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One explanation of the benefit of directed forgetting is the reduction of proactive interference (PI) from the first list during encoding of the next list. To explore this possibility, I used a forget instruction in a release from PI paradigm. The forget instruction did not reduce the PI on subsequent lists as measured by the immediate tests usually given in this paradigm in Experiments 1-3. The other variable manipulated in this study was the recall tests. A reverse test effect was found on a final recall measure in Experiment 2. Immediate recall tests in a PI paradigm may already serve as an implicit forget cue to the participants. This finding has implications for both directed forgetting and proactive interference research.

DIRECTED FORGETTING IN A PROACTIVE INTERFERENCE PARADIGM

by

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CHAPTER I

INTRODUCTION AND LITERATURE REVIEW

Is it possible to forget information intentionally? And if so, why would we want to? Colloquially, we talk about forgetting as an accidental and often frustrating process, usually associated with a decline of cognitive abilities. However, there is plenty of everyday information we deem irrelevant or no longer useful; inaccessibility of that information would not be so bad. As we shall see, research into this positive application of forgetting indicates that yes, we can intentionally forget information. Used in this way, forgetting is a useful and adaptive tool for updating old information. The ability to discard irrelevant and out of date material actually might aid in the learning of current information through a reduction of interference. If for no other reason, directed forgetting is an interesting possibility because its existence suggests that willed processes can trump the effects of prior learning.

Researchers study directed forgetting effects using two different methods: the item method and the list method. Recent literature attributes the results from these two methods to different mechanisms (Basden, 1993; Basden & Basden, 1998; MacLeod, 1998). The item method consists of the presentation of one word at a time followed by an instruction to either remember or forget that word for a later recall test. After the presentation of all the words is complete, participants are tested on their memory for both the to-be-forgotten words and the to-be-remembered words. Basden and Basden (1998) attribute the results of this method to differential processing of the to-be-forgotten words and the to-be-remembered words. The participants only have to retain a forget word until the instruction appears following the word, whereas they know the remember words must be retained until the recall test at the end. The item method models a situation where the to-be-forgotten material is not encoded well because the instruction or intent to forget comes immediately after the information. For example, if a friend recites his phone number but immediately follows it with the statement 'you don't need to remember that, I already programmed it in your speed dial', the other person most likely does not make an attempt to learn the phone number as she knows she will be able to rely on her speed dial.

In the list method, participants are presented with two lists of words and warned prior to the presentation that they will be tested for their memory of the words. In between the presentation of list 1 and list 2, the experimental group is told that the first list is to be forgotten, it is no longer needed for the memory task. The control group does not receive any further instructions. The participants are all tested on their memory for both lists. The effects found are a cost and a benefit (Bjork, 1970). The experimental group recalls fewer list 1 words than the control group; this is the cost. The experimental group, however, recalls more list 2 words than the control group. This is the more interesting finding of the two and is referred to as the benefit of directed forgetting. The participants are told to forget list 1 words, but there is nothing explicit in those instructions to cause them to remember list 2 words better than the control group. This benefit is the part of directed forgetting that makes it seem like an adaptive function of memory. By whatever method the old information becomes inaccessible, the new information benefits from the inaccessibility. Outside the lab, it is necessary to update information about our lives, for example, a new home phone number after a move; and unless it helps remember the new information, what would be useful about forgetting the old information? The list method is a better lab demonstration of intentional forgetting of information that was learned at one point and needs updating. The participants do not have any reason not to learn list 1 to the best of their ability because the instruction to forget does not come until after they have learned the list.

Some researchers use the concept of inhibition to explain the effects of the forget instructions. According to Bjork, Bjork, and Anderson(1998) the forget instruction causes inhibition of the list 1 words, rendering them inaccessible at recall (the cost). The words are understood to remain suppressed until re-exposure of the words releases the inhibition (Basden & Basden, 1998). The inaccessibility of the words also reduces proactive interference on the list 2 words (Bjork et al., 1998) and allows more of them to be recalled (the benefit). The most simple explanation is the forget instruction itself triggers the inhibition mechanism, something about the word "forget" or the concept of unnecessary information initiates the inhibition of list 1 words. Even in his earliest work, however, Bjork (1970) argued that some additional material must follow the to-be-forgotten material if the inhibition mechanism is to work. In this view, it is the competition at encoding that triggers the mechanism. If inhibition is the mechanism at work in the directed forgetting paradigm, a forget instruction and competing material at encoding are necessary and sufficient to cause the directed forgetting effects observed.

CHAPTER II

EXPERIMENT 1 METHODS

In the first experiment, I used a release from proactive interference paradigm (Wickens, 1972) with categorized lists similar to Loese (1967) to test the idea that inhibition of list 1 words reduces the proactive interference on subsequent lists in the directed forgetting paradigm. The release from proactive interference paradigm is used in the interference literature to demonstrate the build-up of proactive interference (PI) during learning. In the type of release from PI paradigm I used, three lists of words from the same category are presented with a recall test following each list. The fourth list in the sequence contains words from a different category. The results show a decreasing pattern of recall until the fourth list, which demonstrates a rebound in number of words recalled. The argument based on these results is that the words from one category do not cause as much interference on a list from a different category as do words from the same category. Because this paradigm is a clear example of a build-up of proactive interference, and the inhibition explanation argues that inhibition of list 1 words reduces PI in the directed forgetting paradigm, it follows that a forget cue should reduce proactive interference in the release from PI paradigm as well. In this design, both of the necessary factors for directed forgetting according to the inhibition account are present, the forget instruction and competing material following the instruction.

Participants

The participants were 124 University of North Carolina at Greensboro undergraduates who received credit in their Introductory Psychology class. They were tested in groups of 3-5.

Materials

Twelve lists of eight high-frequency exemplars from four categories were taken from Overschelde, Rawson, and Dunlosky (2004). Each list contained eight exemplars from the same category. The lists were counterbalanced across the order of presentation and the words within a list were presented in random order. There were twelve different orders of the lists. The lists were presented using E-Prime software.

Procedure

The participants were told they would see lists of words and they were to learn the words for a recall test. The words were presented at a rate of 1750 milliseconds per word (Kane & Engle, 2000). After all eight words of a list were presented, the participants performed a short even/odd decision distracter task. Thirty random numbers (1-100) appeared on the screen one at a time and the participants were instructed to press "E" for even numbers and "O" for odd numbers. The participants were then given 45 seconds to write down as many of the list words they could remember. The recall instructions specified to write down only words from the list most recently presented. After recall, the participants were given either a forget instruction or a neutral instruction and the next list began. The forget instruction was "You no longer need the information from the list you just recalled. Please FORGET the contents of that list. You will now begin the next list”. The neutral instruction contained only the last line “You will now begin the next list”. The instruction (both versions) remained on the screen for 5 seconds. This procedure repeated for three lists from the same category and one list from a different category.

