiqued by fm authors on the grounds that they were unscientific (a direct contradiction of the rm position). Ofshe and Watters (1994) state that Freud was “unconstrained by any substantiated or agreed-on body of data, close observation or demonstrated effects” (p. 294) and thus his theories were “prescientific and based on no reliable empirical evidence” (p.295). According to Pendergrast (1997), Freud’s theories were developed from his work with “a limited patient pool” (p. 60), or “his own rather idiosyncratic self-analysis” (p. 96). As a result, they have “never withstood scientific scrutiny” (p. 60). Furthermore, Wakefield and Underwager (1994, p. 238) point out that Freud himself stated that the weight of clinical evidence for his theories made empirical validation redundant.

This disregard for the scientific method is also evident in Freud’s early work, according to Ofshe and Watters (1994) and Pendergrast (1997). Both provide extended critiques of Freud’s development of the seduction hypothesis (which is ignored by most other fm authors). They argue that, like repression, the seduction hypothesis was flawed because it was based on only a small number of cases. Ofshe and Watters state that the eighteen cases identified by Freud provide “scant evidence” (1994, p. 290), while Pendergrast (1997, p. 462) questions whether there were even as many as eighteen. However, their major criticism of the seduction hypothesis is that it was a product of the suggestive therapeutic techniques employed by Freud. Both quote extensively from Freud’s own writings to show that he “relentlessly forc[ed] his conclusions on clients” (Ofshe and Watters, 1994, p. 291). Indeed, Pendergrast claims: “There is no question . . . that Freud cajoled and bullied [his patients] unmercifully, exhorting them to remember” (1997, p. 466). Freud is seen as imposing his own expectations and assumptions on his patients to the extent that they eventually produced ‘memories’ that supported those assumptions (Ofshe &Watters, 1994, p. 293; Pendergrast, 1997, p. 466). Since Freud had “manipulated his clients’ beliefs about their upbringing to confirm his theories” (Ofshe & Watters, 1994, p. 293), the seduction hypothesis could not be scientifically valid.

For some fm authors, Freud's failings as a scientist are partially explained by the historical context. Pendergrast (1997, p. 459) notes that Freud was "very much a man of his time", while Yapko describes his theories as "outdated" (1994, p. 76). In other words, Freud's work is old-fashioned and can therefore not be expected to be as scientifically valid as more modern thought. Ofshe and Watters (1994) make this more explicit, by discussing the cumulative nature of scientific knowledge (an important component of the 'standard view' of science, as discussed above):

As a discipline becomes scientific, it becomes progressively more difficult to create sweeping grand theories. As knowledge develops through research, all-encompassing theories based on supposition or anecdote tend to be less marketable. . . . As the standard for evaluation of a given hypothesis shifts, demonstrations and data replace demagoguery. (Ofshe & Watters, 1994, p. 297)

This account can be seen as a version of the 'truth will out' device, which depicts "experimental evidence . . . as becoming increasingly clear and conclusive over time and as enabling scientists to recognise, discount, and eventually eliminate the influence of contingent factors" (Gilbert and Mulkay, 1984, pp. 109–10). As Gould (1987) notes, this type of formulation relies on an understanding of the nature of history as being linear and progressive. History is constructed as inevitably moving us forward – the passage of time itself produces more accurate science. Utilizing this understanding of history is an effective tool for scientists to discount the relevance and veridicality of past researchers. Here its effect is to make it seem inevitable that Freud's methodologies and theories will be found wanting by the standards of modern science.

However, historical context alone does not fully explain Freud's deficiencies as a scientist for pro-fm authors. Also emphasised are several personality characteristics which they claim contributed to his failure. For example, Pendergrast highlights what he sees as Freud's credulity, noting that he "believed much of the quackery that had preceded him" (1997, p. 460), including the existence of a "nasal reflex arc" (a neurological connection between the genitals and the nose) and the pernicious psychological effects of masturbation. Pendergrast (1997, p. xxiii) also discusses Freud's early belief in

the curative powers of cocaine and his ongoing use of the drug. Ofshe and Watters (1994) suggest that Freud's cocaine habit may have contributed to his narcissism, which they see as being responsible for his cavalier attitude towards empirical evidence. They claim that Freud was mostly "motivated by his grand conception of his historical importance" and as a result "his patients became a means to an end: that end being the confirmation of his brilliant theories" (Ofshe & Watters, 1994, p. 296). Pendergrast (1997) holds similar beliefs about Freud's character, describing him as vain (p. 461), arrogant (p. 465) and more interested in the confirmation of his own theories than in the welfare of his patients (p. 461).

Both Pendergrast's and Ofshe and Watters' texts deal with Freud in a similar way – they examine in detail his claims about sexual abuse and memory, and then attempt to discredit these findings by highlighting his deficiencies as both a scientist and a man. This strategy requires that the authors be willing to spend time producing a painstaking analysis of Freud's writings, in order to undermine the positive formulations of Freud presented by some of his authors. However, an examination of other pro-Freud texts shows that this is not the only rhetorical strategy that can be used to deal with Freud. For example, Loftus and Ketcham (1994) suggest that Freud's work is essentially irrelevant to the recovered memory question, and that it has been misinterpreted by modern therapists to provide support for their position. In fact, they twice suggest that "... Freud would be turning over in his grave ..." (1994, p. 62 & 254) if he could see what recovered memory therapists had done with his theories. These theories are constructed by Loftus and Ketcham as "speculative musings on the workings of the unconscious mind" (1994, p. 51) and "scholarly speculation about repression" (1994, p. 52). Additionally, Freud's 'speculations' about repression are presented as having little or nothing to do with memory or sexuality, as in the following descriptions:

Freud viewed repression as a defence mechanism that serves to repudiate or suppress emotions, needs, feelings or intentions in order to prevent psychic "pain" (Loftus & Ketcham, 1994, p. 50)

Thus, according to the original Freudian definition, repression can be an intentional, deliberate process of pushing emotions, ideas, or thoughts out of the conscious mind. (Loftus & Ketcham, 1994, p. 51).

In fact, according to Loftus and Ketcham's version of Freud, it seems that almost everything *but* memories (emotions, needs, feelings, intentions, ideas and thoughts) are repressed. Additionally, although repression is described by them as a response to "psychic pain", the specific source of that pain remains unidentified – in spite of the fact that childhood trauma (actual or phantased) as the root cause of adult psychopathology is the major constant of Freud's theorising over the course of his career.

In addition to claiming that recovered memory therapists had misunderstood Freud's theories, Loftus and Ketcham also claim that these therapists have misunderstood his intent:

It seemed to me that what Freud intended as a free-ranging metaphor (the poetic notion of emotions and experiences buried in a secret, inaccessible compartment of the mind) had been captured and literalized. Freud used repression as an allegory, a fanciful story used to illustrate the unknowable and unfathomable reaches of the human mind. We moderns, confused perhaps by the metaphorical comparison and inclined to take things literally, imagined we could hold the unconscious and its contents in our hands.

(Loftus & Ketcham, 1994, p. 52)

In this description, Loftus employs the language of literature ("metaphor", "poetic", "allegory", "story") to underline her contention that Freud never intended repression to be understood as a 'real' psychological mechanism. Repression is not bad science – it was simply never supposed to be scientific at all. The interpretative mistake lies with 'we moderns' who are unable to appreciate this literary subtlety. Loftus's use of 'we' here is clearly a rhetorical strategy, as she is exposing the faults of interpretation rather than committing them. However, her inclusive language highlights that the fault lies with Freud's readers, rather than with Freud himself.

Loftus and Ketcham (1994) choose to discount Freud, rather than trying to discredit him. This discursive move can be seen as a version of the rhetorical strategy known as ontological gerrymandering. Woolgar and Pawluch (1985) originated the term to describe the analytic practice of explaining certain

phenomena as socially constructed, while positioning such explanations as being beyond the boundaries of constructionist analysis. Potter (1996a) expanded this definition to cover the wide variety of boundary-defining strategies that may occur in discourse. Participants engage in ontological gerrymandering by selectively describing the argumentative terrain in such a way that it excludes potentially disadvantageous areas from consideration. Thus, the construction of Freud's work as essentially irrelevant to the matter at hand, means that a convincing critique of this work is no longer required. This contrasts with the approach of Pendergrast (1997) and Ofshe and Watters (1994), who recognize the potential relevance of Freud's work by undermining it. By constructing Freud as both an unreliable and a biased observer, they challenge his credibility as a scientist and therefore his utility as a legitimiser of the modern recovered memory movement.

#### Repression on the Battlefield

Thus far, this analysis of the 19<sup>th</sup> century origins of repression as described in my sample of texts has illustrated clearly the selective nature of these accounts. Authors have two options in dealing with historical events that potentially threaten their positions. If they choose to consider the events, then they must also provide a convincing critique and supporting argument in order to mitigate the events' importance. Alternatively, they can engage in ontological gerrymandering to define these events as irrelevant to their accounts. Both of these strategies are also employed by authors around another historical 'site' – repression of battlefield trauma.

Most of the pro-fm authors exclude the possibility that soldiers in major 20<sup>th</sup> century conflicts (the First and Second World Wars, Vietnam) may have repressed memories of traumatic battlefield events. Ofshe and Watters (1994) and Wakefield and Underwager (1994) omit any reference to the subject, while Loftus and Ketcham (1994) explicitly discount the presence of *any* instances of repression after Freud:

When I began to search through journal articles and textbooks . . . I was confronted with an eerie silence. It was as if repression itself had gone to sleep in the nearly 100 years since Freud first proposed the theory of a defence mechanism that protects the conscious mind from painful feelings and experiences. (1994, p. 49)

Yapko (1994), on the other hand, utilizes quite a different strategy. He begins his book with a vivid vignette, describing the psychological pain and eventual suicide of a Vietnam veteran, apparently suffering from post-traumatic stress disorder. At the end of the story, Yapko reveals that later investigation showed that the man in question had never been in Vietnam; in fact, he had constructed a false memory of battlefield trauma. This is the only reference that Yapko makes to war trauma, but he repeatedly refers back to it to reinforce his argument that recovered memories of sexual abuse are more likely to be constructions than memories of actual events. In this way, Yapko also undermines the possibility that battlefield traumas can be repressed.

