Sentence Processing of Taboo Words: Evidence from Eyetracking

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Abstract

The reported studies investigate online processing of taboo words (e.g. *shit*) and their censored equivalents (e.g. *s**t*), relative to semantically matched non-taboo words (e.g. *junk*). Participants' eyes were tracked as they read sentences which contained one of the critical words. In Experiment 1, participants also encountered censored-neutral words, known as masked (e.g. *j**k*), but in Experiment 2, participants only encountered the taboo, censored, and neutral conditions, thus manipulating the perceptual certainty of censored words. Taboo and neutral words required similar processing time across all reading measures; liberal post-hoc analyses replicated the null effect. With regards to the censored words, Experiment 1 revealed that early word-recognition requirements were similar between censored, taboo, and neutral words, with censored words requiring additional processing time in later sentence integration measures. However, the results from Experiment 2 revealed no differences in reading time between conditions, suggesting that the masked words in Experiment 1 motivated participants to double-check the censored words due to their orthographic similarity. After reading all of the sentences in Experiment 2, participants' memory of the sentences was tested. Participants were able to differentiate between whether they encountered a neutral or profane word (i.e. either taboo or censor), but participants were unable to identify the specific profane word that they encountered in the reading task. We argue that the results relating to the taboo words further clarifies language's role within the functional architecture of cognition while the results relating to censorship informs how statistical regularities of language are used to process lexical-semantic information.

Introduction

Taboo words

It would be difficult to find an adult who is unfamiliar with profanity, also referred to as taboo words (e.g. *shit*, *fuck*), because learning which words are taboo is a normal part of language development (Jay, 1992); profanity knowledge and use is quit adult-like by the age of 12 (Jay & Jay, 2013). Even though frequency of profanity use can differ greatly across individuals, profanity is a common linguistic act (Mehl & Pennebaker, 2003), especially between close friends or within strongly cohesive groups (Baruch & Jenkins, 2007). High frequency of profanity use is particularly true among college undergraduates, whose swearing frequencies have been estimated at 7-8 occurrences per 100 words (Cameron, 1969; Nerbonne & Hipskind, 1972). Given the high frequency of profanity, it may seem surprising that profanity has been minimally examined with the language sciences (Jay, 2009); exceptions include language-related disorders (e.g. aphasia, Tourette's Syndrome; see Van Lancker & Cummings, 1999; Code, 2011), second-language processing (Harris, Aycicegi, & Gleason, 2003), and the role of conflict monitoring in speech production (Severens, Janssens, Kühn, Brass, & Hartsuiker, 2011; Severens, Kühn, Hartsuiker, & Brass, 2012).

Early language studies of taboo words reported that participants are more delayed in uttering taboo words compared with non-taboo words (McGinnies, 1949). Researchers do not agree on the cause of this effect (for a review, see Jay, 2009). Some argue for perceptual defense (McGinnies & Sherman, 1952); others claim that taboo words are rare and thus difficult to comprehend (Howes & Solomon, 1950); and others argue that comprehension is unaffected, but participants simply feel uncomfortable uttering taboo words (Zajonc, 1962). Since that time, McGinnies' original finding has been consistently observed and further explored: Comprehending taboo words delays ongoing cognitive processes such as those relating to attention (Anderson, 2005; Arnell, Killman, & Fijavz, 2007; Mathewson, Arnell, & Mansfield, 2008; Bertels, Kolinsky, & Morais, 2010), executive control (MacKay & Ahmetzanov, 2005; Mackay, et al., 2004), and language production (Motley, Baars, & Camden, 1983). Effects are observed even when taboo words are presented as distractors in peripheral vision: Participants are slower to name

foveally-presented pictures (Dhooge & Hartsuiker, 2011) and their eye movements veer further away from taboo words than from non-taboo words (Weaver, Lauwereyns, & Theeuwes, 2011).

Evidence from the *Spoonerisms of Laboratory-Induced Predisposition* (SLIP) task has been particularly informative in understanding the relationship between language production and taboo words. The SLIP task requires participants to pronounce pairs of letter-strings which could form legal phrases if their initial letters are switched (e.g. darn bore -> barn door). Taboo-eliciting trials (e.g. tool kits -> cool tits) have been associated with increased response time and lower rate of spoonerisms than non-taboo trials (Motley, Camden, & Baars, 1981;1982), suggesting a higher degree of scrutiny for taboo-trials. In order for such scrutiny to occur, the language production system must include a mechanism akin to an “internal monitor” which filters undesirable speech prior to language production (Motley, Baars, & Camden, 1983; Dhooge & Hartsuiker, 2011). The existence of such a monitor would explain the classic taboo-delay observed by McGinnies’ and support Zajonc’s claim that the delay is based in production.

Electrophysiological research utilizing the taboo-SLIP task provides further evidence that taboo words are subject to greater inhibitory processes than non-taboo words. Using ERP methodology, Severens et al. (2011) observed an increased negativity approximately 600 ms after word-pair onset relative to a non-taboo trial. This ERP component is associated with internal conflict and inhibition (Möller, et al, 2007), suggesting that taboo-trials required additional inhibitory resources. Using fMRI methodology, Severens et al. (2012) observed that taboo-trials elicited greater activity in the right inferior frontal gyrus, which is also associated with response inhibition (Aron, et al., 2003). Severens and colleagues argue that people utilize their internal monitor to filter imminent speech according to internalized social rules (such as those about taboo language) so as to inhibit socially undesirable behavior.

Although inhibiting taboo words appears to require greater cognitive resources than inhibiting non-taboo words, there is little evidence to suggest that taboo and non-taboo words are processed differently during language comprehension. Severens et al (2011) did not observe any effects on lexical-semantic ERP components (e.g. N400; see Kutas & Hillyard, 1984) and Severens et al (2012) did not

observe relative neural activation differences in language specific brain regions (e.g. left inferior frontal gyrus, left superior temporal gyrus; see Kuperberg, et al., 2000). These findings are in line with Mackay et al. (2004), who reported similar response times for taboo words and matched controls in a lexical decision task, but also observed that taboo words impaired performance on a Stroop task and an attentional blink task. Mackay and colleagues argue that taboo words can be particularly taxing on cognitive resources, but the effects of a taboo word only emerge after the meaning is accessed.

In summary, the existing evidence suggests that taboo words are more difficult to inhibit than non-taboo words, but comprehension requirements of taboo and non-taboo words may be quite similar. However, evidence mainly comes from single-word processing studies, and so it is unclear how post-lexical mechanisms handle taboo words. Therefore, the purpose of the current study is to test whether the single-word comprehension findings extend to sentence integration. If taboo-related delays do emerge during integration, researchers could examine the effects of these delays on comprehension and memory, potentially informing researchers on how these systems communicate. However, if taboo-related delays do not emerge during integration, then that would suggest that taboo-related features are not processed by the language comprehension system, even though these same features are considered salient by the rest of the cognitive system.

Censorship

Although profanity is quite common within casual conversation (Cameron, 1969; Jay, 1992; Nerbonne & Hipskind, 1972), politeness laws in the United States generally require profanity to be censored if the words appear in public media (Calvert, 2004). Profanity is usually censored in text-based media by replacing certain letters with non-linguistic symbols (e.g. shit -> s**t) and in sound-based media by replacing the word's sound with an artificial tone. Media studies have shown that auditory censorship does not prevent comprehension (Kremar & Sohn, 2004), but the online processing requirements associated with censored words are relatively unknown.

