Measuring the activation level of critical lures in the Deese–Roediger–McDermott paradigm

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In four experiments, the activation level in memory of critical lures was assessed after encoding Deese–Roediger–McDermott (DRM) lists. The results demonstrated that studying longer, 14-item lists resulted in superadditive priming of the lures because they were more available in memory than truly studied items. Studying shorter DRM lists resulted in activation levels of the lures that was similar to studied items. Collectively, the results suggest that a first stage in creating false memories with the DRM paradigm is making the critical lures highly available in memory during list encoding. Moreover, the results suggest that false memories are likely to have occurred at the time a list is studied by a mechanism such as an implicit associative response, but a monitoring phase at retrieval is acknowledged that could be used to avoid them. Other theoretical accounts are also considered.

False recognition and false recall of words can be easily induced in people using variants of the Deese–Roediger–McDermott (DRM) paradigm. After studying a list of semantically related words such as *cigarette, puff, blaze,* and *billows,* people often erroneously claim that a critical lure (*smoke*) was among the words originally presented (Deese, 1959; Norman & Schacter, 1997; Read, 1996; Reyna & Lloyd, 1997; Roediger & McDermott, 1995; Seamon, Luo, & Gallo, 1998; Sommers & Lewis, 1999). Because the effect has been found so readily, many of the early research reports on the topic tried to reduce the incidence of false memories. For example, detailed warnings to avoid them at test only attenuate, but do not eliminate them (Gallo, Roberts, & Seamon, 1997; McDermott & Roediger, 1998). Only modest reductions in the false memory effect have been obtained by studying pictures rather than words (Schacter, Israel, & Racine, 1999). Greater reductions can be

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obtained when the critical item is truly experienced in a different episodic context from other studied items and participants realize this (Dodhia & Metcalfe, 1999) or when test instructions are given that highlight this fact (Smith, Tindell, Pierce, Gilliland, & Gerkens, 2001). In our own work, we discovered that source-monitoring instructions reduced false recall (Hicks & Marsh, 1999) but the same was not true of false recognition (Hicks & Marsh, 2001). Therefore, the general theme from this research is that these errors are tenacious.

Several explanations for the occurrence of false memories exist that all make highly similar predictions. Therefore, these explanations are difficult to empirically disambiguate from one another. For example, the implicit associative response (IAR) account proposes that the critical lure comes to mind consciously during the encoding of list items (Gallo, McDermott, Percer, & Roediger, 2001; Roediger, McDermott, & Robinson, 1998). At the time of test, participants come to believe that the critical lure was studied because they have a source-monitoring failure in being able to distinguish between items generated internally and those presented to them externally (Mather, Henkel, & Johnson, 1997). The fuzzy trace account proposes that false memories are the result of relying on gist memory traces rather than verbatim traces (Reyna & Lloyd, 1997) or that participants come to have a "phantom" recollection for the critical lures (Brainerd, Wright, & Reyna, 2001). However, most of the empirical work that has examined the conditions that reduce false memories may not speak directly to the theoretical mechanisms that contribute to the false memories in the first place. Rather, these studies demonstrate that strategies (or forms of monitoring) can be brought to bear at test that can be used to counteract what has already occurred at encoding. For example, if a candidate lure lacks recollective details of the encoding experience, then it can be rejected as not having been studied (Brown, Buchanan, & Cabeza, 2000). The studies manipulating pictures and words (Schacter et al., 1999) and our own work with source monitoring are merely variants on the same message. Nevertheless, encoding manipulations that affect the magnitude of the false memory effect probably have the greatest bearing on the mechanisms underlying the initial creation of false memories or at least an elevation of their stature to becoming good candidates for later false remembering (Smith & Hunt, 1998; Robinson & Roediger, 1997).

In the Robinson and Roediger (1997) article, association strength of the presented lists was manipulated to show a titration effect on the level of false memories. Fifteen-item lists were presented and various numbers of DRM items were presented on a given list. With one DRM item on a study list, 14 unrelated items composed the remainder of the list, with 5 DRM items, 10 unrelated items filled out the list, and so forth.

At test, the level of false recognition was proportional to the number of DRM items studied. An identical set of results was found by merely manipulating how many associates were studied and letting list length vary. The authors concluded that the critical item was more likely to be evoked at learning by an IAR with greater numbers of related items being studied. If this hypothesis is true, then the activation level of the critical lure after list presentation should be equivalent to that of the items that were studied. If multiple IARs occur, then perhaps the activation level of the critical lure will be even greater than list items because it was "studied" multiple times during list presentation.

