Perceptually Fluent Features of Study Words Do Not Inflate Judgements of Learning: Evidence from Font Size, Highlights, and Sans Forgetica Font Type

Nicholas P. Maxwell, Trevor Perry, & Mark J. Huff

The University of Southern Mississippi

Word count: XXXX

**Author Note**

Correspondence concerning this article can be addressed to Mark. J. Huff, School of Psychology, The University of Southern Mississippi, 118 College Dr. #5025, Hattiesburg, MS 39406, United States. Study materials and analyzed data are available via OSF (https://osf.io/3xwdr/). This study was completed as part of the Honors Thesis requirements for Trevor Perry. Email: mark.huff@usm.edu

Abstract

The judgment of learning (JOL) task is often used to assess memory monitoring at encoding. In the JOL task, participants study a cue-target word pair (e.g., mouse-cheese) and are asked to rate the probability of correctly recalling the target in the presence of the cue at test (e.g., mouse - ?). Prior research has shown that JOL accuracy is sensitive to perceptual cues. These cues can produce metamemory illusions in which JOLs overestimate memory, such as the *font-size effect* (Rhodes & Castel, 2008), which occurs when participants inflate JOLs for pairs presented in large versus small fonts. The present study further tests the font-size effect and examines whether other perceptual manipulations can affect the correspondence between JOLs and recall. Experiments 1A and 1B were designed to replicate the font-size effect and test whether the effect extended to highlighted pairs that were related or unrelated in the same study list. Experiment 2A and 2B then examined font size and highlighting effects on JOLs using only unrelated pairs. Finally, Experiment 3 tested whether Sans Forgetica—a perceptually disfluent font designed to improve memory—would result in inflated JOLs and/or recall. Across experiments, pairs designed to be perceptually fluent did not result in an overestimation of later recall relative to non-fluent pairs, and Sans Forgetica font in Experiment 3 yielded a memory cost (though no effect on JOLs). Collectively, perceptually fluent and disfluent study pairs do not appear to inflate JOLs at study.

Word Count: 240

*Keywords:* Judgments of Learning; Font-Size Effect; Perceptual Fluency; Sans Forgetica; Cued-Recall

Perceptually Fluent Features of Study Words Do Not Inflate Judgements of Learning: Evidence from Font Size, Highlights, and Sans Forgetica Font Type

The ability for individuals to accurately monitor their learning progress is important for successfully encoding new information. Successful monitoring allows individuals to maximize retention by adjusting their study strategies and can inform what strategies are used in future study tasks (Nelson & Narens, 1990). Metamemory judgments (i.e., having individuals judge aspects of their memorial abilities) are commonly used by researchers to obtain information about the learning process. While researchers use several types of judgments to assess metacognitive processes, the judgment of learning (JOL) task is commonly used. In a standard JOL task, participants study sets of cue-target word pairs (e.g., mouse-cheese) and are asked to estimate the likelihood of correctly retrieving the target word in the presence of a cue (e.g., mouse - ?). While JOL ratings can be made using a variety of measurement scales (e.g., Likert Scales or binary “yes-no” responses; Hanczakowski, Zawadzka, Pasek, & Higham, 2013), JOLs are commonly elicited using a continuous 0 to 100 scale representing the percent likelihood of the target item being successfully recalled at test (e.g., 100% = definitely would remember; 0% = definitely would not remember). The use of a 100-point scale is beneficial because it allows for an easy comparison between predicted recall (via JOLs) and the proportion of items that are correctly recalled at test.

Although JOLs can be predictive of future recall (e.g., Nelson & Dunlosky, 1991), certain situations can produce metamemory illusions in which JOLs underpredict or overpredict later recall. For example, relatedness cues such as the associative direction between cue-target pairs have repeatedly been shown to induce an *illusion of competence* in which JOLs overpredict later recall for certain types of paired associates (Koriat & Bjork, 2005). Specifically, forward associates, in which the cue is highly predictive of the target (e.g., lamp-shade), tend to produce JOLs that are well-calibrated with later recall. However, backward associates, in which the cue does not readily converge upon the target (e.g., shade-lamp), display a marked overconfidence effect such that JOLs greatly overestimate subsequent memory. Castel, McCabe, and Roediger (2007) have reported an illusion of competence pattern on identical pairs and, more recently, Maxwell and Huff (in press) have extended this pattern to symmetrical associates (e.g., king-queen), in which the forward and backward relations between pairs are matched. Like Koriat and Bjork (2005), Maxwell and Huff (in press) found that JOL ratings were generally well-calibrated for forward pairs, but produced an illusion of competence pattern for backward, symmetrical, and unrelated word pairs. Additionally, the illusion of competence was robust and persisted across a variety of experimental manipulations designed to improve the correspondence between JOLs and recall, such as JOL timing (e.g., concurrent, immediate, or delayed JOLs) and pacing (e.g., self-paced vs experimenter paced). Thus, JOLs can accurately predict later recall, particularly when cues are associated with the target in the forward direction.

In addition to relatedness cues, other factors have been shown to influence judgments. For example, perceptual cues have been shown to affect a variety of judgment tasks, including affective judgments (e.g., judging a target item’s beauty, Reber, Winkileman, and Schwarz, 1998), veridicality judgments (e.g., truthfulness of statements; Reber & Schwarz, 1999), and JOLs (Rhodes & Castel, 2008). Typically, studies investigating the effects of perceptual cues on judgment making do so by varying the ease with which participants can encode stimuli (see Schwarz, 2004, for a review). These ease-of-processing manipulations typically occur by changing some aspect of the stimuli (e.g., size, clarity, etc.) such that certain items are made more difficult to encode relative to others. For example, Reber et al. (1998) reported that participants judge perceptually fluent items as being more affectively pleasing versus disfluent items. Additionally, Reber and Schwarz (1999) showed that participants are more likely to judge more perceptually fluent statements (e.g., a statement presented in black ink against a white background) as being true compared to less perceptually fluent statements (e.g., a statement presented in yellow ink against a white background). Collectively, items classified as more perceptually fluent are processed more quickly and are more likely to encourage agreement than those that are not.

Importantly, these effects have been shown to extend to JOLs, as ease-of-processing has similarly been found to influence the magnitude and accuracy of these judgments. For example, Rhodes and Castel (2008) tested participants on word pairs that were studied in either large (48-pt.) or small (18-pt.) font sizes. A *font-size effect* was found in which JOLs were greater when pairs were presented in large versus small font, however, this increase in JOLs did not translate to recall, as both font types were equivalent. Subsequent experiments indicated that the font-size effect was largely driven by the additional ease-of-processing afforded by the large-font pairs. For example, the font-size effect was largely diminished when ease of reading was manipulated such that words were presented by alternating between uppercase and lowercase letters (e.g., HoUsE) and, furthermore, the effect was moderated by pair relatedness, as the effect was reduced when participants studied related pairs relative to unrelated pairs (Rhodes & Castel).

The font-size effect has been reported in several studies. For example, Kornell, Rhodes, Castel, and Tauber (2011) replicated the font-size effect and showed that this pattern holds even when pairs are studied repeatedly. More recently, Hu, Li, Zheng, Su, Liu, and Luo (2015) divided participants into groups that either studied or observed the participants who had been assigned to the study group. Participants in the study group made JOLs for pairs presented in either large or small fonts, while participants in the observer group were asked to guess the JOLs that participants in the study group would make and were only made aware of the font size of the pair that was being viewed, not the pair itself. Participants in both groups provided higher JOLs for large- than small-font pairs. Finally, Price and Harrison (2017) examined whether the font-size effect influenced the magnitude of pre-study JOLs. Overall, they showed that participants tended to assign higher JOLs for items presented in a large font higher relative to small font regardless of whether the JOL was made pre- or post-study.

Although the font-size effect has been reported under several conditions, the underlying factors driving the effect remain unclear. Two theories have been proposed to account the font-size effect—the fluency account and the beliefs account. First, the fluency account states that larger words are more perceptually fluent than smaller words. Due to their greater perceptual fluency, participants process larger words more efficiently and/or effectively, leading to greater JOLs relative to smaller words. In a test of the fluency account, Undorf, Zimdahl, and Bernstein (2017) presented participants with images of objects, faces, and words which were initially too small to perceive and incrementally increased the size of the stimuli. Participants were asked to make a JOL once they could recognize the stimulus, with the recognition latency recorded. Overall, JOLs were found to be inversely related to the recognition latency, indicating that items judged as more memorable were processed more quickly. Similarly, Yang, Huang, and Shanks (2018) tested the fluency account by comparing the results of a continuous identification task (CID) to the results of lexical decision tasks. The CID task tested the relationship between perceptual fluency and JOLs by alternating between a word and a corresponding mask (e.g., switching between the word “ball” and “####”). The speed in which alternations occurred was gradually decreased over time such that the word was made visible on the screen for longer durations (e.g., 20 ms in the first cycle, 40 ms in the second cycle, etc.). The goal of the CID was to slowly increase fluency by gradually making the word less obscure. Like Undorf et al., JOLs were greater for words that could be identified faster (i.e., those with a higher perceptual fluency).