Pendergrast (1997) uses similar examples to undermine the validity of repression. For example, he suggests that the “flashbacks” suffered by war veterans are not memories of real events, but are actually “worst fear scenarios” (1997, pp. 120–1). This is backed up by three illustrative examples, one from the Second World War and two from Vietnam. Again, these examples impress upon the reader that soldiers are more likely to construct false memories of war, than to repress real ones. This idea is reinforced later in Pendergrast’s text, where he presents an interview with a therapist who states:

Those who suffer post-traumatic stress disorder, such as Vietnam veterans, never forget what happens to them. Their difficulty is *not* being able to forget. (1997, p. 238)

In the final chapter of his book, Pendergrast offers yet another example of a false memory of battlefield trauma:

During the 1940s, psychiatrists encouraged World War II veterans to ‘abreact’ traumatic memories while under sodium Pentothal [*sic*] or hypnosis. As a result, one soldier acted out the entire battle of Iwo Jima, even though he had never left the United States. (1997, p. 552)

This is utilized as one of several historical examples to illustrate his contention that “Once an idea enters the cultural mainstream, it has a way of resurfacing like a bloated corpse every few years” (1997, p. 552). This rather colourful

simile works to discount historical examples of repression. Where it appears that repression has occurred in different historical contexts, it can be seen as a product of the inevitable “resurfacing” of cultural explanations, rather than as a genuine phenomenon. This understanding of history as cyclical and indiscriminate contrasts with the linear, progressive construction of history discussed above (although it is just as useful in discounting historical instances of repression). Here, the passage of time holds no guarantee of more accurate knowledge. Rather, history is seen as simply recycling ideas which have somehow become part of a culture’s shared understandings, with no consideration of the validity or correctness of these ideas.

In complete contrast, Herman (1992, 1997) claims that the study of trauma and its psychological after-effects (including repression) is periodically suppressed because of its threatening and unpleasant nature. She acknowledges, but specifically refutes, the idea that it is a simple cyclical change: “This intermittent amnesia is not the result of the ordinary changes in fashion that affect any intellectual pursuit” (Herman, 1992, p. 7). Rather, she suggests that without the support of a social context which affirms the reality of the trauma, studies of traumatic repression are not recognized as valid. So, the periodic waxing and waning of interest in trauma and repression result from a collective cultural retreat to avoid the unpleasant realities of abuse and war.

Herman (1997) develops this argument in the first chapter of her text, offering a detailed survey of what she calls the “forgotten history” of psychological trauma. The second section of this history deals with war neuroses. Herman charts the brief revivals of interest in “combat neurosis” (including the use of abreactive techniques) during the First and Second World Wars. However, she contends that it was not until the Vietnam War that the anti-war movement provided the cultural context for recognizing the existence of PTSD and accepting “... psychological trauma as a lasting and inevitable legacy of war” (1997, p. 7).

Herman also explicitly links the symptoms of combat neurosis with hysteria (a product of sexual trauma, according to the seduction hypothesis). For example, she notes that many soldiers in the First World War, “Confined and rendered



helpless, subjected to constant threat of annihilation . . . began to act like hysterical women. . . . They lost their memory and their capacity to feel” (1997, p. 20). And during the Second World War, she credits Kardiner with recognizing that war neuroses were a type of hysteria, and with developing “...the clinical outlines of the traumatic syndrome as it is understood today” (1997, p. 24). Finally she notes that PTSD is “congruent” with the traumatic syndrome that Kardiner had previously developed (1997, p. 28) By stressing these similarities, Herman provides a chain of argument that links 19<sup>th</sup> century hysteria with 20<sup>th</sup> century PTSD (which she sees as the psychological syndrome responsible for recovered memories). In her text, historical instances of repression are proof of the robust reality of the concept.

Other pro-rm authors offer similar constructions. According to Fredrickson, “The understanding of PTSD gained from Vietnam veterans validated and amplified the work we do with sexual abuse survivors” (1992, p. 35). And Bass and Davis claim that:

The fact that people experience amnesia for traumatic events is – or should be – beyond dispute. It has been documented not only in cases of child sexual abuse but among war veterans, battered women, prisoners of war, and others who have suffered severe ongoing trauma. (1994, p. 513)

In this passage repression is presented as a response to trauma in general, rather than childhood sexual trauma in particular, so that historical occurrences of repression serve as a ‘triangulation’ of the concept. This formulation relies on yet another specific understanding of the ‘nature’ of history. Rather than being seen as linear or cyclical, history is a ‘terrain’. Historical events can be understood as objects in a geographical space, which can be triangulated to provide support for the existence of repression.

This ‘spatial’ understanding of history is the only construction deployed by authors which allows historical instances of a phenomenon to be considered as evidence of the ‘real’ existence of that phenomenon. As MacMartin and Yarmey (1999) note, it draws its power from the concept of corroboration. Potter (1996a) discusses how the corroboration of independent witnesses works as a potent device to construct facts as objectively ‘real’. The report of one witness can be

discounted as a mistake or even a lie, and the consensus of a group of witnesses can be critiqued on the grounds that they have collaborated in its construction. However, the concurring reports of independent witnesses are difficult to undermine rhetorically.

In this context, reports of repressed memories from different historical eras can be considered as independent of each other, and thus they provide corroboration of the objective existence of repression. It is clear that pro-rm authors find much support for repression in their selective and general descriptions of the experiences of war veterans. Furthermore, the existence of repression in contexts other than sexual abuse is a powerful argument against claims that all repression is iatrogenically produced by biased therapists working with suggestible women. Pro-fm authors counter this either by choosing to ignore the subject altogether, or by highlighting specific cases where war veterans are described as having constructed false memories of trauma. These examples open up the possibility that other memories (of sexual abuse) may also be confabulated. Both sides rely on ontological gerrymandering to construct historical instances of battlefield trauma as simply and unproblematically supporting their own positions. The static and non-interactive format of these texts no doubt contributes to the ability of the authors to construct their arguments in this way. As Potter (1996) notes, where the possibility of argumentative engagement exists, different rhetorical strategies will be required and employed.

### The Cultural Context of Recovered Memory

As well as drawing upon historical events to legitimate their rhetorical position, authors in the debate also construct the modern social context in ways which support their arguments. In order to do this, the authors draw upon the various understandings of the nature of history which have been identified in the preceding analysis. This linking of the present with the past is an acknowledgement of the continuity of history – that the current social situation will be the historical context of the future. Thus, by constructing the nature of

history in differing ways, authors can use this context to either support or undermine the reality of recovered memories.

Both rm and fm advocates agree that the development of the recovered memory movement was nurtured by changes in the American social and cultural context in the 1970s and 1980s. Where they differ is in the meaning which they ascribe to these changes. The pro-rm authors claim that the changing social climate, bought about by the advances of the women's movement, simply allowed existing abuses to finally be acknowledged. This is illustrated in the following passage from Bass and Davis:

As late as the 1970s many clinicians were still taught that incest was extremely rare, affecting only one in a million children. Considering this history, our present ability to recognize and confront child sexual abuse is nothing short of phenomenal. The advances of the past twenty years are a direct outgrowth of the women's liberation movement that gained force in the 1970s. Women courageously spoke out about rape and battering, wrote books analyzing the ways in which our society condoned such violence, and worked to establish battered women's shelters and rape crisis centres. Simultaneously, a few pioneering clinicians and researchers, both men and women, were beginning to study child sexual abuse and set up models for treatment. (Bass & Davis, 1994, p. 482)

In this account, the current social acceptance of the reality of child abuse is contrasted with a history of denial. The extent of this denial is highlighted by suggesting that unspecified "clinicians" (psychiatrists? psychologists? doctors?) were taught that only "one in a million" children were victims of incest. The vagueness and formulaic nature of this figure (which commonly serves as a metaphor for rarity in everyday language) serves to emphasise the minimisation and ignorance of abuse which occurred in the past, and reinforces the claim of the authors that "our present" approach to child sexual abuse is "nothing short of phenomenal". This huge shift in societal attitudes is attributed to the "women's liberation movement", which allowed women to raise awareness of the wide range of abuses which they suffered, including abuse as children. By stressing the difficulty of changing societal attitudes (the individuals who did so were "courageous" and "pioneering", the change is "phenomenal"), the authors further reinforce the depth of the ignorance and denial surrounding child abuse which characterised past societal attitudes.

This in turn provides an explanation for why it is only now that repressed memories are being uncovered. And this explanation serves as a form of defensive rhetoric against charges by its opponents (examined below) that the relative 'recency' of the recovered memory movement argues against its credibility. By drawing on a linear conception of history, Bass and Davis instead contend that these memories have always existed, but it is only now that society has finally progressed to the point where these accounts can be understood and accepted:

We began to insist that children be protected, survivors be supported, and perpetrators be held responsible for their acts. This monumental advance in our willingness to be aware, to care and to respond has come about only in the past two decades – most visibly in the past decade. This is the first time in history that children and adults who were sexually abused have been listened to, respected and believed. (Bass & Davis, 1994, p. 483)

The authors describe a historical "advance" in "our" attitudes to sexual abuse, inviting the reader to feel virtuous that they belong to this new, caring and supportive social order. They also claim these attitudes as unique to this particular time and place – it is "the first time in history" that sexual abuse survivors have been treated compassionately and respectfully. A linear understanding, in which the progression of history advances knowledge and civilization, works here to provide an argument for the validity of recovered memories.

In addition, we have already seen how Herman (1997) develops an extended argument based on the 'terrain' metaphor, which links the recognition of trauma and its effects (including repression) with social movements. In her account, the study of hysteria was an outgrowth of the anti-clerical movement in nineteenth century France, and the recognition of 'combat neurosis' and PTSD was supported by the anti-war movement of the 1960s. Similarly, she claims that

... the initial work on domestic violence and the sexual abuse of children grew out of the feminist movement. ... The pioneering research on the psychological effects of victimisation was carried out by women who saw themselves as active and committed participants in the movement. As in the case of rape, the psychological investigations of domestic violence and child sexual abuse led to a rediscovery of the syndrome of psychological trauma. (Herman, 1997, p. 32)

Like Bass and Davies, Herman utilises metaphors of exploration, describing the early feminist research as “pioneering”, and resulting in the “rediscovery” of the psychological effects of traumatic experience. So again, what is stressed is that the feminist movement merely provided a societal climate which was more open to seeing and accepting the reality of existing abuses. According to Herman, these abuses include rape and domestic violence, as well as sexual abuse of children. Linking recovered memory (a feature of “the syndrome of psychological trauma”) to this range of other issues, whose prevalence and reality are more firmly established and less open to question, works to legitimise the status of rm ‘by association’. The implication is that memory repression is not just an isolated psychological curiosity, but is a result of the much larger problem of violence against women and children, which society has only recently begun to recognise.