CHAPTER III

EXPERIMENT 1 RESULTS

An alpha level of .05 was used for all analyses. Figure 1 shows the mean recall rates for all the lists. A 4x2 (ImmediateList x Instruction) mixed ANOVA was performed with ImmediateList as the repeated measure. The levels of ImmediateList were the recall following the presentation of each list and the levels of Instruction were forget instructions and controls. It revealed a significant effect of ImmediateList, $F(3,366) = 34.46$, $p < .001$. There was no significant effect of Instruction, $F(1, 122) = .043$, *n.s.,* nor a significant interaction, $F(3, 366) = .414$, *n.s.* Within-subjects contrasts showed a significant difference between List 1 recall and List 2 recall, $F(1,122) = 13.66$, $p < .001$ and a significant difference between List 3 recall and List 4 recall, $F(1,122) = 44.99$, $p < .001$. There was no significant difference between List 2 and List 3 recall, $F(1, 122) = .12$, *n.s.*

Discussion

The significant effect of ImmediateList on recall confirms the build-up and release from PI pattern usually observed in this paradigm. Most of the build-up in Experiment 1, however, occurred between the first and second list of words. The build-up of PI between List 1 and List 2 is consistent with the findings of Kane and Engle (2000). The lack of significant difference between List 2 and List 3 may be a result of learning the routine of the study offsetting the additional PI from List 2. All tests were used for analyses, there were no practice tests for the participants to learn the demands of the test prior to the experiment. My particular question of interest for this study was the effect of a forget instruction on the immediate recall of the categorized lists. I expected to observe a benefit of the forget instruction on the recall of the words presented after the forget instruction. Contrary to my predictions, no directed forgetting benefit was observed in the PI paradigm.

CHAPTER IV

EXPERIMENT 2 METHODS

In a second experiment, I sought to reconcile some of the differences in the methods between the release from PI paradigm and the directed forgetting paradigm to determine if there is another necessary condition to cause directed forgetting effects. One of the major differences between the methods is the nature of the recall test. In the directed forgetting paradigm, participants are tested on their memory for all the words from list 1 and list 2. The participants see list 1, the forget instruction, and list 2 before they are asked to recall any words. In the release from PI paradigm, participants are asked to recall each list right after they see it and there is no cumulative test for their recall of all the words. Once the participants recall the list after its presentation, they are never tested on those words again. The release from PI paradigm could be seen as a situation where intentional forgetting should be happening without an explicit instruction, as the participants do not have to remember the words once they have recalled them (Bjork et al, 1998). The decrease in number of words recalled from each list as the number of lists increases, however, indicates that the benefits of directed forgetting are not present with or without explicit instruction. So in Experiment 2, I included the final recall dependent measure from the directed forgetting paradigm in my release from PI paradigm; after presentation of the four lists, a final recall test was administered for which the participants were instructed to recall as many words as they could.

The other major difference between the method in Experiment 1 and the directed forgetting list method concerns the timing of the recall test relative to the forget instruction. The recall test occurred prior to the forget instruction in experiment one whereas in the normal procedure, the forget instruction comes prior to a recall test. Only one study in the extant literature includes a recall task prior to the forget instruction (Sahakyan, Delaney, & Kelley, 2004). In this study, the recall attempt is in the form of a self evaluation of memory for list one words. Before receiving the forget instruction, participants are told to recall the list one words, causing them to note their memory performance. In this study, the comparison of their 'standard' forget group to their 'forget with evaluation' group revealed no difference between the two groups. In Experiment 2, I assessed whether or not the recall test prior to the instruction changes the effect of the instruction. Each group only recalled one of the lists immediately after the presentation and before the instruction, allowing us to determine if this changed the effect of the forget instruction

Participants

The participants were 126 University of North Carolina at Greensboro undergraduates who received credit in their Introductory Psychology class. They were tested in groups of 3-5.

Design

In this design, there were two independent variables: Instruction and ImmediateTestTime. Instruction had two levels: forget and control. The forget groups received a forget instruction after every list presentation, the control groups did not.

ImmediateTestTime, the timing of the immediate recall test, was also manipulated between groups. Each of the four immediate recall test conditions was designated $\text{Experimental}_{(X)}$ and received a forget instruction after each list or $\text{Control}_{(X)}$. Participants saw four lists of words and the dependent measure of interest was their recall of the words from the lists. In $\text{Experimental}_{(1)}$ and $\text{Control}_{(1)}$, participants only completed an immediate recall test after the first list of words. In $\text{Experimental}_{(2)}$ and $\text{Control}_{(2)}$, participants only completed an immediate recall test after the second list of words; $\text{Experimental}_{(3)}$ and $\text{Control}_{(3)}$ only after List 3; and $\text{Experimental}_{(4)}$ and $\text{Control}_{(4)}$ only after List 4. By this design, each group only completed one immediate recall test but received either a forget or a neutral instruction after every list. An additional dependent measure, a final recall test after the presentation of all four lists, was administered. The participants were instructed to learn the words on the lists but were not given any specific information on what kind of test or which lists would be tested.

Materials

Same as Experiment 1.

Procedure

The procedure was the same as Experiment 1 with the following exceptions. The participants only received the instructions to recall the words from the last list they saw after one of the lists depending on their group assignment. After the participants saw all four lists and took one immediate recall test, they were instructed to recall all the words they saw from all the lists. They were given two minutes to complete the final recall task.

CHAPTER V

EXPERIMENT 2 RESULTS

Immediate Recall

The means for immediate recall can be found in Figure 2. In this experiment, the immediate list recall was a between subjects manipulation. In order to assess the build-up of proactive interference and the effect of a forget instruction, a 4x2 (ImmediateList x Instruction) mixed ANOVA with ImmediateList as the repeated measure was performed. It revealed a significant main effect of ImmediateList, $F(3,121) = 27.22$, $p<.001$. A post-hoc Tukey HSD test revealed a significant decrease in recall from List 1 to List 2, a decrease between List 2 and List 3, and an increase between List 3 and List 4. There was no main effect of Instruction, $F(1, 121) = .039$, p = *n.s.*

Final Recall

Words recalled on the final recall task were coded according to their original list membership, creating the variable FinalList. The mean recall on the final recall measurement can be found in Figure 3. In order to assess the cost of directed forgetting on the final recall measure I used 4x2x4 (FinalList x Instruction x ImmediateTestTime) mixed ANOVA with FinalList as the repeated measure. The test revealed a significant main effect of FinalList, $F(3, 363) = 49.365$, $p < .001$. The only significant contrast of final recall was the difference between List 3 and List 4 final recall. A significant main effect of Instruction was also found in the final recall measures, $F(1, 121) = 10.360$, p =

.002. An independent samples t-test was used on the total recall from lists 1-3 to compare the forget groups to the control groups. This test revealed a significant cost, $t(124) = -2.979$, $p < .01$. The t-test on List 4 recall, however, revealed no benefit of directed forgetting, $t < 1$, n.s. The interaction of ImmediateTestTime x FinalList was also significant, $F(9, 363) = 7.283$, $p < .001$.

The effect of the immediate recall of a list prior to the final recall task was explored with one-way ANOVAs and planned contrasts. A one-way ANOVA on F1 (List 1 words recalled on final recall test) revealed a significant effect of ImmediateTestTime, $F(3, 125) = 4.27$, $p < .001$. The same effect was found for F2, $F(3, 125) = 5.19$, $p < .001$, for F3, $F(3, 125) = 3.62$, $p < .01$, and for F4, $F(3, 125) = 2.71$, $p < .05$. Planned contrasts were constructed in order to compare the group who completed an immediate test after each list to the three groups who did not on the corresponding final measure score. The difference between the group who had an immediate recall test after List 1 and the other groups on final recall of List 1 was marginally significant, $t(125) = 1.817$, $p = .072$. The difference between the group who had an immediate recall test after List 2 and the other groups on final recall of List 2 was significant, $t(125) = 2.736$, $p = .007$. The difference between the group who had an immediate recall test after List 3 and the other groups on final recall of List 3 was significant, $t(125) = 3.239$, $p = .002$. The difference between the group who had an immediate recall test after List 4 and the other groups on final recall of List 4 was significant, $t(125) = 3.498$, $p = .002$. In all cases, the groups taking an immediate test recalled fewer words than the group who did not. Figure 4 displays the means for the group that completed the test and the groups that did not, collapsed over Instruction condition (as the main effect was not significant). There was no significant effect of list presentation order in Experiment 2.

