1 Is This Going to Be on The Test? Test Expectancy Moderates the Disfluency Effect with Sans

2 Forgetica

3 Jason Geller^{1,2}

4 Daniel Peterson³

5 ¹ University of Iowa

6 ² Rutgers University Center for Cognitive Science

7 ³ Skidmore College

8 Author Note

9 In press at JEP:LMC

10 Jason Geller 0000-0002-7459-4505

11 Correspondence concerning this article should be addressed to Jason Geller, Rutgers
12 University Center for Cognitive Science (RuCCS), 152 Frelinghuysen Road, Busch Campus,
13 Piscataway, New Jersey 08854. E-mail: jason.geller@ruccs.rutgers.edu

14 The preregistered analysis plan for Experiment 1 can be found here: <https://osf.io/wgp9d>.
15 The preregistered analysis plan for Experiment 2 can be found here: <https://osf.io/3xak9>. The
16 preregistered plan for Experiment 3 can be found here: <https://osf.io/hjnk5>. All raw and summary
17 data, materials, and R scripts for preprocessing, analysis, and plotting for Experiments 1, 2, and 3
18 can be found at <https://osf.io/cqp6s/>.

Abstract

Presenting information in a perceptually disfluent format sometimes enhances memory. Recent work examining one type of perceptual disfluency manipulation, Sans Forgetica typeface, has yielded discrepant findings; some studies find support for the idea that the disfluent typeface improves memory while others do not. The current study examined a boundary condition that determines when disfluency is and is not beneficial to learning to explore this discrepancy. Specifically, we investigated whether knowledge about an upcoming test (high test expectancy) versus not (low test expectancy) helps clarify when mnemonic benefits arise for perceptually disfluent stimuli. In Experiment 1 (preregistered, $N = 231$), we found that Sans Forgetica is a memory-improving desirable difficulty, but only when there was no expectation of a final test. In Experiment 2 (preregistered $N = 232$), we conceptually replicated the Sans Forgetica effect using a cued recall test. In Experiment 3 (preregistered, $N = 232$), we ruled out a time-on-task explanation while replicating the results of Experiment 2. Though these data provide some evidence of Sans Forgetica's mnemonic benefits, caution should be taken in interpreting these results. Not only were the effect sizes moderate, but low test expectancy may not be realistically achievable in actual educational contexts. Though more research is warranted, we echo our prior arguments (Geller et al., 2020) that students wanting to remember more and forget less should stick to other, more empirically supported desirable difficulties.

Keywords: Disfluency, Recognition, Recall, Desirable Difficulties, Test Expectancy

Is This Going to Be on The Test? Test Expectancy Moderates the Disfluency Effect

Imagine if you could remember more and forget less just by making the perceptual features of to-be-learned material harder. While this runs counter to the widely held belief that learning should be fluent (easy) and errorless (Pan et al., 2020), the concept of *desirable difficulties* (Bjork & Bjork, 2011) indicates that making learning more disfluent (harder) and error-prone can sometimes help learners process the information more deeply and make it more likely they will retrieve the information at a later time. This general finding has been shown across various encoding contexts (e.g., spacing and interleaving; Carpenter, 2014). One provocative line of research that has piqued researchers' interest is the influence of extraneous factors, such as the perceptual format of to-be-learned material (e.g., size, font/typeface, or clarity), on memory. In some cases, making to-be-learned material perceptually disfluent (hard-to-read) is desirable for memory—a phenomenon dubbed the perceptual interference effect (Nairne, 1988), or the perceptual disfluency effect (Geller et al., 2018). While perceptual disfluency has the potential to be valuable (and easy to implement), a recent meta-analysis has called into question whether perceptual disfluency is desirable for learning (Xie et al., 2018, c.f., Weissgerber et al., in press). The current research investigates under what conditions disfluency is and is not beneficial for learning using Sans Forgetica as a proxy for perceptual disfluency.

Sans Forgetica

A typeface known as Sans Forgetica has garnered attention from both researchers and the media due to its purported promises to stave off forgetting and enhance memory. Sans Forgetica is a typeface developed by a team of psychologists, graphic designers, and marketers (Earp, 2018), that consists of intermittent gaps and back-slanted letters (see Figure 1 for an example). The disfluent perceptual characteristics are thought to provide the optimal level of disfluency to produce a desirable memory effect. The claims made about Sans Forgetica have led to extensive

press coverage from major news outlets (e.g., *NPR*, *Washington Post*) and the development of browser extensions and OS applications that allow users to place content in the novel typeface. The question, of course, is whether Sans Forgetica merits such attention. As Carl Sagan famously said, “Extraordinary claims require extraordinary evidence” (1980).

Figure 1

An example of Arial typeface (on the left) compared to Sans Forgetica (on the right). Sans Forgetica is licensed under the Creative Commons Attribution-Non Commercial License (CC BY-NC; <https://creativecommons.org/licenses/by-nc/3.0/>)

nation – state

Two recent studies provide some initial evidence against these claims. Taylor et al. (2020) and Geller et al. (2020) examined whether Sans Forgetica is *really* desirable for learning. In one of the first studies to look at the mnemonic benefits of Sans Forgetica ($N = 882$ across four experiments), Taylor et al. (2020) found that while Sans Forgetica was perceived as more disfluent by participants (Experiment 1), there was no evidence that it yielded a mnemonic boost in cued recall with strongly related cue-target pairs (Experiment 2) compared to a fluent (Arial) typeface, or when learning simple prose passages (Experiments 3-4). Shortly after the publication of this paper, Geller et al. (2020) contributed to the debate with three preregistered experiments ($N = 820$) finding, similar to Taylor et al. (2020), Sans Forgetica did not enhance memory for weakly related cue-target pairs (Experiment 1), a complex prose passage (Experiment 2), or a yes/no recognition memory test (Experiment 3). Taken together, two independent laboratories

conducting seven experiments with well over 1,500 participants make for a compelling argument that there is little, if any, evidence that Sans Forgetica qualifies as a desirable difficulty.

Effects of Perceptual Disfluency on Learning

While there is evidence that Sans Forgetica does not enhance memory, a growing literature has shown that other types of perceptual disfluency can improve learning. In a seminal study, Diemand-Yauman et al. (2011) presented to-be-learned material in difficult-to-read typefaces (e.g., **Comic Sans**, Bodoni MT, **Haettenschweiler**, *Monotype Corsiva*). These typefaces enhanced learning and retention in both the laboratory (Experiment 1) when learning about space aliens and in the classroom (Experiment 2) where students studied PowerPoints in difficult typefaces across several different content areas (AP English, Honors English, Honors Physics, Regular Physics, Honors US History, and Honors Chemistry). Follow-up studies have since shown positive effects of disfluency with a wide array of perceptual manipulations such as high-level blurring (Rosner et al., 2015), inversion (Sungkhasettee et al., 2011), handwritten cursive (Geller et al., 2018), and other unusual or difficult-to-read typefaces (Weissgerber & Reinhard, 2017; Weltman et al., 2014).

However, the research is unfortunately inconsistent. For instance, Rhodes and Castel (2008) showed that words in a smaller-sized font (18 point) were judged as being more disfluent compared to words printed in a larger-sized font (48 point), but the smaller font did not lead to better memory (see Ball et al., 2014; Kornell et al., 2011; Susser et al., 2013, for similar failures to replicate the font size effect; but see Halamish, 2018, and Luna et al., 2018, for exceptions). In another study, Yue et al. (2013) examined the perceptual disfluency effect using a low-level blur manipulation. They examined the effect of blurring across several factors: type of task (recall vs. recognition), study duration (500 ms vs. 2 s), and design (within- vs. between-item lists). None of

their experiments revealed a memory benefit for low-level blurring (but see Rosner et al., 2015, for evidence with a high-level blur manipulation). Failures to replicate the disfluency effect also extend to other types of perceptual manipulations (e.g., hard-to-read fonts, Magreehan et al., 2015; hard-to-hear auditory information, Rhodes & Castel, 2009) and more complex learning situations (e.g., in the classroom, Carpenter et al., 2013; longer learning materials; Rummer et al., 2016; Strukelj et al., 2015).

In some instances, research on perceptual disfluency has demonstrated not just null but negative effects, complicating matters even further. Yue et al. (2103, Experiments 1a and 1b) found that a low-level blur manipulation hurt recall compared to a clear, normal font. Similarly, in the aforementioned Taylor et al. (2020) exploration of Sans Forgetica, outcomes from Experiment 2 suggested not only was the novel typeface not beneficial for learning, but it also impaired memory for briefly presented (100 ms) cue-target pairs.

Because of these mixed findings, studies have begun to investigate specific conditions under which perceptual disfluency does and does not enhance learning. For example, Lehmann et al. (2016) observed that perceptually disfluent fonts only improved learning for individuals with high working memory capacity. Further, Geller et al. (2018) demonstrated that the level of perceptual disfluency matters. Using handwritten cursive, they varied the disfluency level of cursive (i.e., easy-to-read and hard-to-read). Outcomes revealed cursive stimuli produced better memory. However, in a small-scale meta-analysis, they observed an inverted U-shaped pattern wherein easy-to-read cursive produced better memory than type-print and hard-to-read cursive, despite the hard-to-read cursive being more disfluent. Such a pattern suggests that not all disfluency manipulations are created equal; there is an optimal disfluency level (also see Seufert et al., 2017). Finally, Weissgerber & Reinhard (2017) found that time of test influences whether disfluency enhances memory. They used a hard-to-read font and tested participants at two time

points spaced two weeks apart. On the immediate test, the hard-to-read font did not produce better memory compared to transposed-letter (e.g., *jugde* for *judge*) and normal font conditions. At the second time point, however, material in a hard-to-read font produced less forgetting than the other two conditions suggesting that there might be a disfluency sleeper effect where the benefits of perceptual disfluency are seen only after a longer retention interval (Oppenheimer & Alter, 2013).

