BEHAVIORAL AND BRAIN SCIENCES (2006) **29**, 499–551
Printed in the United States of America

The unified theory of repression

Matthew Hugh Erdelyi
Department of Psychology, Brooklyn College and the Graduate School, The City University of New York, Brooklyn, NY 11210-2889
iyledre@patmedia.net

Abstract: Repression has become an empirical fact that is at once obvious and problematic. Fragmented clinical and laboratory traditions and disputed terminology have resulted in a Babel of misunderstandings in which false distinctions are imposed (e.g., between repression and suppression) and necessary distinctions not drawn (e.g., between the mechanism and the use to which it is put, defense being just one). "Repression" was introduced by Herbart to designate the (nondefensive) inhibition of ideas by other ideas in their struggle for consciousness. Freud adapted repression to the defensive inhibition of "unbearable" mental contents. Substantial experimental literatures on attentional biases, thought avoidance, interference, and intentional forgetting exist, the oldest prototype being the work of Ebbinghaus, who showed that intentional avoidance of memories results in their progressive forgetting over time. It has now become clear, as clinicians had claimed, that the inaccessible materials are often available and emerge indirectly (e.g., procedurally, implicitly). It is also now established that the Ebbinghaus retention function can be partly reversed, with resulting increases of conscious memory over time (*hypermnesia*). Freud's clinical experience revealed early on that exclusion from consciousness was effected not just by simple repression (inhibition) but also by a variety of distorting techniques, some deployed to degrade latent contents (*denial*), all eventually subsumed under the rubric of *defense mechanisms* ("repression in the widest sense"). Freudian and Bartlettian distortions are essentially the same, even in name, except for motive (cognitive vs. emotional), and experimentally induced false memories and other "memory illusions" are laboratory analogs of self-induced distortions.

Keywords: avoidance; Bartlett; defense; denial; distortion; Ebbinghaus; false-memories; Freud; inhibition; repression; suppression

1. Introduction

Repression has been a puzzle for scientific psychology. It is not clear to many, even at this date, whether repression is best regarded as an obvious fact of mental life or an outright (and even dangerous) myth. In this article I sketch out a theory of repression that integrates the largely dissociated data of the clinic and the laboratory into a unified framework that is simple, rich – and right.

The article is organized into four sections besides this Introduction. First, in a historical analysis (sect. 2), I show that the classic conception of repression, from Herbart to Freud, is consistent with modern laboratory research, but that confusion has resulted from a semantic distortion introduced, ironically, by Anna Freud, who insisted that repression needed to be an unconscious process, its conscious counterpart being "suppression." Sigmund Freud, actually, used repression and suppression interchangeably and insisted on "the unity of mental life" across the conscious–unconscious continuum, so that "repression" could be *both* conscious and unconscious. The historical analysis is thought to be important because it dissolves much of the controversy surrounding repression. Building on this historical foundation, the third section articulates the unified theory of repression that is being proposed. Repression, conceived of as a class of consciousness-lowering processes, is divided into two subclasses, inhibitory and elaborative processes. *Inhibitory* (or *simple*) *repression* involves cognitive avoidance (not-thinking) of some target material and leads to loss of accessible memory. Some of the lost memory may, however, express itself indirectly and may be partially recovered with subsequent retrieval effort. *Elaborative repression* distorts the original memory through a variety of transformations and false additions. These two subclasses of memory degradation subsume most of the classic clinical manifestations of repression and, critically, are extensively buttressed, as shown in Sections 4 (on inhibition) and 5 (on elaborative distortions), by the experimental literature. Consequently, a viable unified framework for repression is afforded.

2. History and definition of repression, including distortions of the concept

Although traditionally associated with Freud, the term as well as basic concept of *repression* was introduced into psychology more than half a century before the advent of psychoanalysis by one of the founders of scientific

MATTHEW ERDELYI was born in Budapest, Hungary, lived in Venezuela for several years, and finally settled in the United States, where he obtained his Ph.D. at Yale. He speaks no language without an accent. He is Professor of Psychology and formerly the Stern Professor of Humor at Brooklyn College, City University of New York. Erdelyi has pursued experimental and theoretical work on subliminal perception, the recovery of inaccessible memories, and psychodynamics. Along with many articles, he has published two books, *Psychoanalysis: Freud's Cognitive Psychology* (1985) and *The Recovery of Unconscious Memories: Hypermnesia and Reminiscence* (1996). He has been a visiting scholar at various universities and was a Fellow at the Center for Advanced Study in the Behavioral Sciences in Stanford.

© 2006 Cambridge University Press 0140-525x/06 $12.50

https://doi.org/10.1017/S0140525X06009113 Published online by Cambridge University Press

psychology, Johann Herbart (1824–1825), to designate the inhibition of ideas by other ideas. Herbart's "repression" was not a defensive repression. It is important to distinguish between *the mechanism* and *the defense* (Anderson & Green 2001; Erdelyi 1993). The mechanism is the basic process; the defense is the use to which the mechanism is put – and there could be many uses other than defense. For Herbart, repression was a necessary consequence of the limited capacity of consciousness. By being aware of one idea, we necessarily foreclose being aware of another idea. According to Herbart, the inhibited ideas do not cease to exist but pass into a "state of tendency" – the philosophically correct term of those days for "unconscious" – and fall below the "threshold of consciousness." The repressed ideas can, with changed circumstances or in recombinations, overthrow the ideas currently in consciousness and repress them in turn.

Freud, who was influenced by Herbart, at least indirectly (see Sand 1988), also maintained that repressed ideas do not cease to exist. Later in this article I examine some experimental data that bear on this controversial assumption. Even before the creation of psychoanalysis, while he was still tinkering with hypnotherapy and catharsis, Freud already espoused the notion of defensive repression (though "repression" was not the first term he used), hewing to a model of mind reminiscent of Herbart's dynamic model of consciousness. Freud parted with the reigning philosophical assumption of his day that the mind was coterminous with consciousness – as we know, consciousness became for Freud, as it has now become for scientific psychology, a mere tip of the psychological iceberg – and conceived of the conflict unfolding on a deeper plane: Psychological subsystems, not all accessible to consciousness, were in conflict with each other and tried to inhibit the operation and accessibility of the other.

Some years before *The Interpretation of Dreams* (Freud 1900/1953), which for many marks the beginning of psychoanalysis, Freud (1892–1893/1966) published a two-part case history of a patient who was having problems breast-feeding her baby. Freud sketched out a dynamic formulation of the woman's problem. She was conceived to be in conflict – without being aware of the conflict: She both *wished* and *did not wish* to breast-feed her child. Because awareness of her reluctance would have been painful to her, she "inhibited the antithetic idea" (the reluctance to breast-feed) and "dissociated" it from her awareness. The antithetic idea does not thereby get abolished but "establishes itself, so to speak, as a '*counter-will*', while the patient is aware with astonishment of having a will which is resolute but powerless" (p. 122).

The terms *dissociated* and *inhibited* were soon joined by *repression* and *suppression* in Breuer and Freud's (1893/1955) "Preliminary Communications," which became the first section of their 1895 classic, *Studies on Hysteria* (in my view, the effective starting point of psychoanalysis). In this work Freud and Breuer write:

> It was a question of things which the patient wished to forget, and therefore intentionally repressed from his conscious thought and inhibited and suppressed. (Breuer & Freud 1895/1955, p. 10)

Some 20 years later, in his article "Repression," Freud (1915a/1957/1963) provided a straightforward definition of repression: "*the essence of repression lies simply in the function of rejecting and keeping something out of consciousness*" (p. 147, emphasis in the original).

It should be noted that there is no mention in either the 1895 formulation or the 1915 definition that repression – the process – is itself unconscious. Yet, introductory texts in psychology or psychoanalysis treat as axiomatic that the defenses, including repression, are *unconscious* processes. The conscious counterpart of repression, it is averred, is "suppression." Yet, as I have been suggesting for some time (e.g., Erdelyi 1990; 1993; 2001a), this rendering of repression as a necessarily unconscious process, and its distinction from suppression, constitutes a "grand-Bartlett effect" – a reconstructive memory distortion operating at the level of the field. Although Freud's half-century of psychological writing on repression is not without some ambiguities and even contradictions, the overwhelming textual evidence is that Freud used repression and suppression interchangeably, from his earliest writings (e.g., 1893, as we have seen) to his last (e.g., *An Outline of Psycho-Analysis*, Freud 1940/1964).

It was *Anna* Freud (1936/1937), trying to tidy up her father's often messy work, who introduced in her book, *The Ego and the Mechanisms of Defence*, the diktat that repression was unconscious and suppression was its conscious counterpart. This was, by the way, a retrograde tack, since her father had pursued a Gordian knot-cutting strategy of insisting on the *continuity of mental life*, whether for "thought," "resistance," "guilt," or other mental processes. Thus, "it would be unjustifiable and inexpedient to make a break in the unity of mental life for the sake of propping up a definition" (Sigmund Freud 1938/1964, p. 286). Complex mental processes could be conscious *or* unconscious, according to Freud père; they did not become something else just because they crossed some hypothetical threshold of consciousness. (For the view that repression can be conscious or unconscious, see also Brewin [2003] and McNally [2003]).

