Memories Improbable: "Recovered Memories" in Perspective

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Abstract

The recent phenomenon of "recovered memories" of childhood trauma - particularly childhood sexual abuse - has sparked a debate about the extent to which these recollections are in fact confabulations. A careful examination of recovered-memory literature leads to conclusions opposite to those commonly drawn from it. In particular, an absence of memory for such trauma is *not* typically attributable to repression, nor are a host of "symptoms" a certain sign of an abusive past. However, a variety of well-known phenomena in the area of memory and suggestibility are reviewed, with reference to a reconstructive model of memory. Literature is reviewed which shows that numerous practitioners provide a setting in which susceptible individuals are likely to confabulate a history of sexual abuse in the hope of alleviating symptoms.

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Table of Contents

Recovered Memories 8

Narrative vs. Historical Truth 9

Repression 12

Dissociation 15

Dissociative Identity Disorder: 18

***A Validation Study*:** 21

***Some Case Reports*:** 25

DID In The Lab: 26

Borderline Personality Disorder: 28

The Psychobiology of Stress 30

Implicit and Explicit Memory 33

A Case Study of Implicit Memory 34

Type I and Type II Trauma 38

What Does An Absence of Memory Mean? 42

"Abuse is Common" 42

Abuse, Forgetting and Symptoms 45

Retrospective Clinical Studies: The Basic Methodological Problems 46

The Williams Study 49

Basic Flaws: 50

The Absent Analysis: 52

A Conditional Probability Model 53

A Critical Reappraisal 56

Abuse, Symptoms and Clinical Populations 58

A Revised Estimate of p(A /~M) 64

The Reliability of Continuous Memories 66

Validation of Recovered Memories 67

Simulations and Expectations: 67

Memories Improbable: 70

Summary: Recovered Memories 73
False Memory Syndrome 74
Satanic Ritual Abuse 74
Retractors 78
Childhood Amnesia 79
Childhood Amnesia and The Unquestioned Assumption: 82
Manufacturing Memories 84
Suggestibility 85
Hypnosis and Suggestibility 85
Hypnosis Does Not Improve Recall 89
Evidence that Hypnotic Methods Distort Memories 90
"Truth Serum" 91
Subtle Suggestions 92
Self-Generated Validity 93
"It Just Isn't Done" 94
Questioning the Methods 98
Affective Components and Memory Distortion 99
Narrative Truth and FMS 103
A Reconstructive View of Memory 106
Summary 109
References 111
Appendix: Tables and Figures 134

At the present time, some mental health professionals and many psychological researchers are sharply divided over the nature and validity of what have come to be called "recovered memories" (Berliner & Loftus, 1992). "Repression" is a construct that clinicians have invoked to explain the apparent failure of their clients to recall past events, most notably traumatic ones (e.g., Freud, 1950; Bass & Davis, 1988). It is also a construct that many years of research has failed to validate (Loftus, 1993a; Loftus & Ketcham, 1994; Holmes, 1990). In view of this fact, dissociation has now supplanted repression as the explanatory mechanism invoked to explain this phenomenon (Kristiansen, 1994b), and this argument is based in part on research which has tenuous (if any) links to the construct.

Much of the heat in the present debate arises from the polarization of two larger groups. On one side, anti-child-abuse advocates argue quite rightly that the sexual abuse of children is a problem that must be dealt with (e.g., Enns, McNeilly, Corkery & Gilbert, 1995; Herman, 1992). They offer the well-intentioned but ill-founded argument that such abuse almost invariably has significant psychological consequences later in life (Rind, 1995; Rind & Harrington, in press). A central point of the first major section of this paper is that research in this area has been misinterpreted it to mean that psychological symptoms are diagnostic of a abuse history in people who have no such memories.

While the general class of recovered-memory practitioners who are at the centre of this storm are not a clearly homogeneous group, they have a number things in common, and are fairly numerous (Poole, Lindsay, Memon & Bull, 1995). One, they emphasize past trauma as the primary if not sole explanation of current problems. Two, they share a view that if a person has no memory of having been traumatized, they must recall the "repressed" or "dissociated" trauma in order to get over their present difficulties. Three, a good therapist can identify people with such memories. Four, techniques such as hypnosis can be used to retrieve these memories.

These therapists share one more thing, namely, a belief that the most common form of trauma is sexual abuse, which they often claim is part of the history in a third of female clients and an unknown proportion[1] of male clients (Tavris, 1993). Thus, they very commonly spend

[1] Without evidence, Enns et al. (1995) claim "one in six" men are survivors of childhood sexual abuse. Like other "statistics," this figure is likely to get cited and re-cited until it becomes a "fact" (see Tavris, 1993, for a discussion of this phenomenon).

much time searching for memories of childhood sexual abuse, particularly in female clients. Some simply assume that every client was probably abused, whether they remember it or not (e.g., Blume, 1993, March). Techniques used to troll for memories are what has brought them into conflict with researchers, and lately, the judicial system (Associated Press, 1995; Bureau of National Affairs, 1995). This "recovered memory therapy" (RMT; Hochman, 1994) has been increasingly invoked to retrieve these alleged memories (Poole, Lindsay, Memon & Bull, 1995).

Even though, as will be shown, an absence of memory for abuse is far more indicative of a lack of abuse history than "repression" individuals have been "diagnosed" as "abused" via RMT, and placed in treatment for the inferred abuse despite having no memory of such trauma (e.g., Herman & Schatzow, 1987). They must simply have had "all the symptoms" despite the lack of any established identifiable pattern of symptoms that would indicate abuse. The sequelae of such a misdiagnosis can be disastrous because under some circumstances, people can come to believe that they were so abused when in fact they were not.

In RMT, the recovery of memories is supposedly effected through the use of hypnosis, guided imagery, sodium amytal interviews and similar techniques that rely upon the elimination of critical thinking. These methods have a long but chequered history. The use of hypnosis as a forensic technique showed a marked increase by the early 1980's (Holden, 1980), shortly before the incidence of "recovered memories" of childhood sexual abuse began to rise (Pendergrast, 1995). The notion of hypnotic refreshment of memory goes back at least as far as Freud, who realized that fantasy plays a large role in these productions[2]. Research examined in the second part of this paper confirms that such methods can increase the quantity of material recalled, although much of the content that is gained is simply false. Indeed, methods of social influence that do not utilize overt hypnosis can pressure an individual to remember things that did not happen, and believe quite strongly that the remembered material is accurate (Ofshe & Watters, 1993, 1994; Yapko, 1994).

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[2] See Powell & Boer (1995) for an interesting alternative to Masson's (1984) view that Freud was dismissing the possibility that actual sexual abuse had occurred for less than scientific reasons.

Because of the toxic side-effects of such therapy, there is a growing movement calling for the curtailment of these "memory-retrieval" techniques (e.g., Dawes, 1994; Ofshe & Watters, 1994). The argument from this quarter is that the very techniques that supposedly retrieve repressed memories are as likely to create false memories of abuse as they are to recover lost ones. This movement has been fuelled by individuals who have claim to have been falsely accused of (or even sued for) commiting acts of abuse by their own children and siblings (Wakefield & Underwager, 1992). Recovered memories of childhood sexual abuse have been advocated as sound evidence (Vella, 1994), and vilified as violating rules about the admissability of scientifically invalid testimony (Underwager & Wakefield, in press).

Through the work of the False Memory Syndrome Foundation (FMSF), a growing number of people have banded together to call for thorough investigations into recovered-memory claims. Partly as a consequence, the term "False Memory Syndrome" (FMS) has entered the vernacular. FMS has been given a definition by FMSF Scientific Advisory Board member John Kihlstrom (False Memory Syndrome Foundation, n.d.) who states that:

> *"a condition in which a person's identity and interpersonal relationships are centered around a memory of traumatic experience which is objectively false but in which the person strongly believes. Note that the syndrome is not characterized by false memories as such. We all have memories that are inaccurate. Rather, the syndrome may be diagnosed when the memory is so deeply ingrained that it orients the individual's entire personality and lifestyle, in turn disrupting all sorts of other adaptive behavior. The analogy to personality disorder is intentional. False Memory Syndrome is especially destructive because the person assiduously avoids confrontation with any evidence that might challenge the memory. Thus it takes on a life of its own, encapsulated and resistant to correction. The person may become so focused on memory that he or she may be effectively distracted from coping with the real problems in his or her life."*

How can a person come to believe so strongly that he or she suffered a trauma if it never happened? To recovered-memory theorists, that is a rhetorical question. The strength of the belief implies the factualness of the memory. However, it is *of necessity* an empirical question, if only because there are now so many clear cases of false memories (Pendergrast, 1995). The

answer to that empirical question lies in what is known about the foibles of memory. Let us begin by examining the case for recovered memories.

Recovered Memories

The idea that people will "repress" or "dissociate" memories of traumatic events - particularly in childhood - goes back a long way. The fact that we can only remember and be conscious of one thing at a time implies that at any given moment, there are far more things out of awareness than are in our conscious mind. For example, I am presently aware of a computer screen in the centre of my awareness, and a Charlie Parker CD playing at the edge of my awareness. When I wrote the preceding sentence, I was not aware of a memory of my father's grocery store, although my conscious effort to recall something from my childhood for the purposes of this sentence brought that particular item to the fore. This fundamental reality - that there is far more in storage that we can recall at any moment, lends credence to the idea that memories of which we are not aware at one time can be recalled at another time.

The concept of recovered memories as it is used in this context, however, requires rather more than this. It requires the invocation of a construct such as repression or dissociation to explain what would seem to be an anomaly. People who claim not to have been able to remember traumatic events for anywhere up to forty or fifty years after the trauma is alleged to have occurred are remembering such events, often after being subjected to questionable "memory enhancement" techniques.

Repression and dissociation both require a storage facility for memories that goes above and beyond the repository from which I drew my memory of my father's store. That facility seems to require some sort of "lock." If, say, for twenty years, I had been unable to remember that scene, and it suddenly and inexplicably came to mind, some would argue that it had been "repressed." Because the RMT canon states that traumatic events lead to repression to protect the psyche from the pain of the trauma (e.g., Bass & Davis, 1988; Herman, 1992; Terr, 1994), it would then be argued that something traumatic must have happened in the store because it was repressed. "If something is traumatic, it is likely to be repressed. This event was repressed, therefore it must have been traumatic." Formally, this is the logical fallacy of affirming the

consequent. Following this reasoning, I could argue that the fact that I haven't thought about a pet rabbit I had as a small child in over 15 years implies that owning a rabbit must have been traumatic.

Of course, it can be argued that not all things that stay out of conscious memory are necessarily repressed or dissociated. It is possible that I just never had occasion to think about my rabbit for a very long time. But this raises the difficulty of determining whether or not I *could have* thought about the rabbit say, five years ago. In cases where memories of trauma appear where they did not seem to exist before, this raises a logical impossibility for researchers that has been described by Ofshe and Watters (1994); namely, that it is impossible to sample people with allegedly "recovered" memories and determine whether or not they could have remembered the events during the time they were allegedly repressed.

However, beyond this simple limitation of repression research, there is a more fundamental problem. That is the difficulty of determining - in the absence of corroborating information - whether or not what are presently believed to be veridical memories of past trauma are indeed accurate representations of past events, or pseudomemories produced by a variety of processes that are known to influence memory.

RMT advocates argue that the extent to which a person feels and truly believes a memory to be accurate is a valid indicator of the accuracy of the memory. Some (e.g., Bass & Davis, 1988) indicate that the smallest feeling that childhood sexual abuse may have occurred is conclusive proof that it did. Thus, before discussing the evidence for unusual memory processes, it is important to make a key distinction between what Spence (1982) terms "narrative" vs. "historical" truth.

Narrative vs. Historical Truth

Spence (1982) notes that a good therapist can construct a good story out of the disconnected and fragmentary pieces that a client provides. This in no way implies that the story refers to any historical event that actually occurred, because some RMT proponents recommend using the most horrific themes imaginable as organizing principles for clinical material. For example, Mangen (1992, p. 154) suggests that the therapist remain *"open to the*

possibility" that Satanic Ritual Abuse (SRA[3]) *"can serve as a possible organizing theme for understanding the patient's behaviour and test responses,"* (compare also Olio, 1989, p. 93).

Spence further notes that *"a well-constructed story possesses a kind of narrative truth that is real and immediate and carries an important significance for the process of therapeutic change,"* (1982, p. 21). Importantly, Pendergrast (1995, p. 81) observes, *"Good fiction can be more convincing than truth."* Remaining open to the possibility of that which is not impossible is one thing, but affirming the reality of the improbable because it is not (in principle) impossible is quite another. Given the power of such affirmations by persons who present themselves as experts, this distinction is absolutely vital.

A model in which the productions of the client are taken as reflecting some kind of repressed or dissociated historical reality makes it too tempting to adopt a simplistic, naive realism wherein the construction of a coherent narrative is taken as evidence that no other alternative explanations need be considered. Further, Spence (1982) points out that the persistence of this model is due to the heightening of the *"special virtues of the analytic situation,"* in which the analyst, *"by virtue of his special training, does indeed have privileged access to the past and...is in fact a special reporter of a kind that makes almost no mistakes,"* (p. 25). The end result of this may in some cases be *"overzealous attempts on the part of the reporting analyst to fit the findings to the theory,"* (p. 27).

This is particularly evident in more extreme feminist-based theory (which tends very much to favor RMT; see Enns et al., 1995). This approach is predisposed *"to condemn the dualism between action and reflection, thought and feeling,"* (Luepnitz, 1988, p.100). Rejecting the distinction between narrative and historical truth may be seen by some as resulting in a "healthy" deconstruction of history (e.g., Foucault, 1992) where injustice in power structures is

[3] Organized ritualistic abuse by Satanists has been reported by a growing number of clinicians as part of the case histories of severely dissociative individuals. This despite the fact that law enforcement agencies have consistently failed to find any evidence of the claims being made of mass murders and the like (Lanning, 1991) .

exposed (see Herman, 1981, for an example of this). But more malignantly, it paves the way towards the reificiation of narratives as historical truths (e.g., Bass & Davis, 1988).

The intellectual climate in which this failure to distinguish fact from fiction has been identified. Modern ideologically-driven literary criticism from an extreme populist, anti-scientific position has devalued rational inquiry (Gross and Levitt, 1995). Radical feminism has likewise decried logic and reason as a viable way of gaining understanding, in favour of "alternative ways of knowing" that tell us nothing about the world outside the doctrinaire mind of the critic (Koertge, 1995). Thus, the tendency to equate a good, coherent story with a true one is hardly surprising.

Even in the area of clinical studies, the narrative/historical distinction is critical. We may compare the difference between classic case reports (e.g., McDougall, 1926) wherein the relevant details of an actual case are presented, and composite reports based on a variety of cases (e.g., Herman, 1992). In the former, direct challenges to the interpretation are possible, because the details contain historical information. In the latter, direct challenges can be deflected because of the fictitious nature of the contents, despite the *intent* of the author to present *factual* elements. In Herman (1992), interpretation *is* fact. McDougall, on the other hand, provides premises from which conclusions can be drawn. Herman presents conclusions and pieces together the necessary premisses, none of which are known to have co-occurred in any case. A blurring of distinctions between narrative and historical truth, and premisses and conclusions is necessary to accept the latter as providing any kind of evidence.

A more reasonable position assumes that there *is* a distinction between the two kinds of truth. While narrative may be useful for conveying information, it should never be mistaken for evidence of the literal truth of the memories, even where some abusive themes may be present (e.g., van Benschoten, 1990). It is entirely possible that these themes may simply reflect the client's conforming to therapist expectations, or an attempt at self-justification through blaming others. In addition, the reasonable stance admits that the facts and fictions are ordinarily extremely hard to separate even when there is some corroborating or disconfirming evidence, and utterly impossible to separate when there is no such proof (Yapko, 1994).

Unfortunately, narratives are too easily reified by reference to their explanatory utility. For example,

> *"The adult with a non-verbal memory of abuse from childhood must now put words to the picture or body feeling in order to explain it. These words are created now, not at the time of the abuse. The memories as verbalized may not be exact but do provide an explanation of what was experienced then...To be sure that these words describe a real experience, a therapist listens carefully..."* (Centre for Treatment of Sexual Abuse & Childhood Trauma, 1994, p. 2).

There are clear consequences of making this distinction that impact on therapist motivations. Assuming there is no distinction makes scientific inquiry impossible, because there "is no wrong answer." Thus, we step out of science and into politics. However, admission of the distinction reduces the therapist's privileged position as "reporter," and implies that stepping out of the role of therapist into that of political advocate may have tragic consequences when the historical reality is distorted into a fictional tale of abuse in the name of an ideology. We must keep this key distinction in mind as we examine further the claims on the RMT side of the debate.

Repression

In its strong form, the repression argument posits mental mechanisms which protect a central self from awareness of painful memories by keeping those memories out of consciousness without actually changing the content of the memories in any way. This argument is sometimes caricatured as a "videotape" model of memory in which nothing is ever forgotten, but "censors" only allow certain things to be recalled. Unfortunately, the caricature captures an essential truth about how some clinicians think about memory (see also Yapko, 1994). Thus, Laura Pasley, a "retractor" who, discovered that her "recovered" memories were false, writes:

> *My therapist not only DID NOT tell me that recovered memories may not be entirely correct, he told me over and over that to recover the memories of childhood abuse was THE ONLY way I would get well and that the memory was stored in my brain and when I had flashbacks or abreactions, it was a tape [like a computer tape] spitting out actual data from my early childhood,* (personal communication, Feb. 12, 1995).

Of course, to recover memories and abreact the trauma, these "censors" must be bypassed, and various methods are held to be "expressways" to the past.

In a weak form, the repression argument posits cognitive processes that subsequently recode the traumatic information in such a way that it is possible for the individual to cope with it, whether or not it is ever truly inaccessible in some form or another to conscious awareness. However, the reprocessing of information is a basic feature of consciousness, and non-traumatic material is subject to incorporation and distortion through this mechanism. If all recovered memories are to be taken as infallible evidence of trauma, something like the strong argument is required along with the assumption that no memories of trauma can be recovered that are reconstructions of other material. If recovered memories are to be taken as *fallible* evidence of trauma, then the recoding variant is sufficient.

However, recoding is only one explanation. It is generally agreed that four broad phases exist in the life span of a memory. These may be described as encoding (the process of putting material into storage), storage (over a period of time), retrieval and recounting (American Psychiatric Association, 1994). If a memory disappears, it could be due to a failure at any of these stages. Repression research tends to focus on motivated failure to retrieve memories that are assumed to have been encoded and stored.

As an example of "motivated forgetting" research, we may look at a study done by Glucksberg and King (1967). They studied 16 college students who learned A-B paired lists, where A was a list of nonsense syllables and B a list of words. Memory for words that had been learned but which were subsequently paired to an electric shock in a second task were forgotten more frequently than words learned without being paired to the shock. "The differential forgetting shown is specific to an unpleasant event, shock, and is not attributable to the differential recall of shock-associated words," (p.519). Approximately 15% of the target A-B pairs were forgotten, but only 5% of the control pairs. While the stimulus is scarcely traumatic, and so may not generalize to cases of childhood sexual abuse, the differential rates of forgetting seem to indicate that memory is sensitive to associations with negative stimuli.

However, the extent to which this is due to interference with encoding, storage or retrieval is unclear. Interference with encoding is plausible, though storage in a separate unconscious memory system was not established, nor was any effort made to recover the memories through hypnotic or amytal procedures and assess them for accuracy (including pseudomemory rates).

This study also fails to support the view that repression (if that is what was actually observed) is common. Indeed, 85% of "traumatic" pairs were *remembered*. If we are allowed to stretch this to the case of recovered memories of sexual abuse (as we must to argue that there is even such a thing as repression), this supports the far greater likelihood that people will *not* repress (as has been observed in clinical data; Williams, 1992, 1994).

Several other points should be made about this type of research. First, as noted above, the memory process that gave rise to the forgetting cannot be determined. Second, the generalization from shocks in a lab to abuse in the real world is not guaranteed. Third, and most importantly, we are only talking about the probability of forgetting, given "abuse." This latter probability is not the same as the probability of abuse, given that a person has no memory of it. This distinction is vital, because the failure to make it (as shown below) is a central flaw in the reasoning of some therapists.

A wealth of clinical research has been reviewed by Erdelyi (1985). He notes Freud's refusal to consider the possibility that experimental research might have something to say on the topic (p. 244). This review arrives at the conclusion that a central problem of all of all of this clinical work is the veridicality of the memories. The lack of evidence, after over half a century of clinical work with recovered memories, is striking. Erdelyi realizes that the issue is whether a "subject is actually remembering more or merely reporting more," and in fact points out that we "must be content with a Bayesian approach, involving conditional probabilities rather than certainties," (1985, p. 247).

Despite over 60 years of research, there is no basis for the concept of repression other than the fact that therapists have invoked the concept to explain why their clients who eventually report traumatic events did not report these problems earlier. Holmes (1990) examined a variety of methods and findings associated with repression research and found them sorely wanting. The differential recall of pleasant and unpleasant experiences was found to be confounded with intensity of affect, with more intense personal experiences being more likely to be remembered whether pleasant or unpleasant. Individual difference research (where it does not suffer basic logic problems in identifying "repressors") contradicts the concept by showing *better* recall. Percepetual defense research, where reaction time differences to stressful and non-stressful

words was predicted to show repression-like processes was confounded by word familiarity (i.e., unfamiliarity of "stressful" words).

While the argument has been advanced that *"perhaps our methods are just not powerful enough to detect it yet,"* (Kristiansen, 1994b) this argument applies equally well to the concept of *false* memories, research on which is already bounded by comparable ethical limitations. If we admit repression on its possibilty, we must also admit the possibility of false memories.

Some RMT advocates insist that *"researchers who are doing cognitive psychology experiments are not the ones who can make a value judgment on repression. It is the clinicians who can,"* (Terr, 1993). However, experimental cognitive research is vital to distinguishing exactly what is at work, because the clinical context is almost entirely retrospective and based on self-report. In addition, it is not the unproven value of repression and recovered memories as clinical constructs (Bloom, 1994) that is central to the debate, but accuracy of memories recovered from a "repressed" state.

In view of the generally accepted lack of solid evidence for the process of repression (Holmes, 1990; Loftus and Ketcham, 1994) by even central RMT proponents (e.g., Kristiansen, 1994b), dissociation has now been posited as the process by which memories of abuse are kept out of conscious awareness (e.g., Kristiansen, 1994b).

Dissociation

The construct of dissociation relies upon parallel systems of memory, one of which is conscious and one of which is not. The argument for dissociation suggests that traumatic memories are a special class of memories that are encoded separately from others directly at the point of initial perception. In this respect, it is not unlike the weak form of the repression argument, differing primarily in the feature that the memory is never conscious because the traumatic event is kept out of awareness from the moment of its occurrence.

With respect to simple absence of memories, James and Levy (1994) discuss dissociative amnesia as the *"inability to recall significant personal information beyond what could be explained by ordinary forgetfulness."* (p. 374). This apparent amnesia is usually attributed to the effect of some kind of traumatic stressor. These authors note that free association, hypnosis and amobarbitol interviews are used by some clinicians to make available repressed material that they suspect to be hidden. However, these methods are viewed with suspicion by others.

Amobarbitol is seen as the only psychotropic medications that is at all helpful. However, these authors do not specify mode of action or what it produces, simply that *"During therapy, the amnesia often clears rapidly, sometimes completely."* They do not make any effort to discern whether the material recalled is authentic.

Terr (1994) invokes the concept of the "hidden observer" to support the notion of dissociated memories. Derived from Hilgard's (1979) work in hypnosis, it is a theoretical construct that explains how highly hypnotizable individuals seemingly both experience and fail to experience environmentally-induced pain. Hilgard's research programme uses methods designed to examine the presence or absence of suggestions in working memory. For example, a subject is told that he or she will be seeing a broken watch, with the shorter hour hand missing. When shown a watch, the short hand is negatively halluncinated[4]. When asked the time, the subject may report the time as if the short hand is obscured by the long one, and report 2:10, 3:15, 4:20, etc.. In related studies, a subject's arm is immersed in ice water with instructions that hypnotic analgesia will remove the pain. Overt reports are elicited from the "conscious" part, while covert reports are elicited from the "hidden observer" through tapping a message or automatic writing. Thus, working memory (where the instructions are presumably stored) and current information appear to be "dissociated," and behaviour is based on how the watch or the pain is currently perceived.

To the extent that this supports the notion of seemingly-independent systems of consciousness, there is a weak analogy to dissociated memories of trauma. Hilgard (1979, p. 49) observes that *"The crucial correctives against distortion that working memory ordinarily provides are missing."* This aspect is not emphasized when he is cited by RMT advocates. The focus, as with Terr (1994), is on the supposed support for dissociation, and not the wider implications.

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[4] A "positive hallucination" is a hallucination in which an object that is not present is "seen." A "negative hallucination" is one in which an object that is present is reported as "not seen."

On the one hand, Hilgard's argument leaves open the door to the possibility that there exists some degree of dissociative amnesia in some individuals (at least by analogy to hypnosis and the "hidden observer" effect). However, on the other hand, a key implication is that the coupling of extreme suggestibility (see below) with an absence of correction against error in working memory leaves another door wide open to the implantation of false memories through explicit or implicit suggestions. As will be seen below, this accounts for the observation that attempts to recover dissociated or "repressed" memories increase confabulation (American Medical Association, 1985; Dywan & Bowers, 1983; Timm, 1984; Holden, 1980).

The idea that such distortions can occur is also supported by cognitive research. At least one study (Park, Holzman & Lenzenweger, 1995) revealed working memory deficits in people who experience an unusual number of perceptual aberrations (high PerAb) similar to those commonly attributed to "survivors," such as distortion and disturbance of body image and other objects (e.g., Bass & Davis, 1988; Blume, 1993; Engel, 1990). Park, Holzman and Lenzenweger (1995) used the Wisconsin Card Sort[5] to study working memory deficits in high PerAb individuals, suggesting that these deficits are of the sort that appear in individuals predisposed to schizophrenic-like psychosis but who do not develop the full-blown disorder. Such individuals tend to be less able to hold information in working memory (at least, as measured by the card sorting test) than control subjects who do not experience such aberrations.

However, it is also necessary to consider that "hidden observer" effects have been noted in subjects instructed to fake hypnosis (Spanos, deGroot, Tiller, Weekes, & Bertrand, 1985) and

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[5] The Wisconsin Card Sort is an established test on which deficits have been linked to dorsolateral frontal lesions of the brain (Warrington, 1974) and other biological aberrations tied to schizophrenia (Deiken, 1995; Catafau, 1994). This may differentiate it from other research on working memory that focusses on storage of and conflicts between verbal/digital information. Evidence for visuospatial and verbal dissociations between cognitive tasks is beginning to appear (see Schacter, 1991), and so further research is needed to examine whether the card sorting task reflects the *kind* of working memory impairment that concerns Hilgard.

other contexts (Spanos, 1988). Whether "hidden observers" experience higher or lower degrees of pain has been shown to depend on the expectations created in the subjects by the "hidden observer" instructions (Spanos & Hewitt, 1980). If Spanos' (in press) social psychological interpretation of such phenomena is correct, this points to hypnosis as a possible method for inducing apparent dissociation as in Dissociative Identity Disorder (Spanos, Weekes & Bertrand, 1985; Spanos, Weekes, Menary & Bertrand, 1986).

If Hilgard (1979) is right (see above), then psychosis or a milder degree of confabulation may be linked to subtle prefrontal cortical abnormalities that are in turn tied to undiagnosed schizophrenic tendencies.[6] The tendency of schizophrenia and schizotypy to run in families (Park et al., 1995) implies that the presence of relatives with this disorder increases the likelihood of the kind of extreme suggestibility that can lead to false memories - and false allegations. The study of patterns of schizophrenia in families of uncorroborated recovered-memory "survivors" could provide useful data relevant to this hypothesis.

