

on an emotion face–word matching task. Subjects had to decide as fast as possible if a word, for example *happy*, matched the expression on a face. McGivern's team found that at age 11, children's speed in this task dropped by up to 20%, recovering each year thereafter, but only returning to pre-pubescent performance levels by age 18. So, when you're next faced with a teenager explaining just how much they hate you, be patient, their prefrontal cortex is probably just undergoing a little re-structuring. *CBJ*

 Letters

No need for repression

The recent paper by B.J. Levy and M.C. Anderson on 'Inhibitory processes and the control of memory retrieval' [1] describes interesting new developments in the study of the self-regulation of memory, but the authors' reach exceeds their grasp when they suggest that the processes they have studied might 'provid[e] a mechanistic basis for the voluntary form of repression (suppression) proposed by Freud' (p. 303).

According to psychoanalytic theory, repression operates unconsciously on threatening mental contents, especially those related to primitive sexual and aggressive instincts, so that the person can avoid conscious conflict and anxiety. Moreover, the 'repressed' material must continue to affect the person's ongoing experience, thought and action implicitly, outside of awareness (Freud called this 'the return of the repressed'). Finally, repressed memories have to be recoverable (this was what psychoanalytic interpretation was all about). Consider, for example, the study by Anderson and Green [2], which also has been touted as revealing a mechanism for repression. In that study, the memories in question were pairs of innocuous words, deliberately suppressed by the subjects at the request of the experimenter. Even after 16 suppression trials, the average subject still recalled more than 70% of the targets (and note the worst recall performance depicted in Levy and Anderson's Fig. 3 is still above 65% [1]). There was no evidence presented of persisting unconscious influence of the suppressed items. And there was no evidence that the 'amnesia' could be 'reversed'. Finally, although Anderson and Green apparently did not ask this question in their study,

Psychologist wins Nobel Prize

Daniel Kahneman, the Eugene Higgins Professor of Psychology at Princeton University, has been awarded the 2002 Nobel Prize in economic sciences. His work on human decision-making challenged the traditional foundations of economics, which assume that humans are always rational and motivated by self-interest. He has previously been the recipient of the Hilgard Award for Lifetime Contribution to General Psychology, the

Warren Medal of the Society of Experimental Psychologists, and the American Psychological Association's Distinguished Contribution Award. The last member of Princeton's faculty to win the Nobel Prize in economics was John Nash, about whom the 2002 Hollywood film *A Beautiful Mind* was made. *CBJ*

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it is doubtful that any of their subjects forgot that they had participated in a laboratory experiment.

Of course, Levy and Anderson refer to 'voluntary' suppression, not unconscious repression, but even this turn of phrase assumes that traumatic memories are in fact forgotten, even if they are not repressed in the classically Freudian sense. The fact is, as Piper *et al.* document convincingly [3], the vast majority of trauma victims remember all too well what happened to them, and when we observe instances in which trauma has been forgotten, we rarely if ever need to resort to concepts such as repression, or even 'suppression', to explain what has happened. That is not to say that there are no genuine cases of functional, psychogenic amnesia; there are [4]. It is only to say that trauma and repression have little or nothing to do with them.

The repression (or suppression) of trauma appears to be a clinical myth in search of scientific support. It is unfortunate that Levy and Anderson apparently feel the need to supply it.

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Repression can (and should) be studied empirically

Reply from Anderson and Levy

In his letter regarding our article 'Inhibitory processes and the control of memory retrieval' [1], J.F. Kihlstrom disagrees that the work we report has relevance for understanding Freudian repression [2]. Although the linkage to Freudian theory was not a central point of our review article, we would like to reaffirm our statement here.

The account of Freudian theory presented in Kihlstrom's letter is not universally accepted. Not all scholars believe that Freud intended repression to be an exclusively unconscious process. Erdelyi, for example, reports a scholarly analysis of Freud's writings that dismantles this view – a view that he contends is a historical distortion of Freud's theory [3,4]. Although many in the psychoanalytic field (and all those seeking to criticize it) presume that Freud intended repression to be unconscious, Erdelyi argues that Anna Freud, not Sigmund Freud, imposed this requirement. By contrast, Erdelyi persuasively illustrates that Sigmund Freud wrote about repression in terms that sometimes allow for it to be an active, intentional process, of exactly the sort characterized in the work we reported in *Nature* [5].