I make a fuss about this semantic point because if the unconscious-rendering process, repression, can be both conscious and unconscious, then there is no controversy. If repression = suppression, then everybody believes in repression. The *myth* morphs into the obvious.

Let us return to Sigmund Freud's first use of "repression" (Breuer & Freud 1895/1955, p. 10). Sensing a problem here, the later psychoanalytic establishment deployed the following explanatory footnote:

> This is the first appearance of the term "repressed" ('*verdrängt*') in what is to be its psycho-analytic sense. . . . The word "intentionally" merely indicates the existence of a motive and carries no implication of *conscious* intention. (Footnote 1, in Breuer & Freud [1895/1955], *Studies on Hysteria*, p. 10)

Although I have left out some of the arguments in the long, full footnote, it is hard to reconcile with the case studies making up the *Studies on Hysteria*. The patients discussed are often deliberately and consciously pushing out some memory or thought from consciousness. Indeed, at one point, Freud uses the expression, "conscious rejection" (p. 134). In his already-cited article "Repression," Freud (1915a/1957/1963) speaks of the "censorship of the conscious" (pp. 149–150) and actually feels obliged to warn the reader against assuming that repression is always a conscious undertaking: "it is a mistake to emphasize only

https://doi.org/10.1017/S0140525X06009113 Published online by Cambridge University Press

the repulsion which operates from the direction of the conscious upon what is to be repressed" (p. 148). In the Case of the Psychotic Dr. Schreber, Freud states that repression "emanates from the highly developed systems of the ego – systems which are capable of being conscious" (Freud 1911/1958, p. 67). In the Case of the "Rat Man" (Freud 1909/1955), thought-avoidance is effected by conscious thought-distraction: "His original intention ... had been repressed by his praying" (p. 193). In a footnote added in 1914 to *The Interpretation of Dreams*, Freud remarks, "In any account of the theory of repression it would have to be laid down that a thought becomes repressed as a result of the continued influence upon it of two factors: It is pushed from the one side (by the censorship of *Cs.*) and from the other (by the *Ucs.*)" (1900/1953, p. 547). In his article, "The Unconscious," Freud (1915b/1957) states, "Consciousness stands in no simple relation ... to repression (p. 192).

There are occasional materials tending to point in the opposite direction. For example, in an 1896 paper, Freud places at one point a parenthetical "unconscious" in front of the term "defense" – "(unconscious) defence" (Freud 1896a/1962). In *The Interpretation of Dreams* (1900/1953), Freud suggests that repression "lays more stress" than suppression "upon the fact of attachment to the unconscious" (p. 606). But such materials are isolated and even ambiguous tidbits in a massive corpus.

In psychiatry and clinical psychology of the last two decades, the conscious–unconscious issue in repression and defense has played out in contradictory trends. In the *DSM-III-R* (American Psychiatric Association 1987), the treatment of the material is in the revisionist vein. *Defense*, which had been conceived of by Freud as a voluntary ("intentional," "motivated") act, is rendered as "relatively involuntary" (p. 393). In a formulation that touches neither on the question of intention nor the conscious or unconscious nature of the process, *repression* is defined as "a mechanism in which the person is unable to remember or be cognitively aware of disturbing wishes, feelings, thoughts, or experiences" (p. 394). Completing the revisionist arc, Phebe Cramer (1998; 2000) disallows both *consciousness* and *intentionality* in defense mechanisms; when they are present, the processes are recast – and forcefully distinguished from defense mechanisms – as *coping mechanisms*. The *DSM-IV-TR* (American Psychiatric Association 2000) significantly backtracks, however: Defense mechanisms and "coping styles" are treated as equivalent (p. 807), and defense mechanisms are described as processes of which "individuals are *often* unaware (p. 807, emphasis added), even if it is suggested, somewhat self-contradictorily, that "*Defense mechanisms* (or coping styles) are automatic psychological processes" (p. 807). It is no longer clear what, if any, substantive distinction is retained between *repression* ("expelling disturbing wishes, thoughts, or experiences from conscious awareness") and *suppression* ("intentionally avoiding thinking about disturbing problems" [p. 813]). Recently, George Vaillant (1998) has urged "a way to circumvent the false dichotomy of conscious/unconscious" (p. 1156).

In a strict psychophysical sense, true subliminality is problematical because there may be no true limen (see Erdelyi 1986; 2004a; 2004b; Macmillan 1986; Merikle & Reingold 1991) and therefore the conscious–unconscious distinction may not be categorical but polar, representing opposite ends on a grainy continuum – much like the distinctions between child and adult, day and night, perception and memory (Erdelyi 1986; 1996; 2004a; 2004b). Freud himself, in his later writings, began to sense this problem. An inspection of his 1923 and 1933 diagrams of the structural model (Freud 1923/1961, p. 24; 1933/1964, p. 78) suggests that there is no limen or threshold separating consciousness from unconsciousness, but a fuzzy region in which the two merge. Thus, even if one sought it, a crisp, categorical distinction between suppression and repression may be unrealizable.[1] Laboratory evidence shows (see sect. 4) that conscious memory waxes and wanes over time. Consciousness for the past, therefore, is not either-or but an integral (as in calculus) over time, varying with the time at which testing begins and the temporal window over which it is evaluated (Erdelyi 1996; 2004a; 2004b; Ionescu & Erdelyi 1992).

It is noteworthy that none of the experiments in the laboratory literature evaluated by David Holmes (1974; 1990) in his influential reviews, which caused him to suggest the abandonment of repression for want of laboratory confirmation, ever sought to establish that the variable investigated (repression) was unconscious. Thus, none of the evaluated experiments satisfied Holmes's own criterion for repression. It should be noted also that the trait approach to repression, in which "repressiveness" is assessed by personality tests (e.g., Byrne 1961; Weinberger 1990; Weinberger et al. 1979), cannot establish whether the putative repressors are doing their repressing unconsciously. Not surprisingly, in a recent review of defense mechanism measures, Davidson and MacGregor (1998) found that no instrument succeeded in establishing the unconscious criterion for defense mechanisms (see their Table 1, p. 984).

In recent years, the idea of repression as inhibition or suppression of selected mental contents has increasingly moved into the mainstream of psychology. An important development in the experimental literature on memory has been the recognition of "retrieval inhibition" as a "missing link" in intentional forgetting (Bjork 1989; Geiselman et al. 1983). The influence of neuroscience has pushed the field past pat computer models to more realistic biological conceptualizations and has led to an appreciation of the evolution of the brain yielding inhibition by higher-order regions of more primitive ones (e.g., Bjork 1989). In a recent book on repression, Michael Billig (1999) suggests that normal language development involves the mastering and automatization of skills for both expressing *and* repressing ideas. Evolutionary psychology (e.g., Badcock 2000; Wright 1994) has pointed to the importance of repression – and self-deception in general – for various adaptive purposes, including the more effective deception of others. Anderson and his colleagues (e.g., Anderson 2001; Anderson & Green 2001; Levy & Anderson 2002) have experimentally underscored the "adaptive" function of retrieval inhibition for the purpose of avoiding potentially interfering or otherwise unwanted memories. In recent neuroscience research, Anderson and colleagues (Anderson et al. 2004) identify a neurological circuit involved in intentional retrieval-inhibition and consequent forgetting: Activation of prefrontal cortical areas (associated with "executive

https://doi.org/10.1017/S0140525X06009113 Published online by Cambridge University Press

control functions") results in the inhibition of hippocampal activity, with resultant declines in accessible memory. Some convergence is thus in evidence.

A final task remains in this historical overview: to differentiate between Freud's *repression in the narrow sense* and *repression in the general sense*. In Freud's earliest writings, he meant by repression the simple inhibition or exclusion from consciousness of some idea or impulse or memory. Almost immediately, as his clinical experience forced him to be more nuanced about his material, he noticed that the process of repression could be partial. In 1894, for example, he noted the phenomenon of isolation (or intellectualization): Certain patients seemed to dissociate the *factual* and *affective* components of an idea and repress *only* the affective part. Such individuals could evidence endless conscious knowledge of the facts and yet be deeply out of touch with the emotional aspects of these facts. (Laboratory psychology, in its experimental assays, has ignored such variations on the basic process.) With further clinical experience, Freud was able to identify a whole basketful of such partial repressions, including, *projection*, *reaction formation*, *displacement*, *symbolization*, *rationalization*, and even *representation by triviality*. At first, Freud treated all these inhibitions or distortions of reality as "repression," but by 1926, he felt compelled to simplify for fear of losing his original simple concept of repression (inhibition, suppression, etc.). And so he made a distinction between repression in the narrow (original) sense and "repression in the widest sense" (Freud 1937/1964, p. 257), which subsumed all the defensive mechanisms, including repression in the narrow sense, and renamed the whole set *defense processes* or *defense mechanisms*. (For example: "It is of advantage to distinguish the more general notion of 'defence' from 'repression.' Repression is only one of the mechanisms which defence makes use of" [Freud 1926a/1959, p. 114].)

3. The unified theory of repression

The theory is *unified* in that it integrates largely dissociated traditions of the clinic and laboratory. It builds substantially on the just-reviewed work of Sigmund Freud (with some emendations) and harmonizes this material with well-established experimental knowledge derived from the psychological laboratory and, to some extent, from neuroscience.