The goal of the present study was to compare the activation level of critical lures with that of truly studied items after a DRM list was encoded. We embedded the critical lure along with two studied items in a lexical decision task. More highly activated items in memory are responded to more quickly in a lexical decision task than are less activated items (Anderson, 1983; Ratcliff & McKoon, 1978). Thus, the logic used here is that there is an inverse relationship between lexical decision time and activation: Shorter latencies to identify an item as a word indicate that the word's representation is more highly activated. We have used this same logic in a number of different paradigms and in a number of different contexts (Marsh, Hicks, & Bink, 1998; Marsh, Hicks, & Watson, 2002; Marsh & Landau, 1995). After participants study a list of items, if the critical lure is highly activated as it might be from the occurrence of an IAR, it should be identified as a word faster than a nonpresented control item that is matched on word frequency, syllabic length, and number of letters because these are the variables known to affect latency independent of activation status. If the critical lure is no more activated than a control-matched word that is not presented, then an IAR account would be severely weakened because that outcome would suggest that the critical lure's representation in memory has not been affected by the presentation of the study list.

Therefore, we are simply exploring whether one obvious prediction from the IAR account comes to pass, namely that critical lures should be highly activated if they consciously occurred during list encoding. Thus, we are studying one early stage that may be a precondition for the occurrence of a later false memory. The activation that we are studying should not be construed as the cause of a false memory. Rather, we are assessing whether it is a viable precondition. One could attribute even highly activated lures to oneself by a monitoring process during the test and then later deny having studied them (Mather et al., 1997). However, these items may not have even become candidates on the memory test unless they were activated initially. In this fashion, we are studying whether the activation of lures is one reason that they could become candidates for false memories independent of the type of paradigm that might be used for testing (e.g., free recall, recognition, source monitoring).

All four experiments that follow are highly similar to one another, and for the sake of brevity we present a general method first. Basically, our approach was to present 24 DRM lists, follow each with a brief distractor task, and then assess the activation levels of 20 letter strings in a lexical decision task. Consequently, participants took 24 lexical decision tests. The main difference across the experiments was the two particular list items that were tested in the lexical decision task.

GENERAL METHOD

Participants

All participants in the four experiments were undergraduates at the University of Georgia who volunteered in exchange for partial fulfillment of a course research requirement. All volunteers were tested individually in sessions that lasted approximately 45 min. No participant repeated his or her participation in the experiments reported in this article.

Materials and procedure

Twenty-five lists were chosen from the Stadler, Roediger, and McDermott (1999) norms. One list (the *smoke* list) was designated as a practice list and was always presented first. The remaining 24 lists were randomized anew for each participant by software that was written for this series of experiments. Each list contained the 14 top associates, and the list members were presented in descending order of backward association strength (Roediger & McDermott, 1995). Items on the study list were presented at a 5-s rate for a total study time of 70 s. After list presentation, the computer beeped and simple addition problems were presented for 30 s. The computer provided feedback on incorrect computations to ensure that participants were motivated to pay attention to the distractor activity. After this task, a 5-s message appeared that instructed participants to prepare for the word–nonword judgment task. During this time participants poised their two index fingers over the “word” and “nonword” buttons on the keyboard (we have traditionally used the home keys “F” and “J” for this purpose and did so in these experiments). Twenty letter strings were presented, with 10 words and 10 nonwords. The nonwords were pronounceable and were created by changing a consonant or a vowel in English words other than those on studied lists (e.g., *flurb*).

Participants began the experiment by reading instructions from the computer monitor about the sequence of events. These instructions provided details about the study phase, the distractor task, and the lexical decision task. Participants were also informed that they would repeat this cycle 25 times. As motivation to study each list, participants believed that their memory would be tested at the end of the 25 cycles. In reality, no such test was given. The experimenter reiterated these instructions and then took the participants through the practice list's sequence of events. The software paused after the first list to allow the participant to ask the experimenter questions. When the experimenter was convinced that the participant understood the procedure, the experimental trials were initiated. The composition of the letter strings in the lexical decision task was half words and half nonwords. The valid English words were 6 items randomly drawn from a pool of 150 words unrelated to the DRM lists, the critical lure, the critical lure's control-matched word that was never studied, and two items from the list that were studied just before the distractor task. To reiterate, each control-matched word was identical in word frequency, number of letters, and syllabic length to the critical lure for a given list. Which two studied items were tested varied by experiment.

