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The Residual Protective Effects of Enactment

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Abstract

Research has demonstrated the importance of the quality of initial retrieval events (Test 1) for performance on later memory tests (Test 2). We explored whether enacting words at encoding, relative to simply reading them, provided protection against the detrimental effects of a degraded retrieval experience, through the addition of motor processing to the extant memory representation. Participants encoded a mixed list of enacted and read words, then completed Test 1, and a later Test 2. Encoding and Test 2 were always completed under full attention (FA). Critically though, Test 1 was completed either under FA, or under divided attention (DA) with a distracting task requiring semantic and phonological processing. We predicted a larger enactment effect following DA relative to FA, indicating greater preservation of enacted words from dual-task interference. In Experiment 1, we demonstrated that the enactment effect was indeed larger following DA than FA, indicating greater preservation of enacted words after dual-task interference. In Experiment 2, we showed that this effect was even more potent over longer time scales, which served as a conceptual replication. In Experiment 3, we showed that enactment provides little to no protection when the distracting task requires motor processing, and in Experiment 4, we returned to the phonological distracting task and showed that in contrast with enactment, generation at encoding *does not* afford the same protection to memory. Taken together, these finding suggest that enactment renders words relatively immune to the detrimental effects of dual-tasking during testing, through the addition of a different kind, rather than a greater degree, of processing to the memory trace at encoding.

*Keywords: M*emory; divided attention; enactment; consolidation; representation

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The Residual Protective Effects of Enactment

While much research has been dedicated to the development of strategies that improve memory encoding, recent work has also highlighted the importance of retrieval rehearsal through testing, in improving later memory performance (e.g. Whitten & Bjork, 1977; Roediger & Karpicke, 2006a, 2006b). Specifically, the experience of retrieving studied material, called retrieval practice, leads to better later retention than having the opportunity to restudy that information (Roediger & Karpicke, 2006). Such findings highlight the importance of the quality of a retrieval experience in dictating later memory success. It follows from these findings that any conditions that degrade the quality of a retrieval experience could lead to impairment in later recall. In our day-to-day experiences, we unfortunately, and frequently, find ourselves in situations in which multiple tasks are to be completed consecutively, or even concurrently with one another, and this can compromise retrieval (Fernandes & Moscovitch, 2000; Hicks & Marsh, 2000; Lozito & Mulligan, 2006). As such, retrieval is rarely a process that occurs without at least some competition from other thoughts, tasks, or processes. Given the foregoing research, which indicates that the quality of a retrieval experience is important to later memory, finding ways to protect memory from the negative outcomes associated with dual-tasking during testing is an important endeavor.

Previous work (Dudukovic et al., 2009) explored whether divided attention (DA) during an initial retrieval phase led to observably poorer memory performance on a later final test. In this work, participants studied 120 images, and their memory for these images was tested using a recognition task, completed either under conditions of full attention (FA), or DA (while simultaneously completing a pattern detection task with auditory stimuli). Participants then returned 2 days later to complete a second recognition task under FA. Results showed that there were robust differences in memory performance, indicating that despite Test 2 being completed under FA, there were lingering detrimental effects from DA during Test 1. Critically, this work was the first to provide evidence that future memory is limited by conditions of dual-task during initial retrieval. This finding is consistent with other work, showing detrimental effects of DA at retrieval when measured after-the-fact, on a final test (Guez & Naveh-Benjamin, 2013; but see Kessler, Vandermorris, Gopie, Daros, Winocur & Moscovitch, 2015). The aim of the current work was to determine whether an encoding strategy exists which would provide protection from dual-task interference during testing, of the sort outlined above.

Dual-Task Interference at Retrieval

To understand how to protect memory from dual-task interference, one must first understand how DA likely exerts its effects on memory. Previous research has shown that competition for common materials or more recently, for common processing resources between concurrently performed tasks (Wammes & Fernandes, 2015; Fernandes, Wammes, Priselac, & Moscovitch, 2016) can lead to interference with episodic retrieval of previously studied information. Fernandes and Moscovitch (2000) were the first to highlight how DA during retrieval can precipitate failures in the retrieval process. Contrary to prevailing views at the time that retrieval was automatic (Baddeley, Lewis, Eldridge & Thomson, 1984) or obligatory (Craik, Govoni, Naveh-Benjamin & Anderson, 1996), Fernandes and Moscovitch (2000) showed that if the distracting task required the same representational system (in this case, words), as was required for the target retrieval task, there was substantial interference with memory performance, relative to when the concurrent task consisted of non-overlapping materials. This finding has since been replicated many times (e.g. Barnes and Dougherty, 2007; Benjamin Clarke and Butler, 2008; Ciaramelli Ghetti, & Borsotti, 2009; Fernandes and Moscovitch, 2002; 2003; Fernandes, Pacurar, Moscovitch and Grady, 2006; Fernandes, Moscovitch, Ziegler & Grady, 2005; Wais, Martin & Gazzaley, 2012; Wais, Squire, & Wixted, 2010). Subsequent work has expanded upon this material-specific account of

interference by providing evidence that decrements in memory can occur under DA at retrieval, when the concurrent tasks overlap in terms of processing requirements (Fernandes et al., 2016; Fernandes & Guild, 2009; Fernandes, Wammes & Hsiao, 2013; Wammes & Fernandes, 2016).

The foregoing work leads to an alluring possibility: If one could employ encoding strategies that are already known to bolster memory performance under FA (e.g. production, MacLeod, Gopie, Hourihan, Neary & Ozubko, 2010; generation, Slamecka & Graf, 1978; enactment, Cohen, 1981), this extra layer of coding of the memory trace could provide a safety net for when the native processing type (phonological or semantic) is degraded, or rendered unavailable due to dual-task conditions during an initial test. For example, performing relevant actions associated with to-be-remembered information at encoding could add motor information to the representation, and thus provide another processing route through which to access the encoded information at retrieval following a dual-task condition.

The Enactment Effect and Memory

One subject performed task (SPT), resulting in what is now known as 'the enactment effect', leads to a robust and reliable improvement in memory performance. This SPT encourages participants to complete an action that is associated with to-be-remembered information. This information could be a short clause ('kick the ball'), a word ('ball'), or a visual representation of an object (an image of a ball), to which a participant is asked to execute an action they would commonly perform with that object (Engelkamp & Zimmer, 1997; Knopf, 1991; Senkfor, 2008). The enactment effect has proven to be robust, resulting in exceptionally high recognition memory, even when studying long lists (Engelkamp, Zimmer & Biegelman, 1993). Further, it has been shown that enacted items are more resistant to forgetting (Knopf & Neidhardt, 1989) and that enactment can bring online motor processing in addition to the standard phonological and semantic processing typically evoked when trying to encode verbal stimuli (e.g. Lesch & Pollatsek, 1998). As such, enactment is an ideal candidate for a SPT that could protect memory from the negative effects of DA at retrieval.

For our purposes, enactment during encoding was seen as a means of adding a distinct type of processing, or mode of representing target words, over and above simply reading the words during study. Research indicates that the latter would likely engage primarily phonological and semantic representations (Adams, 1994; Jared, Levy, & Rayner, 1999; Lee, Binder, Kim, Pollatsek, & Rayner, 1999; Lesch & Pollatsek, 1998). What we are suggesting is that utilizing a SPT could not only boost memory performance, but also provide some form of protection to the memory trace, in the face of dual-task interference or distraction during a later retrieval phase. Thus, on the basis of previous work (e.g. Dudukovic et al., 2009), it is a logical prediction that DA at retrieval should lead to poorer later memory performance as measured on Test 2. More importantly however, based on the foregoing rationale, enacted words might be *more* preserved under such conditions, relative to read words.

Since it has been suggested that dual-task interference is largely contingent on the degree of cortical overlap between regions of activation for the respective tasks (Klingberg & Roland, 1998), there is a neurological basis for our hypotheses as well. Differences in brain activity have been shown in neuroimaging studies, such that action words involving a particular body part (arm vs. leg. vs. face), when read silently, preferentially activate regions associated with veridical movement of that body part (Hauk & Pulvermuller, 2011; Hauk, Johnsrude & Pulvermuller, 2004; Pulvermuller, 2005). Further, words that are enacted, relative to those read aloud, showed differential activation in the supramarginal gyrus, which has been associated with perception of limb location in space, as well as processing gestures (Russ, Mack, Grama, Lanfermann & Knopf, 2003). In addition to the activation of regions largely associated with word processing, enacted words also engaged brain areas related to the movement performed (Masumoto et al., 2006). Accordingly, under DA conditions during retrieval, these additional regions should allow for some preservation of the memory trace when the lexical regions are otherwise occupied (by a distracting task). Indeed, Shebani and Pulvermuller (2011) showed this to be the case in a

working memory task, albeit using a distracting task completed during the retention period. They found that motor tapping tasks, completed during the retention interval between encoding and retrieval of arm-related (e.g. clap, grab) or leg-related (e.g. kick, hop) words, impaired their memorability. Specifically, this work demonstrated that hand-tapping resulted in greater reduction in memory for arm-related words than did foot-tapping, while the opposite pattern occurred for memory of leg-related words.

These findings are in line with Engelkamp and Zimmer's (1984; 1985) suggestion that the motor program is an integral part of the representation of an action word or phrase, and is largely independent from other memory subsystems. Thus they invoke multiple codes to explain the benefit enactment provides, akin to Paivio's (1971) dual-code theory, which has been used to explain picture superiority (Paivio et al., 1968). Alternatively, other researchers (Kormi-Nouri & Nilsson, 1998; Kormi-Nouri, Nyberg & Nilsson, 1994) have argued that enactment improves memory by making enacted items more self-referential, proposing a difference in degree, rather than in kind, between enacted and read items. If our data show that enacted words are more resilient to dual-task interference, our results would be consistent with the former account, as it suggests that the distracting task had interfered with just one of the representational codes, leaving the motor code of the representation intact. However, we note that we do not directly test a self-referential account in the current work.