3.1. Conceptualization of repression

Repression, essentially, is a *consciousness-lowering process*. It consists of a class of operations that reduces the accessibility to consciousness of some target material. Repression, further, is *instrumental* in bringing about a discernible goal – for example, preventing the span of consciousness from being breached or preventing some intolerable psychological material from entering consciousness.

The null hypothesis is adopted that repression is the same mechanism whether it is used for defense or some other purpose. Only when the goal of repression is to defend against psychological distress is the mechanism of repression conceived of as a mechanism of defense (see also Erdelyi 1990; 1993). Thus, the mechanism of repression can be probed in the laboratory irrespective of its goal.

In the unified theory, repression is divided into two subclasses: (1) *inhibitory* or *subtractive processes* (e.g., degrading the "signal"), and (2) *elaborative* or *additive processes* (e.g., adding "noise" to the signal). This division aligns with the psychological development of the concept both in Freudian and experimental psychology. As noted in the preceding historical review, Freud's initial conception of "repression" was of the inhibitory or subtractive variety ("suppression," "inhibition," "dissociation") followed in short order by a number of elaborative "transformations" ("constructions," "distortions"), among them, *rationalization*, *projection*, *reversal*, *displacement*, *symbolization*. Experimental psychology, similarly, began with subtractive processes – for example, cognitive avoidance that results in forgetting (Ebbinghaus) – followed gradually, as the field began to deal with more complex stimuli (short stories instead of lists of nonsense syllables or words), with constructive/reconstructive processes (e.g., Bartlett). This two-part division of repression is easily accommodated by Bartlett since one of his basic "reconstructions" is *omission* (e.g., Bartlett 1932, p. 66, pp. 99–102, pp. 125–126), with the other operations comprising standard elaborative reconstructions (see sect. 5.1).[2]

3.2. Inhibitory or subtractive processes

In inhibitory repression the consciousness-lowering operation is readily conceived of as some type of psychological subtraction that results in lower consciousness (e.g., we subtract attentional allocation from a channel, we reduce or eliminate thinking about some material).

3.2.1. Simple inhibition. In simple inhibition, about which a huge amount of research literature has accumulated (see sect. 4), some specific target (a list of nonsense syllables, some dreadful event, a taboo desire) is inhibited or subtracted out from consciousness. The classic clinical literature, dissociated from laboratory research for most of its history, did not assimilate the Ebbinghausian tradition and conflated the inhibitory operation with the resulting effects on memory. In the present work, the two are separated: Inhibition of consciousness for some target material results, over time, in the progressive degradation of accessible memory for the target material. Thus, remove the inhibition, and the material – at least some of it – will have been forgotten. Contrary to Ebbinghaus, however, *forgetting does not imply decay* – permanent loss – of the original material. With *retrieval effort* ("retrieval time" [Roediger & Thorpe 1978]; "the work of remembering" [e.g., Breuer & Freud 1895/1955]), the forgotten partially returns: With retrieval effort, memory is hypermnesic (enhanced), just as it is amnesic (diminished) with retrieval inhibition. Thus, Ebbinghaus represents only one side of what happens to memory over time.

Here is a clinical fragment, The Case of B., which illustrates concretely some of the foregoing proposals and which points to several additional considerations that bear on inhibitory repression:

> B., a young man who sought therapy for a pathological grief reaction to the death of his brother five years earlier, noted in his intake interview that he avoids thinking of the traumatic

https://doi.org/10.1017/S0140525X06009113 Published online by Cambridge University Press

event. (For example: "I clearly avoided to know anything; everything I know bothers me" [1st Evaluation Interview]; "I couldn't metabolize-digest the event. It makes me still sad – don't feel comfortable discussing" [1st Therapy Session]; "I don't think about many details of things that happened. I don't remember too many different situations about him, I just remember him ... like smoke .... It's blurry, it's not a very clear view" [3rd Therapy Session]. But, by the fourth therapy session, B. states: "I am [now] thinking more [of my brother] – I remember him more."

For years, B. had avoided potential reminders of his brother's death. He could not bear to attend the funeral and later, to avoid the dreaded topic, would lie to acquaintances who inquired after the brother, leading them to believe that the brother was alive. The whole-scale repression of the brother complex was not altogether successful. The repressed brother returned to haunt him in a variety of ways. Past the midpoint of his therapy, when he was complaining of sexual problems, B. reported that the image of his brother's face appeared to him as he was attempting intercourse with his (B.'s) girlfriend. (Adapted from Erdelyi 1993, pp. 137–138, with permission from Prentice Hall)

Although there is no independent evaluation of B.'s ongoing recall level for his brother up to and through his therapy, it is noteworthy that the alleged amnesia and subsequent hypermnesia resemble the effects obtained in the laboratory (see sect. 4): Thought-avoidance yields amnesia and the subsequent refocusing of retrieval effort, hypermnesia. Also worth underscoring is that the material being avoided by B. was a major emotional event and not some innocuous laboratory stimulus. Finally, B. seemed to be conscious of his efforts to avoid thinking of his dead brother.

I now consider some nuances regarding the interpretation of empirical data on defensive inhibition.

3.2.1.1. Inaccessibility associated with emotionality need not imply defense. Consider the experiment on "cognitive masking" by Erdelyi and Appelbaum (1973). Observant Jewish students (pilot work had shown weak effects with mixed groups of students) were flashed a slide with a *swastika* or a *window* (the swastika with arms extended at right angles to form a square framing perpendicular cross-bars) in the center, and neutral symbols circularly arrayed around the central element. No sensitivity (d') differences were found for the swastika versus the window, but there was a d' diminution for the surround items arrayed around the swastika. Was this a case of perceptual defense/repression not being powerful enough to stave off the perception of the center swastika, but sufficient to defend against its peripheral surround? There was a third, identically treated group that was flashed a stimulus with a different central item, a *Star of David*, which also, like the swastika center, produced a diminution of d' for the surround items. On the assumption that the Jewish students did not have a propensity to defend against a Star of David, the conclusion reached was that attention had been captured by either of the emotionally significant center items (swastika or Star of David), resulting in diminished processing resources for the surround items. This type of phenomenon has been persistently observed in the experimental literature, under various labels (e.g., "narrowing of attention," "weapon focus," "mental shock"): Central items are well processed, but the peripheral items are poorly perceived or recalled (see Easterbrook 1959; Loftus & Burns 1982; Schooler & Eich 2000). The amnesia that results from cognitive masking can be retrograde (Loftus & Burns 1982) or anterograde (Erdelyi & Blumenthal 1973) – or it may be amnesia for contemporaneous details (as in Erdelyi & Appelbaum 1973). The relevance of cognitive masking for any putative defense involving decrements in performance with emotional stimuli is that diminished performance may be due to nondefensive allocation of processing resources to other items or channels. Thus, it cannot be automatically assumed that poorer performance in connection with emotional stimuli is a consequence of defensive repression. For this reason it is particularly convenient to have clinical cases (like the Case of B.) in which the patient explicitly asserts a conscious effort to avoid thinking about the disturbing material because of the distress that the material would provoke.

The phenomenon of cognitive masking has significant implications for the contentious area of memory for trauma (for far-ranging overviews, see Brewin 2003; Brown et al. 1998; Brown et al. 1999; Freyd 1996; Kihlstrom 2006; McNally 2003; Pope et al. 1998; Terr 1993; van der Kolk et al. 1996). Does emotionality enhance or degrade memory? The answer, actually, seems to be *yes* and *yes*. There is widespread consensus that emotionality enhances memory for *central* elements of stimuli but degrades memory for *peripheral* items. This simple formulation is readily linked to laboratory studies where the stimulus is relatively simple. In the Erdelyi and Appelbaum (1973) experiment, for example, there is, literally, a central item (e.g., a swastika) and an array of items surrounding the central item. In real-life settings, where the "stimulus" may last minutes, hours, days, and even years, it is not so obvious what is "central" and what is "peripheral," except by post-hoc definition. The simple central-versus-peripheral distinction, in any case, yields an ambiguous answer for *overall* memory, which would be made up of central and peripheral items (however defined). Since, as the current consensus holds, emotionality has opposite effects on central and peripheral items, it is not difficult to conceive of different experiments yielding different conclusions about overall memory for emotional stimuli on the basis of different framings of what constitutes "central" and "peripheral" components.

Perhaps a more basic question is what constitutes "memory," which it would be a mistake to conceive of as some homogenous mass about which a sweeping generalization is possible. Memory, in many significant respects, is a heterogeneous construct, and what might apply to one aspect of memory might not apply to another (see, particularly, Brewin 2003; Erdelyi 1990; 1996; Schooler & Eich 2000; van der Kolk 1994; van der Kolk et al. 1996). Consider the Case of B. Let us assume, for the sake of argument, that what B. states is roughly accurate and that his memory for his dead brother is "blurry," "not ... very clear," "like smoke." Is B. actually suffering psychogenic amnesia for his brother – or the opposite, hypermnesia? The answer, as already suggested, is *both*. For some five years, B. has avoided thinking, talking, or being reminded of his brother. Not surprisingly, his memory for his brother has become sketchy, though, of course, he has not outrightly forgotten his brother. Yet, in significant respects, B.'s problem, as in posttraumatic stress disorder

https://doi.org/10.1017/S0140525X06009113 Published online by Cambridge University Press

(PTSD) (or kindred disorders from the past, such as "traumatic hysteria," or "neurosis"), is that B. remembers abnormally too much. After five years, most of us would have moved past our bereavement for a dead brother, along with its memorial entanglements. Such "fixation" on the past, the failure to move on, is what essentially defined neurosis for Freud. Similarly, for Pierre Janet (1889), "fixed ideas" played out as hysterical phenomena.