#### *Making concessions?*

It could be argued that the success of this strategy can be measured by the fact that even some of rm’s staunchest opponents concede that the women’s movement did, at least initially, have a positive role in raising awareness of child abuse in the wider community (see Ofshe & Watters, 1984, p. 9). Pendergrast, for example, in a passage remarkably similar to the one which later appeared in the third edition of *The Courage to Heal* (quoted above), notes that:

In the 1970s, during the early days of the women’s movement, the horrifying extent of sexual abuse and incest first began to surface, although children had been subjected to such abuse for all of recorded history. Up until then, official statistics claimed a tiny incidence in the general population. In one ‘definitive’ 1955 study, researchers estimated that there were only 1.1 cases of incest per million persons. Even where incest did occur, it was often minimized or even sanctioned by male psychologists. (Pendergrast, 1997, p. 5)

Here, Pendergrast invokes history as a confirmatory device to establish the reality and prevalence of sexual abuse, with the incidence of incest “for all of recorded history” providing the weight of evidence to support the argument (an example of the ‘geographical’ understanding of history discussed above). Like Bass and Davis, this newfound recognition of the problem of abuse is contrasted with the previous ‘official’ stance on child sexual abuse, which is shown to be ignorant (by the ironisation of the ‘definitive’ statistic) and sexist (it is *male* psychologists who minimise and sanction incest).

This, however, is the extent of the similarity between their positions, for unlike the pro-rm authors who discursively link recovered memories with this wider movement, Pendergrast clearly differentiates them from the stories of 'real' abuse that were finally able to be told:

Very little of this early material about incest mentioned repressed memories, though Freud had made the concept of repression a theoretical given. ... The notion of repressed incest memories had been quietly growing during the 1970s, however. ... Consequently, it is not surprising that a few of the women in the 1983 Bass anthology also talked about having repressed all memory of their abuse. ... [these women's] stories – and their attitudes – signalled a fundamentally different kind of incest memory, wrapped in mystery, horror, rage, and gender politics. They contrasted sharply with the stories of always-remembered incest, which described the molestation within the context of daily life. (Pendergrast, 1997, pp. 7-8)

In this passage, the author argues that there are two different types of incest memories (real and recovered). This allows him to concede the reality of 'always-remembered' incest, while questioning the validity of claims of abuse based upon repressed memories. In classical rhetoric, this strategy is known as *paramologia* – where a minor point is conceded in order to make a larger one. As Megill and McCloskey (1987) note, it indicates to the audience the interlocutor's belief in the strength of their own position: "Even if I concede such-and-such a point to my opponents, my argument wins" (p. 229).

Here, the larger point is made by questioning both the timing and the content of recovered memory accounts. Pendergrast stresses that the first reports of recovered memories of abuse appeared long after the initial recognition of the prevalence of incest, in spite of the fact that theoretical framework of repression was firmly in place at the time. The suggestion that the "notion" of repressed memories took some time (a whole decade, in fact) to "grow" subtly undermines its reality, as the author implies that if repression of abuse memories *really* occurred, this should have been noticed as soon as serious attention began to be paid to the incidence of incest. This argument relies upon a negative use of the 'geographical' metaphor – the fact that the phenomenon of repressed memories was *not* observed during a historical period (when, according to the author, it should have been) is presented as evidence of its unreality. Ofshe and

Watters (1994) make a much more direct use of this argument in the following excerpt:

Considering the powerful and automatic nature of the newly proposed robust repression mechanism, the obvious question becomes: Why are we the first generation to notice that people can repress often-repeated trauma so completely as to have no knowledge that they experienced a life filled with horror and brutality? Does it make sense that although adults and children have experienced trauma throughout the history of the human race, we are the first generation to document the fact that victims can walk away from endless brutalizing experiences with no knowledge that something bad has happened to them? (Ofshe & Watters, 1994, p. 36)

Here the authors utilise the classic device of the rhetorical question or *interrogatio* – the question with an answer so obvious that the author or speaker can rely on their audience to supply it (Cockroft & Cockroft, 1992). According to Hyland, evaluative questions such as the one above serve an important rhetorical purpose, drawing the reader into a “...virtual debate to support the author’s evaluation through the question” (2002, p. 547). He notes that asking questions treats the reader as a knowledgeable and active participant in the debate, but feels that rhetorical questions are manipulative in presupposing the reader’s response. However, if one of the functions of such a question is to engage the reader on an equal footing, then it could be argued that rhetorical questions reinforce this effect by implying a level of confidence that the reader has followed the author’s argument closely enough to be able to supply the ‘right’ answer. Thus, the appeal to the reader to use their own reason to determine whether it “makes sense” that no one else in the entire history of humanity has ever observed this occurring before, is actually an invitation to accept the authors’ previous arguments about repression. For the fact that the correct answer to this question is obviously “no” is due, in large measure, to the way in which these authors have characterised repression. As discussed in Chapter Six, one of Ofshe and Watters’ main rhetorical strategies is the construction of “robust repression”, a version of repression which is so extreme that it cannot possibly be true. Here we see them deploying this definition to make their historical argument work convincingly: it is this particular type of repression (which totally suppresses all knowledge of repeated trauma) which has never been seen before. This allows other historical instances of repression, such as those invoked by Herman as evidence for the historical

continuity of trauma repression, to be ignored. They are not instances of “robust repression”, so therefore they cannot provide evidence for its existence.

### *Psychotherapeutic fads*

As I argued in Chapter Six, pro-fm authors need to provide alternative explanations alongside their negative argumentation strategies. Arguments which question the legitimacy of recovered memory therapies beg the question of its popularity. Why is it so widely accepted if it has no basis in reality? According to some pro-fm authors, one reason is that the feminist movement has created a social climate that is hyper-sensitive to issues of incest and abuse. When combined with other features of the modern social context such as a generalised public paranoia (Wakefield and Underwager, 1994), societal stress and family fragmentation (Yapko, 1994; Pendergrast, 1997), and a sensationalising media (Yapko, 1994; Pendergrast, 1997) this provides one explanation for the both the development and subsequent popularity of recovered memory therapy. This is achieved by constructing the psychotherapeutic enterprise as being particularly sensitive to social influences, as was discussed at the end of the previous chapter. One of the ways this is achieved is via a historical survey, such as the one provided by Yapko in the following passage:

At any given time, there is an “in” diagnosis and a “revolutionary new approach” to therapy, which are often greeted enthusiastically by the profession but with little of the objectivity necessary to evaluate accuracy and effectiveness. Half a century ago, orthodox psychoanalysis was the mainstream approach. Techniques like “free association” and “dream analysis” were typical in treatment. Then came behaviorism and its more mechanistic approaches. The 1960s gave rise to humanism, and on the wave of the drug craze came “LSD therapy” and encounter groups. More recently, the “adult children of alcoholics” have been in the limelight, and there has been an enormous surge of recovery groups for these and other “adult children” harmed by parents. Diagnoses and treatments are often products of the era in which they arise. (Yapko, 1994, p. 43)

Here, the author characterises psychotherapy as constantly in flux, with new treatments appearing and being adopted on a regular basis. To substantiate this claim, he traces this change over a period of fifty years, listing a variety of treatment modalities (which, interestingly, appear to become increasingly unorthodox as time goes by). This particular argument relies upon the cyclic



view of history discussed above. Treatments come and go, old ones are discarded, and new ones adopted, but there is no suggestion that progress is being made. Indeed, Yapko specifically notes that “accuracy” and “effectiveness” have little to do with the development and popularity of these therapeutic techniques. Instead, they reflect whatever cultural preoccupations are current at the time – they are “products of the era in which they arise”.

Ofshe and Watters also draw on this cyclic construction in their text:

On another level, this book is not about the current fad in the psychotherapy community, but what that fad represents. While the results of the mistakes made in recovered memory therapy are so egregious they can no longer be ignored, the mistakes themselves are not new but have been made, to varying degrees, by many talk therapies both past and present. ... While mental-health professionals look with horror at the seemingly barbaric mistakes of their predecessors, each generation appears unable to stop the birth of new damaging practices. Of the lessons learned from the wide use of prefrontal lobotomy only two generations ago, University of Michigan professor Elliot Valenstein writes in *Great and Desperate Cures* that fad treatments continue to be the “very bone and marrow” of the profession. (Ofshe & Watters, 1994, p. 8)

By labelling recovered memory a “fad” they signal both its frivolity and its transient status. However, there is also a continuity signalled here – a continuity of error. The “mistakes” of recovered memory therapy are not new mistakes, but ones which have been repeated over and over. The reason for this cyclic repetition is, according to the authors, because the psychotherapeutic community is unable to learn from its own history, and is thus doomed to repeat it, rather than progressing to more effective and beneficial forms of treatment. Lobotomies seem to be far removed from the “talk therapies” with which Ofshe and Watters are apparently concerned, yet they make an appearance here to reinforce their argument that “the profession” is susceptible to adopting even the most harmful of treatments, seemingly unconcerned about their effect on the people who they are supposed to be helping. By constructing recovered memory therapy as only the most recent example of therapeutic gullibility and incompetence, the authors offer readers an alternative way to understand how and why it has become accepted in the psychotherapeutic community.

## Historical Events and History Itself

The analysis in this chapter suggests that in the memory debate, historical events are employed as rhetorical resources, which are carefully selected and worked up to support the writer's position. Clearly this is not a new insight: The way in which historical events can be selectively described in order to support a particular rhetorical position was noted by Butterfield as long ago as 1931. Nonetheless, it is useful to examine the means by which this selection is discursively accomplished. What is particularly interesting in this analysis is how the selected historical events acquire their particular meaning from the author's constructions of the nature of history itself. It is this that allows the same events to become evidence both for and against the existence of recovered memory. The nature of history is constructed in the sample texts in at least three different ways: as linear, cyclical and spatial. These understandings are so common in the western intellectual tradition that they have become virtual clichés (Gould, 1987). What I have attempted to highlight here are the differing assumptions about knowledge, truth and reality that underlie these constructions. It is these contradictory ontological and epistemological assumptions that make 'the nature of history' a useful argumentative resource.

When history is understood as linear in nature, it is assumed that, over time, the special characteristics of the scientific method will produce successively closer approximations of objective reality. This being the case, past ideas and theories will necessarily be imperfect, while those of the present will be more reliable and complete. Therefore, it is logical for all rational thinkers to interpret the events of the past in light of 'what we now know to be true'. Historians may long ago have abandoned this kind of interpretation as being inappropriate (in that history itself does not reveal truth), but it is clearly an understanding that still has currency in the social sciences. In this debate the linear understanding is particularly useful for providing a basis for undermining the relevance of the knowledge claims of historical predecessors (where they contradict an author's position). Indeed, it can provide the basis for an entire text, as in Wakefield and Underwager's (1994) *Return of the Furies*. Their central thesis is that

When Athens . . . changed from a matriarchal tribal society to patriarchy and constructed the first free capitalist democracy, the linear development from that time to this has produced Western civilisation. It is fact that this Western civilization is better than many others and may, indeed, be the best' (Wakefield & Underwager, 1994, p. 20)

It is this 'best of all possible worlds' that is under attack by the "Furies" – the emotional, irrational supporters of recovered memory.