Since Hilgard's "hidden observer" model is based on such working memory deficits, it is plausible that confabulation and inability to distinguish suggestions from reality may arise very commonly in these individuals. Practically, it implies that in people who have a family history of schizophrenia, there is a higher risk of extreme susceptibility to suggestion. In recovered-memory cases, then, the presence of these "symptoms" may increase the risk that the memories are false, rather than true, and that latent schizophrenic tendencies might underlie a tendency to intermittent psychotic confabulations that may or may not concern sexual abuse.

Terr (1994) is aware of objections to the hidden observer concept, in particular the more parsimonious social psychological explanations such as those offered by Spanos (in press) that relate to wanting to "please the hypnotist." Unfortunately (and rather nimbly), she rejects the more parsimonious model because it contradicts clinical experience, all the while oversimplifying both models. On the one hand, the social psychological model does not necessarily require conscious efforts to please the experimenter as she suggests. Yet on the other hand, the "hidden observer" in Terr's writing is no longer a sub-component of a consciousness

[6] Persinger (1994) points out that "The inhibitory functions of the prefrontal cortices are critical for discriminating between the memories of experiences normally produced by external objects or events and those induced by the vivid images evoked by words and metaphors," (p. 650)

that is functioning in atypical fasion. Rather, it is an homunculus, a set of homunculi, or even the whole patient herself: *"Some patients do seem to keep hidden observers around...These patients watched their abuse as children from the ceiling of their childhood bedroom...."* (p. 78). Of course, in some cases, these these little hidden observers seem take on a life of their own.

Dissociative Identity Disorder: A central dissociative construct in the current debate is the DSM-IV (1994) diagnosis of Dissociative Identity Disorder (DID**)**, formerly known as and often referred to as Multiple Personality Disorder (MPD). This is

> *"a condition in which there are two or more identities or personalities with each having a distinct pattern of perception, thought, and relating to the environment, and which recurrently take control of the person's behavior,"* (James & Levy, 1994, p. 379).

Associated symptoms are *"depression, nightmares, suicide attempts, phobias, rapid mood shifts, severe anxiety, depersonalization or derealization and headaches,"* (James & Levy, 1994, p. 379).

Virtually unknown until the publication of popular books such as *The Three Faces of Eve* (Thigpen & Cleckley, 1957) and *Sybil* (Schreiber, 1973), the diagnosis of MPD exploded in the 1970's, at which time (after the case of Sybil became well-known) sexual abuse was proclaimed to be an etiological factor (Coons, 1986; Hacking, 1995). Today, internet support groups are replete with multiples who write in several different voices within the same message, presumably with the same - undissociated - intention of communicating via this medium to other individuals that all personalities know.

"Splitting" has been used synonymously for "dissociation," albeit with several different senses (enumerated by Brook, 1992). Brook also distinguishes it from "repression." He points out that Freud linked dissociation to unconscious function first as a way of explaining the unconscious function of post-hypnotic suggestion, thereby linking hypnosis to this disorder. The splitting of representations of objects, affects and self into "good/bad" items that merit "affection/hostility" is another sense. The third sense refers to splitting of the "ego," which Brook identifies primarily with splitting of the attitudes. Dissociated sleep-like states, fugues and absences fall under this umbrella, and Freud saw multiple personalities as an extreme case.

Similarly, Price (1987) views the phenomena surrounding DID as one end of a continuum. It is distinguished from repression in terms of "levels." Repressed material

> *"is different from* [unrepressed] *material in a number of ways: cognitive sophistication, level of psychological integration, rawness of affect, and so on...But there is always some difference of level...by definition, repressed material ceases to appear in consciousness, and in affect and behaviour, except in deep disguise...In all forms of splitting...the material on both sides of the split is at the same level...When a representation splits, parts of the same object appear on both sides of the split...When the ego splits by taking up contradictory attitudes to something, the two or more attitudes taken up are both attitudes, and are taken up to the same object or event,"* (pp. 343-344).

The latter split seems closest to our notion of multiple personality, and the holding of contradictory attitudes seems to imply alternation between "states." However, the overall picture seems to imply that if splitting is used extensively by an individual, contradictory attitudes towards not only a victimizer but towards all people will evolve to maintain the illusion of consistency for each personality.

For example, in a hypothetical case where repeated abuse by a father is reported as preceding a "split," the diagnosis would be based on the appearance of personalities with global attitudes and behaviours that flow from them (such as those reported in the case of Billy Milligan; Keyes, 1981). Logically, these personalities would have to be sufficiently distinct from one another and continuous over time and re-appearances to be identifiable as such. To the extent that negative, hostile and violent attitudes are characteristic of a particular personality, it would seem that these attitudes are directed at those people (in general) who fall on the negative side of the split. However, the problem is, "Does the hatred for the father stem from the abuse *by* the father, or is it part of an undeserved generalization from other hate-generating problems to all people?"

The latter possibility becomes important when we consider the suggestibility of individuals diagnosed as DID. This is because hypnosis and amobarbitol are considered to be useful in getting the "personalities" to manifest themselves, and these techniques are questionable at best (see below). Virtually all cases are reported to have had histories of extreme childhood trauma, and the "mechanism" is seen as a way of protecting the psyche from being overwhelmed - a coping strategy. Biological "rewiring" of the brain's integrative temporal-limbic system has been proposed as an explanation, but is viewed as speculative by critics such

as James and Levy, (1994, p. 381) who note that MPD can make a "celebrity" out of the patient, who gets lots of reinforcement, and that malingering or elaboration of symptoms in a person who is facing criminal charges is a strong possibility.

James and Levy (1994) note that an interesting contradiction arises that casts some doubt upon the accuracy of memories of abuse in cases of DID. In the diagnostic evaluation of dissociative amnesia, organicity is indicated when (among other things) confabulation and disorganized flow of thought associations are present. (Delusional disorders in which other individuals are misidentified are also related to visual-perceptual abnormalities; Silva & Leong, 1995). James and Levy (1994) also acknowledge that a number of studies have shown DID patients to exhibit differences between personalities on a large number of physiological measures. Since this distinguishing feature of the population is strongly suggestive of deviant organic functioning, one would expect the likelihood of confabulation in the reports of such patients to be very high. Even if it were demonstrated that *"almost all cases are preceded by physical, sexual or emotional abuse,"* (p. 381), then it may well be that the *fact* of a traumatic history renders impossible any kind of certainty about the *details* of the trauma (at least in the absence of corroborating evidence). If we allow ourselves to compare memory to an LP record, and trauma to sandpaper, we cannot expect clinicians to be able to reconstruct Beethoven from a smooth piece of vinyl.

Ganaway (1989) also notes that DID patients are remarkably hypnotizable, and are frequently unaware of the source of a memory and so unable to distinguish fantasy from reality. Thus, the reality of abuse reports is a chicken-and-egg question: is the report the accurate-but-dissociated result of the abuse, or is the allegation the product of the psychopathology? In such cases, the rich fantasy life that produces the multiple personalities precludes *prima facie* acceptance of the reports of abuse. While severe psychological trauma may or may not be at the root of the disorder, the nature of the trauma is possibly obscured forever, except in cases of corroborating evidence to which we now turn.

A Validation Study: Only one study appears to have been done that makes an effort to validate reports of abuse among DID (then DID) patients. Although Coons (1994) would appear to indicate that a number of MPD cases may have validated histories of abuse, this study says nothing abot the problem of recovering long-repressed memories. In this paper, the cases were

all selected *because* the abuse was recent, it had been reported, and the individuals were in treatment for it. In short, the subjects were selected in such a way as to maximize the probability that evidence would be available, and the discovery of evidence was reported as if it were surprising.

The *only* case among the 31 subjects (9 MPD, 10 DDNOS[7], 12 other of whom 2 were factitious MPD) in which hypnosis was used was an example of work from an M.A.-level clinician. This individual "retrieved" memories of completely unsubstantiated multigenerational Satanic Ritual Abuse (SRA; see below). Another example was from a custody case where *"Despite the lack of evidence, the girl's therapist testified that she believed that child abuse had occurred and that custody should remain with the mother, "* (Coons, 1994, p. 463).

A key problem with Coons' (1994) study is the kind of evidence that was taken as corroboration. While some victimizations were witnessed by social workers and/or another parent, these cases are in the minority. Two perpetrators were convicted, but insufficient details are provided to rule out the possibility of false confessions (Ofshe, 1992; Ofshe & Watters, 1994). Other types of 'corroborating evidence' involve social workers' belief that something happened; divorces occurred after the allegations were made; or someone was charged (but not convicted) for the offense. The *cause* of the divorces doesn't appear to have been investigated. Rather, is left for the reader to infer.

Inferences drawn from hospital and clinic records, and interviews with aunts, uncles and other people not in the house at the time seem to have been viewed as corroboration as well, although the criteria are never stated. As in Herman and Schatzow (1987; discussed below), confirmation by the family is taken as evidence that the abuse happened, but vehement disconfirmation is not taken as evidence that it did not happen.

In additon, the production of false memories and/or confabulation in some cases are directly noted by Coons (1994). Aside from the SRA memories produced in one of the

[7] Dissociative Disorder Not Otherwise Specified.

adolescents with factitious DID, *"previous knowledge about DID and observation of other patients with dissociative disorders appeared to provide a basis for simulation,"* (p. 464).

Finally, the point that Kihlstrom (1994a) has made is worth reiterating one more time: the fact that some kind of abuse occurred does not imply directly that the memories of abuse are accurate. If Mary was fondled in a park at age 5 by her brother, and at age 25 recalls under hypnosis that her father and uncle took her infant sister (whose birth certificate was hidden by conspiring government officials) and sacrificed her to Satan after raping her repeatedly on a bed of nails, miraculously not killing her through this act, then the fact of fondling does not validate the recall of the violent crimes. If Coons (1994) tried to match records to reports to rule out this kind of confound, he should have given more details in the paper. We are left with something that looks like validation, but which fails to go beyond the kernel of truth that may be in many reports. While the kernel is important to clinicians for understanding patterns in the person's self-narrative, we must keep in mind the importance of what the kernel has grown into for those falsely accused of abuse.

In fact, some go so far as to claim that Satanic cults torture children to teach them how to dissociate, and then take advantage of this ability to "program" in custom-made personalities that are trained for allegiance to the cult or to commit crimes (e.g., Coleman, 1994; Goodwin, 1994b; Hammond, 1992; Rockwell, 1994). Some find that alter personalities have alter personalities of their own (e.g., Lovern, 1993), or hold that loud protestations of innocence by accused paedophiles are due to their own history of childhood abuse which they are repeating, but are not aware of because the abuse is perpetrated by alter personalities (Rockwell, 1994). Yet others misrepresent this statistically rare disorder as "common long-term effect" of childhood sexual abuse who are purported to represent half of women and a third of males (Health and Welfare Canada, 1993).

Today we hear that that *"Incest victims often become skillful child actors...many develop a false self with which to relate to the outside world, to act as if things were fine and normal,"* (Forward & Buck, 1990, p. 155). The "hypervigilant" ability to read tacit social scripts and skillfully improvise parts in interpersonal settings is, of course, taken as a symptom of abuse, which is itself inferred from the hypervigilant abilities. The circularity is simply ignored. Likewise, the confidence that therapists have in their ability to identify the *"real self inside"* is

not questioned in this setting where identifying people who are *"living a lie"* (Forward & Buck, 1990, p. 156) is naturally a key to therapy. However, deciding whether a life is a lie or not logically requires the ability to distinguish the "true" from the "play-acted" roles. As Dawes (1994) points out, the basis for this ability is assumed in RMT, rather than proven.

Logic traps and paucity of evidence aside, we must consider that the therapeutic processes that commonly precede a diagnosis of DID are precisely the problematic methods that are questioned by FMS advocates. The speculative biological explanations proffered (e.g., van der Kolk, 1994; see below) are less parsimonious than simple social compliance. A good deal of literature is indicative of the fact that DID is not seen except by a small number of therapists who believe strongly in it, and who use methods that are known to create and promote the symptoms (e.g., Mai, 1995; Merskey, 1995; Spanos, Weekes & Bertrand, 1985; Spanos et al., 1986; Weissberg, 1993). As Spanos (1994, pp. 155-156) observes, to the extent that people with DID-like symptoms are exposed to such methods, *"the idea that early abuse is associated with MPD becomes a self-fulfilling prophecy."*

In addition, Spanos (1994) points out that it parallels other phenomena (cross-culturally) such as demonic possession and glossolia, which have been shown to be strongly linked to social factors. Indeed, similar to present-day protests of "denial" and Rockwell's (1994) "alter paedophiles," the denial of demonic possession by the supposedly "possessed" individual have been construed as attempts by a wily demon to avoid divine punishment (Spanos, 1994).

To explore the extent to which the identification and treatment of DID is non-standard practice, Mai (1995) conducted a survey of Canadian psychiatrists. He reports that a substantial number appear to follow Merskey (1995) in doubting the validity of the diagnosis, and that *"the diagnosis of DID is made by a small number of psychiatrists who make a relatively large number of new diagnoses of the condition,"* (p. 156). A similar observation was made by Poole et al., (1995), who found that 5% of the clinicians in their samples accounted for 58% of reported SRA cases; indicating that those who believe strongly in the phenomenon find it at a rate twenty-six times as high as other therapists (see also Weissberg, 1993). The argument here is essentially that the markedly non-random distribution of cases is indicative of iatrogenic processes in treatment. In short, the belief system of the therapist enhances or creates dissociative tendencies.

The culture- and belief-bound nature of the disorder also suggests that the belief of therapists is at work, for it is rare in the United Kingdom (Burrell, 1994) but relatively common in North America and other cultures where professional belief in DID is more common (Seltzer, 1994). However, the importation of the notion of SRA through books such as that of Sinason (1994) leads us to the prediction that an outbreak in the UK is due to arrive within the next couple of years, as the ideas become current with uncritical practitioners.

Some Case Reports: Close examination of some case reports of DID leads directly to the conclusion that in some unknown number of instances, iatrogenic processes were at work. One case has been reported by Coons (1988) in which a police hypnotist used extremely leading questions and methods to elicit a very dubious confession of murder from a woman who during the session appeared to "split" into different personalities. Coons deduces that since "true DID" arises from extended childhood abuse and is observable before and after hypnosis, the complete absence of dissociative symptomatology at either time indicates the willingness of the subject to go along with the suggestions when in a vulnerable state. He concludes that since this case more closely parallels the many experimental demonstrations of the creation of DID in a lab, it constitutes a *"flagrant abuse of forensic hypnosis,"* (p. 1).

Bowman, Blix and Coons (1985) report on an apparently corroborated case of repeated rape that was the backdrop to **DID.** However, in this case, not only was the client raped, but her sister was as well. Both were subjected to the same kind and degree of abuse. The sister disappears from the case report quite quickly and is not reported as having developed symptomatology. What differentiated the sisters? Nothing, for over four years, as far as can be told from the report. However, despite a complete lack of psychological symptomatology from ages 8.5 to almost fourteen, when she was assessed by therapists committed to the idea of multiple personalities, hypnosis and age regression, the various alter personalities (or “alters”) began to appear. What may well have been an imaginary childhood friend evolved into a second personality, which came (over the course of diagnostic interviewing under hypnosis) to be dissociated, despite conscious "interplay" at the outset.

Interestingly, amnesia was eventually reported for everything *except* the abuse. More interestingly, she started to report having been amnesic for abuse-related periods during times in which she was assessed as psychologically very healthy by other therapists who were aware of

the history. By the time she had been in therapy for 8 months, she was able to exhibit amnesic episodes in therapy, replete with voices in her head. Indeed, the authors note in the discussion that it is important for the therapist to provide information to parents about *"how to deal with the child using his 'multiple personalities' or 'amnestic* (sic) *episodes to avoid responsibility,"* (p. 113), but they do not connect the reinforcement that this provides to the etiology of the disorder.

Other case-based clinical evidence has been put together by Seltzer (1994), who treated five cases of DID in which a particular *"empathic, competent"* therapist *"trained in MPD technique"* (p. 443) used hypnosis and guided imagery to "uncover" memories of sexual abuse. The patients' descriptions of the process of therapy revealed their insight into the iatrogenic factors that produced the many "alter egos" or "alters" that appeared in therapy. Not least among these factors was a marked desire to produce symptoms in order to please the therapist.

The failure in these cases of professional therapists to perceive iatrogenesis while in the therapeutic situation is analogous to the Ouija board effect of not perceiving one's own direction of a process when in a "dialogue" with another person. Like a good horror story, this can lead to experiencing traumatic reactions despite the physical absence of stressors. Indeed, one of the cases has been left with post-traumatic stress syndrome (PTSD) symptoms and identifies the trauma as the former DID therapy itself (Seltzer, 1994).

How can this happen? Easily, according to Spanos (in press), whose research programme has led him to the conclusion that

> *hypnotic procedures do <u>not</u> contain intrinsic properties that enhance responsiveness to suggestion, facilitate the development of alter personalities, or generate complex behavioral responses of any kind. Instead, hypnotic procedures influence behavior indirectly, by altering subjects' motivations, expectations and interpretations. Whether or not diagnostic interviews or other procedures facilitate the development of alter personalities will depend upon the extent to which those procedures provide cues for such enactments, create expectations that legitimate such enactments, and provide reinforcements for the manifestation of such enactments.*

DID In The Lab: More importantly, DID symptoms have been elicited in studies where normal samples were given a leading hypnotic induction (e.g., Spanos et al., 1986; Weekes, 1985). Spanos' research program reveals the possibility of producing multiple personality

disorder even in lab situations where the strong "transference love" at work in therapy (Seltzer, 1994, p. 444) is not present to amplify the effect. Another weakness of the method used in an experimental setting that would be expected to reduce the intensity of the effect is that the experimenter is aware of the attempt to induce the symptoms, and so does not reify them. Thus, the potent reinforcement that a credulous and convincing therapist would provide is absent. Furthermore, the experimenters only had the participants in the lab for very brief time, in contrast to the months or years that some clients spend in therapy before being diagnosed.

Part of this research program was carried out by Weekes (1985; reported in Spanos et al., 1986), who randomly assigned 40 men and 40 women into 'experimental' and 'control' groups, and told them they were to role play an accused murderer in sessions with a 'psychiatrist' (role played by a confederate). Baseline measures were taken on a sentence completion test, a semantic differential, MMPI subscales, and from interviews about their parents. At a second session the experimental group was given a hypnotic induction taken almost verbatim from the Hillside Strangler transcripts.

Twenty-four of the 40 who got the "Bianchi treatment" developed symptoms of MPD, but none of the controls did. Thirty-two of this group reported 'amnesia' for the crimes. Again, none of the controls did. Of those reporting 'different parts' of themselves with different names, 22 reported amnesia and 2 did not. Of those reporting 'different parts' but with the same name, 10 reported amnesia and 6 did not. Males and females did not differ on incidence of MPD (despite other research that indicates females to be somewhat more suggestible; Weekes & Lynn, 1988), and nearly all of the Bianchi group and the control group initially denied the murders, but 10 of the Bianchi group later admitted and described the murders (compared to zero controls). All of these 10 claimed amnesia.

The psychometric tests revealed "MPD" profiles in the Bianchi group after hypnosis, but not the controls. Baseline measures were taken at the first session; hypnosis was used at the second session and follow-up psychometric testing was done at a third session, by another "personality" where applicable, or under hypnosis where no new personalities had appeared. The second measures differed markedly for the "MPD" subjects, but not for the others.

The summary of these results is worth quoting at length, because it underscores the inferences to be drawn from this area of research:

> *"The retrospective accounts of the role-playing multiples were fabricated strategically in order to buttress and authenticate their multiple personality enactments. Similar retrospective accounts found in the multiple personality case literature are usually taken at face value as evidence of traumatic developmental experiences. Our findings, of course, do not mean that all retrospective accounts by psychiatric patients are fabricated. On the other hand, many patients may well become invested in legitimating their self-presentations by selectively shaping historical information to make it consistent with the preconceptions held by the therapist. Given these circumstances, it is difficult to determine the extent to which retrospective accounts of patients are distorted in line with their new self-definitions,"* (p. 86).

Of course, the rate of production of symptoms may be lower than 53% in groups where the motivation to confabulate is lower. But it is doubtful that it stands at zero, simply because motivation is such a many-faceted thing, and in some individuals the need to 'get better' may prompt even higher motivation. People (in normal or clinical populations) all want some kind of reason for their problems, particularly when personal responsibility for the problem is embarassing. The lower motivation of some people may also be offset by greater susceptibility to suggestion in clinical settings. For a therapist to provide a rationalization that itself enhances symptomatology runs counter to the principle *primum non nocere* ("do no harm").

The social psychological view expounded by Schumaker (1991) points to dissociation as an ordinary adaptive process that allows us to function in society, and process information efficiently. On one hand, it can function independently of the process of direct suggestion (as usually conceived). On the other hand, for some people, endless repetition of a message is sufficient to induce a state of dissociation where logical and critical thought is overridden, and the person *"can accept suggestions for which there is* ***no logical foundation****,"* (p. 128, emphasis in original).

Many if not most individuals diagnosed as DID report some kind of abuse in childhood. Some are validated quite clearly, others less so. Yet others are clearly factitious (e.g., Coons & Milstein, 1994). It would appear that the current literature can establish little more than this, and so reification of abuse reports as authentic "histories" (e.g., Ross, Miller, Bjornson, Reagor, Fraser & Anderson, 1991) is scarcely warranted.

***Borderline Personality Disorder**:* Among the various "dissociative" effects that is often supposed to stem from childhood sexual abuse is borderline personality disorder (BPD; Herman, Perry & van der Kolk, 1989). Paris & Zweig-Frank (1992) reviewd the literature, and asked why BPD is rare, if sexual abuse is common (repeating the "1/3 of women have been abused" statistic discussed in more detail below). A variety of factors are invoked to explain this on the basis of representative research, but they add up to one thing: the relative rarity of events sufficiently traumatic to produce such strong and lasting effects. Other confounds are at work, such as biological predisposition, high levels of dysfunction in families where both incest and non-familial molestation has occurred, other social psychological factors and interactions with developmental factors.

Borderline cases exhibit affective, impulsive and interpersonal psychopathology (Paris & Zweig-Frank, 1992), and are rather notorious for abusing drug prescriptions, taking advangage of supportive therapy and talking incessantly about symptoms and introducing new problems (Chessick, 1979, p. 405). The most troublesome (i.e., "symptomatic") cases are those who have reality-testing deficits and are unable to *"distinguish internally from externally derived perceptions of the analyst,"* (Bellak & Faithorn, 1981, p. 25). Given this , the motivation and ability to confabulate symptoms should be present in abuse-centred therapy, due to the impulsive adoption of maladaptive interpersonal suggestions about sexual behaviour in the probably-dysfunctional but not-necessarily-incestuous family of origin . The evolution of ersatz recovered memories of abuse is thus likely in cases where (due to the toxic effects of childhood and/or adult drug usage on the brain) memory blanks manifest themselves in later life, asking to be filled.

It appears that the relationship of reported abuse histories to BPD is something of a chicken-and-egg question. If psychopathologies arise from the unsucessful processing of reality, then in the absence of corroboration, "recovered memories" are indistinguishable from the products of mental disorders that may have emotional effects comparable to those of actual trauma. Thus, it is important that we consider the view that some memories can precipitate intense physiological and psychological reactions (as in PTSD; van der Kolk, 1994), which has

often been mistaken for evidence that memories of trauma are historically accurate (e.g., Mack, quoted in Seebach, 1995[8]; Wylie, 1993).

The Psychobiology of Stress

In turning to the biological arguments for loss-of-memory for traumatic events, it is perhaps best to point out first that those making such a case have to have very solid evidence. That is because the idea that a memory system would evolve that systematically *forgets* stimuli that are potentially dangerous runs counter to what would be expected on the basis of the theory of evolution. In short, species that forget where danger lies are unlikely to survive and reproduce. While the evolutionary argument is imperfect with regard to non-adaptive and potentially harmful features such as the appendix in humans, the presence of a strong tendency to forget about dangers in most of a species would be much more maladaptive than an occasionally-malfunctioning evolutionary remnant.

Nevertheless, the argument from biology has been made. Memory and emotion have a complex relationship that has been noted in empirical studies. Memories of central aspects of stressful events can be fairly good while memories of the details are not (Christianson & Loftus, 1990). For example, no one who heard the news and saw the videotape is likely to forget that the *Challenger* space shuttle blew up, even though the details of how one heard the news are easily confused with other events going on at the time to produce (over time) largely false memories (Neisser and Harsch, 1992). This factor points towards the action of some physiological processes as mediators of memory. This line of reasoning has led to some

[8] Harvard professor of psychiatry John Mack is quoted in the context of his belief in space alien abductions as follows: *"...there may be some value in challenging our restricted epistemology and expanding our criteria for evaluating information to include the power or intensity with which something is felt and communicated."* Once again we see a challenge to our basic epistemology on the basis of completely unsubstantiated claims, in which a total lack of evidence appears to be irrelevant.

speculation as to the nature of traumatic memories, some of which are being passed off as factual accounts of underlying processes (e.g., Burgess, Hartman & Baker, 1995).

Some of the intense reactions to various environmental stimuli (which must be distinguished from actual memory cues) have been described as "flashbacks," or intense re-experiencing of a traumatic event. A central problem with this idea is that much of the literature on these memories simply cannot validate the accuracy of a given flashback or "body memory." Body memories are supposedly physiological ways of remembering past trauma that manifest themselves as peculiar physical pains (e.g., Kristiansen, 1994a).

If van der Kolk is right in stating that people suffering from PTSD have a problem of misinterpreting *"innocuous stimuli, such as unexpected noises, as potential threats,"* (1994a, p. 255), then a tragic problem arises. Since not every "survivor's"

> *"disclosure is sure to be accurate...when we are dealing with such extreme trauma, there will likely be confusions, distortions, and reports of events that could not literally be true,"* (Bass & Davis, 1994a, p. 33),

then confabulation may well be a symptom of trauma. People who have suffered some kind of traumatizing event will be much more susceptible to misinterpretation of their symptoms and attribution of their cause to unrelated or possibly even non-existent events.

As well, van der Kolk speculates that in cases of PTSD, nightmares related to trauma may be *"timeless and unmodified by further experience,"* (p. 261). The trauma-specific "videotape" model is contradicted by several clinical cases. One, reported by Yapko (1993), is of a self-professed Vietnam veteran who appeared to suffer from PTSD. He responded with terror to his wife's sneakers, because his Viet Cong captors wore similar shoes. It was only after his suicide that it was discovered he had never served in the armed forces, much less gone to Vietnam[9]. In another case one woman was able to identify the sources of her flashbacks after retracting allegtions of Satanic abuse. Scenes from books, movies and other fictional sources

[9] This factor is consistent with the possibility of Munchausen's syndrome, in which masquerading as a war veteran is common (Goodwin, 1988).

had come to be part of her "personal history" (Pendergrast, 1995, p. 328). A third case, reported by van Dyke, Zilberg and McKinnon (1985) is of an actual Second World War veteran who had a recurring nightmares of combat in which "his grandchildren (aged 8 and 12 years) were incorporated into these dreams as adults."