3.2.1.2. Temporal dynamics. One phenomenon that is not in dispute in the disputatious literature on traumatic memory is that patients vigorously try to avoid thinking of traumatic materials (e.g., Brewin 2003; Horowitz 1986; McNally 2003; *DSM-IV* 2000). What is not sufficiently emphasized, however, is that this effort need not immediately succeed. The effort at repression may eventually yield the desired result, but it may take time – days, weeks, or months – and so success at inhibitory repression may vary over time. It is also possible that the protracted effort at repression fails – there is no guarantee that any defense will work – and the patient may never succeed at inhibiting the traumatic material. Further, even if success at inhibitory repression is eventually achieved, resulting in diminution of accessible memory over time, there is no guarantee that at a later juncture there might not be a "return of the repressed" (e.g., Freud 1900/1953; 1917/1961/1963) or a "spontaneous recovery" (Pavlov 1927) of some of the material (see also Schacter 1996).

Thus, it may prove critical to consider temporal dynamics in responses to emotional stimuli because time may obscure shifts from hypermnesia to amnesia. Failure of repression at Time 1 may no longer be a failure at Time 2 – or vice versa. The possibility of response pattern reversals over time has only recently begun to be probed (e.g., Calvo & Eysenck 2000; Erdelyi 2004a; 2004b; Mogg & Bradley 1998; Shane & Peterson 2004a).

3.2.1.3. Individual and stimulus differences. A serious flaw in laboratory efforts to demonstrate repression is that they fail to take individual differences into account. As noted already, a few trait measures of "repressiveness" have been introduced that, however, depend on arguable assumptions (e.g., that repressiveness is indexed by the intersection of low anxiety and high social desirability). What would be germane for the present treatment of repression is a straightforward test of ability to repress (not-think) about some target-material, irrespective of reason. Pilot work in my laboratory, in which subjects were asked to avoid thinking or free-associating about a provocative stimulus, suggests, as shown by on-line free-associative fantasy, that subjects differ substantially in the extent to which – and for how long – they can maintain inhibition for the target material (e.g., Moore 2004). Further, as already intimated, there are likely to be substantial individual differences in the techniques of repression favored by different individuals. Thus, experiments using only one type of inhibition or distortion may lack content validity.

Further still, different stimulus materials are not equally likely to trigger defensive repression. On the one hand, the innocuous stimuli of the laboratory may not be sufficient for the purpose and may yield only cognitive masking effects. Traumatic events in the real world, on the other hand, may overwhelm efforts at defensive repression. Finally, stimulus intensity cannot be considered a simple dimension. Different traumatic stimuli – for example, a car accident versus a rape, or a rape by a stranger versus a rape by a caretaker (Freyd 1996) – may have quite different impacts. In sum, a serious effort at scientifically probing defenses requires a more complex approach than has been typical heretofore.

3.2.2. Structured inhibition. Just as a fig leaf is not plastered randomly on the body of a censored nude, defensive repression may be deployed in a nuanced, meaningfully structured way to subtract-out only the most problematic elements of an overall target. *Isolation* (or *intellectualization*), in which a person dissociates – or "isolates" – the factual aspects of an emotional memory from its emotional aspects and inhibits the emotional aspects, is one example of structured inhibition, and it has already been briefly mentioned.

In this section, I focus on *denial*, which is a ubiquitous form of structured repression – and is arguably the most neglected phenomenon of repression in experimental psychology. Denial has been defined in a number of different ways, some overlapping, some conflicting (e.g., Brenner 1973; A. Freud 1936/1937). The present treatment follows the conceptualization of denial as a *defensive failure of insight* (Erdelyi 1985). This conceptualization is broadly consistent with mainstream clinical use as well as everyday parlance (e.g., being "in denial"). The individual in denial does not fail visually to "see" some event but fails to "see" the deeper meaning or significance of the event.

A formalization of denial can be realized by making the classic Freudian distinction between the *manifest* (surface) and *latent* (deep) semantic content of stimuli. Experimental psychology often treats this vertical type of dissociation as suspect, probably because examples are usually taken from the realm of dreams or symptoms, which often yield disagreements about the interpreted latent contents. A methodologically more fruitful venue is the study of jokes (Erdelyi 1985; 1999), to which Freud (1905/1958) devoted a whole book to show how jokes essentially captured the proposals he had set forth for dreams a few years earlier (Freud 1900/1953). In the case of jokes, it is obvious – to anyone who "gets" a joke – that below the surface meaning resides a different meaning. Without interpretation of the latent content there is no "getting" a joke – the interpretation is the *getting*. The interpretation of latent contents is not chancy. Subjects claiming to get a joke usually come up with the same interpretation. Application of signal detection theory (SDT) techniques (ROC curves, d') to measuring the sensitivity of subjects to latent contents of jokes reveals substantial individual differences (with split-half reliabilities easily exceeding $r = .60$) (Bergstein 2006; Bergstein & Erdelyi, in preparation; Nishioka & Kozbelt, in preparation). Thus, the interpretation of latent contents can be reliable and valid, even if some of us are more sensitive to latent contents than others (see also, Erdelyi 2001b).

Freud realized early on that *context* was critical for interpretation. For example: "dream-symbols ... frequently have more than one or even several meanings, and, as with Chinese script, the correct interpretation

https://doi.org/10.1017/S0140525X06009113 Published online by Cambridge University Press

can only be arrived at on each occasion from the context" (Freud 1900/1953, p. 531); "mistakes have meaning [which] can be guessed or confirmed from the attendant circumstance" (Freud 1917/1961/1963, p. 51); "representation by the opposite," as in "ironic jokes" or sarcasm, which can often pose the "danger of being misunderstood," yields its "true meaning" by the speaker's "tone of voice" or by "some accompanying gesture" (Freud 1905/1958, p. 174); and so on. The basic idea seems straightforward enough.

Even so, ambiguities and semantic drifts pervade the subject, both in the current literature (see, e.g., the variegated commentaries on Malcolm Macmillan's [2001] article on interpretation) and in Freud's own writings. Freud routinely defines the *dream work* as the set of techniques by which the latent content is transformed into the manifest content (e.g., Freud 1900/1953; 1917/1961/1963). In his book on jokes, Freud (1905/1958) demonstrates that jokes and dreams employ the "same psychical processes" (p. 88) and that "the joke work is doing the same thing as the dream work," namely (in the case of "tendentious jokes," involving sex or hostility) transforming the "forbidden" or "hidden meaning" into "the outer shell, the façade of the joke" (p. 55), that is, its "surface meaning" (Freud 1917/1961/1963, p. 103). Yet, Freud does not explicitly apply the terminology of "manifest-latent" to jokes, and in other works typically treats the "latent content" as unconscious, which it would not be in jokes, sarcasm, puns, and other figurative modes of expression. Thus, a crisp formalization of the manifest versus latent content distinction is desirable for a scientific treatment of multilayered linguistic materials (see also, Erdelyi 1985; 1999):

Let ε be an event (an utterance, a dream, a mistake), and let $[\varepsilon]$ be some context, that is, a set of other events, external or internal (other utterances, free associations, attendant circumstances, known facts) which the subject brings to bear on the target event, ε. The manifest-latent content distinction arises from the conditionalization of the event, ε, on the context, $[\varepsilon]$, and is defined by the inequality,

$$\varepsilon \neq \varepsilon|[\varepsilon]$$

Thus, an isolated event may not mean the same thing as the self-same event when it is evaluated in a given context. The latent content, $\varepsilon\,|[\varepsilon]$, may be conscious (as in jokes or sarcasm) or unconscious (as in some dreams and errors).

Unduly reductionistic psychology has perennially focused on the surface, manifest content of psychological events, ε, while ignoring the underlying, latent meaning of the psychological event, $\varepsilon\,|[\varepsilon]$, which arises from the interaction of the event with its context. Radical behaviorism, in effect, ensconced a superficial conception of the stimulus in psychology, which was permitted to deal only with palpable ("observable") manifest phenomena. The cognitive revolution may not have fully moved past this phase in regard to repression, restricting its study largely to surface events, when the real action often unfolds at the deeper layer of latent contents, conscious or unconscious.

Thus, denial is not a perceptual failure in the sense that the perceiver fails to "see" ε, or even $[\varepsilon]$, but is a failure of insight, a failure to see – or to remember – the latent content, $\varepsilon\,|[\varepsilon]$.

3.3. Elaborative or additive processes

Denial richly partakes of *both* inhibitory and elaborative aspects of repression. It is inhibitory repression in the sense that $\varepsilon\,|[\varepsilon]$ is inhibited, but it is elaborative or constructive in that the person must decide on what constitutes the operative context, $[\varepsilon]$. (The context includes not only external events but also internal knowledge bearing on the target event.) A person "in denial" will not argue the facts (the ε's) but will argue about what constitutes its relevant context, $[\varepsilon]_a$ versus $[\varepsilon]_b$, and thus fails to extract the latent content or extracts a different latent content.

