Directed forgetting of autobiographical events

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Two diary experiments demonstrated directed forgetting (DF) of autobiographical events, previously observed only for less complex memory items. Using a 2-week diary paradigm, we compared recall between a group of participants who were directed to forget Week 1 memories (forget group) and a group who did not receive a forget instruction (remember group). In Experiment 1, the forget group remembered fewer items from Week 1 than did the remember group. The effect was observed for negative and positive valence events, as well as for high and low emotional intensity events. The effect was replicated in Experiment 2 despite a memorable holiday (Valentine's Day) that occurred during the manipulation week. Forget participants remembered fewer low emotional intensity items in Experiment 2. We conclude that intentional forgetting is a plausible explanation for the loss of some autobiographical memories.

Can a person deliberately forget an autobiographical event? Intentional forgetting has often been used to explain the loss of autobiographical memories, especially memories for unpleasant or disturbing events (e.g., McNally, Clancy, & Schacter, 2001). In fact, Freud's notion of repression, now regarded as an unconscious process, originally involved deliberate thought avoidance (Breuer & Freud, 1893-1895/1955). Moreover, there is a long history of empirical evidence for deliberate forgetting of some kinds of memories (for reviews, see Bjork, 1998; Golding & Long, 1998; Golding & MacLeod, 1998; Johnson, 1994; MacLeod, 1998). Directed forgetting (DF), or intentional forgetting, has been found with a wide range of stimuli, including trigrams (Weiner, 1968; Weiner & Reed, 1969), digits (Brown, 1954), word pairs (Reitman, Malin, Bjork, & Higman, 1973), categorized words (MacLeod, 1975), pictures (Basden & Basden, 1996), sentences (Geiselman, 1974), and even navigation instructions (Golding & Keenan, 1985). However, whether it is reasonable to generalize from such stimuli to autobiographical events is debatable. The work presented here was undertaken to directly address that issue.

We will begin with some background. In DF experiments, there are two basic methods for delivering the forget cue: the item method and the list method. In the item method, participants are instructed to either forget or remember each item immediately after it has been presented. Our study was modeled on the alternative list method. In the typical list method experiment, participants are presented with a list of memory items, one at a time. Midway through the presentation of the list, some

of the participants (the forget group) are told to forget the previous items (F-items). There is usually a cover story that accompanies this instruction—for instance, that first-half items were merely practice or that the experimenter made a mistake. The rest of the participants are given an inconsequential instruction midway through the list (the remember group) and, hence, attempt to remember first-half items (R-items) for the duration of the experiment. For both groups, items presented after the midlist cue (second-half items) are to be remembered for the upcoming test. Finally, all the participants are tested on the entire list.

Two effects of DF have been observed with the list method. First, more second-half items are recalled by forget than by remember participants. Increased recall of second-half items in the forget condition is usually thought to be a function of reduced proactive interference by previously encoded information. The memory load for forget participants is half that for those in the remember group, who must maintain both list halves. The second effect observed with DF is the reduction in recall for F-items. When participants are asked to recall all items, fewer F-items are recalled by the forget group than comparable items recalled by the remember group. In other words, people who are told to forget a subset of memory items apparently do. These two effects have been referred to as the DF *benefit* and *cost*, respectively (Sahakyan & Delaney, 2003).

The explanation for DF has been the subject of considerable debate over the years (for a complete discussion and review, see Basden, Basden, & Gargano, 1993; Johnson, 1994; MacLeod, 1998). The least interesting explanation is that the DF effect is due to demand characteristics. Participants might intentionally withhold F-items at test, either because they misunderstood that the instructions had changed (first they were instructed to forget and then to remember the F-items) or because they guessed the hypothesis and were trying to help.

We thank Katie Postlewait, Sarah Schroeder, and Kristine Chang for their diligent work in coding both experiments. We also thank Barbara Basden, Lili Sahakyan, and an anonymous reviewer for their helpful comments. Correspondence concerning this article should be addressed to S. L. Joslyn, Department of Psychology, University of Washington, Box 351525, Seattle, WA 98195-1525 (e-mail: susanj@u.washington.edu).

However, there is evidence that is inconsistent with this explanation. To begin with, it is unlikely that the typical reduction of proactive interference for second-half items would be observed in the forget condition if F-items were equally available. Moreover, offering monetary rewards fails to increase recall of F-items (MacLeod, 1999).

After a period of debate, two leading explanations for DF effects have emerged. The first is that, as compared with R-items, F-items are rehearsed less often, less well established in memory, and as a result, less well remembered at test. This explanation has generally been applied to the DF cost observed in item method paradigms (Allen & Vokey, 1998; Basden et al., 1993). Because participants know that they will get instructions to forget or remember items immediately after each one, they initially maintain the items in a temporary state and only rehearse those followed by the remember cue. Hence, R-items are rehearsed, but F-items are not.

