What about betrayals, such as adultery, that do not meet criteria for abuse – are these also repressed? One striking phenomenon is when adultery is obvious to those outside the relationship, but the wronged partner is apparently unaware of the betrayal. I have conducted interviews with individuals who later discovered they had been in exactly this situation. In each case my informants described feeling dependent upon their partners. This appears to be *betrayal blindness* (Freyd 1996; 1999) – by not knowing about the adultery, a crisis is forestalled and a necessary relationship is "protected."

Awareness of betrayal is destabilizing. Zurbriggen (2005) has argued that a similar process can occur when people systematically remain unaware of observable betrayals by their own government. One motivation for remaining unaware is likely dependence upon the government for protection from threat. Shay (1994) also pointed to betrayal (particularly by commanding officers) as a key factor in soldiers' harm from war. War memories are among those must susceptible to forgetting (Elliott 1997; Grinker & Spiegel 1945; Sargant & Slater 1941; Thom & Fenton 1920).

It is commonly stated that the primary motive for repression is avoidance of psychic pain. Yet pain is needed to motivate behavior, and lack of pain can be dangerous. Rather than avoidance of pain per se, I argue that repression is most likely when it serves a function such as protecting a necessary relationship. When betrayal is fully perceived, the resulting pain may motivate withdrawal or confrontation. In a betrayal trauma, this reaction could result in an even worse situation for the betrayed person; thus repression is adaptive. There is an analogous effect for physical pain – if mobility is sufficiently urgent for survival, even a serious injury may be ignored. The usual processes that produce severe pain and related unwillingness to move can be spontaneously suppressed in the right circumstances. This is not due to a desire to avoid pain, but a survival need that overrides the usual pain/immobility response. So, too, for psychic repression. Consistent with this, it is apparently more difficult to forget the death of one's own child than many other traumatic experiences (Elliott 1997).

Sharing or not sharing information about events has implications for the memorability of those events. A lack of social sharing of information may lead to decreased opportunities to rehearse information, encode it in more shareable and stable forms (Freyd 1983; 1990; 1996), and eventually impact recall (Freyd 1996; 2003). Consistent with this, child sexual abuse has a strikingly low disclosure rate and also has some of the highest rates of reduced recall (Elliott 1997; Freyd et al. 2005). Similarly, Foynes et al. (2006) found that non-disclosure of childhood abuse was predicted by the closeness of the perpetrator, just as reduced recall is so predicted (Freyd et al. 2001).

Although Erdelyi makes great progress toward a unified theory of repression, he largely neglects a crucial aspect of repression – the social psychological forces and motives. It is of course the case that individual nervous systems instantiate psychological repression, but a full understanding of the phenomena will not occur if the individual psyche is studied in a vacuum. Betrayal and other social forces must be part of a unified theory of repression.

NOTE

1. The words "repress" and "repression" are contested and often avoided, but in this context I follow Erdelyi's target article, which is defining and reclaiming these words.

Forging a link between cognitive and emotional repression

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Abstract: Erdelyi distinguishes between cognitive and emotional forms of repression, but argues that they use the same general mechanism. His discussion of experimental memory findings, on the one hand, and clinical examples, on the other, does indeed indicate considerable overlap. As an in-between level of evidence, research findings on emotion in neuroscience, as well as experimental and social/personality psychology, further support his argument.

Erdelyi's unifying theory convincingly addresses the extensively used, yet frequently misunderstood, concept of repression. He shows the similarities between early definitions in psychodynamic and experimental psychology, literatures largely oblivious of each other. His key point is that repression should be understood as a mechanism regardless of its function, rather than as inherently *defensive*. Repression becomes a universal gatekeeper of the contents of consciousness, irrespective of underlying psychodynamic assumptions and of the nature of to-be-repressed information. Examples, mostly from the experimental memory literature, illustrate the similarity of distortions that render information inaccessible, whether for cognitive or emotional reasons. The question remains: If repression as a mechanism comprises suppression, inhibition, avoidance, and the like, why retain such an embattled concept? The justification reverts to psychodynamic traditions and therefore to defensive or emotional forms of repression.

Defensive repression is primarily a phenomenon of emotion, not memory. Repression of memory content is just one of many possible consequences. Under some circumstances, emotional information may be "repressed" on account of an anticipated or associated unwanted emotional state, rather than because the memory content per se is unwanted (or instructed not to be wanted). Erdelyi summons substantial support from the memory literature for the view that the means by which these forms of repression function are essentially the same. We propose to show that, in fact, the link between the experimental and the clinical findings is even closer than appears from the evidence presented in the target article.

Erdelyi cautions against inducing unbearable stress in the laboratory so as to mimic trauma and potentially trigger defensive repression, thereby questioning the validity of inferences from experimental to clinical findings. However, endocrinological and social psychology research that utilized various forms of stress induction found memory performance that is well in line with Erdelyi's assumptions. For example, experimental manipulations of stress-hormone levels show evidence for both memory enhancing and memory-impairing effects. Consistent with Erdelyi's comment about temporal shifts from hypermnesia to amnesia, delicate temporal dynamics of hormonal shifts seem to be a mediating variable. Administering adrenal glucocorticoid before memory retrieval generally impairs performance, whereas before memory encoding, it enhances performance (Het et al. 2005). This dissociation also obtains with psychosocial rather than direct hormonal stress manipulation (Kuhlmann et al. 2005).

Further, to forge the link between cognitive and emotional forms of repression, the extensive body of literature on the so-called repressive coping style is worth considering in more detail. Erdelyi questions the manner in which repressive coping style is usually established (by questionnaire measures: individuals with low self-reported anxiety and high social desirability). He prefers that individuals be selected by their ability to repress (*not-think* of) some target material, regardless of the reason. What would probably be achieved by such approach is the classification of individuals by their abilities in working memory, executive control functions, fluid intelligence, and so forth. This would provide a basis for personality differences in repressive abilities if, but only if, repression is studied as a general mechanism. However, for the emotional aspect of repression, the investigation of individual differences in defensive repression forms a critical link between experimental and clinical psychology, and this should not be underestimated. Is conventionally measured repressive coping style indeed associated with a higher capability for general (and not just defensive) repression, and is the mechanism the same in

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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

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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

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