Memory Consolidation

After memories are acquired, they undergo a period of stabilization termed memory consolidation (Dudai, 2004). The time course of this consolidation is an open question, however it is clear that during the early stages after encoding, newly acquired memories are labile and sensitive to interference and change (Nader, Schafe & LeDoux, 2000). In line with this, a distracting task experienced during initial retrieval likely disrupts consolidation of a newly acquired memory trace. It has been proposed that rather than unfolding linearly, starting with the encoding of information and followed by stabilization and storage, human memory undergoes a series of stabilizations, and is reactivated and subsequently re-stabilized each time it is retrieved, whether this retrieval is explicit or implicit (Alberini & LeDoux, 2013). It follows then, that if interference is applied at Test 1 during the first explicit reactivation of one's memory for a particular subset of items, those items' representations in memory will be degraded and forgotten more rapidly than those whose first reactivation unfolded unimpeded, and thus were solidified in memory. Similar ideas have been expressed in previous work (Guez & Naveh-Benjamin, 2013). Thus, the influence of interference experienced at Test 1 may be more observable on a later, final test, and might even be more pronounced over longer-time frames. We predict that, in our paradigm, a motor representation of a to-be-remembered word would remain intact in the face of interference from a distracting task that requires phonological/semantic processing; as such, memory for enacted words, or at least for the motor aspect of the representation, would undergo consolidation relatively unimpeded, compared to memory for words that were simply read at encoding. The resultant memory profile would show that costs to memorability, following dual-tasking on an initial retrieval test, are more pronounced for read relative to enacted words. Moreover, as outlined above, we expected that when more time is provided for consolidation, that this difference would become even more apparent.

In the current work, we tested whether enactment could lead to protection from the long-term dual-task interference that has been previously documented (Dudukovic et al., 2009; Guez & Naveh-Benjamin, 2013), following DA conditions in an initial retrieval phase. Prior to our conducting the experiments reported here, it is important to note that we tested for protective effects of enactment in a preliminary experiment that is not reported in this manuscript (See Supplementary Material, where it is reported in full). There were notable flaws in the design of that experiment making it difficult to derive any firm conclusions. However, it did provide some suggestion that the hypothesized protective effects of enactment exist. In the interest of maintaining a focused paper, we will not go into more detail about that experiment here. In Experiment 1, participants encoded a mixed-list of enacted and read words under FA, and

subsequently completed Test 1 (recognition) either under FA or DA with a distracting task requiring animacy decisions to words. Following both FA and DA, participants completed a Test 2 under FA, in which they were given 60 seconds to recall any words they could remember. We hypothesized that words that were enacted, relative to read at encoding, would be more resilient to the effects of DA during Test 1, as indexed by a greater proportion of enacted words in Test 2 output. In Experiment 2, Test 2 was delayed by 8 minutes, in order to both conceptually replicate our findings from Experiment 2, and to test our hypothesis that any potential protective effects of enactment would increase in magnitude when final recall was delayed by several minutes. In Experiment 3, we introduced a motor-based distracting task in place of the animacy task, to determine whether this would diminish any protective effects observed in the former three experiments, by interfering with the added motor representation. Lastly, in Experiment 4, we tested the idea that *any* encoding strategy would protect memory from interference, by asking participants to generate words from their definitions at encoding, a task which likely exerts its effects by augmenting semantic processing (e.g. McElroy & Slamecka, 1982; Slamecka & Graf, 1978).

Experiment 1

There were two goals for this experiment. First we wanted to determine whether enactment at encoding provided any immediate protection (on Test 1) from the detrimental effects of DA at retrieval, and second, we wanted to determine whether enactment diminishes long-term interference effects, on a later memory test (Test 2). Because previous work (e.g. Engelkamp & Zimmer, 1985 Masumoto et al., 2006) has suggested that words that are enacted at encoding are likely encoded via multiple codes or representational systems, the current study aimed to determine whether such words would be more immune to both the immediate and downstream disruptive effects of DA at retrieval. Theoretically, if access to the memory trace via one processing route or mode of representation is hampered by a concurrent task, the memory will be degraded (Fernandes et al., 2013, Wammes & Fernandes, 2015). If however, the memories could be supported by re-activation of another code, these may be better preserved, relative to words that were simply read. To elicit the formation of a memory trace which incorporates a motor representation, we asked participants to enact (i.e. produce a hand movement) half of the words in the study list, during encoding. This task was designed to add a motor level of processing to the memory trace of the studied word (as in Engelkamp & Zimmer, 1983), in addition to the existing phonological and semantic codes (Adams, 1994; Jared et al., 1999; Lee et al., 1999; Lesch & Pollatsek, 1998).

Our study phase was performed under FA, but Test 1 recognition was performed under FA or DA with a concurrent animacy task, which was designed to engage both phonological and semantic processing. A recognition paradigm was chosen (rather than recall) to ensure that the onsets of the memory and the distracting tasks were simultaneous, giving us more control over the interference manipulation. Participants then completed another memory assessment (Test 2) under FA, in which they were asked to recall the words that they had enacted or read. This design provided us with an objective measure of the downstream effects of DA, following an initial test phase. Thus our experiment was a 2 (Instruction: enacted vs read) x 2 (Attention: FA vs. DA) within-subjects design. It was predicted that for both Test 1 and Test 2, there would be a main effect of Instruction type, such that memory for enacted words would be superior to those that were read, and a main effect of Attention, such that memory performance would be better under FA than DA. Critically though, we predicted that words that were enacted, relative to read at encoding, would be more resilient to the effects of Test 1 DA, as indexed by a greater proportion of enacted words in Test 2 output following DA, relative to following FA. These predictions follow from previous work (Dudukovic et al., 2009) showing that detrimental effects of DA at retrieval are observable on a later final test, and research proposing that the effects of DA at retrieval are likely best observed *following* the initial test (Guez & Naveh-Benjamin, 2013). To clarify, based on previous work (Fernandes et al., 2016; Fernandes & Guild, 2009; Fernandes et al., 2013) there should be substantial interference with the consolidation of

phonological/semantic aspects of both read and enacted words. Enacted words on the other hand, would have an extra motor code to rely upon, which should be relatively immune to this particular type of interference applied at initial retrieval. Accordingly, in contrast with read words, the consolidation of enacted words should carry on (relatively) uninhibited. This difference, due to the time course of consolidation (Dudai, 2004) would be more easily observable on Test 2, after some time has passed.

Method

Participants. In all experiments, we aimed to collect data from at least 24 participants and to stop at 30. Because we added multiple timeslots per day and did not turn enrolled participants away, there was some variation within this range. Also, because of technical difficulties with our microphone setup and voice onset latency data collection, some participants' Test 1 data were not properly recorded and thus were unusable. This problem was especially prevalent in Experiments 1 and 2. In this experiment, 23 undergraduate students (12 female) from the University of Waterloo successfully participated either for course credit, or monetary remuneration. Their ages ranged from 18 to 23 (M = 19.74, SD = 1.80). All had normal or corrected-to-normal vision and had learned English before the age of 7.

Materials.

Memory task. 100 hand- and arm- related verbs (e.g. throw, chop) were generated, and pilot participants were asked to perform the action dictated by each. Pilot testing indicated that 40 of these verbs lead to errors or omissions in one or more subjects, leaving 60 verbs that were successfully enacted by every pilot participant. These comprised the list of words for the current memory task. Words ranged in frequency from 0 to 401 (M = 46.82, SD = 78.38) based on the Frequency Analysis of English Usage (Francis & Kucera, 1982), varied in length from 3 to 7 letters (M = 4.60, SD = 1.00), and had either one or two syllables (M = 1.12, SD = .32). Each participant viewed their own independent randomization of words and associated trial types.

Animacy one-back task. Nouns were selected for use in the distracting task such that they had medium frequency, arousal and valence ratings. 30 nouns that represented living things were selected (e.g., flower, rabbit), while 30 represented non-living things (e.g., diamond, clock). These were matched as closely as possible on frequency, length and number of syllables, as well as on ratings of valence, arousal and dominance. A female speaker was recorded clearly speaking each of these words. The audio recordings were edited such that each file was exactly 1500 ms.

Procedure. For clarity, we will first describe the two experimental tasks that were used. These are the Memory task, which includes Study, Test 1 and Test 2) and the Animacy task. Then, in the 'Experimental Phase' section, we describe how these are integrated into the flow of the experiment.

Memory task.

Study. During encoding, participants were instructed to enact (act out a movement associated with the word) or read (simply read the verb silently) each word, depending on the font color (purple or orange) in which the word was presented. The color corresponding to each encoding instruction was counterbalanced across participants. Participants studied 16 words (8 enacted and 8 read) one at a time, in random order. For each trial, a fixation cross was centrally presented for 500 ms, followed by one of the target words, centrally presented for 4500 ms, during which the participant was to either enact or read the item.

Test 1 (Recognition). Participants completed a recognition task. They were shown 30 centrally presented words, one at a time (16 'targets' and 14 'lures'). Each trial began with a 500 ms fixation, followed by the word for 1500 ms. Participants were asked to indicate whether they had studied the word by saying 'yes' or 'no' aloud. Their verbal response and response time was recorded using a Serial Response Box with voice onset relay.

Test 2 (Recall). Participants were given 60 seconds to type in as many words as they could recall from the encoding phase.

Animacy task. Nouns were presented auditorily, one at a time, through a set of headphones. A word was presented every 2000 ms. Participants were given a small response pad, and asked to press the right or left buttons with the corresponding hand to indicate whether each word was a living thing or not. The hand they were to use to indicate each of the two possible

responses was counterbalanced. Two lists were pseudo-randomly created, such that in each case there were 30 words in order to match the time required to complete all recognition trials. There were 15 trials which required a 'living' response, and 15 trials which required a 'not living' response.

