both cases? Research with individual differences in defensive repression allows more direct testing of the suggested similarity of basic experimental and clinical phenomena: Does naturally occurring repression (i.e., repressive coping style) correspond to (non-defensive) repressive abilities in the laboratory? Given a high natural motivation to repress negative affect, does implicitly probing for defensive repression parallel mechanisms of instructed, non-defensive repression? What situational variables are likely to trigger defensive repression? The latter question has been extensively studied in social psychology. The evidence supports processes of self-denial rather than just social impression management, which can be seen as a parallel to psychodynamic defenses (e.g., Myers & Derakshan 2004). Moreover, in order to avoid the – by definition – biased self-reports of individuals with a repressive coping style, psychophysiological measures can be, and have been, used instead of questionnaires to identify individuals with higher or lower awareness of their own emotional state (e.g., Bonanno et al. 1995). Hence, such an approach parallels the assumption that repression is consciousness lowering, here operationalized as habitually lowered awareness for one's own emotions.

It may be more effective to use personality variables for approximating naturally occurring repression in the laboratory, than to equate the intensity of potentially to-be-repressed, traumatic laboratory stimuli. Further, assuming that unconsciousness is not a necessary condition for repression to occur, convergence between cognitive and emotional repression may be found in studies of emotion regulation. For example, Ochsner et al. (2004) reported that down-regulation of one's own emotions (i.e., emotion repression in a wider sense) may evoke brain activity that is very similar to that found in unemotional contexts such as behavioral inhibition and resolution of interference.

Dialectical repression theory

David H. Gleaves

Department of Psychology, University of Canterbury, Private Bag 4800, Christchurch, 8140, New Zealand.

david.gleaves@canterbury.ac.nz

http://www.psyc.canterbury.ac.nz/people/gleaves.shtml

Abstract: Erdelyi's dialectical repression theory attempts to reconcile what appear to be incompatible perspectives in the contentious area of memory for trauma. He partially succeeds and makes a strong case that repression is "an empirical fact," but makes a weaker case that distortions and omissions are due to the same mechanism and that recovered memories are necessarily unreliable. Available data do not suggest that the return of the repressed is any less accurate than the return of the non-repressed.

Erdelyi's theory of repression in the target article purportedly "integrates the largely dissociated data of the clinic and the laboratory" (sect. 1), the same goal as of the recent paper by Gleaves et al. (2004). Many of his conclusions are similar to ours; others are not. We agree on the obvious, that "Repression has become an empirical fact" (target article, Abstract), and that "the recovery of inaccessible memories has been part of scientific psychology for more than a century" (sect. 4, para. 9). Gleaves et al. have also noted a wealth of experimental data that support the reality of what they term *blocked and recovered memories*. However, Erdelyi goes on to argue that the *return of the repressed* is necessarily associated with distortion, and that such memories shouldn't be trusted for accuracy. Here, disagreement arises (at least to a degree), which I address later in this commentary. The common theme in Erdelyi's theory is that omissions and distortions are both due to repression. Thus, if you believe in false memories, you apparently also believe in repression (and vice versa). Given the polarity of the field regarding false and repressed memory, Erdelyi's attempt might better be considered a *dialectical theory of repression*.

There are many dialectics in the field of memory for trauma such as Widiger and Sankis's (2000) position that "difficulty forgetting (or letting go of) a horrifying experience may simply be the opposite side of the same coin of difficulty remembering (accepting or acknowledging) a horrifying experience" (p. 391). A related dialectic, noted by Erdelyi, is how strong emotion both enhances and degrades memory. However, his discussion of central versus peripheral details only deals with the effects of moderate emotion. At higher levels of stress, amnesia and hypermnesia may occur for different modes of memory. As noted by Jacobs and Nadel (1998), "It [traumatic stress] can cause amnesia for the autobiographical context of the stressful event and hypermnesia for the emotional memories produced by them" (p. 1115). This effect is related to Erdelyi's point that repression "knocks out declarative (conscious) memories, but other memory systems (e.g., procedural ones, as in symptoms) are not similarly affected and continue to reflect remembering" (sect. 4, para. 6). Erdelyi asks if memory stops expressing itself through less direct channels once recovered in the conscious channel, and there are clinical data that suggest this to be the case (Wickramasekera 1994).