Discussion

Experiment 2 replicated the finding from Experiment 1 that the forget instruction does not have an effect on the immediate recall tests, indicating that it is not reducing the build-up of PI from test to test. The design of Experiments 1 and 2 did not allow us to measure costs on the immediate recall tests, therefore I can only conclude that the forget instructions and the competing material at encoding were not sufficient to produce directed forgetting benefits on immediate recall tests. I did find a directed forgetting cost-the groups who received an instruction to forget lists 1, 2, and 3 remembered less of those words on the final recall than those who were not given the forget instruction. Although it was not a statistically significant finding, the results suggested a benefit on list 4 recall for the forget groups. I expect that this benefit would be more noticeable if the fourth list contained words from the same category, instead of acting as a release from PI list. The PI acting on the release list may not have been enough in the control group to observe a difference in the forget group.

The immediate recall test after the list also had an effect on memory for the list words. I observed a reverse test effect, fewer words were recalled on the final recall measure when participants has already been tested on these words. This pattern emerged in both the forget groups and the control groups. It does not address my initial interest in how the forget instruction affects immediate recall in the PI paradigm and so I will return to it later in the general discussion.

CHAPTER VI

EXPERIMENT 3 METHODS

The results of my first two experiments led us to the question of when the to-be-forgotten words become inaccessible. It appears from the results that the forget instruction is not working to reduce the proactive interference on the immediate recall of the next list, however, it is working on the words during final recall as evidenced by the cost observed in the forget groups on final recall. According to Bjork's (1970) interpretation of directed forgetting, the words from the previous list should be inhibited after the forget instruction and in response to the encoding of the next list. Therefore, there should be less proactive interference on the immediate recall test of the next list in the forget group than in the control group. I did not observe this effect. The directed forgetting is working on the final recall but not at immediate recall after each list. At this point, I conclude that the forget instruction and new material are necessary but not sufficient to produce directed forgetting effects.

Sahakyan, Delaney, and Kelley (2004) propose a different explanation of directed forgetting effects. Unlike the inhibition account, they postulate different mechanisms for costs and the benefits. List 1 is remembered worse in the experimental group than in the control group because the forget instruction causes a shift in mental context. The mental context of the test then matches the mental context in which List 2 was learned and mismatches the mental context of List 1. Therefore, a cost is observed (Sahakyan, 2004).

The benefits are due to a deeper encoding strategy used on List 2 (Sahakyan & Delaney, 2003). The forget instruction causes participants to evaluate their learning of List 1 and realize it was not very good. This realization causes the participant to switch to a deeper encoding strategy and learn List 2 better than List 1. Participants in the control condition do not receive a forget instruction and therefore do not change mental context or encoding strategy between lists. In this account of directed forgetting, the only necessary condition is the forget instruction, which initiates both a mental context change and a self-evaluative process. The design of both of my previous experiments included a forget instruction and did not reveal a measurable directed forgetting benefit on immediate recall. In Experiment 1, the forget instruction after List 1 should have prompted an evaluation of List 1 learning for the participant and caused him to switch to a deeper encoding strategy on List 2. I did not, however, observe better recall on List 2. According to Sahakyan's explanation, the forget instruction in Experiment 2 should also have caused a self-evaluation of List 1 learning and a switch to a better encoding strategy, but it did not. While the results do not support the strategy change explanation for the benefits of directed forgetting, there are other factors that influence the effect of forget instruction, including list length effects (Sahakyan & Delaney, 2005). Importantly, I did observe a disassociation between the cost and benefit of directed forgetting in Experiment 2: the cost was present on the final recall measure, but not a benefit.

The incongruity of my results with both an inhibition explanation and a context/encoding change explanation caused me to look more closely at the retrieval requirements of the directed forgetting paradigm. Within the directed forgetting literature, two different retrieval tasks have been administered to participants. In most of the studies using the list method, the participants are instructed to remember and record as many words as they can from either list and in any order (Basden, & Basden, 1996; Basden, Basden, & Wright, 2003; Basden, Basden & Morales, 2003; MacLeod, 1999; Wilson et al., 2003). In these studies, both costs and benefits are found. In contrast, Sahakyan (2004) in her first experiment separated the recall tests by list affiliation, instructing participants to first recall list 1, list 2, and then list 3 on separate sheets of paper. In this experiment, she did not find reliable benefits on either list 2 in the condition where the participants received a forget cue after list 1 or on list 3 in the condition where the participants received a forget cue after list 2. However, in a second experiment, she combined the recall task (in addition to changing the stimulus material), instructing participants to recall all three lists, and found benefits on list 3 in the condition where the participants received a forget instruction after list 2. There was no benefit on list 2 for those participants who were instructed to forget list 1, but this could be due to a list length effect. The participants who receive the forget cue after list 2 generalize that cue to both list 1 and list 2, giving them more words to evaluate prior to learning list 3. No studies, to my knowledge, have contrasted directly these two sets of recall instructions and their effect on the benefits of directed forgetting, but the combination of the results from Sahakyan (2004) and my previous two studies raised interest in the effect of retrieval demands on directed forgetting.

In the 'real-life' examples of directed forgetting as a beneficial updating process, the information has the potential to compete at the retrieval phase. A useful example of directed forgetting as an updating tool is the situation where a person has moved homes several times and must update their 'home phone number' for each different residence. In this example, the phone numbers are not learned at the same time, however, they all share the retrieval cue 'home phone number'. It is not useful to bring to mind each phone number every time the current one is required for paperwork or to place a call; therefore it is adaptive to forget the old numbers. In the directed forgetting paradigm, when the recall instructions say to remember all words shown during the study, the words from List 1 and List 2 are in competition for retrieval in the same way each of the phone numbers potentially competes for entry into consciousness when asked for the 'home phone number'. In contrast, the immediate recall tests in my first two experiments did not entail this competition at retrieval; the only demand was for recall of the immediately preceding list. The final recall test, however, did entail competition among lists at retrieval and it was here that I saw an effect of directed forgetting.

Experiment 3 was designed to determine if a shared retrieval cue is necessary for the costs and benefits of directed forgetting to emerge. Two different types of immediate recall tests were included in this study, a single list test and a cumulative test. Single list immediate recall tests required the participant only to recall the words from the list directly prior to the test. Cumulative immediate recall tests asked for recall of words from two or more lists at once. I tested the hypothesis that a shared retrieval cue is the third necessary condition for directed forgetting. Therefore, I predicted that the cumulative tests would show both the costs and the benefits of directed forgetting. At the time of the immediate cumulative test, participants will have seen between one and three lists with a forget instruction and the list directly prior to the test without a forget instruction (the instruction is given after the test). Therefore, the lists with the forget instructions should show lower recall on the cumulative test as compared to the control group. For example, $\text{Experimental}_{(3)}$ and $\text{Control}_{(3)}$ complete a cumulative immediate recall test after List 3; at the time of that test, participants in $\text{Experimental}_{(3)}$ have been instructed to forget List 1 and List 2 words. The instructions for the immediate cumulative test provide retrieval competition for the lists. List 1 and List 2 words in the $\text{Experimental}_{(3)}$ group should show a cost and List 3 should show a benefit when compared to the $\text{Control}_{(3)}$ group because the forget cue, the competing material, and the competition at retrieval are all present in the cumulative test situation. I also predicted that if competition at retrieval is necessary then a reduction of proactive interference would also show up on the immediate recall test following a cumulative recall test. On the test after a cumulative test, there should be fewer lists interfering with the recall, as the words from previous lists should be inaccessible. In the example above, List 3 on the cumulative test should display a benefit because List 1 and List 2 are no longer accessible to interfere with the retrieval of List 3. Then, when the participant learns and retrieves List 4 on the next immediate test, only List 3 words are accessible to interfere, as compared to the $\text{Control}_{(3)}$ group where List 1, 2, and 3 are accessible to interfere with List 4.