Whereas the fluency account is based on the speed of processing items at study, the beliefs account posits that participants beliefs about an item’s memorability is the primary factor influencing JOLs. Regarding the font-size effect, participants may assign higher JOLs to large items because they hold the belief that large pairs are easier to learn than small pairs. To test the beliefs account, Mueller, Dunlosky, Tauber, and Rhodes (2014) had participants first complete a lexical decision task for a set of large versus small items. Unlike findings from Yang et al. (2018), latencies on the lexical-decision task did not differ as a function of font size, suggesting that the perceptually fluent large font did not facilitate latencies as predicted by a fluency account. Importantly, however, reported beliefs about the memorability of large versus small fonts and pre-study JOLs indicated that participants did indeed hold the belief that large-font items will be better remembered.

In addition to font sizes, other perceptual manipulations have also been shown to affect JOLs. For example, Ball, Klein, and Brewer (2014) tested how bolding word pairs affected JOLs and subsequent memory relative to non-bolded pairs. Overall, bolded pairs received higher JOLs relative to non-bolded pairs. However, like the font-size effect, no differences in recall performance were detected between the two. Additionally, Besken (2016) had participants complete a memory task in which images were presented either intact images or with sections removed (i.e., fluent vs. disfluent) and had participants complete a JOL task at encoding. Overall, intact images received higher JOLs relative to incomplete images, indicating that other fluency manipulations beyond font size can affect JOLs.

The present study provided a further test of the font-size effect while extending it to include other perceptual manipulations designed to similarly affect JOLs. Specifically, Experiments 1A and 2A sought to replicate the font-size effect using related and unrelated pairs. Next, Experiments 1B and 2B tested whether highlighting word pairs (vs. not highlighting) would affect JOLs as font size. Finally, Experiment 3 tested whether JOLs and recall rates would be affected by Sans Forgetica font—a disfluent font that is more perceptually difficult to process relative to a standard font such as Arial. The disfluent nature of Sans Forgetica also allowed us to compare predictions based on the fluency and beliefs accounts of the font-size effect.

Finally, we expand upon previous work (e.g., Rhodes & Castel, 2008) by including a pure-control group comparison in which only the standard perceptual condition is used (i.e., all pairs presented in a standard font size), rather than a mix of perceptually fluent/disfluent pairs. These control groups were included because encoding manipulations have been shown to spill over into other encoding tasks when encoding is manipulated within-subjects (Bodner, Taikh, & Fawcett, 2014; Huff, Bodner, & Gretz, 2021). Thus, our inclusion of the control groups allowed us to gauge perceptual effects on JOLs more accurately relative to a baseline condition.

**Experiment 1A: Font-Size Effects on Related and Unrelated Pairs**

The goal of Experiment 1A was to replicate the font-size effect using a set of related and unrelated word pairs. Overall, we expected that because large-font pairs are more perceptually fluent or because participants possess a belief that large pairs are more memorable, JOLs would be greater for large than for small pairs (cf. Rhodes & Castel, 2008). We also included comparisons to a control group who viewed pairs presented using a standard, 32-pt. Arial font to assess whether any effects of perceptually fluent pairs hold when compared to a pure list of standard pairs. Font-size effects were tested using a mixed list of forward, backward, and symmetrical paired associates and unrelated word pairs.

**Method**

**Participants**

Eighty participants were recruited from Prolific (www.prolific.co), an online academic crowdsourcing platform, and completed the study at rate of $4.00 per half hour. Participants were randomly assigned to the font-size group (*n* = 41) which studied large and small font pairs or the control group that studied pairs in a standard font size (*n* = 39). Participants reported a mean age of 32.65 (*SD* = 15.29), and all were native English speakers who reported normal or corrected-to-normal vision.

**Materials**

One-hundred-eighty word pairs taken from Maxwell and Huff (in press) served as study materials. These pairs included 40 forward pairs (e.g., bounce-ball), 40 backward pairs (e.g., ball-bounce), 40 symmetrical pairs (e.g., off-on), 40 unrelated pairs (e.g., pencil-fence), and 20 weakly related buffer pairs that were not tested to control for primacy and recency effects. The University of South Florida Free Association Norms (Nelson et al., 2004) were used to equate the related pairs in associative strength and to ensure that symmetrical pairs were equivalent in associative strength in the forward and backward direction. Pair types were also equated on lexical and semantic properties including word length, SUBTLEX frequency (Brysbaert & New, 2009), and concreteness as reported in the English Lexicon Project (Balota et al., 2007). All pairs were evenly distributed into two study lists which contained 20 forward, backward, symmetrical, and unrelated pairs, and 10 buffer pairs. Study materials for all experiments have been made available at https://osf.io/3xwdr/. Associative strength, lexical, and semantic properties are listed in the Appendix (Tables A1 and A2).

All participants studied both lists which were evenly divided into two study/test blocks, the order of which was counterbalanced across participants. Each list was organized in which five buffer pairs were presented at the beginning and end of each study list with the remaining pairs presented in a newly randomized order for each participant. Counterbalanced versions were produced from each study list that reversed the order of the pair lists (i.e., A-B pairs become B-A pairs), which allowed for greater control of item differences across pair types.

Participants in the font-size group saw lists in which half of the pairs were presented in a small 12 pt. font and the other half of pairs were presented in a large 54-pt. font which was counterbalanced across pair types. All pairs were presented in Arial font style. In the control group, pairs were presented in 32-pt. Arial font.

For the cued-recall test, participants were presented with all 80 cue items from the initial study list (buffers were not tested). The cue was presented alongside a question mark (e.g., bounce-?), and participants were instructed to retrieve the target from memory. Test items were newly randomized for each participant. Test instructions did not mention font size. The cue word in all groups was presented in a standard 32-pt. Arial font.

**Procedure**

All participants were tested online via *Collector*,an open-source program for presenting web-based psychological experiments (Garcia & Kornell, 2015). Participants were informed that they would study a series of cue-target pairs in which the cue would be presented on the left, and the target on the right. They were further instructed that their memory for the target item would be tested following study, with only the cue word presented at test. In addition to studying the pairs, participants were instructed to provide a JOL rating by rating the likelihood they would be able to correctly recall the target if only presented with the cue. JOLs were provided using a scale ranging from 0-100. A rating of 0 indicated that the participant had no confidence in their ability to recall the word at test; a rating of 100 indicated full complete certainty that they would recall the target. Participants were encouraged to utilize the full range of the scale and to avoid anchoring on extremes and mid points when providing ratings (i.e., 0, 50, or 100 ratings). Following instructions, participants then studied the first block of word pairs and provided JOL concurrently with study such that ratings were provided while the word pair was displayed on the screen. Upon completion of the first study list, participants completed a filler task where they had to list the 50 U.S. states in alphabetical order for 2 minutes, which was immediately followed by the cued-recall test. Participants were presented with the cue word paired with a question mark (e.g., credit - ?) and were asked to retrieve the correct target word by typing it into a dialog box. If participants were unable to retrieve the target, they were asked to press the enter key to advance to the next test item. Following the first cued-recall test, participants completed a second study list, filler task, and second cued-recall test which only tested pairs from the second study list. Following the second cued-recall test, participants were debriefed and provided with compensation. The duration of the experiment was less than 30 min across groups.

**Results**

All JOL responses were initially screened for missing responses and outliers (i.e., JOLs made outside the 0-100% range). This screening process removed less than 0.5% of the total responses. All missing recall responses were coded as incorrect. A liberal scoring criterion was used such that misspellings or pluralizations were scored as correct.

A *p* < .05 significance level was used for all analyses. Effect size estimates using partial-eta squared (*η*p2) and Cohen’s *d* were computed for all significant analyses of variance (ANOVAs) and *t*-tests, respectively. To supplement standard null-hypothesis significance testing, we include a Bayesian estimate of the strength of evidence supporting the null hypothesis (Masson, 2011; Wagenmakers, 2007). This analysis compares a model that assumes a significant effect to one that assumes a null effect. A probability estimate is computed termed *p*BIC (Bayesian Information Criterion) which indicates the likelihood that the null hypothesis is retained. Thus, null effects are supplemented with a *p*BIC estimate. Figure 1 plots mean JOL and cue-recall percentages for the large-font, small-font, and control groups as a function of forward, backward, symmetrical, and unrelated pair types. For completeness, all comparisons are reported in Table A3.