If traumatic memories are impervious to later experience and cannot be confabulated then these cases would be impossible. While this again illustrates a problem of the "videotape" model of memory, at least for traumatic memories as a special subset, it is not without additional complications. To understand a key difficulty, we must examine van der Kolk's (1994, p. 258) basic model of memory. This model postulates "declarative" and "nondeclarative" subsystems of memory, where facts and events are stored in "declarative" memory, while skills and habits, conditioned sensorimotor responses, and emotional associations are stored in "nondeclarative" memory. "Nondeclarative" memories arise from extreme stress reactions, where *"Words do not allow for meaningful constructs,"* (p. 335). However, this leads to fundamental problems with his model for RMT advocates. First, it implies that words will invariably fail to be accurate descriptors of any such experience, and therefore verbal reports are to be taken with a very large grain of salt. Secondly, the same sort of physiological evidence that supports it also implies that *"intense affect may inhibit proper evaluation and categorization of experience,"* (van der Kolk, 1994a, p. 261).

Ironically, improper evaluation and categorization of experience lie at the heart of FMS. These aspects are possibly seen in sharpest relief in the cases of children's post-traumatic stress disorder that arise from viewing horror movies that are documented by Terr (1990, p. 335). (In good RMT form, Terr indicates that the writers of such films may be victims of childhood trauma themselves.) An inability to distinguish reality from fantasy is a prime example of improper evaluation and categorization, and would, from this perspective, appear to be a characteristic of trauma survivors.

Another weakness of van der Kolk's (1994, 1994a) model lies in the fact that there is no apparent way for nondeclarative memories to become accurate statements about the trauma itself. Rather, the implication is that while physiological responses to stimuli have over-generalized, the ability to put together a coherent and accurate story must rely on other processes that are reconstructive in nature, the existence of which van der Kolk does acknowledge (1994a,

p. 253). The very over-generalization of fear to innocuous stimuli is indicative of a process whereby heightened affective reactivity results in the misconstrual of non-threatening stimuli as "dangerous." This leads directly to false positives if felt threat is equated with actual threat. The Committee on Sexual Offences Against Children and Youths (1984) reports that almost half of victims are threatened or physically forced by convicted offenders, although only one in eight sustained physical injury, implying that felt threat probably greatly exceeds actual physical harm in most cases.

The strength of such a dissociated model is its apparent consistency with the current views of many cognitive psychologists who view memory less as a unitary construct than as a set of *"systems and subsystems with different operating characteristics,"* (Schacter, in press; Tulving & Schacter, 1990, p. 301). In particular, two well-known subsystems termed "explicit" and "implicit" memory are equated with "declarative" and "nondeclarative" memory (respectively) in the van der Kolk (1994) model.

Implicit and Explicit Memory

With regard to the problem of clinically documented repressions and distortions of memory, Greenwald's (1992) review notes that

> *"A simpler account of self-deception follows from the observation that one can avoid a threatening situation without having done the cognitive analysis needed to know exactly what the threat is...adaptive cognitive defenses should be able to operate on the basis of superficial warning signals, with no more than partial knowledge of the threatening state of affairs,"* (p.773).

Likewise,

> *"A simpler account of repression uses empirically established phenomena of implicit memory as the basis for understanding apparent instances of recovery of repressed memories...this implicit-becomes-explicit memory account is far simpler in its theoretical interpretation than the psychoanalytic account, which requires a sophisticatedly cognizant (and near omniscient) unconscious agency."*

Since Greenwald's model of repression and dissociation is based on the implicit memory literature, we must examine this research. It will be seen that this literature does not extend to

the elaborate processes claimed by RMT advocates, and so it is difficult to extrapolate these notions to richly detailed memories of trauma.

A large body of cognitive research has in the past focused on tests that make quite clear the demands on the individual to recall explicitly items that have been presented in one situation or another. However, much recent cognitive research has indicated that tasks can be designed to identify 'implicit memories' that facilitate performance of some kind, which are nevertheless not verbalizable (Schacter, 1991). Because the impact of the content of the memories is demonstrable, the memories appear to be present, but the evidence thus far still requires us to limit the definition of "implicit memory" to the notion of *"a set of task characteristics rather than ...an underlying cognitive structure,"* (Parkin, 1994). The priming effect (Tulving & Schacter, 1990) is one such task, in which the facilitation of performance in identifying words and objects that have been presented previously cannot be explained by reference to verbalizable memories.

Little work has been done on forgetting in this paradigm, but what has been done appears to indicate that implicit memory is less prone to forgetting than explicit memory (Nilsson & Bäckman, 1989). As well, implicit precedes explicit memory in childhood development (Nilsson & Bäckman, 1989; Parkin, 1987). The possibility that learning can occur of which one is unaware but which nevertheless affects later recall opens wide the door to the possibility that unrecognized suggestions can be incorporated into later memories and not be recognized for the falsities that they are. The concept of implicit memory holds that implicit learning inside *or outside* of therapy may easily influence current recall in subtle ways. Thus, concerns are warranted about the effect on mental health of the media's perpetuation of pseudoscientific ideas about repression through popular TV shows such as *Oprah* and *Geraldo* (Eve & Harrold, 1993).

The presence of implicit, unverbalizable memory for motor skills has been used to argue for the presence of yet another system wherein previously-explicit memories begin to behave like implicit ones for a while, then pop back into explicit memory either under a therapist's guidance or in response to a cue of some sort. Terr (1994, pp. 44-45) briefly describes implicit memory research, and claims that *"no-longer-verbal memories drive action just as effectively as would a conditioning experiment,"* and this model has been used to explain to potential survivors "how memory works," (e.g., Centre for Treatment of Sexual Abuse & Childhood Trauma, 1994).

However, this is where the evidence falls out from beneath this structure, and the argument is clearly made from a virtual vacuum[10].

A Case Study of Implicit Memory

In an attempt to show how implicit memory works in cases of trauma, Terr cites the example of a woman named Eileen Lipsker, who claimed to have witnessed a long-ago murder, and "recovered" the memory one day. Terr (1994, p. 46) refers to her memory as "intact" after twenty years," and describes its sudden, fully detailed emergence one day when she looked into her daughter's eyes, with no other prompting. However, Ofshe & Watters (1994) point out a number of reasons that this is no evidence at all. Key among these reasons is that Terr omits important details. Specifically, the version of the recovery of the memory that she cites is only one of five very different versions related to prosecutors on the case at various times.

Although Terr claims that this woman knew nothing of the concept of repression, testimony at the trial (where Terr also testified) established that she had learned of it the preceding summer, while working with with a different therapist. She also neglects to mention that one of the earlier versions of the story was "recovered" through hypnosis, contradicting her version of the event where Lipsker looked into her daughter's eyes and the whole memory came flooding back intact. As well, she dramatizes the accuracy of the memory in its final form, omitting the many changes that it underwent over the months of therapy, going from a rather fanciful picture that was contradicted by the evidence to a detailed account consistent with information that the newspapers had originally reported (Ofshe and Watters, 1994).

Taken at face value, Terr's account is a stirring example of a recovered memory. Examined closely, however, it becomes a remarkable example of the evolution of a false memory. There is a deeper, ironic twist, however. The fact that the woman truly believed that the memory was real implies that she was unaware of the impact of the prompting and

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[10] "Arguing from a vacuum" is Dawes' (1994, p. 25) term for a mode of reasoning where supportive evidence is hypothesized, while negative evidence is ignored. "What is purported to be true is supported not by direct evidence but by attacking an alternative possibility."

suggestions that she received during the year that she worked on "recovering" the memory. Terr herself reveals a key process by which false memories can be created: *"People who have been taught implicity are unaware of how they were taught,"* (p. 44). This is the main danger of "memory recovery" methods that are informed by a focus on childhood abuse; the extent of the learning is obscured by the fact that it is so subtle.

It would appear, then that the implicit memory paradigm provides little support for the clinical models of memory. However, if we accept it as evidence that contents of consciousness of which we are unaware can impact on current cognitions, then it is equally (if not *more*) supportive of a basic process presumed to underlie FMS.

Unconscious Acquisition of Information

Since the dissociation hypothesis also states that memories are absent from awareness because they were never conscious from the beginning, then some kind of perception-without-awareness is necessary if we are to later admit recovery of them[11]. Merikle (1992) reviews the literature relevant to understanding of how a person could encode things of which they are not conscious. This hypothetical process suffers from conceptual problems related to those noted in repression studies. The acceptance or rejection of behavioural indices of conscious experience is crucial to forming a position on these processes. However, Merikle's (1992) conclusion that behavioural indices differ qualitatively across "aware" and "unaware" conditions in numerous studies suggests that cognitive processes are at work outside of our awareness.

Greenwald (1992) also concedes that there appear to exist cognitive processes that work outside of awareness, but makes a strong case for the comparative simplicity of these processes. Most importantly, with regard to what he terms *"registered-but-unattended stimuli,"* he asks the question *"What memory residues are established by such stimuli?"* (p. 767). In this article, two senses of "unconscious" were distinguished: "Outside of attention," and "not available to conscious experience because unverbalizable." Three research domains are summarized that bear upon the construct: 1) unconscious cognitive activation, 2) unconscious establishment of

[11] Complete lack of encoding of course implies that in the absence of later accurate information about the event, recall is impossible. Whether or not incorporation of later information constitutes a "memory" or not is a matter of semantics. However, because the quality of that information would have to be equal to the original input to be recalled with accuracy indistinguishable from an actual memory, and because in the absence of a video recording of the event, the source's memory is subject to decay in storage and imperfection in recall, "pseudomemory" is probably still a better term.

memory, and 3) unconscious retrieval of memory. He concludes that people *"acquire much more knowledge of attended events than they can later verbalize,"* (p. 772) but that abstract ideas are not learned without attention. Of course, the fact that one *cannot* verbalize much of the information implies that verbal accounts of that information are likely to be misleading if not simply false, and that they are certainly not verifiable in the absence of direct evidence about the situation in which the learning took place.

Lewicki, Hill and Czyzewska (1992) point out the ubiquity of nonconscious acquisition of information by noting the inability of most people to describe the contents of procedural knowledge that they demonstrably have. They draw on a set of experiments where individuals' performance was enhanced without conscious awareness by experimental provision of complex clues to a matrix scanning task. Likewise, when a critical covariation that had been acquired was changed, performance deteriorated. A $100 reward for detection of the critical pattern failed to produce any insight on the part of participants regarding their procedural knowledge. The authors also discuss related research which indicates that there may exist nonconscious generalizations of implicit knowledge.

This latter point leads to an interesting speculative digression that parallels the double-edged swords of Hilgard's "hidden observer" and the implicit memory paradigm. If unconscious information can be generalized to other situations through unconscious processes, then this research implies not only the possibility of recovered memories, but also false memories. If stories are built up out of unconsciously recognized patterns, then the purported tendency of "survivors" to repeat the abusive patterns (e.g., Terr, 1991) is accounted for. However, a pattern of negative interactions with one person could also come to include a history of sexual abuse by an entirely different person on the basis of unconscious generalization. This effect would be most pronounced in people with genuine histories of abuse.

Coupled with other evidence that the unconscious acquisition of information may be able to account for *"encoding and interpretation of stimuli and the triggering of emotional reactions,"* (Lewicki, Hill & Czyzewska, 1992, p. 796), it is at least plausible that information acquired outside of awareness can become part of conscious awareness. This has further implications for the present debate, which can again be cast in terms of false positives (i.e., memories of abuse that did not happen) and false negatives (failure to find memories of actual abuse). First, it is

possible that someone who does not have a memory of abuse might be led to an emotional reaction on the basis of partial information from a stimulus that recalls the abuse (as in the PTSD domain and the van der Kolk model, noted above). Two, it is also possible that subtle suggestions from a therapist can become incorporated into a comparatively innocuous memory of an emotional situation, and produce not only a (partially or wholly) false reconstruction of that situation, but a fearful reaction that might be mistakenly interpreted as indicating that the memory is more accurate that it actually is. However, results from several studies indicate that simply because information is gathered outside of awareness does *not* necessarily imply that it is somehow immune to the same processes of forgetting, distortion and elaboration as is consciously-gathered information. Thus, Williams' (1992, 1994) report on the presence of some small degree of forgetting suggests that means a failure to recall abuse (or at least, making an allegation of it) is within the realm of possibility. Similarly, it is possible that at least some of Herman and Schatzow's (1987) 'amnesic' group had corroboration for abuse that actually occurred. However, evidence summarized later in the present review indicates that distortion and elaboration are also very likely to be at work.

There is some evidence from cognitive psychology that the mind contains material of which we are unaware. Some of this information is simply unavailable, while other data are subject to specific conditions of recall. Other contents can only be inferred by their influence on specific tasks that may be inhibited or facilitated by them. However, to the extent that these results support some RMT claims, they indict RMT methods. For instance, Hilgard's hidden observer suggests not only a separate system, but the likelihood that a breakdown of critical thought will produce confabulation when the opportunity arises. Implicit memory processes may not only be present, but they may permit the incorporation of irrelevant material into what are later taken to be "memories." Nevertheless, the case has been made that traumatic memories are not subject to the rules of cognitive science.

Although fragmentary memories reflect the past about as well as a shattered mirror reflects one's face, Terr (1990) has attempted to link them solidly to historical realities with her model of Type I and Type II trauma processing.

Type I and Type II Trauma

If there is one universal law agreed to by cognitive scientists, it is that *"better learning produces better recall and recognition,* (Schonfield & Stones, 1979, p. 135). In spite of this, Terr (1990) holds that repeated trauma causes the individual to learn how to repress or dissociate the material instead of recalling it better. These "Type II" trauma memories are thus blurry, inaccurate and fragmented (Terr, 1990, p. 184). She distinguishes them from sharp, clear, never-forgotten memories of "Type I" trauma, in which single, unique events are remembered quite well - like *"moving pictures,"* (Terr, 1990, p. 170).

Unique events that stand out in memory have a high probability of being reprocessed through repetition to oneself in an effort to understand it, and being reminded of it by others. Understandably memorable, they are sometimes referred to as "flashbulb" memories (e.g., . Neisser & Harsch, 1992). However the analogy to photographic equipments ends there. The tragic mid-flight explosion of the spaceship *Challenger* provided researchers with an opportunity to study an emotional memory in a naturalistic setting, and while 60% of these memories were fairly accurate, marked distortions were observed in 40% of the psychology students' accounts of the event (Neisser & Harsch, 1992). Terr (1996) has studied children who witnessed the *Challenger* disaster, and observed that those less involved with the event had worse memory than those who were very involved. Nevertheless, within 5-7 weeks of the event, fully 30% of the children harbored misconceptions about it. By 14 months, this had gone down to 8% among the older children (suggesting a good deal of discussion with others) but remained at 29% among the younger ones. Ten percent confabulated events.

Reasonable as the "flashbulb" hypothesis may be, there remains a dearth of research to substantiate the Type II claim. More unfortunately, Terr is quite willing to testify that blurry, inaccurate and fragmented memories nevertheless represent historical realities very accurately (Loftus & Ketcham, 1994), in spite of the fact that her model has been contradicted not only by her definition and her evidence, but by data from Williams (1992, 1994; see below) and a case from Goodwin (1988). Some weak support may be claimed on the basis of another recent study (Belicki, Boucock, Cuddy & Dunlop, n.d., see below), although there is reason to doubt these results.

The case of Eileen Lipsker that Terr (1994, p. 11) cites in support of her theory is inconsistent with her claim. The traumatic incidents that were reported were never repressed,

but Terr claims that the repeated traumatizations enabled this woman to *"develop the knack for repression....she had practiced 'forgetting' so often that she could repress when she really needed to."*[12] She is thus using unrepressed material as evidence for a type of repression that, from its apparent effortlessness and the context of the memory for a murder, seems more like what others term dissociation. This position also reflects a marked change from Terr's (1990) stance that it is almost impossible to reconstruct accurate versions of early childhood events from adult recollections (pp. 4-5).

Other evidence for the Type I / Type II distinction has been presented by Belicki et al. (n.d.). Of 68 cases reporting sexual abuse, "memory disruption" of some kind was reported by 67% of those also reporting repeated events, compared to 18.5% of those reporting single events. However, there is reason to wonder about the validity of this evidence. For example, consider whether or not you have disrupted memories of high school. If you fail to remember every moment of every class, is your memory disrupted? Does that mean that you were continually abused in high school, or does it mean that there are limits to your capacity to recollect long series of events? However, single events which are quite unique are very likely to be recalled. Asking after them is likely to produce one, such as graduation, which by reason of the ability to recall it, is remembered or "not disrupted." Inability to remember *everything* can thus be seen as "disruption," while the ability to remember *anything* can be seen as "continuous," quite independently of whether or not the events occurred.

Recent empirical evidence contradicts the idea that repeated trauma is likely to be forgotten. In a study by Williams (1992, 1994), 62% of a sample of women recalled making an allegation of sexual abuse some 17 years previously. The allegations had been documented at the time of a visit to the hospital, and there was corroborating evidence in some - thought not all - cases. Thirty-eight percent did not report the original incident, but of this group, 68% recalled multiple incidents of abuse that had neither been repressed nor (apparently) "recovered" in

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[12] This is similar to Erdelyi's (1990) view that not thinking about distasteful events is a skill that can be mastered. He differs from Terr, though, in his opinion that the memories are not *in principle* irretrievable.

therapy. Thus, if we assume the 38% "repressing" the memory were doing so due to repeated trauma, then almost two-thirds of the victims of such trauma failed to forget most of it.

In contrast to the Type II hypothesis, it is more likely that the victims will *remember* the fact of the trauma, if not the details. As argued below, the fact that one incident was not reported is plausibly explained by the possibility that it was so distorted by other incidents that it simply wasn't identified correctly by the researchers.

Spanos (in press) points out that this kind of blurring is consistent with what is known about repeated non-traumatic events. A schema is constructed around what usually happens, while the details of specific events are lost. It is, of course, possible that details might be "recovered" later; however, in the absence of corroborating evidence, distiguishing them from confabulations is impossible.

Other counter-evidence from the case cited by Goodwin (1988) involves a woman whose memories of repeated abuse were *not* repressed. The presence of Munchausen's Syndrome (and the Proxy variant) in this case would not appear to account for the memories, because these were apparently corroborated by her mother and husband. Thus, like Williams' sample, repeated abuse did not leave a complete vacuum in memory.

The idea that traumatic memories are a special class of memories with their own properties is not consistent with the evidence. They are very susceptible to distortion as DID researchers are well aware, and there simply is no evidence that they can "flash back" in pristine clarity. While it is possible that the CIA is conspiring to produce Yapko's Vietnam veteran case by hiding military records, and perhaps the young grandchildren of van Dyke, Zilberg and McKinnon's (1985) veteran were in posession of an age-progressing time machine, it is far more likely that this clinical evidence leads to the conclusion that van der Kolk's views are incorrect in holding that traumatic memories are impervious to later experience. Clearly, even if the videotape is a short, one-event cassette, it can still be dubbed when overgeneralization of fear leads to faulty reconstructions. As Schacter (in press) has pointed out,

> *"the subjective experience of remembering does not correspond in any simple way to the reawakening or reactivation of a dormant picture in the mind."* (p. 32).

Unfortunately, a common theme in self-help literature (e.g., Bass & Davis, 1988) is that the subtlest of cues will prompt a "flashback" to a traumatic event.. The original work on which

much of this line of thought is predicated (Grinker and Spiegel, 1945) implies the opposite Many of the bomber pilots they studied had what today would be labelled post-traumatic stress disorder, and yet were able to return to combat as fighter pilots, the 'cure' being due to alleviation of the stress of responsibility for the crew's lives. The absence of the paralyzing results that one would expect after reading Herman (1992), Terr (1994) or Bass and Davis (1988) is strong disproof of their theory, given the nearly-exact reproduction of the trauma by cues such as enemy fighter planes, gunfire and flak. Indeed, it is more than disproof. If the sense responsibility for the well-being of others produces the symptoms, then making allegations of abuse against loved ones on the basis of what one is told about one's childhood in therapy might itself help to produce the symptoms that are then taken as validation of the "memories."

If unique events stand out, while repeated events may blur but not be forgotten, then a parsimonious explanation of the data lies in a single reconstructive system of memory that becomes less able to distinguish particular instances as the number of instances mounts. Where an absence of memory becomes a report of abuse, one must therefore ask what is being reconstructed - old material, or new?

What Does An Absence of Memory Mean?

The repression/dissociation arguments have implications for how we interpret an absence of memory. If we grant that such processes exist, then it is possible that a person who says "no" when asked whether or not abuse occurred was in fact abused but doesn't remember it. It is also possible that they simply don't want to talk about it. But it is also possible that they simply were not abused. Thus, in diagnosis, we wish to estimate the probability of a hidden abuse history when faced with someone who does not report such a history. This means that we need to estimate the proportion of people who report no memory of abuse, but who do have such a history. Fortunately, there is recent evidence that bears upon this, although it is commonly misinterpreted. To understand the implications of this evidence, it is necessary to clarify some key issues surrounding the definition, rates and study of childhood sexual abuse.

"Abuse is Common" Nobody denies that sexual abuse happens, and nobody claims that it is a good thing[13]. However, there are many questions about its prevalence, because a key RMT argument is based upon the commonness not only of repression, but of sexual abuse. Despite the lack of an empirical basis for the claim, "one in three women" and "an unknown number of men" are said to have endured sexual abuse as a child. This figure is cited by many RMT proponents, who cite one another's citations of the pseudo-statistic as "proof" of its validity (Tavris, 1993). The definitional and methodological issues are very slippery and thus warrant discussion.

With respect to sexual abuse, there are strict definitions that relate to explicitly sexual acts with children. These are very clear. However, on the other hand, there are very many extremely loose definitions that equate multiple violent rapes with seeing a parent in the nude on occasion, being seen naked in the bathtub by a parent (e.g., Forward & Buck, 1990, p. 139) or a brief experience with a flasher.

Such loose definitions are common in the recovered-memory literature (for an account of their evolution, see Hacking, 1995, Ch. 4). For example, if at any time a child witnesses a parent without clothes on or is seen naked by a parent, that child has been "abused," (Engel, 1990, p. 40). Since toilet training involves the parent seeing the child *"bathe, excrete, or urinate,"* (p. 40) we must infer that Engel feels that 100% of the human race has been sexually abused as a child. While Engel points out that the intention of the adult is important in deciding whether or not the act was abusive (p. 41), it requires that the child be able to read the adult's mind, a feat not yet achieved even by other adults. Shifting criteria quickly, Engel then claims that if the *"child felt uncomfortable or strange about it, then she or he was sexually abused."*

Likewise, Blume (1993) seems to feel that any situation involving a person who has some degree of power over a child that makes a child feel uncomfortable is sexual abuse. Strange and uncomfortable experiences that a child may be too young to understand or have a label for are

[13] Several studies have nevertheless shown that positive benefits are commonly reported by survivors, although these are weighed against perceived harm (McMillen, Zuravin & Rideout, 1995).

incest, because presumably the only thing that an adult can do to make a child uncomfortable is something sexual. Thus, the meaning of 'incest' is stretched to the point of meaninglessness. However, another point that Blume seems not to appreciate is that retrospective accounts of long-past events that were poorly understood are hopelessly entangled with the interpretations that have come to be put on those events in adulthood - or in therapy.

Similarly, Phillips (cited in Chase, 1987, p. 6) extends the Oxford English Dictionary (OED) definition of incest to include sexual contact with mothers' boyfriends, while Adams (1991) distorts it to include completely non-sexual parental behaviours that service *"the needs and feelings of the parent rather than the child,"* (p.2). Like other RMT definitions, this one seems *"as expandable as a hot-air balloon...anything your parents did that you didn't like is a violation,"* (Tavris, 1993, p. 17). It is thus impossible to formulate a specific definition that will work for all of the literature that is to be examined in this paper. Rather, the effect of loosening definitions of abuse on findings will be a key theme as the results are interpreted.

For example, one retrospective study that found a whopping 100% abuse in a community sample (Maynes & Feinauer, 1994) based their survey on a very presumptive question. They asked about who was the most abusive person related to the most significant abuse experiences. Something in their purportedly random sampling produced an overwhelmingly female sample (199 out of 226, z-test of H_o: p=.5 gives z=17.63, p=0), all of whom apparently experienced at least "little or minimal abuse" such as receiving obscene phone calls. Half experienced fondling or worse, and one in five reported severe sexual assault (e.g., with a weapon).

Unfortunately, little can be concluded from this study, because it is not known if people were excluded or the missing data were non-random as a result of initial questions that functioned (intentionally or not) as "screening" questions. What *can* be concluded is that rubber definitions can be stretched quite widely, at least in biased samples. If, on the other hand, this sample reflects reality and 70% of women were abused, we must ask why so few developed clinical symptoms.

The importance of this issue is laid out in retrospective study by Martin et al. (1993). This study showed that while loose definitions of abuse can generate prevalence estimates of slightly over 30% (our original figure) the "tightening" of the definition to exclude markedly less traumatic "abuse" reduces the estimated rate (in their study) to 19.7%. Because base rates are

critical to our thinking about the probabilities cited in the debate, their clarification is essential. Likewise, we must recognize that as severity of abuse becomes increasingly extreme, the probability of its occurrence becomes ever more remote. For example, Palmer, Bramble, Metcalfe, Oppenheimer, and Smith (1994) report data that reflects this pattern rather well. Table 1 shows the data presented in that paper (p. 677) collapsed across both psychiatric patients and non-psychiatric general male surgery patients. Defined as intercourse, abuse is reported in under 5% of the sample. Defined as "any event" it extends to some 20% of the sample.

----------insert Table 1 around here----------

Data reported by the Committee on Sexual Offenses against Children and Youths (1984) is re-organized in Table 2 to reflect the extent to which the inclusion of less severe (or more easily misinterpreted) assaults in definitions of abuse can bias responses. This presentation is not meant to trivialize these matters, but instead to underscore the need to distinguish them from (rather than equate them with) the more severe forms of abuse that are too-commonly reported as part of recovered memories. Among females, inclusion of fondling, kissing and exhibitionism nearly doubles the rate; among males it is more than doubled (compare also the data presented in Poole et al.'s [1995] Table 2, p. 429).

----------Insert Table 2 around here----------

Unfortunately, the precision of the numbers in Table 2 leaves something to be desired, since the number of acts exceeds the number of victims by about 26% among females and 12% among males, indicating marked non-independence of the categories due to multiple reports by some individuals. Nevertheless, it is clear that a substantial increase in reported abuse can be obtained easily by including less severe (and presumably less traumatic[14]) events in the definition. This is particularly troublesome if the respondents are aware that they are participating in a study of abusive experiences. This creates a contextual demand to interpret (for example) an inadvertent glimpse of a nude parent or a kiss on the cheek as what the researcher terms "abuse."

[14] McMillen, Zuravin and Rideout (1995) examined earlier research on perceived benefits of preadult sexual contact. The prior research and their study all found fairly high rates of women viewing the experiences positively. Their criticisms of previous research concerned the use of overly broad definitions that included hugging and kissing, and sampling bias in favour of women who viewed the experiences positively. These factors were much under much better control in their study.

The stretching of the definition of abuse as it occurs in this debate has less to do with science than with ideology, and must be recognized for the purposes that it serves: to make it easier to believe that a recovered memory of abuse is plausible. If "one in three" have such a history, it's easy to believe. However, if high rates of abuse are typically coupled with the argument that the trauma are likely to be forgotten, then it is easy to deduce that many people are walking around with no memory of a traumatic childhood. One study commonly cited to support this idea (Williams, 1992, 1994) in fact contradicts it when properly evaluated. However, before turning to this study, it is necessary to understand the methodological problems of studies of this type.

Abuse, Forgetting and Symptoms

Following much of the recovered-memory literature (e.g., Herman & Schatzow, 1986; Terr, 1990, 1994; Williams, 1992, 1994), we will accept for the present purposes any self-report of "not having had a memory" as "repression." For example, Forward and Buck (1990, p. 152) offer the following to their readers:

> *"The only way many victims can survive their early incest traumas is to mount a psychological cover-up, pushing these memories so far beneath conscious awareness that they may not surface for many years, if ever."*

The mechanism by which this happens is as apparent to the reader as the evidence on which it is based: both are unstated. Because critics (e.g., Ofshe & Watters, 1994) have pointed out that the precise mechanisms are never clearly defined by RMT advocates, this "blanket" usage will have to suffice for now.