Experimental phase. After a brief practice phase designed to familiarize participants with the aforementioned tasks, the experiment was completed. Participants completed two full memory conditions (i.e. FA and DA, each including Study, Test 1, Test 2) in counterbalanced order. The baseline of the animacy distracting task was performed alone either before or after these memory cycles. To clarify, there were four possible counterbalanced sequences through which a participant could progress:

1. Instructions → Practice → Animacy → FA (Study, Test 1, Test 2) → DA (Study, Test 1, Test 2)
2. Instructions → Practice → Animacy → DA(Study, Test 1, Test 2) → FA (Study, Test 1, Test 2)
3. Instructions → Practice → FA(Study, Test 1, Test 2) → DA (Study, Test 1, Test 2) → Animacy
4. Instructions → Practice → DA(Study, Test 1, Test 2) → FA (Study, Test 1, Test 2) → Animacy

For the FA condition, Study, Test 1 and Test 2 were *all* completed under FA. In the DA condition, Study and Test 2 were similarly completed under FA; Critically however, recognition on Test 1 was completed under DA (while simultaneously completing the animacy task). Participants were instructed to divide their attention equally between the two concurrent tasks when Test 1 was completed under DA.. Immediately following Test 1 in each condition, participants completed free recall (Test 2) for only those words that were studied within that condition, and this was *always* completed under FA. It was made clear to participants in the instructions that there would be absolutely no carryover of words from one condition to the next.

Results

Overall notes. In all subsequent experiments and analyses, participants whose accuracy (hit rate – false alarm rate) under FA conditions fell below a threshold of 0.2, indicating near-chance performance, were omitted from *all* analyses. Also, we note that the relatively small number of studied items per cell (only 8 enacted words under FA for instance), precludes any strong conclusions being drawn from recognition data. Given our preliminary exploration, our only interest in Test 1 was as a means through which to implement our dual-task manipulation, such that we could observe the effects on Test 2. Had we been interested in recognition data, more study items would have been required. However, such a list length was suitable for producing the range of performance we were targeting in later free recall – the main focus of our analysis and conclusions.

Typical statistical analyses for raw differences between conditions are ambiguous in this circumstance, given there are differing baselines for FA relative to DA conditions. To clarify, the effects of SPTs such as enactment are often measured as difference scores (MacLeod et al., 2012) between the two conditions (e.g., memory for enacted – memory for read). Since memory performance was predicted to be, and usually is, lower in DA relative to FA, the raw difference scores between enacted and read words recalled in DA would necessarily be smaller than this difference score in FA. This comparison however, is uninteresting for the purposes of the current work, beyond showing the typical detrimental effects of DA. Accordingly, the computation of a proportional measure of the enactment effect was required (enacted words recalled / total words recalled). To clarify, if a hypothetical participant recalled only 5 enacted words, and 1 read in the DA condition, their 'enactment effect' as calculated by a traditional difference score (between enacted and read words recalled), would be statistically equivalent to their recall of 15 enacted words and 11 read in the FA condition. This is problematic, as the hypothetical participant clearly benefitted more from the enactment instruction in the DA condition, but this critical difference is obscured by the overall lower performance in DA relative to FA. For this reason, all results are also analyzed using a more appropriate and descriptive proportional measure of the enactment effect. To be thorough, we have still included the raw values in Tables 1 through 4, and analyzed these data as well in all results sections. However, we note that analogous computations (i.e. percentage interference) are routinely used to counter similar measurement

issues and different baselines in the dual-task literature (e.g. Anderson, Craik, & Naveh-Benjamin, 1998; Fernandes & Moscovitch 2000; 2002; Kane & Engle, 2000; Logie, Cocchini, Della Sala & Baddeley, 2004). The critical analysis then, was a paired samples t-test with Attention condition as a within subjects factor, and the proportional enactment effect as a dependent variable.

When calculating the proportional enactment (or generation in Experiment 5) effect and interference scores, participants occasionally had missing values from their data. This occurred in any instance where the denominator of the calculation in question was zero. In response to this, we used the average of 100 multiple imputations to replace missing data with a probable value (based on existing data) (Rubin, 1987). This technique is often used to deal with situations wherein a participant fails to respond to one of several questionnaires or items on a questionnaire. Statistical analysis software typically deals with this scenario by removing that participant from the analysis entirely (i.e. list-wise deletion), skewing the data in potentially misleading ways and introducing bias (King, Honaker, Joseph, & Scheve, 1998; Wothke, 2000). Multiple imputation is one commonly used and effective solution to this problem. In the body of the manuscript we report the analyses including the imputed values. However, to be thorough, we also analyzed the data using list-wise deletion (reported in footnotes at the end of each method section). To anticipate, the results from these analyses were found to be completely concordant with those using imputed values, indicating that our findings were reliable and robust to different methods of dealing with missing data.

Test 1.

Memory performance. Accuracy **(**Hit rate – false alarm rate) was analyzed using a repeated measures ANOVA, with Attention (FA, DA) and Instruction (Enact, Read) as within-subjects variables. There was a significant main effect of Attention, F (1, 22) = 143.19, MSE = 0.03, $p < .001$, η_p^2 = .87, such that accuracy was higher in the FA than DA conditions. There was also a significant main effect of Instruction, F (1, 22) = 23.06, MSE = 0.03, $p < .001$, η_p^2 = .51, such that enacted words were better remembered than read words. The Attention X Instruction interaction was not significant, F (1, 22) = 1.04, MSE = .02, $p > .05$, η_p^2 = .05 (See Table 1).

Response time. Mean RTs for correct recognition decisions were analyzed, and compared in the same way as Test 1 memory performance. Results indicated that participants were slower to make correct recognition decisions in the DA relative to FA condition, F (1, 22) = 192.17, MSE = 33566.99, $p < .001$, η_p^2 = .90, but no other effects reached significance (see Table 1 for means).

Proportion enactment effect. The enactment effect was computed as the proportion of hits that were enacted during encoding. The enactment effect for both FA and DA was compared using a paired samples t-test. Results indicated that the enactment effect on Test 1 was not significantly different across the FA and DA conditions, t (22) = .22, SE = 0.03, d = .05, $p > .05$, *ns*.

Test 2.

Recall. The dependent variable of number of words recalled on Test 2 was analyzed using repeated measures ANOVA, with Attention (FA, DA) and Instruction (Read, Enact) as within-participants factors. There was a significant main effect of Attention, F (1, 22) = 9.13, MSE = 1.63, $p < .01$, η_p^2 = .29, such that recall was higher under FA than DA conditions. There was also a significant main effect of Instruction, F (1, 22) = 75.54, MSE = 2.04, $p < .001$, η_p^2 = .77, such that enacted words were better recalled than read words. The Attention X Instruction interaction was not significant, F (1, 22) = 1.24, MSE = 1.07, $p > .05$, η_p^2 = .06 (See Table 1).

Proportion enactment effect. The proportional enactment effects on Test 2, following FA and DA on Test 1, were compared using a one-tailed paired samples t-test. Results indicated that the enactment effect was significantly larger following DA than FA, t (22) = -1.81, SE = 0.04, d = .38, $p < .05$ (see Figure 1).[1]

Dual-task interference. Results were also analyzed in terms of dual-task interference, relative to FA conditions. To achieve this, a proportional interference score ((FA-DA)/FA) was calculated for each participant for both enacted and read words. A one-tailed paired samples t-test indicated substantially higher dual-task interference for read words (M = .46,

[1] There were no missing data for the proportional enactment effect. When list-wise deletion was used for dual-task interference (n = 2), the pattern was the same, t (20) = -2.59, SE = 0.13, d = .59, $p < .05$.

Table 1: Number of Hits for Read and Enacted Words, Number of False Alarms, and the Proportion Enactment Effect under Conditions of Full (FA) and Divided Attention (DA) on Test 1 (Initial Recognition) in Experiment 1. Also shown are the Number of Read and Enacted Words Recalled, as well as Proportional Enactment Effect on Test 2 (Final Recall) in Experiment 1. Standard Errors of the Mean are in Parentheses.

Measure and trial type	Test 1		Test 2	
	FA	DA	After FA	After DA
Hits				
Read	6.09 (.32)	4.22 (.35)	2.61 (.31)	1.57 (.28)
Enact	7.70 (.15)	5.39 (.30)	4.96 (.23)	4.39 (.33)
False Alarms	0.65 (.17)	3.22 (.44)	-	-
Enactment Effect	0.56 (.01)	0.57 (.02)	0.68 (.03)	0.76 (.04)
Accuracy			-	-
Read	0.71 (.04)	0.30 (.04)	-	-
Enact	0.92 (.02)	0.44 (.05)	-	-
Response Time				
Read	934.06 (29.60)	1395.79 (46.13)	-	-
Enact	874.38 (23.28)	1471.81 (44.01)	-	-

SE = .11) relative to enacted words (*M* = .11, *SE* = .07), *t* (22) = -3.04, *SE* = 0.12, *d* = .67, *p* < .01 (See Figure 2).

Animacy task.

Proportion correct. Distracting task performance was measured as the proportion of the 30 items that were responded to correctly. A paired sample t-test indicated that participants scored lower in the DA (*M* = .39, *SE* = 0.03) than Baseline condition (*M* = .93, *SE* = 0.02), *t* (22) = 19.87, *SE* = .03, *d* = 4.60, *p* < .001. The decrease in performance in the DA condition was largely driven by omissions, indicating that participants were likely preferentially attending to the primary memory task.[2]

Response time. Mean RTs for correct classification decisions were tabulated. A paired samples t-test indicated that RTs to make correct responses during DA (*M* = 1256.50, *SE* = 38.18) were not significantly different than during Baseline (*M* = 1190.97, *SE* = 18.85), *t* (22) = 1.51, *SE* = 43.27, *d* = .33, *p* > .05.