Although differential rehearsal is a sensible explanation for the DF cost observed in item method paradigms, it makes less sense in list method paradigms. When the forget cue is issued only once, midway through list presentation, participants must regard the first half as comprising memory items for a substantial period of time. In other words, both R- and F-items are likely rehearsed. Moreover, improving rehearsal does not eliminate DF costs when the list method is used. DF costs are observed regardless of whether elaborative or shallow rehearsal is employed (Sahakyan & Delaney, 2003), suggesting that the difference in memory accuracy has some other cause. In addition, when the list method is used, DF effects extend to incidental memory items, which participants have no reason to rehearse (Geiselman, Bjork, & Fishman, 1983). Reduced recall for all items presented before the forget cue, including incidental items, suggests that the entire episodic segment is less available. This gives rise to an alternative explanation for DF effects in the list cue paradigm. The explanation is that R- and F-items are encoded and established in memory equally well. However, the entire first half of the list is suppressed or inhibited when the forget cue is given midway through presentation. Consequently, first-half items are less accessible when retrieval is attempted during the later memory test.

Until recently, there has been a general consensus for the theoretical dichotomy just described: Differential rehearsal best explains DF effects in item method paradigms, and inhibition/suppression best explains DF effects in list method paradigms. However, new evidence has begun to reopen the theoretical debate, especially with respect to the list cue paradigm. Some new evidence has suggested an impact of differential rehearsal with the list method (MacLeod, Dodd, Sheard, Wilson, & Bibi, 2003). In addition, Sahakyan and colleagues (Sahakyan & Delaney, 2003; Sahakyan & Kelley, 2002) have suggested that DF effects are due to something else entirely: a change in mental context induced by the presentation of the forget cue midway through the list. According to this explanation, reduced alignment of mental context

between first-half items and test induces DF costs. DF benefits are thought to arise from a shift to more efficacious memory strategies following the change in context in the forget condition.

The work presented here is concerned primarily with issues of generalizability, although it does raise issues related to the theoretical explanation. As was previously mentioned, research to date has focused on DF with fairly simple memory items, such as individual words on lists. However, the ultimate aim of this and similar research is to gain a better understanding of forgetting in complex real-life memories. We pose the question, "Is directed forgetting a plausible explanation for forgotten autobiographical memories?" It is reasonable to question whether lab-based word list evidence supports generalizing to autobiographical memories. Life events, due to their complexity, contextual relevance, coherence, integration, and salience, may be resilient to deliberate efforts to forget them. Autobiographical memories may be especially resilient, because they are personally experienced—hence, rich in sensory and emotional detail—and because they are self-relevant.

Nonetheless, we obviously forget many autobiographical events. Although some events result in treasured memories that become the backbone of our autobiographical structure (Conway, 2000; Shum, 1998), most events rarely come to mind again. Clearly, there are multiple reasons for forgetting life events, including inadequate encoding and retrieval failure (Shum, 1998). In addition, we and others (Anderson & Green, 2001; Barnier, Hung, & Conway, 2004) believe that deliberate, effortful processes play a role in targeting some memories to be remembered and others to be forgotten. Indeed, this type of explanation has been provided for the forgetting of even traumatic memories (Anderson & Green, 2001; Epstein & Bottoms, 2002; Koutstaal & Schacter, 1997; Spinhoven, Nijenhuis, & Van Dyck, 1999).

Thus, although autobiographical memories may be generally more memorable than words on a list, instructions to forget a subset of autobiographical memories may reduce recall, as compared with autobiographical memories for which no such instructions are given. In other words, DF effects may hold for autobiographical memories when the hypothesis is tested systematically, as it has been for word lists and other memory items. To date, there has been no direct test of the impact of DF on autobiographical memory.

The two experiments reported here provide that direct test. The general experimental paradigm of the two experiments was similar. In the context of a 2-week diary study, we compared the recall of a group of participants who were directed to forget Week 1 memories with the recall of a remember group. We expected that forget participants would remember fewer Week 1 events. Because autobiographical memories are complex and the components may be affected differently by the direction to forget, we examined memory for the gist, as well as memory for the details and the emotionality, of the event. We thought that the forget cue might reduce memory accu-

racy for vulnerable aspects of remembered events, such as details and emotional valence. Previous work suggests that peripheral details are more likely to be forgotten than are central features (Barclay & Wellman, 1986; Christianson, 1992; Linton, 1986; Neisser, 1981). Likewise, some evidence suggests that there are inaccuracies in memory for emotions related to autobiographical events (Barclay & Wellman, 1986; Levine, 1997; Thomas & Diener, 1990). Evidence also suggests that emotional valence and intensity influence the recall of autobiographical memories (Barnier et al., 2004; Brewer, 1988; Byrne, Hyman, & Scott, 2001; Christianson, 1992; Linton, 1986; Wagenaar, 1986). For these reasons, we examined such factors in light of the expected group difference in memory for Week 1 events. In addition, we examined Week 2 recall to see whether the forget group would recall more second-half items, as is the case with word lists.

Finally, we tested the impact of DF on incidental items. Both the forget and the remember groups recorded items, purportedly exempt from testing, during both weeks. Any memory for *test-exempt* items was considered incidental. Assuming that neither group applied memory strategies to them, we ruled out differential rehearsal as the explanation for any DF effects observed with incidental memories. Furthermore, decreased memory of Week 1 events, both incidental and intentional memory items, by the forget group would suggest that the entire episodic segment prior to the forget cues had been suppressed.

GENERAL METHOD

Materials

The participants received two identical diary packets, one at the beginning of Week 1 and one at the beginning of Week 2. Each diary packet comprised 10 blank forms. Each form was designed to record a single event and was marked with the day and date of the event (a copy is available at <http://faculty.washington.edu/susanj/>). For each 5-day period, there were two forms per day, for a total of 20 forms. Half of the forms were pink, designated as practice, and half were white. Each diary form had six sections and was similar to the forms used by Barclay and colleagues (Barclay & Subramaniam, 1987; Barclay & Wellman, 1986).