Method

Participants

One hundred forty undergraduate psychology students from the University of North Carolina at Greensboro participated in the experiment in order to fulfill research requirements for course credit.

Design

In this design, there are two independent variables: Instruction and CumulativeTestTime. Three of the six groups were the experimental groups and received forget instructions after each list and the three control groups did not receive forget instructions. In this between subjects design, the experimental groups were compared to the control groups. In addition, the requirement of the immediate recall tests was manipulated between groups. All of the groups had an immediate recall test after the presentation of each list. Each immediate recall test instructed the participants to either recall only the last list they saw (single list recall) or to recall all of the lists they have seen so far (cumulative recall). The groups each completed one cumulative recall immediate test, giving CumulativeTestTime three levels. The placement of this test varied between groups such that it occurs after either list 2, 3, or 4. The lists not followed by the cumulative test had a single list recall test. Each of the three immediate recall test conditions were designated $\text{Experimental}_{(X)}$ and received a forget instruction after each immediate recall test or $\text{Control}_{(X)}$. Therefore, forget groups were compared to control groups while controlling for test requirements. Participants saw four lists of words and the dependent measure of interest is their recall of the words from the lists. Memory for the presented words was tested both immediately following each list presentation (immediate recall) and after all four lists were presented (final recall).

Materials

Sixteen lists of eight words each from four categories were used in this experiment. Each set of four lists contained words from a single category (e.g., fruits) drawn from Overschelde, Rawson, and Dunlosky (2004) and Battig and Montague (1969). The words were presented in random order within a list using E-Prime software. The categories were counterbalanced across conditions.

Procedure

Each participant saw four lists from the same category. Each word in the list was presented for 1750 ms. In between the presentation of each list, after a short distracter task, participants performed an immediate recall test. Following the immediate recall test, participants received instructions to proceed to the next list. Experimental groups were given a forget instruction indicating that they no longer need the information in the previous list and will now begin the next list. The Control groups were told it was time for the next list to begin. The instructions for the immediate recall test depended on the group assignment. $\text{Experimental}_{(2)}$ and $\text{Control}_{(2)}$ completed a cumulative immediate recall test following the presentation of List 2 and a single list recall test after List 1, List 3, and List 4. $\text{Experimental}_{(3)}$ and $\text{Control}_{(3)}$ completed a cumulative recall test following the presentation of List 3 and a single list recall test after List 1, List 2, and List 4. $\text{Experimental}_{(4)}$ and $\text{Control}_{(4)}$ completed a cumulative recall test following the presentation of List 4 and a single list recall test after List 1, List 2, and List 3. The immediate recall tests were performed using pen and paper. At the completion of the four lists and immediate recall tests, participants performed an unrelated puzzle task for five minutes. After the distracter task, all participants were asked to recall all the words they saw on all of the lists in the experiment.

The dependent measures of interest are the number of words recalled on each of the immediate tests and the number of words recalled on the final test for each participant. I expect these measures to be affected by both the instruction type and the immediate recall instructions.

CHAPTER VII

EXPERIMENT 3 RESULTS

Thiry-two participants were eliminated from the analyses for failure to comply with the instructions. For each immediate test, instructions appeared telling the participants either to recall only the last list they saw or all of the lists, depending on if it was a single list immediate recall test or a cumulative immediate recall test. The participants received a set of these instructions after each list and were then given a pen and paper to complete the recall test. The experimenter observed that after the first two lists, some participants were not reading the instructions after each list to notice a change from single list test to a cumulative list test presumably because they assumed the instructions were the same each time. Those participants who did not recall any words from any lists other than the list directly prior to the test on a cumulative immediate recall test were excluded for failure to read instructions. This exclusion primarily affected the groups with a cumulative immediate test on List 3 or List 4 as the participants has already formed the habit of only recalling a single list.

Immediate Recall

In order to assess the effects of a forget instruction and a cumulative recall instruction on the immediate recall tests, a 4x2x3 (ImmediateList x Instruction x CumulativeTestTime) mixed ANOVA with ImmediateList as the repeated measure was performed on the immediate recall tests. Some of the immediate recall tests in this analysis were cumulative and therefore included more than one list of words, only the words recalled from the most recent list presented were included in this analysis. For example, if the participants took a cumulative immediate recall test after the presentation of List 3, the words they recalled from List 1 and List 2 on that test are not included in this analysis. The words recalled on the cumulative tests from lists other than the more recently presented were analyzed separately. The mixed ANOVA of ImmediateList x Instruction x CumulativeTestTime revealed a main effect of ImmediateList, $F(3, 285) = 12.89$, $p < .001$, indicating a build-up of proactive interference on immediate tests. There was no main effect of Instruction, $F(1, 95) = .82$, *n.s.*, replicating the ineffectiveness of a forget cue from Experiment 1 and Experiment 2. The means for the Instruction groups on immediate recall can be found in Figure 5. There was also no main effect of CumulativeTestTime, $F(2, 95) = .05$, *n.s.*; the type of test (cumulative versus single list) did not affect the number of words from the most recent list recalled by the participant. My hypothesis that both a forget instruction and a cumulative recall instruction are necessary for directed forgetting benefits predicted an Instruction x CumulativeTestTime interaction. This interaction was not significant, $F(2, 95) = .52$, *n.s.*

In addition to the benefit of directed forgetting emerging on cumulative immediate recall tests, I was able to measure cost on cumulative immediate recall tests. I was not interested in the differences between cumulative recall on different tests, only the difference between the Instruction groups. Therefore, I ran independent samples t-test for each comparison of list on each cumulative recall test. On the cumulative immediate recall test after List 2, there was an almost reliable cost of the forget instruction on the recall of List 1, $t(43) = -1.77$, $p = .08$ (see Figure 6). On the cumulative immediate recall test after List 3, there was no difference between the forget and control group on List 1 recall, $t(25) = .03$, *n.s*. However, there was a significant difference on List 2 recall, the forget group recalled significantly more List 2 words than the control group, $t(25) = 3.170$, $p = .004$, opposite of the predicted cost (see Figure 7). The cumulative immediate recall test after List 4 showed no difference on either List 1 recall, $t(26) = 1.54$, *n.s.*, or List 2 recall $t(26) = -.96$, *n.s*. List 3 recall on the cumulative test after List 4 showed the same pattern as List 2 on the List 3 cumulative recall test. The forget group recalled significantly more List 3 words than the control group, $t(26) = 2.22$, $p = .04$ (see Figure 8).

Final Recall

The final recall test was coded by list membership as in Experiment 2, creating the variable FinalList. The means can be found in Figure 9, demonstrating an increasing pattern of recall on later lists. A 4x2x3 (FinalList x Instruction x CumulativeTestList) mixed ANOVA with FinalList as the repeated measure revealed a main effect of FinalList, $F(3, 285) = 10.90$, $p < .001$. There was no main effect of Instruction, $F(1, 95) = 2.57$, *n.s.* or CumulativeTestList, $F(2, 95) = 1.61$, *n.s*. The three-way interaction of FinalList x Instruction x CumulativeTestList was significant, $F(6, 285) = 3.67$, $p = .002$. I unpacked this interaction by selecting for and analyzing each CumulativeTestList group separately. A mixed ANOVA of FinalList x Instruction on the group who received a cumulative immediate recall test after List 2 revealed no significant main effect of Instruction, $F(1, 44) = 1.04$, *n.s.* or FinalList x Instruction interaction, $F(3, 132) = 1.22$, *n.s.* The mixed ANOVA of FinalList x Instruction on the group who received a cumulative immediate recall test after List 3 also yielded no significant main effect of Instruction, $F(1,25) = 2.59$, n.s. or FinalList x Instruction interaction, $F(3,75) = 2.27$. The repeated measures ANOVA of FinalList x Instruction on the group who received a cumulative immediate recall test after List 4, however, revealed a significant FinalList x Instruction interaction, $F(3, 78) = 5.38$, $p = .002$. This interaction was explored with a series of univariate ANOVAs. There was a main effect of Instruction on the recall of Lists 1 and 2 on the final recall measure. By looking at the means of these two groups, the effect is in the opposite direction for the two lists. The Forget group recalled more than the Control group on List 1 and the Control group recalled more than the Forget group on List 2. The explanation for this effect of Instruction is unclear. There was no main effect of Instruction on the recall of Lists 3 or 4 on the final recall measure.