During reading, phonological and orthographic information is extracted from specific letter combinations; removing such sources of information increases processing time (Lee, Rayner, & Pollatsek,

1999; Rayner, Pollatsek, & Binder, 1998; Miellet & Sparrow, 2004; Rayner, Sereno, Lesch, & Pollatsek, 1995; Brysbaert, Grondelaers, & Ratinckx, 2000; Schotter, Angele, & Rayner, 2012). Censorship replaces letters with non-linguistic symbols, effectively removing certain orthographic and phonological features. Comprehension of censored words may require a certain amount of reconstruction prior to lexical access, resulting in an additional step that is costly to word recognition processes.

Research on the processing of text message shortcuts (e.g. *gr8*, *txt*) and acronyms (e.g. *FBI*, *CIA*, *NFL*) may inform hypotheses regarding the processing of censored words. Similar to censored words, shortcuts and acronyms are systematic alterations of complete words in which bottom-up features have been removed or replaced. ERP evidence suggests that pre-lexical orthographic and phonological operations are similar for shortcuts and complete words (Ganushchak, Krott, & Meyer, 2010; Berger & Coch, 2010), which suggests that shortcuts do not require reconstruction. In fact, shortcuts may be unique lexical items, given that common shortcuts do not prime the semantic associates related to their complete word (Ganushchak, Krott, & Meyer, 2012). Although shortcuts are more difficult to recognize than complete words (potentially related to their frequency), sentence integration processes are similar for shortcuts and complete words (Ganushchak, Krott, Frisson, & Meyer, 2013). Eyetracking evidence suggests similar findings for acronyms; phonological operations on acronyms are not governed by the phonological rules of their referents (e.g. *FBI* is processed according to the phonological rules pertaining to *FBI*, not *Federal Bureau of Investigation*; see Slattery, Pollatsek, & Rayner, 2006; Brysbaert, Speybroeck, & Vanderelst, 2009). If such results extend to censorship, then censored words may not require lexical reconstruction or differ from complete words in sentence integration processes

Examining how the language comprehension system processes censorship may provide a useful perspective into the relationship between statistical learning and lexical-semantic processes. Researchers argue that language comprehension is guided by statistical regularities that are learned across a lifetime (Misyak, Christiansen, & Tomblin, 2010; Misyak & Christiansen, 2012; Chang, Janciauskas, & Fitz, 2012; Conway, et al, 2010). Participants can even adapt their processing strategies to fit the statistical regularities of individual experiments (Fine & Jaeger, 2013; Fine, Jaeger, Farmer, & Qian, 2013; Jaeger &

Snider, 2013; Hanulíková et al, 2012) and individual speakers within an experiment (Kamide, 2012; Trude & Brown-Schmidt, 2012). World knowledge about sociolinguistic regularities, such as speaker-specific inferences, also guide processing strategies at the level of phonology (Brunellière & Soto-Faraco, 2013), semantics (Van Berkum et al, 2008) and syntax (Hanulíková et al, 2012). In the real-world, only taboo words are consistently censored and this statistical regularity may inform online processing. We intend to examine how this implicitly learned information affects processing of censored words by conducting two experiments: one which violates the assumption that all censored words are taboo, and another which maintains the assumption that all censored words are taboo.

The current study

The current study was designed with two primary questions in mind: a) are taboo words processed differently from non-taboo words? and b) how does the language comprehension system use knowledge concerning conventions for censoring these words during online processing?

In regards to the first question, we used an eyetracking methodology to probe whether taboo and non-taboo words are subject to similar sentence processing requirements. Previous research on taboo words has primarily examined single-word processing and found little evidence for differences in comprehension difficulty between taboo words and appropriate controls. We examined whether these findings extended to sentential integration by recording participants' eye movements as they read taboo words vs. matched control words within the context of a sentence.

A key concern was selecting an appropriate control condition which would be compared to the taboo condition. The features of the control condition would dictate what could be observed regarding language comprehension of taboo words. Taboo words embody more features than just social inappropriateness or offensiveness. Taboo words can contribute semantic meaning to a sentence (Jay & Janschewitz, 2008; Jay, 2009) and taboo words themselves form a semantically cohesive category (Buchanan, et al., 2006; Talmi & Moscovitch, 2004). These features need to be controlled for in order to isolate the unique effect of taboohood. We chose not to examine distinct taboo-related features because of the wide variety of potential variables. In addition to the semantic information presented above, taboo

words are highly offensive, arousing to the autonomic nervous system, their valence is generally considered to be emotionally negative (Mackay, et al., 2004), and certain social rules are used to gauge their appropriateness (Jay, 1992). Given the current sparseness of research on taboo words in the linguistic literature, we did not want to examine something as specific as the interaction between arousal and valence or as complicated as the pragmatic rules that govern the use of taboo words. We determined that a reasonable starting point would be to control for the effect of semantic content so as to isolate the unique effect of taboohood (e.g. emotionality, offensiveness, social inappropriateness, etc). We controlled for semantic content by pairing each taboo word with a semantically similar non-taboo word (e.g. *shit* – *junk*). These conditions are referred to as *taboo* and *neutral* respectively, while the manipulation is referred to as *taboohood*. Sentences were designed such that overall meaning did not change between conditions because sentence-level meaning is known to influence word-reading times (Frisson, Rayner, & Pickering, 2005). Controlling for word-level and sentence-level meaning would remove semantic confounds, thus allowing us to isolate and examine the unique effect of taboohood on comprehension over and above a word's semantic contribution to a sentence.

In regards to the second question for the present research, we examined how the language comprehension system is able to use the statistical regularity of censorship during online processing. If censorship's statistical regularity informs lexical processing, then taboo words are likely initially considered as lexical candidates when censored words are encountered; letter-strings that violate this statistical regularity should require more processing time than letter-strings that conform to this statistical regularity. We examined the effect of cue-violation at the lexical level by presenting critical words within sentences that conformed to the lexical cue (e.g. *s**t*) or violated the lexical cue (e.g. *j**k*). These conditions are known as *censored* and *masked* respectively while the manipulation is referred to as *censorship*. We also examined the effect of cue-violation at the level of the linguistic environment by conducting one experiment which contained the cue-violating masked words (Experiment 1) and one experiment which did not contain the cue-violating masked words (Experiment 2), allowing us to examine the effect of the linguistic environment's ecological validity regarding censorship.

Finally, we examined the memory representations for taboo, neutral, and censored words with a forced-choice memory task after the sentence reading task was complete (Experiment 2). Participants saw each sentence again, but the critical word was replaced with a blank space, and participants were required to indicate which critical word they thought they had read. Given that taboohood is primarily a meaning-based manipulation and censorship is not (i.e. *shit* and *s**t* refer to the same word, but *junk* refers to a different word), examining both language and memory processes enabled us to more precisely identify the cognitive systems involved in processing taboohood and censorship. If memory processes were affected, but online language processes were not, such findings may help clarify functional divisions within the architecture of the cognitive system (i.e. certain features are more accessible to memory mechanisms than to language mechanisms).

Predictions. Previous researchers (Severens et al, 2011; 2012; Mackay et al., 2004) reported that taboo words are difficult to inhibit and highly memorable, but little evidence was reported to suggest that taboohood influences comprehension requirements. Based on these findings, we predicted that taboo and neutral words would be similarly processed during early lexical processes as well as during sentential integration, but their memory representations would be dissociable.

