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The current study investigated the claim that in list-method directed forgetting, List 2 must be as long as List 1 in order to obtain directed forgetting (Pastötter & Bäuml, 2010). Participants studied two lists of words for a later memory test, and were instructed to forget or remember List 1 following its presentation. The length of List 1 was fixed (18 items), whereas the length of List 2 varied across conditions. In the *equivalent* condition, the objective number of items on List 2 was equivalent to the number of items on List 1. In other conditions, the objective number of items on List 2 was fewer than on List 1 (nine items), but the items on List 2 were repeated within the list so that the total number of trials was identical across the two lists. In the *spaced* condition, List 2 items were repeated in a spaced fashion, with a lag of eight items between the repetitions, whereas in the *massed* condition, List 2 items were repeated back to back with a lag of zero. Finally, in the *short* condition, List 2 was half the length of List 1 items (nine items), and the items were presented only once. The *spaced* group did not show directed forgetting impairment, whereas the *massed* and the *equivalent* groups showed impairment of identical magnitude. The *short* group showed numerical impairment, and the magnitude of the effect was similar to the *massed* and the *equivalent* groups, although the results were trending toward significance. The results are discussed in terms of the theoretical mechanisms underlying directed forgetting.

THE EFFECT OF LIST TWO LENGTH ON CONTEXT INFORMATION IN LIST-METHOD DIRECTED FORGETTING

by

Kendra Joi Gray

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Approved by

____
Committee Chair

APPROVAL PAGE

This thesis written by Kendra Joi Gray has been approved by the following committee of the Faculty of The Graduate School at the University of North Carolina at Greensboro.

Committee Chair ________________________________

Committee Members ________________________________

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Date of Acceptance by Committee

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Date of Final Oral Examination

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CHAPTER I

INTRODUCTION

Forgetting is often viewed in a negative light. Frequently, people have difficultly recalling important information at the precise moment they need it, suggesting that forgetting is a negative process. In contrast, Bjork (1989) has tried to make a case that forgetting serves an adaptive function by removing irrelevant or unnecessary information from our memory system. For instance, one may need to forget information from one class in order to effectively learn competing information from a similar class. This act involves intentional forgetting, where people engage in forgetting information that is no longer needed because it may interfere with another task. The current experiment will focus on the directed forgetting paradigm, one method for investigating intentional forgetting (Bjork, LaBerge, & Legrand, 1968).

Directed forgetting procedures involve asking participants to study one or more lists of items. Participants are instructed to forget or to remember certain portions of those lists. In the item-method version of directed forgetting, each item in the list is given a forget or a remember cue following its presentation (e.g., Bjork, LaBerge, & Legrand, 1968). Typical findings with this procedure show that participants have a worse memory for the to-be-forgotten (TBF) items than the to-be-remembered (TBR) items.

This effect emerges both in tests of free recall (Bjork, LaBerge, & Legrand, 1968) and in recognition (Elmes, Adams & Roediger, 1970).

In the list-method of directed forgetting, participants are given an entire list to study and then receive a cue to remember or to forget that list. Then all participants receive the second list, which they are always told to remember for a later test. When memory is unexpectedly tested for both lists at the end, the typical findings show that the forget group displays impaired recall of the first list and enhanced recall of the second list compared to the remember group. These are known as the *costs* and the *benefits* of directed forgetting, respectively (e.g., MacLeod, 1988).

In contrast to the item-method directed forgetting, which emerges in tests of free recall and recognition, the list-method directed forgetting is usually not evident in recognition tests (Elmes, Adams & Roediger, 1970). However, recognition conditions that encourage the retrieval of contextual details have recently reported List 1 costs (e.g., Sahakyan, Waldum, Benjamin, & Bickett, 2009), and likewise long lists were shown to produce List 2 benefits (e.g., Sahakyan et al., 2009; Sahakyan & Delaney, 2005; Benjamin, 2006).

The current experiment will focus on the list-method directed forgetting procedure. A recent study using this method suggests that there are conditions that limit whether one observes directed forgetting in the list-method. The length of the competing list was recently cited as a determining factor (e.g., Pastötter and Bäuml, 2007; Pastötter & Bäuml, 2010). Before discussing the methods of the proposed study, I will describe the theoretical mechanisms that explain list method directed forgetting.