In our analyses, we first compare JOL and recall percentages across pair types in the font-size group and then compare between the within large- and small-font pairs and control groups. In the font-size group, a 2(Measure: JOL vs. Recall) × 2(Font Size: Large vs. Small) × 4(Pair Type: Forward vs. Backward vs. Symmetrical vs. Unrelated) within-subject ANOVA yielded an effect of measure, *F*(1, 40) = 10.11, *MSE* = 1258.14, *η*p2 = .20, in which JOLs exceeded recall percentages (50.34 vs. 41.53). An effect of pair type was also found, *F*(3, 120) = 414.56, *MSE* = 218.49, *η*p2 = .91, in which JOL/recall percentages were greatest for forward pairs (64.28), followed by symmetrical pairs (61.25), backward pairs (45.23), and unrelated pairs (12.99), with all pairs differing from each other, *t*s >3.27, *d*s > 0.24. An effect of font size was also found, *F*(1, 40) = 12.20, *MSE* = 66.26, *η*p2 = .23, indicating that JOL/recall percentages overall were greater for large font than small font pairs (47.05 vs. 44.83). Importantly, all interactions with font size, including the three-way interaction, were not reliable, *F*s < 1.63, *p*s > .18, *p*BICs > .99, indicating that the large font size did not differentially inflate JOLs relative to recall rates across pair types (cf. Rhodes & Castel, 2008).

A measure × pair type interaction was found, *F*(3, 120) = 45.27, *MSE* = 146.88, *η*p2 = .53, which indicated the presence of an illusion of competence pattern for some pair types. JOLs were well-calibrated to recall on forward pairs (61.84 vs. 66.72, for JOLs and recall percentages, respectively), *t*(40) = 1.41, *SEM* = 3.46, *p* = .17, *p*BIC = .70, but JOLs exceeded recall on symmetrical pairs (64.75 vs. 57.75), *t*(40) = 2.28, *SEM* = 3.08, *d* = 0.40, unrelated pairs (16.56 vs. 9.42), *t*(40) = 2.91, *SEM* = 2.46, *d* = 0.64, and especially on deceptive backward pairs (58.20 vs. 32.26), *t*(40) = 6.95, *SEM* = 3.73, *d* = 1.48.

We then compared JOLs/recall percentages on large and small font pairs relative to the control group to evaluate font size effects relative to a pure group that presented all pairs in a single font size. The control group similarly showed robust pair type differences on JOLs/recall percentages, *F*(3, 114) = 421.14, *MSE* = 100.03, *η*p2 = .92, and the same illusion of competence pattern found in large- and small-font pairs, *F*(3, 114) = 68.49, *MSE* = 49.12, *η*p2 = .64, no main effects or interactions were found when comparing large and small font size pairs relative to the control group, all *F*s < 1.47, *p*s > .22, *p*BICs> .99. Collectively, increasing font size increased both JOLs and recall percentages equally relative to the small-font sizes and the large-font sizes did not differ relative to a standard font size control.

**Experiment 1B: Highlighting Effects on Related and Unrelated Pairs**

Experiment 1B was a replication of Experiment 1A but used a highlight perceptual manipulation in which half of the pairs were presented using a yellow-highlight format and the other half were presented in a standard, non-highlight format. All pairs were presented using the same font size with the only perceptual difference being the difference in highlight presentation. We selected this manipulation, as under some conditions, the use of highlighting can be beneficial to comprehension and learning, as highlighting makes text distinguishable from non-highlighted material (Fowler & Barker, 1974; Yue, Storm, Kornell, & Bjork, 2015).

Like Experiment 1A, we expected that highlighting pairs would increase perceptual fluency and thus increase the likelihood that participants would provide elevated JOL ratings relative to non-highlighted pairs, a pattern consistent with large font-size effects reported by Rhodes and Castel (2008). However, given that large-font pairs were only found to produce a small and equivalent increase to both JOLs and recall relative to small-font pairs in Experiment 1A, it is possible that highlighting pairs would also increase both JOL and recall percentages equally. We also included comparisons to the control group used in Experiment 1A (non-highlighted pairs of the same font size) to gauge whether any highlighting benefits would hold when compared to a pure list of non-highlighted pairs. Again, highlighting effects were compared across forward, backward, symmetrical, and unrelated pair types.

**Method**

**Participants**

An additional 41 participants were recruited from Prolific to complete the study and were compensated at rate of $4.00 per half hour. Participants reported a mean age of 32.68 (*SD* = 15.27), and all were native English speakers who reported normal or corrected-to-normal vision.

**Materials and Procedure**

The same materials and general procedure in Experiment 1A was again used in Experiment 1B, with the only difference being the highlight versus no highlight presentation of word pairs. All pairs were presented in a 32-pt. Arial font type and half of the pairs were presented in a bright yellow highlighted format, whereas the other half were presented in a standard non-highlighted format. The cued recall test was identical to Experiment 1B, and all test pairs were presented in a randomized order using a non-highlighted format. The control group from Experiment 1A was also used.

**Results**

Experiment 1B followed the same data screening procedure as Experiment 1A, and less than 0.5% of the total JOL trials were removed. Figure 2 plots mean JOL and cued-recall percentages for highlight and no-highlight pairs across the four pair types. As in Experiment 1A, we first compare JOL/recall percentages across highlight and no-highlight pair types and then compare the within-subject highlight pairs relative to the control group. A 2(Measure) × 2(Highlight) × 4(Pair Type) within-subject ANOVA yielded an effect of measure, *F*(1, 40) = 7.69, *MSE* = 1346.04, *η*p2 = .16, in which overall, JOLs exceeded recall (50.65 vs. 42.70). An effect of pair type, *F*(3, 120) = 410.75, *MSE* = 197.25, *η*p2 = .91, indicated that JOL/recall percentages were greatest for forward pairs (64.87), followed by symmetrical pairs (60.88), backward pairs (45.06), and unrelated pairs (15.90). All pair types differed from each other, *t*s > 3.10, *d*s > 0.34. Unlike Experiment 1A, however, the fluent highlighting factor did not result in a main effect, *F* < 1, *p*BIC = .83, nor were any interactions with this factor reliable including the three-way interaction, all *F*s < 1, *p*s > .72, *p*BICs > .99.

The measure × pair type interaction was again significant, *F*(3, 120) = 56.96, *MSE* = 114.88, *η*p2 = .59, indicating an illusion of competence pattern. For forward pairs, JOLs were lower than recall (61.64 vs. 68.10), *t*(40) = 2.17, *SEM* = 2.94, *d* = 0.46, however, illusion of competence patterns were found in which JOLs exceeded later recall rates for symmetrical pairs (64.85 vs. 56.90), *t*(40) = 2.51, *SEM* = 3.16, *d* = 0.49, and backward pairs (57.21 vs. 32.91), *t*(40) = 6.89, *SEM* = 3.53, *d* = 1.55, but were only marginally greater than recall on unrelated pairs (18.91 vs. 12.90), *t*(40) = 1.90, *SEM* = 3.16, *p* = .06, *p*BIC = .52.

We then compared JOLs/recall percentages on the within-subject highlight and no-highlight pairs relative to control group pairs. Consistent with Experiment 1A, no effects or interactions were found when comparing the control-group pairs to either of the highlight pairs, all *F*s < 1.56, *p*s > .19, *p*BICs > .99. Collectively, highlighting pairs had no effect on JOLs or recall rates when compared to either no-highlight pairs in a mixed list or when compared to the pure list of non-highlighted pairs in the control group.

**Discussion**

The goal of the previous set of experiments was to replicate the font-size effect using a set of related and unrelated word pairs (Experiment 1A) and to test whether the effect extended to a highlighting manipulation (Experiment 1B). First, Experiment 1A did not show evidence consistent with font-size effect reported by Rhodes and Castel (2008), as large font increased both JOLs and recall rates relative to small fonts as the expected interaction was not observed. Furthermore, no differences were detected between either large or small pairs and the control group. Thus, large font did not selectively increase JOLs relative to small font without affecting recall. Next, to test the effects of other types of perceptual manipulations on JOLs and recall, we introduced a highlighting manipulation in Experiment 1B. However, the presence of highlighting did not affect JOLs or recall relative to non-highlighted pairs or the control group.

One explanation for the absent font-size effect in Experiment 1A may be due to our inclusion of both related and unrelated stimuli pairs within a mixed list. While Rhodes and Castel (2008) found a font-size effect on both related and unrelated pairs (Experiment 3), they noted that the effect was stronger when participants studied only unrelated pairs. Thus, our inclusion of related pairs may have negated potential fluency effects on JOLs. To test this possibility, Experiments 2A and 2B followed the same methods as Experiments 1A and 1B, but used only unrelated pairs.