The fact that world knowledge is able to operate in tandem with lexical processes (Van Berkum et al, 2008) suggests that censorship's orthographic cues (i.e. asterisks) will guide processing strategies like any other character string (Norris & Kinoshita, 2012). We predicted that when censorship was a viable lexical cue, there would be no differences in reading time between censored words and taboo or neutral (i.e. complete) words. However, when censorship's statistical regularity was violated, we predicted that censored words would require additional processing time than both neutral and taboo words. The masked words' unfamiliarity leads us to predict that masked words will require more reading time than all other conditions. We predicted that this difference in reading time would gradually decrease throughout the experiment because each subsequent violation would likely cause participants to update their expectations regarding censorship's statistical regularity, thus reducing familiarity-based delays on masked words.

Within the memory task, we predicted that censored and taboo words would be more easily confused with each other than with the neutral words because the censored and taboo words refer to the same meaning. Although the research with acronym and text-shortcuts suggests that censored words may be unique lexical items, their degree of semantic similarity may not be sufficient to enable dissociation in memory. Unless the representation of censored words contains distinctly non-taboo words, such as those relating to orthography, comprehension methods, or meaning, it appears unlikely that participants will be able to differentiate between whether they encountered a taboo or censored word. However, neutral words explicitly excluded taboo-related features and previous research has clearly shown that taboo-related features affect memory processes. Therefore, we predicted that participants would be able to reliably differentiate between whether they had encountered a neutral word vs. a taboo or censored word.

Experiment 1

Method

Participants. Thirty-two college-aged individuals from the University of South Carolina were recruited. All participants volunteered to be in the study and were given course credit or \$8 per hour to compensate for their participation.

Apparatus. An Eyelink 1000 eye-tracker (spatial resolution: 0.01°; sampling rate: 1000 hz) monitored eye movements of participants' right eyes during reader. Participants were seated 90 cm from 20-inch monitor (refresh rate: 140 hz) and chin and head rests were used to minimize head movements. The experiment was conducted with SR Research Experiment Builder software.

Materials. We selected 5 common taboo words (Jay & Janschewitz, 2008) and matched each taboo word to a semantically similar, but non-offensive word. The censored versions of the taboo and neutral words were created by replacing two characters in the middle of each word with an asterisk. Together, these resulted in four conditions: taboo (e.g. *shit*), neutral (e.g. *junk*), censored (e.g. *s**t*), and masked (e.g. *j**k*). We constructed sixteen sentences for each of these word-pairs, resulting in a total of

eighty sentences. The critical words did not differ in character length and no word-pairs shared letters in the initial or final letter position. These items can be seen in Appendix A.

Procedure. Participants were led into a testing room which contained the eyetracking equipment. They sat in front of a computer monitor and were given a consent form. The consent form stated that their eyes would be tracked as they read sentences and that some of these sentences contained taboo words. The consent form stated that the participant was free to leave without penalty at any time if they experienced any discomfort due to the taboo words and the experimenter orally reinforced this information. No participants left or mentioned that they considered leaving.

After the consent form was signed, the experimenter calibrated the eye-tracker and gave more specific instructions about the experiment. Participants were to read the sentences at a normal pace and indicate their completing the sentence by looking at a dot in the upper-right hand corner. This fixation cued the eye-tracker to continue to the next trial. Participants were instructed to make sure they comprehended the sentences because some sentences were followed by a plausibility judgment.

In addition to the 80 sentences from this experiment, participants read an additional 192 sentences which were fillers or contained items from another experiment, totaling 282 sentences. All of these sentences were counterbalanced and randomly presented for each subject. Participants read the 282 sentences during the experiment, which was broken up into twelve blocks of approximately twenty-five sentences each. After each block, participants were allowed to take a break. When they were ready to continue, the eye-tracker was recalibrated and the experiment continued.

Data analyses. Data were preprocessed and extracted using SR Research Data Viewer software. Fixations less than 100 ms and greater than 1000 ms were excluded from the analysis. Specific reading measures on the critical word were selected in order to observe processes related to lexical access and post-lexical sentential integration. The selected reading measures were: first-fixation duration (duration of first-fixation on the critical word), single-fixation duration (duration of first-fixation when only one fixation was made on the critical region), gaze-duration (summed duration of all fixations made on critical

region before the eye left the word for the first time), and total-time on the critical region (for a review of these measures, see Rayner, 1998; Kuperman, 2013).

To assess the effect of our manipulations on processing time, a linear mixed-effects model was fit to the data (Baayen, Davidson, & Bates, 2008) for each log-transformed reading measure, with subjects and items coded as random effects. Tabooness (taboo, neutral), and Censorship (complete, obscured) were coded as fixed effects and these conditions were also coded as random slopes on the random effects. To test for practice, or learning effects, another linear-mixed-effects model was constructed with total-time as the dependent measure. Tabooness (taboo vs neutral), Censorship (complete vs. obscured), and Trial number (ordinal value of experiment trial) were coded as fixed effects, subjects and items were coded as random effects, and Tabooness and Censorship coded as random slopes. For both of these models, we only report the model that contained significant predictors and random effects. Analyses were conducted using R, an open source programming language for statistical computations (R development core team, 2007), particularly the lme4 package for linear mixed effects models (Bates, 2005) and the lmerTest package (Kuznetsova, Christensen, & Brockhoff, 2012).

Results

The descriptive statistics can be seen in Table 1, the first regression model can be seen in Table 2, and the second regression model (designed to examine practice effects), can be seen in Table 3.

[insert Table 1 here]

According to the first regression model, there was no effect of tabooness on reading time. Participants spent a similar amount of time processing the taboo and neutral words across all reading measures. The regression model also showed that first and single-fixation duration did not differ between censored words and the complete words, but masked words were associated with longer first and single-fixation durations. Censored words required a longer amount of time than complete words in gaze duration and total-time, and masked words required even more processing time than censored words in gaze duration and total-time.

[insert Table 2 here]

The second regression replicated the first models' reported effects on total-time and also revealed an effect of trial number. All conditions were subject to a reduction in reading time as they progressed through the experiment. The effect of trial was similar for taboo, neutral, and censored words, but masked words were subject to a greater trial effect. It appears that as the participants progressed through the experiment, the reduction in reading time was the greatest for the masked words, suggesting that participants were updating their strategies for processing the masked words.

[insert Table 3 here]

To summarize, the neutral and taboo words were read similarly across all reading measures. The first and single-fixation durations of censored words was similar to neutral and taboo, but in all later measures, censored words were subject to increased processing time relative to the complete words. The masked words required more processing time than all other words for every reading measure. Finally, the effect of trial showed that all words were subject to a reduction in reading time across the experiment, but this reduction was greatest for masked words. The degree of reduction was not different between the taboo, neutral, and censored words.

Discussion

That taboo and neutral words did not differ in processing time is consistent with previous research and supports our prediction that taboo-related features do not influence online processing strategies after the effects of semantic contribution have been accounted for. However, previous research suggests that tabooeness should influence memory mechanisms. We conducted a second experiment in which a primary goal was to test if the memory representations of taboo and neutral words could be reliably dissociated.

The fact that complete words differed from censored words in gaze-duration, but not single-fixation duration, means that the gaze-duration difference was caused by an increased refixation probability. Previous research then suggests that word recognition of censored words was similar to

complete words and the increased gaze-duration resulted from top-down interpretative processes (Rayner, Warren, Juhasz, & Liversedge, 2004). We postulate that one of two causes is driving this effect: 1) censorship altered the requirements of sentence integration (e.g. censorship may render prosodic features less accessible) or 2) the linguistic environment affected refixation strategies. The masked words violated the statistical regularity of censorship and may have motivated participants to double-check the censored words. In Experiment 2, we removed the masked condition so as to create a more ecologically valid linguistic environment which maintained censorship's statistical regularity. If censorship affects sentence processing, then processing time on censored words should still be greater than on complete words. If the linguistic environment biased participants to double-check the censored words, then these differences in processing time should not be observed.