For research purposes, the issue may be framed as a question of *weights* assigned to the elements of the context, $[\varepsilon]$: $w_1\ \varepsilon_1,\ w_2\ \varepsilon_2,\ w_3\ \varepsilon_3, \ldots\ w_i\ \varepsilon_i,\ \ldots,\ w_n\ \varepsilon_n$. In effect, different weightings, including zero weightings, of each of the contextual elements, yields a different context, and therefore a different latent content. Determining the weights assigned by different subjects, implicitly or explicitly, would be an easy laboratory project, as would be the *change over time* of these weights (with the concomitant changes in the operative context and latent meanings).

If the weights can be thought of as values on an inhibition–excitation continuum, the *underweighting* of unwelcome contextual elements may be understood as another implementation of inhibition. It is quite clear from clinical or everyday examples of denial – see the Case of N., in the next paragraph – that contextual elements are being weighted differently and tendentiously, with drastic changes in the meaning of target events. Thus, not only is $\varepsilon \neq \varepsilon\,|[\varepsilon]$, but $\varepsilon\,|[\varepsilon]_a \neq \varepsilon\,|[\varepsilon]_b$. Different operative contexts, which the person selects from both the external world and from internal knowledge structures, impart different meanings to particular target events. Thus, even without the massive repression of some event, biased forgetting and remembering of contextual elements can sculpt drastically different versions of the reality accessible to consciousness.

Another clinical fragment, The Case of N., is now considered, which illustrates concretely some of the points just discussed and makes a full transition to elaborative types of distortions that are the hallmark of a large swath of defensive processes.

> This patient continually rails against homosexuals, whom he detests with a violent passion. After a brief, unsuccessful marriage, followed by impotence, he began to experience delusions of persecution, according to which the CIA and the FBI were continually observing him with the ultimate purpose of getting him to submit to the sexual advances of Richard Nixon. He gave up all attempts at heterosexual sex, because he would "not make love in public," that is, in front of the lurking agents. He soon came to understand also that his impotence had been imposed on him, via laser rays, by Nixon's agents. Satellites specifically sent up for the purpose began to bombard him with homosexual messages. Finally, he constructed a special protective metal hat, fitted with a complex electrical jamming device. He wore this hat continually, at home and in public places, including restaurants and work (he was soon dismissed). Even so, the messages that he should submit to Nixon increased in intensity and began to "penetrate" at times. Around this period he took all his jackets to a tailor and had the tailor sew up the slits (or flaps) in the back of the jackets. He implored his male acquaintances to do likewise, lest they be taken for "slot-jacket ass panderers."

https://doi.org/10.1017/S0140525X06009113 Published online by Cambridge University Press

> He deplored tight dungarees because they revealed buttocks too openly and therefore constituted a disgraceful invitation to sodomy. He complained that the CIA was spreading rumors that he was a homosexual; indeed they had contrived to find "doubles" for himself and a friend and photographed them – the doubles – in "disgusting" homosexual acts, all for the purpose of blackmail, so that he might submit to the homosexual importunings of the "anarcho-communist sodomite" Gerald Ford, the new President, who, as he now came to realize, was really "behind" the conspiracy. (Nixon, it turned out, was just a "front"). N. consciously hated homosexuals; indeed, the extent of his hatred was worrisome, for he began to mutter darkly about destroying them, seeing himself an innocent heterosexual victim of homosexual persecution. (Adapted from Erdelyi & Goldberg 1979, p. 372, with permission)

N. is in laborious denial of what seems to be a transparent homosexual disposition. At the manifest level, he presents himself as a heterosexual who hates homosexuals, but at the latent level, he is homosexually charged. Clearly, most of us would weight contextual elements differently than N.

There are numerous elaborative transformations that are presumably intended to reduce N.'s consciousness for his underlying homoeroticism. Two notable examples are *projection* (in which the subject and object of a proposition are transposed, e.g., from "I love him" to "He loves me") and *reaction formation* (in which the predicate is reversed into its opposite, e.g., from "I love you" to "I hate you") (Erdelyi 1985; Freud 1909/1955). N. uses another elaborative distortion, *rationalization* (also emphasized by Bartlett), to explain his impotence: He cannot make love to women while being spied upon by the FBI and the CIA agents, and, moreover, Nixon's satellite-launched beams impede his functioning. He does not consider – or, gives zero weight to – the alternate notion that a homosexually inclined man might not be sexually aroused by women. He explains the "homosexual messages" he experiences as emanating from Nixon's satellites – in short, it is not he but Nixon who is the homosexual. His lurid homosexual fantasies are *displaced* upon the CIA-concocted "doubles." There are "rumors" about his being a homosexual but these are attributed to Ford. Apparently, zero weight is given to other reasons for such rumors. *Symbolization* is used copiously by N. He constructs a mechanical defense mechanism, an impressive electronic helmet (he had been an engineer before his breakdown). This defense mechanism, however, fails him, for the homosexual messages get more intense and penetrate at times. He sews up the flaps on the back of his jackets, as if to say, "No entry!"

These strange elaborations or additions are presumably deployed by N. to diminish consciousness for his own homoerotic cravings. He is too disturbed to manage the camouflage effectively, and so his efforts are sadly transparent to most observers. His psychological defenses, like his electronic helmet, are not working properly.

Is N. ever conscious of his homoeroticism? Currently, we have no effective methodology for settling such a question in a given case. If we asked N., he would violently deny it, but it is likely that, as with other appetitive drives, his urges wax and wane, as might his degree of awareness for his cravings. Thus, in the case of unbearable drives, the degradation of the target material (by simple inhibition or more complex reconstructions) is not likely to lead to long-lasting amnesia.

4. What happens to repressed (inhibited, suppressed) memories?

If we instruct a subject to "reject and keep something out of consciousness," can the subject do this, and if yes, what happens to the "something"? Do we have laboratory data on this question? Of course, and in large quantities.

Dan Wegner (1989) has, for example, shown in programmatic research that instructing subjects *not to think* of a WHITE BEAR succeeds in keeping the thought out of consciousness (though not perfectly, since the idea does seep into ongoing thought to some extent). What happens if now the subject is freed from the task of repression (Wegner calls it "suppression")? Wegner claims, based on some of his data, that a *rebound effect* results – a return of the repressed/suppressed phenomenon – in which the subject becomes exaggeratedly involved – "obsessed" – with white bears. The experimental literature, actually, is mixed on this rebound effect, and some recent laboratory work has failed to obtain it (e.g., Anderson & Green 2001; Gildston 1998). It is likely that different stimuli and different subjects will produce different outcomes. Also, different tactics for "keeping something out of consciousness" will likely be more or less effective. *Not-thinking* about some material may be made easier by *else-thinking*, that is, thinking of something else (e.g., the Rat Man's repression-by-prayer in sect. 2). As shown experimentally (e.g., Smith et al. 2003), exposure to massive interference can produce massive disruptions ("memory blocking") of target items, which, however, remain available, as shown by memory cuing.

There has accumulated, further, a considerable experimental literature on "intentional" or "directed" forgetting. Though there have been different explanatory stances taken toward the phenomenon, including selective rehearsal, selective search, and retrieval inhibition (I suspect all of them are partly right and that the effect is multiply determined), there is little question that forgetting can be intentionally induced (e.g., Epstein 1972; Geiselman et al. 1983; Roediger & Crowder 1972; Weiner 1968). Ironically, this may be one of the most classic yields of laboratory psychology. In 1885, the founder of the experimental study of memory, Hermann Ebbinghaus, addressed the question of the fate of perfectly mastered memory materials (lists of nonsense syllables, in this case) when this material was "left to itself," that is, not attended to, thought of, and so on – an easy task with nonsense syllables (Ebbinghaus 1885/1964). The well-known Ebbinghaus "curve of forgetting" (never actually plotted by Ebbinghaus) shows that retention of material intentionally excluded from consciousness declines over time (and, apparently, without a rebound effect). Note that the forgetting is the result of the thought-avoidance, since diminished accessibility is evident when the repression is discontinued at testing. This inaccessibility, or forgetting, does not necessarily mean permanent loss or unavailability of the forgotten. Indeed, the *obvious* results of Ebbinghaus (as he himself viewed them) may be conceptually refractory, even misleading (Erdelyi 1996). Consider the following two questions: (1) What if memory were tested

https://doi.org/10.1017/S0140525X06009113 Published online by Cambridge University Press

differently – or, to put it in starker theoretical terms – what if there were no one memory but a multiplicity of memory systems, then would the single Ebbinghaus function describe the temporal dynamics of all memory systems? (2) What if repression (suppression, thought avoidance) were lifted and the opposite cognitive process instituted, namely, conscious effort to retrieve the previously avoided "something"? With this "work of remembering" (Breuer & Freud 1895/1955; Freud 1914a/1958), can the forgotten be recovered? Or, put differently, can the Ebbinghaus curve reverse course and actually rise with time?