**Experiment 2A: Font-Size Effects on Pure Unrelated Lists**

Because Rhodes and Castel (2008) found that the font-size effect was dampened when participants studied related pairs, Experiment 2A sought to replicate the font-size effect using only unrelated pairs. Overall, our predictions were the same as Experiment 1A. Specifically, we anticipated that pairs presented using a large font would have inflated JOLs relative to small font pairs. We again expected that there would be no differences in recall as a function of font-size, now that relatedness had been controlled for. Finally, as in Experiment 1, we again included comparisons to a control group who studied a pure list of pairs presented using 32-pt. Arial font.

**Method**

**Participants**

Sixty-five participants were recruited from Prolific and completed the study at a rate of $4.00 per half hour. An additional 12 undergraduates were recruited from The University of Southern Mississippi’s psychology research pool and completed the study in exchange for course credit. Participants were randomly assigned to either the font-size or control group. Data for 9 participants were excluded due to low recall rates (e.g., correct recall rates < 5%), which suggested that experiment instructions were not properly followed. This resulted in 36 participants in the font-size group and 32 participants in the control group. Participants reported a mean age of 25.41 (*SD* = 11.24). All participants were native English speakers who reported normal or corrected-to-normal vision.

**Materials and Procedure**

Experiment 2A followed the same procedure used in Experiment 1A with the exception that participants studied only unrelated pairs rather than a mixed list of related and unrelated pairs. To ensure a sizeable list of unrelated pairs, unrelated pairs from Experiment 1A were combined with a new set of unrelated pairs, leading to a total of 160 unrelated study pairs (80 pairs per block; see Table A4 for lexical characteristics). All other materials, including buffer pairs and the procedure, were identical to Experiment 1A.

**Results**

Figure 3 plots mean JOL and cued-recall percentages for large- and small-font pairs in the mixed group and pairs in the control group. For completeness, all comparisons are reported in Table A5. We first compared font size differences in the mixed group using a 2(Measure) × 2(Font Size) within-subject ANOVA. Across pair types, JOLs were not greater than recall rates (27.99 vs. 23.30, *F*(1, 35) = 2.15, *MSE* = 369.08, *p* = .15, *p*BIC = .67, but JOLs/recall rates were greater for large than small fonts (27.00 vs. 24.30), *F*(1, 35) = 19.10, *MSE* = 13.76, *η*p2 = .35. Importantly, however, font size did not affect JOLs and recall rates differently, *F* < 1, *p*BIC = .85.

When compared to the control group pairs, JOLs were greater than recall rates—an illusion of competence pattern—both when compared to large-font pairs (27.15 vs. 22.04), *F*(1, 66) = 4.43, *MSE* = 202.28, *η*p2 = .06, and when compared to small-font pairs (25.80 vs. 20.53), *F*(1, 66) = 5.75, *MSE* = 164.00, *η*p2 = .08. JOLs/recall rates were marginally greater for large-font pairs than control pairs (27.00 vs. 21.89), *F*(1, 66) = 3.54, *MSE* = 249.20, *p* = .06, *η*p2 = .05, *p*BIC = .58, but no difference occurred between small-font pairs and control pairs (24.30 vs. 21.89, *F* < 1, *p*BIC = .85. Like the large- and small-font pair comparison above, font size did not differentially affect JOLs from recall rates relative to control pairs, *F*s < 1, *p*BICs > .88.

**Experiment 2B: Highlighting Effects on Pure Unrelated Lists**

Experiment 2B provided a replication of Experiment 1B using only unrelated item pairs. Our predictions followed Experiment 1B and were in line with Rhodes and Castel’s (2008) font-size effect. We expected that highlighted pairs would be more perceptually fluent and would receive inflated JOLs relative to non-highlighted pairs. Recall was again not expected to differ as a function of highlighting. Consistent with the previous experiments, a control-group comparison was included. Thus, both JOLs and recall for highlighted and non-highlighted pairs were compared to the pure-control group from Experiment 2A.

**Method**

**Participants**

An additional 37 participants from Prolific completed Experiment 2B. Participants completed the study at a rate of $4.00 per half hour. Participants reported a mean age of 25.00 (*SD* = 10.39). All were native English speakers who reported normal or corrected-to-normal vision.

**Materials and Procedure**

The same unrelated pairs from Experiment 2A were again used in Experiment 2B. All procedures were identical with the exception that instead of large/small font sizes, half of the pairs were presented in a highlighted modality as in Experiment 1B, and the other half were presented in a non-highlighted modality. The font size for pairs in the highlight group was also identical to Experiment 1B, which matched the font size of the pairs in the control group.

**Results**

Figure 4 plots mean JOL and cued-recall percentages for highlight and no-highlight pairs and control-group pairs. Highlight differences were first compared using a 2(Measure) × 2(Highlight) within-subject ANOVA. Consistent with an illusion of competence pattern, overall JOLs exceeded later recall rates (29.17 vs. 20.54), *F*(1, 36) = 6.26, *MSE* = 440.59, *η*p2 = .15; however, like Experiment 1B, the highlight main effect was not reliable, *F*(1, 36) = 2.82, *MSE* = 18.23, *p* = .10, *p*BIC = .60, nor was the interaction, *F* < 1, *p*BIC = .86.

We then compared highlight and no-highlight pairs to the control group. Again, JOLs exceeded recall rates both in the highlight/control comparison (27.40 vs. 20.20), *F*(1, 67) = 8.58, *MSE* = 201.54, *η*p2 = .11, and in the no-highlight/control comparison (26.85 vs. 19.47), *F*(1, 67) = 8.73, *MSE* = 80.57, *η*p2 = .12. There were no differences when comparing either highlight or no-highlight pairs relative to the control group, *F*s < 1, *p*BICs > .85. The interactions were also not reliable, *F*s < 1, *p*BICs > .87.

**Discussion**

Findings from Experiments 2A and 2B are quite clear. First, in Experiment 2A, the font-size effect was not in evidence, even after removing related pairs and presenting participants with only unrelated pairs at study. While large font was again found to increase JOLs, it also produced an equivalent increase in recall, replicating the pattern observed in Experiment 1A. Finally, font-size did not affect JOLs or recall relative to the control group. Next, Experiment 2B replicated the pattern of results observed in Experiment 1B. Specifically, JOLs and recall did not differ between highlighted and non-highlighted pairs, and no differences were detected for either pair type relative to the control group. Thus, while highlighted pairs are likely to be perceptually fluent relative to non-highlighted pairs, neither memory predictions nor recall are affected.

Because neither the font-size nor a highlighting effect was observed, Experiment 3 tested whether using a perceptually disfluent font would affect JOL estimations. Specifically, we tested whether Sans Forgetica, a font specifically designed to improve retention, would affect JOLs and recall relative to Arial font. Sans Forgetica is a specialized font developed by researchers at Royal Melbourne Institute of Technology that was created to aide with retention (Earp, 2018). This font was purposely designed to be disfluent and is presented in an italicized, back-slanted, and hashed style (e.g., Sans Forgetica), which has been suggested to encourage deeper encoding due to its perceptual difficulty (i.e., desirable difficulties; Bjork & Bjork, 2011). Recent findings, however, indicate that Sans Forgetica does not benefit memory, with some studies showing no memory benefit or even a memory cost relative to a standard control font (Geller et al., 2020; Taylor et al., 2020).

Despite findings indicating that Sans Forgetica does not improve memory as claimed, it is possible that the disfluent nature of this font may cause participants to *think* that it is beneficial to memory, consistent with a beliefs-based account (Mueller et al., 2014). Experiment 3 examined Sans Forgetica font on JOLs as a means of evaluating the fluency and beliefs accounts of how perceptual features affect JOLs. Based on the fluency account, the disfluent nature of Sans Forgetica should result in lower JOLs relative to a more fluent font such as Arial. However, if participants hold a memory belief that pairs presented in Sans Forgetica are more likely to be correctly recalled at test, then JOLs may be inflated for the Sans Forgetica pairs.

**Experiment 3: Unrelated Word Pairs in Sans Forgetica Font**

The goal of Experiment 3 was to test the effects of Sans Forgetica font on JOLs and recall. Based on the fluency account, JOLs for Sans Forgetica pairs should be decreased relative to pairs presented using Arial font, as Sans Forgetica is a less perceptually fluent font. Alternatively, the beliefs account posits that Sans Forgetica should increase correct recall. Because this font was designed to improve retention, participants may hold the belief that Sans Forgetica is easier to learn and make higher JOLs accordingly. Furthermore, based on previous research indicating that Sans Forgetica does not increase recall and patterns predicted by the font-size effect, no effects of Sans Forgetica on memory were expected. Finally, as in the previous experiments, we again included a control group comparison in which all pairs were presented using 32-pt. Arial font. All word pairs were unrelated, as based on Rhodes and Castel (2008), unrelated pairs may be more sensitive to perceptual manipulations relative to related pairs.