Whereas censored words differed from complete words only in later reading measures, masked words required more processing time than all conditions across all reading measures, including single-fixation duration. This means that participants were processing censored and masked words differently within the first 300 ms, which is quite surprising given their orthographic similarity. However, the learning effect showed that participants revised their processing strategies on masked words without altering processing strategies on censored words. If the statistical regularity of censorship was a top-down lexical cue used to strategically consider taboo candidates, then violating this cue should negatively impact the reading time of censored words; such effects have been observed for syntactic cue-violations (Fine & Jaeger, 2013; Fine, et al., 2013; Hanulíková, et al, 2012). However, revising the processing strategy for masked words did not alter processing strategy for censored words. Perhaps censorship is not a top-down cue, but instead the lifetime statistical regularity of censored words being taboo has assisted participants in lexicalizing censored words. Censored words may be processed just like any other lexical item, by combining a sequence of characters and attempting to identify the lexical item which is unique to that particular sequence (Norris & Kinoshita, 2012).

In summary, Experiment 1 revealed the following: first, taboo and neutral words required a similar amount of processing time across all reading measures, second, word-recognition requirements was similar between censored and complete words, but censored words required more processing time than complete words during sentential-integration processes, third, masked words required more processing time than all other conditions across all reading measures, and fourth, as readers progressed through the experiment, total-time on all words decreased, with the greatest decrease being on masked words whereas a similar decrease occurred for censored, taboo, and neutral words.

Experiment 2

The first aim of Experiment 2 was to examine the online processing of taboo and censored words in a more ecologically valid linguistic environment than Experiment 1. We accomplished this by removing the masked words so that participants only read sentences that contained taboo, censored, or neutral words, thereby preserving censorship's statistical regularity.

The second aim of Experiment 2 was to examine the memory representations of the taboo, censored, and neutral words. After reading all of the sentences, participants completed a memory task which required them to indicate which word they remembered reading within each sentence. The memory task enabled us to examine the similarity of the encoded information across the three conditions.

Method

Participants. Thirty participants were recruited from the same population as in Experiment 1 and received similar compensation. None had participated in Experiment 1.

Apparatus. The features relating to the eye-tracker were identical to those in Experiment 1.

Materials. The stimuli were identical to those used in Experiment 1. The only difference is that 75 items were used from the original 80 from Experiment 1 to allow for a balanced 3 level design (Taboo X Neutral X Censored). One item from each word-pair was eliminated, which meant that 15 sentences were presented for each of the five word-pairs, instead of 16 sentences for each of the five word-pairs, as was the case for Experiment 1. In appendix A, the items with asterisks next to their number represent the

items that were eliminated from Experiment 1. Participants also read 105 fillers from another experiment. After the participants read all of the sentences, they were presented with a forced choice recognition task to examine their memory of the sentences that they just read. Participants were presented with one of the sentences from the reading task, but there was a blank space where the critical word should be. Below this sentence were three choices that consisted of the three potential critical words for that item.

Procedure. The procedure was identical to Experiment 1 with the exception of the memory task. After the participants had read all of the sentences, the instructions for the memory task appeared. Participants were told that they would see a number of sentences that they had just read, but a single word would be replaced with a blank space. Below the sentence were all of the possible conditions for that item and participants were told to indicate their choice with the provided button box. Participants responded to the seventy-five items from our study as well as thirty-six fillers from another experiment.

Data analysis. Experiment 2 used similar pre-processing methods, measured similar dependent variables, and examined the data using linear-mixed effects regression, as in Experiment 1. However, the structure of the regression model differed slightly to accommodate the different design (one variable with three levels: taboo, neutral, and censored). Taboo words were coded as the intercept and the data associated with taboo words was compared to the neutral and censored conditions; interaction effects were not coded. Fixed effects, random effects, and random slopes were inputted into the regression model and tested similarly to the methods described in Experiment 1.

For the memory task, we organized the response data according to the condition that was read in the reading task (taboo, censored, neutral). The dependent measure was the likelihood that the participant would indicate a specific choice for each reading task condition. Given that participants encountered 25 sentences from each reading task condition, the data for the response variable was allocated based on the participants' responses. These raw numbers were converted into a percentage which is the dependent variable reported here. We examined likelihood to indicate a categorical response by running three separate one-way repeated measures ANOVAs, with each ANOVA examining likelihood to indicate a

specific categorical response at a different level of the condition encountered in the sentence reading task. When an ANOVA was significant, we conducted planned t-tests to test if participants were differently likely to indicate the different categorical responses at that particular level of sentence-reading condition.

Results

[insert Table 4 here]

The descriptive statistics for the reading task can be seen in Table 4 and the full regression model for each reading measure can be seen in Table 5. The results of these analyses support our predictions because reading times on taboo, neutral, and censored words did not differ across first-fixation duration, single-fixation duration, gaze-duration, or total reading time.

[insert Table 5 here]

The descriptive statistics for the memory task can be found in Table 6. When participants encountered a taboo word in the sentence reading task, there was a significant effect of response ($F_{(2, 62)} = 9.376, p < 0.01$). Follow up t-tests revealed that likelihood to indicate taboo was not different from likelihood to indicate censored $t(31) = 1.097, p > 0.05$, but participants were more likely to indicate taboo than they were to indicate neutral $t(31) = 3.61, p < 0.001$. When participants encountered a censored word in the sentence reading task, there was a significant effect of response ($F_{(2, 62)} = 8.488, p < 0.01$). Follow up t-tests revealed that likelihood to indicate censored was not different from likelihood to indicate taboo $t(31) = 1.237, p > 0.05$, but participants were more likely to indicate censored than they were to indicate neutral $t(31) = 2.87, p < 0.01$. Finally, when participants encountered a neutral word in the sentence reading task, there was a significant effect of response ($F_{(2, 62)} = 6.702, p < 0.01$). Follow up t-tests revealed that participants were more likely to indicate neutral than they were to indicate taboo $t(31) = 3.61, p < 0.001$ and participants were more likely to indicate neutral than they were to indicate censored $t(31) = 2.57, p < 0.05$. Likelihood to indicate taboo or censored did not differ $t(31) = 0.71, p > 0.05$. Taken together, these findings suggest that participants accurately remembered whether they encountered

a neutral or a profane (either taboo or censored) word, but participants were unable to accurately identify the specific profane word that they had read in the sentence reading task.

[Inset Table 6 here]

Discussion

Experiment 2 examined the online processing and eventual memory representations for taboo, censored, and neutral words. The results from the reading task replicated the following findings from Experiment 1: a) taboo and neutral words require similar amounts of processing time and b) early word-recognition processes did not differ between censored, taboo, and neutral words. However, whereas censored words were subject to later processing costs in Experiment 1, there were no differences in processing time between taboo, neutral, and censored words across all reading measures in Experiment 2. Although no differences were observed in the reading task, we did observe differences in the memory task. As can be seen in the descriptive statistics, participants could reliably indicate whether they had encountered a neutral or profane word, but participants were unable to indicate the specific profane word that they had encountered in the reading task. It appears that the meaning of the critical word was encoded, but the specific orthographic content was not.