Much research has accumulated on both of these questions, and it has become clear that inaccessibility need not be general, nor permanent (e.g., Ballard 1913; Erdelyi 1996; Payne 1987; Roediger & Thorpe 1978; Tulving 1983; 1987; Tulving & Pearlstone 1966; Tulving & Schacter 1990). It has become commonplace in modern cognitive psychology and neuroscience to posit the existence of multiple memory systems (e.g., "explicit," "implicit," "procedural") which produce contradictory or "dissociated" outputs. Clinical neurology had long since pointed to such memory dissociations, as in the classic report of Claparède (1911) of an amnesic patient unable to recognize Claparède from a previous encounter and yet able in some sense to remember Claparède, or at least his nasty behavior (shaking her hand while holding a pin between his fingers). As a former neuroscientist and the author of an 1891 book on aphasia, Freud recognized kindred phenomena in the psychological clinic, and he advanced the view that hysteric symptoms were "reminiscences" of materials inaccessible to consciousness (Breuer & Freud 1895/1955). In modern times, huge neurological literatures have accumulated establishing the reality of neurologically based memory dissociations (e.g., Cohen & Squire 1980; Corkin 1968; Parkin 1987; Tulving 1983). Further, the modern neuroscience literature has demonstrated that some amnesic patients show part-availability for what appears inaccessible: Despite their failure to remember events consciously, they nevertheless evidence unconscious priming effects (e.g., Weiskrantz et al. 1974) or produce errors that are semantically meaningful (Werner 1956).

Vast laboratory literatures have also accumulated on normal subjects that show similar unconscious priming effects with subliminal presentations: The subjects respond differently or with different reaction times to subsequent supraliminal test stimuli (e.g., Bargh & Chartrand 1999; Bornstein & Pittman 1992; Fisher 1956; Jacoby & Witherspoon 1982; Marcel 1983a; 1983b; Pötzl 1917). In their influential paper, Jacoby and Witherspoon refer to such unconscious memory effects as "remembering without awareness."

Thus, different measures, or perhaps different memory systems, yield different levels of memory at different times, and so the Ebbinghaus function, though reliable, is circumscribed in its generality. The multisystem view implies an alternative to Freud's *conversion* hypothesis (which holds that the materials that are repressed from consciousness are converted into alternative modes of remembering, such as dreams, slips, symptoms, etc.). A simpler hypothesis (Erdelyi 1990; 1993; van der Kolk 1994) is that the alternative modes of remembering are not conversions of the repressed memories but *spared memories*. By this view, 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. (For relevant brain research on the dissociability of declarative memories from their emotional counterparts, see Bechara et al. 1995.) Freud may still turn out to be right, and psychological research may yet show that, once the repressed memory is recovered into the conscious channel, the memory stops expressing itself through the less direct channels. This is a researchable question.

The experimental literature does corroborate Freud's claim (Breuer & Freud 1895/1955) that with "concentration" and repeated recall effort, the inaccessible may eventually be recovered into consciousness. In general, multiple testing produces memory increases (hypermnesia) over time. For example, in two studies reported by Erdelyi and Kleinbard (1978), subjects' recall of a list of pictures was repeatedly tested for a period of a week. Memory improved by more than 50% after the week of protracted testing. (To control for shifts in response bias, subjects were required to produce the same number of recalls in each trial, guessing if necessary.) The Erdelyi-Kleinbard retention curves look like upside-down versions of the Ebbinghaus function, with retention increasing rather than decreasing over time. Interestingly, the corresponding multi-trial tests of memory for words failed to produce memory improvement.

There are two discernible reasons for the difference between the Ebbinghaus and the Erdelyi-Kleinbard functions. The first, and most interesting for the repression issue, is that Ebbinghaus intentionally avoided thinking of the to-be-remembered materials (nondefensively repressed them), whereas the subjects in the Erdelyi and Kleinbard study did the opposite, namely, concentrated on the target material and tried repeatedly to retrieve the material. The second relevant factor is the type of stimulus involved: nonsense syllables in Ebbinghaus's case and pictures in Erdelyi and Kleinbard's successful tests. An overview of the laboratory literature by Payne (1987) suggests that there is a stark stimulus effect operating in the laboratory studies: Virtually all (96%) of the studies using pictorial stimuli produced hypermnesia, whereas fewer than half (46%) produced it with word-lists learned in the laboratory. The percentage of successful studies with nonsense syllables, moreover, is about 0% (Erdelyi 1996). This simple stimulus factor caused laboratory psychology, which was fixated on verbal and mostly meaningless materials, to doubt the existence of upward trending memory for half a century (see Erdelyi 1996). Obviously, the clinical literature invariably deals with highly meaningful materials and diverges in this respect from the artificial stimuli so standard in the older laboratory literature.

Actually, the recovery of inaccessible memories has been part of scientific psychology for more than a century (see Erdelyi 1996). The phenomenon was probed by P. B. Ballard in his 1913 monograph, "Oblivescence and Reminiscence." In most of his studies, Ballard used two recall trials, separated usually by one or two days' retention interval. In this two-trial situation, the following outcomes can (and do) occur. Some stimulus items are never recalled, that is, they are recalled neither in the first nor in the second trial. Another possible outcome is

https://doi.org/10.1017/S0140525X06009113 Published online by Cambridge University Press

that some items are recalled on the first trial but forgotten on the second. Ballard designated such forgetting as *oblivescence*. The opposite outcome, or *reminiscence*, is observed when initially unrecalled items are recovered in the second trial. Finally, initially recalled items may be recalled again, that is, an item may be recalled on the first trial and then on the second test as well.

Ballard had the foresight to carry out his work with different types of stimuli, including poetry and nonsense syllables. Regardless of the stimulus used, Ballard found that *both* oblivescence and reminiscence occurred from first to second trial, but that the balance between the two varied with the stimulus. With poetry, for example, reminiscence exceeded oblivescence, resulting in an overall "improvement" of recall (which is today referred to as *hypermnesia*); with nonsense syllables, on the other hand, oblivescence exceeded reminiscence, for an overall decrement of memory (*amnesia*). Thus, Ballard was able to obtain Ebbinghausian results – amnesic memory – when he used Ebbinghausian stimuli (nonsense syllables). When the stimuli become less artificial (e.g., poetry), the opposite, hypermnesic memory, was observed. Oblivescence may be ubiquitous, but so is reminiscence, and in this we see the paradoxical side of memory: Over time, memory simultaneously becomes better and worse, hence Ballard's title, "Oblivescence and Reminiscence." Thus, regardless of the stimulus used, there is always an *up-trend* and a *down-trend* in memory over time, a most convenient feature for a memory system involved in selectively enhancing or degrading memory.

Despite the beautiful simplicity of Ballard's discovery – that both reminiscence and oblivescence are fundamental to memory over time, and that their balance depends in part on the type of stimulus used – another grand-Bartlett effect imposed itself, this time in experimental psychology (see Erdelyi 1996). The memory literature following Ballard almost immediately began to confuse *reminiscence* with *improvement* (hypermnesia) and, attempting to replicate Ballard's improvement effect – typically with nonsense syllables – often failed in the effort. Researchers began to worry about the reliability of memory improvement, even though they were actually replicating Ballard's failure to get improvement with nonsense syllables. With the use of ineffective stimuli and the confusion of reminiscence with improvement, the field drifted into disarray. Claude Buxton, in his influential review of 1943, concluded that "reminiscence" was "a now-you-see-it-now-you-don't" phenomenon. Buxton, however, was no longer talking about reminiscence but of Ballard's "improvement" (what I call hypermnesia). In fact, reminiscence is a rock-solid phenomenon, on par with forgetting, and it is improvement, as Ballard had clearly shown, that may be chancy when the wrong stimulus is used.

Recent laboratory work (Erdelyi 1996, Ch. 8) has documented hypermnesic recall beyond a few days' retention interval, including an unexpected jump in recall level after periods of months. The multi-trial recall performance over some nine months of testing of a $9\frac{1}{2}$-year-old child, Karina, for Bartlett's story, "The War of the Ghosts" (332 words long), was probed. Repeated recall effort in the first three months (atypically) failed to produce hypermnesia but after a six-months' fallow interval (in which there was no testing), a dramatic increase in recall level (by more than 200%) was observed. False recalls, however, increased even more after the six-months interval. A Criterion-Controlled Free Recall (CCFR) measure for narrative recall (Erdelyi 1996, appendix), in which hits (true recalls) are conditionalized on a pre-set false-recall level, still showed a substantial jump in correct recall (of about 75%) after the six-months interval. Thus, with false-recall level controlled, a big jump in recall level was still observed. Later research on Karina's $6\frac{1}{2}$-year-old sister, Maya, essentially replicated Karina's long-term delayed recovery of memory, both with raw and controlled recall measures.

If we can trust such long-term but small-sample findings (though other laboratories have begun reporting hypermnesia effects over weeks, e.g., Bahrick & Hall 1991; 1993; Herrmann et al. 1987), it would seem that the recovery of memories is a standard feature of repeated recall over time, both in short-term single-laboratory probes as well as over periods of weeks and months. It is true that false recalls are also churned up over time, but one phenomenon does not invalidate the other. Both occur. Roediger et al. (1997) focus on this "paradox" of memory, emphasizing that repeated recall enhances "accurate retention while sometimes leading to increased recall of wrong information" (p. 119). Signal detection theory (e.g., Macmillan & Creelman 2004; Swets 1964) resolves this surface paradox by parlaying the functional relation between hit rates and false alarm rates into a single isosensitive function, the ROC curve. In the clinic, it is difficult, maybe impossible, to distinguish true recalls from false recalls because we don't have the original event against which to check the recalls. The laboratory, however, helps us cinch the phenomenon of recovery of (true) memories and of overall memory improvement, hypermnesia, because of the availability of the original stimulus.