The insertion of items into the lexical decision task was random under the following constraints. To avoid semantic priming effects, the two studied list items and the critical lure were always separated by a minimum of three items (nonwords or unrelated items). In lexical decision tasks, semantic priming from one trial to the next (e.g., *smoke* after *puff*) would result in facilitation for the second word (Neely, 1977). This sort of priming had to be avoided to make the results from these experiments interpretable. Fortunately, short-term priming does not survive an intervening unrelated item, which is why we interposed several unrelated items. The survival of priming past an intervening trial has been discovered only recently and requires that all the nonwords be constructed as pseudohomophones of real words (e.g., *phrog;* Joordens & Becker, 1997). Why priming survives in this very restrictive case is beyond the scope of this article. Suffice it to say that none of the nonwords used in this study were pseudohomophones. Finally, the list items and critical lure were never tested as the first five items in order to avoid any warm-up effects that could have existed at the beginning of each lexical decision task. The same was true of the control-matched word. We believed that participants might be a bit slower at the beginning of each lexical decision task as they reacquainted themselves with the task, and therefore we believed it was prudent that the first few trials not contain items most critical to the study. All other items were randomly placed in the lexical decision sequences for all 25 lists.

Each lexical decision trial began with a 250-ms fixation point accompanied by a short warning tone. A 250-ms blank screen followed, and then the letter string appeared and remained on the screen until a judgment was entered. A 750-ms intertrial interval occurred during which the screen was blank before the next trial was initiated by the software. In the experiments that follow, error trials were eliminated (on average 2.3%). In addition, reaction times that were more than three standard deviations from a given participant's average reaction time were declared outliers and were eliminated (an additional 0.6%). Because nonwords generally receive the longest reaction times, they made up the majority of the outlying responses.

EXPERIMENT 1

The purpose of this experiment was to assess the activation level of the critical lure in comparison to list items and its nonpresented control word. If IARs occur during the study sequence, then the critical lure should be responded to as quickly as list items that were studied. If IARs are not occurring, then the average response latency to the critical lures should be longer than truly studied items and no shorter than that of the control items that were never studied. In this experiment we included the seventh and eighth studied items in the lexical decision task because these words represent only moderate backward association strength to the critical lures.

METHOD

The general method was followed in this experiment. Twenty-five volunteers participated and received partial course credit. One nonnative speaker of English was replaced because of inordinately long reaction times.

RESULTS AND DISCUSSION

Unless specified otherwise by a p value, statistical significance does not exceed chance by the conventional 5% of a type I error. The results of this experiment (and the others that follow) are summarized in Table 1. As is customarily found in lexical decision tasks, nonwords received the longest latencies. The critical lures were responded to much more quickly than their control-matched counterparts, $t(24) = 6.09$. That outcome suggests that the representations of critical lures were highly activated from studying the DRM list associates. In fact, the critical lures were responded to much more quickly than truly studied items, $t(24) = 3.69$, which suggests that activation accrues very strongly to the critical lures. The fact that response times are shorter for critical lures than for studied items is consistent with the notion that IARs may occur multiple times. Responses to the unrelated words were obviously slower than either the critical lures or studied items, both $t(24)$s > 5.0, but were not different from the control words, $t(24) = 1.82$, *ns*. Bonferroni correc-

Table 1. Latency (ms) to the five trial types in Experiments 1–4

	Critical lure	Control matched	Studied items	Unrelated	Nonwords
Experiment 1	576 (14)	636 (17)	595 (14)	651 (15)	727 (25)
Experiment 2	577 (13)	632 (14)	596 (14)	659 (14)	716 (25)
Experiment 3	592 (20)	653 (24)	592 (23)	672 (25)	771 (51)
Experiment 4	603 (18)	652 (20)	615 (18)	674 (19)	783 (30)

Note. Standard errors are in parentheses.

tion for multiple comparisons does not change the outcomes of this experiment.

The results from this experiment are consistent with the notion that the representations of critical lures are highly activated by list encoding. To the extent that activation is a form of availability (Marsh & Landau, 1995), the critical lures are very available in memory after a DRM list is studied. Because the critical lures were never actually studied, their greater availability than studied list items represents a form of superadditive priming. By using the term *superadditive* we mean only that they are responded to more quickly than truly studied items. This outcome dovetails well with results presented by McDermott (1997). She found that studied items resulted in less priming than the critical lures in a conceptual implicit memory task, whereas the opposite occurred in a perceptual implicit task where studied items were primed more than critical lures. The fact that critical lures are more activated in a conceptual implicit test fits with the notion that the lexical decision task used here is conceptual as well. That is, people must access conceptual representations of words in order to make word–nonword judgments, and this is especially true given that pronounceable nonwords were used in the lexical decision task (Shoben, 1982). Therefore, the generalization might be made that critical lures will always be more available on tests that tap conceptual features.