An effective counter to this discursive move is to conceptualize historical events as providing corroborative evidence for the objective existence of a phenomenon. Although phenomena recur, this is not seen as a cyclic process. Instead, I have suggested a 'terrain' metaphor mirrors this understanding, with historical events working as locations in geographical space. Through a process of 'triangulation' events occurring in disparate times and places can provide confirmation of the objective reality of a phenomenon. Like the linear understanding, the spatial metaphor also allows history to bring us to the truth. And again, this understanding can provide the underpinnings of an entire argument, as my interpretation of Herman's (1997) text demonstrates. She uses historical instances of traumatic events, and responses to them, to validate the reality of memory repression. Not only do individuals repress disturbing events which happen to them, but so does society, providing an explanation for the sporadic attention that the subject has received. It is important to note, however, that understandings of history are not necessarily tied to specific argumentative positions. This spatial understanding of history works just as well for Yapko (1994), who uses it to demonstrate the existence of confabulated memories.

The third understanding of history that I have discussed in this chapter is the only one that does not conflate history and truth. On the contrary, if history is viewed as merely recycling ideas through some unspecified process, then reports of the existence of a phenomenon in different historical periods have no confirmatory power. This cyclical understanding of history also contradicts the simple linear metaphor. Rather than bringing us progressively closer to truth, history merely allows cultural ideas (regardless of their veracity) periodically to become important, and then to diminish. Although this understanding serves a useful rhetorical purpose (undermining arguments

utilizing the geographical metaphor), it is not so satisfactory as the basis for an entire argument. While Pendergrast dismisses interest in repression as a product of “cultural fashion” (1997, p. 552), he maintains a privileged position for the existence of false memories as real phenomena (supported by scientific evidence). So, historical evidence of confabulation, by Vietnam War veterans and others, is elsewhere (1997, p. 129) cited to support his argument. This further illustrates the flexible nature of historical events as rhetorical resources. It appears that different (and possibly contradictory) constructions of history can be deployed by authors in the same text without sacrificing argumentative coherence. This ability of rhetors to flexibly construct and deploy evidence was also highlighted in the previous chapter, where the ‘interpretability’ of scientific research was discussed. Therefore, it is not surprising to find it also emerging as a theme in the next (and final) chapter of my analysis, where I discuss the use of personal experiences as a rhetorical resource.

## Chapter 9: Personal Experience as a Rhetorical Resource

While science and history are argumentative resources which are extensively drawn upon in popular texts, the most obvious and arresting feature of these texts is their rhetorical utilisation of descriptions, narratives and accounts of individuals' experiences. Presentation of experiential narratives, usually in the form of cases studies, is a commonplace in the self-help genre to which these texts loosely belong (Coyle & Grodin, 1993; Plummer, 1995). As a result, proponents of both rm and fms exploit the opportunity (and perhaps, to some extent, expectation) this provides to recount, often in explicit detail, the experiences of those directly involved in the events at the heart of the debate. In the telling of these accounts a variety of standpoints are taken up – sometimes experiences are recounted in the first person, from the perspective of one of the individuals involved, while at other times the author of a text acts as an omniscient narrator, with access not only to the events of interest, but also the thoughts and feelings of the protagonist(s). Often the authors' own personal experiences as individuals or professionals are also described (see Plummer, 1995).

My analysis suggests that these experiential accounts serve a number of different persuasive purposes in the texts. I begin by making some brief comments on how descriptions of personal experience can 'work' to create credible authorial warrants (*ethos*), referring to and expanding upon the analysis presented in Chapter Five. I then explore how these accounts may be constructed with persuasive intent to recruit the emotions of the reader (*pathos*). I conclude with a consideration of the ways in which fm authors manage the possible conflicts inherent in their utilisation of experiential accounts despite their commitment to scientific values which proscribe drawing any conclusions from such 'anecdotal evidence'.

### Experience and *Ethos*

In Chapter Five, I discussed some of the ways in which narratives of personal experience could be used to construct a credible authorial ethos by establishing

membership of particular categories with privileged knowledge status in relation to the memory debate. Using examples from *The Courage to Heal* and *Victims of Memory*, I also explained how being accepted as a member of a category such as 'survivor', while conferring a certain 'expert' status on the one hand, also carried with it the potential for attributions of stake and interest which might be used by opponents to undermine authorial credibility (perhaps requiring some 'repair' work if the text is successful enough to run to a second edition). The fact that I am returning to this theme here illustrates the way in which the rhetorical resources and strategies identified in this analysis work together in an interconnected, layered and flexible fashion to produce their persuasive effects. I am arguing that, while one of the ways in which the resource of authorial credibility can be produced is through the use of personal narratives, such narratives are also a resource in their own right with other rhetorical functions and effects.

For now, I wish to focus upon the role of narratives of experience in constructing authorial ethos. While accounts of the author's individual experiences (such as those presented by Davis and Pendergrast) provide opportunities for establishing credibility as a by-product of category membership, there is another means by which authors can use experience to enhance their ethos. This is through narratives in which the author's experience intersects with that of people directly affected by rm/fms. Many of the authors in my sample are key figures in the memory debate, and as such they have the opportunity to interact with 'survivors' (of all kinds) in their professional lives as therapists, researchers, consultants and expert witnesses. By recounting these interactions authors have the opportunity to reinforce their credibility and expert status by 'demonstration', as it were, rather than by mere claims making.

This is well illustrated in Terr's text, which is structured as a series of narratives, in which the author features in a variety of roles. The first story which Terr tells is that of Eileen Franklin Lipsker, whose recovered memories eventually led to her father's conviction for the murder of her childhood friend, Susan Nasson (although he was released six years later, upon appeal). Terr appeared as an expert witness for the prosecution, presenting evidence about

memory processes, repression and trauma, although she was not allowed to offer any opinion about the reliability of Eileen Franklin Lipsker's memories. In the first chapter of her book Terr tells Eileen's story, and in the second chapter she tells the story of her involvement in the case. This account of her involvement in this 'real life' story contributes to her construction of a persuasive authorial ethos, as the following analysis demonstrates.

In Chapter Five I argued that establishment of category memberships was a useful strategy for developing the 'intelligence' component of the authorial ethos, as membership often confers 'knowledgability'. However, the usefulness of this strategy is counterbalanced by dilemmas of stake and interest that often accompany memberships. For example, stressing category membership as a survivor may raise questions about your personal motives as an author. This type of dilemma also inheres in other category memberships. For instance, Dyer and Keller-Cohen (2000) have shown how university lecturers use personal narratives to construct their professional expertise, but also simultaneously attempt to downplay their expert status. They draw on the concept of the 'hunched-shouldered authority' (Billig et al., 1988) as a possible explanation for this; that is, the dilemma of being an expert (with its implied superiority) in a democratic society premised upon the fundamental equality of its members. This explanation meshes well with rhetorical considerations, where identifying yourself as an expert, while establishing your intellectual authority, seems to carry with it the possibility of undesirable attributions of arrogance and condescension.

Terr appears to be particularly sensitive to this possible dilemma, and orients her account accordingly. For example, in Terr's narrative of the Franklin case, her status as an expert is conferred by someone else, rather than claimed herself:

"First and foremost," Elaine [Tipton, the prosecutor] told me after the trial was over, "in the course of jury selection I was going to have to find twelve people who were willing, able, and ready to accept the concept of repressed memory. ... It was clear to me that I was going to have to present powerful psychiatric expert testimony regarding memory in order to educate the jury. And that's where you came in. ..." (Terr, 1994, p. 24)

In this passage, Terr's expert status is articulated by another – the lawyer who requests her testimony in the trial. By using Elaine's own 'voice', Terr reinforces her 'chosen' rather than 'assumed' status, and further distances herself from any possible negative attributions which might be made if she had claimed to be a "powerful expert" herself. This is reiterated several pages later, where Terr re-emphasises that her involvement was by invitation, even mirroring the turn of phrase earlier attributed to Elaine:

This is where I come into the Franklin story. Elaine Tipton needed an expert witness to explain "repression" and the "return of the repressed". She needed someone who understood the mistakes children make at horrible moments in their lives. She particularly wanted a child psychiatrist. She also wanted a researcher.

Repressed memory had caught my interest by then, and I was studying the adult manifestations of childhood trauma. ... But there were reasons not to testify, too. This case was bound to create controversy and publicity, two factors potentially detrimental to me and to psychiatry. In retrospect, I think what persuaded me to join *People v. Franklin* was the sound of Elaine Tipton's voice. She was totally committed to her eyewitness. Her obvious admiration, as she told me of the risks Eileen Lipsker had taken in coming forward to testify, convinced me to go ahead.

So I said "Yes" to Elaine Tipton. And I went home and told my husband that I must be crazy. (Terr, 1994, pp. 30-31)

In the first four lines of this passage, Terr's expert status is again constructed as something which is recognised by others, but is essentially outside of, and perhaps even incidental to, her own conception of her professional identity. She has earlier explained to the reader (pp. 7-8) that she is a child psychiatrist, a researcher, and someone who studies "... what happens to children who go through terrible events." However, she only becomes an 'expert' because her knowledge and experience fit with what *Tipton* wants and needs.

Terr further distances herself from her expert status by communicating her reluctance to become involved in the trial at all. While acknowledging that she does have an academic interest in the area, she is not enthusiastic about becoming involved in the trial. Her misgivings are related not just to her personal well-being, but to the potential damage that might be done to her entire professional field, indicating a commendable level of collegial concern. Terr depicts herself as being convinced to take part by Tipton, in spite of her better judgement. Finally, the humorous self mockery of the closing sentence



("I must be crazy") both reinforces her reluctance and reassures the reader that the author does not 'take herself too seriously'.