Theoretical Mechanisms of Directed Forgetting

Historically, four explanations for the list-method directed forgetting effect have been proposed: the selective rehearsal account, retrieval inhibition account, dual-factor account, and contextual account. The selective rehearsal account proposes that after participants hear the forget instruction, they stop rehearsing the items on List 1 and devote all their rehearsal to List 2 (Bjork, 1970). The participants, who are told to remember both lists, continue rehearsing List 1 during List 2. This is why List 1 is forgotten, and List 2 is better remembered for the participants in the forget condition when compared to the remember condition. Although selective rehearsal was initially a popular explanation, it later had difficulty explaining why participants still showed directed forgetting under incidental learning instructions. Specifically, Geiselman, Bjork, and Fishman (1983) provided evidence against selective rehearsal. Participants were given a list of items to be judged for pleasantness intermixed with items to be learned for a later test. After the first list, participants were told to forget the items they had been asked to learn thus far, because they would not be tested on them. Then they saw the second list, which again contained a mixture of incidentally encoded and intentionally learned items. Afterwards, participants were asked to recall all items, including the ones they had judged for pleasantness. Results showed that both incidentally learned and intentionally learned items showed the same magnitude of directed forgetting costs and benefits. In other words, the items that participants rated for pleasantness, without intending to memorize also showed directed forgetting. Therefore, it was unclear why the incidentally encoded items also showed directed forgetting if the mechanism producing directed forgetting involved selective rehearsal. Because of these findings, and also the lack of directed forgetting in recognition tests, the retrieval inhibition account was proposed. The retrieval inhibition account proposes that the forget cue initiates an inhibitory process that blocks or inhibits access to List 1 items at the time of retrieval (Geiselman et al., 1983; Bjork, 1989). This explains why both types of items that were intermixed on List 1 showed forgetting; they both suffered from retrieval inhibition. Later work also confirmed the absence of directed forgetting in incidental learning, using pure lists rather than mixed lists of intentionally and incidentally encoded items (Sahakyan & Delaney, 2005).

Sahakyan and Kelley (2002) provided another account of directed forgetting that recognizes the importance of episodic context in memory. When an item is stored in memory, it is stored along with contextual information (Gillund & Shiffrin, 1984). Context refers to the information about the environment, the mood, the time, and other background aspects that are experienced as an item is being encoded. Context is important in free recall, because in the absence of other search cues, context cues are the first cues to assist in the search for the desired item in memory (Raaijmakers & Shiffrin, 1981). The storage of context with an item plays a big role in retrieving that item. For example, recall is better if the environment in which items are encoded matches the environment in which the items are retrieved (e.g., Godden & Baddeley, 1975; Smith & Vela, 2000). Not only is the match of the retrieval context and study context critical in retrieving the items, but also items with more context information stored in their memory trace are more likely to be recalled than items that have less context information associated with them (Malmberg & Shiffrin, 2005). This is because items associated with more context information will have a sampling advantage and will be retrieved at the expense of items with less context information.

Recognizing the importance of contextual cues in retrieval of items, Sahakyan and Kelley (2002) proposed a context account of directed forgetting. According to this account, participants may attempt to intentionally forget information by changing their own mental context by thinking about something else unrelated to the experiment in response to the forget cue. When participants are asked to forget the items on List 1, they cease maintenance of the context from List 1 and begin to focus on List 2 context. Because the retrieval context no longer matches List 1 and matches List 2 at recall, the costs of directed forgetting can be observed. The benefits are observed, because the change of context between the lists reduces interference between the two lists. Hence, List 2 items avoid proactive interference in the forget group.