**Method**

**Participants**

A total of 86 participants completed Experiment 3. Of these participants, 33 were recruited via Prolific and compensated at a rate of $4.00 per half hour, with the remaining 53 undergraduate students recruited from The University of Southern Mississippi’s psychology research pool who completed the study in exchange for partial course credit. Data from 6 participants were excluded using the same criteria as Experiment 2, resulting in 39 participants in the Sans Forgetica group and 41 in the control group. Participants reported a mean age of 22.34 (*SD* = 7.33). All were native English speakers with normal or corrected-to-normal vision.

**Materials and Procedure**

Experiment 3 used the same set of unrelated pairs used in Experiments 2A and 2B and followed the same general procedure with the following exception. Participants were randomly assigned to either the Sans Forgetica or control groups. Participants in the Sans Forgetica group studied mixed lists in which half of the word pairs were presented in 32-pt. Sans Forgetica font while the other half were presented in a standard, 32-pt. Arial font. For participants assigned to the control group, all pairs were presented 32-pt. Arial font (as in previous experiments). In both groups, participants made JOL ratings concurrently with study. All other materials and procedures were identical to those used in Experiments 2A and 2B.

**Results**

Figure 5 plots mean JOL and cued-recall percentages for Sans Forgetica and Arial font types in the mixed group as well as JOL/recall rates for the control group. Table A6 reports all comparisons for completeness. We first evaluated Sans Forgetica font effects using a 2(Measure) × 2(Font: Sans Forgetica vs. Arial) within-subject ANOVA. Consistent with Experiment 2, an effect of measure was found, *F*(1, 38) = 7.69, *MSE* = 383.54, *η*p2 = .17, in which JOL rates exceed correct recall (30.49 vs. 21.79). An effect of font was also found, *F*(1, 38) = 17.77, *MSE* = 28.66, *η*p2 = .32, in which Sans Forgetica produced *lower* JOL/recall rates relative to Arial font (24.24 vs. 27.95). The interaction was not reliable, *F*(1, 38) = 1.98, *MSE* = 25.19, *p* =.17, *p*BIC = .70.

We then separately compared Sans Forgetica and Arial pairs in the mixed group to the control group. Starting with the Sans Forgetica pairs, an effect of measure was found, *F*(1, 78) = 8.46, *MSE* = 166.33, *η*p2 = .10, in which JOLs exceeded recall (26.12 vs. 20.82). No difference was found on JOLs/recall rates between Sans Forgetica and control pairs, *F* < 1, *p*BIC = .86, but a marginal interaction was found, *F*(1, 78) = 3.64, *MSE* = 166.33, *p* = .06, *η*p2 = .05, *p*BIC = .59. Follow-up comparisons indicated that this interaction was due to an illusion of competence pattern for Sans Forgetica pairs, but not control pairs. Specifically, for Sans Forgetica pairs, JOLs exceeded recall rates (29.25 vs. 19.42), *t*(38) = 3.06, *SEM* = 3.21, *d* = 0.62, but for control pairs, JOLs and recall rates were well-calibrated (23.14 vs. 21.10), *t* < 1, *p*BIC = .82.

Turning to Arial pairs, an effect of measure was again found, *F*(1, 78) = 5.43, *MSE* = 169.94, *η*p2 = .07, in which JOLs exceeded recall (31.73 vs. 24.17). JOLs/recall rates were greater for Arial font than the control pairs (27.95 vs. 22.12), *F*(1, 78) = 5.01, *MSE* = 271.12, *η*p2 = .06, indicating that although Arial and control pairs were perceptually identical (same font type and size), Arial pairs that were in the same context as Sans Forgetica pairs were rated as more likely to be remembered than control pairs presented without a Sans Forgetica context. The interaction was not reliable, *F*(1, 78) = 1.73, *MSE* = 169.94, *p* = .18, *p*BIC = .78.

**Discussion**

Experiment 3 tested whether a Sans Forgetica font type would affect JOLs and recall rates relative to a standard Arial font. Based on a beliefs account, we expected that JOLs would be inflated for Sans Forgetica pairs, as participants would hold the belief that Sans Forgetica was beneficial to memory. Alternatively, the fluency account predicted that JOLs would be decreased for pairs presented using the more perceptually disfluent Sans Forgetica relative to when pairs were presented using Arial. Overall, the beliefs account was not supported, as Sans Forgetica did not produce the predicted increase in JOLs relative to either the standard Arial pairs or the control group. The fluency account, however, was upheld, as JOLs were lower for pairs presented using the less fluent Sans Forgetica font. Additionally, Sans Forgetica produced a cost to recall when compared to Arial pairs that had been presented within the same study list, suggesting that Sans Forgetica is costly to memory. Furthermore, Arial pairs in the mixed list received higher JOLs and were recalled at a greater rate relative to the control group, which presented pairs using the same font and size. These findings suggest that participants favor more fluent fonts when placed in the same context as disfluent fonts, and our inclusion of a control group allowed us to test this context effect.

**General Discussion**

The primary goal of our study was to evaluate the effects of perceptual fluency on JOLs and on the subsequent recall of word pairs. We based our study on Rhodes and Castel (2008), who reported a font-size effect in which JOLs were inflated for pairs presented in a perceptually large font relative to small font but had no effect on later recall. The present study similarly evaluated the font-size effect in addition to testing highlighting (vs. not highlighting) and Sans Forgetica (vs. a standard Arial) font type on both JOLs and recall rates.

Experiment 1A examined the font-size effect using a set of related (i.e., forward, backward, and symmetrical paired associates) and unrelated word pairs presented in the same study list. We expected that pairs presented using a large font, which are more perceptually fluent and thus easier to encode, would have inflated JOLs relative to small-font pairs without affecting recall. We also compared these mixed-list font sizes to a pure-control group in which all pairs were presented using a standard font size. The control comparison allowed us to evaluate large- and small-font size effects relative to a baseline font size. Regardless of whether large-font pairs were compared to small-font pairs or the control pairs, a font-size effect was not found. While the large-font size increased both JOLs and recall rates similarly relative to small-font pairs, JOLs for large-font word pairs did not increase at a greater rate than recall as predicted by the font-size effect. Finally, neither JOLs nor recall differed as a function of font-size relative to the control group.

Experiment 1B used the same set of related and unrelated stimuli pairs to test whether the font-size effect extended to highlighting. We similarly expected that the more perceptually fluent pairs (i.e., highlighted pairs) would receive inflated JOLs relative to the less perceptually fluent non-highlighted pairs but have no effect on recall rates, consistent with a font-size effect. Consistent with the Experiment 1A, highlighted pairs did not affect JOLs when compared to both non-highlighted pairs mixed group and the control group.

Because the perceptual manipulations did not differentially affect JOLs in Experiment 1 using a mixed list of related and unrelated word pairs, Experiments 2A and 2B provided an additional test of potential font-size and highlighting effects on JOLs using lists that only contained unrelated pairs. Experiment 2A again found that large-font sizes significantly increased both JOLs and recall rates relative to small-font pairs and marginally relative to control pairs, but again, a font-size effect was not found. In Experiment 2B, highlight again produced no effect on either JOLs or recall rates. Thus, font size and highlighting effects were not due to differences in the related vs. unrelated pairs presented within the study list.

Finally, Experiment 3 evaluated the effects of Sans Forgetica font on JOLs. We selected this font because, although it is perceptually disfluent, it is purported to enhance recall (Earp, 2018). While several recent studies have indicated that presenting study materials using Sans Forgetica does not benefit memory (e.g., Geller et al., 2020; Taylor et al., 2020), it is possible that participants may hold the belief that Sans Forgetica improves retention. Our use of Sans Forgetica in Experiment 3 allowed us to test the fluency and beliefs accounts of the font-size effect. According to the fluency account, the disfluent nature of Sans Forgetica should result in lower JOL ratings relative to a standard Arial font, whereas the beliefs account posits that Sans Forgetica will boost JOLs if participants hold a belief that Sans Forgetica font facilitates memory. Consistent with a fluency account, Sans Forgetica was found to decrease JOLs relative to the Arial font, but only when Sans Forgetica was compared to the Arial pairs in the mixed list and not the pure Arial pairs in the control group. Recall was also impacted by font type, as Sans Forgetica pairs were lower relative to mixed Arial pairs but not the pure Arial pairs—a Sans Forgetica cost. Interestingly, mixed Arial pairs produced greater JOLs and recall rates than Arial pairs in the pure group, suggesting that the mixed list context increased participants both JOLs and the encoding of Arial pairs.