Theoretical Accounts of Perceptual Disfluency

Despite these null (and sometimes negative) effects, the positive findings reported suggest that perpetual disfluency can be desirable for learning under some conditions. What, then, is the proposed mechanism underlying such an effect? The perceptual disfluency effect can be explained against the backdrop of traditional dual process (e.g., System 1 and System 2; Evans, 2006), depth of processing (Craik & Lockhart, 1972), and metacognition models. A popular account is the metacognitive account of perpetual disfluency (Alter, 2013; Alter et al., 2007; Diemand-Yauman et al., 2011). This account refers to the idea that the difficulty encountered during encoding, as a result of perceptual disfluency, forces more System 2 processing, which is slow, effortful, and deep. What is critical here is not the objective disfluency of the material but the subjective disfluency—that is, the experience of disfluency. It is the experience of disfluency that is hypothesized to stimulate metacognitive processes (monitoring and control), which serves to strengthen memory. It is also important to note that this account does not differentiate between disfluency manipulations (Weissgerber et al., 2017). That is, anything perceived as disfluent should engender better memory.

An alternative account is the compensatory processing account (Hirshman et al., 1994; Mulligan, 1996). The compensatory processing account is heavily influenced by a classic word recognition model—the interactive activation model (McClelland & Rumelhart, 1981). Within

the compensatory processing account, the disfluency effect is tied to processes occurring *during* the word identification process. Specifically, difficulty in identifying a stimulus increases the amount of top-down feedback from a higher-level (i.e., lexical/semantic) to a lower-level (i.e., features and orthography). Strong evidence for this account comes from studies using masking to impede word recognition. Masking involves presenting a word quickly (e.g., around 100 ms) and masking it with either forward or backward hash marks (Narine, 1988). The rapid presentation of the word along with the presentation of the mask renders visual information insufficient to recognize the word correctly, increasing higher-level processing. It is this feedback that results in better memory for stimuli. While further research is needed, an important commonality among the metacognitive account and compensatory processing account is the importance of higher-level semantic or metacognitive processes in producing the positive effects of perceptual disfluency on memory.

Disfluency and Sans Forgetica: A Potential Moderator

The literature reviewed here provides ample evidence that presenting materials in perceptually degraded formats can enhance memory and learning outcomes and act as a desirable difficulty, but also that the effect may be fickle. This fickleness has led to the exploration of different moderating or boundary conditions of the perceptual disfluency effect.

A recent publication demonstrated that Sans Forgetica can be optimal for learning, but only for those with high spelling ability. Monitoring eye movements, Eskenazi and Nix (2020) had participants learn the spelling and meaning for low-frequency words presented in sentences. For half the participants, the to-be-learned material was presented in Sans Forgetica, while for the other half, the material was presented in a more fluent typeface. During the test phase, orthographic discriminability (i.e., choosing the correct spelling of a word) and semantic acquisition (i.e., retrieving the definition of a word) were assessed. Critically, the authors

reported that Sans Forgetica was indeed perceptually disfluent (i.e., the gaze duration was longer in the Sans Forgetica condition) and that it had a positive effect on memory. However, spelling ability moderated this effect: only good spellers benefited from Sans Forgetica.

Though the authors argue spelling ability moderates the perceptual disfluency effect with Sans Forgetica, there is another possibility. Probing into Eskenazi and Nix's (2020) design features, one critical difference between their design and a recent failure to replicate (Geller et al., 2020) was test expectancy. Eskenazi and Nix (2020) surprised participants with the orthographic and semantic tests, whereas Geller et al. (2020) explicitly told participants about the upcoming memory test. One common feature of studies showing a desirable effect of perceptual disfluency is low test expectancy (e.g., Geller et al., 2018; Hirshman & Mulligan, 1991; Mulligan, 1996; Mulligan, 1998, Experiments 1-3, Hirshman et al., 1994, Experiments 1-3; Westerman & Greene, 1997; but see Rosner et al., 2015, Experiment 3a; Sungkhasettee et al., 2011). Accordingly, it is crucial to examine the role of test expectancy in relation to the perceptual disfluency effect and Sans Forgetica.

Test expectancy is known to exert a positive influence on memory. While test expectancy can be assessed with different methods (c.f., Dunlosky & Thiede, 1998), here, it refers to whether or not participants are told about an upcoming test. Expecting a test of any kind can lead to enhanced processing of studied material by either reducing learners' mind-wandering during studying (Szpunar et al., 2007) or by reducing interference from previously studied information (Weinstein et al., 2014). In the context of perceptual disfluency effects, Eitel and Köhl (2016) reasoned that if the disfluency effect arises because of deeper, more effortful processing, telling participants about a memory test should eliminate the effect. The eradication of the disfluency effect occurs because test expectancy countervails the benefit of perceptual disfluency by eliciting enhanced processing for both fluent and disfluent stimuli.

In contrast, low test expectancy is less likely to impact the processing of individual items, leaving effects of processing difficulty intact. While Eitel and Köhl found evidence for a general test expectancy effect (better memory for high vs. low test expectancy), they were unable to find an overall disfluency effect, nor did they find evidence that test expectancy moderated the disfluency effect. Following up on this, Geller and Still (2018) demonstrated that test expectancy could moderate the disfluency effect with a masking manipulation. Looking at the impact of item-by-item judgments of learning (JOLs; i.e., JOLs given after each item is studied) and list-wide JOLs (i.e., JOLs given after all the items are studied), they found a disfluency effect only when test expectancy was low and participants provided list-wide JOLs. Given this, it is possible the failure to find some disfluency effects (such as with Sans Forgetica) might only arise under low test expectancy. The current experiments more directly test this hypothesis.

The Current Experiments

The empirical work reported here was designed to investigate the effect of Sans Forgetica on memory for words and whether observation of a perceptual disfluency effect depends on test expectancy. If test expectancy moderates the disfluency effect, it would have important theoretical implications for researchers working in this domain. Further, it would support accounts suggesting that encoding difficulty brought forth by perceptual disfluency arises from an attentional mechanism that leads to deeper, more effortful processing. Conversely, the failure to find a disfluency effect would further drive the nail into the coffin of perceptual disfluency as a desirable difficulty.

Experiment 1

In Experiment 1, we examined whether the impact of Sans Forgetica on memory is moderated by test expectancy. Using an old/new recognition test, we manipulated test expectancy by alerting only half the participants that their memory was to be assessed. In addition, we collected list-wide JOLs for each typeface and study durations to examine the subjective and objective influence of Sans Forgetica on encoding. The choice to use list-wide JOLs was influenced by recent findings suggesting a reactive effect of item-level JOLs on memory (Janes et al., 2018; Myers et al., 2020; Soderstrom et al., 2015). The very act of making a JOL for each word serves to mitigate the beneficial effects of perceptual disfluency on memory (Besken & Mulligan, 2013).

In our preregistration, we predicted an interaction between Typeface (Arial vs. Sans Forgetica) and Test Expectancy. Specifically, we anticipated seeing a memory boost for Sans Forgetica, but only under low test expectancy (e.g., Geller et al., 2018; Hirshman & Mulligan, 1991; Mulligan, 1996). Further, we predicted that we would not see JOL differences as a function of Typeface or Test Expectancy. This prediction was based off prior studies looking at the influence of Sans Forgetica on JOLs (Geller et al., 2020, Experiment 2; Taylor et al., 2020).¹ Finally, concerning study times, we predicted we would see longer study times for Sans Forgetica, but only in the low test expectancy group.

¹ It is possible, however, that with the within-subject manipulation of fluency we will see effects on JOLs (see Besken & Mulligan 2013; Geller et al., 2018; Rhodes & Castel, 2008, 2009

Method

The preregistered analysis plan for Experiment 1 can be found here: <https://osf.io/wgp9d>. All raw and summary data, materials, and R scripts for preprocessing, analysis, and plotting for Experiment 1 can be found at <https://osf.io/cqp6s/>.

Participants

We preregistered a sample size of 230. All participants were recruited through Prolific (prolific.co) and completed the study on the Gorilla platform (www.gorilla.sc; Anwyl-Irvine et al., 2020). Sample size was based on a priori power analyses conducted using PANGAEA v0.2 (Westfall, 2016; Geller et al., 2020). Sample size was calculated based on the smallest effect of interest (SEOI; Lakens & Evers, 2014). In a 2 (within) $\times$ 2 (between) mixed design, we were interested in powering our study to detect a medium-sized interaction effect ($d = 0.35$). We choose this effect size as our SEOI due to the small effect sizes seen in actual classroom studies (Butler et al., 2014). Therefore, assuming an alpha of .05 and the desired power of 90%, a sample size of 230 is required to detect whether an interaction effect size of 0.35 differs from zero. No participants met our preregistered exclusion criteria (i.e., did not complete the experiment, started the experiment multiple times, experienced technical problems, or reported familiarity with the stimuli). Data collection resulted in the collection of 231 participants. Participants were compensated for their time. We used Prolific's custom prescreening measures and included participants that resided in the United States, were native English speakers, had an approval rating between 80% and 100%, and had not participated in any prior studies conducted by the researchers.

Materials

Stimuli included 188 single-word nouns taken from Geller et al. (2018). All words were from the English Lexicon Project database (Balota et al., 2007). We controlled for both word

frequency (all words were high frequency; mean log HAL frequency = 9.2) and length (all words were four letters). The full set of stimuli can be found at <https://osf.io/dsxrc/>.

Design

Typeface (Arial vs. Sans Forgetica) was manipulated within-subjects and test expectancy was manipulated between subjects. Per our preregistration, d' , JOLs, and study times were analyzed with a 2 (Typeface: Arial vs. Sans Forgetica) $\times$ 2 (Test Expectancy: High vs. Low) mixed analysis of variance (ANOVA).