The main goal of the work reported in [5], however, was not to evaluate Freudian theory, but rather to address a straightforward empirical question: when people encounter reminders to memories that they do not wish to think about, what effect does pushing the memory out of

awareness have on their later ability to recall it? People confront this situation all the time, whether one's goal is to simply avoid a mildly distracting thought, or to prevent oneself from being overwhelmed by unpleasant reminders. That people engage in such behavior is self-evident, but its consequences for episodic memory are not. What our work shows is that with time, and repeated effort, trying to keep an unwanted memory out of awareness does under some circumstances render that memory less accessible, even when people want to recall it. These findings have clear relevance to motivated forgetting.

Kihlstrom's letter also questions the potency of the suppression effect reported in the *Nature* article. Kihlstrom notes that after many suppression attempts, subjects could still recall many of the word pairs, suggesting that suppression was not very effective. This observation is misleading. Although subjects attempted to suppress items as many as 16 times, each attempt lasted only 4 seconds, with the total time spent suppressing an item barely exceeding one minute. That after such a brief interval, subjects were up to 10% worse at recalling the memories suggests a very effective process. Furthermore, the tendency reported in the article was for suppression to increase with repetitions. In naturalistic cases of memory avoidance, people are likely to persist in suppressing unwanted memories for more protracted periods than we tested in our study. Thus, there is ample reason to suspect that suppression can be effective with sustained effort.

However, it is also important to highlight what the work does not establish. We agree with Kihlstrom on several points. First, our work does not demonstrate unconscious repression. Those clinicians who believe in the instantaneous and automatic thrusting of traumatic memories into the unconscious will find little support in our work. Indeed, we suspect that most cases of motivated forgetting arise from persisting attempts to control awareness strategically. Second, our work does not yet establish whether inhibitory control processes are effective for emotionally charged memories. Undoubtedly, emotionally charged memories will be more intrusive and more difficult to suppress. Nevertheless, we suspect that emotional and neutral memories differ primarily in their degrees of intrusiveness, and that with time and persistence even emotional memories may be subject to suppression.

Some will disagree with the above conjectures. Such disagreement is reasonable, and we would like to encourage the field to pursue these questions empirically. What seems less useful is to classify repression as a myth and to discourage scientists from careful inquiry into its properties. Whether the processes reported in [5] and reviewed in [1] account for real cases of motivated forgetting is ultimately an empirical question that seems quite worthwhile to pursue.

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Frames of reference and language concepts

The claim that language affects thought – the 'Whorfian Hypothesis' – has a long history, and over the years has elicited strong support as well as fierce criticism. Gallistel [1] reviewed some recent studies that tested the Whorfian hypothesis in the spatial domain, that is, whether differences in spatial language affected non-linguistic conceptualization of space.

Languages differ in the frames of reference used for describing spatial locations. Three distinct frames of reference have been identified: intrinsic, absolute and relative [2]. Some languages only use one of these, whereas others might use a combination of two, or even all three. Pederson *et al.* conducted a series of experiments demonstrating that speakers of a relative-frame language differed in

their performance in a memory task from speakers of an absolute-frame language [3]. When asked to recall and reproduce a display of toy animals, relative-frame speakers reproduced the display consistent with a relative frame of reference, whereas absolute speakers reproduced it in an absolute frame. This result is consistent with the Whorfian hypothesis. However, in a recent study, Li and Gleitman [4] argued that there was a confound in the Pederson *et al.* study. Li and Gleitman claimed that the difference in recall between the different language groups was due to the conditions in which the groups were tested: indoors versus outdoors. They presented empirical evidence that appeared to show that the differences Pederson *et al.*, reported were due to environmental features rather than differences between the languages.

However, since Gallistel's report, Levinson *et al.* [5] have published an article that refutes Li and Gleitman's claims. First, they show that across the language groups that Pederson *et al.* tested there was no confounding of test location: speakers behaved in accordance with the frame of reference of their language, regardless of whether they were tested indoors or outdoors. This is counter to the claims of Li and Gleitman. Secondly, they show that Li and Gleitman confounded absolute and intrinsic frames of reference in the experiments that they themselves conducted. When Levinson *et al.* made the appropriate distinction between these two frames of reference, and tested Dutch speakers, they found that Dutch speakers only ever responded in a way that was consistent with the way their language codes space. No environmental manipulation made Dutch speakers change to an absolute coding of space. Again, this is counter to the evidence presented by Li and Gleitman.

Gallistel wrote that all human beings perceive space in the same way and neurally encode space in the same way; furthermore, he stated that the brain has to encode spatial relations in several different coordinate frameworks. Given these facts, he asked how Whorfian effects would be instantiated in the brain. There are two important points to note about this.

First, neither Pederson *et al.* nor Levinson *et al.* make any claims about the *perceptual* abilities of different language groups; their claims are about *conceptual* representations. The claim is that the habitual use of particular linguistic