Erdelyi moves the field both forward and backward at the same time. He moves it forward by clarifying that the conscious/unconscious distinction allegedly attributed to repression (i.e., that only evidence of an unconscious process counted – see also Gleaves 1996) is a "reconstructive memory distortion operating at the level of the field" (sect. 2, para. 5). As Erdelyi notes, when repression is defined to include conscious or intentional processes, there is no question that it exists, and he is obviously right about that. However, he moves the field backward by overstating the degree to which memory is distorted and subject to reconstruction. This commonly accepted position is not supported by either experimental or clinical data. Erdelyi refers to Wegner's (1989) research demonstrating a rebound effect of thinking about white bears. It is noteworthy that there was not a rebound of pink bears, black bears, or white rabbits. What returned was the same as was initially repressed. In terms of clinical data (not cited by Erdelyi), the only research on the accuracy of recovered versus continuous memories (i.e., those that were never repressed; Dalenberg 1996) suggests that they are equally accurate. Erdelyi also argues that it was also Freud's position that previously repressed material is invariably altered. However, in citing Freud's work, Erdelyi either provides quotes that are out of context (from Freud 1896a/1962), or cites his later work after Freud had abandoned his seduction theory in favour of his fantasy-based reformulation. Regarding the first issue (citing out of context), the quote that "Re-activated memories ... never re-emerge into consciousness unchanged" (sect. 5, para. 10) was from Freud's discussion of obsessional neuroses in which the reactivated (which is not the same as recovered) memory was conjectured to be transformed into the neurotic symptom. This was different from his theory of hysteria in which he did not doubt the accuracy of memories and, in fact, based the theory on the assumption that they were accurate. There is no question that Freud didn't trust unconscious memories in his reformulated theory, and this was allegedly one of his reasons for changing his theory. However, whether or not he was correct in doing so is a different issue (see Gleaves & Hernandez 1999). Erdelyi simply describes Freud's initial theory as "erroneous" when there is far greater evidence that his reformulated theory was erroneous as well as tremendously harmful. Even supporters of the false memory movement (e.g., Powell & Boer 1995) argue that Freud subsequently interpreted real memories of abuse as fantasies, and subsequent to that time, when women reported sexual trauma to psychoanalysts, it was interpreted as sexual fantasy.

All memory is subject to error, but according to Bartlett, mental schemas intrude on and distort memory. That being the

https://doi.org/10.1017/S0140525X06309119 Published online by Cambridge University Press

case, intrusions/distortions are likely to be schema-consistent. No false memory research published to date would suggest otherwise. In the Deese paradigm, participants might misremember the word *sleep* after hearing "*bed, rest, awake, tired, dream, wake, and snooze*" (target article, sect. 5), but they don't misremember the words *rape* or *incest* (see also Freyd & Gleaves 1996). Although this seems obvious, it doesn't stop some writers (including Erdelyi) from arguing that such data support the current false memory movement. When people mis-remember, what they do remember is likely to be consistent with their existing schemas (Pezdek et al. 1997), or perhaps with their actual experiences. Thus, there continues to be no evidence to support the commonly expressed false memory position that that people who come from non-traumatic family environments can be induced to mis-remember their childhoods as traumatic and abusive. Following Freud's mistakes, reports of abuse were interpreted as fantasy. Now they are routinely interpreted as false memory. The *corrective programs* (alluded to by Erdelyi) that are *really* needed are those that will undo such damage.

On the continuing lack of scientific evidence for repression

Harlene Hayne,[a] Maryanne Garry,[b] and Elizabeth F. Loftus[c]

[a]*Department of Psychology, University of Otago, Box 56, Dunedin, New Zealand;* [b]*Victoria University of Wellington, School of Psychology, Box 600, Wellington, New Zealand;* [c]*Department of Social Ecology, 2393 II, University of California, Irvine, CA 92697-7085.*

hayne@psy.otago.ac.nz http://psy.otago.ac.nz/staff/hayne.html
maryanne.garry@vuw.ac.nz
http://www.vuw.ac.nz/psyc/staff/maryanne-garry/index.aspx
eloftus@uci.edu http://www.seweb.uci.edu/faculty/loftus/

Abstract: The forgetting and remembering phenomena that Erdelyi outlines here have little to do with the concept of repression. None of the research that he describes shows that it is possible for people to repress (and then recover) memories for entire, significant, and potentially emotion-laden events. In the absence of scientific evidence, we continue to challenge the validity of the concept of repression.