As noted above, the empirical arguments for RMT rely upon a high reported prevalence of sexual abuse among children and the incidence of apparent amnesia for abuse among survivors. The research commonly cited as evidence for amnesia involves one of two types of data. The first type is where people who did not originally have a memory for an event remember it either in or out of therapy and the event is assumed to have occurred despite the absence of corroboration (e.g., Briere & Conte, 1993; Feldman-Summers & Pope, 1994). The second type is where broadly-defined "corroboration" exists, such as indications that the alleged perpetrator had done something similar to someone else (Herman & Schatzow, 1987) or previous records that an allegation had been made years ago (Williams, 1992, 1994).

Retrospective Clinical Studies:

The Basic Methodological Problems

There are a variety of problems inherent to the study of the effects of sexual abuse (Briere, 1992b). Numerous methodological problems in this literature have been outlined by Briere (1992a), who underscores biases intrinsic to self-report data. Briere notes both the problem of including abused individuals in control groups due to "amnesia" (false negatives) and the complementary problem of the inclusion of unabused people in groups reporting abuse (false positives). Although he observes that experimenters *"attempts to provide an environment supportive of disclosure,"* (p. 197), he concludes that false positives are not as plausible as false negatives, because there is no *"secondary gain"* for the ostensibly-abused subject (p. 198). How an environment supportive of disclosure can fail to provide secondary gains through the approval of therapists, support groups and/or researchers (or continued participation of low-SES subjects in paid-participation research; e.g., Williams, 1992, 1994) is not explained.

From an epidemiological viewpoint, the false negative problem would be viewed as the inverse of the well-known "recall bias," which is

> *"generally thought to arise from greater effort on the part of cases* [i.e., respondents in a study] *to remember past exposures and/or from a tendency for changes in behavior following diagnosis or treatment to influence the accuracy of recall among cases,"* (Austin, Hill, Flanders & Greenberg, 1994, p. 70).

For example, suppose a person had a rash on the left arm of unknown origin, and then participated in study run by a researcher committed to the notion that this type of rash is the result of exposure at some time in one's life to carbon monoxide (CO). If simply asked about what might have caused the rash, the subject would probably list some corrosive chemicals or unusual plants that he or she had recently been in contact with. If, on the other hand, the subject was informed that the rash was very likely due to exposure to CO, the list of exposures would probably focus almost exclusively on being near sources of CO - spuriously confirming the researcher's hypothesis. What is worse, the rash might simply go away due to normal healing processes, but if the subject studiously avoided exposure to CO following the initial questions in the study, both the researcher's and the clients beliefs about the caustic effects of CO would appear (to the uncritical eye) to be vindicated.

As well, there is what is known as the "sampling effect." This is the tendency of clinical samples to yield higher rates of comorbidity than community samples. This is due to the built-in bias arising from the sampling procedure itself, where people with more symptoms are more likely to be found in the clinical setting (el-Guebaly, 1995). Often ignored by clinicians, it can produce a powerful illusion that makes it seem as if one can infer a cause from symptoms, despite a poor statistical association (Dawes, 1994; Tavris, 1993).

An example of this is the work of Terr (1990, 1994) who feels that one *"can't always guess the nature of a trauma from a series of symptoms, but sometimes you can,"* (1994, p. 55). In this work she emphasizes correct guesses, apparently feeling that wrong guesses are irrelevant. After describing her remarkable diagnosis of writer Stephen King on the basis of his horror stories and a snippet of a conversation he was having that she overheard in a café[15], she admits that it is possible to implant a false memory, and that people with false memories experience symptoms, but denies that the false memory is likely to create *"a cluster of symptoms and signs,"* (p. 55). Apparently she is forgetting that people who are in therapy *already have* symptoms and signs, and that the purported link between abuse and just about any symptom imaginable (Tavris, 1993) makes virtually any complaint a fishing license for repressed trauma, at least in the minds of some RMT therapists. Admission of this biasing factor supports a central FMS contention: that many studies and clinical reports are biased in the direction of over-identifying abuse as a causal factor in various psychological disorders.

The rate at which corroboration can be obtained in studies is also an issue. Despite purportedly high rates of authentication of memories in at least two studies (Belicki et al., n.d.; Herman & Schatzow, 1987), demands for corroboration are viewed as unnecessary and harmful (e.g., Bass & Davis, 1988) partly because it is very difficult to obtain evidence, and partly because proof is deemed unnecessary anyways, the “memories” serving as their own proof. In research, where proof is seen as more important, Briere views the problem of corroboration as a

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[15] The conversation she overheard was King describing a story from his childhood that his mother told him years after the event. A friend had been horribly killed on the train tracks, and he came home from visiting this friend visibly shaken. Although (if he was a witness to this) it would be a Type I trauma, he had never recalled anything about the incident other than what his mother had told him, and neither one knew whether he was present, or whether it had happened before he arrived. In Danse Macabre (King, 1981, p. 82), he notes of another therapist who made a similar inference: "I believe this is a totally specious idea -- such shoot - from - the hip psychological judgments are little more than jumped-up astrology," (p.82)

methodological confound in studies. With respect to the presence or absence of evidence, he points out the tendency of investigations to be limited to severe cases of abuse and possibly to certain social classes. This would affect the external validity of corroboration studies. However, he omits the equally plausible possibility that cases not brought to the attention of authorities are much less likely to have corroborating evidence, and that this would produce a marked upward bias in estimates of the extent to which memories are corroborated. Likewise, he neglects the well-known bias that can be introduced by the conscious or unconscious influence of researchers who are not blind to the purpose of the study (see Austin et al., 1994, p. 71 for a discussion of this problem).

Nevertheless, his conclusion that *"the accuracy of sexual abuse reports cannot be assured, in terms of ruling out either false positives or false negatives,"* (p. 198) does at least implicitly recognize the need to think probabilistically in these matters (also see Nash, 1994, for a discussion of false postitives and false negatives). From a design perspective, Briere also points out that the correlational design of such studies does not allow clean inferences about causality, and the cross-sectional retrospective designs (in which people with unknown history are asked to recall events) are weak by comparison to prospective longitudinal designs that study people with documented histories of trauma.

In terms of reporting bias, a major longitudinal study of almost 1,000 subjects (Henry, Moffitt, Caspi, Langley & Silva, 1994) found retrospective agreement with prospectively gathered data about major injuries and family processes (among other things) to be very poor, even thought the participants were with 60 days of their 18th birthday when questioned, and they were asked about events occurring after their 7th birthday. Recall of major injuries was slightly better than chance; recall of family characteristics was low to moderate; recall of their own behaviour problems was poor. Unfortunately, in recovered-memory research the prospective design has rarely been used.

This basic flaw of retrospective studies has been also been noted by Dawes (1993) who makes an additional point: *"if we identify the degree of statistical contingency in prediction with the degree we find in retrospection, we seriously overestimate,"* (p. 1). This is shown through algebraic results in which *"the degree of predictability appears to be systematically greater*

when the analysis is retrospective than when it is prospective...We systematically overestimate," (p. 7).

The Williams Study

Due to this central flaw in retrospective studies, the following discussion will instead focus on a major prospective, longitudinal study by Williams (1992, 1994, 1994a, 1995). These papers attempt to detail the extent to which memories might be suppressed in a population that has documented history of abuse allegations. Williams herself appears to view this study as supportive of the idea of recovered memories. Although she avoids the term "repression" in the paper, she implies it in her (1994) conclusions. While she recognizes that lack of rehearsal of a memory can lead to forgetting (p. 1174) she nevertheless feels that forgotten material is tucked away somewhere in some inaccessible corner of the mind, because she observes that

> *"If, as these findings suggest, having no recall of sexual abuse is a fairly common event, later recovery of memories of child sexual abuse should not be surprising."*

Without a repression-like construct, this step is completely unwarranted.

In a broader context, this research has been widely cited as evidence that people block out traumatic memories. For example, Terr, (1994) states directly that *"This was repression or some other extreme 'forgetting' defense in action,"* (p. 53). Whether or not some sort of "repression" was at work, however, one mistaken interpretation of these data has already surfaced in several contexts. This misinterpretation is the notion that if "a number of abused people forget trauma," and "sexual abuse is widespread" provides proof that recovered memories are almost all true memories (e.g., Kristiansen, 1994a; Vella, 1994; Whitehead, 1992).

Close examination of the results of this study will be coupled with an examination of the arguments based on the data cited by RMT advocates along with some results from cross-sectional retrospective research to reveal that the RMT position is not, in fact, very far from the FMS position. Contrary to the claim that most people have a history of abuse but just don't remember it (e.g., Blume, 1993, March; Blume, 1993; "Leslie Watkins," quoted in Pendergrast, 1995, p. 274), we will now see that if a client has no memory of abuse, then a repressed history of abuse is an unlikely hypothesis. It will be shown that the preceding misinterpretation of the data is based on simply ignoring or dismissing not just a large class of evidence, but the mathematical consequences of the standard RMT position. As Herman Rubin (1988) has pointed

out with reference to statistical methods, *"we may look for robustness arguments to enable us to deal with difficult situations, but we must avoid selectively accepting those theorems which we like and rejecting those we do not like as a basis for behavior,"* (p. 293). The same, of course, applies to classes of evidence and their logical implications in research. To deal with this study, it is first necessary to examine the basic methodological flaws that it contains.

Basic Flaws: Williams (1992, 1994) reports that 38% of a sample of women with a history of abuse allegations experienced a period when they did not recall the abuse. While the data are reported as if the allegations made years before had been substantiated, in fact, two-thirds of the cases were included even when no evidence apart from the allegation itself was in the original medical file (Williams, 1992). As well, the response rate was just under 63% of the women selected on the basis of records documenting the allegation.

An additional methodological flaw in this study parallels one that has been pointed out in Briere and Conte's (1993) study by Ofshe and Watters (1994) and noted above. This has to do with the logical quandary of asking people whether or not the person had ever experienced a period when they could not remember the abuse. It assumes that the subject "would have knowledge of the status of a memory during a period when that memory by the subject's own admission never came into consciousness" (Ofshe & Watters, 1994, p. 308). This question loads the dice in favour of producing reports of "repression" by inducing people with relatively continuous memories of abuse to confuse not thinking about the events in question with not remembering them. Taken literally, this question also would get a 'yes' from someone who did not recall the abuse for the entire period of his or her life **prior** to the time that the abuse occurred (Wakefield & Underwager, 1992a).

A further criticism lies in the plausible alternative explanation that many of the women simply did not want to talk about the events in question. This would be particularly true of individuals who initially fabricated the allegations (three of whom were dropped from the study when they admitted to this). In addition, a conceptual difficulty lies in the fact that at no time is failure to report the incident distinguished from a failure of memory or unwillingness to discuss it. While Williams (1994) dismisses this claim, her evidence is questionable.

She reports that a measure was developed to determine willingness to discuss personal matters. However, unless there were only three items on the whole "measure", the whole

instrument is not be described. Without theoretical justification, she merely reports that women who "reported a prior history of undergoing an abortion, prostitution, or having a sexually transmitted disease" (STD) were equally likely to recall the index event as those who did not report. Why they should differ in an inner-city, low-SES population where the base rates would be expected to be high is not discussed.

Since the research was explicity identified as being connected with a hospital (Williams, 1994, p. 1169), it is not surprising that most subjects were willing to talk about medical matters, and a history of prostitution is so logically connected to STD's and abortion that willingness to discuss this is not surprising, either. It was just assumed that people might be equally candid about discussing medical conditions and things that happened to them personally as they would be about either identifying family members or friends as paedophiles (if the allegations on file are accurate) or parents/guardians as liars (if the allegations on file were made up for vindictive purposes). The generalization from talking about *personal* medical issues in a hospital study to what in many cases were private *family* issues is an undocumented and probably unwarranted leap of faith. This possibility is supported by her finding that where the alleged perpetrator was a family member or close friend, the women were less likely to report the index hospitalization.

As well, while the failure of the two groups to differ in reporting STD's, abortions and prostitution appears *prima facie* to fit with her claim, the fit may be *too* good: her chi-square statistic is .0023, which she reports as $p > .9621$. If we think of this as a test of *excessive* goodness-of-fit (as Fisher did with Mendel's data; Gigerenzer et al., 1990, p. 151)[16], then $p < .05$. In view of the extensive array of items that could have picked up a difference, the chance selection of three that so precisely fail to find one is peculiar. Because we cannot be certain that other unreported items were not used, and there is no theoretical rationale for the selection of these three, the oddity of the statistic is problematic for her interpretation. Capitalization on chance is not ruled out. Two requests from the author of this paper for a copy of the measures or details of other items that might have been used but not reported have thus far gone unanswered, so this remains a mystery for the time being.

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[16] Essentially, Fisher re-examined Mendel's data and found that the random deviations from the theoretical values were too small to have occurred by chance, leading to the charge that Mendel had "fudged" the data.

If we take for granted the probability that *any* social scientific research is bound to have flaws and limitations of some sort, and grant this study the consideration it deserves, we can begin to understand what its central findings say to the present debate. However, in order understand its proper place in the debate, this study must be examined with reference to the RMT claims that are commonly based on it. This analysis is absent in Williams' (1992, 1994) papers and all the papers I have managed to find that cite it.

The Absent Analysis: In spite of claims that "high" abuse rates and "high" repression rates imply the ability to diagnose abuse from a set of symptoms, no attempt appears to have been made to run the appropriate analyses, despite the fact such analyses for inferring back from symptoms to a cause are well-known (e.g., Colton, 1974). And yet, diagnoses are being made where, for instance, *"patients with only vague nonspecific symptoms have been informed after a single consultation that they have undoubtedly been the victims of a Satanic cult,"* (Herman, 1992, p. 180; see also Loftus, 1995). Specifically, what is missing is an estimate of the probability that someone was abused, given that they have no memory of it. If this probability is high, then there is perhaps something to be said for recovered memory therapy, especially if the probability of symptoms, given abuse, is substantial. If these values are low, however, then any risks associated with this therapy (such as false memories and consequent false accusations of abuse) probably outweigh the (as-yet undemonstrated; Lindsay, 1995) benefits. The latter problem is particularly acute because if current causes of symptomatology are ignored due to an overattribution of causality to childhood trauma, healing becomes unlikely.

The Williams study (1992, 1994) is essential in that it provides a useful probability estimate that contradicts claims that a repressed history of abuse is likely. This estimate, the probability of "not remembering," given a history of abuse, is vital. Thus it makes sense to examine these commonly-cited numbers (along with some others) from an analytic perspective that permits inferences about the likelihood of abuse from what is given by Williams and what is drawn from other research. While the available numbers are as yet somewhat tenuous, it is hoped that the implications of the present analysis will prompt more suitable re-examinations of existing data, as well as guide the collection of data in the future.

A Conditional Probability Model: Setting aside basic criticisms of Williams' (1992, 1994) study that have been outlined above, let us use her alleged 38% "forgetting" rate

uncritically (for the moment). Let us also assume that approximately 30% of women in the general population have been sexually abused by age 18. This is a figure representative of some commonly-cited large-sample estimates. For example, Elliott (1994) observed 32% in a sample of 2,963 women volunteers, but with a low response rate of 55%. Nevertheless, this rate is somewhat higher than others (e.g., Moeller, Bachmann, & Moeller, 1993, who reported just under 20% in 668 men and women, with a 76% response rate).

As well, Mullen, Martin, Anderson, Romans and Herbison (1994) obtained reports of childhood sexual abuse from 11% of 2,250 women who where mailed a questionnaire in a community sample, of whom 1376 responded (making the rate of abuse in respondents 18%). Mullen et al. claim that 32% of the sample reported some form of childhood sexual abuse before the age of 16, but this appears to have been obtained by adding up the percentage reporting penetration (3.8%), genital touching (15.9%) and non-genital fondling or non-contact forms of abuse (12.3%). In the whole sample, Mullen et al. report that a maximum of 23% gave accounts of some sort of sexual abuse, so the 32% figure has apparently been inflated by double-counting individuals who report two or more forms of abuse.

Lastly, a U.S. study of the incidence and prevalence of abuse has found an incidence of *"14.8 to 22.6 abused children per thousand, including 1.9 to 2.1 children per thousand who are victims of sexual abuse,"* (cited in Kihlstrom, in press). Incidence is the number of new cases each year, and prevalence is the proportion exposed at a given point in time. Thus, with 14.8 to 22.6 new cases a year, a cohort of 1000 would, from time of birth to age 16, have a prevalence of up 240 to 360 cases (24% to 36%) of some kind of physical abuse by age 17, with *sexual* abuse accounting for up to 34 cases (or 3.4%) based on this estimate. Unfortunately the criteria for identifying an episode as 'abuse' was not detailed. Comparable numbers have been derived by Levitt and Pinnell (1995) with similar assumptions.

The proximity of the 3.2% figure to Mullen et al.'s (1994) 3.8% figure for penetration suggests that a strict definition was applied in this study. This was probably done to avoid mis-interpretation of "non-contact forms of abuse," which in other studies (including the U.S. Project on the Status and Education of Women of 1978) have been labelled "harassment" but not "abuse," *per se*. This confusion is critical in studies where a researcher or clinician inquires about abuse, and labels harassment as such. It confounds the results because of what Ofshe and

Watters (1994, p.308) term the *"patient's propensity to redefine their experiences in terms of the questions the doctor asks."* For example, receiving an obscene phone call might be construed by the subject as sexual abuse, if the researcher provides a definition conducive to this misinterpretation.

Next, in order to apply the correct statistical analysis using conditional probabilities, we must consider the common claim that "nobody would make up abuse if they weren't abused" as implying that the probability of a memory of abuse, given that one was not abused is about zero (or "*quite minuscule*"; Kristiansen, 1994b). This leaves the probability that no memory of abuse will be present in unabused people as equal to approximately one. With these numbers, we can now work out the conditional probability of a repressed abuse history in people who have no memory of abuse through a simple application of Bayes' Theorem.

Bayes' Theorem (Bayes, 1763/1958) is commonly used to infer the probability of an antecedent event, given a consequence of known conditional probability, given the presence and absence of the cause. Thus, for consequent C and antecedent A , (where '~' implies "not" and '|' means "given"),

$$p(A|C) = \frac{p(A)\cdot p(C|A)}{[p(A)\cdot p(C|A) + p(A)\cdot p(C|\sim A)]}$$

Adopting a notation where A = abused, ~A = not abused, M = a memory of abuse is present, and ~M = a memory of abuse is not present, we can see the computational steps involved in Fig. 1. Essentially, the prior probabilities (the base rate of abuse and non-abuse in the population) are multiplied by the conditional probabilities of remembering and not

----------Insert Fig. 1 around here----------

remembering to arrive at the joint probabilities in the rightmost column. The steps in the calculation of p(A|~M) are given as an example, where it can be seen that the proportion of those not remembering abuse in the population is the sum of two proportions: those who were in fact abused and did not recall, and those not abused who do not recall. The proportion of this

total who were in fact abused is equal to the required probability, p(A | ~M). Expressing the numbers claimed by Williams in these terms, and disallowing FMS, we obtain the following:

p(A) = .3, p(M | A) = .62, p(~M | A) = .38

p(~A) = .7 p(M | ~A) = 0, p(~M | ~A) = 1

Applying Bayes' Theorem provides the following:

P(A | M) = 1.00 P(~A | M) = 0

P(A | ~M) = .140 P(~A | ~M) = .860

Applying Bayes' theorem to Williams' data, we obtain a result that runs counter to the notion that the absence of memory for abuse is a “symptom” of repression. Specifically, if no memory is present then the chances that a person was *not* abused are over six times as high as the chances that they were in fact abused, and so the assumption that abuse is likely in a case where no memory is present (e.g., Blume, 1993, March; 43% of Yapko's [1994, p. 232] sample of therapists) is not only unwarranted, but rather likely to be wrong.

These numbers have tragic implications for therapists who interpret denials in the face of a "diagnosis" of abuse as "defensive avoidance" rather than as accurate statements of fact.[17] They are much more likely to erroneously identify non-abused clients as having been abused than to correctly identify "repression," even though they are probably mistaken in their "diagnosis." This parallels other results in a related area. In Wakefield and Underwager's (1994) review of Bayesian analyses of child sexual abuse allegations, false positives were the predominant error, meaning that the probability of no abuse having occurred, given an allegation was made, is much higer than the probability that abuse occurred but no allegation was made.

We may also note here that this result can be reproduced on other self-report data concerning recovered memories. Feldman-Summers and Pope (1994) report on a sample of psychologists where p(A) = .218 and p(M | A) was .595 . If we grant the accuracy of these reports (despite the fact that 50% were uncorroborated), then using the same model as above, if

[17] For example, Edwards and Derouard (1995) cite such positive factors as being focused and determined at work, having a sense of humour, keeping peace in the face of impossible odds and being able to persevere as merely pathological defense mechanisms that enhance dissociation.

no memory of abuse is present then it is almost nine times more likely that no abuse in fact occurred.

A Critical Reappraisal: Let us now view Williams' 38% figure critically. We must consider that of the 38% reporting no memory for the *specific incident* that was documented in the hospital records, most recalled other incidents of abuse. This is consistent with Blume's (1993, March) observation that many incidents of trauma can be compressed into an aggregate memory (see also Neisser, 1981 or Olio, 1989, on "condensing"), and is more indicative of the confusion of facts that follows trauma (Bass & Davis, 1994) than repression of a specific incident. Loftus, Garry and Feldman (1994) point to other research that indicates that a substantial proportion of people fail to recall even recent hospitalizations. Pendergrast (1995) also points out that people with a history of repeated abuse are less likely to recall the details of a specific incident simply because they would blur together, and some would simply be forgotten, thereby vitiating the need for a mechanism such as repression.

This latter possibility is particulary plausible in Williams' data, where 53% of the "amnesic" women reported other instances of abuse (1992,p. 20; but note that this figure jumps to 68% in the 1994 report). This implies that 20% of the total may have been "compressing." Another plausible explanation is offered by Loftus, et al. (1994), who suggest that the memory for the incident was so distorted that it appeared to be a separate event, rather than another actual episode of abuse, thus leading to misclassification by the researchers.

Only 12% of the whole sample appear to have reported remembering no incidents at all, and so this is viewed by critics as an upper limit to the arguable rate of "amnesia" for the events. This number also mirrors the 9% rate of "never thinking about the trauma again" observed in Christianson and Loftus' (1990) study of personal traumatic memories. They did not identify this as "repression" *per se*, but is not inconsistent with Williams' definition.

This estimate is slightly lower than the 19% rate reported in female out-patients at a substance abuse clinic by Loftus, Polonsky and Fullilove (1994). However, before increasing the 12% estimate, we should note that this latter figure may be inflated by at least two key factors; one, the inclusion of organic or chemically-induced forgetting in that population; and two, the inclusion of subjects who deliberately tried not to think about the abuse for a time, and so classified themselves as having "forgotten."

Changing the above probabilities to reflect a 12% forgetting rate,

p(A) = .3, p(M | A) = .88, p(~M | A) = .12

p(~A) = .7 p(M | ~A) = .0, p(~M | ~A) = 1

Bayes' Theorem gives:

P(A | M) = 1.00 P(~A | M) = 0

P(A | ~M) = .049 P(~A | ~M) = .951

and so "no abuse" is nineteen times as likely as "abuse," given that there is no memory of it. Indeed, if the estimates of rates of abuse that hover around .3 are themselves exaggerated by definitions that include non-traumatic incidents, incidents of harassment and self-report bias such that .15 is a more accurate figure, then "no abuse" is over 47 times as likely as "abuse," given no memories.

In short, while p(~M | A) is most defensibly around .12, the probability of an abuse history in the absence of any memory for it is less than .05. *Even disallowing* FMS, some alternative must be the case, and should be seriously considered, particularly in clinics where more than 1 in 20 or 30 people who have no memory of abuse are nevertheless recalling it after intensive RMT.

It should also be noted here that Williams (1994a) states that research indicates up to 8% of allegations are fabricated. Without evidence, she claims that this rate was probably lower in the 1970's[18]. Ignoring the latter claim, her observation that 3 of the 136 subjects were dropped from the study for admitting falsification, this implies that up to 8 more cases were possibly fabricated (8% of 136 is 11, less the three that were dropped). On the plausible assumption that they would not report remembering because they knew the allegation to be false, we subtract this

[18] It can be argued that, as with the recent boom in DID diagnoses, awareness of sexual abuse as a problem might increase the actual *number* of false allegations thereof. However, the *proportion* of false allegations is subject to the problem of non-reporting of abuse observed by many RMT writers (e.g., Bass & Davis, 1988; Herman, 1992). In the 1970's, the shame of being abused was strong motivation to not report it. This must be weighed against the fact that whatever motivations underlie false allegations are far more likely to result in a report being filed. Thus, the effect of decreasing shame in reporting is probably balanced by the effect of the presumed increase in false allegations, which are still far more likley to be reported (almost by definitinon). In short, when looking at the ratio of false to factual allegations, we cannot ignore a change in the numerator without considering a compensatory change in the denominator.

group from the "no memory" group, which lowers the proportion not reporting from 12% to 5.8%.

Williams (1994a) claims that this has no bearing on her research because there is no evidence about falsification in retrospective adult studies. But this is not the issue, because the false allegation rate pertains to the original records from the time the women were children, and not the retrospection done in the study. Her argument is specious. Assuming a 30% abuse rate and 5.8% repression, with false memories held to zero, the odds against an abuse history in a person with no memories of it still exceed 40:1, and increase from there as we tighten the definition of abuse, restricting its prior probability.

Abuse, Symptoms and Clinical Populations: An objection to the preceding is that population rates do not apply to clinical populations, i.e., the '1 in 3' figure is an underestimate. It has been argued (e.g., Blume, 1993, March) that few people enter therapy who were not abused, and so the probability of a history of abuse, given that a person is in therapy, is much greater than about .30. Indeed, Blume (1993, March) seems to imply that it is near one, while Herman (1992) claims a "modest" 40-70% (p. 122). In this section, I will examine what the literature implies about this topic.

To arrive at an estimate of this probability, we must first consider that people typically arrive in therapy as a result of symptoms, and, with a notable exception (Elliott & Briere, 1992), the evidence to date appears to have focused on clinical populations assumed to have been abused rather than comparing abused and non-abused groups. Thus, the lack of a control group is a serious methodolgical problem in this literature. Nevertheless, one rationale for this approach is noted by Browne and Finkelhor (1986) who point out that the use of control groups

> *"may actually underestimate the types and severities of pathology associated with sexual abuse, because problems that sexual abuse victims share with other clinical populations will not show up as distinctive effects,"* (p. 76).

While this is an important methodological consideration, it underscores an important point: namely, that the symptoms purported to be diagnostic of abuse are not, in fact, *unique* to abused groups. The position of Browne and Finkelhor (1986) also seems to assume that the probability that an abused person will land in a control group because they have no memory of

being abused is fairly high. However, the figures cited above, based on Williams' (1992, 1994) study, would seem to suggest that the problem is not severe.

There is substantial evidence that the symptoms purported to be due to abuse are not unique to that factor (Rind & Harrington, in press). One problem is that memory deficits on a variety of cognitive tasks have been linked to depression (Burt, Zembar & Niederehe, 1995). Consistent with clinical observations, their meta-analysis indicated that greater deficits were apparent in younger depressed patients and unipolar (as opposed to bipolar) depressed groups on verbal tasks. The effect was more marked for inpatients than for outpatients for recognition and recall, although depressed individuals show markedly less impairment than other psychiatric groups (e.g., dementia). The clinical implications for RMT, then, are that in the absence of differential diagnosis, there remains a plethora of possible explanations for impaired memory.