In addition to our use of other perceptual manipulations beyond font-size, an important distinction between the current study and Rhodes and Castel (2008) is that each of our experiments included a pure-control group, a novel comparison that has not been included in previous studies investigating the font-size effect. This control task involved participants studying only one type of word pair rather than both perceptually fluent and disfluent word pairs like in the experimental groups. The inclusion of this task allowed us to assess the effects of list presentation (e.g., mixed vs pure lists) and control for potential carryover effects (e.g., Huff et al., 2021). Relative to the control group, Experiments 1 and 2 showed no significant effect of font-size or highlighting on JOLs or recall. Experiment 3, however, showed an increase to both JOLs and recall for words presented for Arial pairs in the mixed list relative to the same pairs in the control group. Thus, our inclusion of pure list control groups allowed us to assess the effects of context on fluency effects, providing a more complete assessment of how these processes affect both JOLs and recall.

Taken together, our experiments showed that memory predictions were largely unaffected by manipulations designed to affect perceptual fluency. This finding was surprising, given that the font-size effect has been shown to be highly robust and has been replicated on multiple occasions (see Halamish, Nachman, and Katzir, 2018). However, even though we used large sample sizes and tested across a variety of modalities, the expected increase in JOLs relative to recall did not occur. Although the font-size effect as reported by Rhodes and Castel (2008) did not emerge, Experiments 1A and 2A did show that font-size affected both JOLs and recall equally. Our effect of large font on JOLs, however, was smaller than that reported by Rhodes and Castel, and this effect was reduced despite a greater size difference between pairs in our experiment (12-pt. vs. 54-pt.) than those used by Rhodes and Castel (18-pt. vs. 48-pt.). While our experiments did not yield a font-size effect, large font pairs still appeared to produce some boost to JOLs and recall rates, a finding consistent with other studies (e.g., Miele, Finn, & Molden, 2011; Muller et al., 2014; Susser, Mulligan, & Besken., 2013; Undorf et al., 2017; Yang et al., 2018).

While font-size produced a small benefit to both JOLs and recall, neither highlighting nor Sans Forgetica font type produced a similar increase to JOLs or recall. Indeed, Sans Forgetica produce a mixed-list cost to memory relative to Arial font. This finding lends support to a growing body of literature suggesting that Sans Forgetica may not be an effective tool for improving retention (e.g., Geller et al., 2020; Taylor et al., 2020). Although Sans Forgetica was designed to improve retention through desirable difficulties, which have been suggested to contribute to other memory benefits (e.g., spacing and retrieval-practice effects; see Maddox, 2016, and Rowland, 2014, for reviews), it is not always clear what constitutes a sufficient level of difficulty to promote memory (e.g., McDaniel & Butler, 2010). Regarding the desirable difficulty of Sans Forgetica, recent work by Eskenazi and Nix (2021) has shown that within the context of learning, any benefits of this font may be moderated by individual differences, such as spelling and reading level. However, a trend is emerging in which Sans Forgetica may be more appropriately termed “Sans Remembrica” due to memory costs.

[IOC PARAGRAPH]

Though the font-size effect consistently was not found and the expected increases to JOLs did not extend to the other perceptual manipulations, we note one departure from the literature within our study that merits further discussion. Across all experiments, participants studied cue-target pairs, and at test, recall of the target word was always cued. Rhodes and Castel (2008) initially found the font-size effect by having participants study individual words rather than paired associates and using free recall testing. Indeed, other studies investigating the font-size effect have also used free-recall testing (e.g., Mueller et al., 2014; Yang et al., 2018), though we note that Price, McElroy, and Martin (2016) showed that large font increased JOLs using a cued-recall testing paradigm. While font-size has been shown to affect the magnitude of JOLs in cued-recall, our use of cued-recall testing versus free recall may be one explanation as to why the font-size effect consistently failed to emerge across our experiments. Thus, more research is needed to assess the effects of test type on the font-size effect and other perceptual manipulations of study items.

**Conclusion**

The present study investigated the effects of perceptual fluency on JOLs and recall by (1) evaluating the font-size effect and (2) testing whether highlighted pairs and perceptually disfluent Sans Forgetica font would affect JOLs and subsequent cued-recall. While font-size increased both JOLs and recall rates similarly for lists with both related and unrelated pairs and in lists with only unrelated pairs, JOLs were not overly inflated. The highlighting manipulation, however, produced no effect on JOLs or recall, regardless of list type. Furthermore, Experiment 3 showed that Sans Forgetica font, which was designed to improve retention, can induce a memory cost under certain circumstances. Finally, our inclusion of control groups within each experiment provided a baseline comparison group given possible mixed-list carryover effects that have previously been unaccounted for within this context. Collectively, this set of experiments provides a greater understanding of how perceptual features influence JOLs and recall, particularly within the context of cued-recall testing.

References

Ball, B. H., Klein, K. N., & Brewer, G. A. (2014). Processing fluency mediates the influence of

perceptual information on monitoring learning of educationally relevant materials.

*Journal of Experimental Psychology: Applied*, *20*(4), 336.

Balota, D. A., Yap, M. J., Hutchison, K. A., Cortese, M. J., Kessler, B., Loftis, B., Neely, J. H., Nelson, D. L., Simpson, G. B, & Treiman, R. (2007). The English lexicon project. *Behavior Research Methods, 39* (3), 445-459.

Besken, M. (2016). Picture-perfect is not perfect for metamemory: Testing the perceptual fluency hypothesis with degraded images. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *42*(9), 1417.

Bjork, E. L., & Bjork, R. A. (2011). Making things hard on yourself, but in a good way: Creating

desirable difficulties to enhance learning. *Psychology and the real world: Essays illustrating fundamental contributions to society*, *2*(59-68).

Bodner, G. E., Taikh, A., & Fawcett, J. M. (2014). Assessing the costs and benefits of production

in recognition. *Psychonomic Bulletin & Review*, *21*(1), 149-154.

Brysbaert, M., & New, B. (2009). Moving beyond Kučera and Francis: A critical evaluation of

current word frequency norms and the introduction of a new and improved word frequency measure for American English. *Behavior Research Methods, 41*, 977–990.

Castel, A. D., McCabe, D. P., & Roediger, H. L. (2007). Illusions of competence and overestimation of associative memory for identical items: evidence from judgments of learning. *Psychonomic Bulletin & Review*, *14* (1), 107–111.

Earp, J. (2018). Q&A: Designing a font to help students remember key information.

Eskenazi, M. A., & Nix, B. (2021). Individual differences in the desirable difficulty effect during lexical acquisition. *Journal of Experimental Psychology: Learning, Memory, and Cognition, 47*(1), 45-52.

Fowler, R. L., & Barker, A. S. (1974). Effectiveness of highlighting for retention of text material.

*Journal of Applied Psychology*, *59*(3), 358.

Garcia, M. & Kornell, N. (2015). Collector [Computer software]. Retrieved April 3rd, 2020 from

https://github.com/gikeymarica/Collector.

Geller, J., Davis, S. D., & Peterson, D. J. (2020). Sans forgetica is not desirable for learning.

*Memory*, *28*(8), 957-967.

Halamish, V., Nachman, H., & Katzir, T. (2018). The effect of font size on children’s memory and metamemory. *Frontiers in Psychology, 9*, 1577.

Hanczakowski, M., Zawadzka, K., Pasek, T., & Higham, P. A. (2013). Calibration of metacognitive judgments: Insights from the underconfidence-with-practice effect. *Journal of Memory and Language, 69*(3), 429–444. doi: 10.1016/j.jml.2013.05.003

Hu, X., Li, T., Zheng, J., Su, N., Liu, Z., & Luo, L. (2015). How much do metamemory beliefs

contribute to the font-size effect in judgments of learning? *PloS one*, *10*(11), p. e0142351.

Huff, M. J., Bodner, G. E., & Gretz, M. R. (2021). Distinctive encoding of a subset of DRM lists yields not only benefits, but also costs and spillovers. *Psychological Research, 85*, 280-290.

Koriat, A., & Bjork, R. A. (2005). Illusions of competence in monitoring one’s knowledge during study. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *31* (2), 187–194.

Kornell, N., Rhodes, M. G., Castel, A. D., & Tauber, S. K. (2011). The ease-of-processing

heuristic and the stability bias: Dissociating memory, memory beliefs, and memory judgments. *Psychological Science*, *22*(6), 787-794.