Dyer and Keller-Cohen suggest that this type of self mockery allows their university lecturers to achieve "...the delicate balance necessary for social acceptance between self-aggrandizement and self-effacement." (2000, p. 297), and this strategy seems to serve the same function for Terr. A further strategy which is oriented to this concern is the author's frequent presentation of herself as being in need of, or receiving, help from others. For instance, in her prologue she describes how she is "helped" to understand her first case of childhood trauma ("a challenge I might or might not be able to meet", p. xi) by the publication of research on the 'battered-child' syndrome, and how she is "helped" to understand the adult effects of childhood trauma by letters written to her in response to her first book (p. xiii). In relation to the Franklin case, she stresses her need for assistance in preparing to testify, given the constraints imposed by the judge:

I have appeared as an expert witness a couple of dozen times, but the Franklin case was the first in which I was asked to testify entirely in hypotheticals. I found the prospect disconcerting, and I wanted a little rehearsal myself. (Terr, 1994, p. 47)

... Elaine came over to Stanford to prepare me. ... the prosecutor was theoretically on holiday. Luckily for me, she was willing to dispense with her day off. I needed the help. (Terr, 1994, p. 32)

Here Terr purposefully underplays her previous experience with the casual expression "a couple of dozen times" – the vagueness of the phrase suggests that the author doesn't feel it is 'worth' providing a more accurate figure, while avoiding the impression of self-importance that may have been created by using an alternative such as 'many' or 'numerous'. What is highlighted is her unease in relation to this specific trial, her lack of expertise at being *this particular kind* of expert witness and her ability to recognise her shortcomings in this regard and receive help and instruction from someone else (see also her account of her own memory lapse while testifying, p.49). This all works to construct Terr as a rather unassuming expert, thus achieving the 'delicate balance' referred to by Dyer and Keller-Cohen (2000).

Perhaps the most interesting feature of Terr's text in regard to the construction of her expert status is the way in which she makes this rhetorical dilemma totally explicit for the reader:

A person must qualify before the court each time he or she appears as an expert witness. What this means is that you are required to discuss your credentials in an almost shameless fashion the moment you take the stand. You need to establish before the judge and jury your training and experience in the area in which your opinion is being asked. ... While qualifying, the expert must simultaneously make friends with the jury. First impressions are often lasting, yet a witness must invariably spout so many academic degrees, licenses, board certifications, and years in practice that potential friends may be put off. Qualifying as an expert makes it almost impossible to get a decent start with twelve of your peers.

Elaine and I did the nasty deed as efficiently as we could, and then we got down to the important issue of the day, memory. (Terr, 1994, p.48)

Terr is obviously cognisant of the dilemma she faces, recognising that constructing one's expert status makes it difficult to "get a decent start" with your "peers" (the use of this word highlights the dilemma of expertise versus egalitarianism which she faces). She obviously does not believe that admitting she is concerned with winning the jury over will damage her credibility in the eyes of the reader (who she is presumably also wanting to get 'on side'). This is perhaps because of the way she constructs the relationship that she wishes to have with the jury. It is not one of influence, but of camaraderie – she simply wants to "make friends" with them. In Terr's account, the courtroom situation *forces* her to "spout" her qualifications and experience "in an almost shameless fashion", suggesting that her own preference would be to avoid this "nasty deed". Her concern is that this obligation will make it difficult for her to get a fair hearing from the jury. They will get the wrong "first impression" of her (as superior and self-important), which will bias their reception of the facts about memory which she has to share with them. But what is the purpose of revealing this to the reader, who presumably stands in much the same relation to her as an author, as the jury did to her as an expert witness? Terr must believe that she has done enough work here (and previously) to establish her humility, so that the reader will sympathise with the dilemma she outlines, rather than wondering how she has attempted to "make friends" with them. This seems a risky strategy, but the payoff is that it allows Terr to communicate to the reader her possession of the "degrees, licenses, board certifications, and years in

practice” which make her opinion a credible one, while simultaneously showing her unease with a process which requires her to make these claims explicit, further reinforcing the reader’s impression of her as a modest and approachable expert.

### Experience and Emotion

Clearly, accounts of experience play an important role in the construction of authorial ethos. However, this is only one of the persuasive functions performed by personal narratives. As well as recounting their own individual experiences (or their experiences with survivors), authors also recount stories in which they have had no direct involvement. The presence of so many detailed narratives of survivors’ experience in both the rm and fm texts suggests that these authors consider them to be a very important rhetorical resource. I propose that this is because of the role which these accounts play in recruiting the reader’s emotions for persuasive effect.

Considering how discourse may be designed to evoke feelings for the reader (or audience) is quite a different approach to emotion to that which is usually found in discourse analytic studies. From a discourse analysis perspective, emotions are generally studied with regard to their functionality in achieving various kinds of interactional business (such as the management of accountability) for conversational participants (Edwards, 1997). That is, the focus tends to be on what emotion talk *does* for people when they deploy it. In contrast, the rhetorical analyst is concerned with how talk might be designed to make other people (the audience or reader) *feel* emotion. This is because, in the rhetorical tradition, appealing to emotion has always been considered a central route to persuasion – if the audience can be made to feel a certain way about an issue, then they can be persuaded to think and act in line with those feelings (Covino & Joliffe, 1995).

Arousing the emotions of the audience requires the rhetor to construct their discourse particularly skilfully and subtly. As Corbett (1990) points out, if you tell your audience that you intend to arouse their emotions, you may destroy much

of the effectiveness of the appeal. This is because, in order for this strategy to be successful, the emotions aroused must feel spontaneous and authentic to the individual: The power of the emotional appeal lies in the way it allows the reader to directly *experience* feelings as they read the text. Direct personal experience of a phenomenon is generally a very effective persuader. Even professional scientists (who are usually highly sceptical of 'superstitious' beliefs) will acknowledge the possible existence of entities such as alien life forms and ghosts on the basis of their own personal experiences (Coll & Taylor, 2004). While the author of an rm or fm text cannot create such an experience directly for the reader, they can attempt to make them feel sympathetic emotions for survivors or accused parents, and thus to identify with their plight.

So how can emotion be evoked in the reader? Corbett (1990, p. 88) tells us that "we arouse emotion by contemplating the object that stirs the emotion". In the context of the memory debate, this 'object' is the suffering and pain of survivors (rm) or accused parents (fm). Therefore, in order to evoke emotion most effectively, the author must construct the situation of these individuals as vividly as possible. Corbett suggests that this can be achieved through the use of specific, sensory detail to create a 'word-painting' of the scene. This is a concept which also appears in discourse analysis. In his discussion of techniques of fact construction, Potter (1996a, p. 150) suggests that the use of detail and narrative in discourse work to "paint a scene as it might have been observed ... Indeed, one way of thinking about such descriptions is as providing an impression of remote sensing; they pull the recipient into the scene ...". In Potter's theoretical framework the function of this is to create the "out-there-ness" (independent existence) of the scene. Rhetorical theorists argue that this technique also results in the actualisation of emotions, as recipients of these descriptions react and respond to the scene described with such "graphic vividness" (Cockcroft & Cockcroft, 1992).

I will confess that initially I felt uncomfortable embracing this rhetorical understanding of emotional appeals. Talking about 'evoking emotions' in the reader did not seem to sit well with my grounding in discourse analysis methodology, which cautions against going beyond the text to make

assumptions about what is occurring 'under the skull'. Since direct reference to emotions was not necessarily evident in these narratives (to avoid the dilemma outlined by Corbett, 1990, discussed above), could I really argue that the arousal of emotion was one of the functions of such stories? Eventually, however, I was persuaded by my own reading of the texts; that is, by my own personal experience! I could not discount my own response to these narratives. As I have already acknowledged (see Chapter One), I found reading both the rm and fm texts moving and, at times, difficult because of the emotional impact that these stories had on me. What is more, after some contemplation, I was struck by the way the rhetorical perspective actually reinforces the social constructionist understanding of emotions. My earlier discomfort had been caused by an error on my part, in thinking of emotions as something which would occur 'inside' the reader, rather than as interpersonal accomplishments achieved through the deployment of linguistic resources (Howard, Tuffin & Stephens, 2000). In this case, I did not personally know the people whose experiences were described in these texts, yet I responded with sympathetic emotion to their stories. This indicates to me that my feelings were aroused purely by the adroitly constructed discourse of the authors, thus demonstrating the power of language to create the reality of an experience powerfully enough to involve me emotionally.

An excellent example of an experiential narrative with features which evoke emotion can be found in the opening chapter of Terr's text, in which she recounts the return of Eileen Franklin's memory of Susan Nason's murder. The chapter is entitled "Ringside Seats at an Old Murder", which clearly signals to the reader that they will be spectators gaining a 'close up' view of the action taking place. Terr begins the narrative by doing some scene setting. The techniques which she uses to do this were familiar to classical rhetoricians, as discussed in Cockcroft and Cockcroft (1992) and include: *topographia* - describing the location ("The family room of the Lipsker's home in Canoga Park, California"); *chronographia* - describing the time of year and day ("One unusually smog-free January afternoon"); *prosographia* - describing the participants (Eileen, her daughter Jessica and two of her friends, plus baby Aaron); and *pragmatographia*, describing the activities taking place (the girls

drawing, Eileen feeding the baby). Terr even describes the background music playing at the time (“folksy children’s songs”). In this sunny, warm, music-filled room, we are told “Eileen’s mind begins to drift” (Terr then quotes Eileen to reinforce this description of her mental state “... I was spaced out” – this technique is known as *dialogismos*). All of the detail provided in this preliminary description works to develop for the reader the impression of ‘remote sensing’ discussed by Potter (1996a). The promise of having a ‘ringside seat’ is being fulfilled. Having set the scene, Terr goes on to depict the pivotal moment of remembering, which I quote here in full:

Reddish blond strands of fine, little-girl hair brightened in the sun. Jessica twisted her head to look at her mother. To ask something? Her chin pointed up in inquiry. She looked up and over her shoulder. Her eyes brightened. How odd! The young girl’s body remained stationary, while her head pivoted around and up. Now. Now. Mother’s and child’s eyes met. The girl’s eyes so clear, so blue.

And at exactly that moment Eileen Lipsker remembered something. She remembered it as a picture. She could see her redheaded friend Susan Nason looking up, twisting her head, and trying to catch her eye. Eileen, eight years old, stood outdoors, on a spot a little above the place where her best friend was sitting. It was 1969, twenty years earlier. The sun was beaming directly into Susan’s eyes. And Eileen could see that Susan was afraid. Terrified. The blue of Susan Nason’s eyes was the bluest, clearest blue Eileen had ever seen. Suddenly Eileen felt something move to one side. She looked away from those arresting eyes and saw the silhouette of her father. Both of George Franklin’s hands were raised high above his head. He was gripping a rock. He steadied himself to bring it down. His target was Susan.

“No!” Eileen felt a scream welling up in her throat. But not a sound disturbed the family room that January day in 1989. The shout stayed trapped in her mind. “A chill ran up me” she told me, months later. “It was so, so intense. And I said to myself a very clear, very loud ‘No’, as if I had the power to stop the memory from coming.”