The results provide evidence that the social inappropriateness of taboo words has little effect on language comprehension mechanisms, but once a taboo word's meaning is accessed, post-comprehension mechanisms treat taboo words differently from non-taboo words. This interpretation is evidenced by the fact that processing time on taboo and neutral words was similar across all reading measures but participants were able to accurately differentiate between taboo and neutral words in the memory task. Given that sentence memory is not highly specific (Sampaio & Brewer, 2009; Brewer & Lichtenstein, 1975), it is unlikely that taboo and neutral words were distinguished on semantic meaning alone. Differences in taboo-related features were more likely the cause of this dissociation.

The results also provide information regarding the online processing of censored words. It appears that when censored words are presented in an ecologically valid linguistic environment (i.e. all

censored words are taboo), processing difficulty is similar across censored, taboo, and neutral words. It appears likely that the processing time differences between censored and complete words observed in Experiment 1 were not caused by features inherent in the censored word. Instead, the effect was driven by the linguistic environment in that the masked words biased participants to double-check the identity of the censored words. We argue that the null effect observed in Experiment 2 provides evidence that censored and complete words are similarly processed during prelexical and lexical operations.

In the memory task, participants were unable to differentiate between taboo and censored words but they were able to differentiate between whether they had read a profane or neutral word. This pattern suggests that the taboo and censored words embody similar information, but this information differs greatly from the information contained in the neutral words. The fact that we observed similar processing time across all three conditions, but different patterns of memory dissociability bolsters our argument that taboo-related features are not considered salient by online processing mechanisms. The features that enabled memory mechanisms to create distinct memory representations for taboo and neutral words did not influence online processing strategies.

Post-hoc analyses

Across two experiments, we observed minimal differences in reading time between taboo and neutral words across all reading measures. In order to examine the legitimacy of this null effect, we pooled participants from both experiments, resulting in a sample of size of sixty-two, and conducted a series of post-hoc analyses to test for any effect of taboohood on reading time. We report the results here, but the specifics of these analyses can be seen in Appendix B. We conducted uncorrected t-tests on every reading measure, averaged both by-subjects and by-items, and found no differences in processing time between taboo and neutral words. We next considered that the word-pairs might differ in their ability to affect reading time (e.g. *shit* is more offensive than *damn*; see Mackay et al, 2004). We conducted uncorrected t-tests across every reading measure on every word-pair, averaged both by-subjects and by-items, and found no consistent effect of taboohood. Although two word-pairs did show differences in early

reading measures, the effects for the two words were in opposite directions. Finally, we constructed a post hoc variable by grouping participants into two groups based on the total-time they spent reading the taboo words – fast vs. slow readers - to test for individual difference effects. For example, perhaps slow readers were easily offended and likely to avoid the taboo words compared to the fast readers. However, no consistent effect of tabooeness was observed here either. In summary, we failed to find an effect of tabooeness after increasing our sample size to sixty-two, conducting uncorrected t-tests on every reading measure, averaging by-subjects and by-items, both at the group-level and within individual word-pairs, and dividing participants into groups based on reading speed. It appears that tabooeness is not associated with increased processing time after controlling for a word's semantic contribution to a sentence.

General discussion

Taboo words

This study provided information on how the language comprehension system processes profanity and censored profanity within sentences. According to our results, sentence integration of a taboo word is similarly difficult to that of a semantically matched non-taboo word, but the extracted information is dissociable within memory. Previous research has shown that long-term memory encodes the gist of a sentence and critical words cannot be reliably distinguished from synonyms (Sampaio & Brewer, 2009). Given that our items were designed to be semantically similar, it is unlikely that semantic meaning enabled this dissociation. Therefore, memory mechanisms must have considered taboo-related features to be valuable during encoding processes, but these same features did not inform language processing strategies.

Taboo words are not unique in their ability to affect cognitive systems while sparing language comprehension. Aversive words, such as those related to a PTSD patient's trauma (McNally, English, & Lipke, 1993) or those related to one's specific phobia, affect cognitive processes similarly to taboo words (Watts, et al, 1986; Williams, et al, 1996), but there is little evidence to suggest that comprehension processes are affected by aversive meanings (Ferraro, Christopherson, & Douglas, 2006; Hill & Kemp-

Wheeler, 1989). Aversive words gain their status after the word's meaning has become associated with psychological suffering, such as the word *spider* to someone with arachnophobia. New examples of aversive words can even be created via classical conditioning by pairing an electric shock with a neutral word (Staats, Staats, & Crawford, 1962). Interestingly, most people remember being reprimanded or physically punished as children for swearing (Jay, King, & Duncan, 2006) and it has been suggested that the degree of childhood punishment for swearing is positively correlated with one's autonomic reaction to taboo words (Tomash & Reed, 2013). Perhaps taboo words are another example of an aversive word, but one in which the aversive status is agreed upon by the vast majority of a population.

The null effect of taboohood on comprehension processes may be relevant to the field of emotional language processing. Emotional language researchers primarily examine the interactive effects of emotional features (e.g. arousal, valence, dominance) and lexical features (e.g. frequency, word-length, age-of-acquisition, etc.) on processing strategies, but semantic meaning is rarely controlled (Citron, 2012). According to Citron's review (2012), the interaction between emotional and lexical features during word-recognition is highly complex, with effects being observed at pre-lexical, lexical, and post-lexical processing points. Unfortunately, few consistent conclusions have been reached with regards to language comprehension of emotionality. Based on our findings regarding taboo word processing, we propose that semantic meaning may be an important variable to account for while examining emotional language processing. Manipulations of emotionality could be semantically anchored to a core meaning so as to prevent spurious results. Osgood and colleagues, who first quantified emotionality in the early days of psycholinguistics (Osgood & Suci, 1955), warned researchers not to disregard the importance of non-emotional meaning (Osgood, 1952), and advocated that researchers interested in meaning should work to integrate these two sources.

One can envisage the gradual construction of a "functional dictionary of connotative meanings" – a quantized thesaurus...wishing to find an adjective which would be like *warrior* in meaning, but derogatory...words like *viscous*, *savage*, and *barbaric* (Osgood, Suci, & Tannenbaum, 1957, p. 330)

Emotional language researchers should consider accounting for semantic meaning so as to more precisely examine the unique influence of emotionality during language processing.

Censorship

The current results add to the literature on linguistic adaptation because they reveal that the language comprehension system utilizes statistically reliable cues in order to process lexical information. Censored words were altered such that orthographic and phonological information was replaced with non-linguistic information, but after a lifetime of experience with censorship, participants suffered no observable deficits from the missing orthographic and phonological information. Processing of censored words was so unimpeded that the memory representations of censored words did not differ from taboo words. Even with the missing orthographic information, participants required a similar amount of time to attain the same result when processing censored words as when processing taboo words. How did censored words not involve any additional processing requirements?

One theory of language comprehension that may explain these results is the noisy-channel model. The noisy-channel model argues that communication occurs through a noisy-channel and the comprehender must process input that is often corrupted from a variety of sources (e.g. production errors, phoneme coarticulation, orthographic similarity, environmental noise, ambiguity, etc.). The comprehender must identify statistical regularities within the corrupted input in order to extract, and even infer, the content of the speaker's intended message. From the perspective of the noisy channel model, the language comprehension system can be described as being a Bayesian decision maker (Gibson, Bergen, & Piantadosi, 2013; Bicknell & Levy, 2010). In the case of reading, the Bayesian Reader (BR) accumulates noisy evidence from the environment and makes sub-optimal decisions when sufficient evidence has been gathered (Norris, 2006; 2009). Norris & Kinoshita (2012) argue that word recognition is a computational result where perceptual data is matched to the perceptual information attached to each lexical item. Each lexical item is given a *likelihood-value* which determines how likely it is that the lexical item is the currently fixated word. The BR continues to accumulate noisy perceptual data and eventually, a specific

lexical item has a sufficient *likelihood-value* that the BR determines the identity of the currently fixated word.