Maddox, G. B. (2016). Understanding the underlying mechanism of the spacing effect in verbal

learning: A case for encoding variability and study-phase retrieval. *Journal of Cognitive Psychology*, *28*(6), 684-706.

Masson, M. E. (2011). A tutorial on a practical Bayesian alternative to null-hypothesis

significance testing. *Behavior Research Methods,* *43*(3), 679-690.

Maxwell, N. P., & Huff, M. J. (in press). The deceptive nature of associative word pairs: Effects of associative direction on judgments of learning. *Psychological Research*, 1-19.

McDaniel, M. A., & Butler, A. C. (2010). A contextual framework for understanding when difficulties are desirable. In A. S. Benjamin (Ed.), *Successful remembering and successful forgetting: A festschrift in honor of Robert A. Bjork* (pp. 175-198). New York, NY: Psychology Press.

Miele, D. B., Finn, B., & Molden, D. C. (2011). Does easily learned mean easily remembered?: It depends on your beliefs about intelligence. *Psychological Science, 22*(3), 320-324.

Mueller, M. L., Dunlosky, J., Tauber, S. K., & Rhodes, M. G. (2014). The font-size effect on

judgments of learning: Does it exemplify fluency effects or reflect people’s beliefs about

memory? *Journal of Memory and Language*, *70*, 1-12.

Nelson, D. L., McEvoy, C. L., & Schreiber, T. A. (2004). The University of South Florida free association, rhyme, and word fragment norms. *Behavior Research Methods,* *Instruments, & Computers*, *36* (3), 402–407.

Nelson, T. O., & Dunlosky, J. (1991). When people’s judgments of learning (JOLs) are extremely accurate at predicting subsequent recall: The delayed-JOL eﬀect. *Psychological Science, 2*, 267–270.

Nelson, T. O., & Narens, L. (1990). Metamemory: A theoretical framework and new findings. *Psychology of Learning and Motivation,* *26*, 125-173.

Price, J., & Harrison, A. (2017). Examining what prestudy and immediate judgments of learning

reveal about the bases of metamemory judgments. *Journal of Memory and Language*, *94*, 177-194.

Price. J., McElroy, K., & Martin, N. J. (2016). The role of font size and font style in younger and older adults’ predicted and actual recall performance. *Aging, Neuropsychology, and Cognition, 23*(3), 366-388

Reber, R., & Schwarz, N. (1999). Effects of perceptual fluency on judgments of truth.

*Consciousness and cognition*, *8*(3), 338-342.

Reber, R., Winkielman, P., & Schwarz, N. (1998). Effects of perceptual fluency on affective

judgments. *Psychological science*, *9*(1), 45-48.

Rowland, C. A. (2014). The effect of testing versus restudy on retention: a meta-analytic review

of the testing effect. *Psychological Bulletin*, *140*(6), 1432.

Schwarz, N. (2004). Metacognitive experiences in consumer judgment and decision making.

*Journal of Consumer Psychology*, *14*(4), 332-348.

Soderstrom, N. C., Clark, C. T., Halamish, V., & Bjork, E. L. (2015). Judgments of learning as memory modifiers. *Journal of Experimental Psychology: Learning, Memory, and Cognition, 41*(2), 553-558.

Susser, J. A., Mulligan, N. W., & Besken, M. (2013). The effects of list composition and perceptual fluency on judgments of learning (JOLs). *Memory & Cognition, 41*, 1000-1011.

Taylor, A., Sanson, M., Burnell, R., Wade, K. A., & Garry, M. (2020). Disfluent difficulties are

not desirable difficulties: the (lack of) effect of Sans Forgetica on memory. *Memory*, *28*(7), 850-857.

Undorf, M., Zimdahl, M. F., & Bernstein, D. M. (2017). Perceptual fluency contributes to effects

of stimulus size on judgments of learning. *Journal of Memory and Language*, *92*, 293-304.

Wagenmakers, E. (2007). A practical solution to the pervasive problems of *p* values.

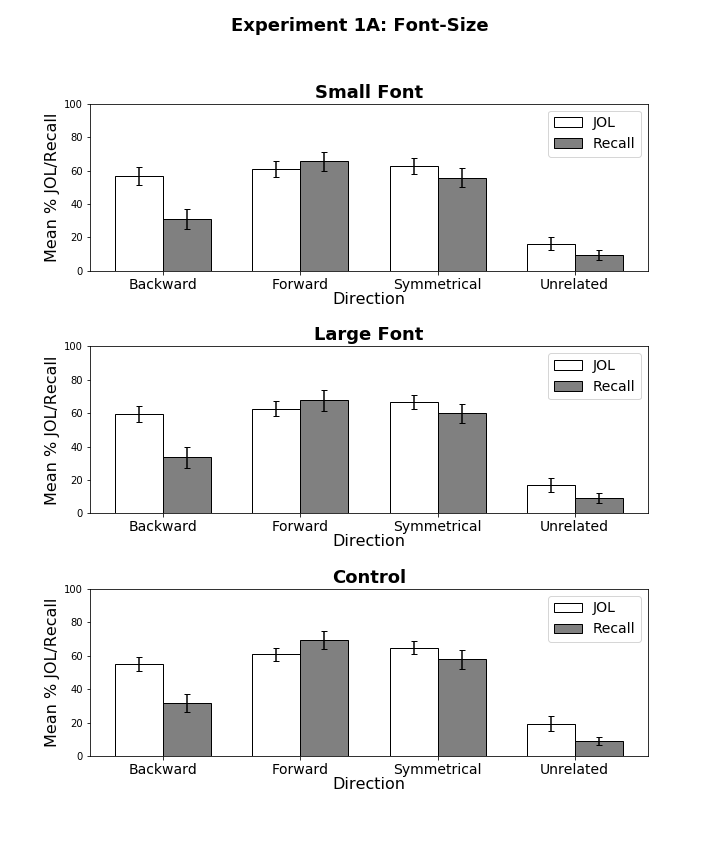
*Psychonomic Bulletin & Review,* *14*(5), 779-804.

Yang, C., Huang, T. S. T., & Shanks, D. R. (2018). Perceptual fluency affects judgments of

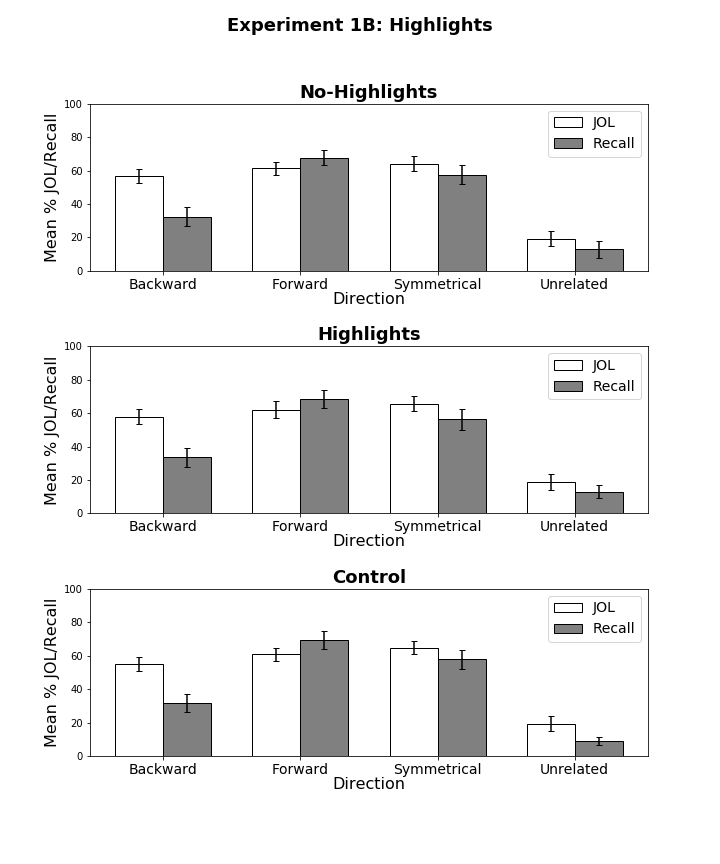
learning: The font size effect. *Journal of Memory and Language*, *99*, 99-110.

Yue, C. L., Storm, B. C., Kornell, N., & Bjork, E. L. (2015). Highlighting and its relation to

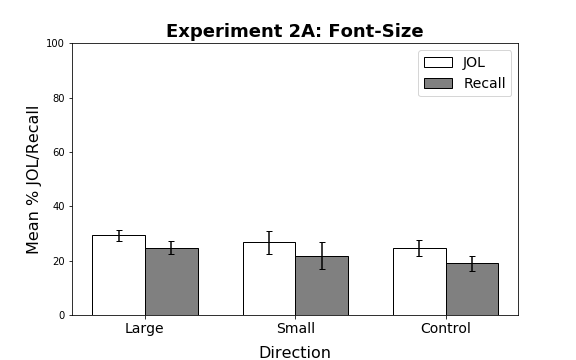
distributed study and students’ metacognitive beliefs. *Educational Psychology Review*, *27*(1), 69-78.