At the top of each form, the participants were instructed to write a narrative description of one unique and memorable event that had occurred that day. The instructions explained that the diary entry should include a self-selected event, the context for that event, and the emotional reaction to the event. In order to increase the likelihood that the target was an individual episodic memory, the participants were advised to avoid selecting events that occurred frequently or events that were a part of their everyday routine (e.g., lab class), because they might be difficult to recall later. For the most part, the participants complied with this suggestion, recording events that were unique. For example, 1 participant wrote the following: "A few friends and I were walking through campus when we suddenly saw a crow running around on the ground following a squirrel. It was so funny! We stood and watched them for a few minutes, exchanging funny squirrel stories and other animal stories."

The participants were then asked to reduce the event to a two-word summary. The two-word phrase was intended to capture the central features or gist of the event, such as *crow chase*. Several fill-in-the-blank questions requested specific details, including the location and time of day, as well as the other people and objects that were involved in the event. There were nine 7-point Likert-type

scales requiring mood and intensity ratings for each event (Csikszentmihalyi & Larson, 1984; Csikszentmihalyi, Larson, & Prescott, 1977; Larson & Csikszentmihalyi, 1983).¹

Design

The forget cue was manipulated between participants, using the list method. The cue was given following the 1st week of the 2-week recording period. The dependent measure was recall amount. There was one between-groups independent variable, memory instruction (two levels: forget cue vs. no forget cue), and one within-group independent variable, intention to learn (two levels: incidental vs. intentional).

Procedure

Phase 1: Instructions. Researchers met groups of 3–12 participants in a classroom to provide written instructions and the diary packets. Following informed consent procedures, the participants learned that they would record 10 events per week for 2 weeks in the packets provided. They were told that they would be assigned to one of two experiments running in parallel. One experiment focused on memory, so they might be asked to recall the recorded events in a later phase of the experiment. The other experiment was a cross-cultural comparison of the number of self- and group-oriented autobiographical events recorded among Japanese and American students. No memory test would be involved in the second experiment. However, the participants would not be assigned to a particular experiment until the following week. So, for the 1st week, the participants were to assume that they were in the memory experiment and to remember the events. In fact, the cross-cultural experiment was a cover story to justify the forget instructions in Phase 2.

Then the researchers distributed the Week 1 diary packets. Within the packet, one of the two forms for each day was pink, and the other was white. The participants were to record a test-exempt item on the pink form. The cover story for the test-exempt item was different for each experiment. In Experiment 1, the participants were told that it was for practice, ostensibly to familiarize them with the format. In Experiment 2, they were told that it was for a separate experiment. The event recorded on the white form was the potential memory item (intentional item). The participants were instructed to avoid differentiating between the pink and the white forms when initially selecting the event but to remember only the events recorded on the white forms. To avoid potential within-day order effects, the order of the pink and white forms was randomized. Sometimes, the first form for the day was pink; sometimes, the first form was white.

To control exposure time to the original record, the participants were told to place each form in a manila envelope as soon as it was completed. They were instructed not to reread or edit the record in any way after it had been finished. Results from the posttest manipulation check administered in Experiment 2 suggested that the participants had complied with this instruction. The experimenters answered questions and reminded the participants to return the next week with a completed diary packet.

Phase 2: Forget cue manipulation. One week later, the participants returned and submitted their Week 1 diary packets. Then approximately half the participants were told that Week 1 events would not be part of the memory study (although Week 2 events recorded on the white pages would be tested). They were further informed that an effort should be made to forget the Week 1 events, since this would facilitate Week 2 event memory. The remaining participants were informed that they would participate in the memory study. They would eventually be asked to remember all of the events recorded on the white forms from both the first and the second weeks. Finally, all the participants were given the Week 2 diary packets and cautioned not to talk to anyone about the study, to avoid contaminating their memories for the events.

Phase 3: Recall test. One week later, the participants returned again and exchanged their completed Week 2 diary packets for a

test packet. The test packet comprised 20 blank diary forms, with the dates of the previous 2 weeks' data collection days. The forms were arranged in chronological order. Half of the forms were pink, and half were white. As before, the order was randomized. The experimenters apologized to the participants for previously deceiving them and asked them to recall all the events recorded during both Week 1 and Week 2, including those designated as practice events (pink forms). The participants were to complete the diary in a manner as closely as possible to the original, attempting to transcribe the recalled event on the appropriate form. The participants had 55 min in Experiment 1 and 90 min in Experiment 2 to fill out the forms. The retention interval was at least 2 days but was as long as 12 days for some events.

Data Coding

Three independent coders, blind to the condition, compared the diary forms with the test forms to classify events as *recalled*, *not recalled*, or *unsure*. Classification was based on the two-word summary, intended to capture the *gist* of the event. If the two-word summary written at test was identical to the original summary, it was coded as *recalled*. Coders also accepted slight differences, including tense mismatches, quantity mismatches, synonyms (*grocery store* and *market*), category–category exemplars (e.g., *grocery store* and *Safeway*), and differences in the number of words. An exact date match was not required for an event to be classified as *recalled*. If coders were unsure whether a diary form matched a test form, they classified the event as *unsure*. All the remaining events were classified as *not recalled*. A similar procedure was used to classify each of the details reported for the recalled events, including date, time, place, and other people or objects. Agreement between the dates on the test and on the original diary form indicated recall of the date.