Procedure

Like Geller et al. (2020; Experiment 3), a total of 188 words were divided across four lists (94 words each; 47 in each typeface condition). Each word appeared in each 2 (old/new) $\times$ 2 (Arial/Sans Forgetica) condition. Counterbalancing these lists ensured that each word served equally often as a target and a foil in both typefaces across participants. In two lists, 94 words were chosen to be “old” (47 in Arial and 47 in Sans Forgetica), and 94 words were chosen to be “new” (47 in Arial and 47 in Sans Forgetica) and were only presented during the test phase. In the last two lists, items presented as “new” were presented as “old” and vice versa. Word order was randomized, such that Arial and Sans Forgetica words were randomly intermixed in the study phase, and Arial and Sans Forgetica old and new words were randomly intermixed in the test phase, with old words always presented in the same typeface at test as they were at study.

Participants were randomly assigned to one of two groups: the high test expectancy group or the low test expectancy group. Interested readers can view the entire task, including instructions for each condition, by following these links (high test expectancy experiment: <https://gorilla.sc/openmaterials/72765>; low test expectancy experiment:

287 <https://gorilla.sc/openmaterials/116227>). Those in the high test expectancy group received the
288 following study description: “In this study, your memory will be tested for words in different
289 typefaces. In the first part, you will study words. In the second part, your memory will be tested
290 for the words you studied.” They were also explicitly told before the experiment that there would
291 be an upcoming memory test. In the low test expectancy group, participants received a different
292 study description: “In this study, you will be reading words in different typefaces.” The
293 instructions before the experiment proper made no mention of any memory test.

294 The experiment consisted of four phases: encoding phase, JOL phase, distractor phase,
295 and test phase. During the encoding phase, a fixation cross appeared at the center of the screen
296 for 500 ms. A word then replaced the fixation cross in the same location. To continue to the next
297 trial, participants pressed the “continue” button at the bottom of the screen. Thus, each trial was
298 self-paced. Though the presentation of the words was a single, heterogeneous mix of Arial and
299 Sans Forgetica words, the JOL phase required participants to provide two list-wide JOLs wherein
300 they retrospectively judged on a scale from 0 (not at all likely) to 100 (most likely) how
301 successful they would recall words presented in Arial and Sans Forgetica. Then, during a three-
302 minute distractor, participants wrote down as many US state capitals as possible. Finally,
303 participants were given an old/new recognition memory test. During the test phase, a word
304 appeared in the center of the screen that either had been presented during encoding (“old”) or had
305 not been presented (“new”). Old words occurred in their original typeface, and following the
306 counterbalancing procedure, each of the new words was presented in either Arial typeface or
307 Sans Forgetica typeface. All words were individually randomized for each participant during both
308 the study and test phases, and progress was self-paced. After the experiment, participants were
309 debriefed. The entire experiment lasted approximately 15 minutes.

Analysis

For all experiments reported in this paper, we employed a 2 (typeface, manipulated within-subjects) $\times$ 2 (test expectancy, manipulated between-subjects) mixed ANOVA. We report a variation of Cohen's d (d_{avg} ; Buchanan et al., 2019) and generalized eta-squared (η_g^2 ; Olejnik & Algina, 2003) as measures of effect size. Alongside traditional analyses that utilize null hypothesis significance testing (NHST), we also report the Bayes Factor (BF) for reported null effects. As a rule of thumb, BFs ≥ 3 provide substantial evidence, while BFs ≥ 10 provide strong evidence for one model over another model (Jarosz & Wiley, 2014). All data were analyzed in R (vers. 4.0.2; R Core Team, 2020), with models fit using the *afex* (vers. 0.27-2; Singmann et al., 2020) and *BayesFactor* packages (vers. 0.9.12-4.2; Morey & Rouder, 2018). All figures were generated using *ggplot2* (vers. 3.3.0; Wickham, 2006). We used the *ggpirate* package (vers. 0.1.2; Braginsky, 2021) for the pirate plots. See the appendix for a list of all R packages used.

Results and Discussion

Recognition Memory

Performance was examined with d' , a memory sensitivity measure derived from signal detection theory (Macmillan & Creelman, 2005). Hits and false alarms at the ceiling/floor were changed to .99 or .01, respectively. Figure 2a presents d' values along with difference scores (Figure 2b). The analysis revealed that when told about a memory test, participants had better discriminatory ability than those not told about a memory test, $M_{\text{diff}} = 0.16$, $F(1, 229) = 4.11$, $p = .04$, $\eta_g^2 = .014$. Individuals were better at discriminating target words presented in Sans Forgetica than Arial, $M_{\text{diff}} = 0.12$, $F(1, 229) = 10.73$, $p = .001$, $\eta_g^2 = .010$. This was qualified by an interaction between Test Expectancy and Typeface, $F(1, 229) = 4.34$, $p = .038$, $\eta_g^2 = .004$. Planned comparisons showed that individuals in the low test expectancy group had better

recognition memory for words presented in Sans Forgetica compared to Arial, $F(1, 229) = 14.297, p < .001, d_{\text{avg}} = 0.31$. In the high test expectancy group, there was substantial evidence for no difference between typefaces, $F(1, 229) = 0.716, p = .398, d_{\text{avg}} = 0.07, \text{BF}_{01} = 5.83$.

JOLs

In Figure 1, JOL responses (Figure 1c) and difference scores (Figure 2d) are presented. We excluded seven participants for not providing JOLs to each typeface. Using the same model as above, participants in the high test expectancy group gave higher JOLs than the low test expectancy group, $M_{\text{diff}} = 16.2, F(1, 221) = 16.01, p < .001, \eta_g^2 = .065$. Arial elicited higher JOLs than Sans Forgetica, $M_{\text{diff}} = 4.0, F(1, 221) = 27.05, p < .001, \eta_g^2 = .004$. There was no interaction between Test Expectancy and Typeface, $F(1, 221) = 0.13, p = .715, \eta_g^2 < .001$. Compared to a main effects-only model, there was substantial evidence for no interaction ($\text{BF}_{01} = 7.28$).

Study Times

Although not preregistered, we removed study times less than 150 ms and study times greater than 2.5 SD above the mean per condition for each participant. This outlier procedure removed ~3 % of the data.² Given the data's heavy positive skew, we log-transformed study times to better approximate a normal distribution (see Fig.2e). Evidence for test expectancy effects on log-transformed study times were inconclusive, $F(1, 229) = 1.97, p = .162, \eta_g^2 = .008, \text{BF} = 1.822$. However, typeface did influence study time: study times were slower for Sans

² The decision to omit these observations did not meaningfully impact any of the conclusions reported here.

Forgetica than Arial, $F(1,229) = 30.91, p < .001, \eta_g^2 = .001$. There was no interaction between Test Expectancy and Typeface, $F(1,229) = 1.10, p = .296, \eta_g^2 < .001$. Compared to a main effects-only model, there was substantial evidence that there was no interaction between Test Expectancy and Typeface ($BF = 5.25$).

Figure 2

A. Pirate plots showing raw data (individual points), bean density, and central tendency (mean is shown as a black line on bars for memory sensitivity (d') as a function of Typeface and Test Expectancy in Experiment 1. B. Violin plots for sensitivity difference scores, with labeled means and bootstrapped 95% CIs, as a function of Test Expectancy in Experiment 1. C. Pirate plots showing raw data (individual points), bean density, and central tendency (mean is shown as a black line on bars) of JOLs as function of Typeface and Test Expectancy in Experiment 1. D. Violin plots for JOL difference scores, with labeled means and bootstrapped 95% CIs, as a function of Test Expectancy in Experiment 1. E. Pirate plots showing raw data (individual points), bean density, and central tendency (mean is shown as a black line on bars) for study times (log-transformed) as a function of Typeface and Test Expectancy in Experiment 1. F. Violin plots for study time difference scores, with labeled means and bootstrapped 95% CIs, as a function of Test Expectancy in Experiment 1.

369

370

As predicted, we observed a significant interaction between typeface and test expectancy. Specifically, participants were better able to discriminate words in Sans Forgetica compared to Arial, but only with low test expectancy. The moderating effect of test expectancy provides one potential explanation for why Geller et al. (2020, Experiment 1) failed to find a disfluency effect with high test expectancy. We also found subjective evidence that Sans Forgetica is perceptually disfluent. Participants gave lower JOLs to stimuli studied in the Sans Forgetica typeface, regardless of test expectancy. That is, despite the novel typeface improving recognition memory, participants subjectively rated it as an inferior context for word learning. These findings are inconsistent with the predictions preregistered and contradict the findings of Geller et al. (2020, Experiment 2) and Taylor et al. (2020, Experiment 1). One possible reason for this is that in the current experiment, we used a within-subject manipulation of typeface, whereas Geller et al. and Taylor et al. used a between participants typeface manipulation. The finding of lower JOLs to disfluent stimuli is in line with other studies using a within-participant manipulation of fluency (Besken & Mulligan 2013; Geller et al., 2018; Rhodes & Castel, 2008, 2009).

Concerning study times (the more objective measure of disfluency), we found that regardless of test expectancy, participants studied words presented in Sans Forgetica longer than words presented in Arial, contradicting our prior research (Geller et al., 2020; Experiment 3). It is important to note, however, that the examination of study times in our previous study was unplanned and purely exploratory, making it difficult to draw conclusions about this discrepancy. In considering possible explanations for this inconsistency, we explored whether our decision not to correct for the skew of raw data or omitting outliers led to the null effect of study time observed in Geller et al. (2020, Experiment 3). Reanalyzing these study time data with a similar

procedure to the one outlined above showed longer study times for Sans Forgetica ($p = .049$, one-tailed).

The finding that test expectancy moderates the disfluency effect in recognition contradicts Rosner et al.'s (2015, Experiment 3a) study that used a high-level blur manipulation and manipulated test expectancy but did not find the same interaction. Given the novelty of the current findings, in Experiment 2, we attempted to conceptually replicate this pattern of results using a different criterion test: cued recall.