Recently, dual-factor accounts have been proposed that explain the costs and the benefits of directed forgetting by relying on two separate mechanisms (Sahakyan & Delaney, 2005; Pastötter and Bäuml, 2010). The dual-factor accounts emerged because the costs and benefits were not always found under the same manipulations. According to Sahakyan and Delaney's (2005) version of the dual-factor account, the costs are due to mental context change between List 1 and List 2; where the benefits are due to a strategy change between List 1 and List 2 (Sahakyan & Delaney 2003). Specifically, if participants are given a forget cue, they think about their encoding strategy for List 1 and may decide that it was not beneficial. Then they change their strategy for List 2 to a deeper encoding strategy and remember List 2 better than those who did not receive a forget cue (Sahakyan & Delaney, 2005). Thus, a combination of context-change and strategy change accounts explain the costs and the benefits of directed forgetting under the dual factor account. Another version of the dual-factor account uses a combination of retrieval inhibition to explain the costs, and a reset of encoding processes on List 2 to explain the benefits (Pastötter and Bäuml, 2010). According to this version, the benefits arise because participants in the forget group can more effectively rehearse List 2 items due to the inhibition of List 1 items. Pastötter and Bäuml (2010) proposed that following the forget cue, the encoding processes for List 2 items are reset. They suggested this explanation mainly because the benefits were obtained in the first few items of the serial position curve of List 2 in the forget group. In other words, there was an enhanced primacy for List 2 items. These authors argued that if the benefits were driven by a strategy change, as suggested by Sahakyan and Delaney (2003; 2005), then those benefits should have been evident throughout the entire serial position curve; whereas, the recent studies analyzing serial position curves, showed that the benefits were limited only to the first few items (e.g., Lehman & Malmberg, 2009; Sahakyan & Foster, 2009; Pastötter and Bäuml, 2010). Given the findings with serial position analyses, Pastötter and Bäuml (2010) argued that List 2 benefits arise from a reset of encoding processes in the forget group.

Conditions that Limit Directed Forgetting

One of the conditions that affects whether or not directed forgetting can be obtained is the presence or absence of List 2 (Pastötter and Bäuml, 2007; Gelfand &

Bjork, 1985). For example, Pastötter and Bäuml (2007) had participants encode List 1, and subsequently instructed them to either forget that list, to remember that list, or to think of something else in order to change their mental context. Then half of their participants were shown List 2, whereas the remaining participants did not receive List 2, and instead were given a filler task to complete. Pastötter and Bäuml (2007) demonstrated that when List 2 was absent, directed forgetting was not obtained. They also showed that changing the participants' mental context caused the participants to forget List 1, much like the forget instruction, but this only occurred when List 2 was present. They argued that without List 2, there was no need to inhibit List 1 items, because there was no competition from List 2 items that needed to be overcome by inhibition. Therefore, in the absence of List 2, no directed forgetting was obtained. Note that a context account also made the same prediction regarding the outcome of directed forgetting in the absence of List 2. If there was no List 2, the retrieval context during the test matched the encoding context of List 1 items very well, and therefore, one would not expect to obtain directed forgetting. Even if participants changed their mental context by thinking of other things after List 1, it might have been easier to reinstate List 1 context during the test in the absence of List 2. Therefore, directed forgetting would not be observed.

More critical to the current proposal is a recent extension of this boundary condition investigated by Pastötter and Bäuml (2010). These authors showed that not only is the presence or absence of List 2 a critical factor in directed forgetting, but also that the length of List 2 matters. Namely, their findings showed that when there are fewer items in List 2 than List 1, directed forgetting is diminished compared to when there are a comparable number of items on List 2 and List 1. Thus, Pastötter and Bäuml (2010) have argued that having equal items on List 1 and List 2 is necessary to obtain the costs of directed forgetting, whereas with fewer items on List 2 there is insufficient inhibition of List 1 items. Hence, directed forgetting is diminished or not observed. In other words, these authors are suggesting that the items on the lists constitute a source of competition, and when List 2 is not as long as List 1, there is not enough competition between the items to trigger inhibition of List 1.

Current Study

The current study sought to examine whether competition takes place at the level of individual items or at the level of contextual information stored in the memory trace of those items. The study also attempted to determine if List 2 must have an equivalent number of items compared to List 1 in order to obtain directed forgetting, or whether the amount of contextual information stored across the two lists was more critical regardless of the number of items.