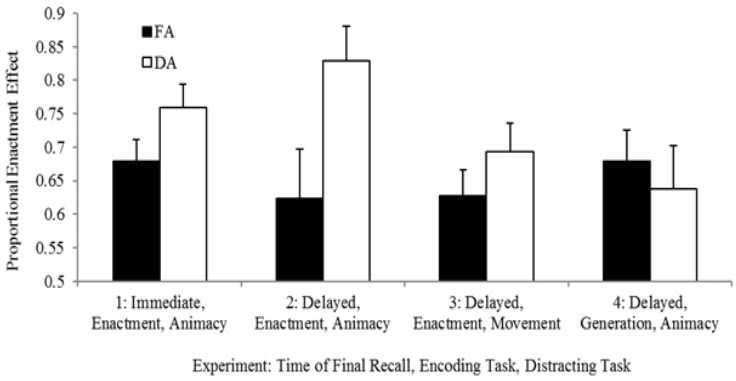


Figure 1. Mean proportion of enacted (or generated) words remembered (enacted words/total words) on Test 2, following Test 1 completed under either full attention (FA) or divided attention (DA) conditions. Means are displayed for when Test 2 was administered almost immediately after Test 1 (Exp. 1), when it was completed following an 8-minute delay from Test 1 (Exp. 2), when DA on Test 1 was concurrent with a motor distracting task (Exp. 3), and when generation was completed at encoding, rather than enactment (Exp. 4). Error bars represent the standard error of the mean.

Discussion

Performance on the initial recognition test (Test 1), and final recall (Test 2) was consistent with previous work showing better memory for words that were enacted than read (Engelkamp & Zimmer, 1997). Results also indicated a substantial main effect of DA at retrieval, which adds to the growing body of

[2] Importantly, there were a significantly higher proportion of omissions when the distracting task occurred concurrently with read items (*M* = 0.53, *SD* =0.24), relative to enacted items (*M* = 0.41, *SD* =0.19), *t* (22) = 2.90, *SE* = 0.04, *d* = .58, *p* < .01. This indicates that if anything, participants were more attentive to the memory task when read items were presented than enacted.

evidence in support of both material- (Fernandes & Moscovitch, 2000) and processing-specific dual-task interference (Fernandes & Guild, 2009; Fernandes et al., 2013; Wammes & Fernandes, 2016). That is, our distracting task that involved word stimuli, and required semantic processing, led to substantial interference with memory for a list of words.

The critical finding in this experiment comes not from Test 1 but from Test 2. Importantly, and in line with our main hypothesis, we showed that not only was the proportional enactment effect significantly larger on Test 2, following DA than FA on Test 1, but that words that were enacted at encoding suffered drastically less dual-task interference than did words that were read at encoding. These findings from the final recall test provide compelling evidence to indicate that there is a large observable benefit to enacting words at encoding. The benefit is observable particularly following an initial retrieval phase under non-optimal, DA conditions. These findings suggest that even when an initial test phase is undermined by distraction, enacted words are relatively immune to these effects.

Experiment 2

Experiment 1 provided support for our claim that enacted words are better protected from dual-task interference than their read counterparts. These effects however, were not immediately observable and seemed to take time to develop. In other words, the detriment that read words suffered relative to enacted words was not evident on Test 1, but only on Test 2, when the residual outcomes of earlier DA, relative to FA, were assessed in a final recall test. With this in mind, the goals of Experiment 2 were twofold. First we wanted to determine whether the effect observed in Experiment 1 was reliable in a near-direct replication, and second to determine whether the effect remained when the final recall test was delayed by several minutes. Thus, the paradigm for Experiment 2 was identical to Experiment 1, except that final recall was delayed by 8 minutes rather than immediate. Given that items that were enacted at encoding are resistant to forgetting on a delayed recall test (Knopf & Neidhart, 1989), it is clear that the act of enactment can exert more pronounced effects across a longer time-course than typical immediate testing. Dudukovic et al. (2009) showed that DA at retrieval also exerted its effects following a delay, which in their case lasted 2 days, a finding that was corroborated by other researchers (Guez & Naveh-Benjamin, 2013). Accordingly, we hypothesized as in previous experiment, that the proportion enactment effect on Test 2 might be even larger following DA than FA, when Test 2 was delayed, allowing for a longer period of consolidation.

Consolidation is the term used to describe the period of time, following encoding, during which new memories are not yet completely stabilized, and are sensitive to change or disruption (Dudai, 2004). The idea that newly encoded memories are still labile after encoding was initially proposed over a century ago by Muller and Pilzecker (1900), based on data showing that memory improved in the first few minutes following encoding, but that memory was also sensitive to retroactive inhibition from intervening stimuli during this time. The motivation for Experiment 2 was to take advantage of the fragile state of memory in the stages just after encoding, to determine whether interrupting consolidation early-on would also stunt the stabilization process of newly formed memories. We predicted that because memory normally benefits from consolidation which occurs in the minutes following encoding (Dudai and Morris, 2000), that the deleterious effects of dual-tasking would prevent this consolidation and stabilization for words that were read, but less so for those words that were enacted during encoding. As such, enacted words would have an additional form of representation that could successfully undergo consolidation. To operationalize this, we expected to again observe a larger enactment effect after DA than FA, and that the effect size might be larger in magnitude than that observed in Experiment 1. We expected this difference to be driven by massive interference with read words, while enacted words would remain relatively unaffected.

Method

Participants. 25 undergraduate students (14 female) from the University of Waterloo participated in this experiment either for course credit, or monetary remuneration. Their ages ranged from 18 to 25 (M = 20.71, SD = 1.67). All had normal or

Table 2: Number of Hits for Read and Enacted Words, Number of False Alarms, and the Proportion Enactment Effect under Conditions of Full (FA) and Divided Attention (DA) on Test 1 (Initial Recognition) in Experiment 2. Also shown are the Number of Read and Enacted Words Recalled, as well as Proportional Enactment Effect on Test 2 (Final Recall) in Experiment 2. Standard Errors of the Mean are in Parentheses.

Measure and trial type	Test 1		Test 2	
	FA	DA	After FA	After DA
Hits				
Read	6.09 (.34)	3.35 (.36)	1.13 (.20)	0.52 (.17)
Enact	7.52 (.15)	4.09 (.47)	2.26 (.36)	2.57 (.33)
False Alarms	0.83 (.26)	2.30 (.45)	-	-
Enactment Effect	0.56 (.01)	0.53 (.04)	0.62 (.10)	0.83 (.05)
Accuracy			-	-
Read	0.70 (.05)	0.25 (.05)	-	-
Enact	0.88 (.03)	0.35 (.07)	-	-
Response Time				
Read	860.32 (21.91)	1400.30 (80.31)	-	-
Enact	849.42 (21.65)	1405.14 (90.60)	-	-

corrected-to-normal vision and had learned English before the age of 7.

Materials. Same as Experiment 1

Procedure.

Study, Test 1 (Recognition), and Test 2 (Recall). The only difference from Experiment 1 was that a delay was introduced between recognition and recall. During the delay, participants moved to a different computer in the same room, and completed a filler task for 8 minutes (a video game similar to PacMan), then returned to the original computer to complete the recall phase. The animacy task and paradigm were otherwise unchanged from Experiment 1.

Results

Test 1.

Memory performance. Accuracy was analyzed as in Experiment 1. Two participants were excluded from all further analyses because their performance was less than 0.2 under FA (Accuracies of 0, -.17). There was a significant main effect of Attention, F (1, 22) = 76.67, MSE = 0.07, $p < .001$, $\eta_p^2 = .78$, such that accuracy was higher under FA than DA conditions. There was also a significant main effect of Instruction, F (1, 22) = 20.39, MSE = .02, $p < .001$, $\eta_p^2 = .48$, such that enacted words were better identified than read words. The Attention X Instruction interaction was not significant, F (1, 22) = 1.87, MSE = 0.02, $p = .19$, $\eta_p^2 = .08$ (See Table 2).

Response time. RTs were analyzed as in previous experiments. Results indicated that hits were slower in the DA relative to FA condition, F (1, 21) = 57.26, MSE = 115306.96, $p < .001$, $\eta_p^2 = .73$, but no other effects reached significance, ps > .86 (see Table 2).

Proportion enactment effect. The enactment effect was computed as the proportion of hits that were enacted during encoding. The enactment effect for both FA and DA was compared using a paired samples t-test. Results indicated that the enactment effect was not significantly different across the FA and DA conditions, t (22) = .62, SE = 0.04, $p = .54$, d = 0.14, *ns*.

Test 2.

Recall. Recall was analyzed as in Experiment 1. There was a significant main effect of Instruction, F (1, 22) = 25.84, MSE = 2.24, $p < .001$, $\eta_p^2 = .54$, such that enacted words were better recalled than read words. All other effects were not significant, ps > .10.

Proportion enactment effect. The enactment effect on Test 2, following FA and DA on Test 1, was compared using a one-tailed paired samples t-test. Results indicated that the enactment effect was significantly larger following DA than FA, t (22) = -2.26, SE = 0.09, d = .48, $p < .05$ (see Figure 1).

Dual-task interference. A proportional interference score ((FA-DA)/FA) was calculated for each participant for both enacted and read words. A one tailed paired samples t-test indicated substantially greater dual-task interference for words that were read (M = .63, SE = .10) relative to enacted during encoding (M = -.46, SE = .26), t (22) = 4.29, SE = 0.25, d = 1.02, p < .001 (See Figure 2).[3]

Animacy task.

Proportion correct. Distracting task performance was measured as the proportion of the 30 items that were responded to correctly. Paired samples t-tests indicated that participants scored lower in the DA (M = .31, SE = 0.05) than Baseline condition (M = .58, SE = 0.05), t (22) = 6.49, SE = .04, d = 1.62, p < .001. As in Experiment 1, this decrease in score during DA was driven largely by a high number of omissions.

Response time. Mean RTs for correct classification decisions were tabulated. Paired samples t-tests indicated RTs to make animacy decisions in the Baseline condition (M = 1049.27, SE = 59.63) were significantly faster than in the DA condition (M = 1268.81, SE = 53.00), t (22) = 2.46, SE = 89.29, d = 0.53, p < .05.