A review of the literature on the long-term effects of childhood sexual abuse (Rind & Harrington, in press) found that clinical research tended to overestimate the effects of such abuse, with larger unbiased samples showing markedly less such effects (Rind & Harrington, in press). These authors seriously question the causal role of childhood sexual abuse in producing long-term effects, and note that a clinical focus on a non-causal factor can lead to ignoring more proximal causal elements at the expense of healing. Again, we must conclude that a focus on abuse as a causative agent is not only misleading, but potentially harmful in view of the time taken from the actual problem.

For example, Jaffe, Wolfe, Wilson and Zak (1986) found that children who were abused, those who merely witnessed familial violence and a community sample of controls exhibited comparable levels of social skills. Abused children and witnesses to violence exhibited more internalizing behaviour problems than controls, but did not differ from each other. Abused children differed from witnesses of violence in degree of externalizing behaviour problems, but by a much smaller margin than that by which the two "exposed" groups (together) differed from controls.

Another problem arises from studies of victims. A large-sample study of women by Elliott and Briere (1992) found some very trivial correlations between eight self-reported aspects of abuse and five purported "major symptoms" of abuse, namely anxiety, depression, dissociation, sleep disturbances and sexual problems. (These correlations were based on the

26.9% of the sample designated as 'survivors.') Of the 40 correlations, 27 were not significantly different from zero (using $\alpha = .01$). The rest did not exceed .12, despite very high statistical power.

T-tests for these symptoms were also carried out to distinguish those reporting sexual abuse (N = 761) from those who did not (N = 2,072). Although the effect sizes were moderate (between .27 and .40), we must recall that the probability of symptoms given abuse is not the same as the probability of abuse, given symptoms. The base rate of abuse (as defined in the survey) is 26.9%, hence the difference in group sizes. The definition of abuse that was used in the survey was not reported.

One of the observed effects was for anxiety, where a significant *t* was obtained, with an effect size of .34 . If we define a person as 'clinically anxious' who scores more than 1 standard deviation above the aggregate mean (i.e., above the 86th percentile of the population), then the proportion in each group that would be considered 'anxious' can be estimated from the group means and standard deviations. The cutoff is established from the aggregated data, and then the z-value of the cutoff with respect to each group's parameters is established. This permits estimation of the proportion in each group that exceeds the cutoff. However, to allow for the base rate of abuse, we convert these to frequencies and apply the final step of Bayes' Theorem.

Of the 'abused' group, approximately 187 score above this cutoff on anxiety, while in the 'no abuse' group, about 274 would have been 'anxious.' Thus, the probability that a person was abused, given an anxiety z-score of 1 or more is [187/(187+274)] = .41, and the probability that they were not is .59. Hence the odds of abuse given high anxiety are around 1.5:1 *against.* This holds even when we define anxiety as 1.645σ or more above the mean. A $+1\sigma$ definition for depression gives a probability of abuse given depression equal to .43, which is approximately equal to the value we obtain for the probability of abuse given sexual problems. We obtain a slightly lower result with dissociation (.39).

All of these values shrink rapidly when we adjust downwards for the 26.9% rate of "abuse," which is suggestive of the use of an overly broad definition thereof. Table 3 shows the increase in odds **against** an abuse history across a range of base rates. These odds are based on the definition above, $p(\sim A \mid \text{score} > 1\sigma) / p(A \mid \text{score} > 1\sigma)$ across each these variables.

----------Insert Table 3 around here----------

It could be argued these figures are skewed by the possibility that a lower baserate for abuse would reflect more severe abuse, and more severe abuse would increase the likelihood of symptoms. This argument fails because the complete lack of meaningful correlations between symptomatology and severity measures implies that severity of abuse is independent of symptoms such as whether or not penetration occurred, whether the incident(s) involved physical force and/or threats. For example, less than 1% of the variance in dissociation was accounted for by use of threats ($r^2 = .008$, $p<.01$), with near-zero values for all other analyses. Indeed, Williams' (1994) data has a near-significant t-test that suggests that severe abuse may be *more* likely to be recalled than less-severe abuse,[19] (Williams, 1995).

Even as they stand, these values are somewhat *below* 50%. Accordingly, this study supports quite strongly the contention that that these symptoms are *not* unique to abuse. Indeed, they must be primarily due to factors *other than* abuse. Their diagnostic utility for detecting an abuse history is therefore suspect, since the odds ***against*** a history of abuse given the symptom exceed 1:1 in every case, and exceed 2:1 for all variables when the assumed base rate drops below 20%.

Similarly, a recent study by Kendall-Tackett, Williams and Finkelhor (1993) examined the literature on the incidence of purportedly abuse-related symptoms in both abused and non-abused groups of children. From their report, it appears that for a variety of symptoms, p(each symptom | Abuse) = .25. Interestingly, this value is similar to the 27% of children who met DSM-IIIR (1987) criteria for post-traumatic stress disorder (PTSD) after a shooting event at their school (Schwarz & Kowalski , 1991), although it is slightly higher than the 16.3% unweighted average prevalence of PTSD noted across studies of Vietnam veterans reviewed by Boyle et al. (1989).

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[19] The 1994 paper reported that more severe abuse was *less* likely to be recalled, as a result of a typographical error pointed out to Dr. Williams by Evan Harrington. This error was corrected in a later erratum notice.

Comparable values are implied by the National Hospital survey reported by the Canadian Committee on Sexual Offences Against Children and Youths (1984). Only 30.1% of female and 14.9% of male sexually abused children exhibited distressed or uncooperative behaviour, as judged by the examining professional. Fully 32% were cooperative and not distressed, while 26.4% exhibited other reactions such as fidgeting, nervousness, flippancy and so on. Less than 15% of this latter group (just under 4% of the total) exhibited personality disorder of some type. Psychological and behavioural reactions were either absent or not reported in approximately half of victims of either gender, with about 17 or 18% judged to be susceptible to long-term negative effects. Where reported, an average of 3.4 symptoms for females and 2.1 for males was noted. No single symptom appeared in more than 12.8% of either group. Unnecessary persistent fears, disturbed sleep patterns and nightmares were most common in both groups (12.8%, 11.8% and 12%, respectively in females, and 6.8%, 6.8% and 8.1% in males). Angry outbursts, irritability, change in behaviour at school and acting out/running away were also somewhat more common in males, although not by a large margin (all fluctuated between 6.8% and 9.5% in males and 6.2% and 6.9% in females).

The .25 value is also a good approximation to the estimated incidence of psychopathology in female victims of childhood sexual abuse that Browne and Finkelhor (1986) arrived at in their review of the literature on effects of such abuse. It should be noted that up to half of victims appeared to by asymptomatic, with p(asymptomatic | abuse) ranging between .21 and .50 . This implies two things. One, that no single symptom is diagnostic of sexual abuse, and two, even *patterns* of symptoms may not exist in many victims. This is consistent with Ney, Fung and Wickett's (1994) observation that in their sample of 167 abused children, the most serious cases exhibited patterns of abuse that *excluded* sexual abuse.

Given this, one must have some reservations about claims that *"People who have endured horrible events suffer predictable psychological harm,"* (Herman, 1992, p. 3). If this is true, then researchers have clearly impeded their ability to find predictors by including unharmed people who fit a too-broad definition of "abuse." If it is not true, then it is quite impossible to infer horrible events given only psychological problems, because such problems have only a loose association with specific events. Indeed, the huge variety of symptoms that are alleged to be

"caused by" sexual abuse (Poole, Lindsay, Memon & Bull, 1995) suggests that there is very little agreement among professional clinicians about what symptoms are indicators. The implication of this is that claims about *predicatable* harm are at the very least exaggerated.

In Kendall-Tackett et al. (1993), the conditional probability p(Symptoms | ~Abuse), appears to be approximately p = .50, based on case-control studies that indicated clusters of symptoms are equally likely to occur in both abused and non-abused groups. These authors expend some effort in attempting to dismiss this finding by suggesting that the symptomatic control groups weren't really control groups because their symptoms indicate that they were abused but just didn't report it. Of course, this argument assumes what is to be proven, namely that symptoms are due to abuse. To do otherwise ultimately contradicts the authors' intent of demonstrating unique effects of abuse, as per Browne and Finkelhor (1986).

In fact, their claim that *"The actual frequency of such symptoms in the population of abused children can be an important guide to clinicians in diagnosis and treatment,"* (Kendall-Tackett et al., 1993, p. 167) completely ignores the Bayesian requirement that this information be combined with rates of symptoms in non-abused children in order to arrive at a proper diagnosis of abuse, or in other words, a statement about p(Abuse | Symptoms). They are plainly confusing this latter probability with p(Symptoms | Abuse). As has been reiterated time and again (e.g., Carver, 1978), and has been demonstrated above, these two probabilities are absolutely *not* the same thing.

Again employing Bayes' theorem, the mathematical implications of the results just reviewed are clear. If, as just seen, p(Symptom|No abuse=.5), and in general, about a quarter of abused individuals develop symptoms (i.e., p(Symptom|Abuse=.25) then with a prevalence of abuse estimate set at 30%, we arrive at p(Abused | Symptoms)= .176. In contrast, p(~Abused | Symptoms) is 4.7 times more likely, at .824 . Thus a general clinical sample where these proportions hold would actually have rate of history-of-abuse substantially *below* the assumed 30% population rates of abuse. (This may not apply to substance abuse groups, where the rate may indeed climb to 54%; Loftus, Polonsky & Fullilove, 1994).

A Revised Estimate of p(A /~M) in Symptomatic Populations: Given the preceding results, a general clinical sample would likely have a history-of-abuse rate substantially *below*

the commonly-cited 30% population rates for abuse. (This may not apply to substance abuse groups, where the rate may indeed climb to 54%; Loftus, Polonsky & Fullilove, 1994). This could imply one of two things. One, that the assumed population rates are exaggerated due to the retrospective nature of the studies on which they are based. Two, if repression or dissociation occurs in response to trauma, then it is actually a relatively successful adaptive process that works fairly well - until it is interfered with (see also Erdelyi, 1990, p. 19) for an interesting discussion of this point).

Naturally, if the population baserates for abuse have been overestimated by inclusion of non-traumatic events in the definitions of abuse, then it is possible that the 30% figure corresponds quite well to what would be expected from RMT claims. This would make the hypothesized success of repression/dissociation somewhat less plausible, but it still would not significantly alter the very high likelihood that people who do not remember abuse were not, in fact, abused, as will now be shown.

Re-working the prior probability of abuse to reflect $p(A \mid Symptoms) = .176$ in clinical populations, we assume $p(M \mid A) = .88$ with $p(M \mid \sim A)$ (i.e., FMS) held to zero. Following the results worked out above, we find that if a symptomatic person does not have a memory of abuse, it is 39 times more likely that "lack of abuse history" is the explanation than "repression or dissociation." When the definition of abuse is strictly limited to fairly severe sexual abuse, and the prior probability reduced to 3.2% (see above) the odds ratio exceeds 250:1 against. Hence, it would appear that even in therapy, $p(A \mid \sim M)$ is sufficiently small that other causes of symptomatology are much more plausible, and should clearly be investigated before a repressed or dissociated abuse history is even considered.

We should also note here that the .176 figure derived above actually corresponds quite closely to the .197 rate obtained in the retrospective study by Martin et al. (1993; cited above). The high rates of sexual harassment in adolescence (Roscoe, Strouse and Goodwin, 1994) and the inclusion of events more accurately defined as harassment in "abuse" definitions (such as obscene phone calls; Maynes and Feinauer, 1994) must also be taken into account. If we could compensate for these factors, the rates of 18% to 30% would be expected to drop markedly towards the lower rates found in more strictly designed studies. As well, the .176 figure was

based on an assumed base rate of 30%, and shrinks as a function of definitional changes. Furthermore, if we take ‘symptoms’ as a very broad term, then by definition, anyone in therapy who was not abused has symptoms, and by the same logic, p(S|~A)=1, resulting in p(A|S)=.097 with a 30% baserate. This is not entirely unwarranted, given the findings of Poole, et al. (1995) regarding the massive disagreement among therapists about what symptoms are supposed to be signs of an abuse history

A central concern in the debate is the rate of familial abuse, which is markedly lower than the general prevalence rates in the population (in spite of unsupported claims to the contrary; e.g., Whitehead, 1992, p. 154) . Base rates for incest are estimated by at least one pro-RMT researcher at below one-quarter of the cases, or less than 10% of the population, with father-daughter incest as quite rare, at about 1% (Herman, 1981). The latter estimate, coupled with 12% "repression," provides posterior Bayesian odds of 825:1 against father-daughter incest in a person with no memory of it.

Naturally, none of the preceding rules out the need to ask at intake whether a memory is present (Enns et al., 1995; Yates, Kathol & Carter, 1994, p. 17). It does, however, preclude premature suggestions from the therapist that a negative answer to this question is probably attributable to repression, dissociation or some other such construct.

Unfortunately, such questions have been recommended by some writers as proper methods for screening. For instance, (Bala, 1994, p. 929) suggests *"Examples of possible questions include...'Sometimes women with these symptoms tell me they were sexually abused as a child; was that your experience?'"* The suggestion that the symptoms are due to abuse is already there, and all the woman has to do is re-define some experience as abuse. Thus Bala recommends that:

> *"the physician's questions can give the victim permission to begin to reframe her experience for the first time. When past abuse is suspected, a negative response should be respected, but does not rule out the possibility that abuse occurred. The continuity of the doctor-patient relationship will allow the question to be addressed again."*

If the patient does not report abuse, she is forced to wonder why she has those symptoms. Unfortunately, in RMT she is too often given but one alternative to consider, and so has very little choice in the matter.

Suppose that a physician is misled into believing that the *"denial, repression, dissociation, minimization or intellectualization"* (Bala, 1994, p. 929) are more likely alternatives to the "nothing happened" explanation of negative responses to such questions. Perseveration on the matter by the doctor will qualify as a barely-disguised suggestion that it occurred but that the patient just doesn't remember or is "in denial," - a suggestion to which suggestible people would be expected to respond (see below). As indicated by the American Psychiatric Association (1994), the British Pyschological Society (Morton et al., 1994) and the International Society for the Study of Dissociation (Barach, 1994), a neutral stance towards the historical veracity of recovered memories is imperative. Perhaps a recommendation about reifying the abuse-symptom-repression relationship without clear qualifiers is also in order (see also Sarbin & Mancuso, 1995). Nevertheless, where more than a small minority of clients are recovering memories of childhood sexual abuse in a given office, an alternative explanation should be sought.

The Reliability of Continuous Memories

Finally, it should be pointed out that none of the above may be used to discredit the large majority of victims who have not forgotten the abuse. From the same analyses, we can calculate that A is many times more likely than ~A, given that the memory has never needed recovering, even if we grant FMSF estimates of the rate of FMS in therapy. For example, FMSF Board member Joe DeRivera estimates a comparatively low rate of FMS in Massachussetts:

> *There are 59,500 licenced mental health workers in Mass! If only 1% are incompetent and if each has 20 patients (I think both figures are low),and if 15% of patients are susceptible (hypnotizability) we have 1,785 cases in Mass (and less than 100 reported to the [FMS] foundation).* (Personal communication, Tue, 27 Apr 1993)

Doing the math, this produces a clinical rate of FMS around .0015, or 1 false memory in 667 cases. Applying this to the conservative base rate estimate (17.6%), this produces the odds ratio $p(A \mid M)/p(\sim A \mid M) = 125.3{:}1$, indicating that memories that have never been "repressed" (and so never needed "recovering") are many times more likely to be refer to actual trauma than they are to be simply false.

This last recognition carries with it an ethical burden for the therapist who deals with a "recovered" memory, particularly when action is taken against an alleged perpetrator of abuse. The *hindsight bias* (Dawes, 1994, p. 130) is the tendency to conclude that "we knew it all along" while not actually being able to recall what we believed before an outcome was known. Clients who have no memory to begin with, but who start re-interpreting the past to fit with the new memory quite simply do not fit into the "continuous memory" category. This distinction must remain with the therapist, who cannot conspire with the client in this kind of re-interpretation, because to claim that a "recovered" memory was there all along bestows an undeserved measure of credibility upon the memory. Recovery without validation is unfortunately indistinguishable from confabulation.

Validation of Recovered Memories

Two studies have addressed the problem of finding corroboration for memories alleged to have been "recovered" after a period of "repression." Both suggest that some recovered memories may be validated. However, the first study depends on a failure of 'corroborated' and 'uncorroborated' groups to differ in order to make the inference that lack of corroboration is due to circumstances, rather than falseness of the memory. This fails because of demand characteristics of the study. The second study clearly indicates the unreliability of uncorroborated recovered memories.

Simulations and Expectations: Belicki et al. (n.d.) compared 70 people (mostly women) who reported "no abuse," 68 "abused" subjects and 35 reporting "no abuse" but who filled out the questionnaires as "simulators," (i.e., they were to fill them out as if they had been abused). The measures were an abuse history inventory (designed for the study), which included four increasingly narrow definitions of abuse, ranging from the subjects' own personal definition of "abuse" to outright sexual contact before 16 with someone at least 5 years older. Responding "yes" to any of them classified the individual as "abused." They were further subdivided according to "sexual abuse only," "physical and sexual abuse" and "physical abuse only." "Emotional abuse only" subjects were dropped from the study.

Subjects were asked about their memories of the abuse, rating it on a four-point scale from "always remembered" to "still don't remember it," (with the latter qualified by "someone told me about it"). The middle two categories ("regained" and "in process of regaining" the

memories) were collapsed for analyses into a "disrupted memory" group. They were also asked about whether they had discussed the abuse in therapy, but unfortunately, no data are provided on this matter. The other inventories were measures of emotional and physical dysfunction, including the Beck Depression Inventory, the Clinical Anxiety Scale, the Trauma Symptom Checklist, the Trauma Impact Inventory, the Dissociative Experiences Scale, the Pennebaker Inventory of Limbic Lanquidness (sic) and a physical health inventory. A miscellany of other questionnaires were filled out about such things as sleep and dream experiences, stress and coping, etc.

The subjects were undergraduate psychology students. A MANOVA on the 7 DV's discriminated 'no abuse' subjects from the rest. However, it also revealed some trivial differences between simulators and sexual abuse groups. The direction of these differences is downplayed in the paper. This is perhaps because it leads to the idea that the whole sample had prior notions about repression and the necessary components of the role of "survivor" from their psychology classes, or through popular media accounts of "repression."[20]

Retrospection aside, this confound is the fatal flaw in the study. Twenty-five percent of the simulators (compared to 44.6% of the 'abused' groups) reported disrupted memory, and 5.6% reported absolutely no memory for the abuse (compared to none of the abused groups), indicating that they were aware that the investigator expected to see 'repression' stemming from 'abuse.' As well, comparison of the means across the 'no abuse' group and the three abuse groups indicates that on the symptomatology checklists, where 'simulators' differed from 'abused' groups, they were almost invariably *higher* on symptoms, suggesting strongly that the role of the sexually abused victim was clearly defined in their minds before the study. Only on Physical Health - a domain not usually viewed as being tied to past sexual abuse - were they 'less pathological.'

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[20] Dr. Belicki (personal communication, Oct. 5, 1995) has pointed out that the lecture material for the course in which the subjects were enrolled did not cover childhood sexual abuse or the "forgetting" of trauma through one mechanism or another.

Age at which the abuse occurred was also strongly predictive of memory disruption (61.5% of those reporting childhood abuse, but only 6.3% of those reporting abuse later in life). Simulators invariably indicated 'childhood abuse,' but not abuse later in life. The authors predicted that simulators should therefore have a high rate of memory disruption. However, why memories that were never there should be disrupted is not made explicit. Implicitly, though, it reveals the expectation that these students should 'know' that childhood abuse gets 'repressed.' This demand characteristic was obviously conveyed to the subjects. The fact that fewer simulators (at least 25%, and possibly over 30%[21]) reported memory disruption than did in the Sexual Abuse group (44.6%), is less significant than the fact that so *many* simulators were aware of this expectation.

The definition of 'abuse' that was checked off was also evaluated. 'Some' versus 'all' definitions was the basis for grouping 'abused' subjects, and they were compared to the 'no abuse' groups. The 'some' and the 'all' groups did not differ on anything but the Trauma Impact Inventory, but both differed from the 'no abuse' group. Unfortunately, simulators were not compared here. As well, there was no indication as to the degree of overlap between the provided definitions and the subjects' own definition, so we don't know how much was due to possible classroom learning or other knowledge of cultural notions about repressed memories.

Corroboration items were embedded in the questionnaire, to find out whether the alleged abuser had acknowledged doing it, been prosecuted for it, or if someone else had made similar claims about abuse by the same person . Presumably a 'yes' to any of these items put the subject in the 'corroborated' group, although this is not clear in the paper. Simulators did not differ from sexual abuse groups in rates of reported corroboration, (45.2% vs. 60.3% respectively), again suggesting that they had a good idea about these rates before going into the study.

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[21] If the two who claimed to have no memory at all were not a subset of the 25% then 30.2% reported either disruption or total lack of memroy. If they were included in the 25% then this figure stands. The correct interpretation of this is not clear from the paper.

The strongest evidence for the validity of recovered memories cited by Belicki et al. is the failure of the uncorroborated and the corroborated groups to differ on psychopathology. However, since simulators appear even more pathological than the 'abused,' this population appears to have been well-versed in the "myth of repressed memory," (Loftus & Ketcham, 1994). It is hard to believe that people who infer they were abused on the basis of symptoms that they have learned to expect stem from abuse would **fail** to report such symptoms, whether or not the abuse was 'corroborated.' Thus, the corroborated and uncorroborated groups would not be expected to differ from anyone save those who do not believe that they were abused, particularly when statistical power to detect a difference is severly limited by the small sample. In short, a plausible alternative explanation of the results of this study is that the students were simply aware of what was expected, and provided it. The failure to attain significance in the direction opposite to that hypothesized is probably attributable to low power.

Memories Improbable: With a similar goal, Herman and Schatzow (1987) examined a small clinical group of female outpatients in an effort to establish a rate of verification for recovered memories. On the surface, their data appeared to indicate that a large proportion (74%) of the memories were corroborated, and that degree of reported amnesia was unrelated to ability to corroborate. However, the surface of this study masks some rather deep flaws.

First, it should be noted that although over a quarter of the sample had been classified as "severely abused," a full 40% of this latter group never actually recalled any abuse. They were prescribed a program for the abused even though they had no memory of being abused, and despite the small probability that any of them actually *were* abused, as demonstrated above. In addition, an unreported proportion had been subjected to sodium pentothal or sodium amytal "truth serum," a brace of unvalidated tools in the memory recovery armamentarium (Hanley & Schmidt, 1977, p. 286; James & Levy, 1994; see below) that are more commonly used in neuropsychiatric diagnosis as a result of its ability to anaesthetize portions of the brain.

Second, amnesic subjects were *"encouraged to retain as much voluntary control as possible over the process; for example, by seeking or limiting exposure to sources of information that might stimulate memory..."* (p. 8). Interestingly, even though 60% "remembered" abuse,

> *"Almost all of the women who entered the group complaining of major memory deficits and who defined a goal of recovering childhood memories were able to retrieve previously repressed memories during group treatment,"* (p. 8).

Thus, those who truly wished to remember something generally did so, with the support of therapists and other believers.

Third, although 76% of the entire sample were reported to have obtained some kind of validation, the "severe amnesia" group constituted 26% of the entire group. Herman and Schatzow do not report corroboration statistics broken down by "amnesia" groups, and so it is quite possible that the individuals in the "severe amnesia" group were unable to find corroboration precisely because their memories were largely inaccurate constructions suggested by therapists or created under sodium amytal. The fact that a large number of the subjects had already been placed in an incest survivors' treatment group ***despite having no clear memory of the abuse*** indicates strongly that therapists had already suggested to them that they were abused, and the women had complied in the hope of getting better. Further compliance in the research would not be surprising. Furthermore, the suggestion that the subject was abused might not be denied because ordinary childhood amnesia (see below) precludes the remembering of contradictory evidence.

While Kristiansen (1994b) suggests that *"if you contact Judith Herman, you will learn that the ability to obtain corroboration was totally independent of the degree of memory loss,"* the reason for not publishing this important finding is unknown. However this still leaves 24% of the "severe amnesia" group uncorroborated, which is more than enough to make pseudomemory more plausible than abuse (at least, in the absence of verification and the presence of disconfirmation).

Fourth, whether this evidence speaks to the probability of validation given abuse, or in our notation, $p(V \mid A)$, must be considered. Again, this latter probability must not be confused with $p(A \mid V)$. Even granting the contention that all of these individuals *were* in fact abused, and allowing no false verifications (i.e., $p(V \mid \sim A)$ is held to zero), then assuming (as per the above) that $p(A) = .176$, Bayesian analysis gives us the odds ratio $p(\sim A \mid \sim V)/p(A \mid \sim V) = 18:1$. This again implies that in the absence of verification, the abuse hypothesis is quite unlikely.

Even allowing p(A) = .3, the odds are about 10 to 1 against it. This figure is comparable to a similar value that can be derived from Belicki et al. (n.d.). In that study, the probability of no abuse, given no validation (p(~A | ~V) is 5.9 times as high as p(A | ~V) when we assume a 30% base rate of abuse and the impossibility of validation when no abuse occurred. These odds exceed 11.7:1 assuming the more reasonable 17.6% base rate.

It should be noted that Herman and Schatzow's (1987) study does not permit direct inference about the validity of *recovered* memories, per se. Thirty-eight percent of the whole sample experienced "no amnesia" for the events, and this reflects closely the 40% figure that represents the number of cases of direct corroboration of events. Now, if all of the "no amnesia" group were able to corroborate their memories (as would be expected from the analyses in the previous section), then the remaining 36% of at-least-partially corroborated cases would come from the 62% who reported experiencing amnesia. With these estimates, p(V | unrecovered memories) jumps to .87, while p(V | recovered memories) drops to .67 , suggesting that indeed, recovered memories are markedly less reliable than never-forgotten ones, although they are not entirely unreliable.

As a last point, weak confirmation of abuse was viewed as validation by Herman and Schatzow while strong disconfirmation was never considered to be invalidation. The issue of standards of proof is apparently recondite to these writers, because they do not hesitate to classify people as abused and the abuse as corroborated even when the only evidence is that at least one family member suspects that something happened, or the same alleged perpetrator did something similar to someone else. Societal demands for proof are less extraordinary that standard apparently required by these recovered-memory writers. The latter are extraordinarily inconsistent, and reveal the tenacity with which supposedly objective psychologists cling to the abuse assumption in the face of logic and evidence. Sadly, the unfortunate effect of the validation of one dubious memory is even documented in the paper. In this case, the support for an unvalidated memory of abuse from the therapy group appears to have led an allegedly-abused subject to sever all ties with her family, change her name and find an *"alternative social structure"* for traditional family occasions (p. 12).

Others also accept some notions wholesale on the one hand, while rejecting claims based on the same type of evidence (or better) on the other. For example, Terr (1994, p. xii) reports on the sudden enlightenment she had when reading a single paper reporting *"a newly discovered condition...called the battered child syndrome."* Uncritically adopting that as a model on the basis of one paper, she then rejects False Memory Syndrome as a *"cause [that] is being confused today with a diagnosis...There is no such disorder,"* (p. 164) despite the large amounts of scientific research that support the idea and that are sketched below.

We can see now that both therapists and researchers are human beings who are not immune to the biases of judgement than can arise when strongly held beliefs are challenged. Indeed, research by Kruglanski, Webster and Klem (1993) supports the idea that need for cognitive closure underlies not only the persuasibility of the uninformed, but the rejection of appeals to evidence by people who believe themselves to be knowledgeable based on a body of knowledge which does not have to be complete to be convincing.