To address this question, the current study incorporated theoretical ideas proposed by Malmberg and Shiffrin (2005), who distinguished between item-strength and context-strength. They had participants memorize and recall three lists of items, where items were strengthened using various manipulations known to enhance memory. Two of the lists were pure lists, containing only strong items or only weak items, whereas the third list was a mixed list containing a mixture of strong and weak items. In one of their experiments, items were strengthened using spaced repetitions, which involved repeating the items with intervening items between. In the second experiment, items were strengthened via extra study time. Finally, the third experiment involved depth of processing manipulation. Although all three experiments revealed that strong items were generally better remembered than weak items, the pattern of results varied depending on whether the lists were pure or mixed. Specifically, the pure spaced list was better remembered than the pure massed list. However, on the mixed list, this effect was magnified such that recall of spaced items improved, and recall of massed items declined compared to the pure lists. This pattern is known as *list strength effect* or LSE (Shiffrin, Ratcliff, & Clark, 1990). Malmberg and Shiffrin (2005) obtained LSE only when they employed spaced presentations; however, they did *not* observe LSE when they strengthened items using extra study time or levels of processing. Because the LSE is attributed to contextual competition at the time of test (Shiffrin, Ratcliff, & Clark, 1990), Malmberg and Shiffrin (2005) used the presence of LSE in the spacing condition to assert that each time a word is presented spaced apart, additional contextual information is stored in the memory trace of that item, which gives that item a sampling advantage over items with less context during the time of test.

The current study tested the proposal of Pastötter and Bäuml (2010) regarding the importance of List 2 length in directed forgetting by relying on the assumption that contextual strength is incremented with additional spaced presentations of items. Specifically, List 1 was kept constant, while the length of List 2 was varied with items being repeating in spaced or massed fashion. The theories of directed forgetting yield different predictions regarding the size of the directed forgetting effect under varying conditions of List 2 length.

CHAPTER II

METHOD

Participants

Participants were 192 undergraduate students from the University of North Carolina at Greensboro. Participants were recruited from introductory psychology classes and were tested individually. They participated for course credit. There were 48 participants in each of the four conditions, with 24 participants in the Forget and 24 in the Remember group of each condition.

Materials

There were thirty-six unrelated words used to construct Set A and Set B. The words of the two sets were rotated across the list position such that Set A and Set B served equally often as List 1 and List 2. List 1 items were always randomized during the presentation. One half of Set A items and one half of Set B items served as List 2 equally as often. List 1 items were always randomized during the presentation, whereas List 2 items had a fixed presentation in order to control the lag in the spaced condition.

Procedure

Participants were instructed to rate each word for pleasantness on a scale from one to five. They were instructed to read each word and voice their rating verbally into a microphone. Participants believed the microphone, recording their answers, to be functional. Following Pastötter and Bäuml (2010), the words were presented at a rate of 4 s, with a 1 s lag between items. All participants studied List 1, which always contained 18 items. Afterwards, half of the participants received an error screen, stating that there had been a “Sound Input Failure.” Those participants were then informed that the experimenter had made a mistake, and that the list had not been recorded. All participants agreed to rate another list and were asked to forget about the words and ratings from List 1, so those items did not interfere with List 2. The remaining half was told that there would be one more list to rate. Then, participants were shown List 2, the length and format of which were varied in each of the four different conditions. In the “*equivalent*” group, List 2 consisted of 18 items, making the length of List 1 and List 2 equivalent both in terms of the number of items and also in terms of the number of presentations. In the “*spaced*” group, List 2 consisted of 9 items presented twice, in a spaced fashion, with a fixed lag of 8 items between the first and second presentation. The “*massed*” group studied 9 items, presented twice back-to-back. Thus, both the *spaced* group and the *massed* group had an equivalent number of presentations across both lists (e.g., 18), but a different number of items across the two lists (e.g., 18 vs. 9). Finally, in the “*short*” condition, List 2 consisted of only 9 items. To equate the time until the test, after the presentation of 9 items on List 2, participants performed a filler task, involving math problems for the remainder of the list study time. Thus, the short condition had both fewer items and fewer presentations on List 2 than List 1.

After participants studied List 2, they were given a filler task, consisting of math problems, which lasted for 30 seconds. They were then asked to write as many words as they could remember from List 1 for 90 seconds, even if they were originally asked to forget that list. Finally, they were asked to flip the paper and recall List 2 for 90 seconds, as well.

The data was scored by counting the number of items recalled for either List 1 or List 2. Even if a List 1 item was recalled during the List 2 recall period, it was considered correctly recalled.

Design

The experiment involved a *Cue* (forget vs. remember) x *List 2 Format* (equivalent vs. spaced vs. massed vs. short) x *Study List* (List 1 vs. List 2) mixed factorial design, where Study List was manipulated within-subjects, and Cue and List 2 Format were manipulated between subjects.