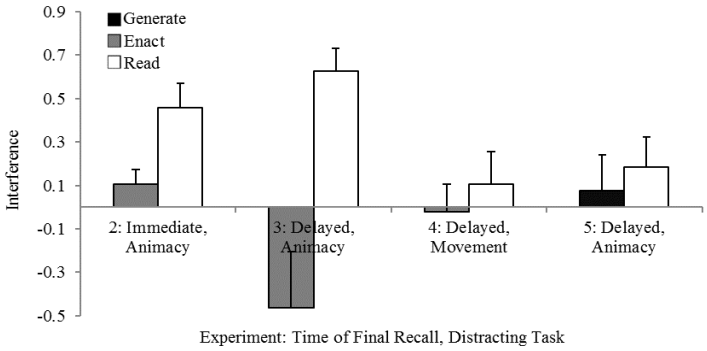


Figure 2. Mean memory interference with enacted (or generated) and read words ((FA-DA)/FA) on Test 2 following Test 1 completed under either full attention (FA) or divided attention (DA) conditions. Means are displayed for when Test 2 was administered almost immediately after Test 1 (Exp. 1), when Test 2 was completed following an 8-minute delay from Test 1 (Exp. 2), when DA on Test 1 was concurrent with a motor distracting task (Exp. 3), and when generation was completed at encoding, rather than enactment (Exp. 4). Error bars represent the standard error of the mean.

[3] For the proportional enactment effect in Test 1**,** the pattern was the same when using list-wise deletion (n = 1), t (21) = .66, SE = 0.04, d = .21, p = .52. For the enactment effect in Test 2 (n = 2), the pattern of results was the same, t (20) = -2.22, SE = 0.10, d = .49, p < .05. Lastly, for interference scores (n = 11), the pattern of results was the same, t (11) = 2.77, SE = 0.29, d = 0.91, p < .05.

Discussion

Results replicated those from Experiment 1, indicating that there is a protective effect of enactment on memory for words, on a delayed final recall test. As in Experiment 1, the enactment effect on recall output was larger on Test 2 when Test 1 was completed under distracting conditions (DA) than when it was not (FA). Further, the magnitude of interference on Test 2 was substantially larger for read words, relative to enacted words, in which there was no evidence of interference whatsoever. In fact, it appears as though DA actually led to an increase in recall of enacted words. This is perhaps because the phonological and semantic aspects of the memory trace were degraded so thoroughly that participants necessarily relied almost exclusively on motor information, thereby increasing the likelihood that enacted words would be recalled.

The analysis of dual-task interference should be interpreted with caution, as low levels of recall led to some missing cells in a subset of participants' data. To clarify, if a participant did not recall any words after the FA condition, the interference calculation was not possible, as the denominator was zero. Importantly however, analysis of data from participants who did not have missing data revealed larger interference for read relative to enacted words, and data including imputed values indicated that our finding, of dual-task interference being substantially greater for read relative to enacted words, was reliable. Together, these analyses provide compelling evidence to support our claim that words that were enacted, relative to words that were read, during encoding were better protected from the negative effects of DA at retrieval.

Experiment 3

The foregoing findings provide reliable evidence that enactment of studied words can prevent the detrimental effects of maladaptive testing environments, in the form of dual-tasking during initial retrieval. Not only do participants rely more on enacted words in their final output when the initial test was completed under DA, but this preferential reliance on enacted words was driven by almost complete preservation of memory for words that were enacted, coupled with massive interference for words

that were read, at encoding. We propose that this may be the case because enacting words at encoding introduces a salient motor processing component to an otherwise semantic and phonological memory trace. This additional component then remains when interference during testing prevents consolidation of, degrades the quality of, or reduces access to, the semantic and phonological representations. Thus, participants can use the additional motor-based component of the memory trace as something 'extra' to draw upon when their memory is probed during a final test. On the other hand, it is possible that the beneficial effects of enactment are brought about simply by a broader strengthening of the memory trace. That is, the benefit to enacted words relative to read words might be driven by a difference in degree, but not kind, of processing.

We sought to test our claim by changing the distracting task from the semantically-driven animacy distracting task, to a motor-based distracting task. If the observed protective effects were driven simply by a stronger or more distinctive memory trace, rather than an added motor component, one would expect the protective effect here to be the same as in Experiments 1 and 2. Alternatively, if enactment is adding a different *kind* of processing code at encoding, then this subtle change in methodology should blunt the protective effects observed in Experiments 1 and 2. We predict the latter, which would be reflected in a non-existent or smaller (in either case, non-significant) difference in the size of the enactment effect across attention conditions. Furthermore, this would be reflected in the interference data, as we should observe increased interference for words that were enacted, and a corresponding decrease in interference for words that were read, at encoding.

Method

Participants. 28 undergraduate students (18 female) from the University of Waterloo participated in this experiment for course credit. Their ages ranged from 18 to 23 (M = 19.86, SD = 1.74). All had normal or corrected-to-normal vision and had learned English before the age of 7.

Materials. All materials were identical to those used in Experiment 2, except for the distracting task materials. Because the task was changed from an animacy decision to a movement task, a different word list was required.

Movement task. A list of 68 nouns was created, such that each could be associated with a particular action (e.g. baseball, flute). These lists were randomly divided into two 30 item experimental lists, and one 8 item practice list. Each 30-item list served as either the items for the baseline movement task, or for the distracting task during DA conditions.

Animacy one-back task.

Procedure.

Study, Test 1 (Recognition), and Test 2 (Recall). The only difference from Experiment 2 was that the Movement task replaced the Animacy task.

Movement task. Nouns were presented auditorily, one at a time, through a set of headphones. A word was presented every 2000 ms. For each word, participants were asked to make a movement commonly associated with that word. They were told, for example, that if they heard the word "baseball" they were to simulate a throwing motion.

Results

Test 1.

Memory performance. RTs were analyzed as in previous experiments. Results indicated that hits were slower in the DA relative to FA condition, F (1, 26) = 158.63, MSE = 29738.19, p < .001, η_p^2 = .86, but no other effects reached significance, ps > .29 (see Table 3).

Response time. RTs were analyzed as in previous experiments. Results indicated that hits were slower in the DA relative to FA condition, F (1, 26) = 158.63, MSE = 29738.19, p < .001, η_p^2 = .86, but no other effects reached significance, ps > .29 (see Table 3).

Proportion enactment effect. The enactment effect was computed as the proportion of hits that were enacted during encoding. The enactment effect for both FA and DA was compared using a one-tailed paired samples t-test. Results indicated that the enactment effect was significantly larger in the DA than during the FA condition, t (27) = 2.48, SE = 0.01, d = 0.51, p < .05.

Test 2.

Recall. Recall was analyzed as in previous experiments. There was a significant main effect of Instruction, F (1, 27) = 21.78, MSE = 2.25, p < .001,

Table 3: Number of Hits for Read and Enacted Words, Number of False Alarms, and the Proportion Enactment Effect under Conditions of Full (FA) and Divided Attention (DA) on Test 1 (Initial Recognition) in Experiment 3. Also shown are the Number of Read and Enacted Words Recalled, as well as Proportional Enactment Effect on Test 2 (Final Recall) in Experiment 3. Standard Errors of the Mean are in Parentheses.

Measure and trial type	Test 1		Test 2	
	FA	DA	After FA	After DA
Hits				
Read	6.64 (.23)	4.75 (.31)	2.11 (.25)	1.54 (.25)
Enact	7.75 (.11)	6.21 (.19)	3.32 (.27)	2.96 (.33)
False Alarms	0.57 (.17)	1.54 (.29)	-	-
Enactment Effect	0.54 (.05)	0.58 (.08)	0.63 (.04)	0.69 (.04)
Accuracy				
Read	0.79 (.04)	0.48 (.04)		
Enact	0.93 (.02)	0.67 (.03)		
Response Time				
Read	859.77 (27.26)	1252.55 (30.05)	-	-
Enact	824.05 (19.94)	1252.20 (39.26)	-	-

η_p^2 = .45, such that enacted words were better recalled than read words. There was also a significant main effect of Attention, F (1, 27) = 4.35, MSE = 1.39, p < .05, η_p^2 = .14, such that participants recalled more words after FA than DA. The interaction was not significant, F (1, 27) = 0.20, MSE = 1.60, p = .66, η_p^2 = .01.

Proportion enactment effect. The enactment effect on Test 2, following FA and DA on Test 1, was compared using a one-tailed paired samples t-test. Results indicated that the enactment effect following DA did not differ significantly from that observed following FA, t (27) = 1.24, SE = 0.05, d = 0.24, p = .11 (see Figure 1).

Dual-task interference. A proportional interference score ((FA-DA)/FA) was calculated for each participant for both enacted and read words. A one tailed paired samples t-test indicated that dual-task interference with words that were read (M = .11, SE = .15) was not significantly different from those that were enacted during encoding (M = -.02, SE = .13), t (27) = 0.67, SE = 0.19, d = .13, p = .25 (See Figure 2).[4]

Discussion

Results indicated that when the semantic/phonological-based distracting task used in Experiments 1 and 2 was eschewed in favor of a distracting task requiring motor processing to initiate movements, the benefit afforded by enactment was substantially reduced and non-significant (from d = .48 to d = .24). Contrary to the findings from Experiments 1 and 2, and consistent with our predictions, the enactment effect was *not* significantly larger following DA than FA, and there was no difference in memory interference depending on whether words were read or enacted during encoding. Thus, there was no evidence to indicate protective effects of enactment[5]. Consistent with the foregoing, the size of the proportional enactment effect after DA was significantly smaller in Experiment 3 (motor distraction), relative to Experiment 2 (semantic distraction), t(49) = 2.03, SE = .07, d = .57, p < .05. These findings are consistent with our claim that enactment protects memory from dual-task interference via the addition of motor processing to

[4] Again, the pattern of the results for proportional enactment effect was the same if list-wise deletion (n = 2) was used, t (25) = 1.16, SE = 0.06, d = .30, p = .13. Interference results were also unchanged if list-wise deletion (n = 3) was used, t (24) = .77, SE = 0.20, d = .21, p = .23.