EXPERIMENT 1

Method

Participants. The participants were 42 undergraduates from the University of Washington. They were recruited on a voluntary basis and received course credit in exchange for participation in the experiment. Of the initial 42 participants, attrition claimed 11 who did not return for all three phases of the experiment. An informal survey of the University of Washington psychology faculty determined that this is a typical level of attrition for a multisession, multiweek

experiment among such populations. The data from 6 additional participants was excluded due to failure to complete at least one half of the diary entries for either week. The final sample numbered 25 participants. There were 18 females and 7 males, with a mean age of 20. Ages ranged from 17 to 26 years. There were 12 participants in the remember and 13 in the forget groups.

Data coding. Of the possible 500 events (25 participants with 20 events each), 12 events (2.4% of the original data set) were eliminated because the emotional scales were not completed. At least two coders agreed on 485 events (99.4%) of the 488 coded events. All three coders agreed on 445 of the events (91.2%). Excluded from the analyses were the three instances (0.6%) in which all three coders assigned different memory designations and the 28 events coded as *unsure* (5.7%). The final data set consisted of 457 events. The numbers of participants in the following analyses vary slightly. A few participants were eliminated from specific analyses because they did not record memories that fell into one of the categories under investigation. However, in each of the analyses reported, no more than 1 participant was eliminated from a single group.

Results

Week 1 and Week 2 events were analyzed separately because the predictions about recall in the experimental group depended on the week.

Impact of forget cue and intention to learn on memory for gist. Recall was operationalized as the proportion of the two-word event summaries (gists) that were recalled by each participant at test. On the basis of the DF list-learning literature, we predicted that the participants given the forget cue would recall fewer Week 1 events than would those in the remember group. In addition, we predicted that the participants would remember fewer incidental than intentional events. Only the first hypothesis was supported. A 2 (memory instruction: forget vs. remember) $\times$ 2 (intention to learn: practice vs. test) mixed-model analysis of variance (ANOVA) yielded a main effect for memory instruction [$F(1,23) = 10.48$, $MS_e = 0.07$, $p = .004$]. As Figure 1 illustrates, the participants in the forget group recalled a smaller proportion of events on average ($M = .31$, $SD = .21$) than did those in

Figure 1. Mean recall proportions for Week 1 and Week 2 as a function of memory instruction and intention to learn (Experiment 1). Error bars are standard errors of the means. $N = 25$.

the remember group ($M = .55$, $SD = .32$). Surprisingly, the main effect of intention to learn did not reach significance [$F(1,23) = 1.92$, $MS_e = 0.07$, $p = .18$]. Slightly larger proportions of events designated as test items ($M = .48$, $SD = .29$) were remembered than those designated as practice items ($M = .38$, $SD = .29$), but the difference was not significant. It must be noted, however, that the estimated retrospective power of the comparison (see Cohen, 1988) was low (two-tailed, $\alpha = .05$, $1 - \beta \approx .23$). The interaction of memory instruction and memory intention did not reach significance [$F(1,23) = 1.97$, $MS_e = 0.07$, $p = .17$].

For Week 2, we hypothesized that the participants given the forget cue would recall more events than would those in the remember group. Again, we hypothesized that the participants would remember smaller proportions of incidental than of intentional events. Neither hypothesis was supported. A 2 (memory instruction: forget vs. remember) $\times$ 2 (intention to learn: practice vs. test) mixed-model ANOVA performed on gist for memory for Week 2 events yielded no significant main effects or interactions (all $F_s < 2.28$).

Emotional rating at recording. We hypothesized that memories would be differentially susceptible to the forget cue, depending on emotional rating. Perhaps events rated as more positive or more intense would be more memorable and would resist the forget cue. Because the hypothesis concerned the DF cost, Week 1 events alone were analyzed. We aggregated the emotional ratings to create a single mood and a single intensity rating for each participant. We identified the median rating for all Week 1 events by collapsing across participants and defining high and low categories for each measure. Then, for each participant, we calculated the proportion of events recalled in each category. Finally, we conducted a pair of 2 (memory instruction: forget vs. remember) $\times$ 2 (median: high vs. low) mixed-model ANOVAs, one for mood and one for intensity. The ANOVA for mood yielded only a main effect of memory instruction [$F(1,22) = 12.23$, $MS_e = 0.068$, $p = .002$]. The participants in the forget group remembered a reliably smaller proportion of events than did those in the remember group, regardless of whether mood was positive or negative. For intensity as well, there was only a main effect of memory instruction [$F(1,22) = 17.25$, $MS_e = 0.06$, $p < .001$]. The participants in the forget group remembered a reliably smaller proportion of events than did those in the remember group, regardless of intensity.

Impact of forget cue on details. We hypothesized that the forget group would remember fewer details (date, time, place, and other people or objects involved) of the Week 1 events for which the gist was recalled than would the remember group. Memory for details was high overall, both in this experiment and in Experiment 2. There were no group differences in either experiment, except for form color in Experiment 1. Color was used to distinguish practice events, recorded on pink forms, from memory events, recorded on white forms. The remember

group correctly classified events by color at test significantly more often than would be expected by chance [$.5$; $M = .68$, $SD = .27$; $t(11) = 2.30$, $p = .04$]. However, the forget group did not [$M = .56$, $SD = .32$; $t(11) = 0.62$, $p = .55$].