Summary: Recovered Memories

If a person has no memories of abuse, it is unlikely that they have a repressed history of abuse, particularly if it is uncorroborated in any way. This is true even in symptomatic (and presumably clinical) samples. However, some clinicians are defying the odds in a remarkable manner, with case after case telling many of the same kinds of stories of childhood trauma (especially in DID; see Hammond, 1992; Mai, 1995).

Repression or dissociation are invoked as explanatory constructs, despite the very weak or nonexistent scientific basis for the remarkable memory systems that they require. Importantly, the improbability of symptoms, given abuse leads to the conclusion that other causes are as likely, if not more so, to be the source of the disturbances. If abuse is not the root of the problem, then recovered memory therapy is unwarranted and potentially dangerous, especially if it has no demonstrable benefits, as suggested by Lindsay (1995).

Advocates on the other side of the debate prefer more parsimonious explanations for the phenomena discussed above. They point to the common threads in the techniques that these clinicians use to "recover" memories. These techniques, it is pointed out, are well-known for producing memories that can come to be strongly believed-in, despite their origin in the imagination of the client, the therapist, or both. This is the essence of the risk involved in RMT.

False Memory Syndrome

Although even some ardent proponents of recovered memories acknowledge that some proportion may be false (e.g., Kristinasen, 1994b), it is worth examining some of the data that show this quite clearly. Moreover, the evidence for false memories comes from a variety of sources. The paradigm of Satanic Ritual Abuse is a classic example where false memories are assumed to be true by some clinicians[22]. Individuals who have retracted allegations upon discovering their memories to be false are another source. Finally, the memory phenomenon of childhood amnesia reveals the spuriousness of a substantial number of memories commonly cited in RMT literature.

Having demonstrated that a substantial number of recovered memories are likely to be at least distortions, at worst outright pseudomemories, a sample of methods known to produce pseudomemories will be examined. This will include hypnosis and sodium amytal therapy, two methods commonly associated with RMT. In the final part of this section I will consider false memories and their creation in the context of a reconstructive model of memory.

Satanic Ritual Abuse

One class of "recovered memories" used as evidence that some recovered memories are false is a set of bizarre memories that fall under the heading of "Satanic Ritual Abuse" (SRA; e.g., Goodwin, 1994a, 1994b; Sinason, 1994; Young, Sachs, Braun & Watkins, 1991). The Ontario Centre for Religious Tolerance (OCRT; 1995) defines Satanic Ritual Abuse (SRA) as follows:

> *Satanic Ritual Abuse has been defined as psychological, sexual, and/or physical assault committed by two or more people whose primary motive is to fulfill a prescribed ritual involving worship of the Christian devil. The terms Sadistic ritual abuse, cult related abuse and ritual abuse are used to define similar mistreatment which is not specifically tied to Satanism.*

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[22] Some clinicians (e.g., Mack, 1994) are also accepting completely unsubstantiated and sometimes physically impossible tales of alien abduction as veridical accounts (Blackmore, 1994; Dittburner & Persinger, 1994; Spanos, in press).

This organiztion (OCRT, 1995) describes unsubstantiated legends of organized baby-killing blood-drinking cults that go back to Roman times, and points out that there is no more reason to believe these legends today than there was 1800 years ago. The popular view of SRA has been defined by books that are cited as factual evidence of SRA in RMT literature (such as *Michelle Remembers*; Smith & Pazder, 1980, cited as evidence by Belitz & Schact, 1994; Burrell, 1994; Demause, 1994; Owen, 1994; Rockwell, 1994) that have reportedly been exposed as hoaxes (OCRT; 1995).

The failure of numerous law-enforcement agencies to substantiate any of the claims being made regarding SRA implies that people are coming to believe that they witnessed extreme violence and depravity when in fact they did not. The central objection to this view by those who support the notion of SRA is that a lack of evidence doesn't mean it didn't happen. A "fringe" objection is that the lack of evidence is due to widespread conspiracies among law enforcement agencies and others to hide it (e.g., Hammond, 1992; see also Ofshe & Watters, 1994, pp. 187-193). However, this must serve to remind us where the burden of proof is properly placed - on the claimant. Demands that *"these reports are entitled to be considered as true and unless clearly proven otherwise"* (Owen, 1994) are demands for the impossible - that a negative be proven.

Evidence exists that some therapists badger clients into believing and "remembering" SRA. Such practices were described in therapy by Vynnette Hamanne, who *"believed she was the victim of bizarre childhood sexual abuse involving satanic rituals, and that she had seen her grandmother stirring a cauldron of dead babies."* The creation of that memory through the use of coercive memory-retrieval techniques by Dr. Diane Humenansky was the basis of a 2.5 million dollar judgement against that therapist, who is presently facing five similar civil suits (Associated Press, 1995; Bureau of National Affairs, 1995).

Similarly, an exposé of practices at Rush Presbyterian Hospital has documented unfounded allegations of SRA that arose in therapy (Bikel, 1995). One woman reports seeing her baby sister decapitated at the age of two days (Pendergrast, 1995, p. 79), a common theme in SRA tales that has never been substantiated. Laura Pasley (personal communication, Feb. 12, 1995) wrote of the delusions of her therapist:

> *I was the only one in a large group of women that did not come to believe my family was a group of satanists, although he insisted my grandfather was in a "backward" cult, to which I impatically* (sic) *said no. Even that caused me to doubt for sure. If any of us gave him anything that might counter the claim that something happened, he insisted we did not want to get well and were in denial. One woman who had a flashback of her twin being hung in a tree later told the therapists she went and researched her birth certificate and it said "single birth." He looked at her and stated, "The coven takes care of those things."*

Closed thought systems and badgering aside, the essence of the SRA argument for false memories lies in *"the complete absence of independent evidence corroborating the existence of such cults or their alleged activities such as human sacrifice, cannibalism, and sex and death orgies,"* (Putnam, 1991, p. 175; see also Wakefield & Underwager, 1992b).

Not all mainstream RMT advocates believe in these fantasies. However, even if these are a small proportion of the claims, they do not change the relative improbability of a history of abuse, nor do they clearly invalidate unrecovered memories. They do, however, point back at an underlying process and provide clues as to the reconstructive nature of memory (Wakefield & Underwager, 1992b).

Another reply to this argument is that lack of corroboration does not necessarily imply that the events did not occur. Indeed, rare individuals throughout history appear to have committed comparable atrocities (Goodwin, 1994a) and one case has apparently been documented in the United States (Ontario Centre for Religious Tolerance, 1995). Rockwell (1994) provides a list of fourteen successfully prosecuted cases, many of which have been overturned.

One of the not-yet-overturned cases is that of Paul Ingram, who was convicted in spite of confessions obtained through coercive leading questioning and which is discussed in detail by Ofshe (1992; see also Ofshe & Watters, 1994, Ch. 8), who indicates that Ingram used autohypnosis to convince himself of the reality of the accusations. However, if huge numbers of people are reported as being killed, cannibalized or cut apart and sewn back together without scars, leaving absolutely no traces for massive international investigations to find (Lanning,

1991; Ofshe & Watters, 1994), then the suggestion that *all* of these memories are valid does appear rather absurd.

Yet another response to the SRA charge has been to accept these memories as valid, but refer to the abuse as "sadistic" ritual abuse (Bass & Davis, 1994a; Goodwin, 1994a, 1994b), thus avoiding the more bizarre "satanic" connotations. While this rhetorical device avoids the association of RMT with current conspiracy theories about rings of satanists or CIA mind control experts taking over the country that are being taken rather seriously by some therapists (e.g., Hammond, 1992), it does not deflect the charge of equivocation. Re-naming the leopard does not change its spots.

As Greaves (1992, p. 45) points out,

"Almost all hypotheses regarding the objective reality of the reports of alleged satanic cult survivors...are **a priori** *in nature based on assumption and imagination, not facts."*

While he denies that *all* reports are necessarily fictitious, he points out the need for a common language in examining the phenomena being observed by clinicians. This common language that Greaves refers to will have to build in a recognition of the most likely causes of the phenomena that are being observed.

One key source is iatrogenic processes. Ganaway (1992), for example, describes briefly a DID case where *"blatant evidence of new material of a cult-related nature"* had been introduced to the client *"through ideomotor signaling and verbal suggestion,"* (p. 121). His argument is based on the fact that the SRA memories and cultish alter personalities vanished after leaving the coercive therapist for one who strictly limited hypnosis. The disappearance of the "symptoms" upon loss of reinforcement is thus indicative of iatrogenesis.

While there appears to be a debate within the mental health community about SRA, it is arguably a tempest in a teapot. Despite hundreds of official investigations, no corroborated evidence exists to support the claims of cult-organized human sacrifice, rape, torture and cannibalism that have been made (Putnam, 1991). Lanning (1991) of the U.S. Federal Bureau of Investigation makes several key points, not least of which is that

"we now have hundreds of victims alleging that thousands of offenders are murdering tens of thousands of people, and there is little or no corroborative evidence."

As well, he notes that he overcame his initial credulousness about the allegations when he realized that victims who had never met each other began reporting the same events, and that some of these events are physically impossible. He suggests that acceptance of a claim on the sole grounds of the fact that it is not physically impossible should be tempered by the fact that perhaps

> *"whatever causes a victim to allege something impossible is the same or similar to what causes a victim to allege something possible but improbable,"* (p. 173).

But perhaps most importantly, he makes the point noted above: that the credulity of therapists in these cases is fuelling a backlash against genuine cases of sexual abuse (see also Loftus, Milo & Paddock, 1995).

The assertion that not all memories are objectively true is the cornerstone of the FMS position. Recall again the charge levelled at FMS advocates who are said to believe "all memories are not true," (e.g., Courtois, 1995). Consider the actual claim, that "not all memories are true." This is the same sort of logical confusion as noted above (i.e., that $p(\sim M \mid A) = p(A \mid \sim M)$), and is as invalid as it is inflammatory.

Even small proportions add up. As noted by deRivera (personal communcation, Apr. 27, 1993), 15% of higly hypnotizable clients of the 1% "bad therapists" in Massachussets is a large number. This figure becomes even larger when multiplied by the number of people who are affected by the average false allegation. The reports of physical impossibilities (i.e., victims being cut up and put back together, severe injuries with no scars) noted by Lanning (1991) clearly demonstrate that not all of the claims are true. Given that at least some memories are plainly false, we can begin to cut through the rhetoric which has obscured the fundamental issues of the debate (Schacter, 1995) and again begin to think probabilistically about what is going on.

The existence of the social influence techniques is clearly acknowledged in much RMT literature, however their use is restrictred to perpetrators who use a variety of methods to cover their tracks (see Hammond, 1992 and Lovern, 1993 for extreme examples of this). The possibility of misuse of such methods by therapists is not so recognized. Further evidence of the therapeutic misuse of social influence techniques comes from the retractors, to whom we now turn.

Retractors

Additional evidence that false memories are produced in therapy comes from people who have found themselves to be the victims of overzealous therapists (Ofshe & Watters, 1994; Pasley, quoted above; Pendergrast, 1995; Schacter, in press). These recantations of allegations of abuse on the grounds of impossibility or extreme improbability are dismissed by RMT advocates as essentially false memories; confabulations due to familial pressure and suggestion, or simply "false denials" (Jonker & Jonker-Bakker, 1991). However, as with SRA, the presence of logically inconsistent memories implies that one of the memories *must* be false, and so **some kind** of process exists whereby realistic memories of events that did not occur can come to be truly believed. To make this argument, then, RMT advocates must admit what what they wish to deny (or like Johnson, 1994, simply not mention) - that pressure can be brought to bear that produces false memories.

The evidence from retractors has problems, of course. It is impossible to prove a negative when the target doesn't stand still. If someone is accused of robbing a store in one part of town at 10 p.m., but a banking machine has that person on tape making a withdrawl at 10 p.m. the same night in a different city, the accusation is disproved. However, allegations of long-ago abuse are often so broad and vague as to make any rebuttal impossible (e.g., the case of Bart Stafford in Pendergrast, 1995, p. 308).

Most of the "proof" of recovered memories comes from clinical reports. Many of these are not case studies at all, but rather fictionalized distillations of clinical experiences that support the hypotheses of the writer (e.g., Herman, 1992). Distasteful as it may be to some, the acceptance of this standard of proof for recovered memories implies acceptance of the same standard for false ones. Thus, the growing body of evidence from retractors cannot be simply dismissed, especially given that at least one recovered-memory *therapist* has recanted her methods, having recognized the harm that she was doing (see Pendergrast, 1995a).

A further problem for claims of recovered memory arises from what is known about memory in very early childhood.

Childhood Amnesia

Over half of the accused individuals who responded to a membership survey by the FMSF report that the alleged events were supposed to have happened when the "survivor" was

between 2 and 6 years old. Twenty-nine percent were accused of molesting a child under the age of two on the basis of "recovered memories" of the event (Underwager & Wakefield, in press). This raises the question of whether it is even possible to remember things from that time.

It is well-known that over time, memories simply decay, and so people forget things (Schonfield & Stones, 1979). However, while this mundane forgetting is familiar to all, there is a special problem with childhood memories. Children cannot properly encode things that they do not have the cognitive structure to understand. Nevertheless, there are bold RMT claims that having "amnesia" for parts of one's childhood are "highly indicative" of a history of sexual abuse (Ratican, 1992, p. 38). Extended to the youngest years, this becomes absurd. Early writers noted a general (but not complete) absence of memories for events below the age of six to eight years (Freud, 1938). Today, it is more generally accepted that the probability of someone accurately recalling events before approximately three years of age is rather remote, with some trivial exceptions. Kihlstrom (1994) notes that these exceptions are largely based on the way children remember things, with story-telling strategies and social reinforcement of memories as the key to remembering things from about age two onwards.

This point is conceded by some RMT advocates such as Kristiansen (1994b), who cites van der Kolk's (1994) observations about inadequate neuronal development as a basis for the absence of verbalizable memories. While the argument is made for the possibility of "body memories" on the basis of the advanced development of other subcortical structures, no evidence is provided that this actually happens other than some clinical observations from Terr (1991) that are subject to a variety of interpretations (see above).

As well, Usher and Neisser (1993) view childhood amnesia as having "multiple determinants," since memory for some episodes appears to be in place by age two and a half. They used a lenient criterion for recall, and defined the "offset" of childhood amnesia as the age at which half of the subjects would recall events. Going to the hospital and the birth of a sibling had an offset of 2 to 2.99 years, while a death in the family and moving had an offset of 3 to 3.99 years. However, as Loftus (1993) points out, *"educated guesses, general knowledge of what must have been, or external information acquired after the age of 2"* are plausible alternative explanations.

The latter explanation in particular bears examination. These events fit conceptually with what would be expected under Kihlstrom's (1994) social reinforcement model. Parents are more likely to remind the child of trips to the hospital, and the presence of a sibling is a constant reminder of its birth. On the other hand, discussions of deaths with children and reminders of previous places of residence are much less likely to occur than the other experiences. Hence, for a small class of events, there appears to be a 50% chance (i.e., a *conditional* probability) that they will be recalled below the age of 3 (but not 2), provided the memory is refreshed.

This latter hypothesis is consistent with Williams (1994), who reports that some of the individuals in her study recalled events that occurred before the age of 3 years. This prospective study (described above) was based on documented allegations of sexual abuse, so the hospitalization actually occurred, as did the allegation and possibly the abuse. However, the claim she makes is sufficiently extraordinary that it warrants much closer scrutiny.

Williams (1994) provided breakdowns of recall by age (in years) in a footnote. Predictably, none of the children between zero and 1.99 years at the time of the documented incident recalled the hospitalization. Of the two two-year olds who recalled abuse, one was hospitalized only three months short of the third birthday (Williams, 1994, p. 1174). In the notes to the preprint, she points out that *"Some of the 'memories' may be attributable to information they received from others later in life...this was not explored systematically in this interview.* It requires no stretch of the imagination to see the single remaining case being reminded of the hospitalization enough times over her childhood (perhaps by a parent who held a grudge against the alleged perpetrator) to remember enough detail to fit Williams' relatively broad criterion for recall of the index event.

Notably, a dozen of the 'no recall' group were under the age of 4.99 years. That is 9.3% of the 129 in the sample, a figure just below the 12% who might defensibly be said to have 'forgotten' the event (see derivations in the earlier section that dealt with her study). Whether or not they actually reported no memory of anything at all is unfortunately not reported. However, the figure is very suggestive of ordinary childhood amnesia for most of those cases.

Williams (1994) groups the continuous age data into four categories; 0-3, 4-6, 7-10 and 11-12. Presenting the data thus, she argues that age does not explain the tendency of people who were younger to have lower recall rates, despite a t-test that shows subjects recalling the index

event to be, on average, two years older than those who failed to recall. She uses her age groupings to argue that the 7% difference between the two youngest age categories (55% vs. 62%, respectively) is theoretically more significant than the approximately 32% difference between the two lowest versus the two highest age groups (60% vs. 28% when the four groups are collapsed into two).

It is better to look at age as it was reported in the footnote, since vital information is not obscured by misleading aggregation. Using the footnote, Williams' dataset was accurately reconstructed[23]. A sensitivity analysis was carried out to analyze the rate of misclassification on the recall variable using age as a predictor. This analysis shows the extent to which one can accurately predict "hits" and "misses" about class membership on the basis of some continuous attribute (such as age). Here, a false positive is a misclassification of a person who reported having no memory of the event as having "remembered," while a false negative is a misclassification of a person who remembered the event as having reported no memories.

The false positive and false negative curves from this analysis are presented in Fig. 2. This figure shows that predicting that a subject who was hospitalized before the age of 4.8 years will recall abuse should produce false positive rates approaching 80%, and false negatives below 20%. As Freud (1938) would have predicted, the false positive rate actually starts to skyrocket below six years of age. The point at which the false positive and false negative rates are equiprobable occurs around 8.5 years of age. (This is the point where using age to predict forgetting produces a balance between the two types of error.) Predicting that the person remembers if the hospitalization occurred above the age of 8.5, and that the person will have forgotten if they were 8.5 or less is the best model, and is thoroughly consistent with the childhood amnesia explanation. This model was confirmed by running a weighted optimal discriminant analysis via ODA 1.0 (Soltysik & Yarnold, 1993). The results of this analysis strongly support the 8-9 year range as the separation point between the distributions; using 8.5

[23] The accuracy of the reconstruction was checked by re-running several of her analyses. All results were accurate to several decimal places.

years as the cutpoint, we achieve 66% accurate classification, with 99.7% confidence that $p < .002$.

----------insert Fig. 2 around here----------

***Childhood Amnesia and The Unquestioned Assumption*:** In spite of the preceding facts, some RMT advocates do not question the assumption that memories purported to be from early childhood are generally veridical (e.g., Briere & Conte, 1993; Hartman, Finn & Leon, 1987; Herman & Schatzow, 1987; Ross et al., 1991; Zlotnick et al., 1994). This is based on the van der Kolkian assumption that trauma is something that cannot be forgotten at the physiological level. This assumption ignores the rival (and well-documented) hypothesis of childhood amnesia. In this view, it appears that memories which were never encoded verbally can magically become verbal with complete accuracy. However, this is inconsistent with social reinforcement and the need to "keep secrets." Unless the abuser continually reminds the child of the abuse (and this might implicitly happen in some cases of repeated abuse, or explicitly in cases where the child is constantly reminded never to tell anyone about one or more incidents of abuse), or unless memories of trauma are permanently 'videotaped' and replayed and interpreted later, then vague memories recovered from the period covered by childhood amnesia are unreliable.

Detailed memories which contain concepts only a child of greater age or an adult would be able to comprehend are almost certainly confabulations. Spence (1994) has discussed the 'voice of the child' and the 'voice of the adult,' in recollections of childhood memories, and suggests some criteria that therapists should attend to when such material is brought out. For example, a person regretfully recalling the absence of a parent during the latter part of the first year of life is almost certainly imposing inferences drawn later in life on a period where no memories exist. On the other hand, a narrative that seems inarticulate relative to adult norms, and full of child-like misperceptions and misattributions is more likely to be genuine.

Childhood amnesia is a strong argument on the FMS side of the debate. RMT research is often based on such recovered memories with no suggestion that they may be false. Consider Briere and Conte (1993), who attempt to discriminate between individuals "amnesic" for abuse and those "not amnesic." Their "amnesic" group reported the the first abuse occurred, on average, at age 5.84 years. Working backwards from their F statistic for the discriminant

function between the groups, the pooled estimate of the standard deviation for both groups may computed as 3.292. Using this estimate of group variability, we observe that 19.5% of their "amnesic" sample and 9.85% of the "non-amnesic" group reported abuse during a period in which the probability of obtaining a non-fictional memory of abuse is remote.

Using similar calculations, we can estimate that 6.2% of Hartman, Finn and Leon's (1987) "incest" group have dubious under-3 memories. Likewise, Zlotnick et al.'s (1994) group had a median age of onset of abuse equal to 5, with a mean of 6 and a range of 1 to 18. A very substantial proportion of this group likely reported events under 3, although the breakdown is never specified, and the value of σ is not given. In all of these studies, it should be noted that 100% of the samples were in therapy, and none of the studies investigated the means by which "repressed" memories were "recovered."

In similar fashion, Ross et al. (1991) report that 26.6% of their DID cases indicated abuse before three years of age and 10.6% told of events ocurring before their first birthday. These reports were taken unquestioningly as factual accounts, despite their implausibility.

Likewise, 21% of Herman and Schatzow's (1987) "severe amnesia" group (discussed above) fall into this category. However, this bears closer examination in view of the 6% false memory rate for Hartman et al. (1987). If we consider that 26.4% of the sample were in the "severely abused" group, and that 21% of this group were under the age of 3 when the "abuse" purportedly occurred, we find that (.265)·(.21) gives .055. This figure rounds off quite nicely to the 6% rate of total non-corroboration in cases where efforts were made to obtain it. In fact, this matches the rate of ***dis***confirmation, if we hold these authors to the same standard of evidence that applies to confirmations. Again, the case for confabulation in the presence of normal childhood amnesia is strong.

There is substantial evidence that memories are being recovered in therapy at a rate that far exceeds their probability of occurrence. As Lindsay (1995, p. 285) wrote, *"...it is not at all clear that cases of recovered accurate memories outnumber cases of iatrogenic illusory memories.*" An examination of what is known about the methods of RMT provides a basis for understanding iatrogenic illusory memories might arise.

Manufacturing Memories

> *"What we are considering here...are the psychic and social consequences of the designs or patterns as they amplify or accelerate existing processes. For the 'message' of any medium or technology is the change of scale or pace or pattern that it introduces into human affairs."*
>
> - Marshall McLuhan (1964, p. 8).

McLuhan noted the technological world around us and the impact that its messages have upon us. To some extent, the technological metaphor extends into psychology as we progress towards "psychotechnology" in the form of Prozac, dream-inducing light-goggles and EEG. But the manipulative forms of "soft" technology have been with us for a long time, and are now become reified in the public mind as the methods become known to anyone who absorbs them through television and mass media. McLuhan acknowledges such "soft" technologies when he observes how the psychiatrist's couch "extends the integral being," (p. 8). In that sense the message of hypnosis (when it is used to recover memories) and group work is the expansion of ourselves into our own (and others') previously-obscure childhoods, or even past lives (Chamberlain, 1990). Unfortunately, that message is too often mythical. This section will explore research and theory as it pertains to the methods (mis)used to manufacture memories.

Suggestibility

The concept of suggestibility implies that not all people can be persuaded of various things with the same degree of ease. A host of personality measures have been studied as they relate to persuasibility, and this research points toward a stable individual differences trait of suggestibility that is independent of context (e.g., see Hovland & Janis, 1966). Thus, not everyone would be expected to be equally susceptible to persuasive communications such as those employed in RMT.

On the other hand, however, corporations spend vast sums every year to advertise their wares through methods that have little to do with the merit of those commodities. Loftus' (1980) work on advertisements indicates that people *do* respond to suggestions that make little sense from a logical perspective. Thus, while not all individuals are equally likely to develop false memories under all circumstances, there remain individuals would respond strongly to even the most bizarre suggestions.

Hypnosis and Suggestibility: Hypnosis has traditionally been linked to the construct of suggestibility via the existence of a distinct subgroup of people who respond very well to hypnotic induction. These individuals accept suggested information as real, and act accordingly. But the question remains as to whether hypnosis is something more than just extreme suggestibility.

Hilgard (1991) feels that hypnosis is something unique for several reasons. One, factor analytic research indicates that hypnotic and non-hypnotic tests of suggestibility load differentially. Two, well-known forms of social suggestibility such as conformity and gullability do not correlate with hypnotic-like behaviours. Three, placebo research produces increased vulnerability to indirect suggestions that does not produce hypnotic-like responding per se. Thus, while there are a host of other factors that can induce unhypnotized people to conform to suggestions (or "primary suggestibility,"; Hilgard, 1991) hypnosis produces some unique effects that are greater than the sum of the "ordinary suggestion" parts (or "secondary suggestibility,"; Hilgard, 1991). These effects include, but are not limited to, an increase in "primary suggestibility," (Spiegel & Cardeña, 1991).

Several of Hilgard's points are relevant to the present debate. First, subtle non-hypnotic suggestions and pressures to conform are at work, and that individual differences in suggestibility make some people more prone to conforming than others. Secondly, the low correlation between social conformity/gullibility and hypnosis implies that the domain of people susceptible to either hypnotic or non-hypnotic suggestion is much broader than just the domain of highly-hypnotizable people, because people are just as likely to be gullible and conforming high-hypnotizables as they are to be gullible and conforming low-hypnotizables. Third, we must take note not only of the potentiating effect of hypnosis on responsiveness to the hypnotist's suggestions but also the individual differences in responses to suggestions not clearly given under the influence of hypnosis. Fourth, receptivity to suggestions can be facilitated by the extent to which the social context makes these suggestions plausible (Hacking, 1995). Lastly, methods such as progressive relaxation, the use of goal-directed imagery, and "alert inductions" have produced equal responsiveness to suggestion (Kirsch, Mobayed, Council & Kenny, 1992). In this latter study, experts in the field of hypnosis were unable to distinguish between the simple relaxation training and hypnosis based on open-ended reports from the subjects. Taken together,

these effects point towards a large population that will be susceptible to a wider range of suggestions than can be examined in this brief paper.

Hypnosis was once touted as *"the most important, the most fruitful and far-reaching, method of experimental psychology,"* because of its capacity to induce *"almost all the phenomena of functional disorder,"* (McDougall, 1926, p. 82). Some twenty years later, there are references to the use of hypnosis not only to eliminate recall, but to facilitate the recall of apparently-lost memories during the Second World War (Herman, 1992).

Today, these views have given way to powerful social psychological understandings of the "trance" state, which is seen as extreme receptiveness to suggestion and conformity to suggestions from the hypnotist (Spanos, 1982, in press; Wagstaff, 1991). This includes a body of research that shows how attempts to improve recall also increase confabulation. As Frankel (1985) put it,

> *"he* [i.e., the hypnotic subject] *will confabulate ..in order to meet what he sees as the expectations of the individual who asks for the information. The distinction between fact and fiction is perhaps academic in the clinical context. In the forensic use of hypnosis, however, it is crucial..."* (p. 25).

The literature cited in support of hypnosis as a facilitator of the retrieval of memory-like recollections dates back to the Second World War (Herman, 1992) and is somewhat out of date. Today, major textbooks on psychotherapy either neglect hypnosis entirely (e.g. Stoudemire, 1994; Wolman, 1978) or never mention it as a useful or valid tool for recovering memories of childhood events (e.g., Wolman, 1976). Nevertheless, we are seeing claims that hypnosis is a special sort of channel through which intact memories of childhood abuse - or even of prenatal *in utero* experiences (e.g., Cheek, 1992) or past lives (Loftus, Garry, Brown & Rader, 1994; Spanos, 1988; Spanos, Menary, Gabora, DuBreuil & Dewhirst, 1991) - can be obtained.