Experiment 2

In Experiment 2, we decided to examine a different paradigm from Geller et al. (2020; Experiment 1) that employed weakly related cue-target pairs assessed via cued recall. In that experiment, there was strong evidence against there being a Sans Forgetica effect ($BF > 100$) but critically, participants were explicitly informed their memory was to be tested. In the present experiment, we examined whether this null effect persists regardless of test expectancy.

Method

The preregistered analysis plan for Experiment 2 can be found here: <https://osf.io/3xak9>. All raw and summary data, materials, and R scripts for preprocessing, analysis, and plotting for Experiment 2 can be found at <https://osf.io/cqp6s/>.

Participants

We preregistered and collected a sample size of 232 participants. Participants were recruited on Amazon's Mechanical Turk (MTurk) platform, all of whom completed the experiment through Pavlovia (pavolvia.org). As in Experiment 1, we only included participants

that resided in the United States, were native English speakers, had an approval rating between 80% and 100%, and had not participated in any prior studies conducted by the researchers.

Design

Accuracy, JOLs, and study times were analyzed with a mixed factorial design, with typeface (Arial vs. Sans Forgetica) manipulated within-participants and test expectancy (high vs. low) manipulated between participants.

Materials and Procedure

Experiment 2 was programmed in PsychoPy (Peirce et al., 2019) and hosted on Pavoliva (pavolvia.org). The materials were adapted from Geller et al. (2020, Experiment 1; also see Carpenter et al., 2006). Participants were presented with 24 weakly related cue-target pairs. The pairs were all nouns, 5–7 letters and 1–3 syllables in length, high in concreteness (400–700), high in frequency (at least 30 per million), and had similar forward ($M = 0.031$) and backward ($M = 0.033$) association strengths. Two counterbalanced lists were created for each testing condition (high and low test expectancies) so that each target could be presented in each typeface condition (Arial vs. Sans Forgetica) without repeating any items for an individual participant.

A version of the experiment can be run by following the following link:
https://run.pavlovvia.org/Jgeller112/sf_low_cb1. The experiment consisted of four phases: encoding phase, JOL phase, distractor phase, and test phase. As in Experiment 1, some participants were told about an upcoming memory test while others were not. During the encoding phase, each participant was randomly presented with a series of word pairs, one at a time with the cue always presented in Arial on the left-hand side from the center and the target word presented in Sans Forgetica or Arial on the right-hand side from the center. Typefaces of the target words were randomly intermixed. The encoding phase was self-paced: participants

were instructed to press a button on the screen after reading each word. Like in Experiment 1, participants then made two list-wide JOLs. Following a short distractor task (3 min), participants were given a cued recall test which began with instructions. Each trial started with participants being presented cues from the encoding phase, one at a time, in lowercase letters. Participants were instructed to type in the corresponding target or to guess if they could not remember. The test phase was self-paced. All cues were presented in Arial font. The entire experiment lasted approximately 10 minutes.

Scoring

The *lrd* package in R (Maxwell et al., 2020) automatically scored typed responses. The *lrd* package provides an automated way to score word responses. We used a partial match threshold of 80% to determine whether a typed response was correct or not.

Results and Discussion

Cued Recall

Like Taylor et al. (2020), we were interested in how Sans Forgetica enhances memory. To answer this question, we calculated the proportion of Sans Forgetica targets correctly recalled by participants and the proportion of Arial targets correctly recalled by participants. Figure 3 shows performance on the cued recall test (Figure 3a) along with difference scores (Figure 3b). Participants in the high test expectancy group performed better than participants in the low test expectancy group, $M_{\text{diff}} = .20$, $F(1, 230) = 38.26$, $p < .001$, $\eta_g^2 = .126$. Participants recalled more target words in Sans Forgetica than Arial, $M_{\text{diff}} = .05$, $F(1, 230) = 13.57$, $p < .001$, $\eta_g^2 = .008$. This was qualified by an interaction between Test Expectancy and Typeface, $F(1, 230) = 10.74$, $p = .001$, $\eta_g^2 = .006$. A Bayesian analysis revealed that the interaction model was strongly preferred to the full model ($\text{BF} = 21.77$). Planned comparisons showed that individuals in the low test

expectancy group recalled more words presented in Sans Forgetica than Arial, $t = 4.92$, $p < .001$, $d_{avg} = 0.36$, 95 % CI [-0.55, -0.18]; In the high test expectancy group, there was substantial evidence that there was no difference between Sans Forgetica and Arial, $t = 0.287$, $p = .778$, $d_{avg} = 0.03$, 95 % CI [-0.21, 0.16], $BF_{01} = 9.31$.

JOLs

Figure 3 shows participant-level JOLs (Figure 3b) and difference scores (Figure 4c). Using the same model as above, participants in the high test expectancy group gave higher JOLs than those in the low test expectancy group, $M_{diff} = 5.91$, $F(1,229) = 13.57$, $p < .001$, $\eta_g^2 = .028$. Arial elicited higher JOLs than Sans Forgetica, $M_{diff} = 15.15$, $F(1,229) = 87.05$, $p < .001$, $\eta_g^2 = .161$. There was an interaction between Test Expectancy and Typeface, $F(1,229) = 13.65$, $p < .001$, $\eta_g^2 < .029$. A Bayesian analysis revealed that the interaction model was strongly preferred to the main effects-only model ($BF > 100$). Planned comparisons revealed that the JOL effect was larger in the low test expectancy group ($d_{avg} = 1.65$, 95 % CI [1.37, 1.93]) than in the high test expectancy group ($d_{avg} = 0.72$, 95 % CI [0.51, 0.92]).

Study Times

Figure 3 shows log-transformed study times (Figure 3e) and difference scores (Figure 3f). Like Experiment 1, we excluded study times less than 150 ms and study times greater than 2.5 SD above the mean per condition for each participant. The outlier procedure removed ~2% of the data. Study times were overall larger for the high test expectancy group compared to the low test expectancy group, $M_{diff} = 0.34$, $F(1,230) = 17.02$, $p < .001$, $\eta_g^2 = .068$. Cue-target pairs yielded longer study times for Sans Forgetica compared to Arial, $M_{diff} = 0.06$, $F(1,230) = 27.74$, $p < .001$, $\eta_g^2 = .002$. There was no interaction between Test Expectancy and Typeface, $F(1,230) = 0.39$, $p =$

483 .533, $\eta_g^2 < .001$. A main effects-only model was strongly preferred over the interaction model (BF
484 = 6.03).

485 **Figure 3**

486 *A. Pirate plots showing raw data (individual points), bean density, and central tendency (mean is*
487 *shown as a black line on bars) for cued recall (proportion correct) as a function of Typeface and*
488 *Test Expectancy in Experiment 2. B. Violin plots for cued recall difference scores on the final*
489 *test, with labeled means and bootstrapped 95% CIs, as a function of Test Expectancy in*
490 *Experiment 2. C. Pirate plots showing raw data (individual points), bean density, and central*
491 *tendency (mean is shown as a black line on bars) for JOLs as a function of Typeface and Test*
492 *Expectancy in Experiment 2. D. Violin plots for JOL difference scores, with labeled means and*
493 *bootstrapped 95% CIs, as a function of Test Expectancy in Experiment 2. E. Pirate plots showing*
494 *raw data (individual points), bean density, and central tendency (mean is shown as a black line*
495 *on bars) for study times (log-transformed) as a function of Typeface and Test Expectancy in*
496 *Experiment 2. F. Violin plots for study time difference scores, with labeled means and*
497 *bootstrapped 95% CIs, as a function of Test Expectancy in Experiment 2.*

498

499

The results complement those from Experiment 1 and suggest the disfluency effect elicited by Sans Forgetica is not unique to a particular criterion test. Using cued recall, we once again demonstrated that Sans Forgetica could constitute a desirable difficulty, but only when test expectancy is low. Notably, the effect we observed was modest; Sans Forgetica conferred roughly a 5% increase in cued recall performance above and beyond Arial, a more fluent typeface. When looking at the low test expectancy group alone, we observed a 9% increase. We once again found longer study times and lower JOLs for words studied in Sans Forgetica. There are two points of divergence that merit mention. Compared to Experiment 1, JOLs for the Sans Forgetica condition were tightly bunched around the middle of the response scale.³ This could reflect participants' uncertainty around how well they would remember Sans Forgetica target words. Additionally, study times for Sans Forgetica were longer for the high test expectancy group compared to the low test expectancy group. This most likely reflects participants studying word pairs longer in preparation for an upcoming test.

Experiment 3

In Experiments 1 and 2, we observed a benefit for Sans Forgetica under low test expectancy. Although this result constitutes an example of a desirable difficulty effect resulting from perceptual disfluency, the mechanisms underlying such effects remain an open issue. Our preferred interpretation is that encoding difficulty from the typeface is an attentional response eliciting deeper processing that leads to better remembering. However, another possible explanation is that Sans Forgetica is remembered better simply because participants spend more

³ Experiments 2 and 3 used a slider scale that ranged from 0-100 in increments of 10 while Experiment 1 had participants type in a number between 0-100.

time processing words in Sans Forgetica, as indexed by slower study times during encoding in both Experiments 1 and 2⁴. To examine if time-on-task can account for the desirable effect of Sans Forgetica on recall, we manipulated time spent encoding by having participants either encode stimuli at their own pace (self-paced) or by removing control over the duration of study. If time-on-task moderates the Sans Forgetica effect, we expect an attenuated effect on memory when time is constrained to be equal between Arial and Sans Forgetica. However, when encoding is self-paced, we expected better memory for Sans Forgetica compared to Arial. Corroborating this, Köhl et al. (2014) showed that self-paced study produced better learning outcomes compared to constrained study time. Because of this, we hypothesized that we would observe a disfluency effect for Sans Forgetica only when study time was self-paced.