Discussion

We found that people are capable of intentionally forgetting autobiographical memories, just as they forget words on memory lists. This was true regardless of whether the events were rated as positive or negative in mood and regardless of emotional intensity. The participants instructed to forget diary entries remembered fewer than a third of the events recorded during the 1st week of the experiment, whereas the rest of the participants remembered over half of them. Forget participants remembered 24% fewer events from the 1st week, on average, than did remember participants. This proportion is comparable in size to that found previously in list-learning studies (e.g., Basden et al., 1993). Although both the forget and the remember groups were forewarned that they might not be tested on Week 1 material, the successful recall of Week 1 events by the remember group suggests that encoding was unimpaired by the warning. Because both groups were subjected to the identical procedure prior to the forget cue, lack of encoding is eliminated as a plausible explanation for the DF cost observed here.

We found no group differences in recall of Week 2 events, a result that will be discussed in the General Discussion section. In addition, there was no detectable impact of the forget cue on details of events for which the gist was recalled. Memory for details was generally high. Closer examination of the details recorded in this and the subsequent experiment suggests that they were often central to the event, rather than the peripheral details we intended. If most of the details selected were central, the fact that the gist was recalled may have almost guaranteed memory for those details by virtue of their close association to the gist.

EXPERIMENT 2

Experiment 2 was undertaken for two main purposes: (1) to further explore the interaction of the forget cue with the emotional content of autobiographical events and (2) to rule out misunderstanding test instructions as an explanation for the DF effect. One might argue that neutral events are relatively easy to forget. Perhaps the effect we observed in Experiment 1 is limited to such events. On the other hand, autobiographical events with enhanced emotional content might be more memorable in the first place (Thompson, Skowronski, Larsen, & Betz, 1996; Linton, 1986; Wagenaar, 1986), making them resistant to DF. Experiment 2 took advantage of the fact that Valentine's Day fell in the manipulation week. Valentine's Day could give rise to some emotionally enhanced events for college-age students. Thus, we were able to observe the impact of the forget cue on memories

likely enhanced in emotion. In addition, we wished to address a concern that arises in all DF paradigms. It could be argued that the forget participants in Experiment 1 misunderstood the instructions to recall all events at test and continued to adhere to the initial instructions to forget Week 1 events, leading to the observed reduction in recall. In an attempt to rule out this alternative explanation, a posttest questionnaire, or manipulation check, was administered asking the participants whether they attempted to remember or forget events recorded in Weeks 1 and 2. Finally, we increased the number of participants and improved the cover story for the incidental items, in an attempt to detect an effect for intention to learn.

EXPERIMENT 2

Method

Participants. The participants were 55 undergraduates from the University of Washington. They were recruited on a voluntary basis and received course credit. Of the initial 55 participants, attrition claimed 5 who did not return for all three phases of the experiment. The data from 3 additional participants were also excluded, due to failure to complete at least half of the diary entries for 1 week. The final sample numbered 47 participants (27 females and 20 males), with a mean age of 19 that ranged from 18 to 25. There were 22 participants in the forget group and 25 in the remember group.

Materials. We added the emotional ratings (mood and intensity), taken only at recording in Experiment 1, to the test form in Experiment 2. We wished to compare ratings on mood and emotional intensity made at recording with those made at test. Because the affective component of autobiographical memories has been shown to be vulnerable to distortion and changes in intensity (Barclay & Wellman, 1986; Levine, 1997; Thomas & Diener, 1990), we hypothesized that directions to forget might accelerate this process.

We also added a posttest manipulation check. After completing the test phase, the participants answered three yes–no questions, indicating whether they made an attempt to (1) forget Week 1 events, (2) remember Week 1 events, and (3) remember Week 2 events. When answering *yes*, the participants were asked to provide a short description of *why* and *how* they attempted to remember or forget. Finally, the participants rated their ability to remember and the amount of effort expended in remembering each week's events on a 7-point Likert scale. If the forget participants complied with the forget instruction, fewer should report making an attempt to remember Week 1 events, and more should report making an attempt to forget them. We expected to see no group differences for Week 2 events. If the forget participants understood that they were to recall all events at test, the amount of effort estimated should be approximately equal between groups.

Procedure. Some minor procedural changes were made. With respect to the cover story for the incidental items, the participants were told that an additional, concurrent study was being conducted exploring the linguistic aspects of autobiographical events. Thus, events entered on the pink forms would not be tested (incidental items) but were important to complete to allow for linguistic analysis. Finally, we increased the test phase from an hour to an hour and a half.

Data coding. Of the possible 940 events (47 participants with 20 events each), 8 events (0.9%) were eliminated because diary entries were not fully completed. Of the 932 coded events, at least two coders agreed on 897 events (96.2%). All three coders agreed on 643 of the events (69%). Excluded from the analyses reported below were the 43 instances (4.6%) in which all three coders assigned different memory designations, as well as those events coded as *unsure* (126 events; 13.5%). The final data set consisted of 763 events (81.1%). The numbers of participants in the following analyses vary slightly. A few participants were eliminated from specific analyses because

they did not record memories that fell into one of the categories under investigation. However, no more than 2 participants were eliminated from a group in each of the analyses reported below.