Right off the bat, Erdelyi comes out swinging. "Repression," he declares, "has become an empirical fact that is at once obvious and problematic" (target article, Abstract). Is it an empirical fact in the same way that, say, gravity is an empirical fact? Well, no. We can demonstrate the effects of gravity simply, quickly, and whenever we want; the same is not true of repression. Is repression a widely accepted theory, in the same way that, say, the theory of evolution is? Well, no. Converging evidence from biology, biochemistry, and anthropology supports evolution. This is not true of repression. Is repression an idea that sounds interesting but turns out to be something that nobody can find any evidence for – like, say, cold fusion? Yes. That's more like what repression is: cold fusion. Repression has become the clinician's cold fusion, at once obvious and problematic.

Erdelyi draws on three research findings to argue that repressed and recovered memories are supported by science: We forget things, we remember things, and sometimes, if we try really hard, we remember more things over time. Take, for example, Erdelyi's appeal to studies by Bartlett and Ballard. Yes, subjects remembered more studied material if they attempted to recall it over and over again, but they never forgot that they had encountered the material in the first place. Likewise, patient "B." never forgot having a brother, and never forgot that he had died. These examples tell us a lot about our ability to forget and then remember specific details about a particular event, but they tell us nothing about the wholesale repression of an entire experience, particularly one we can later recall in great detail.

Erdelyi also uses research by Anderson and his colleagues to validate the notion of repression, but what do the Anderson studies actually show? In Anderson and Green (2001), subjects first learned a set of word pairs (such as *ordeal*, *roach*). Next, they saw one word from a pair (*ordeal*) and tried to recall its partner (*roach*), tried to suppress its partner, or did not see the words from a pair. Finally, subjects took a memory test: they were shown the first word of the pair, and they tried to recall the second word. The crucial question was whether subjects would be less likely to report words they had tried to suppress compared to words they had not studied in the second phase (baseline). Anderson and Green found that sometimes being asked to suppress the word impaired memory for it. They concluded that their results "support a suppression mechanism that pushes unwanted memories out of awareness, as posited by Freud" (Anderson & Green 2001, p. 368).

But a closer look at these findings shows us that whatever mechanism was operating, it was not pushing many memories anywhere. Even after 16 suppression attempts, people still recalled the words about 75% of the time. Although this recall rate was below baseline (about 83%), it wasn't overwhelmingly lower, and, more to the point, we assume that none of the subjects had forgotten that they had participated in the experiment in the first place. Once again, research on the forgetting of small bits of information tells us nothing about repression of entire events.

More recently, Anderson et al. (2004) used fMRI to measure subjects' brain activity during the phase when they were recalling or suppressing. Different patterns of brain activity occurred when subjects tried to recall words than when they tried to suppress them. For example, when subjects tried to remember words, they showed more activity in the hippocampus; when they tried to suppress words, they showed more activity in other regions. This is not support for "repression." Rather, brain activity in other regions undoubtedly reflects subjects' attempts to think about something else, shuttling cognitive resources elsewhere.

It is also worth noting that others have not replicated Anderson's findings, despite several careful attempts to do so (Bulevich et al., in press). Even if we assume the suppression effect does occur, it seems small and unreliable. Erdelyi argues that the architecture of repression is agnostic to content – that we must distinguish between the mechanism and the reason the mechanism is unleashed. Yet, if Freud himself had described a mechanism that interferes with recall only 8% of the time, he would have been a footnote in the history of science, and none of us would have lived through the memory wars.

Erdelyi describes decades' of research on memory distortions as supporting Freud's view about the reconstructive nature of memory; he tells us that Freud knew that memory was reconstructive. Yet Erdelyi omits the relatively recent work showing just how extreme this reconstructive process can be. In the past decade, nine published studies conducted with adults reveal a 33% reporting rate for rich false memories for made-up experiences ranging from spilling punch at a wedding, to being attacked by an animal, to riding in a hot air balloon (Strange et al. 2005); the false memory rate for children is even higher (Gross et al. 2006). Freud never anticipated this capability of the human mind, or the role of therapy in helping to construct such rich false memories (Loftus & Bernstein 2005).

Ironically, for all Erdelyi's insistence that we could have spared ourselves a century of wrongful convictions and divided families if only we had listened to Freud, we find ourselves thinking the same thing about psychological science: If only people would listen. Many years ago, when scientists found that post-event information could alter memories for details, many skeptics viewed the findings as neat laboratory "tricks," but questioned their clinical or legal significance. When wrongfully convicted people were exonerated by DNA analysis, some skeptics stopped criticizing and started listening. Scientists tackling

https://doi.org/10.1017/S0140525X06309119 Published online by Cambridge University Press