In addition, unlike Experiment 1 and 2, the costs of directed forgetting were assessed in the immediate recall tests by using the cumulative immediate recall tests. Due to the design of the experiment, different groups took cumulative immediate recall tests after different lists. Therefore, in comparing the costs, independent sample t-tests were used to compare the appropriate groups. An independent samples t-test comparing the recall of List 1 on the cumulative test following List 2 between $\text{Experimental}_{(2)}$ and $\text{Control}_{(2)}$ revealed a marginally significant difference, $t(43) = -1.77$, $p = .08$.

Discussion

The main question I wanted to address with Experiment 3 was whether competition at retrieval, in the form of a cumulative test, was necessary to produce directed forgetting effects. My hypothesis was that on cumulative immediate tests, I would observe the effects of a directed forgetting instruction, namely a cost and a benefit. Furthermore, I predicted that the immediate tests subsequent to the cumulative immediate tests would show the benefit of directed forgetting as the PI build-up should have been reduced at the point of the cumulative test. I did not, however, find a benefit of directed forgetting on any of the immediate tests, cumulative or single-list. This finding replicated Experiment 1 and 2 that there was no benefit of a forget instruction on immediate recall tests. In addition, I did not observe the predicted cost on the cumulative immediate recall tests. Instead, the forget instruction had the reverse effect on the recall of List 2 on the cumulative recall following the presentation of List 3 and on the recall of List 3 on the cumulative recall following the presentation of List 4.

It is possible that the repeated presentation of the forget instruction caused participants to doubt its sincerity and work harder to maintain the words from previous lists. Unlike the control group, the repeated forget instruction may have caused suspicion about a subsequent test on the to-be-forgotten words. This would explain the forget groups recalling significantly more of the lists they were supposed to have forgotten on the cumulative immediate tests after List 3 and List 4 but not after List 2. After List 2, the participants had only received one forget instruction, and although it was not reliable, the forget group did recall less of the List 1 words at that point. Contrary to this explanation, however, were the self-reports collected by the experimenter in conversation following the debriefing session indicating that the participants tried to forget the words when instructed. The participants denied ignoring the forget cue after seeing multiple presentations or growing suspicious about a subsequent test. The forget cue was worded such that it was not misleading to the participants. Instead of claiming they would not be tested on the material, it instead stated that forgetting the material would help their performance on subsequent tests. In a way, considering the benefits of directed forgetting, this was not a lie.

An additional question addressed in Experiment 2 and Experiment 3 was the effect of the forget instructions on a final recall measure. The cost observed in the forget instruction group in Experiment 2 was not replicated in this experiment. I also did not find the predicted benefit on final recall of List 4.

CHAPTER VIII

CONCLUSION

In the course of analyzing the data from Experiment 3, I decided to look at the data in the first two experiments from a different perspective. Using a method from Kane and Engle (2000), I calculated the proportion of PI build-up across the lists. To calculate these proportions, the participant's immediate recall of List 1 is used as a baseline. The baseline is subtracted from immediate recall on the subsequent Lists and then the difference is divided by the baseline, producing a proportion of PI build-up. The PI proportions collapsed over Instruction group from Experiments 1-3 can be found in Figure 1.

Discussion

The magnitude of difference between the proportions of PI build-up in Experiment 2 from those in Experiment 1 and Experiment 3 was startling. The build-up in Experiment 1 seems to look the closest to the PI build-up observed in Kane & Engle (2000), while the PI build-up in Experiment 2 is much larger, especially for List 2. The main difference between Experiment 1 and Experiment 2 is the lack of an immediate recall test after each list presentation. The presence of the recall test seems to reduce the PI build-up on the next list. In Experiment 3, participants are once again receiving an immediate recall test after each list presentation, sometimes on more than one of the lists (in the case of cumulative tests) and the PI build-up is even lower than in Experiment 1.

In terms of my original question of whether a directed forgetting instruction can affect the immediate recall tests in the PI paradigm, it looks as if there may already be an implicit one in place during the task. The recall of the list words directly after presentation may serve to trigger an implicit forgetting instruction to the participants. They may assume that the information is no longer necessary after completing the recall task. If this is the case, this paradigm is an interesting way to study forgetting in addition to PI.

General Discussion

Overall, I found no evidence for a benefit of a forget instruction on immediate recall tasks in a PI paradigm. I found some evidence for the cost of directed forgetting on a final recall measure (Experiment 2) and in the cumulative immediate recall tests (Experiment 3). This disassociation potentially provides evidence for a dual mechanism model for directed forgetting (Sahakyan & Kelley, 2003). However, this research originally sought to address the question of whether instructions to forget could reduce PI in a PI paradigm, as it is suggested the instruction does in a directed forgetting paradigm (Bjork 1970). The answer at this point is no, the forget instruction does not reduce PI in this paradigm. However, the reason for this lack of reduction of PI is more complicated than anticipated.

The post-hoc analyses of PI build-up in these experiments presents an alternate explanation for why PI is not reduced by a forget instruction, even though there is some evidence for a cost due to the forget instruction. The PI paradigm itself may provide an implicit forget instruction to the participants. Support for this idea comes also from the analysis of the effect of immediate tests on the final recall measure in Experiment 2. The lists on which the participants had not had an immediate recall test were better recalled on the final recall measure. This finding requires replication, as the sample sizes were uneven for this comparison. If the findings hold up, this idea has implications for the use of the paradigm as a good measure of PI build-up. From the results, I suggest the immediate tests themselves could be providing a partial release from PI for the participants. In directed forgetting paradigms, even when List 1 words are forgotten, and a cost is observed, some of the List 1 words are still recalled on a final recall measure. This indicates that part of the list is still accessible; those words have the potential to interfere with the subsequent list. In the same way, in the PI paradigm, if the immediate test causes forgetting, there are still some of the words from the previous list accessible to interfere with the next list, thus, a build-up of PI is observed. The PI build-up observed, however, is not the maximum amount of PI build-up, as the tests provide some release between each list.

The suggestion that the immediate test causes forgetting, and presumably worse recall on a subsequent test, stands in opposition to the empirically validated "test effect" (Carrier & Pashler, 1992; Kuo & Hirshman, 1996). In their work, Kuo and Hirshman looked at the test effect in a modified Brown-Peterson paradigm and concluded that testing improved recall on a final recall test (1996). Their methods, however, vastly differ from my own and provide the potential for differences in my findings. Of interest in their study is the comparison of repeated study trials of three word lists to test trials of the same. There is not a comparison of tested lists to non-tested lists without repeated study trials. I suggest that the presentation of competing stimuli directly after the test (with no prior suggestion of a final recall test) is the key element in my study. In addition, the proportion of items correctly recalled on the final test was lower than the proportion recalled on my final measure by the groups who received an immediate test.