But Eileen Franklin Lipsker’s buried memory, once it started rising to the surface, could not be stopped. She remembered the feeling of a scream invading her throat. She remembered Susan, just four days short of her ninth birthday, sensing George Franklin’s attack and putting up her right hand to stave him off. *Thwack!* Eileen could hear the sound, a sound like a baseball bat swatting an egg – the worst sound of her life. “No!” she yelled inside her head. “I have to make this memory stop.” Another *thwack*. And then quiet. Blood. Blood everywhere on Susan’s head. “Something whitish” along with the blood. And “some hair that was no longer attached to her body”. Blood covered Susan’s face. Her hand was smashed. (Terr, 1994, pp. 2-3)

I suggest that there are a number of discursive features in this passage which work to produce an emotional effect on the reader. One important technique is the recurring repetition of words and phrases. In the first paragraph, we also see a repetition of ideas. Jessica's body position is described in no less than four different ways – she “twisted her head up”, “her chin pointed up”, “she looked up and over her shoulder”, her “body remained stationary, while her head pivoted around and up”. Interspersed with the other elements of Terr's description, this continual return to the same single action seems to create a sense of suspended animation – a long moment of expectation reinforced next by the repetition of the word “Now”. The reader is waiting for something to happen - and the thing that happens to resolve the tension turns out to be the return of Eileen's repressed memory: “And at exactly that moment Eileen Lipsker remembered something”.

While repetition does play a role in the development of suspense, it also has other functions related to the evocation of emotion. Cockcroft and Cockcroft (1992, p. 131) suggest that repetition works as a rhetorical technique because it has “...(the) closest affinity to the spontaneous expression of emotion”. Thus, when Terr has Eileen describe her response to her returning memory, she tells us that it was “so, so intense”. Repetition can also contribute to the graphic actualisation of a scene, and thus heightening its likely emotional impact. Thus, as the reader observes the murder of Susan Nason through Eileen's senses we have the twice repeated sound of her head being smashed by the rock, which reinforces the feeling that the reader is viewing the scene as it actually happened, as blow followed blow. And finally, at the climax of the scene, we find the recurrent use of the word “blood”, as Terr describes the scene through Eileen's eight-year-old eyes: “Blood. Blood everywhere on Susan's head. ‘Something whitish’ along with the blood”. This repetition reinforces the impression of quantity – there is so much blood that she sees it “everywhere”, and the horror of this sight is reinforced by the additional descriptions of “something whitish” and “some hair that is no longer attached to her body”, which, in their relative vagueness, give the impression of a scene so horrible that the young Eileen has had to quickly look away, rather than being able to fully process and identify exactly what she saw.

The use of an abundance of sensory detail also works alongside the repetitions to help depict the scene as vividly as possible. Terr provides not only visual, but kinaesthetic and auditory details to fully actualise the scene for her reader. For example, George Franklin's actions as he murders Susan Nason are described in a series of short declarative sentences at the end of the second paragraph (also note the repetitive structure of the final three sentences). This provides the reader with a number of separate 'word pictures' of Eileen's father, almost in the manner of stop-motion animation. We see him standing with arms raised; we see him gripping the rock; we see him steady himself – all as discrete actions. This creates a much more vivid impression than if Terr had simply described this in one sentence, thus: "Her father stood gripping a rock above his head, about to bring it down on Susan's head". In addition, Terr also describes vividly the sound of Susan's murder, in the middle of the final paragraph. As well as the repetition of the gruesomely onomatopoeic "thwack", she tells us that "Eileen could hear the sound, a sound like a baseball bat swatting an egg – the worst sound of her life". In this sentence, Terr again uses repetition to build to a climax. The first phrase is relatively innocuous (she could "hear the sound"), but it is followed by a simile ("a sound like ...") which draws attention to the fragility of Susan's skull and the violent force used against it, so by the end of the sentence the reader is in full sympathy with Eileen's feeling that this is "the worst sound of her life".

The preceding example highlights and illustrates some of the ways that narratives of personal experience can be constructed to create a sense of graphic vividness which, in turn, encourages the emotional engagement of the reader with the text. Such engagement is sought by the author as a means of persuasion. Rhetorical theory suggests that if the reader can be made to feel sympathetic to the plight of victims (whether parents or children) then they are more likely to accept the position of the author as to the reality (or otherwise) of recovered memories.



## Scientific Warranting and Experience

At this point it should be clear why experiential accounts are a very useful resource for authors. Not only do they provide opportunities for authors to construct their expertise and credibility, but they can also recruit the emotions of the reader in support of the author's position. These stories thus have the capacity to engage and persuade the reader in a way that abstract scientific and theoretical arguments do not (Plummer, 1995). From a functional perspective, therefore, it is not surprising that experiential narratives feature strongly in *both* rm and fm texts, in spite of the fact that the rhetorical utilisation of personal accounts would appear to be contrary to the stated epistemological commitments of many fm authors. As discussed in Chapter Seven, all of the fm authors invoke the rhetoric of science to support their position. They claim that the fm position is the correct one because it exemplifies scientific values. One of the criticisms which they level at rm supporters is their reliance on the anecdotal evidence provided by survivors, rather than properly designed scientific studies:

The only 'evidence' [for repression] comes from impressionistic case studies and anecdotal reports. Anecdotal case studies and clinical impressions ... cannot be used as confirming or disconfirming evidence in testing any particular concept. The reason is simply that they are isolated events with no comparative data to rule out alternative explanations. (Wakefield & Underwager, 1994, p. 237)

These authors reinforce their argument with the bald assertion (quoting from Stanovitch, 1992) that it is, quite simply, "...wrong to cite a testimonial or a case study as support for a particular theory or therapy. Those who do so mislead the public..." (p. 237).

Wakefield and Underwager's stance appears, from this passage, to be quite uncompromising. Scientists don't rely on anecdotal evidence, and using it support a theory is both "wrong" and "misleading". However, this does not stop them from presenting a variety of experiential accounts throughout their text. Indeed, the very first paragraph of the first chapter of their book is a brief, but poignant, story of elderly parents, the father accused of abuse, dying "of broken hearts" within months of capitulating to their daughter's demand for financial compensation (Wakefield & Underwager, 1994, p. 9). It appears that narratives

of personal experience, like history and science, are a highly flexible rhetorical resource. They are “wrong” when utilised by your opponent, but somehow justifiable when used to support your own argument.

The justifications provided for using personal narratives, when provided at all, are often cursory. It is as if fm authors would prefer not to draw attention to this possible inconsistency by making too much of their inclusion of such narratives. After all, as noted above, the utilisation of such narratives is a common feature of ‘self-help’ texts, and thus the reader is likely to accept and even expect them to be present. When authors do address the issue, they often frame the inclusion of these narratives as purely illustrative and/or as an aid to the understanding of the reader, as in the following excerpt:

There is perhaps no other way to understand the broad nature of this process than to explore the lives of the individual women and men this therapy has touched. For this reason, we have scattered the stories of individual patients throughout the book. We begin with Christine Phillips story, not because her story allows a great deal of insight into the process of therapy or to attempt to prove that her memories were undoubtedly false, but because it illustrates the ways in which the abuse-survivor identity can take over the lives of patients and their families. (Ofshe & Watters, 1994, p.16)

This justification is constructed particularly cleverly, as the authors present themselves as basically *compelled* to include such stories (“There is perhaps no other way ...”), if the reader is to properly understand their arguments against recovered memory therapy. Thus, the inclusion of these experiential accounts is not a choice made by the authors, but something which they are obliged to do in order to cater for the reader’s need to fully apprehend and appreciate the authors’ position. The idea that the stories act as “illustrations” of particular aspects of the authors’ argument is also included in the final sentence of this passage. As an illustration, a personal narrative has no epistemological status: it is not acting as evidence, but only as exemplification. This simple description allows the authors to neutralise any possible conflicts that might be detected between their scientific principles and their rhetorical practice. It allows them to exploit the persuasive power of both experiential accounts and scientific argument, thus maximising the rhetorical impact of their text.

My analysis in this chapter suggests that experiential accounts are a resource which authors in this debate cannot afford to ignore, even when utilisation of such stories would appear to contradict an author's stated commitment to the invalidity of anecdotal evidence. However, as my analysis demonstrates, these personal stories present an unparalleled opportunity to engage the reader's emotional involvement with the text. And, when the stakes are high, every route to persuasion must be explored and every rhetorical opportunity taken up, even at the risk of seeming to be inconsistent. Indeed, the inclusive, flexible character of persuasive rhetoric has been highlighted throughout the analysis I have presented in this thesis, and thus is a theme which I take up in more detail in the next chapter.

## Chapter 10: The Rhetorical Context of Recovered Memories

The purpose of this final chapter is to provide a satisfying conclusion to this thesis; to tie up loose ends, synthesise themes and ideas and assess the success of the project as a whole. Firstly, I provide a summary and overview of my analysis and consider how it has addressed the original research aims. I also consider the usefulness of my attempt to integrate concepts from rhetorical analysis into a discourse analysis methodology. Next, I draw out some themes from the analysis for comment. I discuss the commonalities in authors' understandings of what makes a persuasive argument, argue for the existence of a "rhetorical imperative", and discuss how the making of arguments is facilitated by the inherent flexibility of discourse. I also consider how the rhetorical context of this debate has moulded and shaped the psychological phenomena which are its central concern. Finally, I reflect on the limitations of this study and consider the possibilities it opens up for future research.

### The Analysis: An Evaluative Overview

My intention for this project, outlined in Chapter Two, was to develop some understanding of the rhetorical resources drawn upon by debate participants, and how they worked to create persuasive arguments for the existence of recovered memories and false memory syndrome. In my analysis, I constructed and examined five broad resources utilised by debate participants. Each of these resources was strategically deployed by both rm and fm proponents in support of their respective positions, and to undermine the arguments of their opponents. The first of these resources was the intellectual and personal credibility of authors. Rhetorical theory suggests that in order for their arguments to be persuasive, authors must establish both their sincerity and their knowledge. My analysis argued that the establishment of relevant category memberships such as 'therapist' or 'survivor' provided a discursive short cut for authors wanting to assert their credibility in both of these areas. However, this advantage was tempered by the potential of negative stake attributions inhering in some categories, and to manage this authors engaged in defensive rhetorical

moves designed to resist such attributions. While these negative attributions were resisted at an individual level (with the author defending their credibility as an individual), they were often made at a collective level. This strategy, category condemnation, involved authors working up the similarity of their opponents, in order to provide a collective criticism, rather than having to attack each individual's credibility specifically. This strategy was efficient and effective because it allowed the authors to provide a more exhaustive list of criticisms (which has greater rhetorical impact), with the implication that they *all* potentially applied to each individual opponent.