Furthermore, as noted above, the use of hypnosis does not have to involve a formal, overt induction process on the part of the therapist (Kirsch et al., 1992). Similar responding can be obtained by subjects who are taught how to perform self-hypnosis (or autohypnosis; Hilgard, 1991). Guided imagery, an indirect method of hypnosis, is recommended as a tool for memory retrieval (Paré & Shannon-Brady, 1996). Autohypnosis is very much a part of the purported dissociation in response to trauma, and is cited repeatedly by Terr (1994) as a method by which

victims create an alternative, more acceptable reality. A similar process has been described by Schumaker (1991), who sees defensive dissociations as involving

> *"the incorporation of suggestions...which enable a person to reconstruct reality in a meaningful alternative manner. The source of the suggestions may be external or internal,"* (p. 126).

If such autosuggestion may occur spontaneously in response to actual trauma, one must wonder why it would ***not*** occur in response to guided images of trauma, particularly when the demand characteristics of the therapeutic situation (at least, in RMT) are laid out clearly by the therapist.

In tracing the history of therapeutic hypnosis, Lankton and Lankton (1983) find that research on hypnotically-induced memory distortion dates back to as early as 1846. As mentioned above, Yapko's (1994) survey revealed some remarkable deficits in the knowledge of therapists about this aspect of hypnosis. Not least among these findings was the 24% who felt strongly that *"hypnosis enables people to accurately remember things they otherwise could not,"* the 24.1% who agreed that a person's *"level of certainty about a memory is strongly positively correlated with that memory's accuracy,"* and the 43.5% who agreed that *"hypnotically obtained memories are more accurate than simply just remembering,"* (Yapko, 1994, pp. 232-236). Since 34.4% of the sample reported using hypnosis at least occasionally (with 6.7% reporting using it "often") to recover memories, the danger of these misconceptions is apparent, given that research in hypnosis contradicts all of these views.

Terr (1990, p. 178) implies that lying or "suggestion" is not at all a component of "false traumatic memory," but rather, that inaccuracies arise as a result of the trauma itself, which is assumed to be historical fact. The confabulations are assumed to be *"an internal truth,"* (p. 178). However, her evidence contradicts this claim. She cites a case where a woman experienced terrible memories of her grandmother's murder, despite not having been present when the crime occurred. As a result of the traumatic feelings aroused by her grandmother's death, she *"transferred perceptual impressions from one modality (feeling or hearing) to another (seeing),"* (p. 178).

In other words, a vivid memory was confabulated from suggestions that Terr declines to call suggestions because they did not come directly from a therapist (in spite of her belief in

autosuggestion noted above). Rather than showing that false memories do not occur in therapy, she demonstrates that even a less coercive setting than therapy can produce outright fabrications from the experience of trauma-like feelings (see also Hilgard, 1991, p. 51; see Powell & Boer, 1994 for an interesting discussion of the extent to which Freud himself used coercive tactics to extract memories consistent with his early hypothesis of the trauma-repression-symptom model).

Repeated questioning about sexual abuse where the client is in an even more suggestible state should not only produce emotional reactions, but those reactions are themselves quite likely to "transfer perceptual impressions of what the therapist is asking about from one modality to another." In other words, this process is precisely what makes hypnosis hazardous as a memory-recovery tool. Whether this cost outweighs any benefits is contingent upon the extent to which hypnosis can improve accurate recall.

Hypnosis Does Not Improve Recall: Anecdotal reports abound of allegedly unobtainable memories being accurately recalled under hypnosis, but these fail to rule out other confounding variables, and tend to disregard inaccurate recollections. Controlled research has shown that hypnosis does not markedly improve recall (Dywan & Bowers, 1983; Laurence & Perry, 1985; Smith, 1983), but can have the effect of increasing confidence about *in*accurate memories (Frankel, 1985; Sheehan, Grigg & McCann, 1984). A review of the literature on hypnotic refreshment of eyewitness memory by Smith (1983) found that (in contradiction to anecdotal reports) researchers have consistently failed to improve recall in controlled laboratory studies. There is even a substantial literature on child eyewitnesses, who have been found to be susceptible to the deleterious effects of uggestion under many circumstances (e.g., Ceci & Bruck, 1993).

Of course, the question of ecological validity may be raised with respect to the type of memories being debated in the present work. Smith (1983, p. 393) notes that the type of material being recalled is commonly different from that of a crime scene in terms of meaningfulness and emotional arousal. Hypnosis might not recover intentionally-learned material, but only incidentally-learned material. As well, the consequences of recalling are not as serious in the lab as they are in an eyewitness situation. The point of this objection would appear to be that hypnosis will be more successful in recovering meaningful, emotionally-charged memories than it would in retrieving the mundane types of memories used in the lab. If this is so, then there is

problem for RMT. Why are mundane memories of childhood easier to access without hypnosis while the more traumatic memories of abuse are allegedly *harder* to access without intensive therapy (e.g., Lovern, 1993)? If dissociation arises from repeated traumatizations that teach one how to dissociate memories (Terr, 1992), then the learning is scarcely incidental, so how is it that such memories *are* recallable under hypnosis? If we accept the lab work, there is no basis for clinical hypnotic refreshement of memories. If we raise these objections to it, then the conflict with RMT theory is obvious.

Hypnosis may or may not aid in the recall of some aspects of traumatic memory. If, as the lab research indicates, it does not, then there is no justification for its use, given the obvious risks. If it does work, however, its value for therapy must be weighed against possible negative side effects, because hypnosis can also have the effect of distorting memories and producing pseudomemories.

Unfortunately, not all therapists recognize this fact. Ewin (1994), for example, disagrees with Yapko's (1994b) survey results on the basis of a few anecdotes where people recalled information accurately under hypnosis. However these anecdotes give no indication of the ratio of *inaccurate* information to accurate detail, and so tell us nothing. An examination of some of this research will tell us more.

Evidence that Hypnotic Methods Distort Memories: Citing Janet's work, even vocal RMT advocate Judith Herman (1992, p. 181) acknowledges that the content of memories could be altered with hypnosis, and she is clearly aware that people with psychiatric disorders such as BPD and DID are extremely suggestible (p. 124). Given this, one is left to wonder on what she bases her criticism of professionals' tendency to be skeptical of recovered memories (p. 124).

A cautious examination of the use of hypnosis by Gravitz (1994) points out that the distortion of memories through hypnosis can be therapeutic by changing the meaning of an event so that the patient's reactions are modified and thus symptamotology is reduced. He points out that this technique is contraindicated in psychotic and borderline cases where there is difficulty in distinguishing reality from fantasy, as well as where legal matters may be involved. As Lippmann (1963) put it, *"A great deal can be done by exorcising bogeys - by refusing to add the terrors of the imagination to the terrors of fact,"* (p. 39).

A literature review by the American Medical Association has warned that the use of hypnosis in forensic contexts produces unacceptably high rates of false memories (AMA, 1985). This is in accord with numerous other reviews and research (Dywan & Bowers, 1983; Timm, 1984; Holden, 1980) which indicate that hypnosis increases the *amount* of information produced by a subject but not the accuracy of the information. Indeed, Dywan and Bowers' subjects produced mostly errors when hypnotized. This effect was most marked for the highly hypnotizable subjects. Their data suggest odds of 9:1 *against* a hypnotically "refreshed" item being accurate. Of course, this depends entirely on the degree of coercion and suggestion that occurs in any given case. Thus it may under- or over-estimate the degree of distortion in clinical settings, as a function of the degree of coercion and suggestion that is used.

The phenomena surrounding hypnotically refreshed memories are summarized briefly but adequately by Morris (1989), who points out that hypnotic subjects

> *"are anxious to please, hypersuggestible, and able to purposefully lie, confabulate, or incorrectly remember things...Hypnosis seems to have the effect of 'cementing' even erroneous memories in the witness's mind, thereby giving him or her greater confidence in these memories."*

There is evidence that formal hypnotic procedures with a hypnotist may be less effective than self-hypnosis (Ruch, 1978), implying that the clinical phenomena observed in RMT are are not even *necessarily* the product of therapists' suggestions. Other factors such as popular media presentations of cases of supposed repression may well be a basis for some productions[24]. Another such factor is what has come to be known as "truth serum."

"Truth Serum": The use of sodium amytal (or other narcotic) interviews to recover memories (e.g., an unknown proporition of Herman & Schatzow's [1987] subjects) is rooted in the work of Grinker and Spiegel (1945). Their oft-cited work on post-combat stress has formed much of the theoretical basis of RMT as it is supposed to be related to PTSD (although the history of this use goes back even further; see Loftus, 1980). Examination of their method, however, reveals the extent to which the reports may be the product of suggestion and so

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[24] The implied reconceptualization of hypnosis as largely a subject-driven phenomenon does not exclude the kind of positive feedback loops that can occur when a person has tenuous (though false) beliefs reified and unconditionally validated as fact by an overtly or covertly manipulative hypnotist.

inadmissible as evidence in court (Hanley & Schmidt, 1977; Jean v. Rice, 1991). Grinker and Spiegel's (1945) technique is illustrated in the following example. After the injection, and once counting backwards from 100, the subject becomes confused, and

> *"The flier is told in a matter of fact manner that he is in his plane with his crew in flight. Depending upon the amount of known history, specific details are added corresponding to the most traumatic parts of his combat experiences. If little or nothing is known of the painful combat experiences, a typical scene is depicted...Some react with the first few words...Others resist...When such resistance is maintained, the stimulation can be made more dramatic and realistic. The therapist can play the role of a fellow crewmate, calling out fighters or flak in various positions..."* (Grinker & Spiegel, 1945, p. 172).

Not surprisingly, *"Persistence is rewarded in almost every case by an account of the scene in progress,"* (p. 172). The details are assumed to be real, despite observing that

> *"The material is not restricted to combat situations or scenes, but associations from the patient's past, his childhood, his family life and the current life setting are freely intermingled. The relationship between combat stress, interpersonal problems, past difficulties and current problems clearly indicates their dynamic ties,"* (Grinker & Spiegel, 1945, p. 391).

This passage highlights the reconstructive nature of memory. Of course, these were only the *obvious* distortions, and there would be no way to tell if the ferocious and bloody combat story being told was factual or a re-visiting of a much milder fight with a sibling in the distant past. The fact that such stories are consistent with the experiences of a soldier in no way validates their accuracy, particularly if it is true that such experiences get "compressed" (Blume, 1993, March) or "condensed," (Neisser, 1981; Olio, 1989).

Subtle Suggestions: Loftus (1980), in her research on advertising, explored "pragmatic implications," in which people can be led to make invalid inferences that are misremembered as facts. "Pragmatic implications" are remarks that *"lead the hearer to expect something neither explicitly stated nor necessarily logically implied in a sentence,"* (p. 151). As an example, she cites the AC Spark Plug ad that stated *"We make the only spark plug with four green ribs...That's so you'll know it at a glance."* Even though the green ribs were only for identification purposes, the ad was misremembered as "green ribs are an important feature of good spark plugs." People

make illogical, invalid inferences from incomplete information and misremember these inferences as part of the original ad. In this way, pragmatic implications differ greatly from logical implications, such as the way that "Mike threw the stone through the closed window" implies that the window broke.

The inference that is made becomes part of the memory. We can see here the basis for very subtle, almost invisible suggestions can work their way into a client's mind. We need look no further than recommended ways of dealing with an absence of memories for abuse. Enns et al. (1995) advise telling clients that *"important material...will eventually surface when [the client] feel(s) prepared to deal with such material,"* (pp. 234-235). Quite indirectly, the client is told that he or she has memories too horrible to remember right now. The indirectness of the communication makes it all too likely that the client will not recognize the suggestion for what it is, and incorporate it.

As an additional example, there is are questions often used by DID diagnosticians, such as "Is there another part of you that wants to talk but can't get out?" Clearly the pragmatic implication is that there ***is*** such an alter identity, and that its absence thus far is due to some mysterious process that the all-knowing therapist has discerned.

Self-Generated Validity

In applied psychology, the problem of self-generated validity has been examined by Feldman and Lynch (1988). In the area of survey data, they analyze the creation of beliefs, attitudes and intentions by the measurement instruments that are used. Working from a cognitive model, they posit a simple theory: that *"the timing, order, and method of measurement of belief, attitude and intentions affects the observed relations among them, and between them and behavior,"* (p. 421). Essentially, there is a disproportionate influence of briefly-activated cognitions on judgements and actions that follow them closely in time, particularly if the memory is very accessible as a result of elaboration, rehearsal, or vividness. Abstract cognitions that have strong "summarizing power" are also more likely to impact on subsequent cognitions.

In the present context, Spanos (in press) reviews literature on the putative validity of the link between dissociation and trauma (e.g., DiTomasso & Routh, 1993). The putative relationship is based on small correlations between reports of dissociative experiences and trauma that can vanish when the order of administration is manipulated. In two studies, for

example, it was found that if a trauma history survey was administered first, the correlations with dissociation were substantial. If the dissociation measure was administered first, however, the correlations vanished.

Spanos argues that these results indicate reported dissociation can be mediated by expecancy effects. They are similar to results from studies of hypnotizability, where the correlations between a measure of creative imagination and a measure of hypnotizability were subtantially larger[25] when the imagination scale was defined to the subjects as a test of hypnotizability (Spanos, Gabora, Jarrett & Gwynn, 1989). If we allow that behaviour is also affected by previous activation of cognitions, it parallels earlier results obtained by Spanos, McPeake and Carter (1973) that indicate the administration of a pretest plays a significant role in affecting later performance. This may perhaps be due to the attentional processes that have been implicated in suggestibility processes (Gibbons & McCoy, 1991).

Importantly, this phenomenon leads to serious questions about the impact of overt suggestions and pragmatic implications in psychodiagnostic interviews (e.g., Bala, 1994). More research is certainly needed on the extent to which the "self-generated validity" phenomenon generalizes to in-person interviews. Given the added salience of what a hired expert - the therapist - tells to a person seeking concrete answers to their problems, it is probably safe to predict that this effect will be enhanced rather than diminished.

In short, the use of suggestive methods alone or in combination is likely to distort the facts, or even create "facts" that never were. However, the FMS case also requires the demonstration that such methods are in fact being used in clinical settings.

"It Just Isn't Done"

The FMS contention that memory-distorting methods are used in therapy has been countered by the objection that very few counsellors engage in certain manipulative techniques (such as hypnosis, sodium amytal and guided imagery) that are supposed to cause confabulation (Kristiansen, 1994b). Thus, even if the processes that undergird FMS exist, they generally do not occur in therapy. For example, Gravitz (1994) argues that unlicensed and unregulated therapists

[25] The correlation of creative imagination to the objective hypnotizability subscale went from .34 to .65; for the subjective subscale, it changed from .50 to .71, both increments $p < .05$.

are the root of the FMS problem, but Poole, Lindsay, Memon & Bull's [1995] survey would seem to indicate that the problem goes well beyond the borders of licensure. Unfortunately, this is another argument from a vacuum.

Yapko (1994, 1994b) refutes these arguments with the results of an extensive survey of therapists that shows an alarming rate of credulity about and misuse of hypnosis amongst people who presumably should know better, since most practice hypnotherapy. As well, this survey also revealed a large number of therapists who subscribed to the "videotape" model of memory, and who, despite contrary evidence, feel that hypnosis is a valid tool for recovering memories. Similarly, Loftus (1980) documents numerous cases in which attempts were made to recover memories through the use of "truth serum" drugs. Indeed, in a published article, Paré and Shannon-Brady (1996) overtly recommend guided imagery as a tool for retrieving repressed memories.

A recent survey by Poole et al. (1995) responded to a similar claim by Pezdek (1994), who argues that trained professionals know better than to use manipulative methods. Two random samples of U.S. and one random sample of British doctoral-level practitioners obtained information about their opinions, practices and experiences with adult female clients. Seventy-one percent reported using hypnosis, dream interpretation, "journalling," etc., to recover memories, even though approximately 90% of all three samples believed that it is possible for a client to *"believe that she was sexually abused as a child if no abuse had actually occurred,"* (p. 432). Nine percent of clinicians asked felt that this occurred "fairly often," while 18% denied that it ever happened. Seventy-three percent felt that such misbelief happens "rarely" or "very rarely."

Intriguingly, significant correlations were noted between the extent to which memory-retrieval techniques were used and the clinicians' estimates of the number of women who recall sexual abuse in therapy. Abuse-oriented clinicians would appear to find between five and ten times as many cases of "repressed memories" as do more cautious clinicians who do not use such memory-retrieval techniques - just as Mai's (1995) small number of clinicians are 26 times more likely to diagnose DID. Generalizing from the patterns observed, it appeared that

> *"25% of the members of those organizations who conduct therapy with female clients believe that recovering memories is an important part of therapy, think they can identify*

> *clients with hidden memories during the initial session, and use two or more techniques to help such clients recover suspected memories of CSA* [childhood sexual abuse]," (Poole et al., 1995, p. 434).

Another argument against the "it's not done" claim is the recommendations made by professionals who seem to be unaware of the capacity of leading questions, drugs and suggestions to produce confabulations. Powell & Boer (1994) note Freud's use of coercive tactics to confirm his hypotheses. Recall Bala's (1994) prompting on the link between symptoms and abuse. Consider also Herman's (1992, p. 177) recommendation that memories be seen as a "movie," and the unknown number of Herman and Schatzow's (1987) of patients who were exposed to "truth serum." Similarly, Barstow's (1995) recommendation that self-injurious patients be asked questions (by nursing staff) such as *"Who did things like this to you?...Who else participated?...What was going on in the family when it was done?"* (p. 21) seems to disregard the impact of such queries. The assumption of familial abuse is built right into the question, despite a litany of other causes for self-mutilation that was recited on the preceding page. Consistent with a videotape model of memory, Barstow also instructs that individuals should be told that their negative self-deprecations are *"'tapes' made by significant others"* that are being played back.

Psychoanalyts are not immune from this, either. Bernardez (1994) sees the "eroticized transference" of a patient onto the therapist as a symptom not only of sexual abuse, but specifically of *parental* abuse (despite the relatively low base rates). In her first case presentation, she admits suggesting an "unbearable experience" to a male patient, and interpreting a dream and some free associations as "female genitalia." He recovers a memory of seeing his mother naked. Episodes of choking then bring forth fragments of forced cunnilingus and "body memories" in which he could not breathe. Similar methods produced similar results in her second case, all without any corroboration. But yet, a brief tip of the hat to concerns about "false memories" is offset by the unwarranted claim that narrative coherence provides authentication of the "memories." A plausible alternative explanation is that earlier interpretations feed into later ones, becoming reified as the story gained coherence.

Siegel and Romig (1990) approach hypnotic memory retrieval from an Ericksonian perspective, and emphasize the use of stories and metaphors under hypnosis to access presumed

abuse issues. In the absence of memories, the therapist is supposed to make them up in such a way that they are analogous to the client's life experiences, apparently missing the point that the life experiences being described can only be those inferred by the therapist, since the patient doesn't remember them, by definition. While they insist that the patient must *"make use of the metaphor as he/she wishes,"* the intention of the therapist is to have the subject *"readily accept what the story seems to imply about his/her life,"* (p. 250). Thus, on the surface they seem to imply a freedom of interpretation on the part of the client, even though their method relies on explicitly constraining the possible interpretations that can be made to those consistent with the hypothesized abuse history.

In addition, there are now cases where investigators posing as patients have recorded the use of highly suggestive techniques in therapists' office. Loftus (1995) outlines three such cases where such pseudopatients were "diagnosed" very quickly as "survivors" on the basis of minor complaints (e.g., sleep disturbances, depression and relationship problems.). In one case, the therapist spoke confidently of body memories, repression, the lack of need for "rational reasons" for feelings of betrayal, and his/her own experience of abuse in order to overcome resistance to the implausible idea of buried memories. The therapist also used the argument that "nobody would make up such memories," urged her to read *The Courage to Heal* (Bass & Davis, 1988), and used guided imagery to try to recover memories of abuse. Pendergrast's (1995) series of interviews with recovered-memory therapists provides an additional and frightening look behind the scenes, as does his account of "Olivia McKillop," a retractor who was diagnosed as "abused" in her first one-hour session on the basis of clinical depression, despite not mentioning any abuse history.

There is also clinical evidence that shows how theory-driven coercion is a subtle but pervasive component in many types of therapy. Wile (1984) reviews case material from a range of psychotherapies, and notes that *"As long as therapists believe that their psychodynamic formulations are fundamentally correct, they may have no way of noticing that their interpretations are accusatory and coercive,"* (pp. 360-361).[26] The general response of clients to such methods involves at best resistance (Mahrer, Murphy, Gagon, & Gingras, 1994) and at

[26] The similarity of this to facilitated communication (see Appendix B) should be noted.

worst a good deal of negative affect. In RMT, one may be certain that such negative affect would be taken as evidence of abuse, rather than annoyance at the therapist's coercion[27]. Likewise, "resistance" would be viewed as denial of a terrible internal reality, rather than a coercive external one. The accompanying negative affect can merely be reconstrued as further evidence of repressed trauma, despite its iatrogenic origin.

We must consider again the distinction between primary and secondary suggestibility set out above (Hilgard, 1991). This distinction implies that even a factual statement that "hypnosis wasn't used" in no way implies that subtle-but-effective suggestions, free association, dream analysis, guided imagery, misleading and repetetive questions or sodium amytal were *not* used. These methods all fit into the class of techniques that *"decrease critical analysis or screening of memories"* and thereby *"degrade memory accuracy,"* (Lynn & Nash, 1994, p. 200). As Lotto (1994) observes, *"refraining from using overt hypnotic techniques in therapy does not eliminate the problem,"* (p. 381). Indeed, it may enhance the problem, when the whole point of using them is to *avoid* the conscious mind and critical thought, as recommended by some (e.g., Paré & Shannon-Brady, 1996).

Questioning the Methods

Repressed or dissociated memories of childhood abuse should not go unquestioned. Is there any corroborating evidence? In the absence of such evidence, we have seen that the odds against the memory being accurate are higher still. Is the clinician who assisted at the recovery of the memory defying the odds by finding many such "repressed memories"? If so, something may well be amiss, simply because reports of "no abuse" are far more likely to be accurate than a "false negative."

Given the emergence of a repressed memory, both the content of the memory and the circumstances which it appeared are important. Are the memories for events that happened before three years of age, or that are physically impossible? Was hypnosis, sodium amytal,

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[27] We may point here again to Anderson and Gold's (1994) observation that not all women take kindly to having their identity molded into that of the "survivor."

guided imagery or a similar method used to recover the memory? If so, the likelihood of confabulation is sufficiently far from zero to warrant serious concern. Were suggestions made about a history of abuse when no such history was reported? If so, suggestion may still be at work, particularly if the individual in question is highly suggestible. Were symptoms identified as "certain signs" that the person was abused (e.g., Blume, 1993; Ratican, 1992), or was the person put into treatment for abuse of which they have no memories (e.g., Herman & Schatzow, 1987)? Once again, the power of suggestion will be strongly at work.

To return to probabilities, there are some figures we must keep in mind. While false memories of abuse are probably far more rare than real ones, the problems they create are so devastating as to warrant serious concern, rather than minimization. If for every memory accurately recovered, there is one falsely created, then the odds that a recovered memory has any historical truth are even - a toss of the coin.

The existence of false recovered memories and coercive techniques is not in question; their relative frequency is. While some critics falsely claim that *all* memories of sexual abuse are identified as "recovered" for strictly political ends (e.g., Courtois, 1995) this is nothing more than rhetorical misrepresentation. Distinguishing between reliable continuous memories and relatively unreliable recovered ones is central to understanding the FMS position. It is even more central to the balanced perspective to which such critics pay lip service.

The abuse-repression-recovery process is a neat model, even if it is not supported by data. From an absence of memories of abuse, it is possible to construct a narrative that fills that void. The common thread or gist of many stories of recovered memories is the editing and re-editing of a memory that is taken as real at each stage because it 'explains' current problems. If the problems get worse - as they all too often do in RMT (Ofshe, 1994) - the explanatory memories need to become worse. Hence SRA. However, the deepening of distress as abuse narratives take shape requires another look at affect and memory. From there, we will return to the idea of narrative truth, which is a key aspect of a reconstructive model of memory.

Affective Components and Memory Distortion

The claim that strong affect and belief guarantee factualness of a memory implies that such affect is not possible in the absence of fictions. This claim is contradicted daily, by audiences of horror films. In this section, we will see more reasons that this is not the case. We

must conclude that inferences based on strong reactions in therapy are no more valid than inferences based on an absence of memory for abuse.

As noted above, even if the videotape of our memory is a short, one-event cassette, it can still be dubbed when overgeneralization of fear leads to faulty reconstructions. Mineka (1992) outlines four laboratory-demonstrated cognitive phenomena that contribute to the overgeneralization of fear:

1) an increase in fear to generalization test stimuli, arguably due to forgetting of specific attributes of the conditioned stimulus with increasing retention intervals;

2) rats "behave in an increasingly conservative fashion in a wider range of frightening situations than they would have immediately following conditioning," (p.174), again probably as a result of forgetting the precise characteristics of the unconditioned stimulus;

3) "conditioned inhibitors of fear are forgotten over time to a much greater extent than are conditioned excitors of fear," (p. 174), and

4) while fears conditioned in situation A will be elicited in situation B, fears extinguished in situation A will not extinguish in situation B.

She quotes Bouton: *"Extinction does not cause unlearning, but instead gives the CS [conditioned stimulus] a second, and therefore 'ambiguous' meaning,"* (p.174-175). Appearance of the CS in a new context can thus provide a basis for relapse, and this accounts for the clinical observation that cues similar to original traumatic events can precipitate recall of that event. That it will not always cause reactions as severe as the original event despite the close similarity of cues is apparent from Grinker and Spiegel's (1945) observation about many shell-shocked bomber pilots successfully returning to combat as fighter pilots.

While this research - like all lab research, according to some (Courtois, 1995; Terr, 1993) - suffers from a certain lack of ecological representativeness, at least some biological researchers feel that in the case of fear, *"the pathways are very similar in mammals and possibly in all vertebrates,"* and so *"We therefore are confident in believing that many of the findings in animals apply to humans,"* (LeDoux, 1994).

Taken at face value the preceding points indicate that it is possible that severe symptoms may be a result of the mundane process of forgetting. Thus, if a memory of genuine trauma is

so far gone as to need "recovering," it is probably because most of the details of any trauma have been forgotten. Unless we are willing to attribute an entire Freudian structure of Id, Ego and Superego to rats and thereby allow them to repress complete "videotape" memories, we may have to accept that vague memories are vague for a reason: mundane forgetting. While a lack of retrieval cues may also account for the failure to recall, this problem must be weighed against the high likelihood that providing cues is a highly suggestive process likely to produce whatever antecedents the therapist believes to be lurking in the background.

Extending this line of thought to applied situations, it implies that therapies which focus on recovering memories in order to deal with them merely attempt to extinguish responses in situation A (through catharsis), while ignoring those in situation B. Dealing with the past through "memory work" should *not* impact on current fears and anxieties, as suggested by RMT advocates. Indeed, encouraging the re-living of traumatic events and discouraging the distinction between reality and fantasy should produce a progressive worsening of these problems by repeated pairing of images with the terrible emotions of 'catharsis.' This is exactly what has been described in many cases, particularly those where the patients learn DID behaviours (Ofshe & Watters, 1994). It is also consistent with another clinical observation, that *"a hypnotist's personality problems could stimulate hysterical behavior in the clients with whom he or she worked,"* (Lankton & Lankton, 1983, p. 132). Indeed, some clinicians recommend the intesification of emotional experiences through techniques such as guided imagery (Paré & Shannon-Brady, 1996).