The explanation of the benefits of directed forgetting put forward by Sahakyan and her colleagues is related to this finding. They believe the forget instruction itself causes self-evaluation of memory for List 1 words and a strategy change for List 2 encoding. Evidence for this claim comes from a comparison of an evaluation condition with a forget condition. The benefit observed in these two groups is the same (Sahakyan, Delaney, & Kelley, 2004). Sahakyan and colleagues use this finding to argue that the forget instruction causes an evaluation of the previous list, therefore, the same benefit is observed in both groups. The explanation for a lack of benefit in my immediate recall tests is a list length effect (Sahakyan & Delaney, 2005), whereby participants' change of strategy is contingent upon a poor self-evaluation. A short list of words does not provide the participants with the opportunity to properly evaluate their performance as it is a relative judgment (six out of eight words may not seem like poor recall to the participants, whereas the same proportion- thirty out of forty words may seem like poor recall). Based on the evidence offered in this study, it is possible that the opposite explanation is causing the same benefit in the evaluation and the forget groups in Sahakyan, et al., (2004). It is unclear in what way the aggregate JOLs in Experiment 2 lead to the same forgetting as recalling the entire list, other than the 15 seconds allowed for the formation of a JOL could have been used to recall the contents of List 1.

The immediate tests in the PI paradigm can be equated to one example of voluntary forgetting outside the laboratory. In the case of the friend who gives you a phone number, you need only to remember that phone number until you have the opportunity to write it down or program it into your cell phone. In this paradigm, the participants may use the immediate recall tests as the opportunity to write the information down, and subsequently, forget it. This particular finding from these data deserves more attention. I plan to replicate and expand on this idea in future studies.

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APPENDIX A. FIGURES

Figure 1. Proportion of words recalled on immediate tests in forget and control groups from Experiment 1.

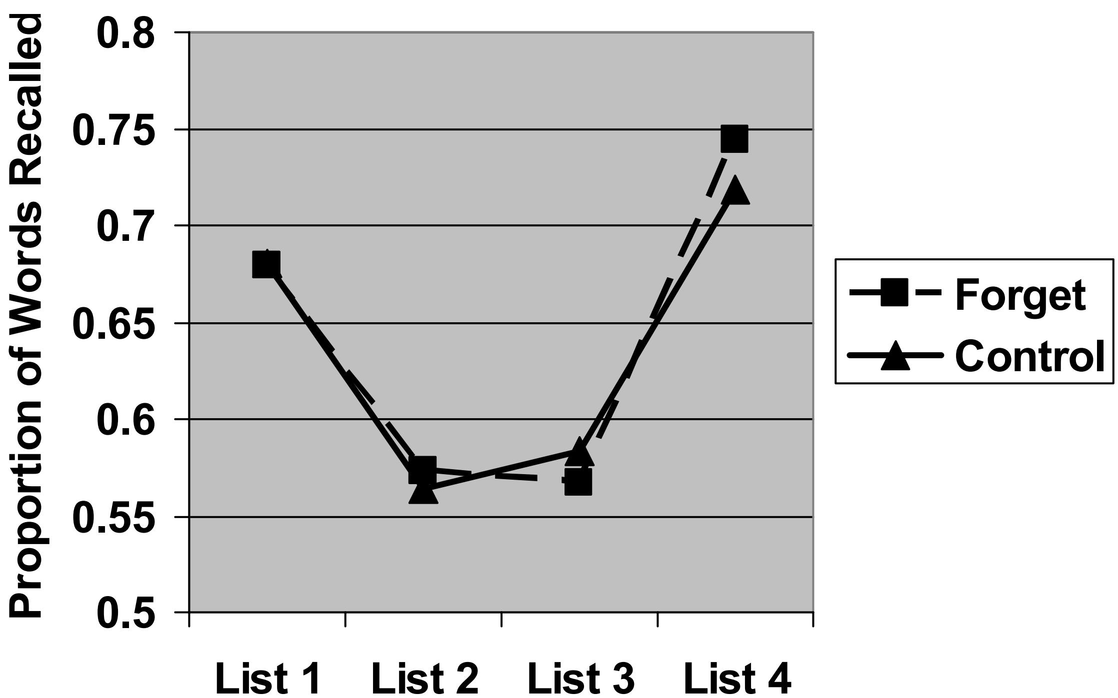

Figure 2. Proportion of words recalled on immediate tests in forget and control groups from Experiment 2.

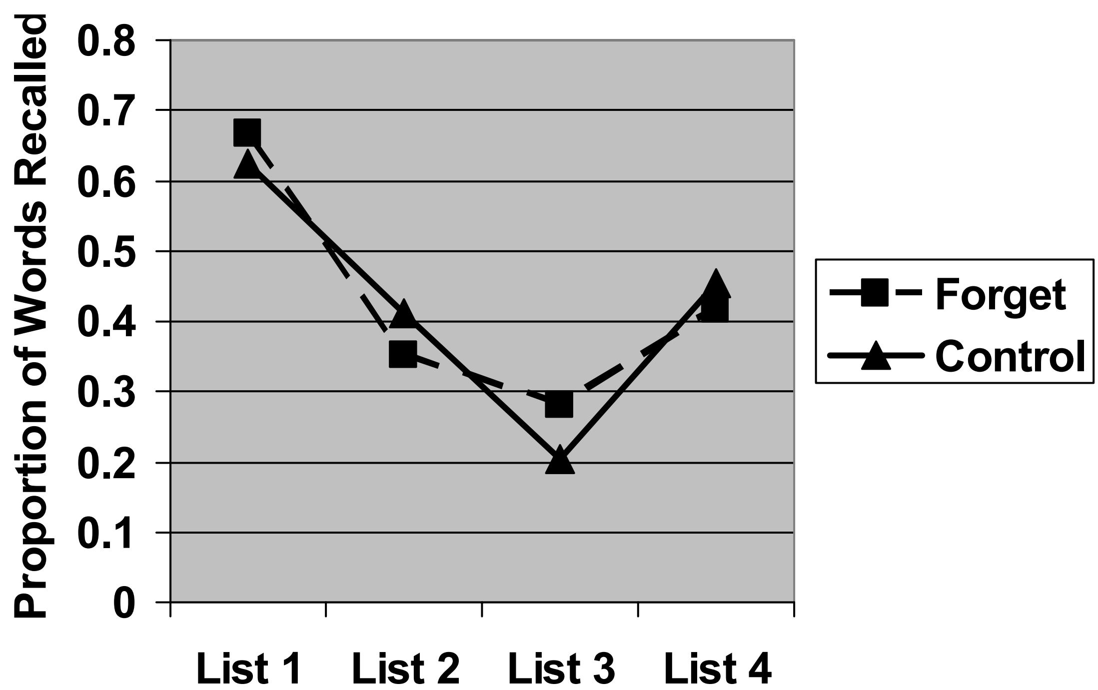

Figure 3. Proportion of words recalled on final recall measure from each list in forget and control groups from Experiment 2.