The second resource I examined was the construction of definitions and descriptions of memory phenomena. I argued that these definitions and descriptions were not extant 'prior' to the argument, but were constructed within the dialogic context of the debate to achieve specific rhetorical goals.

Repression and dissociation, as mechanisms for explaining the existence of recovered memories, were constructed by rm supporters as being logical and common, and thus a rational and 'normal' response to childhood trauma. The reification of these psychological processes was further accomplished by attempts to systematise their operation and effects in a manner similar to established psychological diagnoses. To undermine this construction, fm proponents argued that repression and dissociation do not have the same status as recognised psychological phenomena – because they are definitionally inconsistent, pseudoscientific, and sustained by belief rather than by knowledge. In fact, the rm position was described as having more in common with religion than science, and this was reinforced by linking rm to 'bizarre' beliefs in conspiracy theories, cult programming and so on. Recovered memories were further extrematised by fm supporters in the construction of "robust repression", a definition of repression which makes it so obviously absurd that it cannot be real. This offensive rhetoric was complemented by the construction of iatrogenesis as an alternative explanation for recovered memories. The development of confabulated memories was constructed (by fm supporters) as being an understandable, 'normal' psychological reaction to the pressures exerted in the therapeutic context, and was systematised by some fm supporters as a "syndrome". In turn, this definition was deconstructed by rm

supporters by extrematisation, and by invoking issues of stake and interest on the part of fm supporters which undermined the credibility of their explanations.

Science was the third rhetorical resource discussed in my analysis. Although the debate is often characterised as an argument between researchers (who care about science) and clinicians (who don't), an examination of the popular texts showed that scientific warrants were constructed by both fm and rm proponents. They were assisted in this task by the availability of scientific discourses privileging a number of different values and principles. Thus, false memory supporters were able to characterise their position as being 'truly' scientific, because it encapsulated scientific principles such as organised scepticism, verifiability and falsifiability. Similarly, rm supporters could also claim their position to be the 'truly' scientific one because it is disinterested and resists inappropriate generalisation, insisting that the ecological validity of research be examined. These principles and values are also attended to in the construction of scientific evidence, as shown by the way in which Loftus works up the generalisability of her 'Lost in a Shopping Mall' study. My analysis showed that while generalisability was the central problem for fm arguments based on science, the 'interpretability' of research findings was the major issue for rm studies, as the operation of unconscious psychological mechanisms can only be inferred rather than observed. Finally, I discussed how the availability of scientific arguments to both rm and fm supporters required authors to draw upon Gilbert and Mulkey's (1984) contingent repertoire as a way of undermining the credibility of opposing research evidence, while maintaining the validity of their own scientific arguments.

The operation of the contingent and empiricist repertoires was also noted in my analysis of history as a rhetorical resource. I showed how the significance of Freud's historical theories was constructed and undermined through descriptions which characterised him as either 'discovering' concepts through careful observation, or 'creating' them for a variety of personal motivations. Considerations of how historical arguments depend upon different constructions of the nature of history were also developed in this analysis. These different understandings allowed for historical events to be interpreted as providing

useful evidence in support of a position (linear and spatial), or having no intrinsic meaning (cyclical). They also provided the tools for rhetorically constructing the recovered memory movement as either evidence of social progress (linear) or a psychotherapeutic fad (cyclical).

The final rhetorical resource examined in my analysis was personal experience. I argued that experiential narratives could be used for a number of persuasive purposes. For example, I discussed how Terr's account of the Franklin case allowed her to construct her authorial credibility through establishing her membership of the 'expert witness' category, while also avoiding the negative personal attributions which may accompany this membership. In addition, I examined how specific features of Terr's account of Eileen Franklin's story, such as repetition and inclusion of sensory details, worked to evoke an emotional response in the reader with persuasive effect. Finally, I argued that it was this ability of personal narratives to recruit the emotional engagement of the reader which accounted for their high level of utilisation in both rm and fm texts, even though they contradict fm authors' opposition (on scientific grounds) to the anecdotal evidence utilised by rm proponents.

As this overview suggests, authors have a range of resources upon which to draw in the construction of their arguments. It is worthwhile noting here that although I have constructed these resources separately to facilitate analysis, the patterns of actual utilisation in texts are complex and layered. The resources overlap and interpenetrate, creating additional rhetorical emphasis through their juxtaposition. For example, authorial credibility can be constructed using the rhetorical resources of science (see Loftus & Ketcham) and/or personal experience (see Bass & Davis), while undermining authorial credibility can cast doubt on definitional strategies as discussed above. Resources can also cohere to provide contextual meaning for one another, such as the way in which the construction of science as resulting in cumulative advancement in knowledge is predicated on a linear understanding of history, which in turn provides a scientific basis upon which to judge the relevance of historical research and theories. Furthermore, the rhetorical effects of these resources are realised at a textual level through the utilisation of specific linguistic strategies. In this

fashion, the rhetorical effectiveness of definitions can be seen to operate through strategies such as extrematisation which operate at the level of lexical choice and sentence construction. In this analysis I have attempted to tease out these multiple levels of discourse in order to develop an appreciation for how these texts actually work to persuade.

To facilitate this undertaking, I employed a methodology which co-opted concepts from classical and modern rhetorical theory into a discourse analysis framework. While discourse analysis has always had an appreciation for rhetoric at a broader level (such as recognising the way accounts are oriented towards possible refutations, and the use of offensive and defensive strategies in discourse), it has not utilised rhetorical figures at the more detailed level of analysis. I believe that my understanding of how participants' discourse is constructed has been enriched by drawing upon these concepts at points throughout the analysis. In considering how authors create and manage their own credibility, an awareness of the concept of *ethos* helped me to understand the rhetorical purposes of working up category memberships, as well as appreciating the tensions that exist between demonstrating expertise and maintaining a modest demeanour which evidences good will towards the reader (as discussed in Chapter Nine). Rhetorical concepts were also particularly helpful for understanding how experiential narratives could be constructed to achieve emotional effects, and thus persuade through recruiting the feelings of the reader (*pathos*). Rhetorical theory has helped me make sense of what participants were doing in their texts, and when it is utilised in this way (rather than being imposed upon the text as an analytic framework into which the data is made to 'fit'), I believe it has much to offer the discourse analyst. Over the centuries, rhetoricians have built up a body of knowledge about language and persuasion, and this knowledge should certainly not be neglected. On the other hand, the discourse analysis orientation to language, with its focus on functionality and activity breathes new life into rhetorical tropes and figures which may have threatened to become merely categorisation devices. Thus, I believe my analysis demonstrates that a productive synthesis of discursive and rhetorical analyses is both possible and mutually beneficial.



## What Makes Popular Texts Persuasive?

One of the features highlighted by my analysis is the high level of agreement (as demonstrated by their rhetorical practices) between rm and fm authors as to what makes a persuasive argument. Although the debate is often characterised by participants and observers as being intensely polarised (Corballis, 1995; Crews, 1995) with researchers and science on one side, and therapists and experience on the other, this is not borne out in the popular texts. Instead, I found that all of the authors drew upon the same rhetorical resources and similar strategies in the making of their arguments, sometimes to a surprisingly symmetrical degree (for example, the offensive strategy of extrematisation is used by fm and rm supporters respectively, to construct definitions of both repression and iatrogenesis which are so absurd as to be implausible to the reader). Thus, as Ashmore, Myers and Potter (1995, p. 328) suggest, it does appear that these authors are drawing upon a “particular cultural line of what is persuasive”, even though a formal education in rhetoric is now the exception, rather than the rule.

I noted at the beginning of this thesis that the communicative context provided by a general, rather than an academic, audience should provide freedom for authors to deploy a wider range of arguments. This does seem to be the case, with authors drawing freely upon both the empiricist and contingent repertoires in their texts, whereas in their original study Gilbert and Mulkey found that the only the empiricist repertoire was utilised in published academic discourse, while the contingent repertoire was limited to relatively informal settings such as interviews and personal letters. However, the similarity of the persuasive strategies employed in the texts I have examined does suggest that the ‘pop psychology’ format creates its own constraints and expectancies. For example, each of the fm texts which I studied opened with the recounting of some kind of personal narrative, despite their ‘scientific’ disregard for anecdotal evidence. It is impossible to gauge to what extent this is a strategy chosen by authors, or whether it reflects a level of editorial invention. In either case, it suggests a broad level of agreement as to the desirability of gaining the reader’s attention

and engaging their sympathies through the use of concrete experience rather than abstract argumentation. Furthermore, it suggests that the desirability of creating a persuasive argument overrides the considerations of coherence or internal consistency.

That inconsistency and variability are features of conversational discourse is one of the fundamentals of the discourse analysis approach (Potter & Wetherell, 1987). In fact, variability in talk appears to be the rule, rather than the exception. People can and often do deploy contradictory accounts within the same interaction, to achieve different interactional functions. Furthermore, unless these variations are closely contiguous, they are rarely attended to by participants. Thus, contradictory or variant constructions often appear to blend seamlessly in people's talk (see, for example, Gill, 1993; Howard, Tuffin and Stephens, 2000). Interestingly, this variable feature of conversational discourse is also evident in my analysis of 'pop psychology' texts. Thus, fm authors are able to simultaneously deride the use of anecdotal evidence while employing it themselves, and Terr can critique the ecological validity of laboratory based studies of memory confabulation, while claiming that studies of fruit flies and marine snails support her theory of memory repression. This variability is oriented to the dominant persuasive function of these texts, which is determined by their situation within a highly contested public and academic debate. In my analysis of these texts, I was impressed time and time again by the way this rhetorical imperative overrode other considerations (such as consistency, stated scientific commitments and so on). For example, Wakefield and Underwager (1994) are extremely critical of the biased sample selection and lack of independent corroboration in rm studies supporting memory repression (such as Briere & Conte, 1993), yet discount these very same issues when presenting data drawn from their own survey of FMSF members! This feature of rhetorical texts has also been noted in Dascal and Cremaschi's analysis of a controversy in economics, where they note that in their correspondence the two main protagonists, Malthus and Ricardo, "... can denounce each other's use of a stratagem, while on another occasion employing it themselves ... It is as if they didn't care not even for some sort of *minimal* coherence in their use of moves and countermoves in the heat of the controversy" (1999, p. 1166). Similarly, in

the memory texts, authors seemed to be unconcerned about their inconsistencies, though their presence in the texts would seem to provide a prime opportunity for offensive rhetorical moves by their opponents (for example, note the claims of definitional inconsistency used to undermine the rm position in Chapter Six).