Mineka (1992) also reviews research that indicates (weakly) that cognitive biases may be at work in phobias: *"phobics may have an attentional bias that results in the diversion of attention to cues relevant to their fears,"* (p. 178). Mineka also notes that there is little work on memory bias in phobics, although what there is suggests that hypervigilance for feared stimuli coupled with avoidance of further processing *"may serve to maintain or promote fear."* With accurate evaluation impaired by lack of further processing, fear-provoking cues do not lose their potency.

We should also note here the consistency of this conclusion with the results cited above (Swann, Wenzlaff, Krull & Pelham, 1992; Swann, Wenzlaff & Tafrodi, 1992) in which depressed individuals respond better to people who confirm their negative self-image. Fear of

change in oneself merely biases attention towards evidence that one has not changed, however negative that self may be. Paradoxically, identifying oneself as a victim is not a negative thing to a person who is already depressed because it is consistent with an existing negative self-image.

It is also possible that increasing the range of things that one fears by accepting the "widespread abuse and repression" hypothesis promotes a perverse feeling of security by "bringing to consciousness yet another previously-unknown source of anxiety." If it is "known" that certain people have perpetrated abuse in the past, one can control the situation by cutting off contact. This makes so much sense when abuse occurred that "better safe than sorry" (i.e., a minimax strategy under the p[Memory | ~Abuse] = 0 model) can easily be justified as a strategy. Thus, the benefit of control can seem to outweigh the impact of false allegations (especially if the latter are viewed as "practically impossible").

This would account for the reported hypervigilance among victims of sexual abuse (Bass & Davis, 1988; Blume, 1993), and it supports the position that repression-like cognitive blocks (e.g., deliberate thought avoidance; Erdelyi, 1990) could appear following trauma. However, therein lies a dilemma: if the failure to process the material further results in a failure to encode, then phobic responses coupled with repression indicate not only the presence of some kind of historical trauma, but the impossibility of accurate recall of details concerning that trauma. This brings us back to the unfortunate paradox implied by van der Kolk (1994) and stated by Bass & Davis (1994, cited above): that confabulation appears to be a symptom of abuse.

As well, judgement biases (discussed below) can be found among phobics, in that they show accurate estimation of the experimental covariation of neutral stimuli and shocks, but dramatically overestimate the covariation of phobic stimuli and shocks, indicating that fear can bias information processing towards the maintenance or enhancement of the fear.

Mineka discusses one experiment that coincided with a fatal stabbing incident near the university. A knife was a fear-provoking stimulus for the participants and tended to bias responses. This effect declined as time passed. She concluded that

> *"once fears are acquired, or danger schemata are temporarily activated, a style of confirmatory processing often comes into play which may promote the persistence or even exacerabtion of fear,"* (Mineka, 1992, p. 187).

Following up on this line of thought, Tomarken, Sutton and Mineka (1995) conducted two experiments. First, snake-phobic individuals and controls were shown slides of snakes or neutral stimuli which were paired randomly with shock and two nonaversive outcomes. In another condition, damaged electrical outlets (DEOs) were substituted for the snakes. The covariation bias of snake-phobics was towards overestimation of the pairing of the snake slides with the shock, relative to controls and relative to the DEO group. Experiment 2 found that both high- and low-phobic (for snakes) groups rated DEOs and shocks as "belonging" together better than snakes and shocks, but the high-fear group showed an affective profile more indicative of a snake-shock pairing. As well, affective responses to snakes and the snake-shock similarity profile predicted snake-shock "belongingness" ratings. Overall, the authors conclude that the illusory correlations are stronger when affective similarity (as opposed to semantic) similarity is used as the basis of the pairing.

The research reviewed above implies that there is a cognitive bias towards overestimation of the relationship between feared stimuli and negative consequences. The implication is that upon the recovery of traumatic memories, violent abreactions must occur, and that violent emotions are necessarily the result of some dreaded event. Thus, the presence of an element on one side of the equation implies the necessity of the other element. In abuse-centered therapy, negative affect must be due to abuse, because it is one of the most feared elements that could be brought up. Logically, this is not a necessary connection - but as we have seen above (e.g., Loftus, 1980), logical inferences are not a necessary component of the reconstruction of the past.

It appears, then, that fear and negative emotions are part of potent signal systems that make certain pieces of information very salient (Oatley, 1992). Attentional biases towards the terrifying may be unavoidable. However, linked with the Feldman and Lynch (1988) model of the disproportionate effect of cognitions on later responses, their potential role in the creation of FMS becomes even clearer. Accusatory and coercive questioning that propounds a coherent (if unfounded) set of cognitions such as the myth of repressed memory (Loftus & Ketcham, 1994) may function to write (or re-write) a story that spirals into ever-deepening confabulations. In the context of RMT, the transparency of the process would make the co-construction of a narrative of abuse seem to both parties like the slow emergence of a factual account.

Narrative Truth and FMS

The argument for FMS can be seen here as being reliant upon the strength that a narrative truth can achieve in the mind of a believer. This strength is acknowledged as a potential source of healing in memory-focused therapy (e.g., Fowler, 1994; Paré & Shannon-Brady, 1996). Unfortunately, its potential for harm is too often trivialized. The narrative is likely to be a production of the client, or a co-production of the therapist and the client. The validation provided by the therapist, other clients in group therapies, and stories in survivor literature merely serve to reinforce the story.

The *tweaking*[28] of individual stories so that they contain enough material in common to identify the client as a "survivor" is easy once the gist of the story has been established, such as "the symptoms imply a history of abuse." The deletion of factual details and the addition of false ones to a gist is - to the recaller - an near-invisible process (Schank, 1990). This is the essence of the Ouija effect in memory co-construction[29].

The role of emotion in this process may also be important. Christianson and Loftus (1990) report that the number of central details (but not peripheral details) that are reported in traumatic memories is correlated to the rated emotionality of the event. These memories were apparently rehearsed by the majority of subjects, as well. Thus 'gist' would seem to incorporate a good deal of emotional information in memories of sexual abuse, in which central details would be expected to be more numerous but peripheral details recalled randomly. However, there is a chicken-and-egg problem with generalizing the correlations observed in this study to sexual abuse. For the common events recalled by Christianson and Loftus' subjects, such as deaths of friends or family or traffic accidents, the central details are inherently traumatic.

For other events, it is possible that the retrospective rating of the event as "traumatic" was based on the chance recall of details that are now interpreted as "traumatic." For example, being kissed harmlessly by an uncle whose mustache smells of alcohol would be remembered as unpleasant. The central detail of "unpleasantness" is definitive of "abuse," as we have seen

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[28] *Tweaking* is Schank's (1990) term for "the adaptation of a general pattern to a particular case" (p. 190).

[29] Schank (1990) also views the *failure* to create stories as the process underlying efforts to forget things. Repetetive telling of a story makes it easier to remember, but failing to create the story by linking it with other knowledge makes it rudimentary and in need of reconstruction at a later time when recall is attempted. Accurate recall of unpleasant stories in response to cues may later occur; however, faulty reconstruction is not ruled out.

above. Labelling such trivial details as the mustache "peripheral" after relating stories of other "survivors" who were abused by drunken relatives would alter the retrospective view of the emotionality of the event. It achieves this by making an extrememly unpleasant association with "alcohol" and "relative" in the mind of the subject, and twisting it into something more than it actually was.

The power of narrative truth to supplant historical truth as a means of organizing one's experience is a basic tenet of RMT. The "defensive" processes that are described in the "survivor checklists" (such as denial) rely upon the construction of alternative narratives that mask the abuse history (presumably in the **un**dissociated memory system). The "construction of the survivor identity" (Anderson & Gold, 1994) is held to be of great benefit. However, why this new narrative should be less fictional in any given case than "psychopathological denials" is unclear. What *is* clear is that both sides of the debate seem to agree that therapeutic fictions and historical realities can become inextricably intertwined; one side points out the benefits, while the other side points out the hazards. But as noted above (and pointed out by Schank, 1990), the underlying process is arguably the same - the socially influenced reconstruction of one's past on the basis of incomplete (and potentially incorrect) information.

This is consistent with Spence's (1982) view of memory, particularly non-verbal memories, or 'images.' Non-verbal memories, as we have seen, are a key facet of RMT arguments (e.g., Bass & Davis, 1988; van der Kolk, 1994a). Yet, the attempt to translate pictures and vague sensations into words will at first misrepresent the image, and then (with repetition) *"the translation, no matter how approximate, will tend to replace the original,"* (Spence, 1982, p. 56). This is the essence of memory distortion, and fits with another model derived from cognitive research.

Schank (1990, p. 178) observes that in the telling of a story, the translation of the "gist" of a story into English can alter the central thread of the story by placing demands on memory. He identifies this "loss in translation" as one of several factors in a reconstructive model of memory that can alter a story through successive reformulations. Information about the *listener* is also incorporated in this model and also can modify the story, consistent with what we have seen in the hypnotic pseudomemory literature.

We may operationally define "memory distortion" at this point as any change in a memory that reduces its historical veracity. As well, we may propose that memory distortion is not always apparent because where a narrative has been constructed that "makes sense," the disjunction of memory and fact becomes invisible. Of course, some distortions will be more severe than others, and people will exhibit varying levels of confidence about the memory. However, as we have seen, confidence about a memory should never be taken as proof that it is accurate, but merely as proof that some narrative has been accepted that helps events to "make sense" with respect to some model. But the question arises as to whose model we are using to manufacture meaning (Olio & Cornell, 1993). If the only model provided to a client is the myth of repressed memory (Loftus & Ketcham, 1994), it is hardly surprising that the contents of some or many recovered memories tend to focus on what they are supposed to: childhood sexual abuse.

How can professionals use discredited methods, and insist on their validity after research shows these methods to be invalid? Dawes (1994) notes that they, too, are human and so subject to common cognitive biases, particularly where group processes are involved (Eve & Harrold, 1993).

The method of Facilitated Communication is a prime example. Developed as a tool for communicating with severely autistic children, it has been thoroughly discredited by researchers who have shown that those who facilitate the communication are the ones providing the messages (Bligh & Kupperman, 1993; Eberlin, McConnachie, Ibel, & Volpe, 1993; Green, 1994; Klewe, 1993; Moore, Donovan & Hudson, 1993; Moore, Donovan, Hudson, Dykstra & Lawrence, 1993; Regal, Rooney & Wandas, 1994; Smith, Haas, & Belcher, 1994). Yet practitioners continue to make extraordinary claims about the ability of autistics to communicate, and refuse to believe that they themselves are the ones doing the communication (Bligh & Kupperman, 1993). In some cases, false accusations of sexual abuse have emanated from their keyboards (Dillon, 1993; Green, 1994; Pendergrast, 1995). To borrow a Jungian metaphor, the archetype of the helper is itself a powerful organizing theme that can draw attention away from the darker side of one's methods, as well as the scientific understandings that should - but don't always - inform the discipline (Loftus, Milo & Paddock, 1995).

A Reconstructive View of Memory

The results of a host of empirical investigations point towards an imperfect memory system. Whether or not everything we experience is permanently encoded or not is untestable and really uninteresting. More important is the fact that some things seem unavailable, especially after a time. Even more important is what happens to a memory while it is in storage, and when it is recalled. "Reconstruction" of an event must take place, and we can see the "videotape" model as a special case of reconstruction where the memory is encoded, stored, retrieved and recounted without distortion. The evidence reviewed above points clearly to the implausibility of this position. Rather, there are degrees of imperfection in recall, and conditions that can allow us (like Mark Twain) to "recall things very well that never happened." This latter fact points to the possibility of memories that are less reconstructions than constructions (or confabulations).

In an early examination of the literature on social psychology and memory, F.C. Bartlett stated several tentative principles, which were *"to stand or fall on as more facts become known,"* (1932, p. 267). This was followed by the observation that *"What is beyond dispute is that remembering, in a group, is influenced, as to its manner, directly by the preferred persistent tendencies of that group."* Key to the present work is principle number three:

> *Whenever strong, preferred persistent, social tendencies are subjected to any form of forcible social control (e.g. are disapproved by an incoming superior people, or are opposed to the general immediate trend of social development in the group), social remembering is very apt to take on a constructive and inventive character, either wittingly or unwittingly. Its manner then tends to become assertive, rather dogmatic and confident, and recall will probably be accompanied by excitement and emotion."* (p. 267).

Bartlett's central principle was the schematic organization of memories, in which different weights were unwittingly applied to various elements, most commonly (in humans) in the form of 'interests.' Recalled images involve *"details picked out of 'schemes' and used to facilitate some necessary response to immediate environmental conditions,"* (p. 303), anticipating Spanos' social psychological model of recall under hypnosis or guided imagery.

Although Bartlett's (1932) model did not focus specifically on affectively charged memories, Erdelyi (1990) points out that there is "nothing intrinsic to his theory that precludes

them," (p. 27). Mandler's (1984) schematic model is derived from this perspective, but makes reference to the idea that emotion and cognition are intricately intertwined. Along similar lines, a sophisticated cybernetic view propounded by Oatley (1992) holds that emotions can arise as a result of cognitive processes of which the experiencer is completely unaware, and can become self-maintaining moods when remote from their initial cause. Importantly, Oatley's model also makes reference to emotions as properties of goal structures. As noted in the "self-generated validity" model discussed above, he implies that the effects of clinical interpretation of symptoms can have an effect on the patient's subsequent processing of information:

> *"If the interpretation is accepted, it helps the patient build a model of his or her goal structures to which there is conscious access. In neither case is there any guarantee against suggestive contamination...."* (Oatley, 1992, p. 328).

Bartlett's third principle, which at first glance seems central primarily to the FMS argument, has also highlighted an intriguing parallel between the two main positions in the debate. We must first consider that many RMT advocates acknowledge that it is exactly this principle that is at work in preventing abused children and adults from reporting abuse, in so far as the abusive parent exercises social control over the narrative of the events surrounding the abuse, and uses threats that the victim will be disbelieved to control the situation and avoid disclosure.

Sometimes labelled 'brainwashing,' or 'mind control' these forces are routinely presumed to be part of the "splitting off" of memory (e.g., Hammond, 1992; Lovern, 1993; Olio & Cornell, 1993). Ironically, the kind of authority that therapists' supposed expertise and privileged position granted to them by the client is identified as a key ingredient in this process (e.g., Terr, 1994, p. 177), although the examples are typically parents rather than clinicians.

Such forces are also viewed as being at the root of the fictions that abused people will create to mask an abusive history (Bass & Davis, 1994b; Blume, 1993; Forward & Buck, 1988; Freyd, 1993) - fictions that in cases of severe trauma are sometimes supposed to evolve into separate multiple personalities (Bass & Davis, 1988; Crabtree, 1985; Forward & Buck, 1988; Health and Welfare Canada, 1993; Keyes, 1981). The extreme pattern of dissociation associated with DID is but the psychopathological end of a dissociative continuum which begins with

normal processes of differential daily behaviour as a function of the multiplicity of roles that all people must fulfill (Price, 1987).

There is no small irony in this situation. The process of responding to social pressure with reconstructions and/or fictionalizations is necessary if the usual clinical profile of the traumatized individual is to be accepted (and if constructivist therapies are to work). Even RMT advocates claim that people invent all kinds of stories to avoid "reality" (although that reality is usually taken to be a repressed or dissociated abuse history). Yet when it is suggested that some therapists use comparable techniques to impart an ideological bias to clients through *"a combination of suggestion, misinformation, and conditioning"* (Terr, 1994, p. 176), this principle is immediately disavowed.

The *"making of meaning"* (Olio & Cornell, 1993) or the *"construction of the survivor identity"* (Anderson & Gold, 1994) is an intrinsic part of many therapeutic approaches, and the narratives constructed in therapy are viewed as beneficial. The apparent therapeutic benefit of this process seems to endow these narratives with a ring of authenticity. This, however, is where the danger lies: in failing to distinguish narrative truth from historical truth. Identifying oneself as a survivor may be gratifying and provide some organization to an otherwise chaotic existence. However, it does not validate the memory that logically must be present in order to have survived it. This would be equivalent to claiming that if a person feels better after being regressed to a past life as Napoleon and remembering some details of the battle of Waterloo, it proves that he or she must really have been Napoleon.

Professionals cannot expect clients to critically evaluate what is presented as scientific material. Although it is not necessary to invoke psychopathology to understand the propagation of pseudoscientific beliefs even among the educated and healthy (Eve & Harrold, 1993), the addition of psychopathology and affective disorder to the picture leads us not to predict less credulity, but more. This is particularly true where narrative truth is equated with historical truth. The equation ("abuse is common" + "repression is common" + "you have symptoms" = "you were probably sexually abused") is the basis of a tidy narrative that neatly (if erroneously) constrains the possibilities for a client. Constraining in this way the range of interpretations that can be placed on ambiguous entities such as "an absence of memory" virtually ensures unnecessary psychic conflict, and can reasonably be expected to heighten the likelihood of FMS.

Summary

In the first major section, we saw that in the absence of a memory (and even in the presence of a variety of symptoms), an abuse history is unlikely. Nevertheless, memories of such abuse are being retrieved from people with no previous memories of that sort in numbers that greatly exceed expectations. Indeed, claims such as *"50,000 human sacrifices are committed a year by satanic cults,"* (McShane, 1993, p. 201) are grossly inconsistent with police figures on crime rates (see Putnam, 1991). The absence of corroboration is not taken to be a barrier to the affirmation of the reality of these memories, and subjective reality is routinely confused with historical reality. The methods used to recover these mis-inferred memories have been shown (in the second section) to produce false memories that are indistinguishable from those being "recovered."

The ability of these techniques to produce false memories is consistent with a reconstructive model of memory. Material is encoded, stored, and retrieved imperfectly. While some people may not recall an actual episode of victimization for an extended period of time, it is not (in principle) impossible for this to happen; nor is it impossible for emotion and misinformation to work in concert, and so produce a false memory that is truly believed to be real (Loftus & Hoffman, 1989). The consequences of this can be tragic. When some indistinct set of symptoms is taken to be an invariant result of sexual abuse, and the falsely-inferred past trauma is reified on the basis of an artificial subjective reality, a victim is created, rather than discovered. The methods used to retrieve improbable memories that presumed to have been repressed are not harmless, and it cannot be argued that these techniques "just aren't used"; nor can it be argued that the preponderance of real unrepressed memories of childhood sexual abuse vitiates the small but growing number of false ones. Suitably misled, the human mind can overlay the most recent of materials with a patina of age, and therein lies the greatest threat to the credibility of the actual victims of childhood sexual abuse who are out there.

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Appendix: Tables and Figures

Table 1.

	Age	
	Under 13	13-15
Intercourse	2	8
Any genital contact	13	18
Any contact (non-genital)	13	19
Any event	18	23

(N=215, age is age at time of event, with frequencies in both columns derived from the same set of subjects).

Table 2.

Type of Sex Act Commited Against Child	Female	Male
Bestiality	4	1
Oral/anal act	1	1
Anal penetration-finger	1	3
Anal penetration-object	2	4
Vaginal penetration-object	4	0
Attempted anal penetration-penis	7	6
Vaginal penetration-finger	32	0
Attempted vaginal penetration-penis	54	0
Anal penetration-penis	9	21
Oral/genital act	51	28
Vaginal penetration-penis	192	0
Subtotal	357	64
Fondle/touch breasts, buttocks	100	8
Fondle/touch genital area	120	46
Expose nude body	19	6
Exposed genitalia	51	13
Kiss (mouth or elsewhere)	42	7
Subtotal	332	80

Table 3.

Base Rate of Abuse Assumed	Odds Against Abuse History Given 1 σ above the mean* on:			
	Anxiety	Depression	Dissociation	Sexual Problems
.06	20.37	7.66	9.14	7.50
.07	17.28	6.50	7.75	6.36
.08	14.95	5.62	6.71	5.50
.09	13.15	4.94	5.90	4.84
.10	11.70	4.40	5.25	4.31
.11	10.52	3.96	4.72	3.87
.12	9.53	3.58	4.28	3.51
.13	8.70	3.27	3.90	3.20
.14	7.99	3.00	3.50	2.94
.15	7.37	2.77	3.30	2.71
.16	6.82	2.57	3.06	2.51
.17	6.35	2.39	2.84	2.33
.18	5.92	2.23	2.65	2.18
.19	5.54	2.08	2.48	2.04
.20	5.20	1.95	2.33	1.91
.21	4.89	1.84	2.19	1.80
.22	4.61	1.73	2.06	1.69
.23	4.35	1.63	1.95	1.60
.24	4.11	1.55	1.84	1.51
.25	3.90	1.46	1.75	1.43
.26	3.70	1.39	1.66	1.36
.27	3.51	1.32	1.57	1.29
.28	3.34	1.25	1.50	1.23
.29	3.18	1.19	1.42	1.17
.30	3.03	1.14	1.36	1.11

* Based on Elliott and Briere's (1992) data

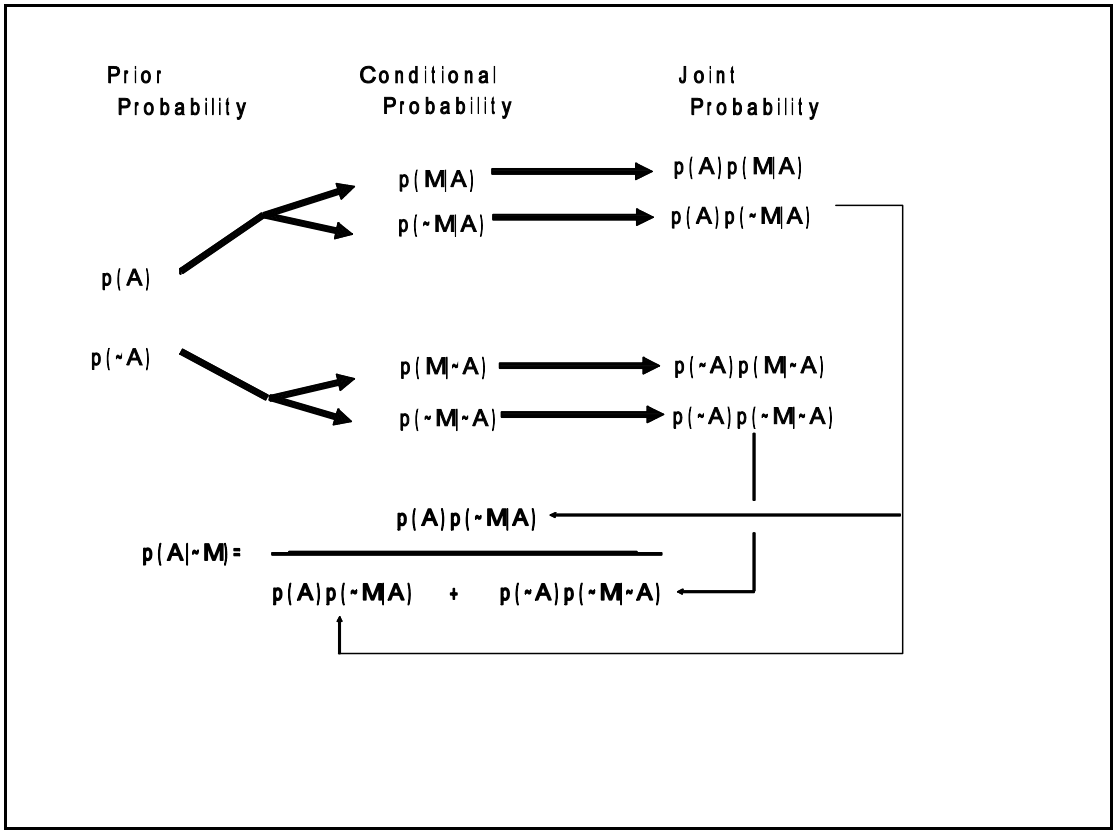
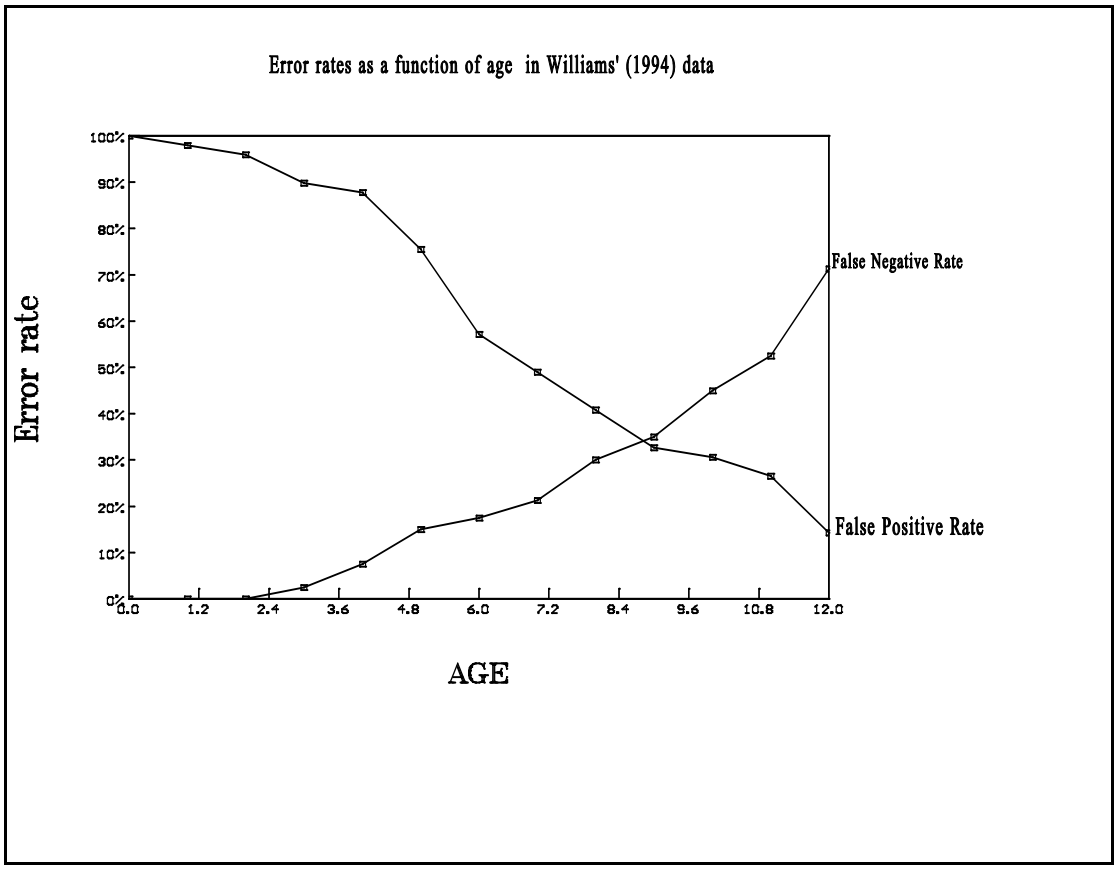

Prior Probability
Conditional Probability
Joint Probability
p(M|A)
p(~M|A)
p(A)p(M|A)
p(A)p(~M|A)
p(A)
p(~A)
p(M|~A)
p(~M|~A)
p(~A)p(M|~A)
p(~A)p(~M|~A)
p(A)p(~M|A)
p(A|~M) =
p(A)p(~M|A) + p(~A)p(~M|~A)

Fig. 1

Fig. 2


Error rates as a function of age in Williams' (1994) data
Error rate
100%
90%
80%
70%
60%
50%
40%
30%
20%
10%
0%
0.0
1.2
2.4
3.6
4.8
6.0
7.2
8.4
9.6
10.8
12.0
AGE
False Negative Rate
False Positive Rate