We argue that the noisy channel model can account for the findings in these studies. Under this perspective, censorship is not orthographically meaningless, but is instead a unique form of orthographic information. The character string *s**t* and the character string *j**k* are both a sequence of symbols. However, the word *shit* has been processed in a censored form much more often than *junk*, which has likely never been processed in such a way. Therefore, participants were more familiar with *s**t* than they were with *j**k*, so accessing the meaning of censored words came more easily than the masked words.

Word identification requires that the target word be differentiated from its orthographic neighbors. It is known that orthographic neighborhood size (number of words that differ from a specific word by one letter) inhibits word-recognition during sentence-integration processes (Pollatsek, Perea, & Binder, 1999). According to the noisy channel model, a large orthographic neighborhood means that words are more difficult to distinguish due to perceptual competition with other viable candidates because more evidence must be collected so as to differentiate the target word from its perceptual competitors (for more information, see Norris, 2013). Based on the findings from Experiment 1, word recognition processes may not be affected when orthographic neighborhoods are artificially increased. This seems reasonable because the frequency of a word's orthographic neighbors tends to drive such effects (Hawelka, Schuster, Gagl, & Hutzler, 2013) and masked words are rare in the real world. That we observed an effect of the linguistic environment on refixations, but not on single-fixation, suggests that certain language comprehension strategies may be isolated from certain types of knowledge. Lexical knowledge that was gleaned from experience within the linguistic environment motivated readers to double-check words, but experience did not modulate base-level word-recognition processes. Experience did not alter comprehension strategies; experience altered the reader's confidence in what the language comprehension system extracted. Future research should be conducted to further examine the relationship

between the language comprehension system and different sources of top-down knowledge, particularly in terms of the distinct processing stages that are affected by these sources of information.

Summary

The present results provide information regarding the underlying architecture of the language comprehension system and its relationship to other cognitive systems. Previous researchers have reported that taboo words automatically affect processing across a wide array of cognitive systems. However, we observed that the language comprehension system processes taboo words no differently than matched controls after accounting for a word's semantic contribution to the sentence. We also observed that censored words are processed very similarly to complete words, with similar processing strategies occurring during word-recognition even when the reliability of censorship as a lexical cue was violated. The presence of masked words within the context of an experiment had little to no effect on early word recognition of censored words, but readers' confidence in the accuracy of word-recognition was impaired, motivating participants to double-check the identity of the censored word. This pattern of results suggests that the statistical properties of the experiment did not affect lexical processing strategies, but later processing mechanisms were informed by this information. In future research it may be useful to manipulate the linguistic environment in different ways to further examine the relationship between world-knowledge and early lexical access.

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Appendix A.

The stimuli for these experiments are presented below. Item numbers marked with an asterisk were not presented in experiment 2.

- 1 At the auction, the new employees were complete (asses/a**es/fools/f**ls) while they were on stage.
- 2 Colin's class started acting like total (asses/a**es/fools/f**ls) and ruined the play for everyone.
- *3 Everyone just wanted those (asses/a**es/fools/f**ls) to stop trying to be the coolest kids in town.
- 4 Frank knew his friends were total (asses/a**es/fools/f**ls) but he didn't know how to leave the group.
- 5 His friends turned into total (asses/a**es/fools/f**ls) whenever they played video games.
- 6 The athletes felt like total (asses/a**es/fools/f**ls) when they made such a rookie error.
- 7 The comedian easily handled the (asses/a**es/fools/f**ls) who tried to heckle him.
- 8 The conductor told the boys they were complete (asses/a**es/fools/f**ls) and kicked them off the train.
- 9 The film maker wanted the kids to be complete (asses/a**es/fools/f**ls) for the upcoming scene.
- 10 The frat guys were complete (asses/a**es/fools/f**ls) at the club so the bouncers threw them out.
- 11 The gang was a bunch of immature (asses/a**es/fools/f**ls) trying to act tough around their friends.
- 12 The grocery store manager told the (asses/a**es/fools/f**ls) to get out of the store immediately.
- 13 The musicians hated the (asses/a**es/fools/f**ls) in the front row who wouldn't be quiet.
- 14 The new kid made the students look like (asses/a**es/fools/f**ls) after he beat them in poker.
- 15 The only way the kids stopped being (asses/a**es/fools/f**ls) was if privileges were taken away.
- 16 The prosecutors felt like complete (asses/a**es/fools/f**ls) when the defense defeated their argument.
- 17 Cynthia grabbed her friends to (bitch/b**ch/whine/wh**e) about every minor problem in her life
- 18 Jack called the complaint department to (bitch/b**ch/whine/wh**e) about his faulty electric stove.
- 19 Jack hated to hear his girlfriend (bitch/b**ch/whine/wh**e) about the way her friends treated her.
- 20 Kate didn't get why people had to (bitch/b**ch/whine/wh**e) about the slightest problem in their lives.
- 21 No one likes to see their lawyer (bitch/b**ch/whine/wh**e) about their personal legal troubles.
- 22 Occasionally, everyone needs to (bitch/b**ch/whine/wh**e) and moan to let off some steam.
- 23 The actor went to the director to (bitch/b**ch/whine/wh**e) about his not receiving the lead role.
- 24 The baseball player stopped the game to (bitch/b**ch/whine/wh**e) about the umpire's bad calls.
- 25 The clerk hated hearing people (bitch/b**ch/whine/wh**e) about the policies on coupons.
- 26 The coach stopped the match to (bitch/b**ch/whine/wh**e) to the referee about the bad call.
- 27 The employees really wanted to (bitch/b**ch/whine/wh**e) about their arrogant boss.
- *28 The gambler suddenly began to (bitch/b**ch/whine/wh**e) about his continual run of bad luck.
- 29 The hunter began to (bitch/b**ch/whine/wh**e) about his failures at every opportunity.
- 30 The mother hated hearing her son (bitch/b**ch/whine/wh**e) about the unfairness in his life.
- 31 The office workers would (bitch/b**ch/whine/wh**e) every day about the policies for their reports.
- 32 The priest was sad to hear the boys (bitch/b**ch/whine/wh**e) about having to attend services.
- 33 As a child Tasha was scared of that (damn/d**n/grim/g**m) book about these evil cannibals.
- 34 Erin fell in love with this (damn/d**n/grim/g**m) painting and began to obsess over it.
- 35 Jacquelin hated going to the (damn/d**n/grim/g**m) funeral because she wasn't ready to mourn.
- 36 No one at the party realized that the (damn/d**n/grim/g**m) ghost story was based in truth.
- 37 On full moons, Janet felt a (damn/d**n/grim/g**m) unsettling feeling she could never shake.
- 38 Phil was becoming very scared of the (damn/d**n/grim/g**m) nightmares he was having every night.
- 39 The camper hated the (damn/d**n/grim/g**m) atmosphere after his friend left the campsite.