[5] A Bayes Factor (BF) was calculated for the difference between proportional enactment effects after FA and DA, using an online BF calculator (http://pcl.missouri.edu/bf-one-sample). Given that this was not a direct replication of previous experiments, we used the default JZS prior (.707). The BF_{null} was 2.50.

the existing memory trace of the target word. Accordingly, when the distracting task was designed to interfere with this motoric representation of the target word, there was no evidence of a protective effect from enactment during encoding. In essence, we had provided participants with a beneficial motor code during encoding, then taken it away using a distracting task completed during Test 1.

Moreover, this finding is consistent with the proposition that interference with memory retrieval often depends on the degree to which the primary and distracting task overlap in their processing demands (Wammes & Fernandes, 2015). Because enacting studied words invokes motor processing (Masumoto et al., 2006; Russ et al., 2003; Pulvermuller, 2005), participants would be able to re-instantiate this motor aspect of the representation to benefit later retrieval. As a result, successful retrieval of these enacted words would be bolstered through recapitulating this motor processing. However, when a distracting task during retrieval also invokes motor processing, there is overlap in processing requirements between the retrieval and distracting tasks. This results in interference (e.g. Wammes & Fernandes, 2016) and here, a reduction in the previously observed protective effect of enactment. Specifically, while the data suggest there was no interference with enacted words, this is still in stark contrast to Experiment 2, in which there were substantial protective effects from enactment (as evidenced by *better* recall after DA than after FA).

Experiment 4

Thus far, our findings are consistent with the hypothesis that adding an extra type of processing (i.e. motor processing) at encoding leads to a memory representation that is more robust in the face of processing-specific dual-task interference. In Experiment 3, we predicted that when the nature of the dual-task interference was now changed, such that it was demanding of motor processing, this protective effect would disappear. Our data supported this prediction. However, the possibility remains that there was nothing special about enactment per se, and that any encoding manipulation designed to augment encoding might confer protective effects. In other words, it could be that it is not adding a different type of processing at encoding that is critical, but rather, increasing the strength of the memory trace. While Experiment 3 provided some preliminary evidence against this competing explanation, it was worth testing, and ruling out this possibility in another way.

To this end, we returned to the animacy distracting task used in Experiments 1 and 2, but changed the encoding task from enactment to generation (Slamecka & Graf, 1978). This was done because generation likely exerts its beneficial effects by encouraging deep semantic processing at encoding (McElroy & Slamecka, 1982). In other words, rather than adding a different type of processing at encoding, generation simply augments the 'native' processing type. In line with our hypotheses then, the act of generation should provide no 'safety net' to rely upon, post-interference. This is because unlike with motor enactment, generation does not add an additional type of code or processing, but rather increases the strength of the extant code. If it were the case that *any* beneficial encoding task provided the protective effects discussed in the earlier experiments, then one would predict a much larger generation effect after DA than after FA. If however, it is important to add an extra *type* of processing at encoding, then one would predict no differential generation effect depending on attention conditions. Based on data from Experiments 1 through 3, we predict the latter; that there will be no protective effects resultant from generation at encoding, which would manifest in a null finding.

Method

Participants. 29 undergraduate students (19 female) from the University of Waterloo participated in this experiment for course credit. Their ages ranged from 16 to 32 (M = 20.48, SD = 2.92). All had normal or corrected-to-normal vision and had learned English before the age of 7.

Materials. All materials were identical to those used in Experiment 2, including our return to using the animacy distracting task, except for the to-be-remembered information. To maintain consistency, the list of words used as target stimuli was the same as in all previous experiments. However, a short generation cue was developed for each word. These

cues included a short phrase adapted from the dictionary definitions for each word, followed by the words' first letter, and a dash for each remaining letter of the word. For example, the generation cue for the word 'chop' was 'separate wood into small pieces with repeated axe blows, c---?', and for 'throw' was 'propel object through the air using arm movement, t-----?' Definitions were between 6 and 11 words long (*M* = 9.31, *SD* = 1.16).

Procedure.

Study, Test 1 (Recognition), and Test 2 (Recall). The only difference from Experiment 2 was that participants were instructed to either read the words silently, or to generate the word aloud using its definition. The items were no longer color coded, as the presentation of a word alone meant they were to read silently, while the presentation of a definition indicated that they were to generate the word. Test 2 still occurred 8 minutes after Test 1.

Results

Test 1.

Memory performance. Accuracy was analyzed as in Experiment 2. There was a significant main effect of Attention, F (1, 27) = 76.44, MSE = 0.04, p < .001, η_p^2 = .74, such that accuracy was higher under FA than DA conditions. All other effects failed to reach significance, ps > .25. (See Table 4).

Response time. RTs were analyzed as in previous experiments. Results indicated that hits were slower in the DA relative to FA condition, F (1, 25) = 110.23, MSE = 45477.80, p < .001, η_p^2 = .82, but no other effects reached significance, ps > .12 (see Table 4).

Proportion generation effect. The generation effect was computed as the proportion of hits that were generated during encoding. The generation effect for both FA and DA was compared using a paired samples t-test. Results indicated that the generation effect was not significantly different across the FA and DA conditions, t (28) = 1.83, SE = 0.04, d = .35, p = .08, *ns*.

Test 2.

Recall. Recall was analyzed as in previous experiments. There was a significant main effect of Instruction, F (1, 27) = 24.94, MSE = 1.56, p < .001, η_p^2 = .48, such that generated words were better recalled than read words, and a main effect of Attention, such that words were better recalled After FA than after DA, F (1, 27) = 21.02, MSE = 1.06, p < .001, η_p^2 = .44. The interaction was not significant, F (1, 27) = 0.01, MSE = 2.81, p = .91, η_p^2 = .00.

Proportion generation effect. The generation effect on Test 2, following FA and DA on Test 1, was compared using a one-tailed paired samples t-test. Results indicated that the generation effect following

Table 4: Number of Hits for Read and Generated Words, Number of False Alarms, and the Proportion Generation Effect under Conditions of Full (FA) and Divided Attention (DA) on Test 1 (Initial Recognition) in Experiment 4. Also shown are the Number of Read and Generated Words Recalled, as well as Proportional Generation Effect on Test 2 (Final Recall) in Experiment 4. Standard Errors of the Mean are in Parentheses.

Measure and trial type	Test 1		Test 2	
	FA	DA	After FA	After DA
Hits				
Read	6.64 (.23)	4.75 (.31)	2.11 (.25)	1.54 (.25)
Enact	7.75 (.11)	6.21 (.19)	3.32 (.27)	2.96 (.33)
False Alarms	0.57 (.17)	1.54 (.29)	-	-
Generation Effect	0.54 (.05)	0.58 (.08)	0.63 (.04)	0.69 (.04)
Accuracy				
Read	0.79 (.04)	0.48 (.04)		
Generate	0.93 (.02)	0.67 (.03)		
Response Time				
Read	859.77 (27.26)	1252.55 (30.05)	-	-
Generate	824.05 (19.94)	1252.20 (39.26)	-	-

DA did not differ significantly from that following FA, t (27) = 0.49, SE = 0.08, d = .09, p = .31 (see Figure 1). In fact, the generation effect following FA was numerically, though non-significantly, *higher* than after DA.

Dual-task interference. A proportional interference score ((FA-DA)/FA) was calculated for each participant for both generated and read words. A one tailed paired samples t-test indicated that dual-task interference with words that were read (M = .18, SE = .14) was not significantly different from those that were generated during encoding (M = .08, SE = .16), t (27) = -0.43, SE = 0.25, d = .08, p = .34 (See Figure 2).[6]

Animacy task. Distracting task performance was measured as the proportion of the 30 items that were responded to correctly. Paired samples t-tests indicated that participants scored lower in the DA (M = .63, SE = 0.03) than Baseline condition (M = .94, SE = 0.02), t (27) = 8.65, SE = .04, d = 1.66, p < .001.

Proportion correct.

Response time. RT for errors or omissions were removed from the analysis. Paired samples t-tests indicated RTs to make animacy decisions in the Baseline condition (M = 1132.33, SE = 22.80) were significantly faster than in the DA condition (M = 1422.85, SE = 24.80), t (27) = 9.91, SE = 29.32, d = 1.88, p < .001.

Discussion

Results indicated that when the encoding task for half of the trials was a semantic-based generation rather than enactment, there were no observable protective effects on either Test 1 or Test 2. Moreover, memory interference for words generated relative to read at encoding did not differ[7]. Consistent with the foregoing, the size of the proportional generation effect after DA was significantly smaller in this experiment, relative to the size of the proportional enactment effect in Experiment 2, despite both applying semantic distraction, t(49) = 2.24, SE = .09, d = .63, p < .05. These finding are in line with our predictions, and bolster the overarching theme of this line of experiments. Namely, adding an alternate type of processing to the memory trace at encoding will provide protection from long-term dual-task interference, but augmenting the same type of processing will not.

One curious finding was that there was no discernable generation effect during Test 1. This could be viewed as problematic, given that we were attempting to rule out a general memory strength explanation for our findings across Experiments 1 and 2. Taken alone, that we did not observe a *main* effect of generation in Test 1 suggests that memory strength might not have been successfully manipulated. However, the only case where we *did not* identify a generation effect was in Test 1 under FA. We note that we did observe a higher proportion of generated words identified (relative to read) during DA on Test 1, and also in Test 2 free recall after both FA and DA. It is important to consider also that we used short lists of items, which may have obscured any beneficial effects of generation. This step was taken intentionally in previous experiments to ensure that all of the words could be enacted successfully and that recall performance would not be at floor. In the current experiment, the list lengths were maintained to ensure that the stimuli were matched with previous experiments. We aimed to use generation as an encoding task to manipulate memory strength, and our Test 2 data (especially under FA) indicate that this was successfully done. Then, we aimed to determine whether this memory strength was sufficient to protect memory from dual-task conditions. Test 1 was simply a vehicle for the implementation of this dual-task manipulation, while Test 2 was where we observed and documented the outcome of that manipulation. The finding that there was no differential generation effect between FA and DA indicates that strength was not enough to provide protection to memory. Thus, the most critical point is that we *did* observe a significant generation effect in Test 2's free recall data (both in FA and DA), and that it was not different

[6] For the proportional generation effect, the pattern of the results was the same if list-wise deletion (n = 1) was used t (26) = 0.25, SE = 0.09, d = .05, p = .40. For interference scores, the pattern of results was also the same with list-wise deletion (n = 6), t (21) = -0.71, SE = 0.31, d = .15, p = .24.