In Experiment 3, we chose to keep test expectancy low and only manipulate time-on-task (self-paced vs. 3 s)⁵. This design also served to replicate the novel findings from Experiment 2, showing that low test expectancy is essential for the Sans Forgetica memory effect. Also, we again examined list-wide JOLs.

Method

⁴ A simple time-on-task account does a poor job of explaining the lack of a Sans Forgetica effect we observed in Experiments 1 and 2 when participants were told about a memory test.

⁵ Three seconds was chosen by looking at overall study times for Experiment 2 ($M = 2,192$ ms). Given this, we thought 3 s would be more than sufficient to allow identification of the cue-target pairs.

The preregistration for this experiment can be found here: <https://osf.io/hjnk5>. All raw and summary data, materials, and R scripts for preprocessing, analysis, and plotting for Experiment 3 can be found at <https://osf.io/cqp6s/>.

Participants

We preregistered and collected a sample size of 232 participants. Participants were recruited on Prolific⁶, all of whom completed the experiment through Pavlovía ([pavolvía.org](http://pavlovvia.org)). Using prescreening questionnaires on Prolific, we limited our sample to participants residing in the USA, native English speakers, and who no record of participating in previous studies by the first author.

Design, Materials, and Procedure

The design, materials, and procedure are identical to Experiment 2, with one exception. Instead of manipulating test expectancy (all participants were naïve to the impending memory test), we manipulated study time (self-paced vs. 3 s) between participants. In the self-paced group (like in Experiment 2), participants spent as much time as they wanted to process the cue-target pairs. In the 3 s group, cue-target pairs were presented for a fixed duration.

Results and Discussion

Cued Recall

Figure 4 shows performance on the cued recall test (Fig. 4a) along with difference scores (Figure 4b). The analysis revealed that there was no reliable difference between the self-paced

⁶ In our pre-registration, we indicated we would collect data on MTurk, but we opted to collect data on Prolific because <insert reason here>.

and timed groups on cued recall, $M_{\text{diff}} = 2\%$, $F(1, 230) = 0.369$, $p < .544$, $\eta_g^2 = .055$. Individuals were better at recalling target words presented in Sans Forgetica than Arial, $M_{\text{diff}} = 5\%$, $F(1, 230) = 15.03$, $p < .001$, $\eta_g^2 = .013$. There was no interaction between Time-on-Task and Typeface, $F(1, 230) = 1.13$, $p = .289$, $\eta_g^2 < .001$. A Bayesian analysis revealed that a main effects-only model was preferred to the interaction ($\text{BF} = 5.50$).

JOLs

Figures 4 shows participant-level JOLs (Figure 4c) and difference scores (Figure 4d). Using the same model as above, participants in the timed group gave higher JOLs than in the self-paced group, $M_{\text{diff}} = 5.91$, $F(1, 230) = 17.43$, $p < .001$, $\eta_g^2 = .055$. Arial elicited higher JOLs than Sans Forgetica, $M_{\text{diff}} = 9.7$, $F(1, 230) = 48.81$, $p < .001$, $\eta_g^2 = .048$. There was an interaction between Time-on-Task and Typeface, $F(1, 230) = 27.17$, $p < .001$, $\eta_g^2 < .027$. A Bayesian analysis revealed that the interaction model was strongly preferred to the main effects-only model ($\text{BF} = 57.24$). A simple effects analysis revealed that the JOL effect (Arial < Sans Forgetica) was larger in the self-paced group ($d_{\text{avg}} = 1.22$, 95 % CI [0.98, 1.46]) than in the timed group ($d_{\text{avg}} = 0.10$, 95 % CI [-0.08, 0.28], $\text{BF}_{01} = 1.466$).

Study Times

For completeness, study times for the self-paced group were examined using the same cleaning procedure as Experiments 1 and 2. Similar to Experiments 1 and 2, we found once again that participants studied pairs in the Sans Forgetica condition longer than pairs in the Arial condition, $M_{\text{diff}} = 0.06$, $t(115) = 4.11$, $p < .001$, $d_{\text{avg}} = 0.11$, 95% CI [-0.08, 0.29].

Taken together, the results from Experiment 3 are clear. With low test expectancy, cued recall performance was better overall for Sans Forgetica targets—it did not matter whether encoding was self-paced or timed. The failure to find better memory for the self-paced condition

contradicts a study by Köhl et al. (2014). It is important to note that our study used simple learning materials, whereas Köhl et al. used more complex materials (multimedia slides). With more complex materials, a time constraint might hurt rather than aid recall. Additionally, the time allotted to study items (3 s) was more than sufficient to encode the pairs. In Köhl et al., the allotted time may have been too short. Despite this, the findings from Experiment 3 replicated the results from Experiment 2, showing that Sans Forgetica enhances memory under fixed-paced (3 s) and self-paced study. Importantly, a simple time-on-task account cannot account for this pattern of results as it predicts superior memory for whichever material is studied longer. Specifically, when participants were free to spend as much time as they liked with the stimuli, the perceptual disfluency effect should have emerged (because participants spontaneously spend more time with the information presented in Sans Forgetica) but when encoding time was held constant, the effect should have disappeared. We did not observe this critical interaction.

Figure 4

A. Pirate plots showing raw data (individual points), bean density, and central tendency (mean is shown as a black line) for cued recall as a function of Typeface and Time-on-Task in Experiment 3. B. Violin plots for JOL difference scores, with labeled means and bootstrapped 95% CIs, as a function of Time-on-Task in Experiment 3.

Turning to JOLs, we replicated the outcomes from Experiments 1 and 2, showing lower JOLs for Sans Forgetica than Arial. This difference was larger in the self-paced group than in the timed group. One possible explanation is that individuals are more uncertain about whether they will remember disfluent targets during self-paced encoding because they were able to proceed at

their own pace, and were not restricted by a time limit. This fact is highlighted by JOLs in that condition clustering around the middle point of the scale.

General Discussion

Sans Forgetica has recently garnered substantial attention from both the media and the scientific community. The present experiments attempted to reconcile mixed findings regarding Sans Forgetica and, more broadly, perceptual disfluency. Following up on recent calls to examine boundary conditions of the perceptual disfluency effect (e.g., Bjork & Yue, 2016; Dunlosky & Mueller, 2016), we focused on one boundary condition: test expectancy. We found evidence that test expectancy moderates the perceptual disfluency effect; Sans Forgetica improved memory (regardless of final test format) but only when participants did not expect a test. Experiment 3 revealed these outcomes could not be explained with a simple time-on-task explanation. Further, we found Sans Forgetica produced lower JOLs and longer study times across Experiments 1 and 2.

These outcomes conflict with some recent findings. First, Rosner et al. (2015, Experiment 3a) did not find a moderating role for test expectancy in recognition memory using a high-level blur manipulation—low and high test expectancy elicited a similar benefit. It is worth noting that these findings have not yet been replicated. In the current set of experiments, we demonstrated a robust effect of test expectancy across different test formats (Experiments 1 and 2) and replicated the basic disfluency effect with low test expectancy (Experiment 3). Assuming Rosner et al.'s finding would replicate, one possible explanation of this discrepancy is that different disfluency manipulations can have differential effects on memory. In Rosner et al., some participants saw a high-level blur (which elicited an effect) whereas others saw a low-level blur (which did not elicit any effect). Curiously, Geller et al. (2018) showed the opposite pattern of results; an easy-to-read

cursive resulted in better memory outcomes than a difficult-to-read cursive. In short, it's possible that those manipulations wherein perceptual disfluency is manipulated at different difficulty levels along a continuum might operate differently than Sans Forgetica.

Second, while we found a general benefit of Sans Forgetica under low test expectancy, Eskenazi and Nix (2020) only found a memory benefit for Sans Forgetica among those participants who were strong spellers. Better spellers are thought to have a more precise mental lexicon which allows for more efficient processing at multiple levels of representation (i.e., orthographic, phonological, and semantic; Perfetti, 2007). When confronted with perceptual degradation, better spellers would be able to process a stimulus at a deeper level, which could give rise to better memory. The disparate findings may be reconciled by the fact that we used high-frequency words in all three experiments. These words were likely well known to the participants, allowing perceptual disfluency to be desirable for learning.

Perceptual Desirable Difficulty: A Time-on-Task Effect?

The most interesting result is that Sans Forgetica, a perceptually disfluent typeface, was associated with better recognition and recall, but only with low test expectancy. As reviewed earlier, researchers generally agree that perceptual disfluency enhances memory via deeper, more effortful processing. An alternative explanation is that perceptually disfluent stimuli take longer to process and this extended encoding time is solely responsible for the observed mnemonic benefits. The results reported here argue strongly against this view. In both Experiments 1 and 2, Sans Forgetica produced longer study times, yet there was strong evidence that there was a null perceptual disfluency effect in the high test expectancy group. Critically, in Experiment 3, where we directly tested a time-on-task account by manipulating whether the cue-target pairs were presented at a fixed pace or not, we found robust effects of perceptual disfluency on cued recall regardless of pacing. While the perceptual disfluency effect was indeed larger under a time

constraint than when encoding was self-paced (possibly due to longer study time), we did not observe an interaction between study format and typeface.

Other research similarly argues against a simple time-on-task account. In Geller et al. (2018), for example, the authors showed that while hard-to-read cursive words engendered longer naming latencies, the memory benefit was weaker than with easy-to-read cursive words. Similarly, Rosner et al. (2015) showed that while a low-level blur manipulation was perceptually disfluent (longer naming latencies), it did not enhance memory at test. In contrast, a higher level of perceptual blur both increased naming latencies and enhanced recognition memory. These results suggest that perceptual degradation affects naming times in the encoding phase in a continuous manner, but perceptual degradation at the time of encoding must surpass some threshold to induce processing that enhances encoding and subsequent memory.