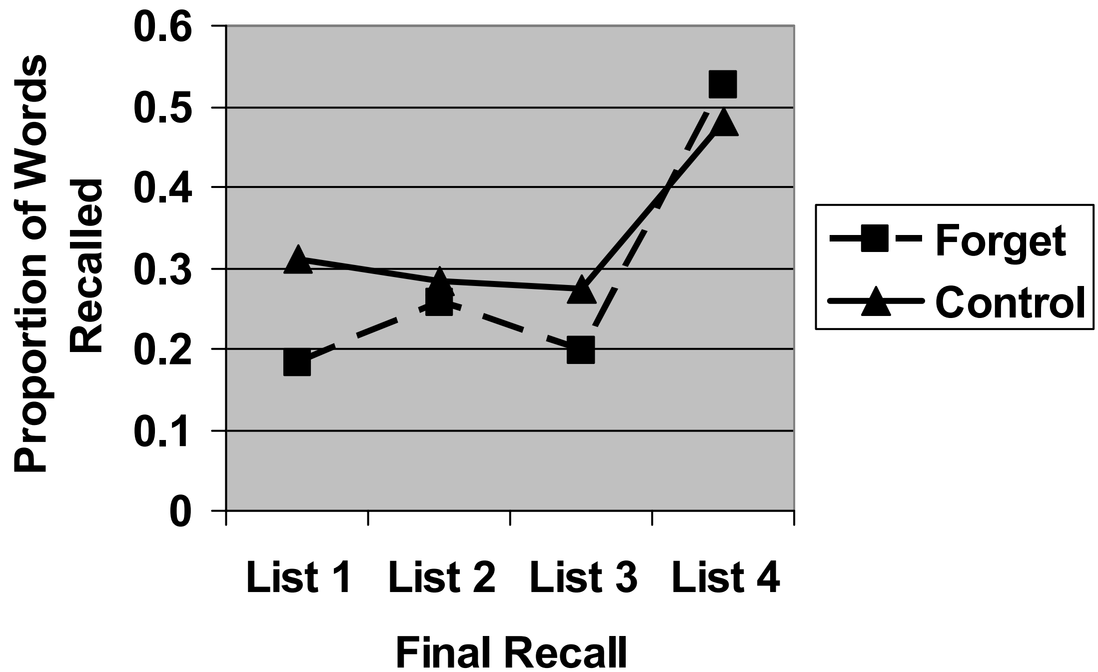

Figure 4. Comparison of proportion of words recalled on final recall measure of immediate test to no immediate test.

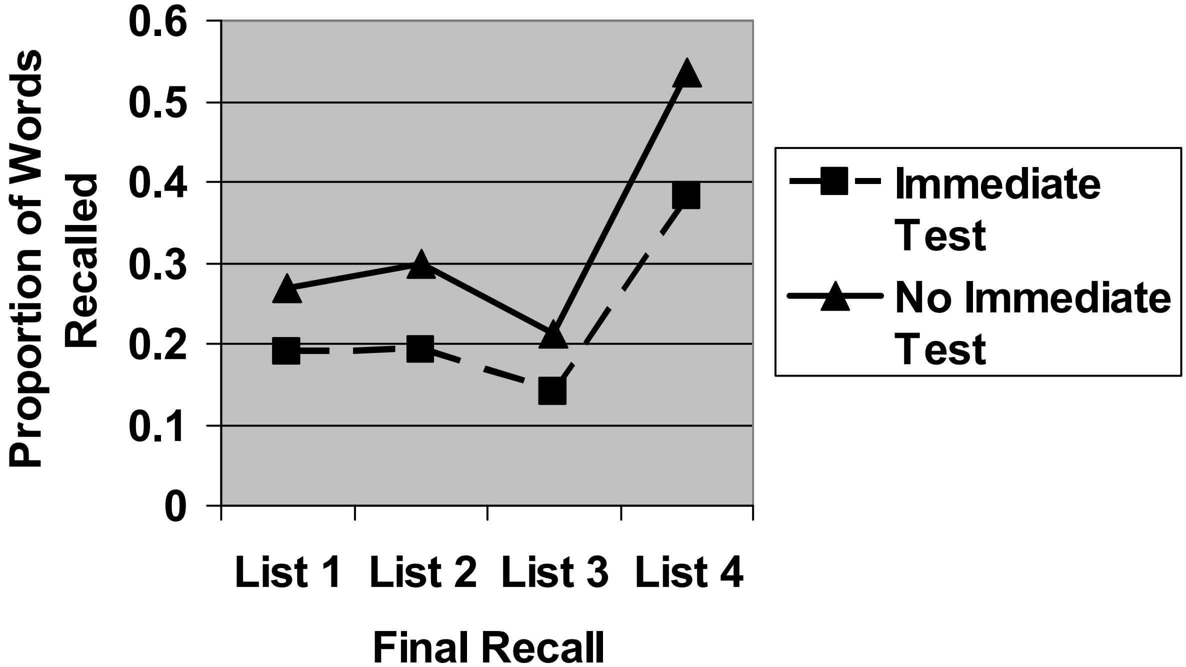

Figure 5. Proportion of words immediately recalled from each list in forget and control groups from Experiment 3.

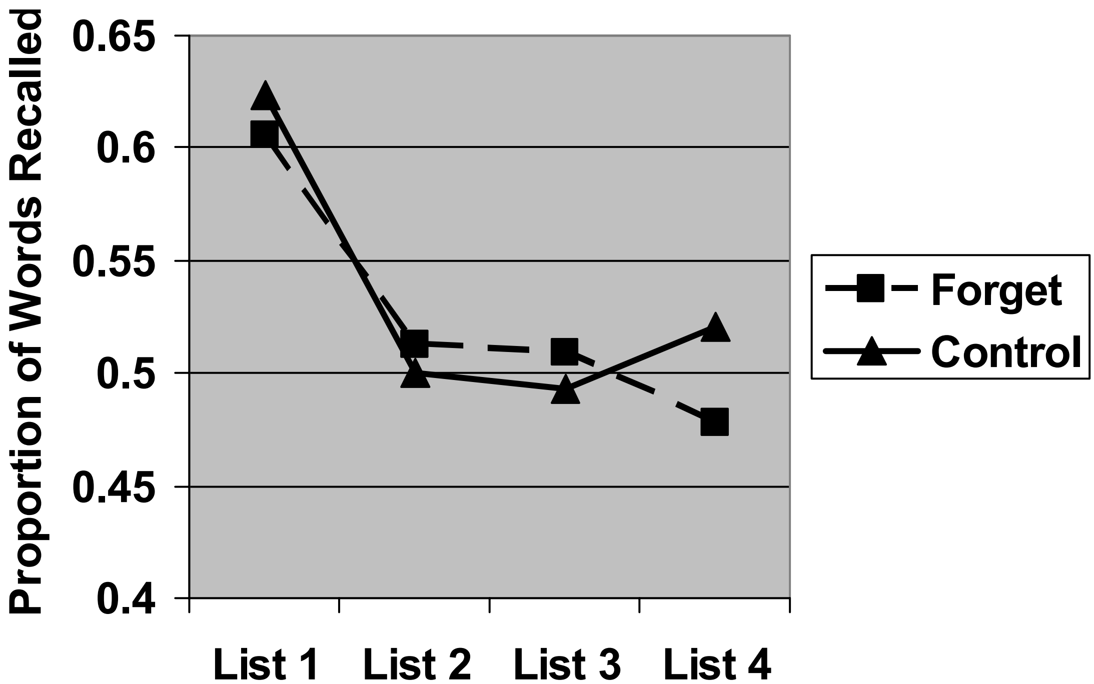

Figure 6. List 1 words recalled on cumulative immediate recall test after List 2.