Results

Impact of forget cue on memory for gist and intention to learn. We predicted that the participants given the forget cue would recall fewer Week 1 events than would the remember group. In addition, because of the improved cover story and increased N , we predicted that the participants would remember fewer incidental than intentional events. As with Experiment 1, only the first hypothesis was supported. To determine the impact of the forget cue and the intention to learn on recall of the two-word summaries, we conducted a 2 (memory instruction: forget vs. remember) $\times$ 2 (intention to learn: incidental vs. intentional) mixed-model ANOVA. There was a main effect for instruction [$F(1,45) = 5.05$, $MS_e = 0.09$, $p = .03$]. The forget group recalled a smaller proportion of events on average ($M = .43$, $SD = .22$) than did the remember group ($M = .56$, $SD = .29$). As with Experiment 1, the main effect of intention to learn did not reach significance for Week 1 events [$F(1,45) = 2.73$, $MS_e = 0.05$, $p = .11$]. Only slightly larger proportions of events designated as test items ($M = .54$, $SD = .27$) were remembered than were incidental items ($M = .46$, $SD = .25$). Although the number of participants had been increased in Experiment 2, the estimated retrospective power of the comparison (Cohen, 1988) was still fairly low (two-tailed, $\alpha = .05$, $1 - \beta \approx .38$). The interaction of memory instruction and intention to learn also failed to reach significance [$F(1,45) = 0.38$, $MS_e = 0.05$, $p = .54$].

In an alternative approach, we combined Week 1 recall amounts observed in Experiments 1 and 2 (see Figure 2) in a meta-analysis. Combining the data was considered appropriate because the two experiments had the same dependent variable, similar methods, and similar standard deviations (see Greenwald, Oakes, & Hoffman, 2003). There were main effects for instruction [$F(1,70) = 13.67$, $MS_e = 0.08$, $p = .0004$] and intention to learn [$F(1,70) = 4.54$, $MS_e = 0.05$, $p = .04$]. The interaction again failed to reach significance [$F(1,70) = 1.93$, $MS_e = 0.05$, $p = .17$].

To determine whether the forget cue affected not only intentional but also incidental events, a simple effects analysis was conducted on the combined data from Experiments 1 and 2. The forget group ($M = .40$, $SD = .22$) remembered significantly fewer intentional events, on average, than did the remember group [$M = .63$, $SD = .29$; $t(70) = 3.77$, $p < .001$]. In addition, the forget group remembered significantly fewer incidental events ($M = .37$, $SD = .22$) than did the remember group [$M = .49$, $SD = .29$; $t(70) = 1.97$, $p = .05$]. It appears that the forget cue had a similar impact on both intentional and incidental events.

We examined gist recall and the impact of intention to learn on memory for Week 2 events in Experiment 2 as well. A 2 (memory instruction: forget vs. remember) $\times$ 2 (intention to learn: incidental vs. intentional) mixed-model ANOVA was performed on gist recall. There was

Figure 2. Mean recall proportions for Week 1 and Week 2 as a function of memory instruction and intentionality (meta-analysis, Experiments 1 and 2). Error bars are standard errors of the means. $N = 72$.

a main effect for intention to learn [$F(1,45) = 11.10$, $MS_e = 0.05$, $p = .002$]. Events designated as test items ($M = .72$, $SD = .26$) were recalled in larger proportions than were incidental items ($M = .57$, $SD = .28$). However, neither the main effect for memory instruction [$F(1,45) = 0.10$, $MS_e = 0.10$, $p = .75$] nor the interaction of memory instruction and intention to learn [$F(1,45) = 0.03$, $MS_e = 0.05$, $p = .87$] reached significance.

Emotional ratings. We hypothesized that memories might be differentially susceptible to the forget cue, depending on mood or intensity ratings made at recording. Since the hypothesis concerned the DF cost, only recall of Week 1 events was analyzed. Median split procedures were identical to those used in Experiment 1. We then conducted a pair of 2 (memory instruction: forget vs. remember) $\times$ 2 (median: high vs. low) mixed-model ANOVAs on the proportion of events recalled (gist), one for mood and

one for intensity (see Figure 3). The analysis for mood yielded a main effect of memory instruction [$F(1,44) = 4.64$, $MS_e = 0.09$, $p = .04$], as did the analysis for intensity [$F(1,43) = 5.00$, $MS_e = 0.10$, $p = .03$]. Those in the forget group remembered a smaller proportion of events overall. However, the second ANOVA also yielded a main effect for intensity [$F(1,43) = 6.89$, $MS_e = 0.07$, $p = .01$]. People remembered a larger proportion of events that were initially rated as highly intense. The interaction of instruction and intensity was marginally significant [$F(1,43) = 3.64$, $MS_e = 0.07$, $p = .06$] as well.