[7] A Bayes Factor (BF) was calculated for the difference between proportional generation effects after FA and DA, using an online BF calculator (http://pcl.missouri.edu/bf-one-sample). Given that this was not a direct replication of previous experiments, we used the default JZS prior (.707). The BF_{null} was 4.47.

between the two. So, while the lack of generation effect on Test 1 is certainly puzzling, our conclusions throughout the experiment are drawn from Test 2 data, so the findings from Test 2 are of much greater importance in comparing with previous experiments. This is consistent with the proposition that the effects of DA at retrieval are best measured *after* the DA manipulation has taken place (Guez & Naveh-Benjamin, 2013).

General Discussion

This set of experiments aimed to determine whether enactment, by virtue of adding an extra type of processing to a word during encoding, could lead to protection from the long-term dual-task interference effects that have been documented previously following DA conditions during an initial retrieval phase (Dudukovic et al., 2009; Guez & Naveh-Benjamin, 2013). Experiment 1 showed that this type of protection was not observable in initial recognition performance (on Test 1). On a later Test 2, however, Experiment 1 showed that the proportion of words recalled on Test 2 that were enacted relative to read, was significantly larger following DA than FA. This result was coupled with the finding that words that were enacted during encoding sustained less dual-task interference than those that were read. Experiment 2 replicated these results, and showed that the difference in enactment effects, as well as susceptibility to interference, for read compared to enacted words, was even larger when Test 2 was delayed by 8 minutes. While these findings were quite interesting, the possibility remained that there was nothing unique about enactment, and that any task which bolsters memory would provide this protection. Thus, we turned our attention to testing this competing explanation. Experiment 3 addressed whether the protective effects reported were actually the result of an additional type of processing (i.e. adding a motor code). When we implemented a motor distracting task during Test 1, in place of the previously used semantic distracting task, the beneficial protective effects from enactment were blunted, suggesting it was the additional motor information that had protected memory. Lastly, in Experiment 4, we tested the competing explanation that *any* encoding manipulation would lead to similar protective effects using a different approach, and demonstrated that employing a semantic generation task rather than enactment at encoding did not lead to protection from dual-task interference. Together, these latter two experiments indicate that to achieve protection from dual-task interference, one must add a different code, or type of processing to the memory trace, rather than simply bolstering the existing means of encoding or increasing memory strength.

Our data suggest that DA during retrieval on Test 1 impairs the consolidation of the initially retrieved memory trace. The mechanism by which this occurs could be typical material- or processing-specific interference, as discussed in our previous work (Fernandes & Moscovitch, 2000; Fernandes & Guild, 2009, Fernandes et al, 2013; Wammes & Fernandes, 2016). To clarify, overlap in the processing requirements in the target memory and animacy distracting task likely degraded the phonological and semantic aspects of the memory representation before it could be laid back down. This is presumed to occur equally for both read and enacted words. However, the enacted words, by virtue of the addition of a motor trace, would have a safety net of a motoric memory representation upon which participants can base subsequent retrieval, thereby making later memory for those words more resilient to decay. To bolster this claim, the introduction of a motor-based distraction during Test 1 was shown to lessen this protective effect of enactment. We argue that this occurred as a result of the same governing principle: The motor distracting task overlapped with the motor processing requirements of retrieving the enacted words, thereby cancelling out any benefit that might have occurred.

Alternatively, it is possible that the decay in read words could be related to source memory confusion (Cohen & Faulkner, 1989; Johnson, Raye, Foley & Foley, 1981), in which concurrent presentation of the studied word during testing, alongside words in a distracting task, make it difficult to later ascertain on Test 2 which words were from the study phase. Such confusion would likely differentially affect read words, as people read words regularly in their everyday lives, while producing an action based on a read word is a rarer occurrence. With this, the enacted

words would be more distinct and novel, and thus less susceptible to source confusion. Similarly, in Experiment 3, this confusion may have occurred between enacted words, and the words with which a movement was associated in the distracting task. This possibility seems unlikely, however, as source confusion is typically greatest with similar items (Johnson & Raye, 1981; Kahan, 1996; Kim & Yassa, 2013), and while stimuli in both the target and distracting tasks were words, the target task was exclusively verbs and the distracting task was exclusively nouns. This was done intentionally to mitigate the impact of these concerns. With this, the literature on the matter (Johnson & Raye, 1981; Kahan, 1996; Kim & Yassa, 2013) would suggest that source confusion between the memory and distracting tasks should be minimal.

A fascinating alternative possibility is that rather than interfering with consolidation of the memory trace, DA during retrieval in our experiments undermined the quality of participants' retrieval attempts, thus diminishing the beneficial effects of testing. Indeed, there is evidence to indicate the importance of retrieval in determining later memory performance (Roediger & Karpicke, 2006a, 2006b; Whitten & Bjork, 1977). It is possible that when attempting to retrieve the information, the phonological/semantic distracting task hindered retrieval of the phonological aspects of the memory trace, but the motor information was retrieved freely. This limited form of retrieval could result in a similar pattern of results as we observed in this line of experiments. If access to certain aspects of the memory trace was restricted during Test 1 retrieval however, there should have been protective effects of enactment on Test 1 that were comparable to Test 2. This was not the case in the Test 1 data. Nonetheless, the importance of retrieval quality remains an interesting target for future research. Borrowing a baseline from testing effect research (Roediger & Karpicke, 2006a, 2006b), one could explore the long term effects of DA during Test 1, relative to DA during a 'restudy phase', thus determining whether our manipulation here affects consolidation, retrieval quality, or both.

It is important to note that not all studies examining the later effects of dual-task at retrieval found detrimental effects on memory. Gaspelin, Ruthruff and Pashler (2013) found a null effect, however the authors in this work were attempting to create ideal conditions under which they could observe *desirable* effects of dual-task. Accordingly, Gaspelin et al. (2013) asked participants to complete a distracting task wherein they were to discriminate between tones, which were presented concurrently with the studied words. Based on previous work showing that dual-task interference at retrieval occurs only when there is overlap between the type of materials or processing used in the memory and distracting tasks (Fernandes & Moscovitch, 2000), it is not surprising that no deficits in accuracy occurred as a result of DA in this work.

In contrast with the foregoing research, Kessler et al. (2014) found that when Test 2 was administered a day after Test 1, DA conditions lead to *better* later memory than FA. There were a few critical differences between our study and this one, which may explain why our results differed. First, Kessler et al. (2014) administered Test 1 a day after the material was studied, ensuring that the studied material was well consolidated, while our work administered Test 1 almost immediately after the study phase. The authors point this out as a critical difference between their findings (Kessler et al., 2014) and those of Dudukovic et al. (2009), which *did* show detrimental effects of dual-tasking during Test 1 on a later Test 2. They suggest that because Test 1 was administered immediately after Study in that work, the memory trace had been altered within its pre-consolidated state, whereas sufficiently consolidated material would be relatively immune to the detrimental effects of DA. The target words in our study too were likely still labile when the dual-task condition for Test 1 was experienced. This might explain in part why Kessler and colleagues (2014) found beneficial effects of DA and we (as well as Dudukovic et al., 2009) did not. It is interesting to speculate about whether we may have observed a different pattern of results had Test 1 been administered after a full day of consolidation. Based on our data, and the evidence to suggest that when reactivated at retrieval, consolidated memories are rendered labile again, and thus sensitive to change (Nader, Schafe & LeDoux, 2000), we would expect a similar pattern of results to what we found here. Though Kessler et al. (2014) showed an improvement as a result of DA after a 24-hour consolidation period,

it is of critical importance that their distracting task was chosen with the expectation that it *would not* lead to dual-task interference on Test 1, while ours was chosen because we were confident that it would. Accordingly, while Kessler et al.'s (2014) distracting task seemingly invoked something akin to desirable difficulty (Bjork, 1994) by simply making retrieval more effortful, ours caused catastrophic interference, but specifically to one trial type (the read words). This is an effect we anticipate would be consistent even after a delay.

We did not find any protective effects of enactment *during* Test 1 performance, a finding which is in line with those from Lozito and Mulligan (2006), suggesting that memory for words encoded with extra semantic processing were not differentially affected by dual-task conditions, relative to phonetically encoded words. While enactment likely added a unique motor, rather than semantic trace, the parallels between this study and the current work are clear. Given the consistent lack of differential Test 1 enactment effects across experiments, it is reasonable to propose that enactment provides no protection from the negative effects of DA at retrieval on an initial test of memory, at least if administered in the short-term. In contrast, we consistently showed a longer-term protective effect of enactment on Test 2 performance. The reason for why we did not observe that enacted words were better protected than read words *within* Test 1 could be similar to the logic invoked by Guez and Naveh-Benjamin (2013) in their explanation of the downstream negative effects of DA at retrieval. These researchers suggest that the reason for the well documented encoding-retrieval asymmetry (e.g. Baddeley et al., 1984; Craik et al., 1996), is that memory for encoded words (under FA or DA) is tested *after* encoding, while memory for retrieved words (under FA or DA) is tested *during* retrieval. The authors argue that confounds exist in most dual-task designs, as they almost always compare "memory 'for' encoded information vs. memory 'at' test" (Guez & Naveh-Benjamin, 2013, p.2). The authors contend that the only way to truly assess the outcome of DA at retrieval is to test for its effects, after the fact (Guez & Naveh-Benjamin, 2013). Accordingly, these researchers might argue that what we observed on Test 2 performance was in fact the appropriate measure of the effects of DA at Test 1, or initial retrieval.