CHAPTER III

ANALYSES & PREDICTIONS

The main predictions involve how the structure of List 2 affects the magnitude of directed forgetting. Various theories make different predictions for List 1 and List 2 findings, and they are discussed separately for each account.

Selective Rehearsal Account

Predictions from the perspective of the selective rehearsal account are not applicable, because participants were not attempting to study the words. They were only instructed to rate them.

Context-change Account

The predictions from the perspective of the context-change account are depicted in Figure 1. If directed forgetting is driven by the amount of contextual information stored along with items and the ease with which the context may be reinstated at the time of test (e.g., Sahakyan & Kelley, 2002; Lehman & Malmberg, 2008), then according to the context-change account, the *equivalent* condition and the *spaced* condition should show similar amounts of directed forgetting costs. This is because while the objective number of items between the *spaced* and *equivalent* conditions will vary, the amount of contextual information stored with each item should theoretically be similar across the conditions. Hence, both conditions should show similar magnitude of directed forgetting impairment. This prediction is based on the assumption that the number of items across the lists is irrelevant, and the contextual information stored with each item is more important. Likewise, the *short* and *massed* conditions should show similar amounts of directed forgetting impairment, because both conditions will have equivalent “shots” of context stored with each item. Importantly, the *massed* and the *short* conditions should show smaller directed forgetting costs than the *equivalent* and *spaced* conditions. This is because there will be fewer “shots” of context across List 2 than List 1 in the *massed* and *short* conditions than in the *equivalent* and *spaced* conditions, and it may be easier to reinstate the original context at the time of test. If the magnitude of the directed forgetting impairment varies between the *equivalent* and *spaced* conditions, or between the *massed* and the *short* conditions, it would imply that item information also plays a critical role in directed forgetting and needs to be considered along with contextual information.

According to the context-change account, the benefits of directed forgetting occur because of reduced proactive interference between the lists, which arises as a consequence of encoding the lists with different contextual cues. Therefore, given that the magnitude of costs was predicted to be the same across the *equivalent* and the *spaced* conditions, the magnitude of the directed forgetting benefits should also be the same for these conditions. Likewise, the magnitude of the benefits for the *massed* and the *short* conditions will also be similar, because those groups should have a similar degree of impairment on List 1. Finally, the number of words recalled from List 2 should be sensitive to List 2 Format, such that the *spaced* items will be better recalled than *massed* items, which in turn will be better recalled than items in the *short* condition.

Inhibitory Account

The predictions from the inhibitory point of view are depicted in Figure 2. One of the more unambiguous predictions of the inhibitory account is that the *short* condition should show reduced or null directed forgetting (both costs and the benefits) compared to the *equivalent* group. This is the finding obtained by Pastötter and Bäuml (2010). Since List 2 is not long enough to compete with List 1 items, there will be less need to invoke inhibition in order to reduce competition. It is less clear what the inhibitory account would predict for the *massed* and *spaced* conditions and how those would compare to the *equivalent* and *short* conditions. In its strict form, the inhibitory account does not predict differences in the magnitude of directed forgetting between *massed* and *spaced* conditions, because the number of items between the two conditions is equivalent and all that is varied is the manner in which they are repeated. Since Pastötter and Bäuml (2010) emphasized the number of items across the two lists, the *spaced* and *massed* conditions should show the same degree of directed forgetting. Furthermore, the size of the effect may be equivalent to the *short* condition, which also has the same number of items on List 2. However, if the degree of List 2's overall strength is critical to how much inhibition is needed, the *spaced/massed* conditions should have a stronger List 2 than List 1 compared to the *short* condition; hence, there may be more need to evoke inhibition in the *spaced/massed* condition compared to the *short* condition. In other words, the size of the directed forgetting in the *spaced*/*massed* group may be greater than in the *short* group, while still being smaller than in the *equivalent* group. Finally, it is possible that repeating an item back-to-back may not create as much competition as repeating the items spaced apart. Therefore, the size of the directed forgetting may be greater in the *spaced* condition than in the *massed* condition.

Dual-Factor Accounts

All of the accounts discussed thus far have been single-process accounts, which predict that any differences in the magnitude of costs across the conditions should be reflected in the similar differences across the conditions in the benefits. However, the dual-factor accounts do not necessarily tie the magnitude of costs to the magnitude of benefits, because they rely on different processes to explain the costs and the benefits. Since both the strategy-change account and the reset of encoding processes account explain the benefits of directed forgetting by relying on encoding differences between the forget and remember groups, they make similar predictions regarding List 2 benefits. Because participants were not attempting to encode List 1 or List 2, benefits should not be observed.