Simple effects analyses revealed that the forget group remembered a reliably smaller proportion of *low*-intensity items ($M = .27$, $SD = .24$) than did the remember group [$M = .53$, $SD = .32$; $t(43) = 3.00$, $p = .004$]. However, the difference between the proportion of *high*-intensity events recalled by the forget ($M = .52$, $SD = .32$) and the

Figure 3. Mean recall proportions for Week 1 by memory instruction and mood/intensity (Experiment 2). Error bars are standard errors of the means. $N = 46$.

remember ($M = .57$, $SD = .28$) participants was not statistically significant [$t(43) = 0.54$, $p = .59$]. We compared the mean mood rating of high-intensity events in Week 1 for Experiment 2 with that for Experiment 1. We discovered that it was more positive in Experiment 2 ($M = 5.46$, $SD = 0.81$) than it had been in Experiment 1 ($M = 4.45$, $SD = 0.98$), suggesting that there was a greater convergence of high intensity and positive mood in the events recorded for Experiment 2, perhaps due to Valentine's Day. Contrary to our expectations, we found no changes in mood and intensity ratings between recording and test. In other words, memory for mood and intensity ratings was accurate.

Impact of forget cue on details. As with Experiment 1, recall of details was high, and there were no group differences.

Manipulation check. We conducted a posttest manipulation check to confirm that the participants understood the instructions concerning the forget cue and the memory test instructions. Their answers suggest that they did. None of the 22 forget participants reported trying to remember Week 1 events, whereas approximately half (44%) of the remember group did. The difference between groups was reliable [$\chi^2(1) = 12.46$, $p = .0004$]. Moreover, over a quarter (27%) of the forget participants reported making an explicit attempt to forget Week 1 events, whereas none of the remember participants did. Again, the difference between groups was reliable [Yates corrected $\chi^2(1) = 5.56$, $p = .02$]. In the written explanation, most participants in the forget group who indicated that they attempted to forget Week 1 events reported doing so by "not thinking" about them or focusing on Week 2 events. We suspect that others in the forget group did something similar but did not consider it a strategy worth reporting. For Week 2, on the other hand, comparable proportions (48% vs. 50%) of each group reported trying to remember. Taken together, these data suggest that the forget cue manipulation was successful.

To determine whether the participants followed the instructions to attempt to recall events from both weeks at test, we examined their answers to questions pertaining to recall effort expended at test. Forget and remember groups reported expending similar effort in recalling both Week 1 and Week 2 events at test, suggesting that the forget group understood the change in instructions. Week 1 ratings of the participants in the forget group ($M = 5.14$, $SD = 1.32$) did not differ significantly from those of the participants in the remember group [$M = 5.64$, $SD = 1.15$; $t(45) = 1.40$, $p = .17$]. Similarly, there was no significant difference between the forget group ($M = 5.09$, $SD = 1.38$) and the remember group ($M = 5.08$, $SD = 1.08$) for Week 2 ratings [$t(45) = 0.03$, $p = .98$].

Discussion

Experiment 2 replicated the basic findings of Experiment 1, despite Valentine's Day, which occurred in the middle of the manipulation week, suggesting that the effect of the DF cost on autobiographical memories is fairly ro-

bust. Although the DF effect was somewhat smaller in Experiment 2 (14%) than it had been in Experiment 1 (24%), the proportions were not statistically different. This is important because Valentine's Day may have generated memorable events for college-age participants. Indeed, there was high recall for Valentine's Day for both groups. Moreover, Week 1 events in Experiment 2 had slightly higher mean mood and intensity ratings than did those reported in Experiment 1. Nonetheless, the participants who were instructed to forget Week 1 autobiographical events remembered a smaller proportion of them, overall, than did the participants in the remember group. Only among high emotional intensity events did the difference fail to reach significance.

We observed the typical effect of intention to learn in the meta-analysis, combining the two experiments. Moreover, simple effects analyses suggested that the forget cue had a significant effect on both incidental and intentional events, a result that fails to support a differential rehearsal explanation. If increased rehearsal by the remember group was responsible for the DF effect, a significant difference would be observed only among items that the participants had reason to rehearse—that is, those designated as test items. No DF effect would be observed for incidental items. However, because the forget participants *did* remember significantly fewer incidental items, the results do not support a differential rehearsal explanation.

Finally, the results of the manipulation check conducted in Experiment 2 are inconsistent with the alternative explanation that the forget participants remembered fewer Week 1 events simply because they did not try to remember them. The forget participants' responses suggest that they understood the change in instructions at test and exerted effort equal to that of the remember group in recalling Week 1 events. Thus, the decrement in the amount of Week 1 events recalled for the forget participants is likely due to a deliberate attempt to forget them prior to test.

GENERAL DISCUSSION

The work presented here demonstrates that people remember fewer autobiographical memories when instructed to forget them. The effect was seen a full week after the forget cue had been issued. This is the strongest support to date for the claim, previously based on list-learning studies and self-report, that deliberate forgetting explains the loss of at least some autobiographical memories (Anderson & Green, 2001; Epstein & Bottoms, 2002; Koutstaal & Schacter, 1997; Spinhoven et al., 1999). It is reasonable to assume that the effect observed here under experimental conditions, in which motivation to forget is induced externally by the experimenter and is minimal at best, generalizes to real-life situations in which much stronger internally generated motivations operate. People may decide that they do not want to remember certain autobiographical events because they are embarrassing

or painful, so they intentionally forget them. Although the DF effect is sometimes shown to dissipate over time in experimental situations (Liu, 2001), we believe that there are important differences in real-life settings that may lead to a more long-lasting effect. Not only is the motivation to forget likely stronger outside of the laboratory, but people are also likely to make repeated attempts to forget such memories over years and even decades. The target of this effort would be unrehearsed and possibly suppressed or inhibited, whereas its neighbors from that autobiographical period would continue to receive rehearsal and elaboration. Under these circumstances, it is conceivable that connections to the unwanted memory would weaken over time, turning a temporary lack of access into a permanent one (Anderson, Bjork, & Bjork, 1994; Barnier et al., 2004).