Just as the laboratory has methodological advantages, so too it has its shortcomings. Especially from the standpoint of defensive repression, *stimulus insufficiency* is a problem, for we can hardly duplicate the kinds of "intolerable" or "unbearable" (Freud 1894/1962; 1896a/1962) stimuli that are the purview of real-life and the clinic. Moreover, it is clear from the clinical literature that what is "intolerable" varies vastly from person to person (just consider the two case histories discussed in Section 2: In the Case of B., the death of a brother; in the Case of N., homosexual cravings). Another refractory problem is the question of *stimulus source*. In the laboratory, the experimenter can control how many times and when the subject is exposed to the stimulus. In real life, the stressful events may be intermittently reproduced or cued. The Ebbinghaus curve would look different if intermittent re-exposures to the stimulus list occurred. Even more problematic is the reality that many of the "stimuli" that are repressed come from *within* and cannot be neatly manipulated, let alone controlled by an experimenter. In most clinical examples of defensive repression, including those of Freud, the crucial stimuli are biological irruptions of unbearable drives – possibly cued by environmental events – that require, as in the Myth of Sisyphus, a repeated renewal of effort. Thus, a substantial swath of the laboratory literature fails to sample the real-life situation and so suffers from questionable content validity.

With practice, the repressive process, like typing or driving, might be automatized and may not be particularly

https://doi.org/10.1017/S0140525X06009113 Published online by Cambridge University Press

salient to consciousness (hence the long-standing assumption that repression is unconscious). When an increase in task difficulty occurs, as in the case of some significant new threat, conscious control may be partly reinstituted (as when a sudden problem arises in a driving situation). The task, also, may prove too difficult and repression may fail or partially fail. For example, repression may break down intermittently and yield *oscillations of consciousness* for the stressful events (*DSM-IV*; Horowitz 1986). "The return of the repressed" may also be observed for systemic reasons, as when the subject is disinhibited because of depleted "cognitive resources" due to other life stressors like illness, intoxication, or old age, or because of psychological state (e.g., sleep).

5. Distortion by reconstructions and false additions

On the strength of informal laboratory work, Bartlett (1932) forced experimental psychology to accept the reconstructive (and therefore distorting) character of memory. According to Bartlett, mental "schemas," reflecting cultural habits, biases, and logical expectations, intrude upon and distort memory. One of the "distortions" Bartlett emphasized was "omission." The clinical and laboratory data show, as we have seen, that at least this Bartlettian distortion can be partially reversed (in this respect, falsifying Bartlett's radical revisionist hypothesis that no memory trace for the original events exists beyond the current overwritten material). The work of remembering diminishes "omissions" but, as just noted, falls far short of eliminating them. Yet, just as nature abhors a vacuum, so psyche abhors gaps and actively tends to fill-in these gaps in a "fundamental effort after meaning" (Bartlett 1932, p. 227). In trying to recall degraded versions of the past, the subject not only recovers some of this material, but also schematically augments the degraded accessible memories for meaning. Schematic augmentation fills in the blind-spots of memory – just as it does our blind-spots in perception. With both recovery and schematic augmentation unfolding over time, it is not surprising that both true recalls and false recalls are observed.

Now, here is the intersection between Freud and Bartlett: The constructions and reconstructions of Freud and Bartlett are the same but for motive. The concept of "schema," popular as it is, is fuzzy. Bartlett emphasized intellectual considerations, cultural habits, logical expectations, and so on. Such intellective schemas do not, however, preclude perfectly obvious *emotional schemas* (wishes, emotional attitudes, defensive needs). It is no accident that the terms are often overlapping, if not identical (see Table 1). Even on the point of motive the differences are ones of emphasis, due no doubt to differences in the stimuli. Bartlett, actually, points occasionally in the direction of defensive distortion, as when he states, "if the affect is displeasing, distortions are more likely to occur" (Bartlett 1932, p. 91). It strikes me as obvious that the different emphases of Bartlett and Freud of the kinds of schemas that structure distortions – intellective versus emotional schemas – arise from *availability heuristic biases*. The stimuli encountered in the clinic are substantially more emotional than those dealt with in the laboratory.

Table 1. *Some overlaps between Bartlettian and Freudian distortions*[a]

Bartlett (1932)	Freud (various)
omission	**omission, censorship, ellipsis**
rationalization	**rationalization**
elaboration	**secondary elaboration**
change in **emphasis**	**displacement of accent**
condensation, importations, transference	**condensation, displacement, smuggled-in** items
items are **symbolized**	**symbolization**
constructions	**constructions**
interpretation	**interpretation**
bias in recall	**tendentious** recall
imposed **links of connection**, striving after **associative links**	**false connections, compulsion to associate**
distortion	**distortion, falsification**
transformations	**transformations**
reversals ("opinions and conclusions are reversed" p. 128)	**reversal, reaction-formation**
imaging	**imaging, plastic word representation**
curious perseveration of the trivial (p. 184)	**representation by triviality**

[a]Actual terms are set in boldface.

Although distortion and bias in information-processing are standard fare in cognitive psychology (Erdelyi 1974; 1985), motive is a difficult problem for science (e.g., Wegner 2002). Still, contemporary social-personality psychology is rife with motivated biases, including *egotistical biases* (e.g., Greenwald 1980; Paulhus & John 1998), *moralistic biases* (Paulhus & John 1998), *trivialization* (e.g., Simon et al. 1995), *temporal bracketing, minimization* (Baumeister et al. 1998), *illusion of unique invulnerability* (Perloff & Fetzer 1986), *illusion of control*, *positive illusions* (Taylor & Brown 1988; Taylor 1989), and the like. Worth noting also is a trend toward schema-like formulations of motivated biases. In a laboratory evaluation of the status of defense mechanisms, Baumeister et al. (1998) conceptualize defenses as efforts by individuals to sustain "preferred views of themselves" (p. 1082). In a similar vein, McAdams (1998) focuses on how we "make sense" (p. 1127) of our identities or life stories. Defense mechanisms are "viewed as *narrative strategies* that shape how a life is told" (p. 1127), and an "affinity between stories and human life" (p. 1125) is emphasized. It no longer seems such a chancy leap from "The War of the Ghosts" to the psychopathology of everyday life.

Martin Conway (1997b) presents a modern cognitive treatment of narrative constructions and reconstructions that are implemented by a "central cognitive processor" in which activation and inhibition occur at different substrates of autobiographical memory. As Conway notes, the narrative approach to autobiographical memory is not new. For example, the psychoanalyst Ernst Kris (1956/1975) advanced the concept of the *personal myth*, which

https://doi.org/10.1017/S0140525X06009113 Published online by Cambridge University Press

individuals to a greater or lesser extent edit over time, omitting details that are emotionally inconsistent with the myth.

The one major defense mechanism which the present constructivist treatment does not obviously accommodate is *regression*, a defense involving a developmental retreat to more primitive styles of acting and thinking. Work in modern social-cognitive psychology provides a simple conduit to this defense in schema terms. The basic idea, though Bartlettian terminology is not typically used, is that the overall cognitive system is made up of multiple, often overlapping schemas (structures, systems, selves, states) that might differentially structure current inputs and that, depending on circumstances, different schemas might be deployed by the overall system (e.g., Horowitz 1988; Markus & Nurius 1986).

Dan Gilbert (1991), in an analysis of cognitive belief systems, suggests multiple levels of truth-value processing, the more primitive system (the first phase of the "Spinozan" system) being a credulous one – as in Freud's "primary process thinking," it knows no *no* and believes whatever it apprehends or imagines. The more advanced information-processing system – as in Freud's "secondary process thinking" – is a critical system that performs "a secondary psychological act" (Gilbert 1991, p. 108): It consciously and effortfully evaluates the output of the earlier credulous system and either accepts or "unaccepts" the current belief. Gilbert shows that when the overall cognitive system is stressed or cognitively overloaded, there is a tendency for the more sophisticated system to drop out and the more primitive belief-system to prevail. In effect, Gilbert is proposing a regression from a more advanced to a more primitive mode of analysis as a result of cognitive stress or depletion. In this vein, Gilbert also emphasizes the paradox of facilitation through disinhibition, a leitmotif of psychoanalysis and neuroscience (e.g., Damasio 1994; Freud 1900/1953; 1917/1961/1963; 1920/1955; Gazzaniga 1985; LeDoux 1996; Pavlov 1927; Pötzl 1917). As in psychoanalysis, when the executive system (the ego) is inhibited or undermined (by overload, sleep, intoxication, therapy, brain damage, old age), the more primitive system becomes disinhibited, and the output of the overall system is infused with the content and style of the primitive system. As both Pavlov and Freud emphasized, disinhibition yields recovery of memory – "spontaneous recovery" of extinguished responses (Pavlov 1927) or "the return of the repressed" in dreams or indirect behavior channels such as symptoms (Freud 1900/1953; 1917/1961/1963; 1920/1955).