CHAPTER IV

RESULTS

List 1 recall

Figure 3 depicts the recall results from List 1. A Cue (forget vs. remember) by List Format (*equivalent* vs. *spaced* vs. *massed* vs. *short*) ANOVA was conducted on List 1 recall. A main effect of cue, $F(1,184)=7.90$, $MSE=.104$, $p=.005$, revealed that participants in the *remember* group ($M=.30$, $SD=.10$) had better List 1 recall than participants in the *forget* group (M=.25, SD=.13). There was also a main effect of List Format, $F(3,184)=5.50$, $MSE=.072$, $p<.001$, indicating that the manner in which List 2 items were studied affected List 1 recall. The interaction between Cue and List Format was not significant, $F<1$.

A post-hoc, one-way ANOVA was conducted on List Format to determine how the List 2 composition influenced List 1 recall. This analysis was significant, $F(3,184)=5.33$, $MSE=.072$, $p=.002$. The difference between the *equivalent* group ($M=.26$, $SD=.10$) and the *spaced* group ($M=.24$, $SD=.13$) was not significant, ($p=.39$). There was also no difference in List 1 recall between the *equivalent* group and the *massed* group ($M=.27$, $SD=.12$), $p=.43$. Similarly, the difference between the *spaced* group and the *massed* group was not significant, $p=.10$. Finally, the difference between List 1 recall for the *short* group ($M=.33$, $SD=.12$) was significantly greater compared to the other three List Format groups, (*Equivalent* ($p=.004$), *Spaced* ($p=.001$), *Massed* (*p*=.032)). The results of the one-way ANOVA suggest that when people studied a short List 2, a greater proportion of List 1 was recalled, compared to when List 2 was longer, massed, or spaced.

Although the Cue by List 2 Format interaction was not significant, I analyzed the costs of directed forgetting separately for each List 2 Format group, using independent samples t-tests. The *equivalent* group showed an effect of cue, $t(23)=2.07$, $p=.045$, with the remember group recalling more List 1 items ($M=.29$, $SD=.11$) than the forget group ($M=.23$, $SD=.08$). The *massed* group also showed an effect of cue, $t(23)= 2.140$, $p=.038$. Again, the remember group recalled more List 1 items ($M=.31$, $SD=.13$) than the forget group ($M=.24$, $SD=.10$), indicating significant costs. However the *spaced* group did not show an effect of cue, $t<1$ (in the remember group, $M=.24$, $SD=.14$; in the forget group $M=.23$, $SD=.12$). There was a numerical effect in the *short* group, with the remember group recalling more ($M=.35$, $SD=.12$) than the forget group ($M=.30$, $SD=.11$); however, the effects did not reach conventional significance, $t(23)= 1.540$, $p=.13$. Overall, the *equivalent* and the *massed* groups showed the costs of directed forgetting while the *spaced* and the *short* groups did not.

List 2 recall

Figure 4 depicts the recall results from List 2. A 2x4 ANOVA, with cue and List 2 format as factors, showed no main effect of cue, $F(1,184)=1.24$, $MSE=.032$, $p=.26$. The ANOVA also showed a main effect of List 2 Format , $F(3,184)=23.325$, $MSE=.587$, $p<.001$, suggesting that at least one of the List 2 Formats yielded a List 2 recall that was different from one of the other groups. There was no interaction between the two variables, $F<1$.

It is possible that the significant effect of the List 2 Format on costs was not obtained, because the spacing manipulation did not induce a typical “spacing effect.” To test for the spacing effect, List 2 recall was analyzed as a function of List 2 Format. I conducted a one-way ANOVA on List 2 recall. This analysis revealed a spacing effect for List 2 recall, $F(3,184)=23.48$, $MSE=.587$, $p=.000$. The *spaced* group recalled a greater proportion of List 2 ($M=.62$, $SD=.16$) compared to the *equivalent* group ($M=.36$, $SD=.12$), $p=.000$, the *massed* group ($M=.49$, $SD=.17$), $p=.001$, and the *short* group ($M=.43$, $SD=.18$), $p=.000$. These results are consistent with the spacing effect, because the *spaced* group should have more context information associated with List 2 than the other groups and therefore, better recall of List 2 (eg., Malmberg and Shiffrin, 2005; Shiffrin, Ratcliff, & Clark, 1990). The *equivalent* group also had significantly poorer recall of List 2 than the *massed* (p=.009) and *short* groups ($p=.033$). The *massed* group and the *short* group did not have significantly different levels of recall for List 2, $p=.07$.