- *40 The couple didn't expect to like the (damn/d**n/grim/g**m) movie, but they were moved by the acting.
 41 The director knew no one liked his (damn/d**n/grim/g**m) speech but they needed to hear it.
 42 The doctor was fed up with the (damn/d**n/grim/g**m) patient's depressed outlook on life.
 43 The lazy student hated the (damn/d**n/grim/g**m) book he was assigned to read in class.
 44 The nomads hated thinking about the (damn/d**n/grim/g**m) reality of the water supply in the desert.
 45 The old man looked at the (damn/d**n/grim/g**m) sky and knew a violent storm was coming.
 46 The oncoming storm made the (damn/d**n/grim/g**m) performance all the more eerie
 47 The school counselor helped Jill see that her (damn/d**n/grim/g**m) fears of death were unrealistic.
 48 William was not hopeful about the (damn/d**n/grim/g**m) situation that faces Tibet.
 49 Anyone who tried to (fuck/f**k/mess/m**s) with the local gang didn't last long in the neighborhood.
 50 Even the new kid at school knew not to (fuck/f**k/mess/m**s) with the school bully.
 51 If the teacher were to (fuck/f**k/mess/m**s) up, the principal told her she would be fired.
 52 Lyle's friend knew he would (fuck/f**k/mess/m**s) up and stopped him from making a fool of himself.
 53 The chess player willed himself not to (fuck/f**k/mess/m**s) up during the intense match.
 54 The director was worried that the new plot would (fuck/f**k/mess/m**s) up the ending he wanted.
 55 The easiest way to (fuck/f**k/mess/m**s) up is to simply stop concentrating.
 56 The fighter was ruthless and did not (fuck/f**k/mess/m**s) around during his matches.
 57 The fitness instructor told her client not to (fuck/f**k/mess/m**s) with their new exercise regimen.
 58 The geeks knew not to (fuck/f**k/mess/m**s) with the football players on game day.
 59 The guitarist knew not to (fuck/f**k/mess/m**s) with the singer directly before the show.
 60 The plumber didn't want to (fuck/f**k/mess/m**s) around while using the rusty old tools.
 61 The pool shark couldn't wait to (fuck/f**k/mess/m**s) with the new guy at the pool hall.
 62 The skateboarder was terrified that he would (fuck/f**k/mess/m**s) up as he came down the ramp.
 63 The worst way Sharon could (fuck/f**k/mess/m**s) up was if she decided to stop trying.
 *64 Tom couldn't stop thinking about the ways he could (fuck/f**k/mess/m**s) up during the competition.
 65 In the basement was a pile of (shit/s**t/junk/j**k) from the manager's old business.
 66 Kyle had a mountain of (shit/s**t/junk/j**k) to sort through back at his office.
 67 The athlete's shoes were a piece of (shit/s**t/junk/j**k) but he refused to throw them away.
 68 The best man picked up the groom's (shit/s**t/junk/j**k) that he'd left at the bachelor party.
 69 The chainsaw was a piece of (shit/s**t/junk/j**k) and no one felt safe using it.
 70 The dirty blackboard looked like complete (shit/s**t/junk/j**k) to the parents of the students.
 71 The fashion designer's new wardrobe was complete (shit/s**t/junk/j**k) according to the reviews.
 72 The hockey goalie's game was complete (shit/s**t/junk/j**k) while he was getting over the flu.
 73 The janitor sighed at the pile of (shit/s**t/junk/j**k) he had to clean up.
 74 The lawnmower was a piece of (shit/s**t/junk/j**k) but the owner couldn't afford a new one.
 75 The mechanic told him his car was a piece of (shit/s**t/junk/j**k) and was not worth fixing
 76 The old man knew his home looked like (shit/s**t/junk/j**k) but he didn't care what others thought.
 77 The pilot's plane looked like (shit/s**t/junk/j**k) but he made sure it was safe to fly.
 78 The runner never ate fast food or any (shit/s**t/junk/j**k) like it prior to a big race.
 *79 The student moved his classmate's (shit/s**t/junk/j**k) out of the way so he could sit down.
 80 The violinist completely despised the (shit/s**t/junk/j**k) that popular radio stations played.

APPENDIX B

Post hoc analyses

The current results from both experiments indicated no difference in reading time between taboo and neutral words. We conducted post-hoc analyses to test the legitimacy of the null effect.

First, we combined the two experiments into a single analysis, thus resulting in a sample size of sixty-two participants. Uncorrected paired t-tests revealed no differences between taboo and neutral conditions in first-fixation, single-fixation, gaze-duration, or total-dwell time (all p-values > 0.4). This null effect was observed regardless of whether these data were averaged based on subjects or items. Descriptive statistics (seen in Table 7) speak to the strength of the null effect. Across all of the reading measures, taboo and neutral words never differ by more than three milliseconds.

We then considered the possibility that some of the word-pairs did not differ strongly enough in tabooeness to invoke an effect (i.e. *damn* and *grim* are likely more similar in offensiveness than *shit* and *junk*, see Mackay et al, 2004). We conducted uncorrected paired t-tests on every reading measure within each individual word-pair to test for consistent effects of tabooeness (e.g. *asses* < *fools*; *damn* < *grim*; etc.). None of the word-pairs differed in total-reading time (although *damn* < *grim* is trending, $p = 0.094$) and the word-pairs associated with *shit* and *fuck* never reach significance according to all reading measures, all p-values > 0.25. There is an effect of tabooeness within the word-pairs *asses* and *bitch* across first-fixation, single-fixation, and gaze-duration (all p-values < 0.05), but the effects are in opposite directions. The word *asses* was read for a longer time than *fools* and the word *whine* was read for a longer time than *bitch*. Perhaps reading time was affected by certain lexical features such as syllable count (*asses* contains more syllables than *fools*) or orthographic complexity (initial consonant cluster in *whine* is more complex than in *bitch*), but it seems reasonable to conclude that tabooeness did not have a consistent effect.

We next considered the possibility that certain individual differences may determine reading time of taboo words. Previous research suggests that individual differences can alter physiological responses to taboo words (Jay, 1980) such as personality traits (Stelmack & Mandelzys, 1975), gender (Nothman,

1962), childhood punishment for swearing (Tomash & Reed, 2013), and even the behavior of the experimenter (Hicks, 1970). It is possible that such a variable is currently influencing reading time, but we cannot observe it. In order to examine whether such a variable may exist, we divided the participants into a low-taboo reading time group and high-taboo reading time group within each experiment using a median-split. This meant that the sixteen participants who spent the shortest amount of time reading taboo words in Experiment 1 and the fifteen participants who spent the shortest amount of time reading taboo words in Experiment 2 were combined into a “short-reading time of taboo words” group and the remaining participants were combined into a “long-reading time of taboo words”. The assumption behind this grouping was that if an individual difference variable could motivate reading time of taboo words, then low reading-time participants may process the taboo and neutral words differently than the high reading-time participants. For example, the low reading-time people may find taboo words very offensive so they try to avoid experiencing them, but this effect of tabooeness would not apply for the neutral words. Therefore, low reading-time participants may spend less time reading taboo words than neutral words, but such an effect is drowned out by an opposite relationship within the high reading-time participants.

We conducted a linear-mixed effects regression examining the effect of reading-time (low vs. high), experiment (first vs. second), and condition (neutral vs. taboo) on the effect of log-transformed total-reading time, with subjects and items coded as random effects. The results for this analysis can be seen in Table 8. There was a three-way interaction between reading-time, experiment, and condition. This revealed that taboo words were read for a longer amount of time than neutral words by high-taboo readers relative to low-taboo readers, but only within the second experiment. No two-way interactions were significant and the effect of reading-time was marginally significant, revealing that high-taboo readers spent a greater amount of time reading regardless of experiment or condition.