A plausible scenario for intentional forgetting is one in which the autobiographical event is particularly unpleasant to remember. Thus, the role of emotional content was important for us to investigate. We reported some intriguing results that bear on this issue. In Experiment 1, we demonstrated that the forget cue was equally effective regardless of whether the memory was rated as positive or negative and regardless of whether it was rated as high or low intensity. In Experiment 2, however, in which there was a memorable holiday and mean ratings for both mood and intensity were higher, high-intensity events appeared to be resilient to the forget cue, whereas low-intensity events were remembered in significantly smaller proportions by the participants who were told to forget them. Does this mean that intensely emotional events are immune to intentional forgetting? It is important to note that the impact here was predominantly on *positive* events. Some evidence suggests that positive autobiographical events are better recalled than negative autobiographical events in the first place (Bahrick, Hall, & Berger, 1996; Linton, 1986; Wagenaar, 1986). Perhaps negative events are more susceptible to a forget cue, counteracting any effect of intensity. Certainly, for most people, there is more motivation to forget negative than to forget positive events. Moreover, this was merely a 2-week experiment, in which forgetting was motivated by the desire to do well on an ultimately inconsequential memory test. With powerful motivation to forget, over the course of years, even intensely emotional memories may succumb to deliberate forgetting. Clearly, this is an issue that warrants more exploration.

What is the mechanism underlying DF of autobiographical memories in this paradigm? Our results shed some light on this question as well. The meta-analysis reported here revealed that the participants, in general, remembered significantly fewer incidental items, suggesting that they were distinguished from memory items and exempted from memory-enhancing strategies. Importantly, the forget group remembered significantly fewer *incidental* items than did the remember group. Assuming that these events were not rehearsed in the first place, we can rule out differential rehearsal as an explanation for the DF effect observed among incidental events. Perhaps Week 1 events were suppressed or inhibited when

the forget cue was issued. However, it must be noted that the participants did a poor job of identifying incidental items at test. Although this does not mean that the participants confused incidental with intentional items prior to test, we are unable to rule out the possibility. Some remember participants may have mistakenly rehearsed some incidental items, contributing to the observed difference.

We also reported and replicated several interesting null findings that require further exploration. We expected people to remember the emotional ratings and details associated with autobiographical memories less well than the gist (two-word summary). In addition, we hypothesized that the forget cue would accelerate this process. However, for remembered events, emotional ratings and details were recalled with high accuracy. Moreover, the forget group remembered these components as well as the remember group did. This suggests that when an event is recalled, it is recalled relatively intact.

We also expected to see group differences in Week 2 memory. In the typical list-learning experiment, participants who were instructed to forget first-half items remember significantly more second-half items than do remember participants. We did not observe any such Week 2 group differences in the autobiographical memories in either of the experiments reported here. The DF benefit is usually attributed to a decrease in proactive interference for the forget group. Perhaps the impact of study-related proactive interference was negligible, as compared with the interference arising from autobiographical memory as a whole. The participants experienced a myriad of autobiographical events over the course of the experiment that could have provided proactive interference at test. As a result, the decrease in proactive interference in the forget group due to the few forgotten Week 1 diary entries may have been small in the context of the other extraexperimental autobiographical events still remembered by the participants.

However, there is another possible interpretation. DF benefit may be due to the fact that the forget group, thinking of the second half of the list as a new task, is likely to switch to more efficacious memory-enhancing strategies midway through the list (Sahakyan & Delaney, 2003). In our experiments, the cover story for the forget cue may have encouraged *both* of the groups to regard second-half memory items in a different light. Recall that initially all the participants were told that they would be involved in one of two experiments. At the halfway point, everyone was told that Week 2 events would definitely be used for the memory experiment. This may have encouraged everyone to apply memory-enhancing strategies in earnest. Thus, it is difficult to tell whether the lack of DF benefit was due to the nature of the stimuli or to the procedure used in this paradigm. It is important to note that the DF cost was observed despite this potential context shift in both groups, suggesting that context shift alone does not account for the DF cost in this paradigm.

To summarize, we have raised many important questions that require further investigation for a better understanding of the mechanisms underlying deliberate

forgetting of autobiographical memories. At this point, little doubt remains that intentional forgetting of autobiographical events is possible. In two separate experiments, we have shown that people remember fewer autobiographical memories when instructed to forget them. The effect persisted despite a memorable holiday occurring in the manipulation week of the second experiment. Although DF has long been used to explain the loss of some autobiographical memories on the basis of list-learning studies, the two experiments reported here are the most direct evidence to date.

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NOTE

1. The questionnaire also included four self-relevance scales, which were omitted from the analyses because a combination of insufficient completion by the participants (Experiment 1) and a narrow range of responses (Experiment 2) gave rise to empty cells in the proposed analyses.

(Manuscript received January 16, 2003;
revision accepted for publication August 3, 2004.)