One might also speculate that the explanation for the lack of enactment-related protection on Test 1 is that consolidation takes some time to occur, as evidenced by delay-dependent memorial advantages for emotional pictures (Sharot & Yonelinas, 2008). Also, previous work has suggested that enactment exerts its influence over a long time course (Knopf & Neidhardt, 1989), and that dual-task effects behave similarly, (i.e. effects are observable only following a delay; Dudukovic et al., 2009). Thus, it is possible that the memories interfered with during Test 1 were not fully consolidated; as such the motor code from enactment during encoding, may not have been stable enough to provide protection.

When we introduced a delay following initial retrieval, thereby providing more time for target memories to consolidate, protective effects, as measured by memory performance on Test 2 were augmented. Of critical importance is that the time elapsed between initial retrieval and final recall was identical across FA and DA conditions, so the resistance of enacted words to forgetting over time (Knopf & Neidhardt, 1989) cannot be invoked as an explanation for the current data. Our findings are consistent with our hypothesis that enacted words are more resilient to corruption from dual-task conditions. As proposed in the introduction, the mechanism by which enactment exerts this protective effect seems to be the addition of an extra code, formed during the encoding phase. This proposition was stringently tested in Experiments 3 and 4, and the available evidence supported our claims.

To reiterate, we propose that enactment adds an extra motoric code to one's memory which remains intact, and allows successful later retrieval, when the memory trace has been subjected to degradation from a semantic distracting task. Thus when the aftermath of a dual-task retrieval phase is gauged using a final recall test, the residual protective effects of enactment become apparent (See Figure 3, Panel A). However, when this reactivation of the motor code is hindered, through interference from a motor distracting task, the residual protective effects are severely blunted (See Figure 3, Panel B). Similarly, when the encoding task is generation, the existing phonological and semantic trace is augmented, but still susceptible to interference from the animacy distracting task, and thus no protective effects occur (See Figure 3, Panel C).

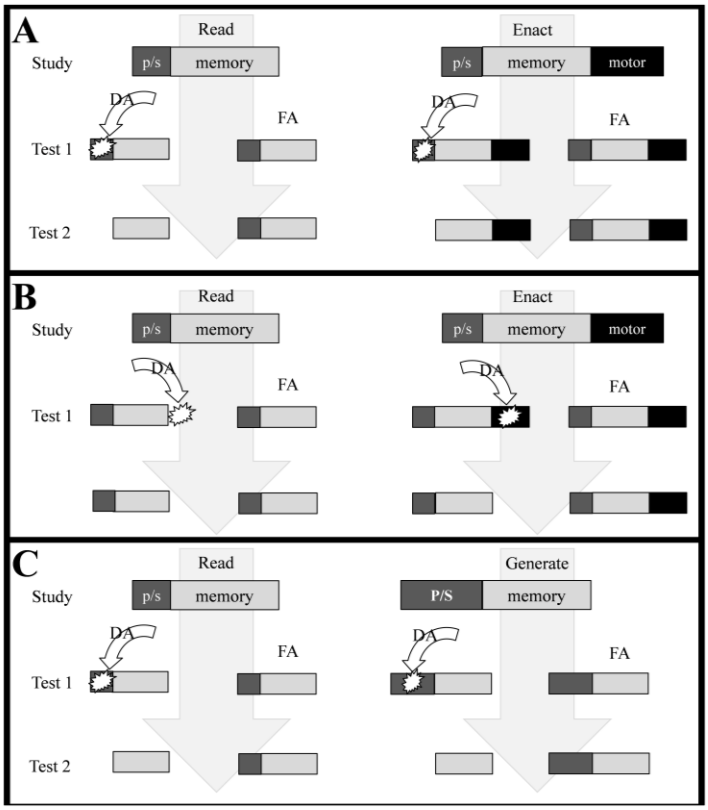


Figure 3. Panel A: Conceptual diagram of the proposed mechanism by which enactment exerts its protective effects: The act of enactment during study adds a motor code to the existing memory trace and its phonological/semantic (p/s) representation. During Test 1, DA results in the degradation of the p/s aspect of the trace. In the case of enacted words, the memory trace still has a motor code which can subserve memory on Test 2. Dark grey bars represent the motor code, while intermediate grey bars represent that p/s code. Panel B: DA with a concurrent motor task degrades the motor representation, thus preventing the previously observed benefits. Panel C: Encoding using a generation encoding orientation enhanced the p/s representation, which is then degraded by DA, thus preventing the previously observed memorial benefits.

Of course, this theoretical account is predicated on the assumption that the benefit of enactment occurs due to the addition of a motor trace. Our findings here are also consistent with this account. An alternative account however (Kormi-Nouri & Nilsson, 1998; Kormi-Nouri et al., 1994), suggests that the benefit derived from enactment is not due to the addition of a supplementary motoric memory trace, but instead memory is improved because studied items are encoded more self-referentially. Indeed, by converting what they read on the screen to a personalized motion that they themselves have created, participants may have rendered that study item more self-referential (D'Argembeau, Collette, Van der Linden, Laureys, Del Fiore, Degueldre, Luxen & Salmon, 2005; Kormi-Nouri et al., 1994), thereby increasing its later likelihood of subsequent retrieval. It would be difficult to reconcile this account however, with our findings on Test 2, in which enacted words showed enhanced memorability relative to read. Because enactment was always performed under FA conditions during encoding, there is no reason to expect that enacted words were made differentially self-referential. Moreover, recent research shows that DA selectively impairs memory for self-relevant information (Turk, Brady-Van den Bos, Collard, Gillespie-Smith, Conway & Cunningham, 2013), which would have resulted in a *smaller* enactment effect after DA if enacted words were more self-referential. Accordingly, our data provide support for the former explanation of enactment: that producing an action integrates a motor trace with one's representation of the target word. However, it is important to note that we did not implement enactment in a traditional way. That is, we asked participants to enact in response to stand-alone verbs, rather than phrases or items. Moreover, we did not directly test a self-referential account. While additional research is necessary to support the claim that enactment adds a motor trace, it would be an intriguing target for an fMRI exploration, given that different brain areas are implicated for self-referential items (D'Argembeau et al., 2005), than for motor-based processing of study items.

Another possible alternative is that enactment provided protective effects via increasing memory strength in general. That is, it could be the case that any beneficial encoding manipulation that increases the strength of an item during encoding would provide these protective effects. We provided evidence against this account by demonstrating the specificity of the protective effects in Experiments 3 and 4. Specifically, enacted words were not protected from motor interference, nor were generated words (i.e. items of greater strength) protected from phonological interference. This suggests at the very least that *not all* encoding manipulations designed to increase strength will support memory in the face of dual-task conditions. However, given that we did not observe as clear-cut a generation effect in our Test 1 as is

typically observed in recognition memory, it is possible that that particular strength manipulation was not potent enough to provide protection. While we contend that the generation effect in Test 2 is evidence enough that the manipulation increased memory strength, future work could test whether other strength manipulations are as effective as enactment in protecting memory, and it is our hope that our findings serve as a starting point for future work on the matter.

Less impactful, but still notable findings include that the enactment effect occurred reliably, even under DA conditions. Given that DA has more striking effects on recollection than on familiarity (Knott & Dewhurst, 2007) it was possible that the memorial advantage of enacted words would be cancelled out by greater interference as a result of enactment's reliance on recollection (Dewhurst & Brandt, 2007). Our results show firmly that enacted words are better remembered than read words, even under conditions of DA. Our findings also indicate that the enactment effect generalizes nicely to stand-alone verbs. This effect has previously been documented using objects with which an action can be performed and action phrases (Engelkamp & Zimmer, 1997; Knopf, 1991; Senkfor, 2008). Our work here adds to the relatively smaller set of studies which found an enactment effect using verbs in isolation (i.e. not embedded within action phrases; e.g. Engelkamp, Zimmer, & Kurbjuweit, 1995; Engelkamp, Zimmer, & Mohr, 1990; Engelkamp, Zimmer, Mohr & Sellen, 1994; Freeman & Ellis, 2003). With respect to the enactment effect, it is important to note that in experiments using pure lists (i.e. all enacted words or all read words), relative to mixed lists, the enactment effect is usually smaller in magnitude (Engelkamp & Zimmer, 1984; Steffens, 2007). Had we chosen pure lists, we might not have found protective effects of enactment, as memory would benefit less from the distinctive, item-specific information which separated enacted from read items. While this is an intriguing target for future research, we would still predict protective effects in pure lists, given that memory retrieval should still benefit from motor information, especially when dual-task interference renders the phonological and semantic information unavailable.

The most striking and important finding from this line of experiments is the existence of a residual protective effect of enactment. Associating a distinct action with some to-be-remembered information is a relatively simple and practical task that one could perform to aid memory in everyday life. A finding such as this may be especially relevant for seniors or those with mild cognitive impairment, who struggle with deteriorating episodic memory. Because motor memory systems often remain steadfast (Smith, Walton, Loveland, Umberger, Kryscio, & Gash, 2005; Walton, Scheib, McLean, Zhang, & Grondin, 2008) in the wake of episodic memory degeneration, the current research is both useful and important for current theories of memory, as it provides preliminary evidence that asking people to build and strengthen associations with their motor system provides effective compensation to scaffold episodic memory under distracting conditions. Given that multi-tasking is a near unavoidable experience in one's life, this is an important notion to pursue. Further, this work opens the door for the exploration of other SPTs that may endow one's memory with similar protection. The current work suggests that enactment is a viable memory preservation technique, one that is effective both against the inevitable decay related to forgetting over time, and the catastrophic effects of dual-task interference.

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