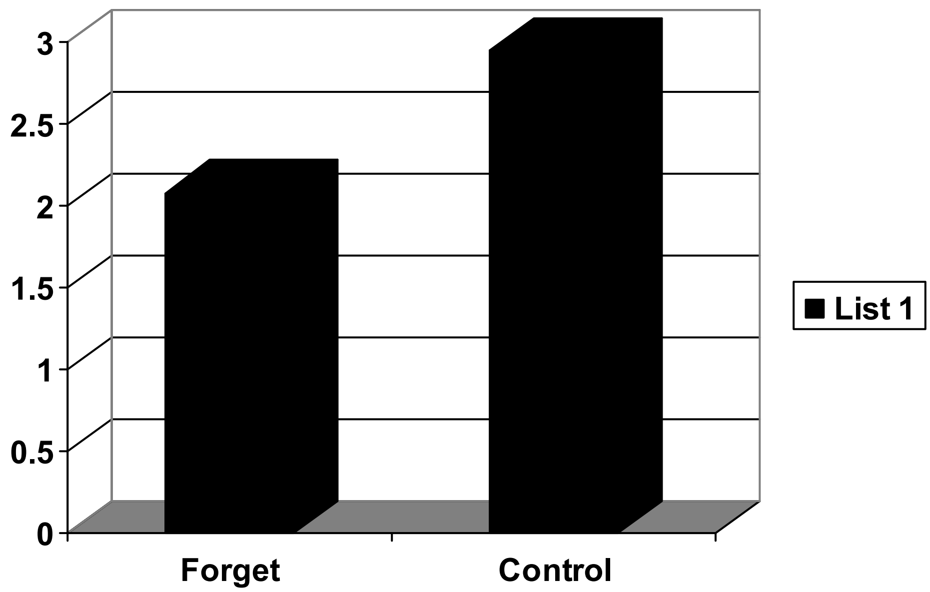

Figure 7. List 1 and List 2 words recalled on cumulative immediate recall test after List 3.

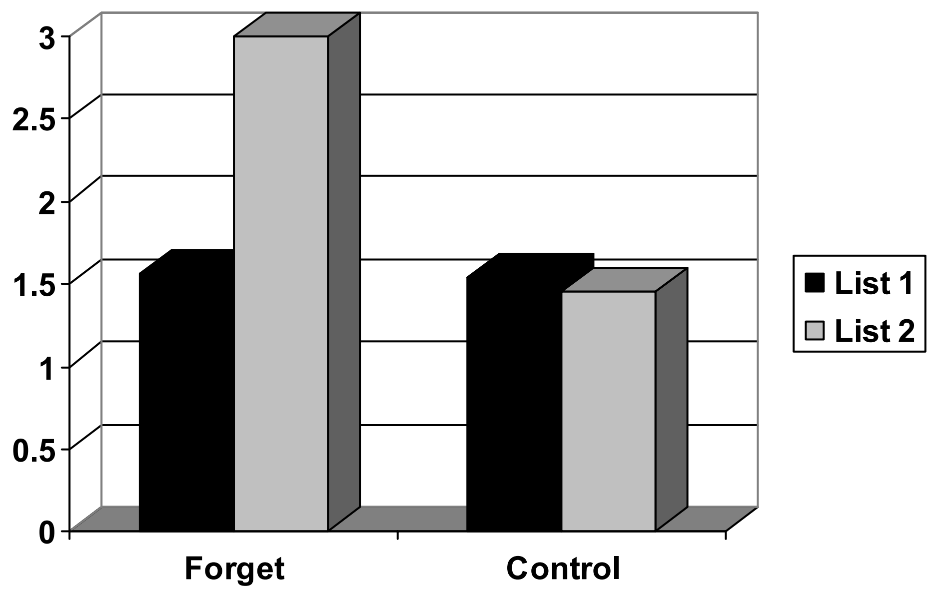

Figure 8. List 1, List 2, and List 3 words recalled on cumulative immediate recall test after List 4.

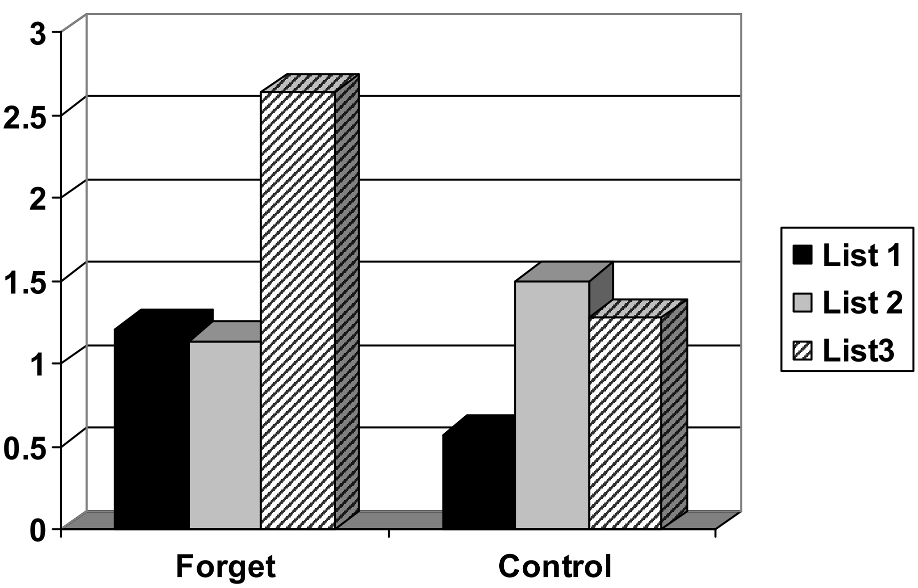

Figure 9. Proportion of words recalled on final recall measure from each list in forget and control groups from Experiment 3.

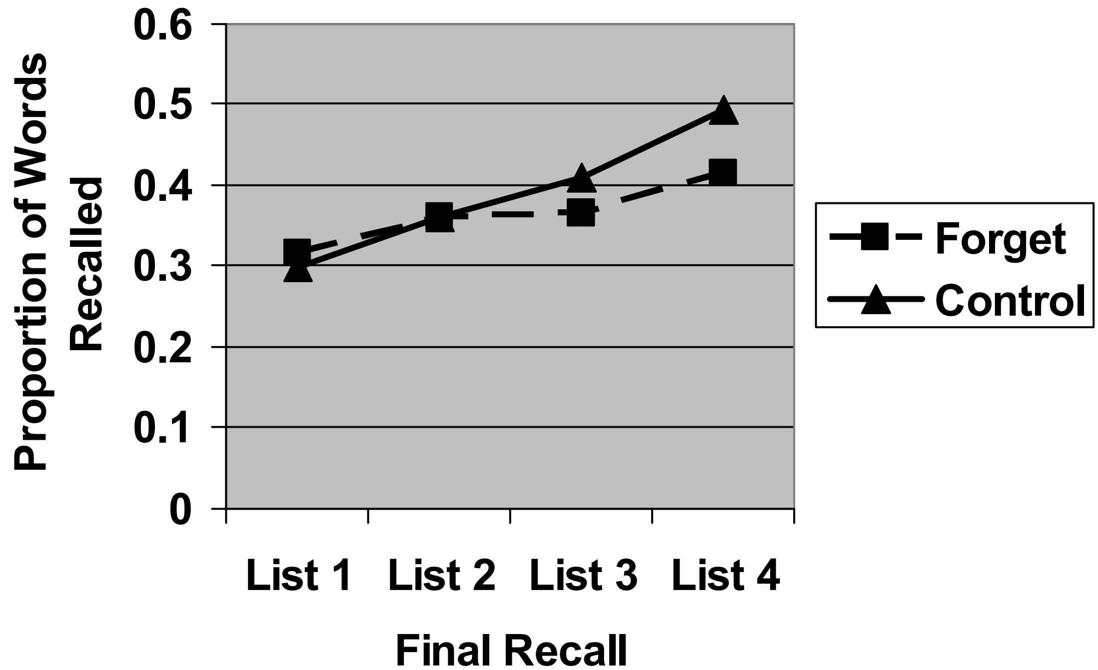

Figure 10. Proportion of PI build-up for immediate recall tests from Experiment 1, Experiment 2, and Experiment 3.