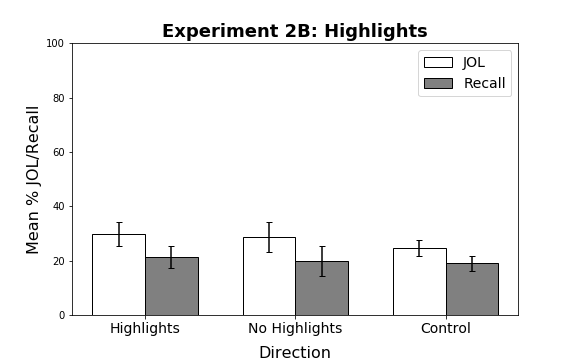
*Figure 1*. Mean JOL and recall rates as a function of pair type for pairs presented in small font (top panel), large font (middle panel), and the control group (bottom panel) as a function of pair direction in Experiment 1A. Bars represent 95% confidence intervals.



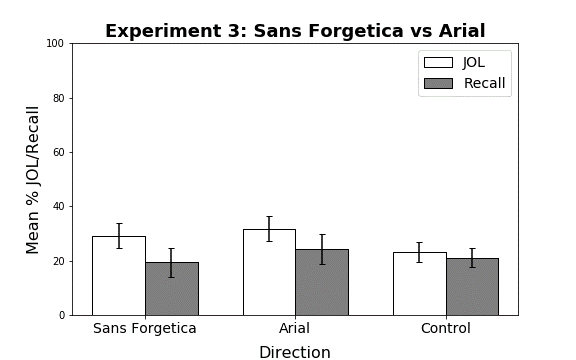
*Figure 2*. Mean JOL and recall rates as a function of pair type for highlighted pairs presented in mixed lists (top panel), non-highlighted pairs presented in mixed lists (middle panel), and non-highlighted pairs presented the control group (bottom panel) as a function of pair direction in Experiment 1B. Bars represent 95% confidence intervals.



*Figure 3*. Mean JOL and recall rates as function of pair type in Experiment 2A. Bars represent 95% confidence intervals. All study pairs were unrelated.



*Figure 4*. Mean JOL and recall rates as function of pair type in Experiment 2B. Bars represent 95% confidence intervals. All study pairs were unrelated.



*Figure 5*. Mean JOL and recall rates as function of pair type in Experiment 3. Bars represent 95% confidence intervals. All study pairs were unrelated.

**Appendix**

Table A1

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| Condition | Variable | *M* | *SD* | *Min.* | *Max.* |
| Forward | FAS | .37 | .21 | .05 | .81 |
|  | BAS | .00 | .00 | .00 | .00 |
| Backward | FAS | .00 | .00 | .00 | .00 |
|  | BAS | .37 | .21 | .05 | .81 |
| Symmetrical | FAS | .19 | .13 | .01 | .46 |
|  | BAS | .19 | .13 | .02 | .52 |

*Mean Associative Strength Summary Statistics Forward, Backward, and Symmetrical Pairs in Experiment 1A and 1B .*

*Note.* FAS (forward associative strength) and BAS (backward associative strength) values for unrelated pairs as these items share zero associative overlap.

Table A2

*Summary Statistics for Cue and Target Concreteness, Length, and Frequency Item Properties as a Function of Pair Type in Experiments 1A and 1B.*

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
| Pair Type | Position | Variable | *M* | *SD* |
| Forward | Cue | Concreteness | 4.97 | 1.22 |
|  |  | Length | 6.20 | 1.86 |
|  |  | Frequency | 3.74 | 0.67 |
|  | Target | Concreteness | 4.96 | 1.14 |
|  |  | Length | 4.46 | 1.27 |
|  |  | Frequency | 2.49 | 0.63 |
| Backward | Cue | Concreteness | 4.96 | 1.14 |
|  |  | Length | 4.46 | 1.27 |
|  |  | Frequency | 2.49 | 0.63 |
|  | Target | Concreteness | 4.97 | 1.22 |
|  |  | Length | 6.20 | 1.86 |
|  |  | Frequency | 3.74 | 0.67 |
| Symmetrical | Cue | Concreteness | 4.93 | 1.36 |
|  |  | Length | 5.05 | 1.62 |
|  |  | Frequency | 3.27 | 0.61 |
|  | Target | Concreteness | 4.44 | 1.37 |
|  |  | Length | 5.38 | 2.23 |
|  |  | Frequency | 3.18 | 0.73 |
| Unrelated | Cue | Concreteness | 4.59 | 1.40 |
|  |  | Length | 5.13 | 1.56 |
|  |  | Frequency | 3.20 | 0.80 |
|  | Target | Concreteness | 4.67 | 1.15 |
|  |  | Length | 5.30 | 1.49 |
|  |  | Frequency | 3.18 | 0.90 |

*Notes.* Frequency is measured using SUBTLEX word frequency measure (Brysbaert & New, 2009). Concreteness and length were taken from the English Lexicon Project (Balota et al., 2007).

Table A3

*Mean (± 95% CI) JOL Ratings and Correct Recall Percentages as a Function of Pair Type (Forward, Backward, Symmetrical, and Unrelated) for the Control, Highlight, and Font Size Groups in Experiments 1A and 1B.*

Pair Type/Group Forward Backward Symmetrical Unrelated

JOL Ratings

Control Group 60.87 (3.85) 55.18 (4.07) 64.84 (3.74) 19.43 (4.76)

Font Size Group

Large Items 62.76 (4.68) 59.59 (4.33) 66.74 (4.30) 16.81 (4.18)

Small Items 60.93 (4.83) 56.81 (5.41) 62.76 (4.56) 16.31 (3.92)

Highlight Group

Highlight Items 61.95 (5.02) 57.86 (4.33) 65.53 (4.43) 18.55 (4.76)

No Highlight Items 61.32 (4.08) 56.55 (4.24) 64.17 (4.40) 19.26 (4.49)

Correct Recall %

Control Group 69.29 (5.39) 31.67 (5.29) 57.76 (5.62) 8.85 (2.50)

Font Size Group

Large Items 67.76 (6.33) 33.47 (6.47) 59.81 (5.64) 9.43 (3.00)

Small Items 65.67 (5.72) 31.06 (5.79) 55.67 (5.58) 9.40 (3.06)

Highlight Group

Highlight Items 68.51 (5.20) 33.51 (5.93) 56.27 (6.39) 12.90 (3.94)

No Highlight Items 67.69 (4.36) 32.32 (5.65) 57.53 (5.76) 12.91 (5.28)

Table A4

*Summary Statistics for Cue and Target Concreteness, Length, and Frequency Item Properties for Unrelated Pairs in Experiments 2A, 2B, and 3.*

|  |  |  |  |
| --- | --- | --- | --- |
| Position | Variable | *M* | *SD* |
| Cue | Concreteness | 4.55 | 1.24 |
|  | Length | 5.16 | 1.50 |
|  | Frequency | 3.04 | 0.84 |
| Target | Concreteness | 4.20 | 1.42 |
|  | Length | 5.10 | 1.36 |
|  | Frequency | 3.13 | 0.76 |

*Notes.* Frequency is measured using SUBTLEX word frequency measure (Brysbaert & New, 2009). Concreteness and length were taken from the English Lexicon Project (Balota et al., 2007).

Table A5

*Mean (± 95% CI) JOL Ratings and Correct Recall Percentages for the Control, Highlight, and Font Size Groups in Experiments 2A and 2B.*

Group JOL Rating Correct Recall %

Control 24.26 (4.87) 16.69 (4.69)

Font Size

Large 27.70 (4.80) 22.31 (5.24)

Small 25.53 (4.33) 19.81 (4.84)

Highlight

Highlight 30.34 (5.23) 19.69 (4.89)

No Highlight 29.64 (5.46) 18.56 (5.34)

*Note.* All study/test items were unrelated.

Table A6

*Mean (± 95% CI) JOL Ratings and Correct Recall Percentages for the Control, Highlight, and Font Size Groups in Experiment 3.*

Group JOL Rating Correct Recall %

Control Group 23.14 (3.56) 21.10 (3.56)

Sans Forgetica Group

Sans Forgetica Font 29.25 (4.59) 19.42 (5.31)

Standard Font 31.73 (4.64) 24.17 (5.51)

*Note.* All study/test items were unrelated.