EXPERIMENT 2

The goal of this experiment was twofold. First, we tested studied items from the 2nd and 13th positions of the lists in the lexical decision task. Our goal was to assess whether their activation levels would differ because they were, respectively, higher and lower backward associates to the critical lure. If reaction time in the lexical decision task is proportional to how related items are to the list theme, then reaction times to the 2nd and 13th items could be different, which might provide some evidence for why the critical lure was the most activated representation in Experiment 1. If their latencies did differ, then we predicted that the 2nd item would receive shorter latencies than the 13th item. Second, this experiment provides another opportunity to replicate the results of Experiment 1.

METHOD

Twenty-four University of Georgia undergraduates volunteered in exchange for partial credit toward a course requirement. Each participant was tested individually. The only difference in the procedure from Experiment 1 was to include the 2nd and 13th list items in the lexical decision task rather than the 7th and 8th list items.

RESULTS AND DISCUSSION

There was no reaction time difference to the 2nd (592 ms) and the 13th (599 ms) items, $t(23) < 1$, *ns*. Therefore, reaction time does not appear to depend on how related an item is to the list theme but rather was determined by the fact that it was simply studied. The critical lures did receive shorter latencies than the control-matched words, suggesting again that these items accrued activation from studying the list items, $t(23) = 7.48$. In addition, the critical lures had latencies that were shorter than the truly studied items, $t(23) = 3.19$. That outcome represents a replication of the superadditive priming found in Experiment 1. There was one outcome that did not replicate Experiment 1: the fact that control words received shorter latencies than the unrelated words, $t(23) = 3.95$. We have no ready explanation for this result except that the participants in this experiment displayed somewhat less variability than those in the first. The overarching point is that this experiment provides additional evidence for the highly activated state of the critical lures after studying lists in the DRM paradigm.

EXPERIMENT 3

According to Robinson and Roediger's (1997) results, the more DRM items that are studied from a given list, the higher the probability of the critical lure evoking a false memory. If this outcome is generally true, then participants who study only the top three items from a DRM list may not exhibit the superadditive priming found in Experiments 1 and 2. In other words, studying only three list items may result in much less activation of the representations of the critical lures. In this case, latencies to the critical lure may be longer than latencies to studied list items. We tested this hypothesis in this experiment.

METHOD

Twenty-six volunteers participated for partial course credit. The general method was changed such that instead of 14 items for each list, only the top 3 items were presented. Obviously, this manipulation limited the items we could place in the lexical decision task, and we arbitrarily chose to include items two and three. With the drastically reduced study list, the time to complete this experiment was less than 25 min.

RESULTS AND DISCUSSION

There was no difference between the response latencies to the second (592 ms) and the third (590 ms) studied items, $t(25) < 1$, *ns*. The critical lures received faster responses than their control-matched words, suggesting that they did accrue activation from the presentation of the top three associates, $t(25) = 5.60$. However, latencies to the critical lures were no different from the average of the two studied items, $t(25) < 1$, *ns*. The absence of the superadditive priming effect found in Experiments 1 and 2 suggests that Robinson and Roediger's (1997) hypothesis about total association strength is correct. Presenting all list items gives the critical lure its maximum activation level, although presenting fewer items at study nevertheless activates its representation to the level of a truly studied item.

EXPERIMENT 4

The purpose of this experiment was to assess whether presenting low associates would further weaken the activation level that accrues to the critical lure. We presented the bottom three associates (items 12 through 14) at study and inserted items 13 and 14 in the lexical decision task. If the low associates have a lower probability of evoking an IAR for the critical lures, then latency to the critical lures should be longer than items that were just studied.

METHOD

Twenty-five participants were tested using the method of Experiment 3. The only difference was that in this experiment rather than studying the first three DRM items for a given list, participants studied the last three items.

RESULTS AND DISCUSSION

As in the previous three experiments, latency to the critical lures was shorter than to the control-matched words, $t(24) = 4.67$. That outcome suggests that even the three lowest associates have the ability to cause activation to accrue to the critical lure. Although latency to the critical lure was numerically faster than to the average of the two studied items, the difference was not statistically significant, $t(24) = 1.83$, *ns*. Thus, like the previous experiment, the representation of the critical lure is only as activated as a truly studied item. We did not expect this outcome, but it is consistent with the idea that even a low total association value of a list can create some level of false memories (Robinson & Roediger, 1997). We turn now to what these results might indicate about the genesis of false memories in the DRM paradigm.