Theoretical Mechanisms of the Perceptual Disfluency Effect

If perceptual disfluency is not driven by time-on-task, then the mechanism warrants further study. The current findings add to our understanding of the mechanisms underlying perceptual disfluency's desirable effects on memory. Eitel and Köhl (2016) postulated that if Sans Forgetica is a desirable difficulty, it fosters learning by increasing mental effort and stimulating deeper processing. When preparing for an upcoming test (under high test expectancy), there is an increased investment of effort allocated to the material, regardless of whether the information is fluent or disfluent—which would attenuate the effects of disfluency. Looking at both test expectancy groups (see Figures 2b and 3b), there is some evidence for this process. In both groups, recognition memory and cued recall were generally higher for Sans Forgetica, suggesting those stimuli received deeper processing. In contrast, the processing of Arial words appeared to be shallower in the low test expectancy group. This means that with high

test expectancy, all words get deeper processing, resulting in a more negligible difference in the high test expectancy group.

Given that high test expectancy eradicated the mnemonic benefit of Sans Forgetica, this points to a similar mechanism of action—deeper processing during encoding. Just how this processing is carried out is still subject to debate. Geller et al. (2018) presented participants with varying levels of handwritten cursive stimuli (easy-to-read and hard-to-read) to adjudicate between current accounts of perceptual disfluency (i.e., metacognitive and compensatory processing accounts). From a metacognitive perspective, the memory benefit should be equal for easy-to-read and hard-to-read cursive words—within that account, all disfluency types are created equal (Weissgerber et al., 2017). However, the compensatory processing account suggests that the memory benefit should be greater for hard-to-read cursive stimuli. This is because during word identification, hard-to-read cursive requires more lexical/semantic processing, which benefits recall (Perea et al., 2016).

In contrast to both accounts, Geller et al. found that easy-to-read cursive words were better remembered than hard-to-read cursive words, despite being read faster. This pattern is difficult for extant accounts to explain. Within their account, perceptual disfluency effects arise due to (1) increased processing difficulty during recognition (i.e., difficulty mapping letters to words) and (2) deeper processing that occurs after recognition, presumably as the result of some combination of semantic processing, metacognitive control, and regulatory components. This account can explain the lack of disfluency effect in the high test expectancy group as a result of increased metacognitive monitoring and control processes eliciting attention to both types of stimuli.

A more general framework that invokes cognitive monitoring and control, such as the conflict monitoring framework (Botvinick et al., 2001), might also explain the present findings

(for a similar discussion, see Geller et al., 2018; Rosner et al., 2015). Within this framework, the up- and down-regulation of monitoring and control are mediated by response ambiguity or conflict (in the current case, difficulty identifying the word). Under low test expectancy, Sans Forgetica would trigger greater control due to the difficulty associated with recognizing the stimulus—this serves to enhance memory. However, under high test expectancy, the goal is switched to one of remembering words for an upcoming memory test. While Sans Forgetica is still harder, monitoring and control processes are directed to both types of stimuli, dampening/weakening the disfluency effect. The exact mechanisms underlying perceptual disfluency remain an open issue.

Practical Implications

The current findings have some educational significance. While it might be tempting to conclude from these findings that Sans Forgetica should be used as a study tool, the present results need to be interpreted with caution. First, and most importantly, the conclusion that Sans Forgetica is only beneficial to memory under low test expectancy makes its use in the educational domain impractical. In the classroom, students rarely encode information incidentally; learning is always purposeful and goal-directed. Second, our experimental paradigms involved simple list learning. It is unclear if Sans Forgetica would benefit from learning under low test expectancy with more complex materials. Some evidence from Taylor et al. (2020, Experiments 3 and 4) suggests it might not. In those experiments, memory for factual and conceptual information in more educationally realistic materials (prose passages) displayed no mnemonic advantage for Sans Forgetica. Thus, even with low test expectancy, Sans Forgetica did not enhance memory when the material was educationally realistic.

Third, the effect sizes from all three experiments were modest by conventional standards (Cohen, 1977; Funder & Ozer, 2019): Experiment 1 - $d_{avg} = 0.31$; Experiment 2 - $d_{avg} = 0.38$;

Experiment 3 - timed: $d_{avg} = 0.32$; self-paced: $d_{avg} = 0.15$. It is unclear if the Sans Forgetica effect would replicate in educational settings where the effect sizes are a lot smaller and more variable (Butler et al., 2014).

Third, there is a fair amount of variability in the number of participants that benefited from perceptual disfluency. The difference scores presented in Figures 2, 3, and 4 highlight this nicely—the presentation of Sans Forgetica does indeed help most participants, but it actually hurts a sizeable minority. Before we start recommending perceptual disfluency as a potential study tool, it is critical we better understand the nature of these individual differences.

We do acknowledge, however, that Sans Forgetica might have some practical implications. Outside the classroom, information is primarily acquired incidentally, without the goal of memorization (Castel et al., 2015). If this is the case, the information presented in Sans Forgetica might serve to enhance memory indirectly. For instance, one area where perceptual disfluency might be desirable is in advertising where we often acquire visual information incidentally (e.g., via billboards, online advertisements, and magazines).⁷ Placing this type of information in a perceptually disfluent typeface like Sans Forgetica might be helpful. Further research, of course, would be needed to support such speculation.

Conclusions

Recent reports have recommended that teachers and students use perceptual disfluency to enhance learning. Although we have shown that a perceptual manipulation (i.e., placing material in Sans Forgetica) can improve learning in a simplified context, its efficacy as a potential learning technique is tempered by the finding that test expectancy can nullify the effect. In

⁷ We wish to thank an anonymous reviewer for this suggestion.

educational settings, learning is explicitly goal-directed, and students accordingly encode information intentionally. Thus, Sans Forgetica (and perceptual disfluency manipulations in general) may not effectively enhance memory in ecologically valid settings. While a recent meta-analysis (Xie et al., 2018; Weissgerber et al., in press) claimed the disfluency effect was null and void, what is clear from the current findings is that the impact of perceptual disfluency manipulations such as Sans Forgetica is not straightforward. Researchers should heed the call to examine further the conditions under which perceptual disfluency is and not desirable for learning.

Disclosures

Acknowledgements. This research was supported by grant number 220020429 from the James S. McDonnell Foundation awarded to the second author. We would like to Gene Brewer and two anonymous reviewers for their helpful comments on an earlier draft of the paper.

Conflicts of Interest. The authors declare that they have no conflicts of interest with respect to the authorship or the publication of this article.

Author Contributions. JG wrote the first draft of the manuscript, collected all the data, and conducted all statistical analyses. DP reviewed and helped edit the manuscript. Both JG and DP approved the final manuscript.

R Packages and Acknowledgments. This paper was written in R-Markdown. In RMarkdown, the text and the code for analysis may be included in a single document. The document for this paper, with all text and code, can be found at: <https://osf.io/cqp6s/>. The results were created using R (Version 4.0.2; R Core Team, 2019) and the R-packages *afex* (Version 0.27.2; Singmann et al., 2020), *BayesFactor* (Version 0.9.12.4.2; Morey & Rouder, 2018), *cowplot* (Version 1.1.0; Wilke, 2020), *ggrepel* (Version 0.8.2; Slowikowski, 2020), *here* (Version 0.1; Müller, 2017), *janitor* (Version 2.0.1; Firke, 2020), *knitr* (Version 1.29; Xie, 2015), *lubridate* (Version 1.7.9; Grolemund & Wickham, 2011), *MOTE* (Version 1.0.2; Buchanan et al., 2019), *papaja* (Version 0.1.0.9997; Aust & Barth, 2020), *patchwork* (Version 1.0.1; Pedersen, 2019), *tidyverse* (Version 1.3.0; Wickham, 2017).

References

- Anwyl-Irvine, A. L., Massonnié, J., Flitton, A., Kirkham, N., & Evershed, J. K. (2020). Gorilla in our midst: An online behavioral experiment builder. *Behavior Research Methods*, 52(1), 388–407. <https://doi.org/10.3758/s13428-019-01237-x>
- Alter, A. L., Oppenheimer, D. M., Epley, N., & Eyre, R. N. (2007). Overcoming Intuition: Metacognitive Difficulty Activates Analytic Reasoning. *Journal of Experimental Psychology: General*, 136(4), 569–576. <https://doi.org/10.1037/0096-3445.136.4.569>
- Alter, A. L. (2013). The Benefits of Cognitive Disfluency. *Current Directions in Psychological Science*, 22(6), 437–442. <https://doi.org/10.1177/0963721413498894>
- Aust, F., & Barth, M. (2020). *papaja: Create APA manuscripts with R Markdown*. <https://github.com/crsh/papaja>
- Hunter Ball, B., 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), 336–348. <https://doi.org/10.1037/xap0000023>
- Balota, D. A., Yap, M. J., Cortese, M. J., Hutchison, K. A., Kessler, B., Loftis, B., Neely, J. H., Nelson, D. L., Simpson, G. B., & Treiman, R. (2007). *The english lexicon project* (Nos. 3; Vol. 39, pp. 445–459). Springer New York LLC. <https://doi.org/10.3758/BF03193014>
- Besken, M., & Mulligan, N. W. (2013). Easily perceived, easily remembered? Perceptual interference produces a double dissociation between metamemory and memory performance. *Memory and Cognition*, 41(6), 897–903. <https://doi.org/10.3758/s13421-013-0307-8>
- Bjork, R. A., & Yue, C. L. (2016). Commentary: Is disfluency desirable? *Metacognition and Learning*, 11(1), 133–137. <https://doi.org/10.1007/s11409-016-9156-8>