Similar themes can be discerned in *terror management theory*, which focuses specifically on defenses against the terror of death (Pyszczynski et al. 1999; 2000). Research suggests that when our mortality is in focal consciousness, we tend to use *proximal defenses*, which basically distort the facts in question. When mortality issues are primed but are subliminal or at the "fringe" of consciousness, *distal defenses* are deployed, which, in effect, operate on schemas themselves – for example, on one's "cultural world view," which becomes exaggerated and less nuanced.

The modern experimental literature on memory has extended traditional interference manipulations that had led to an emphasis of *retroactive* and *proactive interference* in forgetting (e.g., Jenkins & Dallenbach 1924; McGeoch 1932; for overviews, see Baddeley 1976; Crowder 1976; Erdelyi 1996) and merged them with the Bartlettian constructivist theme that memories are malleable and subject to a multiplicity of distorting effects, including "memory illusions" or outright memory delusions (e.g., Ceci et al. 1996; Conway 1997a; Jacoby et al. 1989a; 1989b; Loftus 1979; Loftus & Ketcham 1994; Neisser 1982; Roediger et al. 1997; Schacter 1999).

Thus, in highly influential research by Elizabeth Loftus and her colleagues (e.g., Loftus 1979; Loftus & Palmer 1974; Loftus et al. 1978), memory distortions were experimentally induced through the use of *suggestive querying* ("post-event information"). When subjects were asked, for example, "Did another car pass the red Datsun while it stopped at the stop sign?" in a scene that actually had no stop sign, they were more likely to remember the phantom sign. Similarly, if a question were phrased alternatively, "About how fast were the cars going when they smashed/hit each other?" different speed estimates were generated as a function of the wording; also, subjects exposed to the "smashed" version of the query, were more likely to report seeing "broken glass," even though none had been present. Roediger and his colleagues (e.g., Roediger & McDermott 1995; Roediger et al. 1997) have similarly demonstrated that confabulatory memories can be created through associative *implication*. Employing a paradigm introduced by James Deese (1959), they have shown that a false memory could be induced by presenting items that semantically implied a common target that was never actually presented. For example, a list containing the items *bed*, *rest*, *awake*, *tired*, *dream*, *wake*, and *snooze* tends to yield the false remembering of the semantically implied (but never presented) item, *sleep*. As already noted, Roediger and his colleagues have also emphasized that the very act of repeated retrieval often produces the effect of increasing both true memories as well as false memories. Also, repeatedly thinking about a "non-event" (i.e., engaging in fantasy) often yields false remembering of the non-event (Ceci et al. 1996). These experimental demonstrations provide laboratory analogs of self-induced memory distortions, since, presumably, whatever the experimenter can induce in the subject, the subject can also induce in the subject.

What has been tellingly omitted from most of this literature is the extent to which Freud's conception of mental processes, including memory, both anticipated and extended this narrower constructivist tradition. The elemental point of Freud's psychology is that what is accessible to consciousness is never the true, unfettered information, but highly worked-over reconstructions (brought about, for example, by the "dream work," "defense," the "joke work," and similar transformational techniques). Thus: "Re-activated memories … never re-emerge into consciousness unchanged" (Freud 1896a/1962, p. 170); recall is an "amalgam" of fact and fiction; we "construct memories almost like works of fiction" and therefore "there is no general guarantee of the data produced by our memory" (Freud 1899/1962, p. 315); what returns in the "return of the repressed" are often "false recollections" (e.g., Freud 1901/1960, p. 6) or "substitute formations" (e.g., Freud 1915a/1957/1963, p. 111); and so on.

For this reason, though it seems not to be appreciated in the modern literature, the recovered memory movement that has so energized the scientific community (e.g., Banks & Pezdek 1994–1995; Kihlstrom 1997; Loftus & Ketcham 1994) is antithetical to Freud's psychological

https://doi.org/10.1017/S0140525X06009113 Published online by Cambridge University Press

stance that the "historical truth" (e.g., Freud 1937/1964) of recovered memories cannot (nor should) be ascertained by the therapist. The originators and perpetuators of the memory recovery movement (e.g., Bass & Davis 1994) were not psychoanalysts – often not psychologists. Their program, which fundamentally violated psychoanalytic technique, would have been rejected by Freud, who, early on, underscored the "growing recognition of the untrustworthiness of statements made by witnesses," even when testifying about themselves (Freud 1906b/1959, pp. 103–113). Although Freud had originally proposed his "infantile seduction" theory of hysteria from the problematical recollections of some of his early patients, for which he has been much criticized (e.g., Crews 1995), Freud retracted his erroneous proposal within a decade (e.g., Freud 1906a/1953). Thereafter, Freud emphasized the role of fantasy in such recollections: "If hysterical patients trace back their symptoms to traumas that are fictitious, then the new fact that emerges is precisely that they create such scenes in *phantasy*" (Freud 1914b/1958, p. 17). Such recollections tend to be, even if they are not invariably, "imaginary memories" (Freud 1906a/1953, p. 274). Ironically, if Freudian tenets had not been so muffled in modern psychology, much of the miscarriage of concept and practice might have been averted and the corrective programs by experimental psychologists, such as Beth Loftus, rendered less necessary.

6. Conclusion

Psychology has become pervasively constructivist (as neuroscience and psychoanalysis have always been). It is by now the standard view that memory is not strictly veridical and is subject to wide-ranging and ongoing distortions. The source of these distortions – omissions, elaborations – varies from situation to situation, and one type of distortion need not be incompatible with another: Sometimes the distortions come from intrusions from other lists; other times, from implicit suggestions in the querying process; other times, from long-established internal structures, which sometimes are intellectual in character (Bartlett) and other times emotional (Freud). All these distortions can be exacerbated by the subject himself who, in the process of thinking about and retrieving information, may inhibit memories or amplify errors of previous constructions in a process akin to succumbing to one's own propaganda and the creation of myth.

As scientists, we should resist such trends in our own work and should not ignore the historical facts. Progress by euphemistic relabelling or tendentious silences, though ubiquitous in the real world, is not scientific progress. It does not matter from a scientific standpoint if we labelled the process "repression," "suppression," "retrieval inhibition," "dissociation," "cognitive avoidance" – or "Mary" – though such untrammelled synonymy, without the anchor of viable theory, drifts, over time, into dissociated baronies of discourse and brings upon us a self-inflicted curse of Babel: We do not understand each other; we misunderstand each other; we impose distinctions where none are justified; we fail to make distinctions that are obligatory; we argue about the wrong issues and fail to deal with the real issues; we dissociate ideas that belong together; we suffer amnesia and consign ourselves endlessly to rediscover what we already have mastered.

The laboratory and the clinic have converged on a simple but fundamental insight: Cognition, from perception to memory, is pervasively constructive. We structure our fragmentary reality by omitting from and elaborating on our meager scraps of information. We inhibit and augment our reality by different techniques and for different reasons. We try to make sense of our reality, intellectually as well as emotionally.

ACKNOWLEDGMENTS

For helpful comments on earlier drafts of this article, I thank George Bonanno, Martin Conway, Richard Lazarus, Beth Loftus, Neil Macmillan, Laraine McDonough, Arthur Reber, Mark Solms, and Diane Zizak. Many thanks are due also to Michael Anderson and Stephen Ceci for their extensive feedback in their capacity as reviewers of earlier drafts of the submitted manuscript. Finally, my special thanks to Robert Viscusi, poet and magician of prose, for occasional guidance in the underworld of irony from ancient times to the present.

This work was supported by a Fellowship Leave Award from Brooklyn College and by PSC-CUNY grants from the Research Foundation of the City University of New York.

NOTES

1. Anna Freud might have agreed. In a telling conversation with Joseph Sandler (Sandler & A. Freud 1985), published posthumously, she anticipates one of Daniel Holender's (1986) criticisms of experimental psychology's efforts to demonstrate nonconscious priming: The prime might have been fleetingly conscious but forgotten by the time the experimenter tests for it. Here is how Anna Freud treats the conundrum in an interchange on the defense mechanism of reaction formation:

> *Anna Freud*: Heinz Hartmann would say that it can become automatic.
>
> *Joseph Sandler*: . . . I still think that there must be an awareness of the impulse to evoke the response.
>
> *Anna Freud*: Hartmann and I discussed it at the time, in 1936 and 1937. There must be a momentary awareness. (Sandler & Freud 1985, pp. 22–23)

Charles Eriksen, the great critic of subliminal perception, later proposed also the idea of "automatization" of highly practiced behaviors (Eriksen & Pierce 1968), now a common notion in cognitive psychology.

2. A foreshadowing of the present twofold organization of repression can be found in Freud's *The Interpretation of Dreams* (1900/1953): "There can be no doubt that the censoring agency, whose influence we have so far recognized in limitations and omissions in the dream-content, is also responsible for interpolations and additions to it" (Freud 1900/1953, p. 489).

Open Peer Commentary

Encouraging the nascent cognitive neuroscience of repression

Michael C. Anderson and Benjamin J. Levy

Department of Psychology, University of Oregon, Eugene, OR 97403.

mcanders@uoregon.edu **http://memorycontrol.uoregon.edu**

blevy@uoregon.edu **http://memorycontrol.uoregon.edu**

https://doi.org/10.1017/S0140525X06009113 Published online by Cambridge University Press