CHAPTER V

DISCUSSION

The results obtained in this study did not match all of the predicted results. It was predicted that the *equivalent* group and the *spaced* group would produce a similar magnitude of costs and that the *massed* group and the *short* group would produce a similar magnitude of costs. It was also predicted that the *short* and *massed* groups would show diminished costs compared to the *equivalent* and *spaced* groups. This result did not emerge. Instead, the only groups that showed costs were the *equivalent* group and the *massed* group. The *short* and *spaced* conditions showed no costs. This result is inconsistent with any of the previous explanations of list-method directed forgetting.

The results of the present experiment are consistent with some of the results from Pastötter and Bäuml (2010). Pastötter and Bäuml (2010) showed that when the number of words on List 2 is equivalent to the number of words on List 1, the costs of directed forgetting are obtained, and when the number of words on List 2 is fewer than the number of words on List 1, the costs of directed forgetting are fewer or are not obtained. This may explain why the *short* group showed no costs while the *equivalent* group showed costs. If the amount of context information sampled in List 2 does not affect List 1 recall, the results from Pastötter and Bäuml (2010) may also explain why the *spaced* group showed no costs. However, their results are not consistent with the costs obtained in the *massed* group because the number of items on List 2 in the *massed* group was fewer than on List 1. Yet the *massed* group showed directed forgetting impairment.

Another explanation as to why the *spaced* condition showed no costs may have something to do with this experiment's procedure. For *spaced* items, participants made two pleasantness judgments with 8 intervening items. According to Malmberg and Shiffrin (2005), at the second presentation of the item, a second shot of context information is sampled. Perhaps this did not occur in the current experiment. Perhaps the second presentation of each item brought back the old context of the first presentation, because participants had to recall if they had rated this item initially and how they had rated it. This may have given the item a sampling advantage in List 2 but may not have facilitated enough additional context storage to cause List 1 costs. This may explain why the *spaced* group showed no costs. It is also possible that List 2 in the *spaced* condition was so strong (compared to List 1) that participants could not overcome the sampling advantage of List 2 items in order to access List 1 items.

Perhaps the *massed* group showed unexpected costs, because the List Length manipulation was not severe enough. Pastötter and Bäuml (2010) used 15 items for List 1 and varied the number of items on List 2 to 15, 8, 3, or 0 words. The condition in which there were 8 items on List 2 can be likened to the conditions in the current experiment in which there were only 9 items on List 2. In other words, these are conditions in which List 1 contains approximately twice as many items as List 2. While Pastötter and Bäuml (2010) observed costs in the 8 item condition, the participants who studied 0 or 3 items on List 2 did not show costs. Based on this, it is possible that the list length manipulation in the current study was not extreme enough to elicit the expected outcome.

The lack of support for the predictions in the current study may cause one to wonder if the manipulations in List 2 had no effect on recall. However, it appears that multiple effects did emerge. Participants, who saw spaced items, remembered significantly more words on List 2 than those in the *massed* group. In other words, I obtained the traditional spacing effect similar to Malmberg and Shiffrin (2005), and others (although it was noted that perhaps it was not so traditional). A list length effect was also obtained. Participants in the *short* group remembered more List 2 items than participants in the *equivalent* group, who had more items to remember. Finally, there was an effect of multiple presentations on recall. The participants in the *spaced* group remembered more words than participants in the *equivalent* and *short* groups. Also, the participants in the *massed* group remembered more words than participants in the *equivalent* group, and marginally more words than participants in the *short* group. Therefore, it would seem that the list length manipulation had the predicted effect on List 2 recall, but did not affect the magnitude of directed forgetting costs.