We will not spend a large amount of time trying to explain the interaction effect because it is difficult to understand what is being manipulated across experiments. We will offer one possibility, but the highly extensive post-hoc nature of this analysis must be appreciated. It is possible that the reading-

time-variable simply divided participants into slow and fast readers, with little division being caused by differences between taboo and neutral word reading time; explaining the main effect of reading-time. In Experiment 1, the taboo and neutral words both had censored equivalents (e.g. *damn*: *d**n*; *grim*: *g**m*), but in Experiment 2, only taboo words had a censored equivalent. It's possible that slower readers needed more time in Experiment 2 to read taboo words relative to neutral words because the linguistic environment presented a greater degree of perceptual competition for taboo words than for neutral words. Regardless, it is clear that the linguistic environment interacted with our reading-time variable in affecting reading times of taboo and neutral words. Therefore, the reading-time variable cannot be interpreted as reflecting an individual-difference which specifically influenced the processing of taboo words relative to neutral words.

To summarize, we conducted post-hoc analyses to better examine the limits of the null-effect of taboohood on reading time. We found little evidence that online language processing strategies are influenced by a word's taboohood even after pooling data from all participants, refusing to conduct any corrections for multiple-comparisons, examining patterns within individual word-pairs, and considering the possibility that individual differences between participants could also have influenced reading time of taboo and neutral words. Given that our extensive efforts to break this null-effect have failed, we think that we have evidence to argue that the language comprehension system does not process taboo words differently from semantically controlled neutral words. The taboo-related features that affected memory mechanisms (as seen in Exp. 2) were either not considered by or were not accessible to online language processing mechanisms. Future research that purports to examine taboo-related features (e.g. social-inappropriateness, arousal, emotionality etc.) should consider our failure to break this null-effect and respond accordingly (e.g. Nye & Ferreira's participant-variable reflected *overall reading speed* more than it did extraversion, thus the real effect of extraversion was not observed in their post-hoc analyses).

[Insert Table 7 here]

[Insert Table 8 here]

Table 1

Mean reading times in milliseconds (standard deviations in parenthesis) on critical word (Experiment 1)

Measure	Condition			
	Taboo	Neutral	Censor	Masked
FFD	257 (85)	259 (92)	269 (114)	306 (137)
SFD	259 (86)	259 (91)	268 (115)	311 (138)
GD	271 (99)	277 (112)	335 (213)	433 (322)
TT	310 (178)	312 (163)	409 (310)	657 (614)

FFD, first-fixation duration; SFD, single-fixation duration; GD, Gaze-duration; TT, Total Time

Table 2

Mixed-model output for critical word reading times (Experiment 1; bold indicates significant differences)

Measure	Predictor	Estimate	SE	t value
FFD	intercept (taboo)	5.49	0.028	196.6
	tabooness (neutral)	-0.006	0.022	-0.3
	censorship (censored)	0.018	0.027	0.69
	tabooness*censorship (masked)	0.13	0.035	3.82
SFD	intercept (taboo)	5.51	0.026	207.88
	tabooness (neutral)	-0.012	0.023	-0.55
	censorship (censored)	0.009	0.035	0.24
	tabooness*censorship (masked)	0.15	0.041	3.67
GD	intercept (taboo)	5.54	0.03	183.68
	tabooness (neutral)	-0.0001	0.027	-0.01
	censorship (censored)	0.11	0.033	3.53
	tabooness*censorship (masked)	0.22	0.038	5.83
TT	intercept (taboo)	5.62	0.037	153.64
	tabooness (neutral)	0.007	0.12	0.06
	censorship (censored)	0.15	0.046	3.44
	tabooness*censorship (masked)	0.38	0.1	3.86

FFD, first-fixation duration; SFD, single-fixation duration; GD, Gaze-duration; TT, Total Time

Table 3

Mixed-model output for practice effects on total reading times critical word (Experiment 1; bold indicates significant differences)

Condition	Total Time		
	Estimate	SE	t
intercept (taboo)	5.63	0.032	170.99
tabooness (neutral)	0.0041	0.036	0.12
censorship (censored)	0.15	0.048	3.16
tabooness*censorship (masked)	0.38	0.072	5.25
Trial	-0.0027	0.0009	-2.91
tabooness*Trial (neutral*Trial)	0.0015	0.0013	1.16
censorship*Trial (censored*Trial)	-0.0002	0.0013	-0.2
tabooness*censorship*Trial (masked*Trial)	-0.006	0.0019	-3.35

Table 4

Mean reading times in milliseconds (standard deviations in parenthesis) on critical word (Experiment 2)

Measure	Condition		
	Taboo	Censor	Neutral
FFD	247 (82)	241 (93)	248 (82)
SFD	248 (83)	241 (93)	249 (82)
GD	260 (95)	264 (127)	260 (94)
TT	293 (136)	325 (233)	286 (124)

FFD, first-fixation duration; SFD, single-fixation duration; GD, Gaze-duration; TT, Total Time

Table 5

Mixed-model output for critical word reading times (Experiment 2)

	Condition	Estimate	SE	t value
	Intercept (Taboo)	5.46	0.023	236.16
FFD	Neutral	-0.0003	0.021	-0.02
	Censor	-0.035	0.022	-1.55
	Intercept (Taboo)	5.47	0.023	233.92
SFD	Neutral	-0.0035	0.021	-0.17
	Censor	-0.041	0.023	-1.71
	Intercept (Taboo)	5.51	0.025	213.16
GD	Neutral	0.0007	0.025	0.03
	Censor	-0.011	0.029	-0.37
	Intercept (Taboo)	5.59	0.032	173.47
TT	Neutral	-0.012	0.049	-0.25
	Censor	0.0303	0.04	0.76

FFD, first-fixation duration; SFD, single-fixation duration; GD, Gaze-duration; TT, Total Time

Table 6

Mean likelihood of response in memory task (standard deviations in parenthesis) for each condition that was encountered in the sentence reading task (Experiment 2)

Condition	Response		
	Taboo	Censor	Neutral
Taboo	0.42 (0.19)	0.35 (0.18)	0.21(0.11)
Censor	0.35 (0.20)	0.44 (0.20)	0.20 (0.15)
Neutral	0.26 (0.14)	0.29 (0.16)	0.43 (0.16)

Table 7

Mean reading times in milliseconds (standard deviations in parenthesis) on critical word (all experiments)

Condition	Measure			
	FFD	SFD	GD	TT
Neutral	254 (87)	254 (87)	268 (104)	298 (145)
Taboo	252 (84)	253 (85)	265 (97)	301 (158)

FFD, first-fixation duration; SFD, single-fixation duration; GD, Gaze-duration; TT, Total Time

Table 8

Mixed-model output for post-hoc analysis on total-reading times as a function of condition, experiment, and personality-variable (all experiments; bold indicates significant differences)

Fixed Effect	Estimate	Std. Error	t value
Intercept (Neutral)	5.588	0.042	133.214
Condition (Taboo)	0.007	0.036	0.193
Reading-time (High)	0.101	0.058	1.75
Experiment (Second)	-0.083	0.058	-1.413
Condition:Reading-time	-0.033	0.050	-0.663
Condition:Experiment	-0.068	0.050	-1.356
Reading-time:Experiment	0.007	0.082	0.082
Condition:Reading-time:Experiment	0.192	0.070	2.741