In their observance of the rhetorical imperative, authors are also assisted by the flexibility of the rhetorical resources which are available to them. As noted in my analysis, when drawing on resources such as history and science, authors have available to them a variety of different constructions and understandings of these concepts with which to produce their arguments. The fact that history can be understood as linear, cyclic or spatial makes it possible for authors to produce arguments about the relevance of historical events (linear, spatial) which support their position, while dismissing those which contradict it (cyclical). Similarly, the availability of a wide variety of scientific principles and values enables both rm and fm proponents to claim that their position is the 'truly' scientific one, by privileging a specific subset of these values and principles as being central to science. For fm proponents, organised scepticism, verifiability and falsifiability are stressed, while rm proponents highlight disinterestedness and ecological validity. This lack of a single, unitary scientific ideal is what makes it possible for the research evidence produced by either side to be so singularly unconvincing to their opponents. The existence of multiple scientific values and principles makes it virtually impossible to produce research that meets all the possible criteria against which it can potentially be assessed. The resulting opportunities for critique are not ignored when issues are as highly contested and disputed as they are in the memory debate. And, as Ashmore's (1993, 1995) analyses of scientific 'debunkings' show, a close scrutiny of scientific arguments often provides rhetorical openings which can be exploited by the motivated opponent or analyst. The operation of the rhetorical imperative in the memory debate suggests that any research claiming to bear upon the controversy will continue to be subjected to such highly critical scrutiny, thus reducing its persuasive potential (see, for example, the controversy over Rind, Tromovitch and Bauserman's 1998 research, discussed in Chapter One), making it unlikely that a 'scientific' resolution to the debate will be forthcoming.

## Producing Psychological Knowledge in a Rhetorical Context

As I noted at the beginning of Chapter Six, there is an increasing recognition amongst critical psychologists of the discursively constructed nature of psychological phenomena, which are, in the end, simply ways of describing experiences (Shotter, 1990). The context of the memory debate throws these processes of construction and description into sharp relief, and thus the third aim of this research was to consider how rhetoric has shaped the production of psychological knowledge about recovered and false memories. Even in the popular texts examined in this analysis, it is possible to see how the phenomena of repression, dissociation and iatrogenesis are constructed and created, and to consider the impact of the rhetorical context on these processes. My analysis of definitions and descriptions of these phenomena in Chapter Six explicates some of the ways in which they are constructed to provide persuasive accounts of memory processes. In particular, the rationality and normality of these processes is stressed. The absence of accounts stressing 'badness' or 'madness' in these texts, and in the debate as whole, is a notable one, especially considering the historical currency of such explanations in relation to issues of women's sexuality in psychology and the wider culture (Nicolson, 1992). The impact of feminist theory cannot be discounted here. As Herman notes, her own work on trauma is part of a "...collective feminist project of reinventing basic concepts of normal development and abnormal psychology" (1997, p. ix). It seems that this work has problematised victim blaming explanations for the sexual abuse of women and children to such an extent that they are no longer available as convincing resources in the memory debate. Thus, even the process of iatrogenesis (where women are influenced by therapists to invent untrue memories) is usually presented as being a very natural and normal response to the demands of therapeutic suggestion. Alternative explanations, in which these women are constructed as malicious or mentally disturbed are extremely rare (though see Pendergrast, 1997, who argues for women's 'complicity' in the memory construction process).

The construction of “robust repression” and other extreme descriptions of phenomena can also be seen to be oriented to the demands of the argumentative context. These descriptions are designed specifically to do rhetorical work for their originators. They are intentionally constructed to be so extreme as to seem absurd or ludicrous to the reader. The rhetorical payoff is that if this description is successful, the concept effectively ceases to exist as a ‘real’ phenomenon. This is particularly important for the fm position, because it deals with the problem (highlighted by Pendergrast, 1997, and Loftus, 1994) of being unable to empirically disprove the existence of an unconscious, and therefore unobservable, phenomenon. If, however, repression is discursively constructed as an absurdity, this problem is circumvented – there is no need to prove it doesn’t exist, because its unreality becomes self evident through the process of description.

Finally, the argumentative context of the memory debate also highlights the rhetorical aspects of scientific experimentation. For example, one of the features of Loftus and Ketcham’s text is its description of the ‘Lost in the Shopping Mall’ study. What this description shows (though I doubt this was intentional on the part of the authors) is the way in which the study was specifically oriented to the rhetorical context of the debate. Loftus’s account shows her intentionally setting out to *prove* the validity of iatrogenesis, in order to support her scientific ‘hunch’ that recovered memories might not be real. The entire study was designed to create evidence that would be rhetorically useful for supporting her already established position – suggesting that *The Myth of Objective Science* perhaps could have been an alternative title for her text! Happily for Loftus, this aspect of her text may well be overlooked by the “general” reader, who she is simultaneously instructing in her version of which scientific values and principles are most important (see Chapter Seven).

## Looking Back

As I reflect upon the process of carrying out this analysis, I am acutely aware of how my choices as a researcher have impacted upon this thesis. Issues of selection have been salient at every step of the research process – deciding on a research methodology, selecting the analytic material, categorising resources and choosing specific extracts to exemplify my analytic structure, and to examine in more detail. All of these decisions have been influenced by many factors, including issues of personal preference, and of course, my desire to present a persuasive argument! Thus, I freely acknowledge the partial and situated nature of this analysis and my interpretations. There are many ways in which this project ‘could have been otherwise’, some of which I explore here.

Choosing to ‘access’ the memory debate through an analysis of texts was a decision which was made on the grounds of both personal preference and pragmatism, as well as more theoretical considerations. As described earlier, my initial interest in the memory debate was sparked by a ‘pop psychology’ text (Webster, 1995), albeit a rather academic one. My early reading in the area was similarly informed by these texts, which both intrigued and horrified with their descriptions of mysterious unconscious processes, coercive therapeutic settings and painful personal narratives. I have discussed the methodological advantages of using naturalistic data such as these texts in Chapter Four. However, I also admit that choosing to research these texts was a way for me to engage with this debate in a relatively distanced and ‘safe’ way. The practical and ethical difficulties which would have been involved with a more ‘personal’ approach (such as attempting to interview recovered memory therapists and/or survivors) were not ones which I was keen to confront.

Furthermore, having made the choice to conduct a textual analysis, I am also aware that this has necessarily narrowed and directed my analytic focus. To an extent this is inevitable in any discursive analysis – the vast amounts of text generated in today’s literate, media-saturated ‘knowledge societies’ makes a comprehensive or complete analysis of any controversial issue an impossibility. The most one can hope to achieve is a partial and situated account of the area

of interest, or else be paralysed by the sheer volume of potential analytic material. Nonetheless, even in my selection of a relatively small subset of texts (only ten), I still felt overwhelmed at times by the amount of text and the (seemingly) endless inventiveness of authors, who appeared to produce new arguments and rhetorical figures on every page. Thus, the eventual analytic scheme of five main resources which I settled upon was a way of placing some constraints and boundaries upon my analysis, as well as attempting to impose some order and pattern on unruly and uncooperative texts. I am very aware that there are more resources and rhetorical strategies in my texts than the ones I have identified and discussed here, and even more which are utilised in the wider context of the debate. Initially, I had hoped that my analysis would be comprehensive, but as I worked through the texts, I decided to aim for coherence as being more achievable, and indeed possible, within the time and space constraints imposed by a doctoral thesis. Thus, I admit to engaging in the same process of ontological gerrymandering in my work that I have identified in the arguments of others. I chose texts and excerpts which best exemplified and supported my analytic categories and arguments, and ignored others which did not 'fit' to the same extent. Thus there are some voices in this analysis which are heard more loudly than others – simply because they provided more support for the analytic scheme I had constructed (which was, however, grounded in my reading of *all* the texts).

### Looking Forward

Clearly, from my preceding comments I feel that there is still much discursive and rhetorical work to be done in the analysis of the memory debate. This controversy has been so significant for psychology that it seems important to examine it as fully as possible. As my analysis has indicated, it provides a fruitful area of insight into practices of knowledge production in psychology which are thrown into sharp relief by the rhetorical context. Furthermore, the way in which this controversy has blurred the boundaries between academic and public interests has exposed the internal workings of the knowledge production process to the communal gaze, providing new opportunities for

discursive enquiry. For example, it would be interesting to investigate whether this very public display of 'dissension among the ranks' has made available new discourses problematising the expert status of psychologists/therapists. It would also be interesting to examine what impact this controversy has had on psychologists themselves. Has the existence of such a bitterly fought controversy in the discipline caused any questioning of the basic assumptions of the knowledge production enterprise in psychology, or have existing resources (such as the empiricist and contingent repertoires) proved sufficient to deal with any critical challenges which may have arisen?

My experiences in conducting this analysis have also suggested that there is scope for much more cross-fertilisation between the various approaches to studies of scientific controversies. In my attempts to make sense of this controversy in psychology, my reading and theorising encompassed a range of disciplines and methodological approaches all focussed on examining controversy from a language-based perspective. I am also aware of other potentially relevant literatures and approaches (such as linguistic pragmatics) which I did not have time or space to investigate here. The similarity of interests and approaches to controversy evidenced across discursive psychology, SSK, rhetorical studies and the rhetoric of inquiry argue, from my perspective, for greater cross-disciplinary communication and collaboration. Perhaps developing an appreciation of similarities and differences in these approaches could in turn produce a more comprehensive way of understanding controversies in science and their interpenetration with public concerns and decision making.

And finally ...

Researching and writing this thesis has affirmed and reinforced my understanding of social realities as discursive productions. The power of language to construct and constrain worlds has become even more evident to me as I have reflected on how the material experiences of our lives are given shape and meaning in the stories of our pasts that we tell to ourselves and to others. In considering the ways that language can be used to construct realities



which are painful, debilitating and oppressive, I have also gained a greater appreciation for the possibilities opened up by language to reinterpret, recreate and reassess my life experiences in positive and productive ways. However, I have also become much more aware of the influence that others gain when we allow them to become part of this process. I believe that the memory debate has many lessons to teach psychologists, and they are perhaps not the ones which have been learnt. It is not uncommon to see “the recovered memory movement” explained away as the invention of inadequately trained therapists without the benefit of a proper education in scientific psychology. This discursive move allows the maintenance of the status quo, absolving psychologists from the critical self- and disciplinary examination that might otherwise be prompted by the more disturbing aspects of this controversy – in particular, our propensity to tell other people that we know them better than they know themselves. I would be happy if this research ends up by contributing, in even a small way, to a more critical, questioning, and far less complacent outlook in the psychological community.

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