A future experiment could be performed to discover if a smaller List 2 length might clarify these results. Perhaps if the *short* condition contained only 3 items and the *spaced* and *massed* conditions contained only 3 items, repeated 6 times, the results from Pastötter and Bäuml (2010) might have been replicated. Perhaps a *short* condition with 6 items on List 2, and *spaced* and *massed* conditions with 6 items, repeated 3 times, would show clearer results.

Because the *spaced* group did not show List 1 costs in the current experiment, it is possible that context sampling during List 2 study is not necessary to show the costs of directed forgetting. Perhaps context change between List 1 and List 2 is necessary, and extra context sampling is not necessary to obtain costs. It could be that List 2 item information is more important than List 2 context information for costs. It is also possible that the traditional spacing effect must be obtained with the participants being more aware of the new context in which the second presentation of each item occurs. It is also possible that a more severe manipulation of List 2 length is necessary to show that List 1 cannot be forgotten without List 2.

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APPENDIX A

FIGURES

Figure 1

<u>Predictions from the Context-change Account.</u>

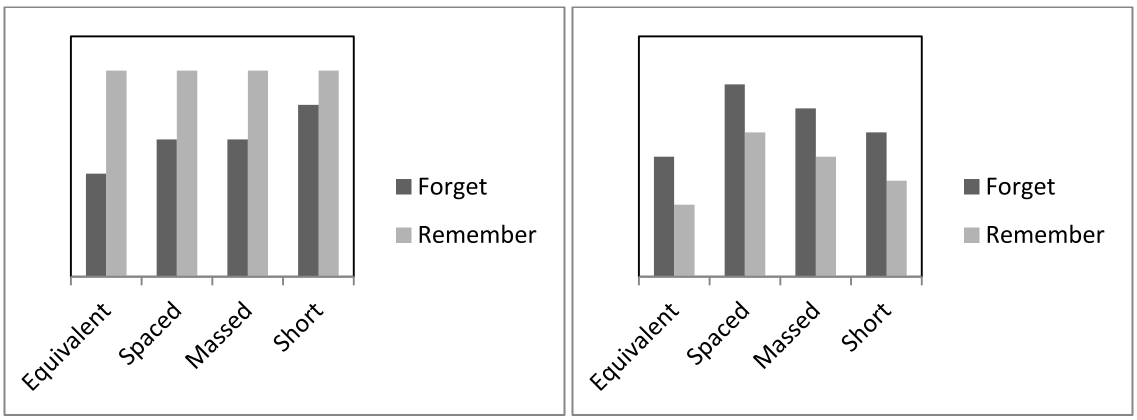

Figure 2

<u>Predictions from the Inhibitory Account.</u>

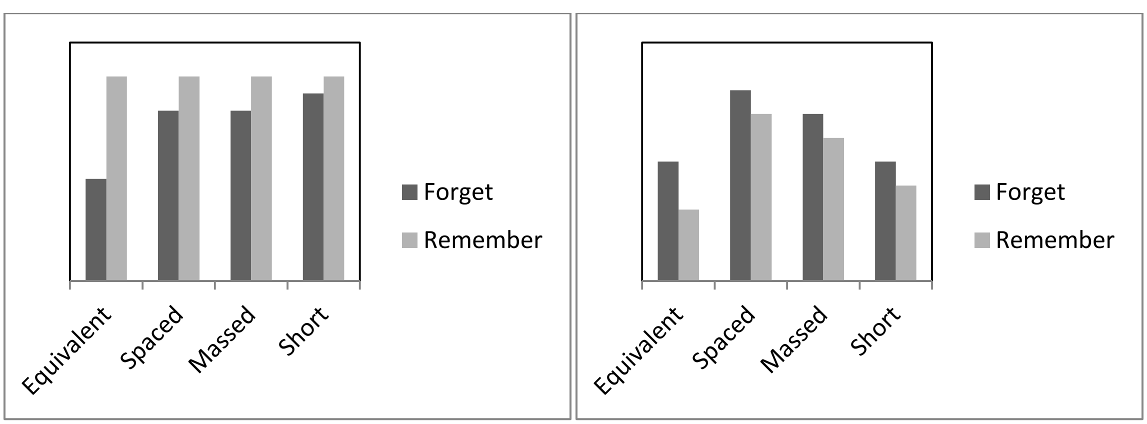

Figure 3

Mean proportion recalled as a function of List 2 length and cue in List 1.

Figure 4

Mean proportion recalled as a function of List 2 length and cue in List 2.