GENERAL DISCUSSION

When a full list was studied (in our experiments, that was 14 items), we found superadditive priming accruing on the critical lures such that latencies to them were shorter than to items from the study list. Moreover, this outcome was true of items on the study list that came from the beginning, middle, or end of the list because all of them appeared to be equally activated. When fewer items were presented, the representations of the critical lures were still activated above a baseline resting state, but not as greatly. In those cases, the representations of the critical lures were approximately equivalent to the items that were studied. These results clearly suggest that the conceptual priming from studying related associates is more powerful than mere repetition priming from having experienced particular words before. To the extent that Robinson and Roediger (1997) found that the levels of false memories were a function of the number of items studied, we found that the activation levels of the critical lures were a function of the number of items studied. By way of inference, our results suggest that the degree to which a list activates its critical lure's representation determines its availability, which in turn may be the first stage in making the lure a candidate for becoming a false memory later.

We attempted to test this hypothesis post hoc by correlating reaction times to critical lures with the numerical ranking of how well particular lists produce false memory effects from the Stadler et al. (1999) normative compendium. We could not find evidence of a significant correlation in Experiment 1 when correlations were calculated within subjects for the 24 lists and then averaged across participants. However, we did not test all lists in those norms, and 24 pairs of numbers is hardly the best empirical basis on which to go looking for correlations after the fact. Perhaps if we had tested all 36 lists in the norms, the desired correlation would have emerged. Thus, the degree to which a list produces false memories may still indeed be a direct function of how well a particular list activates the representation of its corresponding critical lure, but the evidence is not definitive at this juncture.

We tend to believe that our data are most consistent with an IAR account that posits critical lures coming to mind during study. However, the ability to monitor their source of occurrence (one's self vs. presented by an external agent) during test would be one means by which to edit them out (see Hicks & Marsh, 1999). Therefore, the present data suggest that availability of the lures may be just the first stage in producing a false memory, but the item must slip past any subsequent monitoring that may be done at retrieval to become a false memory. A similar account has recently been put forth by McDermott and Watson (2001) in which an activation stage is followed by a monitoring stage. During the activation stage critical lures become activated by a standard spreading activation mechanism. The editing phase is a slow, conscious retrieval phase during the test, much like source monitoring, that allows rejection of items believed not to have been studied. Shades of these source-monitoring accounts at retrieval that can be used to avoid false memories can be found in Schacter's distinctiveness heuristic as well (Schacter et al., 1999). In that theory, distinctive encoding such as studying the items as pictures rather than words allows people to avoid false memories because they realize that if the critical item had been studied then they would be able to recollect distinctive memorial details for it (which they cannot). These two stage accounts are based on two-process theories of recognition (e.g., McElree, Dolan, & Jacoby, 1999), and they appear to fare very well in head-to-head competitions of the various theories that have been proposed to account for false memories.

Nevertheless, our finding of the highly activated state of critical lures would also be consistent with a single-process model that posits that only activation level (or familiarity) is responsible for later false memories (Underwood, 1965). The problem with such models is that they do not explain results from existing manipulations. For example, Seamon, Luo, Schwartz, Jones, Lee, and Jones (2002) recently found a nonmonotonic function of the level of false memories across various numbers of representations of the list (see also Benjamin, 2000). As small numbers of presentations increased, the level of false memories rose commensurately to a point. However, as the number of presentations continued to increase, the level of false memories began to drop off commensurately. Presumably, if activation were the only operative mechanism leading to false memories, then a monotonic increase should have been found across all levels of list repetitions no matter how large that number was. Seamon et al.'s results are more consistent with the activation account from encoding (and the data reported in this article) followed by a source-monitoring account during retrieval (Hicks & Marsh, 2001; Marsh & Hicks, 2001). Moreover, the single-process account does a poor job of explaining why participants often report remembering recollective details for false memories (Roediger & McDermott, 1995). Therefore, the favored general account appears to be that items become activated during encoding (as we have demonstrated here), and if they slip past some monitoring stage, perhaps based on the source-monitoring framework, then they become false memories at test.

To summarize, any theory of the genesis of false memories in the DRM paradigm must account for the highly activated representations of the critical lures after list presentation. The production of an IAR followed by the source-monitoring error in which the internally generated information is mistaken for externally generated information would provide an account consistent with the existing empirical data on DRM false memories. Moreover, the highly activated state of the critical lures makes them good candidates for recall because they are available and, for the same reason, would also account for false memories in recognition paradigms as well. Perhaps others will be able to accommodate the present data in better or different ways than we have done here with the existing theories. Regardless, the experiments reported here represent a step away from struggling to specify the test conditions that increase monitoring and consequently reduce false memories. Rather, they represent a step toward understanding what preconditions must be met in an earlier stage before test instructions are even delivered.

Notes

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