- 794 Bjork, E. L., & Bjork, R. A. (2011). Making things hard on yourself, but in a good way: Creating
795 desirable difficulties to enhance learning. In *Psychology and the real world: Essays*
796 *illustrating fundamental contributions to society*. (pp. 56–64). Worth Publishers.
- 797 Botvinick, M. M., Braver, T. S., Barch, D. M., Carter, C. S., & Cohen, J. D. (2001). Conflict
798 monitoring and cognitive control. *Psychological review*, 108(3), 624–652.
799 <https://doi.org/10.1037/0033-295x.108.3.624>
- 800 Braginsky, M. (2021). ggpirate: Pirate plotting for ggplot2. <https://github.com/mikabr/ggpirate>
- 801 Buchanan, E. M., Gillenwaters, A., Scofield, J. E., & Valentine, K. D. (2019). *MOTE: Measure*
802 *of the Effect: Package to assist in effect size calculations and their confidence intervals*.
803 <http://github.com/doomlab/MOTE>
- 804 Butler, A. C., Marsh, E. J., Slavinsky, J. P., & Baraniuk, R. G. (2014). Integrating Cognitive
805 Science and Technology Improves Learning in a STEM Classroom. *Educational*
806 *Psychology Review*, 26(2), 331–340. <https://doi.org/10.1007/s10648-014-9256-4>
- 807 Castel, A. D., Nazarian, M., & Blake, A. B. (2015). *Attention and incidental memory in everyday*
808 *settings*. In J. M. Fawcett, E. F. Risko, & A. Kingstone (Eds.), *The handbook of*
809 *attention* (p. 463–483). MIT Press.
- 810 Carpenter, S. K., Pashler, H., & Vul, E. (2006). What types of learning are enhanced by a cued
811 recall test? *Psychonomic Bulletin and Review*, 13(5), 826–830.
812 <https://doi.org/10.3758/BF03194004>
- 813 Carpenter, S. K., Wilford, M. M., Kornell, N., & Mullaney, K. M. (2013). Appearances can be
814 deceiving: Instructor fluency increases perceptions of learning without increasing actual
815 learning. *Psychonomic Bulletin and Review*, 20(6), 1350–1356.
816 <https://doi.org/10.3758/s13423-013-0442-z>

- 817 Carpenter, S. K. (2014). *Spacing and interleaving of study and practice*. In V. A. Benassi, C. E.
818 Overson, & C. M. Hakala (Eds.), *Applying the science of learning in education: Infusing*
819 *psychological science into the curriculum* (pp. 131-141). American Psychological
820 Association
- 821 Cohen, J. (1977). *Statistical power analysis for the behavioral sciences* (rev. ed.). New York,
822 NY: Academic Press.
- 823 Craik, F. I. M., & Lockhart, R. S. (1972). Levels of processing: A framework for memory
824 research. *Journal of Verbal Learning and Verbal Behavior*, 11(6), 671–684.
825 [https://doi.org/https://doi.org/10.1016/S0022-5371\(72\)80001-X](https://doi.org/10.1016/S0022-5371(72)80001-X)
- 826 Diemand-Yauman, C., Oppenheimer, D. M., & Vaughan, E. B. (2011). Fortune favors the:
827 Effects of disfluency on educational outcomes. *Cognition*, 118(1), 111–115.
828 <https://doi.org/10.1016/j.cognition.2010.09.012>
- 829 Dunlosky, J., & Thiede, K. W. (1998). What makes people study more? An evaluation of factors
830 that affect self-paced study. *Acta Psychologica*, 98(1), 37–56.
831 [https://doi.org/10.1016/S0001-6918\(97\)00051-6](https://doi.org/10.1016/S0001-6918(97)00051-6)
- 832 Earp, J. (2018). *Q&A: Designing a font to help students remember key information*.
- 833 Eitel, A., Kühl, T., Scheiter, K., & Gerjets, P. (2014). Disfluency meets cognitive load in
834 multimedia learning: Does harder-to-read mean better-to-understand? *Applied Cognitive*
835 *Psychology*, 28(4), 488–501.
- 836 Eitel, A., & Kühl, T. (2016). Effects of disfluency and test expectancy on learning with text.
837 *Metacognition and Learning*, 11(1), 107–121. <https://doi.org/10.1007/s11409-015-9145-3>
- 838 Eskenazi, M. A., & Nix, B. (2020). Individual Differences in the Desirable Difficulty Effect
839 During Lexical Acquisition. *Journal of Experimental Psychology: Learning Memory and*
840 *Cognition*. <https://doi.org/10.1037/xlm0000809>

- 841 Evans, J. S. B. T. (2016). Reasoning, Biases and Dual Processes: The Lasting Impact of Wason
842 (1960). *Quarterly Journal of Experimental Psychology*, 69(10), 2076–2092.
843 <https://doi.org/10.1080/17470218.2014.914547>
- 844 Firke, S. (2020). *Janitor: Simple tools for examining and cleaning dirty data*. [https://CRAN.R-](https://CRAN.R-project.org/package=janitor)
845 [project.org/package=janitor](https://CRAN.R-project.org/package=janitor)
- 846 Funder, D. C., & Ozer, D. J. (2019). Evaluating Effect Size in Psychological Research: Sense and
847 Nonsense. *Advances in Methods and Practices in Psychological Science*, 2(2), 156–168.
848 <https://doi.org/10.1177/2515245919847202>
- 849 Geller, J., Davis, S. D., & Peterson, D. J. (2020). Sans Forgetica is not desirable for learning.
850 *Memory*. <https://doi.org/10.1080/09658211.2020.1797096>
- 851 Geller, J., & Still, M. L. (2018). Testing expectancy, but not judgements of learning, moderate
852 the disfluency effect. In J. Z. Chuck Kalish Martina Rau & T. Rogers (Eds.), *CogSci 2018*
853 (pp. 1705–1710).
- 854 Geller, J., Still, M. L., Dark, V. J., & Carpenter, S. K. (2018). Would disfluency by any other
855 name still be disfluent? Examining the disfluency effect with cursive handwriting.
856 *Memory and Cognition*, 46(7), 1109–1126. <https://doi.org/10.3758/s13421-018-0824-6>
- 857 Grolemund, G., & Wickham, H. (2011). Dates and times made easy with lubridate. *Journal of*
858 *Statistical Software*, 40(3), 1–25. <http://www.jstatsoft.org/v40/i03/>
- 859 Halamish, V. (2018). Can very small font size enhance memory? *Memory & Cognition*, 46(6),
860 979–993. <https://doi.org/10.3758/s13421-018-0816-6>
- 861 Hirshman, E., & Mulligan, N. (1991). Perceptual interference improves explicit memory but does
862 not enhance data-driven processing. *Journal of Experimental Psychology: Learning,*
863 *Memory, and Cognition*, 17(3), 507–513. <http://doi.org/10.1037/0278-7393.17.3.507>

- 864 Hirshman, E., Trembath, D., & Mulligan, N. (1994). Theoretical implications of the mnemonic
865 benefits of perceptual interference. *Journal of Experimental Psychology: Learning, Memory,*
866 *and Cognition*, 20(3), 608-620.
- 867 Kühl, T., Eitel, A., Damnik, G., & Körndle, H. (2014). The impact of disfluency, pacing, and
868 students' need for cognition on learning with multimedia. *Computers in Human Behavior*,
869 35, 189–198. <https://doi.org/10.1016/j.chb.2014.03.004>
- 870 Janes, J.L., Rivers, M.L. & Dunlosky, J. The influence of making judgments of learning on
871 memory performance: Positive, negative, or both?. *Psychon Bull Rev* **25**, 2356–2364 (2018).
872 <https://doi.org/10.3758/s13423-018-1463-4>
- 873 Jarosz, A. F., & Wiley, J. (2014). What are the odds? A practical guide to computing and
874 reporting bayes factors. *Journal of Problem Solving*, 7(1), 2–9. [https://doi.org/10.7771/1932-](https://doi.org/10.7771/1932-6246.1167)
875 [6246.1167](https://doi.org/10.7771/1932-6246.1167)
- 876 Kinoshita, S. (1989). Generation enhances semantic processing? The role of distinctiveness in the
877 generation effect. *Memory & Cognition*, 17(5), 563–571
- 878 Kornell, N., Rhodes, M. G., Castel, A. D., & Tauber, S. K. (2011). The ease-of-processing
879 heuristic and the stability bias: dissociating memory, memory beliefs, and memory
880 judgments. *Psychological science*, 22(6), 787–794.
881 <https://doi.org/10.1177/0956797611407929>
- 882 Lenth, R. (2020). *Emmeans: Estimated marginal means, aka least-squares means*.
883 <https://github.com/rvlenth/emmeans>
- 884 Macmillan, N. A., & Creelman, C. D. (2005). *Detection theory: A user's guide*, 2nd ed. (pp. xix,
885 492–xix, 492). Lawrence Erlbaum Associates Publishers.
- 886 Morey, R. D., & Rouder, J. N. (2018). *BayesFactor: Computation of bayes factors for common*
887 *designs*. <https://CRAN.R-project.org/package=BayesFactor>

- 888 Müller, K. (2017). *Here: A simpler way to find your files*. [https://CRAN.R-](https://CRAN.R-project.org/package=here)
889 [project.org/package=here](https://CRAN.R-project.org/package=here)
- 890 Mulligan, N. W. (1996). The effects of perceptual interference at encoding on implicit memory,
891 explicit memory, and memory for source. *Journal of Experimental Psychology: Learning,*
892 *Memory, and Cognition*, 22(5), 1067–1087. [http://doi.org/10.1037/0278-](http://doi.org/10.1037/0278-7393.22.5.1067) 7393.22.5.1067
- 893 Mulligan, N. W. (1998). Perceptual interference at encoding enhances recall for high-but not low-
894 imageability words. *Psychonomic Bulletin & Review*, 5(3), 464–469.
- 895 Myers, S. J., Rhodes, M. G., & Hausman, H. E. (2020). Judgments of learning (JOLs) selectively
896 improve memory depending on the type of test. *Memory and Cognition*, 48(5), 745–758.
897 <https://doi.org/10.3758/s13421-020-01025-5>
- 898 Nairne, J. S. (1988). The Mnemonic Value of Perceptual Identification. *Journal of Experimental*
899 *Psychology: Learning, Memory, and Cognition*, 14(2), 248–255.
900 <https://doi.org/10.1037/0278-7393.14.2.248>
- 901 Maxwell, N.P., Huff, M.J., Buchananon, E. (2020). *Lrd: A package for processing lexical*
902 *response data*.
- 903 McClelland, J. L., & Rumelhart, D. E. (1981). An interactive activation model of context effects
904 in letter perception: I. An account of basic findings. In *Psychological Review* (Vol. 88,
905 Issue 5, pp. 375–407). American Psychological Association. [https://doi.org/10.1037/0033-](https://doi.org/10.1037/0033-295X.88.5.375)
906 295X.88.5.375
- 907 Lakens, D., & Evers, E. R. K. (2014). Sailing From the Seas of Chaos Into the Corridor of
908 Stability: Practical Recommendations to Increase the Informational Value of Studies.
909 *Perspectives on Psychological Science : A Journal of the Association for Psychological*
910 *Science*, 9(3), 278–292. <https://doi.org/10.1177/1745691614528520>

- 911 Luna, K., Martín-Luengo, B., & Albuquerque, P. B. (2018). Do delayed judgements of learning
912 reduce metamemory illusions? A meta-analysis. *Quarterly Journal of Experimental*
913 *Psychology*, 71(7), 1626–1636. <https://doi.org/10.1080/17470218.2017.1343362>
914
- 915 Olejnik, S., & Algina, J. (2003). *Generalized Eta and Omega Squared Statistics: Measures of*
916 *Effect Size for Some Common Research Designs* (Nos. 4; Vol. 8, pp. 434–447).
917 <https://doi.org/10.1037/1082-989X.8.4.434>
- 918 Oppenheimer, D. M., & Alter, A. L. (2013). *Disfluency sleeper effect: Disfluency today promotes*
919 *fluency tomorrow*. In C. Unkelbach & R. Greifender (Eds.), *The experience of thinking:*
920 *How the fluency of mental processes influences cognition and behaviour* (p. 85–97).
921 Psychology Press.
- 922 Oppenheimer, D. M., & Alter, A. L. (2014). The Search for Moderators in Disfluency Research.
923 *Applied Cognitive Psychology*, 28(4), 502–504. <https://doi.org/10.1002/acp.3023>
- 924 Pan, S. C., Sana, F., Samani, J., Cooke, J., & Kim, J. A. (2020). Learning from errors: students’
925 and instructors’ practices, attitudes, and beliefs. *Memory*, 28(9), 1105–1122.
926 <https://doi.org/10.1080/09658211.2020.1815790>
- 927 Peirce, J., Gray, J. R., Simpson, S., MacAskill, M., Höchenberger, R., Sogo, H., Kastman, E., &
928 Lindeløv, J. K. (2019). PsychoPy2: Experiments in behavior made easy. *Behavior*
929 *Research Methods*, 51(1), 195–203. <https://doi.org/10.3758/s13428-018-01193-y>
- 930 Perea, M., Gil-López, C., Beléndez, V., & Carreiras, M. (2016). Do handwritten words magnify
931 lexical effects in visual word recognition? *Quarterly Journal of Experimental Psychology*,
932 69(8), 1631–1647. <https://doi.org/10.1080/17470218.2015.1091016>
- 933 Perfetti, C. (2007). Reading ability: Lexical quality to comprehension. *Scientific Studies of*
934 *Reading*, 11(4), 357–383. <https://doi.org/10.1080/10888430701530730>

- 935 Pieger, E., Mengelkamp, C., & Bannert, M. (2018). Disfluency as a Desirable Difficulty—The
 936 Effects of Letter Deletion on Monitoring and Performance. *Frontiers in Education*, 3, 101.
 937 <https://doi.org/10.3389/feduc.2018.00101>
- 938 R Core Team. (2019). *R: A language and environment for statistical computing*. R Foundation
 939 for Statistical Computing. <https://www.R-project.org/>
- 940 Rhodes, M. G., & Castel, A. D. (2009). Metacognitive illusions for auditory information: Effects
 941 on monitoring and control. *Psychonomic Bulletin and Review*, 16(3), 550–554.
 942 <https://doi.org/10.3758/PBR.16.3.550>
- 943 Rhodes, M. G., & Castel, A. D. (2008). Memory Predictions Are Influenced by Perceptual
 944 Information: Evidence for Metacognitive Illusions. *Journal of Experimental Psychology:*
 945 *General*, 137(4), 615–625. <https://doi.org/10.1037/a0013684>
- 946 Rosner, T. M., Davis, H., & Milliken, B. (2015). Perceptual blurring and recognition memory: A
 947 desirable difficulty effect revealed. *Acta Psychologica*, 160, 11–22.
 948 <https://doi.org/10.1016/j.actpsy.2015.06.006>
- 949 Rummer, R., Schweppe, J., & Schwede, A. (2016). Fortune is fickle: null-effects of disfluency on
 950 learning outcomes. *Metacognition and Learning*, 11(1), 57–70.
 951 <https://doi.org/10.1007/s11409-015-9151-5>
- 952 Sagan, C. (1980). *Broca's brain: Reflections on the romance of science*.
 953 https://books.google.com/books?hl=en&lr=&id=GLXPqexwO28C&oi=fnd&pg=PR4&ots=65nePfKWk5&sig=CTTgqKJLaozsFvFqBYjBd__EOkxE
- 954
 955 Seufert, T., Wagner, F., & Westphal, J. (2017). The effects of different levels of disfluency on
 956 learning outcomes and cognitive load. *Instructional Science*, 45(2), 221–238.
 957 <https://doi.org/10.1007/s11251-016-9387-8>

- Singmann, H., Bolker, B., Westfall, J., Aust, F., & Ben-Shachar, M. S. (2020). *Afex: Analysis of factorial experiments*. <https://CRAN.R-project.org/package=afex>
- Slowikowski, K. (2020). *Ggrepel: Automatically position non-overlapping text labels with 'ggplot2'*. <https://CRAN.R-project.org/package=ggrepel>
- 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. <https://doi.org/10.1037/a003838>
- Strukelj, A., Scheiter, K., Nyström, M., & Holmqvist, K. (2016). Exploring the lack of a disfluency effect: evidence from eye movements. *Metacognition and Learning*, 11(1), 71–88. <https://doi.org/10.1007/s11409-015-9146-2>
- Sungkhasettee, V. W., Friedman, M. C., & Castel, A. D. (2011). Memory and metamemory for inverted words: Illusions of competency and desirable difficulties. *Psychonomic Bulletin and Review*, 18(5), 973–978. <https://doi.org/10.3758/s13423-011-0114-9>
- 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(7), 1000–1011. <https://doi.org/10.3758/s13421-013-0323-8>
- Szpunar, K. K., McDermott, K. B., & Roediger, H. L. (2007). Expectation of a final cumulative test enhances long-term retention. *Memory and Cognition*, 35(5), 1007–1013. <https://doi.org/10.3758/BF03193473>
- 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*, 1–8. <https://doi.org/10.1080/09658211.2020.1758726>
- Weinstein, Y., Gilmore, A. W., Szpunar, K. K., & McDermott, K. B. (2014). The role of test expectancy in the build-up of proactive interference in long-term memory. *Journal of*

- 982 *Experimental Psychology: Learning Memory and Cognition*, 40(4), 1039–1048.
983 <https://doi.org/10.1037/a0036164>
- 984 Weissgerber, S. C., & Reinhard, M. A. (2017). Is disfluency desirable for learning? *Learning and*
985 *Instruction*, 49, 199–217. <https://doi.org/10.1016/j.learninstruc.2017.02.004>
- 986 Weissgerber, S. C., Brunmair, M., & Rummer, R. (in press). Null and void? Errors in Meta-
987 Analysis on Perceptual Disfluency and Recommendations to Improve Meta-analytical
988 Reproducibility. *Educational Psychology Review*.
- 989 Westerman, D. L., & Greene, R. L. (1997). The effects of visual masking on recognition:
990
991 Similarities to the generation effect. *Journal of Memory and Language*, 37(4), 584–596.
992
- 993 Weltman, D., & Eakin, M. (2014). Incorporating Unusual Fonts and Planned Mistakes in Study
994 Materials to Increase Business Student Focus and Retention. *INFORMS Transactions on*
995 *Education*, 15(1), 156–165. <https://doi.org/10.1287/ited.2014.0130>
- 996 Westfall, J. (2016). *PANGAEA: Power ANalysis for GEneral Anova designs*. Retrieved from
997 <http://jakewestfall.org/pangea/>
- 998 Wickham, H. (2017). *Tidyverse: Easily install and load the 'tidyverse'*. [https://CRAN.R-](https://CRAN.R-project.org/package=tidyverse)
999 [project.org/package=tidyverse](https://CRAN.R-project.org/package=tidyverse)
- 1000 Wilke, C. O. (2020). *Cowplot: Streamlined plot theme and plot annotations for 'ggplot2'*.
1001 <https://CRAN.R-project.org/package=cowplot>
- 1002 Xie, H., Zhou, Z., & Liu, Q. (2018). Null Effects of Perceptual Disfluency on Learning Outcomes
1003 in a Text-Based Educational Context: a Meta-analysis. *Educational Psychology Review*,
1004 30(3), 745–771. <https://doi.org/10.1007/s10648-018-9442-x>

- 1005 Xie, Y. (2015). *Dynamic documents with R and knitr* (2nd ed.). Chapman; Hall/CRC.
- 1006 <https://yihui.name/knitr/>